

**Sketches of Brazil : including new views on tropical and European fever /
by Robert Dundas.**

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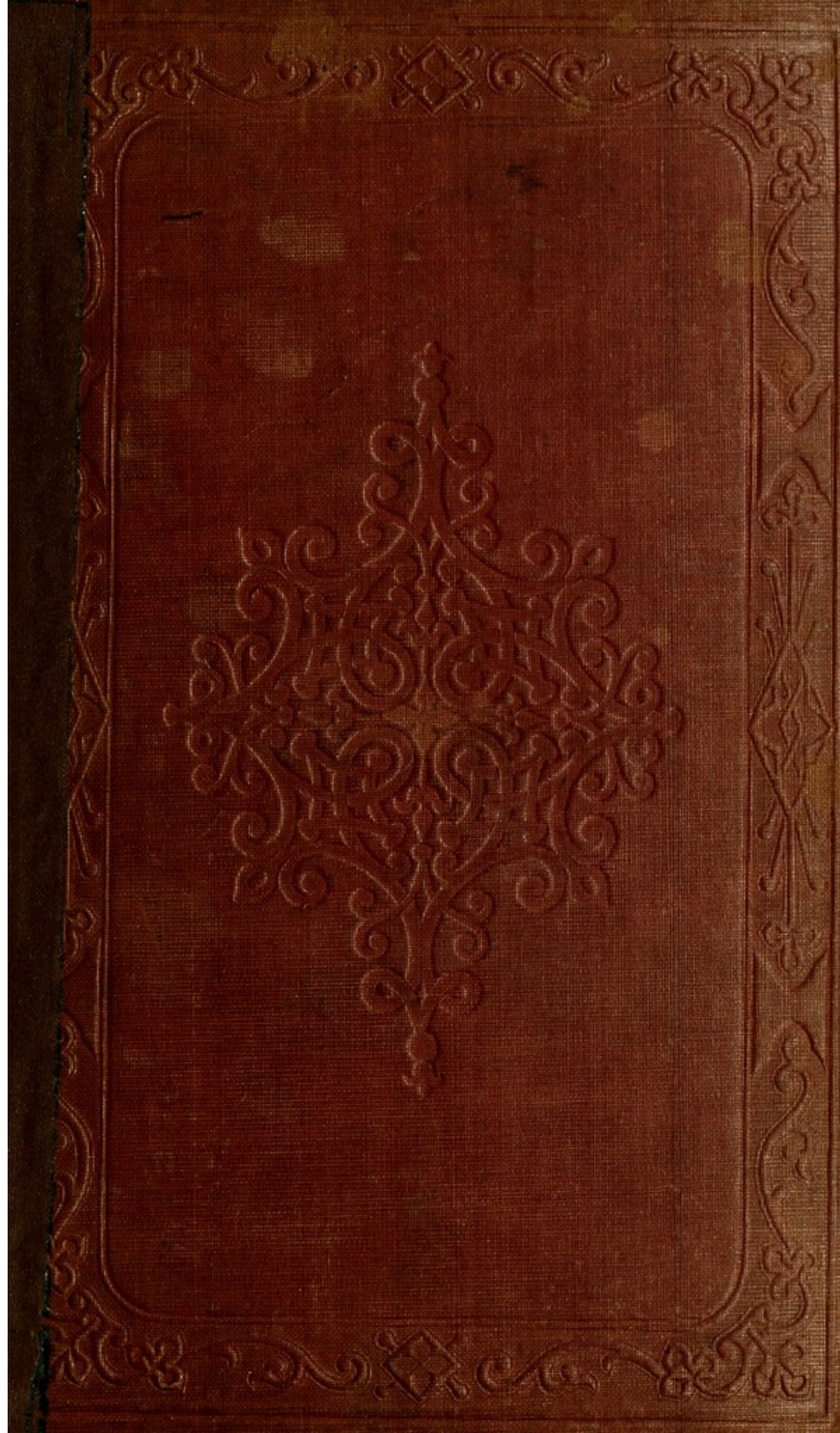
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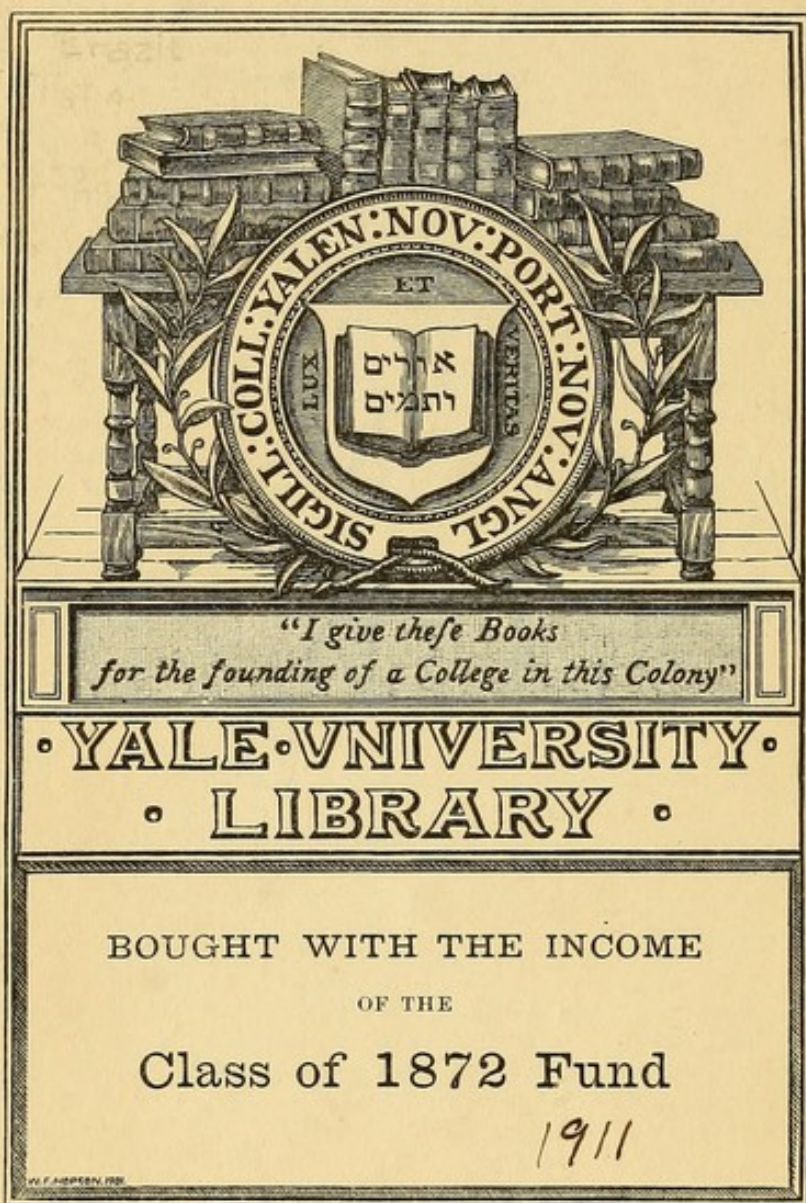
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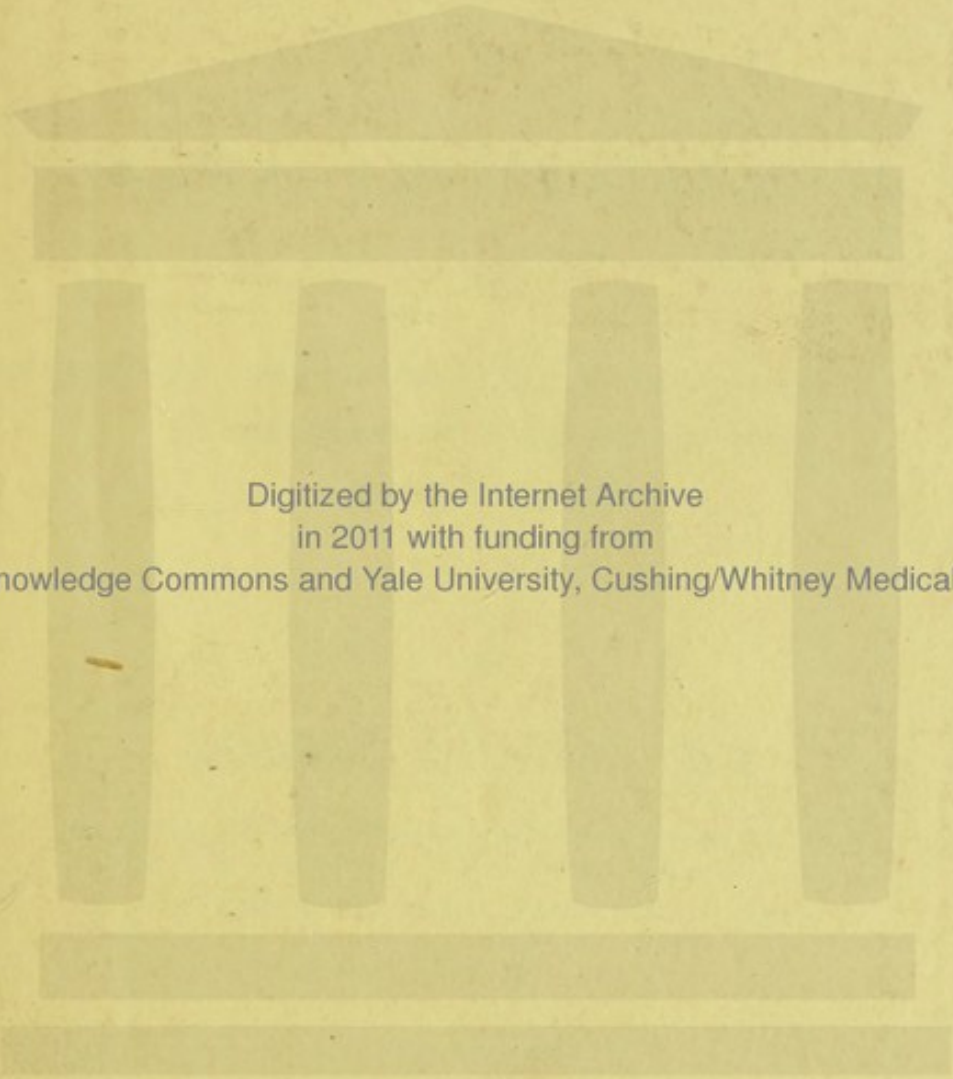


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REPORTS OF THE

COMMISSIONERS OF THE

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FOR THE YEAR 1881

IN RESPONSE TO A RESOLUTION

PASSED BY THE HOUSE OF REPRESENTATIVES

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SKETCHES OF BRAZIL;

INCLUDING

NEW VIEWS

ON

TROPICAL AND EUROPEAN FEVER,

WITH

REMARKS ON A PREMATURE DECAY OF THE SYSTEM INCIDENT
TO EUROPEANS ON THEIR RETURN FROM
HOT CLIMATES.

BY

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SKETCHES OF BRAZIL

1852

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P R E F A C E.

THE principles laid down in the following pages have occupied the writer's thoughts for many years, and in many lands; and a profound conviction of their truth, and of their importance to the preservation of health, and in the treatment of disease, has imposed on him the duty of publishing them.

The Author is well aware that these opinions are in direct opposition to those held by the profession; and he has, therefore, clearly and briefly, submitted the chief evidence on which, against his own early

convictions, he was led first to doubt, and finally to reject doctrines, sanctified, as it were, by the greatest names in ancient and modern medicine.

As to the rest, the Author cares only for truth. He has performed what he deems a duty to his profession, and waits its final judgment.

“Opinionum commenta delet dies; naturæ judicia confirmat.”

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INTRODUCTORY REMARKS.

MODERN AUTHORITIES ON FEVER: CORMACK, JENNER,
&C.—DIFFERENT EPIDEMICS DESCRIBED—TYPHUS,
TYPHOID, AND RELAPSING FEVER, NOT ESSENTIALLY
DISTINCT—QUININE CUTS SHORT—CONTINUED FEVER.

THE observations contained in the following pages are chiefly the result of my experience, as Medical Superintendent of the British Hospital at Bahia, in the Brazils, for twenty-three years, and as a medical officer in the army, in different quarters of the world, during five years. I had contemplated publishing my opinions under a different form from that in which they now appear; but circumstances induced me to embody them in a series of lectures, which I delivered at the Liverpool Northern

Hospital, in the beginning of the present year.

Prior to the delivery of these lectures, it was suggested to me, by competent authority, that I should "read up," and make myself thoroughly acquainted with all that had been previously written on Fever, a subject to which so many of my observations refer. Another of my advisers, of equally high authority, strongly recommended that I should not read a single line on that or any of the subjects to be discussed, until after my lectures had been written and delivered; that I should, in fact, simply give the results of my own experience, unbiassed by the statements or the opinions of others. Influenced partly by conviction, partly by other motives, I adopted the latter suggestion; the lectures were, therefore, delivered without any special preparation—a perusal of the works of Lancisi alone excepted—beyond that general

information which necessarily falls to the share of every practical physician. They are now published almost *verbatim* as delivered, without alteration either in the style or manner, although both, I am conscious, might be largely improved. The motives which influenced me in delivering them without special preparation, have determined me to publish them without material alteration.

Following out the very rational suggestions of both my advisers, I have lately looked attentively into the writings of the best modern authorities on the subject of Fever. Here, I find the various forms, and the succession of the paroxysms in remittent and intermittent fevers, defined with a mathematical precision, unfounded in nature, and showing that the writers had not enjoyed the opportunity of observing these diseases, as they actually prevail, in different regions of the globe. There is nothing in these writings to shake my

previous convictions: on the contrary, I find them supported by an overwhelming mass of evidence, intended for other purposes. One conviction has, indeed, been profoundly impressed on my mind by the perusal of this evidence; namely, that had these eminent observers—men obviously combining great accuracy and candour, with high talents and great practical knowledge—been placed under more favourable circumstances, they would have arrived at conclusions little different from my own.

It is altogether beyond my purpose even to name the numerous, interesting, and valuable publications which have, within the last twenty or thirty years, issued from the medical press, at home and abroad, on the subject of Fever. I shall principally confine my remarks to a monograph, written in 1843, by Dr Cormack,* as embodying, in

* CORMACK, JOHN ROSE, M.D., *Natural History, Pathology, and Treatment of the Epidemic Fever at*

a small compass, a great mass of information on the fevers of that and previous epochs, and as bearing the legible stamp of being the work of a talented, accurate, and candid observer, who looked at disease as it actually presented itself, and not with a view to support some ingenious theory, or preconceived notion—one of a class of men invaluable to medical science. I have, also, lately perused, with great interest, a valuable article on Fever in the *British and Foreign Medico-Chirurgical Review*, for July 1851, containing, with other subjects, an abstract of the published descriptions of the epidemic fever which prevailed in Upper Silesia, in 1847.

In the Scotch epidemic of 1843, the following were the leading features, as described by Dr. Cormack:—

“The symptoms of invasion are in all present prevailing in Edinburgh and other towns; illustrated by cases and dissections. Edinburgh, 1843.

cases remarkably similar, both as to their nature and order of occurrence. The patient is first seized with coldness, rigors, headache, pain in the back, and more or less prostration of strength." "After a period, varying from less than half an hour to several hours, the cold fit terminates, when the severity of the headache greatly increases, and a dry burning heat comes over the whole body, accompanied by much thirst and general uneasiness. The hot stage is succeeded by a sweat, usually very profuse, continuing for a number of hours." "Sometimes, though rarely, there is no sweating for two or three days after the seizure. Occasionally, also, there is no well-marked hot stage between the cold and the sweating fits." . . . "The perspiration has a characteristic disagreeable smell, and is decidedly acid." "Nausea and vomiting usher in and attend the sufferings of the first two days." . . . "A remission on the

third day is very common. It occurred in all the cases which I have had an opportunity of attentively observing from the invasion onwards. On or about the fifth day, there is an evident manifestation of the violence of the disorder being expended; and this change for the better is often very sudden and complete. One day, we hear the patient moaning and groaning in pain; and on the next, *he is at ease and cheerful, his only complaints being of hunger and weakness.* This state is generally ushered in by a copious sweat." "After this change the pulse, tongue, and skin, are quite natural."* (Pp. 3, 4, 5.)

Further on in his book, Dr. Cormack makes the following remarks:—

"The bronzing, leadening, and purpling of the countenance before and after seizure, is to the visitor of our fever wards one of

* "The pulse has been noticed to fall, during an intermission, from 160 to 40."

the most remarkable peculiarities of the prevailing epidemic. Dr. Maclagan upon one occasion remarked to me, upon entering one of my wards, that the bronzed countenances all around strongly reminded him of the military hospital of which he had charge during the Walcheren epidemic. Others, again, familiar with the remittents and intermittents of Canada, the West Indies, and Italy, have assured me, that the facial bronzing bore a strong resemblance to what they have seen in persons affected with them in these countries. The more marked cases of bronzing brought to my own recollection the aspect of the inhabitants of such marshy districts of Italy as the Pontine marshes, and the unwholesome swamps around the ruined temples of Pæstum.* (Pp. 85, 86.)

Dr. Cormack also speaks of the "almost

* Well, indeed, they might; for *the diseases were essentially the same.*

uniform occurrence of one or more relapses, unless antiperiodic remedies were employed." "Third attacks," he says, "I have found to be exceedingly common; fourth attacks not very uncommon; and several instances have occurred, to my knowledge, of patients having a fifth attack." (Pp. 86 and 87).

The author of the able article already referred to, in the *British and Foreign Medico-Chirurgical Review*, arrives at conclusions from which I am compelled *toto cælo* to differ: nevertheless, a few extracts from the accounts given by the historians of the Silesian epidemic may be placed in interesting juxta-position with the details quoted from Dr. Cormack's volume.

Dümmler,* one of the most accurate observers of the Silesian fever, insists on the occurrence of relapses. "Not unfrequently,"

* DÜMMLER, Dr. F., Ueber den Oberschlesischen Typhus.

he says, "the patients were thrown back by one or more relapses, which were true repetitions of the disease." He observes that "the period of the relapses appeared to observe a seven-day rhythm, so *that a typhus, with many repeated relapses at equal intervals, strongly resembled an intermittent fever.*" He himself had an attack, in which he suffered two relapses.

Dr. O'Brien's description of the Dublin epidemic of 1826 is exactly in accordance with the above, and, therefore, need not be more especially noticed. He states the patient to be "harassed by *relapses*, which prolonged the whole duration of his illness, even beyond that of the most protracted typhus;" and Dr. R. Paterson* states that *the relapse "came on like a fit of ague, almost to an hour."* All agree that there

* PATERSON, ROBERT, M.D. An account of the Epidemic Fever, 1847-8, in Edinburgh. *Edinburgh Medical and Surgical Journal*, vol. lxx., p. 371.

was sometimes an intermission, sometimes a remission only.

The obviously accurate details to which I have referred, and other accounts of epidemics of fever which might be mentioned, are evidently the histories of irregular intermittent or remittent fevers, with some symptom, or group of symptoms, made prominent by accidental circumstances; they moreover tend to show the fever to be altogether independent in its origin of any special poison, indigenous or imported. This will, I apprehend, be at once recognised by any practitioner, unbiassed by early habits of thinking, by education, and by the nosological charts of our system-makers, especially if he has witnessed the fevers of hot climates.

It is interesting to remark, (notwithstanding the prevalence of opposite convictions on the question of fever,) how closely the idea of the merely imaginary or

artificial nature of the distinctions between the different types of fever, seems to have haunted the observing mind of Dr. Cormack. He noticed the ordinary and more fatal typhus gradually superseded (as often occurs elsewhere) by another form, "the relapsing fever;" and, although obviously *disposed* to the doctrine of different and distinct poisons, he admits that he could detect no well-marked line of demarcation between the "true typhus" and the new comer. "It may be true," he says, "that the cases of what are termed 'true typhus' come pretty generally from the same houses; but then, do they not come also from the same families? May it not be peculiarity of constitution in these individuals which determines this particular manifestation of the morbid poison? Though the constitution of an epidemic gives to it a character, yet idiosyncrasy causes families and individuals to be affected differently from the generality

of persons. No person who saw the rosy spots in the case of Mary Wallace, *on their first eruption*, could say, that it was not the true measly typhus eruption; and yet, the bronzing, purpling, and jaundice, along with the urgent vomiting, rheumatic pains, and the relapse at the usual period, proclaimed, unequivocally, that she was afflicted with the prevailing epidemic; or rather, perhaps, a sort of bastard between the two forms of fever." (P. 106.)

Dr. Alison admits that he has seen instances "in which strictly typhoid cases, with the characteristic eruption, have been brought from the same rooms, in which a succession of the milder cases (the relapsing fever) have occurred at the same time." Dr. Cormack further states (p. 107) that, at the commencement of his study of the epidemic fever, he "regarded it as *essentially and totally different* from typhus; but recent circumstances, and more matured weighing

of evidence, greatly modified this opinion." He justly adds, in the words of Rousseau, "that the truth is in the facts, and not in the mind which observes them."

The *Charleston Medical Journal and Review*, for July 1851, contains an analysis of a "Treatise on the Congestive Fever," by Dr. S. Ames, of Montgomery, Alabama. From this editorial notice, it would appear that many American writers, as Drs. Wood Bell, and Bartlett, are opposed to considering congestive fever as a distinct disease, and regard it as identical with the pernicious fevers of Europeans. Dr. Ames, however, maintains the distinct nature of these diseases. His definition of fully formed and uncomplicated congestive fever is: "A paroxysmal fever of the tertian type, peculiar to warm climates, to places where intermittent and remittent fevers are epidemic, and to persons between ten and fifty years of age, having no distinction of hot, cold, and

sweating stages in the paroxysm, and being characterised by the following symptoms, viz., moist or profusely sweating skin, surface generally cold, tongue moist, pale, or ash-coloured, pulse small, feeble, and frequent."

This is a striking instance of the extent to which nice nosological distinctions may be carried. The reviewer of Dr. Ames asks, "If it is clearly made out that congestive fever is a distinct disease, . . . may not the congestion arise from, or be simply, a multiplication or aggravation of the paroxysms of remittent or intermittent fever?" The Edinburgh epidemic of 1843, and the Silesian fever of 1847, both presented symptoms of congestion; and Dr. Cormack, in his description, classified the cases under the heads of "moderately congestive" and "highly congestive." Both these diseases were "relapsing" or intermittent. Again, the intermittent fevers of our own country,

as also of the tropics, frequently present well-marked congestive symptoms. How, then, can congestion give a distinctive character to a fever?

From the observations of Dr. Cormack which I just now quoted, we shall be little surprised to find, notwithstanding the urgency of the cases, and "the success of several medical friends," he quickly discovered that abstraction of blood was no safe remedy, (p. 151); that sequences were not effects; that cases of pulmonary inflammation yielded to "the liberal use of morphia and ipecacuan lozenges:" "that the cerebral, pulmonary, and abdominal complications, in which it is proper to abstract blood, are extremely rare, and that, in very many such instances, it is a most hazardous practice;" and that, "in the case of Francis Rose, acute symptoms of enteritis and diarrhœa gave way to morphia and whisky." (P. 156.) He also *suspected* that the good effects of calomel

and opium might be chiefly due to the latter agent.

He fully admits the importance of anti-periodic remedies: yet quinine appears to have been but partially successful in his hands, and utterly unsuccessful in the hands of Paterson, H. Douglas, Smith, &c. A remarkable exception to the general failure in arresting certain sequelæ by the use of quinine is found in the practice of Mr. Wallace of Dublin, whose remarks are exceedingly interesting, and fully in accordance with my own experience of the action of that medicine. He says, "When I commenced the use of bark in this disease, I did not venture to employ it when the inflammatory symptoms were very severe, without preceding its administration by bleeding and purging. But latterly, whenever a case has presented itself, I have prescribed the bark alone, or simply with such medicines as were suited to the regulation

of the bowels; and with the most decidedly good effects. Indeed, I have thought, that the abstraction of blood has, on some occasions, considerably retarded the cure.”*

Yet, with Dr. Jacob of Dublin, Dr. Mackenzie of Glasgow, and other men of acknowledged ability, the remedy totally failed.

Now it must be borne in mind, that, when we indicate any drug as remedial in any individual malady, it is clearly understood that such drug shall be opportunely administered, and in doses adequate to the production of its specific effects on the living organism. No one doubts that mercury or antimony will modify certain diseased conditions; but then it is equally admitted that these drugs, to produce the desired effect, must be given not only opportunely, but in sufficient quantities both positively and relatively. The same

* WALLACE, quoted by Dr. Cormack, *op. cit.* p. 140.

holds good with regard to quinine in fever; to modify the disease, it must be administered in doses sufficiently large and frequent to produce, within a given time, its specific influence on the system; otherwise it is entirely useless, and its administration in small doses is a fatal trifling with time, which too often proves irrecoverable.

The mode of effectually administering this powerful, I might almost say specific agent in fever, I have fully pointed out in my seventh lecture. Ten or twelve grains must be given every two hours, or three grains every hour; smaller doses, such as those ordered by Dr. Cormack, (who appears to have used quinine the least timidly) and others, during the fever, and by Drs. Jacob, Mackenzie, and others, during its sequelæ—one or two grains three or four times a-day—are quite powerless over the disease. The largest doses—five grains every sixth hour—appear to have been given by Dr. Cor-

mack in the fatal case of Michael Dowlands, (p. 50), and continued from the ninth to the thirteenth day, when they were discontinued, though for what reason does not appear. Another patient, "Robert Watson," had quinine in similar doses; and Dr. Cormack states, (p. 168), *it seemed* to prevent the relapses. These two cases alone are sufficient to arrest the attention; and had Dr. Cormack happily progressed—just *doubled* the dose of quinine, and *trebled* its frequency, his history of the epidemic of 1843 would have told a different tale. As an accurate observer, he seemed perfectly alive to the beneficial effects of opium in all forms; and its decided influence (together with stimulants) over the disease, cannot fail to arrest the attention of the most cursory reader.

From a careful perusal of all Dr. Cormack's cases, and numerous others of a similar character, I do not hesitate to affirm

that, in the majority of instances, the fever would have been *at once* arrested by an administration of quinine, such as I have just pointed out.

Two maxims in medicine have unfortunately obtained, with regard to the treatment of disease, an almost equal and world-wide reputation: one, well founded, the result of large and just observation, and of great practical importance—that we should, in all diseases, endeavour to “obviate the tendency to death;” the other, unfounded in nature, most fatal in its consequences, and originating in theory and misapplied observation—“you may *guide* a fever, you cannot *cure* it.” Now, a fever most undoubtedly *can be cured*; that is, in the great majority of cases, all the formidable symptoms may be arrested, and the patient almost immediately placed beyond the sphere of danger: this truth is fully borne out by the cases treated in the Liverpool

Northern Hospital, the Liverpool Fever Hospital, and St. Thomas' Hospital, London, by Dr. Goolden, and others could be abundantly supplied. Far, then, from its being the chief duty of the practitioner to conduct an individual safely through a definite train of symptoms, and the series of morbid changes occurring during the disease, it is rather his bounden duty to meet these symptoms at their commencement, and thus prevent the occurrence of those morbid changes which chiefly jeopardise the life of the patient.

It is urged, on high authority, that there are three, if not more, varieties of fever, originating in three or more distinct poisons—the typhus, the typhoid, and the relapsing.* Let us briefly consider this question.

* It is singular that authors should have set about inventing this new term *relapsing*, when the familiar word “remitting” was equally applicable. Does it not indicate the disposition to support, at all hazards, a preconceived theory?

In the first place, the greater number of the distinctions in the symptoms attempted to be established between these fevers, are obviously either trivial or visionary—distinctions without a difference; and, however broadly they may be defined in the closet, I have never yet met with a physician who could conclusively verify them at the bedside of the patient, although I have witnessed these diseases in this country, in Ireland and in different quarters of the Continent.

As regards the character of the eruption, so constantly insisted on, nothing, I believe, can be more fallacious; for I have myself, more than once, witnessed every variety of eruption, pretty distinctly marked, in the same patient at one period or other of his disease; and practitioners who have seen much of tropical fevers must have observed the occasional appearance of all the eruptions commonly deemed pathognomonic of special forms of European fever. Dr.

Wragg, in a paper published in the *Charleston Medical Journal and Review*, for March 1851, on an Epidemic of "Break-bone Fever" in Charleston in the summer and autumn of 1850, describes the eruption attending the disease as "not at all peculiar in its character, but varying almost infinitely." It resembled, in various cases, scarlet fever, rubeola, impetigo, scarlet rash; in some cases "the skin was intensely red and raised in large and numerous welts;" "in some cases it was erysipelatous, in others petechial; in others like common prickly heat, and, in a few, papular." So vague, indeed, is the value of the eruptions as a diagnostic sign in fever, though so strongly insisted on by authors, that, even at the present hour, we are unable absolutely to diagnose certain forms of eruption from the ordinary appearance of flea-bites.

Moreover, we know that, independently of the English, many of the French, and the

most distinguished German observers, whose claim to accuracy and talent few will contest, have not recognised these varieties of fever as distinct diseases. No practical physician, indeed, will admit that symptoms alone are sufficient to justify a distinct classification of fevers; and neither in the symptoms, nor in the results, of the so-called typhus, typhoid, and relapsing fevers do we find a wider range than is constantly observed in other diseases, and is rationally accounted for by individual peculiarities, difference of season and climate, modes of living, and the sanitary condition of the population attacked, with various other moral and physical influences. In like manner, also, we can rationally explain the difference of mortality in different localities and circumstances.

Dümmler, in his account of the Silesian epidemic of 1847, describes the fever as observing a rhythm, at equal intervals, like

an ordinary ague; and the sequelæ were not very dissimilar. That it was identical with the Scotch epidemic of 1843, (and others observed at different periods in Ireland and Scotland), all admit; yet this Silesian fever presented abundant crops of rubeolous and petechial eruptions, while the Scotch epidemic was characterised by their absence.

Again, the prevalence of several distinct epidemics in the same locality, and at the same epoch, is not only contrary to general experience, but opposed to our knowledge, as far as it goes, of the laws by which epidemics are governed in all quarters of the world; nor does the term "intercurrent" by any means satisfactorily meet the difficulty.

As distinctive of typhus, Dr. W. Jenner, in his valuable work on Fever, observes, that he has never seen a relapse; a proposition, I apprehend, opposed to the general experience of the profession. He also prominently insists on the shorter

duration of the fatal cases of typhus, unaccompanied by any definite anatomical signs, compared with the fatal cases of typhoid fever, in which local lesions are commonly detected, as a proof of the non-identity of the specific causes of the two fevers. Now, I apprehend that this fact admits of another and more satisfactory solution, by reference to the intermittent and remittent fevers of hot climates, as well as numerous diseases of our own latitudes. In the more rapid and fatal form of these maladies, no well-defined local disorganization will be found after death; while, if the disease be prolonged, local lesions are always to be detected. In fact, Dr. Jenner himself states, that if typhus be prolonged beyond twenty-one days, local lesions, sufficient to cause death, (independently of the fever,) are always present. Dr. W. Stokes, in his work on Fever, a work unexcelled in any language, lays down what I believe

to be the true law on this question ; namely, "that in almost any instance of essential fever, local disease springs up at some period or other of its course," . . . "and that a vast proportion of fever cases are carried off by these local affections." He also refers to the question of time.

I am not aware whether either Dr. Wm. Stokes, or Dr. Graves of Dublin, has visited hot climates ; but the latter, in his valuable work on *Clinical Medicine*, observes, that "there is not so much difference between the diseases of Ireland, and of warmer countries, as has been imagined ; they differ, it is true, as to their degree, but not as to their pathology." Now, this is a remarkable statement ; and, supposing Dr. Graves had not himself visited tropical countries, strongly indicative of the genius of the man ; for, of the absolute truth of the doctrine, here so broadly laid down, there exists not the shadow of a doubt. Dr. Cormack justly

remarks, that "were this doctrine more generally appreciated, the accounts of the different fevers unfolded to us would, perhaps, present less picturesque and piquant, but certainly simpler and truer pictures of disease." Most undoubtedly they would.

The parallel, moreover, drawn by Dr. Graves between the Dublin and the tropical yellow fever, admits of no dispute; the correspondence is complete. The same is true of the Edinburgh yellow fever, as observed by Dr. Cormack, although it differed from the Dublin fever in this, that one was of the continued, while the other was of the remittent type; there being, also, instances of intermission. But who that has examined the history of the cases can, for an instant, question the essential identity of these fevers?

I cannot, indeed, resist the conviction that, ere long, the essential identity of fever in all countries will be generally admitted

by the profession: and my chief regret is, that the present advocacy of the doctrine, and the opportunities which I have enjoyed of verifying its absolute truth, had not fallen to the lot of some of the eminent men whom I have named, whose talents would have done justice to its importance, and whose well-established reputation would have at once secured that attention to which questions so interesting to science, and of such high practical import, are so pre-eminently entitled.

I would finally appeal to the undoubted specific influence of quinine, (when opportunely and adequately administered), in controlling the fevers of tropical climates, and to its equal efficacy in controlling the fevers of this country, as has been fully proved by the cases treated at the Liverpool Northern Hospital, the Liverpool Fever Hospital, St. Thomas' Hospital, London, and elsewhere. I would ask whe-

ther, irrespective of all other evidence, the specific power exercised by proper doses of quinine, over all these several forms of fever, does not afford conclusive proof that, in their essential nature, these fevers are identical, and differ only in form and degree?

LECTURE I.

ADVANTAGES OF EXTENDED OBSERVATION IN THE STUDY OF DISEASE.—GOUTY DIATHESIS, THE PRECURSOR OF DECAY IN EUROPEANS RETURNING FROM THE TROPICS.—GOUT RARE IN BRAZIL.—CASES OBSERVED THERE BY THE AUTHOR.—EXPLANATION OF THE FREEDOM FROM GOUT.—HISTORY OF EUROPEAN RESIDENTS AFTER THEIR RETURN FROM TROPICAL CLIMATES: THEIR PREMATURE DECAY.—VICARIOUS ACTION OF THE SKIN AND KIDNEYS, EXPLANATORY OF THE “BREAK UP.”—ANALYSIS OF POST-MORTEM EXAMINATIONS OF THE KIDNEYS OF RESIDENTS IN BRAZIL.—KIDNEYS GENERALLY ATROPHIED.—ACTUAL DISEASE OF THESE ORGANS RARE.—PRACTICAL DEDUCTIONS.—PERNICIOUS INFLUENCE OF THE MODE OF LIFE IN THE HIGHER RANKS OF ENGLISH SOCIETY.

GENTLEMEN,—As we gradually emerge from the narrow and contracted sphere of observation afforded by our native country, our views of men and things become more comprehensive and philosophic; and

the petty theories of provincial or national growth assume the form of generalisations, more or less extensive according to the range of the opportunities which we have enjoyed. In this manner, facts, at first local, and, as it were, isolated, become the source and basis of general principles; and the mind, satisfied at the commencement with the simple enunciation of truths, gradually rises to the formation of general abstractions, and to the consolidation of the elements of its intellectual stores. Thus, from the simple consideration of a plant, or of a rock, we gradually and insensibly ascend to that most wide and ample field, embracing the geographical distribution of plants, or to the more complex but not less interesting philosophy of the physical organization of the globe, comprehended in the expansive science of geology. The same holds with regard to all the various departments of

knowledge; and Medicine, which has, from the earliest periods, commanded the attention and consideration of the most philosophic minds, in like manner now assumes, in the circle of the sciences, that eminent position to which it has so many undoubted claims.

Diseases, not unlike plants and animals, have all their special climes and localities; and though their geographical distribution has not certainly been the object of that special consideration which it so justly deserves, it presents a field for investigation fraught with the highest interest to society, and which gives fair promise of an ample reward to those who would enter on its cultivation. Inquiry of this kind would reflect great and unerring light on pathology, and would, in all probability, connect and consolidate many of those floating axioms which at present—detached and separate—seem only to wait the grouping

hand of a master, to mould them into generalisations of the highest importance.

Though the field of observation on this subject which I have enjoyed, both in the public service and in private life, extends over various portions of the globe, yet I still feel its range not sufficiently comprehensive to justify me in employing that precision of description which the subject seems so particularly to claim, and without which, we must admit, it would be deprived of all its interest. The formidable maladies, however, to which Europeans are subject, apparently from change to the colder latitudes, on returning to their native land, after a more or less protracted sojourn in tropical climates, have been brought before my immediate observation under various aspects, and in a mode at once most momentous and alarming. These diseases, at first sight apparently anomalous, give rise to considerations of the deepest

interest; they admit, however, I believe, of clear elucidation, not only with regard to their essential nature, but to the method of treatment—questions which, I fear, have hitherto been too generally misunderstood, so that the patient has been subjected to a mere routine discipline, totally inadequate to meet the true pathology of the diseased action, and too often calculated to precipitate rather than to prevent a fatal issue.

With these remarks, I beg leave to submit to your consideration certain anomalous and hitherto undescribed modes of development of the GOUTY DIATHESIS, the immediate precursors of a general decay of the system in Europeans, on their return to the colder latitudes, after lengthened residence in the tropics. Although, on the present occasion, my remarks, with regard to tropical influence, will refer more directly to the high latitudes of Brazil, yet they are

by no means limited to that country, but embrace the observations of disease in many other countries, European and American.

GOUT—ITS RELATIONS TO TROPICAL CLIMATES :—THEIR CONSTITUTIONAL INFLUENCE.

Gout, though occasionally met with, is a disease of rare occurrence among the native Brazilians, and is almost equally rare in the foreign resident. This exemption is the more remarkable, as the habits and modes of living prevalent in the higher ranks of society, and to some extent among foreigners, would lead directly to the conclusion, that gout must be necessarily a very frequent disease. For example, the richer classes in Brazil, almost without exception, lead a life of ease and indolence. The mind and body are unexercised; the sensual passions are indulged; and, although not generally addicted to wine, the Brazilians partake often and largely of animal food,

and of a great variety of hot and highly seasoned dishes. Dyspeptic affections are also not uncommon. Here, then, we have a combination, to an unusual extent, of those conditions universally admitted as the most influential in determining the gouty diathesis in Europe; and yet the disease is absent, or exceedingly rare. Nevertheless, three of the severest cases of gout which I have ever witnessed occurred in Bahia. One of the patients was a Portuguese merchant, in whom the disease was caused by great moral distress: the other two were native Brazilians. All three exhibited unequivocal evidence of renal derangement, together with diminished cutaneous secretion—a state of the economy sufficient, with the concurrence of other influences, satisfactorily to account for the invasion and gravity of the disease. As one of these cases is instructive, I will now briefly relate it.

CASE I. Senhor P. R. B., president of the National Bank, and one of the richest proprietors in Brazil, aged about 50, of indolent habits, and plethoric constitution, and much addicted to the pleasures of the table, though he never drank wine, had been subject for some years to occasional slight attacks of gout in the ball of the great toe. During one of these invasions, somewhat more severe than ordinary, the inflammation suddenly disappeared from the foot during sleep, and he awoke with great oppression of breathing, difficult decubitus, and slight cough, but no expectoration. A consultation was immediately summoned, consisting of Drs. Avelino, Franca, Lino, and myself. In addition to the above-mentioned symptoms, we found the patient labouring under great anxiety and restlessness; the surface and extremities cold; the pulse quick, small, and hard. There was slight dulness on percussion all over

the chest, but no râles; and the sounds of the heart were clear and distinct. The inflammation had entirely left the toe. Such were the leading appearances. The consultants differed *toto cælo* in opinion, and were equally divided: two advocated active local and general depletion, two the administration of stimulants, and the application of blisters or sinapisms to the feet. Contrary to the ordinary custom in such contingencies in Brazil, of calling in an additional physician, the patient himself decided the question by adopting the latter opinion. Stimulants were therefore administered internally, and hot stimulating baths to the feet, followed by powerful sinapisms and purgative enemata. The result was conclusive and instructive: in about eight hours, the inflammation returned with great intensity to the toe and instep, while the pectoral and other formidable symptoms vanished, as it were

by magic. This was the only case I ever witnessed of a similar character.

About the same time, another case of gout presented itself, interesting in several points of view to the subject under consideration, and which I shall detail at greater length.

CASE II. Mr. P., British Consul for Bahia, aged about 45, of spare constitution, of studious and abstemious habits, and indisposed to active exercise, was subject to hereditary gout, and had the gouty diathesis strongly expressed. In Europe, he had for many years suffered from periodical and regular attacks of tonic gout, which, subsequently to his residence in Brazil, became gradually less regular and less severe, and, during the last few years of his residence at Bahia, afforded an excellent illustration of the chronic gout of authors. At length, after a residence of upwards of seven years, his constitution was evidently impaired by

the climate, and the disease became suddenly transferred to the stomach. In addition to the usual symptoms, he had nausea, and vomiting, which no treatment could in the slightest degree allay. Nothing could be retained for an instant; and for twenty-one days the constitution was supported solely by the repeated administration of nutritive enemata. He began, however, rapidly to sink; the pulse fell to 40, being weak and irregular; the voice was lost, so that he could be understood with difficulty, and only by holding the ear close to his mouth. At this critical moment, when I had almost abandoned hope, a French frigate, the *Cleopatra*, arrived at Bahia, on her way to Brest. Notwithstanding Mr. P.'s exhausted condition, I decided on immediate removal, as affording him the last and only chance for life. Monsieur le Capitain politely offered every accommodation; preparations were instantly made,

and, at a few hours' notice, accompanied by his daughter, Mr. P. was carried in a cot on board the frigate, with the most melancholy forebodings that we had parted for ever from a gentleman who, both in his official and in his private capacity, had conciliated the respect and esteem, not only of his own countrymen, but of the whole Brazilian community. Scarcely, however, had the frigate cleared the land—that is, within the space of a few hours—when Mr. P.'s stomach became calmer, and soon retained minute quantities of nourishment; and, on the seventh day, prior to passing the equator, he was seated at a large dinner party, and in the enjoyment of as good health as he had ever possessed in his life. I may add, that Mr. P. was afterwards nominated H. M. Consul-General at Rio de Janeiro, where he resided for some years. Subsequently, after an interval of twenty years, I again attended him, now retired from office, (and

in his seventy-sixth year), for an attack of regular tonic gout in the extremities, such as he had formerly experienced when young, and prior to his removal to Bahia in 1817.

This case I consider valuable to the tropical practitioner in several points of view; and especially interesting, as proving the influence of high temperature on the European constitution, in modifying the manifestations of gout under circumstances similar to those of Mr. P., and as exhibiting the influence of a temperate climate in again restoring the economy to that condition which favours a due development of the disease in the extremities. I would further observe that, although the influence of a hot climate will prove unquestionably great in preventing the acquirement of gout, yet, from the observation of other cases somewhat similar to that of Mr. P., I much question the advantage of a *lengthened* tro-

pical residence to those individuals in whom *hereditary* gout is already strongly developed, or after the constitution has been seriously deteriorated by its effects. This opinion is supported, I apprehend, by sound reasoning *à priori*, as well as by the evidence furnished by experience.

CAUSES OF EXEMPTION FROM GOUT. The general freedom from gout enjoyed in hot climates is a well-established fact, and is accounted for by physicians, by assuming for the inhabitants of these countries the exercise of more temperate habits, and the use of a less highly azotised food than are usual among the natives of colder latitudes. Now, this hypothesis is clearly inapplicable to Brazil; and has, perhaps, been too lightly adopted in reference to the upper ranks of society in other hot climates—certainly as regards the inhabitants of those countries, where, as in South America, the higher classes are more immediately of European

descent. We must consequently seek some other solution.

It will be readily conceded, that a diet conjoined with social and moral habits, similar to those prevalent in Brazil, would lead generally in Europe to the development of some form or other of regular or irregular gout. And admitting the received theory—which I believe essentially correct—that an excess of certain principles in the circulating and secreted fluids of the body constitutes the chief pre-disposing, as well as the immediate or exciting, cause of the disease, I think we may perceive rational grounds for this comparative immunity of the tropical resident from gout.

In examining this subject, we must take into consideration the abundant and unceasing elimination of the principles alluded to, by the capillary vessels, in all tropical climates. We must remember, also, that the cutaneous system performs infinitely

more important functions in the higher than in the lower latitudes; removing, through the perspiratory secretion, considerable quantities of animal matter, and especially very sensible amounts of the lactic and uric acids. Tophaceous deposits I have scarcely ever met with. It will thus be sufficiently apparent that the economy, under such circumstances, will be more efficiently relieved from those effete or deleterious principles which, carried into and retained in the general circulation, would constitute a real *materies morbi*; and which, by first impairing the organic nervous power, would induce disorder, or rather increase that already established in the secreting, excreting, and digestive systems. From the disturbance of the functions of these systems, plethora and increased vascular irritability would commonly result; and would give rise, sooner or later, according to idiosyn-

crasy, constitution, and predisposition, to all the varied manifestations of tonic or anomalous gout.

I am satisfied that to the operation of the above-mentioned agencies, and not to the influence of more abstemious habits, or to the habitual use of a less azotised diet, do the inhabitants of intertropical climates owe their general exemption from gout. This view of the case is moreover strongly corroborated by the instructive and too often painful history of that numerous class of individuals of both sexes who return yearly to this country, after lengthened residence in our tropical and colonial dependencies. These individuals, in many instances, *break up*, as it is popularly termed, after a brief sojourn in their native land—a happiness to which they had for years looked forward with all the fond and warm aspirations of youthful remembrance. How melancholy the dissipation of their

dream! As a type of the class, let me briefly sketch an individual—mercantile, military, political, or professional—one, too, under favourable circumstances, whose worldly career has been successful, and who has happily escaped from serious organic disease. After a residence of many years abroad, in the discharge of active and important duties, he returns at length to Europe. At a period of life when new associations are painfully admitted, he is at once removed from the scene of his active and useful career, to find himself a stranger in his native land, and commonly without occupation or employment. With habits and modes of thinking greatly modified by position, and suddenly deprived of all his ordinary pursuits, and of the consideration and influence to which he had been previously accustomed, he is absorbed into the mighty masses of the metropolis, or mingles in the large com-

munities of our overgrown commercial cities. He loses in some measure his identity. By one supported by the healthful energies of youth, these depressing and injurious influences might be resisted; but his prime of life is already passed away, his health is shaken by climate, and the illusions of youth are gone by. Under such circumstances, can we wonder that the constitution should succumb to the first and slightest assault? Succumb, indeed, it usually does; and in a large majority of cases, the *break-up*, as it is aptly enough termed, will be ushered in by some form, more or less distinct, of atonic or anomalous gout, or of rheumatic gout. Now, as no indication of these affections had ever before been apparent in the individual, it becomes a question of much scientific interest, and of great practical moment, to ascertain not only the essential nature of the symptoms, but the exact pathological condition on which they depend.

In the vast majority of cases, the following explanation, though brief, will, I believe, be found correct. In all hot climates, the secretion from the external capillary system is incessant and profuse, while the secretion from the kidneys is in an equal ratio diminished, their functions being, in fact, vicariously and energetically performed by the skin. The urine is consequently scanty in quantity, and acrid in quality, from a deficiency of its watery constituents; for example, often not more than a few ounces of high-coloured urine will be passed during the twenty-four hours, although *gallons* of liquid ingesta will have been taken during the same period. This order of things is commonly maintained for a long series of years. Eventually, the individual returns to the colder latitudes of the north, with his digestive powers seriously impaired, and his capillary system weakened by long continued over-excitement. The accustomed

profuse perspiration is suddenly arrested, and the renal system is as suddenly called on to resume its long dormant functions; but it is too late. The kidneys, from prolonged inaction, have suffered, if not organic disease, commonly an amount of functional change sufficient to interfere with the performance of their now all-important duties, and the most formidable consequences necessarily ensue. Gout, simple or complicated with rheumatism, appears in all its various forms, disposing to apoplexy, paralysis, disease of the genito-urinary system, functional and organic cardiac disease, or anomalous and distressing derangements of the digestive and assimilative functions, with their numberless train of diseased moral and physical sympathies.

It might be here expected that I should specifically enumerate, in detail, those symptoms which characterise each affection; but they differ in no material point from the

signs by which they are revealed under their ordinary conditions; and should they immediately threaten life, they must be met by the ordinary energetic measures. The grand difference is the nature of the cause or causes in which they originate, and which involves the still more important question, the principles of the preventive and curative treatment. In no instance does the Baconian axiom, "*Verè scire est per causas scire*," so exactly apply as in the present. This axiom, the routine practice and classic specifics of the present era would seem completely to have discarded.

Is the above description, then, to be considered an overdrawn or theoretical view of an extensive and important question? It is neither; the sketch is drawn from life, and confirmed by long, and large, and often sad experience. It is, indeed, melancholy to witness the extent to which these cases are

frequently misunderstood by a numerous class of the profession in this country. The blue pills, the alkalies, the salines, the tonics, and the purges, with which the unhappy victim is tormented, whilst the essential nature of his malady is either overlooked or unsuspected, all testify to this truth. The liver, sometimes the stomach, the spleen, the whole associated chylopoietic viscera, are in turn condemned, and assailed with the formidable and multitudinous batteries of the Pharmacopœia and Materia Medica. How judiciously this is done in some cases is satisfactorily shown by perusing some of the prescriptions of fashionable and well-employed practitioners.

The effect, indeed, of lengthened tropical residence on the structure of the kidneys in the European, I have verified by *post mortem* examinations, of which the chief results may be here briefly enumerated.

In five cases out of nine, after a residence of from sixteen to nineteen years, I found atrophy, in a greater or less degree, to have occurred in both kidneys; in one case, one kidney only was affected. The other three cases showed no evidence of change. In four, the organs were decidedly firmer than natural, but the intimate structure was apparently normal: one was softer and more flaccid, and the remaining four presented no appreciable change from the healthy consistence. In none had the *form* of the organs undergone any change. In two of the cases, the nerves of the kidneys appeared decidedly below the usual size. No obvious change from the ordinary size or condition could be detected in the blood-vessels or in the ureters. In all the nine cases, the cortical substance was decidedly paler than natural; and, in those atrophied, the change was limited to this tissue, the tubular cones being, if any thing, more distinct and more

injected than is usual in the healthy kidney of the European. The lining membrane of the calices and pelvis, in the majority of the cases, seemed also rather more deeply coloured.

In four of the above cases, the appearance of the kidneys was contrasted, by means of a longitudinal section, with that presented by the renal organs of four seamen, who had died from acute diseases in the British Hospital, under my care, and who had been but a few weeks from Europe. The age of these men (29 to 46), their stature, the character and duration of their maladies, and the amount of emaciation, differed in no very important degree from the four cases with which they were immediately contrasted. In three of the European cases (as we may term them) the kidneys ranged respectively in weight 191, 235, and 286 grains above the cases with which they were compared. In one the weights were nearly equal.

It must be borne in mind, that none of the above-mentioned nine cases had ever manifested, so far as I could ascertain, any symptoms of urinary disorder during life, and that all died from diseases totally unconnected with the renal system; viz., two from accidents, one from pneumonia, one from compound fracture of both lower extremities, one from acute bronchitis, one from apoplexy, two from dysentery, and one from rupture of the aorta. The duration of the diseases ranged from one to twenty-three days; the patients were all males, from thirty-four to fifty-six years of age.

In making my calculations, I adopted the standard of four ounces as the weight of the kidney in the healthy man, of middle age, and of ordinary stature, although the results of my own experience in Brazil would fix the average standard lower, while the results of a series of observations con-

ducted in the Hospital, during the last few months, at my request, by our intelligent house surgeons, Messrs. Wall and Evans, would show the average weight of the healthy kidney to be decidedly above four ounces. The difference in weight from the standard of four ounces I found to range from 415 grains, the maximum, to 150 grains, the minimum. The former, or maximum difference, was found in the apoplectic patient, aged 51, robust, about 5 feet 9 inches in height, and had been resident in the country for eighteen years: the latter, or minimum difference, occurred in the case of pneumonia, the subject of which was 47 years of age, of about the middle size, emaciated, and resident in the country about fifteen years; he had been ill nineteen days.

I am fully convinced, (and was much embarrassed by the fact,) of the difficulties inherent in all such calculations. In four

of the cases, however, as already stated, I had the important advantage of direct comparison; and in all, I weighed, most accurately at the time, every source of fallacy arising from difference in age, stature, emaciation, disease, &c., and made every allowance that reason could suggest. I therefore feel justified in believing that my calculations are sufficiently exact to bear out my inferences. One point is here worthy of remark, and which does not admit of very satisfactory explanation—in the kidney of the native I have not observed the same tendency to atrophy. Naturalisation will, no doubt, go for something.

So far, then, as these cases go, we may fairly consider the appearances presented as pretty accurately indicating the condition of the kidney, in a large proportion of those Europeans who have long resided in high latitudes; and as affording strong corroborative evidence in support, not only of

the preceding observations as applied to the tropical resident after his return from Europe, but also of the proposition, that the functions of the kidneys are much less energetic in hot than in cold climates; that prolonged diminished function is commonly followed by more or less wasting of the organs themselves, and that, consequently, on the return of the individual to a northern climate, the renal system will in many instances be found incompetent—in some permanently, in others temporarily—to resume its full and healthy action, so that the most disastrous results to the economy must ensue. This investigation claims, and will, I doubt not, ere long receive from the profession, that searching scrutiny to which its practical importance so obviously entitles it.

With reference to actual diseases of the kidney in hot climates—for the above-mentioned condition can scarcely be denomi-

nated disease—I may state that they are of much less frequent occurrence, according to my own experience, than diseases of any of the other important organs of the body. I have, however, occasionally met with the granular kidney of Bright, and also, though rarely and at long intervals, with other of the ordinary affections of those organs.

PRACTICAL DEDUCTIONS.—From the foregoing remarks, the experienced practitioner will readily comprehend the chief questions to be investigated, and the chief indications to be fulfilled, as well as the most appropriate and efficient means of attaining them. I would not of course be understood to counsel inattention to any important organ or system: none, indeed, can be safely neglected, as none can be seriously implicated, without soon involving others. The hepatic system will, no doubt, demand especial care, being, from the nature of its

functions, peculiarly obnoxious to disease in hot climates, where it probably performs vicarious or auxiliary functions with reference to the lungs, though certainly not to the same extent as the cutaneous does to the renal system.

In those persons who return to Europe after a long tropical residence, I would, therefore, above every other consideration, have the attention of the practitioner fixed on the state of the skin and kidneys; for I am satisfied that, unless he succeed in restoring those important systems to the active and healthy exercise of their functions, all efforts to benefit his patient, howsoever skilfully directed, will prove either unsuccessful or pernicious. Hitherto, indeed, attention has been too prominently fixed on hepatic derangement, to the comparative exclusion of disorders of the renal system, which have never yet received from the profession that degree of

consideration in the tropical invalid to which their paramount influence on the economy in such subjects so justly entitles them.

As an efficient means towards the restoration of the cutaneous and renal secretions, the importance of which has already been insisted on, I deem due and active exercise, mental and corporeal, to be of the highest importance. I am, moreover, satisfied that without this, and without strict attention to diet and regimen, all remedial measures will prove utterly valueless. The mental occupation must, of course, be appropriate to the tastes and habits in each individual case; but the bodily exercise will allow of less variety. The muscular and vascular systems, and the different secretions and excretions of the body, must be excited and maintained, not by the fashionable drive, or morning lounge, or afternoon airing on horseback; but by a well-devised plan of

active and long-sustained exercise, suited at first to the condition of the patient, but gradually and steadily increased from day to day, and from week to week, as the strength of the individual will permit. This system may prove painful at the commencement; but, by being persisted in, it will soon become less irksome, and, at last, agreeable. At all events, it must be borne; for, if it be abandoned, the patient must be prepared, at the same time, to relinquish all rational hope of permanent improvement. But the minutiae of the treatment, along with several other important matters, must be deferred to our next meeting.

PERNICIOUS INFLUENCE OF HABITS OF LIFE
IN CERTAIN RANKS IN ENGLAND.

As we have still a few minutes remaining, I cannot, perhaps, more usefully occupy them, than by calling your attention to the

pernicious influence on health of the ordinary habits of life in certain ranks of English society. Although my remarks are partly prompted by the circumstances which immediately surround us, and which, therefore, are of daily and hourly interest to us all, yet they are still more closely linked with the subjects of our present investigation. However injurious these modes of life may *eventually* prove to all classes, they are doubly and at once pernicious to those individuals who have returned from lengthened residence in hot climates, and who, almost of necessity, on their arrival in this country, fall under the control of the social customs incident to that rank of society to which they belong. In illustration of the principle, I select the four classes with whom you will ordinarily come in contact, as represented by the Church, Law, Medicine, and the Exchange.

The cares and duties attached to the dif-

ferent professions, and to the counting-house, are well known to be attended with great mental and bodily exhaustion. Often after the labours of the day in his office, and impressed, probably, with the importance of exercise to the preservation of health, the individual will return to his residence, a distance possibly of two or three miles, on foot, and thus completes the general prostration. He will then sit down, perhaps at seven o'clock, with his nervous power exhausted, to a heavy and luxurious dinner—soup, fish, and variety of meat, puddings, pastry, and dessert! His stomach, as debilitated as his body, is soon oppressed; stimulants are indispensable, and are freely supplied in the strong-bodied and often brandied wines of Spain and Portugal. Tea and coffee succeed, with the too frequent superaddition of close and heated rooms, and the excitement attendant on the discussion of local and general politics; followed by féverish

nights, disturbed sleep, and nervous exhaustion on the ensuing day. Can any constitution, I ask, however happily balanced by nature, long resist with impunity a mode of existence so unreasonable and pernicious? It is impossible. Premature death, or premature decay, from gout, paralysis, disease of the heart, or urinary diseases, with a numerous host of associated maladies, will sooner or later inevitably ensue. Let us, now, therefore, enquire by what measures these formidable evils are to be prevented or removed; and here, again, my reply shall be brief and conclusive.

The individual must altogether abandon some, and modify *all*, the habits of life above enumerated. A calm and philosophic temper of mind must, under every difficulty, be habitually cultivated. The physician, indeed, on urging the point, will be constantly met by, "Oh, Doctor, your recommendation is excellent in theory, but

it cannot be practised." To this I have invariably replied,—“You are mistaken, it can be practised—aye, and much more easily and effectually than you suppose; but it will require time, and perseverance, and firmness of purpose.” And the results of a pretty extensive experience have fully proved to me the practicability and importance of the recommendation. If much exhausted, mentally or bodily, by his daily avocations, the individual must avoid adding to his exhaustion by additional exercise, and should therefore return to his home in a carriage, or on horseback. On arrival, he should rest quietly on a sofa for twenty minutes, or half-an-hour. Dinner should not, if possible, be later than half-past five o'clock; it should consist of not more than one quality of meat, dressed according to taste, and one kind of well-cooked vegetables, with stale bread; there should be no second course, no puddings, pastry, cheese,

or dessert. Repletion, in every sense, must be carefully avoided. Three or four glasses of light French wine (claret is the best) may be taken during the meal; and one or two glasses of old Madeira, port, or sherry, immediately after dinner. The individual should then enjoy mental and bodily rest; and during the evening he may take a cup of weak tea, with bread and butter if required. The rooms should be moderately heated. He should have the company of an intelligent friend, rather than the excitement of a numerous society; and, what should never be forgotten, conversation in preference to argument. He should retire to bed at half-past ten o'clock.

Such, Gentlemen, is the only safe course to be pursued by the over-wrought and exhausted individual of a certain class in this country; and more especially after he has passed his fiftieth year. At an earlier age, or in robust health, and in the enjoy-

ment of ease and leisure, greater latitude may, no doubt, be permitted; but I would observe, that the man who, in early life, will adopt, as his standard of living, animal food in moderation, light French wines, sufficient mental and bodily exercise, with early hours (and these rules imply no privation), will escape a numberless train of physical evils, erroneously regarded as incident to humanity.

In immediate reference to our present subject, it is no less singular than painful to contemplate the influence exerted by fashion on English society as at present constituted. From a host of examples, let us take a single one, and a most absurd and baleful one it is; viz., the present aristocratic dinner hour, from half-past seven to eight o'clock. The vain and thoughtless fashionable idler, whose chief object in life is to "kill time," and gratify his animal wants, who breakfasts at one,

and lounges about town, or at his club, till four o'clock, rides till six, and spends two hours more in the adornment of his person—this man may, indeed, gorge himself at eight o'clock; and the whole powers of his system, (as in the lower animals,) being concentrated in his stomach, he sustains and digests the oppressive load with comparative impunity. Nor, unhappily, are the habits of this specimen of the *bipes implumis* the growth only of our own age, as the following lines of one of our most estimable poets sufficiently attest:—

“ To rise at noon, sit slip-shod and undressed,
To read the news, or fiddle, as seems best,
Till half the world comes rattling at his door,
To fill the dull vacuity till four ;
And, just when evening turns the blue vault grey,
To spend two hours in dressing for the day ;
To make the sun a bauble without use ;
Quite to forget, or deem it worth no thought,
Who bids him shine, or if he shine or not.”

Is this, I ask, the man to dictate social,

or any laws, to the intellectual and overwrought lawyer; the intelligent and enterprising merchant; the zealous and indefatigable clergyman; the harassed and anxious physician? Surely not. Common sense, if not higher principles, must at once reject the proposition; and I cannot abandon the hope that "common sense" will yet exert its power, seldom altogether in abeyance, and consign to oblivion, if not to contempt, these absurd and injurious ordinances of a fashionable inanity—these refined barbarities of ultra-civilization.

LECTURE II.

RECAPITULATION OF REMARKS ON THE BREAK-UP OF
EUROPEAN RESIDENTS RETURNING FROM TROPICAL
CLIMATES—TREATMENT—HYGIENIC RULES—EXER-
CISE—CLOTHING—DIET—WINE—MEDICINAL TREAT-
MENT—EMETICS—ALTERATIVES—DIURETICS—TONICS
—MERCURY MORE LIKELY TO BE INJURIOUS THAN
USEFUL—BATHS—CHANGE OF SCENE AND CLIMATE
—HYDROPATHIC TREATMENT—EUROPEAN FEMALES,
WHY SOONER AFFECTED—PERIOD WHICH ELAPSES BE-
FORE EUROPEAN RESIDENTS ARE AFFECTED—INFLU-
ENCE OF SOLAR LIGHT AND HEAT—FALLACY OF DOC-
TRINE OF ACCLIMATISATION—INDICATIONS AFFORDED
BY URINE AS TO PERIOD WHEN EUROPEANS OUGHT TO
RETURN—IMPORTANCE OF CHANGE OF CLIMATE.

GENTLEMEN,—Keeping in mind the three-
fold character of the causes which especially
influence the condition of the animal body,
in bringing about the premature breaking
up of the system incident to individuals

who have returned from a long residence in warm climates, viz., the exhausted capillary system, the weakened or atrophied condition of the renal organs, and the powerful moral influences which exert their silent but powerful sway in increasing the general evil,—it must be sufficiently apparent that the objects of the practitioner will be better accomplished by duly attending to diet and regimen, and to the functions of the kidneys, than by any special line of treatment to restore the fancied impaired condition of the nervous and hepatic system, or the energies of the digestive organs. All of these may, no doubt, be involved in the general complications, but less unquestionably as cause than as effect, and as symptomatic of the disordered state of the renal and capillary systems.

I have already stated, in the preceding lecture, that this premature “break-up” is commonly ushered in by some form of gout

or rheumatic gout, predisposing the individual to numerous dissimilar and formidable maladies; and that the symptoms which characterise these diseases differ in no essential feature from those by which they are revealed under ordinary conditions—the grand discriminating point being the nature of the cause, and the circumstances under which the diseases arise. To these considerations I have already fully directed your attention. In proceeding to discuss the question of treatment, my chief object will be to lay down sound general principles, as I am satisfied that you can be little instructed by the details of the management of particular cases; and that, although your success as practitioners will depend on the judgment, discrimination, and tact, which you may be able to exercise under the varying circumstances of each individual patient committed to your care, yet these can only be put in force by those who

are familiar with great general principles, and who have obtained a power of applying them—the result of experience.

I must then crave a short indulgence of my senior auditors, while I offer a few brief remarks on the hygienic rules which I have found useful in such cases, and which may, in some measure, serve to guide my younger friends, at the commencement of their professional career.

HYGIENIC RULES.

EXERCISE.—There are few conditions of the animal body in which exercise is so important, I might almost say paramount, as in the condition now described. But the state of the individual implies a certain amount of inactivity and indifference to exercise, which is often surmounted with difficulty, and which it is the duty of the physician to have completely under his control. Of exercise there are various kinds,

each possessing its peculiar advantages, each adapted to special classes of individuals, and each specially intended to fulfil certain ends. Walking possesses many advantages over horse or carriage exercise, though every mode of walking is not equally efficient. One may saunter, or stroll, and meditate in the progress of his walk, and thus favour rather than check the tendency to obesity, to which walking is commonly so adverse; the individual entirely deceiving himself as to the amount of influence of his daily exercise. The observation is, on the whole, sound, that when there is no valid impediment, exercise for the benefit of health should never be deemed efficient unless it induce a slight degree of perspiration, but always within the limits of fatigue. As the subjects in question are usually past the meridian of life, or tending thereto, a great amount of foot-exercise can scarcely be enforced without risk of in-

ducing a tendency to the head. Certain restrictions, however, being attended to, there need be little apprehension. The walk ought to be quick and active, and should range between five and six miles. This exercise will be most efficacious if associated with riding, which has the peculiar advantages of not causing any tendency to the head, of equalising the circulation, and of favourably exciting the animal spirits. Horse-exercise is the best substitute for walking; but combine them if you can. Carriage-exercise is a misnomer, and altogether, or nearly, a downright imposition on all parties, physician, patient, and friends. It does not call one single muscle into active play, and is equally powerless over that commanding portion of the nervous system—the centre of volition—to which so much vigour is imparted by the former exercises. To the weak and broken-down invalid, advantage will no doubt

accrue from exposure to the open air in a carriage; but he belongs to a different class of individuals from those to whom I am at present referring.

Under all circumstances, the indolent tendency of the individual must be broken through as speedily as possible, and the accumulated excitability of the system must be diffused throughout all the tissues of animal and of organic life; so as to counteract, to some extent, the powerful influence of those organic or functional derangements and moral causes, which threaten to sap the vital energies of the whole man.

CLOTHING.—This should be light, loose, and warm. Merino or fleecy hosiery should be worn next the skin during the day, but never slept in.

DIET.—The diet is always a subject of the utmost import to a person labouring under chronic illness; but it demands especial attention in the diseased conditions now

under consideration. For, inasmuch as the kidneys are the great depurators of the azotised matters of the blood, and as our pathological investigations lead to the conclusion that the action of these organs is weakened, it follows that one important indication of treatment is to diminish the ordinary demand on the already enfeebled function of these glands. They must be permitted gradually to recover their power, while the functions of the vessels of the skin are brought, as near as circumstances will allow, to the healthy standard of the climate; and although theoretical objections may be urged, with regard to the source from which azotised products are furnished to the blood—whether from primary or secondary assimilation—experience has fully satisfied me of the practical value of this rule. I deem it of importance that a regular system of diet, partly based on this principle, should be enforced and carried

out. I would moreover observe, that, under all circumstances, the character of the urine demands special attention, as affording information of high import with reference to assimilation and nutrition. If, within three or four hours after the ingestion of food, the urine when passed, or immediately on cooling, should appear cloudy or turbid, we may with certainty conclude that it contains an excess of the urates or phosphates—the result of imperfect chylicification. We should, then, not delay a moment in moderating the quantity or modifying the quality of the ingesta, and thus prevent the economy from being prostrated by its own diseased products—a condition most injurious under all circumstances, but especially dangerous in the class of cases now under review, where the renal organs, unequal to the efficient performance even of their ordinary functions, are still less able to sustain the additional labour demanded for the elimination

of those abnormal salts and other morbid products of imperfect or diseased digestion. This state of things cannot be long maintained with impunity to the kidneys; and from it, I am persuaded, many and grave diseases of these organs date their pathological origin. To this question I may possibly recur at some future day.

The following order might be advantageously pursued in directing the necessary hygienic arrangements of the day. The invalid should rise at seven o'clock in the summer, and at eight in the winter. A shower-bath should be taken immediately on getting out of bed, provided there be no special contraindication: the cold shower-bath will, generally, prove most beneficial, but the tepid may be substituted when the system has not strength to sustain the cold. To either, a portion of salt may occasionally be added with advantage. The body should be vigorously rubbed with coarse towels or

a soft flesh-brush, for some minutes after coming out of the bath, so that an agreeable glow of warmth may be diffused throughout the system. The invalid, warmly clad, may now drink a glass of pure spring water, if the stomach agrees with it, after which, in favourable weather, he ought to take a brisk walk for half-an-hour before breakfast. This meal may consist of a little broiled bacon, with a cup of weak tea (with good milk), or cocoa, along with stale bread and fresh butter. Exercise on foot should again be taken for about an hour before luncheon, for which a biscuit and glass of water, a plate of rice soup, or some light farinaceous pudding, will suffice. Between this and dinner, exercise should be taken for about an hour and a half, and, if possible, on horseback. At dinner, one variety of animal food should be permitted, with one dish of well-cooked vegetables: the quantity of

animal food must also be moderate, and, when the state of the individual is at all precarious, fish or game only ought to be allowed, and the amount carefully regulated. Puddings, pastry, and dessert, are totally inadmissible.

It should ever be borne in mind, that nothing can be more irrational than loading the stomach of an individual, debilitated by climate or disease, with *substantial food*, under the idea of nourishing him; or more fallacious than the idea that we can really improve his strength by any temporary excitement of the nervous or vascular systems, by the administration of wine, or other diffusible stimulants. In this country, the ultra-disciples of the Abernethy school have done infinite mischief, in numerous instances, by their system of "dry feeding." In certain conditions, it will no doubt be wise to caution an invalid against deluging the stomach during, or immediately

after meals, with water or other fluids; but it should never be forgotten, that neither digestion, nor any other vital process, can be satisfactorily carried on without an abundant supply of pure water.

WINE.—I would altogether exclude the trash commonly served up, even at the tables of the wealthy, as “sound sherry;” but which, in truth, is perfectly innocent of the slightest connexion with the district of Xeres, being usually a mixture of Cape, Malaga, and Mediterranean vintages, to which are too often added nefarious compounds of home manufacture. Good sherry, one of the rarest of wines, may of course be allowed; but good Madeira, or old port, is equally beneficial, and sometimes more advantageous. The sherry *conceit* is, indeed, a mere fashionable whim, supported by partial and imperfect chemical views, in which the play of the vital affinities is completely excluded. My experience, which has not

been limited, has satisfied me that, where an acid tendency existed, it could be ascribed with more truth to other causes than the mere chemical character of the wines. Of all the wines, however, none equals Bordeaux of the best quality. I do not mean that extravagantly high priced wine served up at private tables, and the fashionable clubs, which is, in truth, like the "sound sherry" before alluded to, a product of the manipulations of the wine manufacturer, and exceedingly prejudicial to a weakened stomach. Good Bordeaux wine, though it may not agree with certain idiosyncrasies, is unquestionably the most healthful of all wines, and affords the greatest amount of beneficial stimulus with the least amount of injury to the human constitution. Moreover, with reference to gout, that malady is notoriously least prevalent in those countries where claret wine is the ordinary beverage. The notion that

the climate of England demands a stronger or more stimulating wine, I hold to be among certain popular fallacies, originating in prejudice rather than in reason, and which the introduction of sounder views in political economy, as well as in physic, will probably ere long dispel. In advocating the substitution of the lighter wines of France for the strong-bodied and heating wines of Spain and Portugal, we are affording another instance of the sacrifice of professional gain to the public welfare; as I am fully persuaded that, if this change were generally adopted by the higher classes in this country, the favourable results to their health and purses would far exceed the most sanguine expectations.

MEDICINAL TREATMENT.

I differ most essentially from the disciples of Abernethy and others, who have

assumed a royal road to health, curtly embodied in the eternal routine of blue pill and black draught, to which quack, apothecary and physician all run with one accord as to an universal remedy—a panacea for all the ills to which flesh is heir. This system is recklessly followed in every corner of the land, where nostrums are swallowed wholesale by the credulous public; and the real science of the pathology of disease is left to the meditations of the schools, or altogether abandoned, on the false plea urged by the empirics of old, “Nihil istas cogitationes ad medicinam pertinere, eo quoque disci posse, quòd qui diversa de his senserint, ad eandem tamen sanitatem homines perduxerint.” (*Celsus*, Liber I. Præfatio). I have faith in physic, but in physic of a rational description, and such as claims *time* for the cure of diseases which are the result of a long course of years, and which, in their primary

and secondary reactions, involve, directly or indirectly, almost all the functions of organic life.

The indications which it is our object to fulfil, are clear and explicit; and *time* enters into them as an important element. They are to be effected by *emetics*, *alteratives*, *mild tonics*, and *diuretics*, with *baths*, having special reference to the restoration of the renal and cutaneous secretions. Change of scene and climate will also frequently revive the dormant energies of the patient, and prove of the utmost importance.

EMETICS.—The real importance of this class of medicines, like the use of purgatives in France, has almost become a matter of tradition among physicians in this country. Their action, certainly, is much more extensive than is generally conceived by the profession. Though the stomach may seem, as it undoubtedly is in one sense of the word, the *focus* of action, yet this organ,

possessing powerful sympathies with all the tissues and functions of the body, exerts, from the stimulus imparted to it by the action of an emetic, a most powerful and controlling influence over the secreting organs generally, and particularly over the biliary secretion. It modifies the character of all, giving a tone of health to the whole animal economy; and, what is no less important, leaves the system more amenable to the influence of all other medicinal agencies. The beneficial influence of seasickness in dyspepsia is a well-established fact; and the benefit so generally ascribed to a sea-voyage springs, I believe, in no slight degree, from the sickness, and its influence over all the secretions connected with the chylopoietic viscera.

Of emetics, ipecacuanha amply fulfils all the indications required, as its action is not prolonged beyond a few hours, and its effects can be more safely estimated than

those of tartarised antimony. Its administration may be repeated after an interval of seven, fourteen, or twenty-one days, according to circumstances. After the operation of an emetic, a steady persistence in the use of taraxacum in large doses, either alone or combined with an alkaline carbonate, will prove eminently useful, by acting upon the renal and biliary secretions. From the use, for months, of the following form, I have derived very great and durable advantages:—

R. Liquoris Taraxaci,	℥viii.
Liquoris Potassæ,	℥ss. M.

Half an ounce to be taken twice a-day, in a glass of the real or artificial Vichy water.

ALTERATIVES.—Should the bowels prove torpid, and the excretions depraved, a combination of aloes and extract of guaiacum, with two grains of capsicum, and one of acetous extract of colchicum, will prove useful in improving the character of the

secretions, and restoring the action of the bowels. Or the following draught may be substituted:—

℞. Tincturæ Guaiaci Ammon.	℥xxx
Liquoris Taraxaci,	ʒi
Vini Colchici,	℥ x
Decocti Aloes Compos.	ʒ ii. M.

Fiat haustus bis in die sumendus.

When the skin is dry and harsh, the pulse slow, the extremities cold, the lips clammy, the urine turbid, the sleep unrefreshing, and the spirits oppressed, advantage will be derived from the administration of mistura guaiaci with liquor potassæ, and a few drops of vinum colchici, with five or six minims of liquor opii, followed by some bland mucilaginous drink.

Enemata of cold water will afford much relief in moving the lower bowel, and giving tone to the intestinal canal.

DIURETICS.—As a diuretic, and certainly not an inefficient assistant to the assimila-

tive process, the liquor potassæ, taken about an hour after dinner, in pure water, will be found of considerable service. Minute doses of the tincture of sesquichloride of iron, in cold water, will also be found useful where we wish not only to excite the kidneys, but to invigorate the system.

TONICS.—In cases where a tonic is required, the nitro-muriatic acid, or some slightly aromatic bitter, as the infusum aurantii compositum, may be taken with advantage, being changed according to circumstances. Small doses of the different preparations of iron, either alone or combined with quinine, will often be found highly useful.

I am satisfied, Gentlemen, from pretty extensive experience, that the above measures, singly or in combination, or slightly modified according to circumstances, will fulfil all the indications, as far as they can be fulfilled by medicines. I need scarcely point out to you, that the complexity of our

prescriptions offers but a sorry apology for our limited knowledge of the essential pathology of the disease. In these, as in all other maladies, the vast practical importance of the doctrine laid down by that distinguished physician, Dr. Latham, should never be lost sight of—that where great things are to be performed by medicine, they are to be effected by the right understanding of single indications, and by the right use of single remedies which have power to fulfil these indications.

With reference to *mercury*, so generally regarded almost in the light of a specific in all diseases arising from residence in tropical climates, I am decidedly of opinion that, although a dose of it may, occasionally, be given with much advantage, and may even sometimes be imperatively called for in such maladies, there is yet no class of cases in which its administration is worse borne, and attended by more pernicious conse-

quences, than in those persons who have returned to Europe after lengthened residence in warm climates. Rare are the exceptions to this rule; and I cannot too earnestly impress its great practical value on your minds. The indications commonly supposed to demand the administration of mercury will be found more advantageously and permanently fulfilled by a combination of aloes, soap, and taraxacum, with an occasional emetic, than by the use of any other drugs. The notion of aloes causing hæmorrhoids is altogether a popular fallacy, and cannot too soon be exploded.

- BATHS.—In persons affected with the deranged state of the system under consideration, we shall frequently find a want of sufficient vigour to support the powerful
- shock of the cold shower-bath; and yet it is of great importance that they should be subjected to its action. This will be accomplished by immersing them in the hot bath

at 98°, in which they should remain for five minutes; and while the patient is standing in the bath, let the cold shower-bath be at once administered. After this, they are to be vigorously rubbed for some time with coarse towels, or a flesh-brush; then being warmly clad, they should immediately proceed to take active exercise in the open air. In all cases, the bath-room should be properly heated—an important item in the administration of baths, too often, I fear, neglected. In many instances an occasional use of the hot air or vapour bath, immediately followed by a shower-bath—cold or tepid, according to circumstances—will prove a valuable element in restoring a healthy tone to the cutaneous circulation.

CHANGE OF SCENE AND CLIMATE.—Another important element in the treatment of all chronic diseases, and more especially of those now under consideration, is change of scene and climate.

The stimulus of new impressions on the nervous system, and consequently on all the organic functions, cannot be too highly estimated. Often, indeed, when the effects of the most appropriate medicines seem unavailing, a temporary change of scene and air will render the system more amenable to your prescriptions.

I would finally observe, that you will find a modification of the foregoing treatment, in various anomalous and painful diseases of a dyspeptic character in this country, to be attended with decided advantage after the ordinary routine of treatment has totally failed. I am firmly convinced that, in their general principles, the causes of these maladies share much in common with the affections now under consideration. Instead of the daily tonic, the dinner pill, the nightly purge, and the ever-ready mutton-chop, with its modicum of sherry, I have seen that careful attention to the skin and kid-

neys, rational habits, rational diet, and rational exercise, will play a far more important part in the restoration of tone to the stomach, and strength to the system, than the whole host of stomachics, alteratives, purgatives, *et id genus omne*.

In some forms of anomalous gout and rheumatism, occurring in individuals who have returned from tropical climates, when the vital powers are not especially depressed, and the constitution is free from serious organic change, you will obtain important advantages from the modern system of hydropathy, judiciously modified. It exerts a beneficial action on the renal and cutaneous secretions; and I am satisfied, that some form of the hydropathic treatment will maintain a permanent place in scientific medicine, after the mass of obscurity in which it is overwhelmed, through the ignorance and quackery of its present apostles, has been forgotten. In the wards of

our own Hospital, Gentlemen, I have fully tested, under your own eyes, its great value in certain forms of chronic rheumatism.

EUROPEAN FEMALES IN TROPICAL CLIMATES.

In connection with this important subject of the decay of the system, my experience in Brazil has established a proposition which might not have been *à priori* expected; viz., that the European female, especially of the upper classes, feels the injurious influence of climate more sensibly, and at an earlier period, than the male. This unexpected result must, I apprehend, be accounted for by the more indolent habits and mode of life of the former, favoured, if not altogether induced, by the languor inseparable from high temperature, and sanctioned by the prevailing customs in most tropical climates, where household occupations are not attended to as in Europe, where fashion or custom precludes the

enjoyment of active exercise abroad, and where even mental exertion is to some extent laborious, and, consequently, the muscular, nervous, and vascular systems are deprived of their due and healthy stimuli. Notwithstanding, therefore, the more regular and temperate habits of the female, and her exemption from many of the ordinary sources of tropical disease, as exposure to the sun, atmospheric vicissitudes, over-fatigue, &c., yet are these advantages more than counterbalanced by the inactivity and indolence almost necessarily connected with her position. This mode of life, by interfering with the efficient exercise of those depuratory functions which are so essential to health in all warm and humid latitudes, leads directly, and often quickly, to the development of numerous and grave derangements, in which the uterine system is frequently involved.

This question deserves the most serious

attention of the professional advisers of those European ladies who are doomed to a lengthened residence within the tropics; for many of the inconveniences, and some of the dangers, to which they are especially obnoxious, may be prevented or removed by careful attention to the considerations to which I have just alluded.

On their return to this country, these cases will necessarily demand the same careful investigation, and will be benefited by the same principles of treatment as have already been laid down, modified, of course, by the sex of the individual.

TIME DURING WHICH EUROPEANS CAN SUSTAIN A TROPICAL RESIDENCE WITHOUT INJURY.

In immediate relation to the questions which have hitherto engaged our attention, though but indirectly bearing on the subject of the origin and treatment of these

affections, are the inquiry how long an European can sustain a tropical climate without injury, and also the examination into the indications in the animal and organic functions, which determine the necessity for the removal of the invalid to his native land. These considerations are so intimately connected with our subject, that I should deem my observations altogether imperfect, if I did not offer some principles to assist and guide you in determining points of such practical importance. I shall, therefore, devote a few moments to the examination of these highly interesting matters, premising a few remarks on the agency of tropical heat and light, which exert such powerful effects on the animal frame, but have not hitherto been sufficiently considered. The examination of the latter subject, we shall perceive, is not altogether without reference to the immediate subject of my lecture.

INFLUENCE OF SOLAR HEAT AND LIGHT IN TROPICAL CLIMATES. The power of the direct rays of the sun is little less influential on animal than on vegetable life, as is evidenced by the salutary influence which they exert over all the organic and mental manifestations, and by the ill effects which follow their exclusion. Independently of direct physiological proof, this proposition will be fully admitted by every tropical sojourner, in whom years of subsequent exhaustion can never entirely efface the recollection of the buoyancy of spirits, unclouded mind, and exquisite appreciation of mere animal existence, which, unless counteracted by some special influence, characterise the first years of a tropical life. Authors have, indeed, attributed these vivid sensations to the novelty and splendour of a new world; to the brilliancy of its skies; to its perpetual verdure; to the richness, variety, and luxuriance of vegetable life,

&c. :—agencies, I admit, of importance, but utterly trifling, as compared with the direct influence of the heat and light of a *tropical sun*, in supplying a great and novel stimulus to the energetic performance of all the functions, not only of organic, but also, for a time, of animal life. This favourable condition of the economy, however, as in vegetable life under similar circumstances, proves but of limited duration, the precise epoch being determined by numerous concurrent circumstances, as the constitution, occupation, predisposition, and habits of life of the individual. Hence arises the serious and important question:—At what period does a tropical residence begin to affect the European constitution to such an extent as to influence longevity, or permanently to injure health?

The fallacious doctrine of *acclimatisation* by lengthened residence in high and unhealthy latitudes, so long an established

axiom with the British Government, is now happily exploded. This fatal and heartless system, long sustained under false and ill understood motives of economy, has at length slowly yielded to the light of experience, and to the prevalence of sounder and more humane principles; five years being the limit beyond which, I believe, our troops are not now suffered to remain in the West Indian and other unhealthy possessions. This limitation is certainly judicious, and entirely in accordance with the results of my own experience, which fully warrant the conclusion, that a European in the prime of life, and free from any especial tendency to disease, will resist the deleterious influence of the tropics for a period varying from five to seven years, according to circumstances. After this time, first the animal, and then the organic powers, will commonly give evidence of decline; and although the necessity which even then

exists for immediate change of climate may not seem urgent, yet its eventual importance, in enabling the constitution to resist or throw off the ailments incident to all hot climates, cannot possibly be over-rated. Nor should it be forgotten, that the ill effects of a tropical residence are often not displayed *immediately*, but in after life, and subsequently to the return of the individual to Europe—an important consideration, which should not be lost sight of by the judicious practitioner.

In my endeavours to solve the difficult problem of the period at which a tropical residence can no longer be borne with impunity, I have been much aided by the observation of a fact verified by experience and repeated experiment. For the first seven or eight years during which an European resides in hot climates, the urine is characterised by a marked excess of its acid constituents; this state, under the in-

fluence of climate, gradually changes; and finally, as health deteriorates, a neutral or even alkalescent condition of that fluid supervenes. This change, however, is subject to considerable variation, and is largely influenced by the habits of life and the constitution of the individual. Still, the rule will prove sufficiently general to assist the practitioner in arriving, under circumstances of doubt, at a sound opinion on one of the most serious and embarrassing, as well as one of the most frequent questions submitted to his judgment in all tropical countries; viz., the period beyond which a longer residence cannot safely be permitted to the European. As this decision will frequently involve not only the health, but the future career and fortunes of the individual, the bearing and importance of this question will at once be comprehended by all tropical residents, professional and non-professional. I therefore feel anxious that

the above-mentioned test should be submitted to further examination by my professional brethren in other hot countries than Brazil, so that its value and frequency may be satisfactorily defined.

In tropical climates, under favourable circumstances, as where the mind is tranquil and buoyed up by hope, where fatigue, exposure, and the distressing emotions are avoided, where the body and mind are judiciously exercised, where the diet and regimen are attended to, and where to these are superadded an equable atmosphere, never stagnant, nor exposed to great or sudden change, though otherwise loaded with moisture and animal effluvia, it is surprising to observe the length of time during which an individual will pursue his ordinary avocations without the development of any specific malady; although the obvious and unquestionable evidences afforded, by the nervous and vital manifes-

tations, clearly show how profoundly the economy is charged with the elements of disease, awaiting but the slightest touch from any disturbing moral or physical agency to determine an explosion. In Brazil, for example, how frequently have I observed individuals go on for months—aye, and for years—complaining of mental and bodily fatigue on slight exertion—their air languid, the feelings below par, the countenance sallow, the eye dull and with a slight tinge of yellow, the pulse weak and easily accelerated, the appetite modified, the lips slightly clammy, the bowels irregular, the urine commonly scanty and turbid, the testes pendulous, with a peculiar sense of uneasiness, irregular distribution of animal heat, profuse perspiration from trifling causes, moral or physical, and a morbid sensibility to the slightest atmospheric vicissitudes.

The foregoing condition of the economy

will display numerous shades, some slighter, some more intense, but all requiring the careful consideration of the physician. In all, the diet and regimen will demand strict attention; the moral and physical powers must be alike carefully examined and regulated; the secretory and excretory functions must be modified, and, if possible, restored to their healthy condition. I need not particularise the most efficient means for the attainment of these essential objects; first, because they must be familiar to every well-informed practitioner; and secondly, because the successful result must mainly depend on a judicious selection and application of the resources, suited to the ever-varying circumstances of each individual case.

Should, however, (notwithstanding the due administration of remedies), the above-described state of the economy persist, I would strongly impress on the minds of

those among you who are destined to a tropical career, the paramount importance of change of climate, if possible, to Europe; at all events, to some other locality, or even for a few days to sea. Debarred from this, the most valuable of all remedies, the tropical invalid will too commonly find the most skilful treatment unavailing; for the epoch has at length arrived—as arrive it will sooner or later to every European resident—when the administration of medicinal agents proves either useless or hurtful. On your correct appreciation of this critical era will depend, probably the life, certainly the health, and in numerous instances, the prospects and happiness of your patients.

LECTURE III.

OBJECT OF PRESENT SECTION OF LECTURES—UNCERTAINTY OF THEORIES AS TO NATURE AND CAUSE OF FEVER—EXAMINATION OF DOCTRINE OF LANCISI, SHOWING HIM NOT TO BE THE AUTHOR OF THE MODERN EXCLUSIVE THEORY OF MARSH MIASM—HIS ORIGINAL WORK SCARCELY READ—HIS VIEWS ON THE UNITY OF THE MARSH POISON—ITS ACTION ON THE NERVOUS OR SANGUIFEROUS SYSTEM—TWO KINDS OF MARSH POISON DESCRIBED BY HIM—ON THE SUPPOSED PRODUCTION OF ITS EFFECTS IN ONLY ONE MODE—THREE MODES DESCRIBED BY LANCISI—MARSH POISON NOT THE CAUSE OF INTERMITTENT FEVER ALONE—ANIMALCULAR THEORIES OF VARRO, COLUMELLA, AND VITRUVIUS—LANCISI'S STATEMENT, THAT A WIND MAY PRODUCE AGUE—SUMMARY OF HIS VIEWS ON MARSH POISON AND ITS EFFECTS.

GENTLEMEN,—The chief object contemplated in the present section of our lectures is to lay before you certain facts which have come under my own especial notice, during

a period of upwards of twenty-eight years, spent in extensive and active practice in Brazil, and in the public service in different regions of the globe. These facts are completely at variance with the received doctrine that marsh poison is the immediate cause of intermittent fever or ague. I must at the same time plead for some indulgence in entering on the discussion of a subject so extensive, and which has engaged the attention of our medical brethren in almost every quarter of the world. The facts which I have to lay before you are plain, simple, unvarnished; as such, I submit them to your attention, with this special remark, that, as the whole phenomena of nature cannot be conceived to have been as yet completely explored by man, so the statements which I have to offer must be considered as truthful exponents only of the actual experience which I have enjoyed. I may, in all truth and confidence, declare

that I am the partizan of no theory; that, like the practical men of old, I saw first, and formed my conclusions afterwards. To this, indeed, I was ultimately driven, in spite of preconceived notions formed in the schools, and sanctioned by the most celebrated authorities in the profession, and further strengthened by the additional guarantee that upwards of a century could add to the imposing array of great names, connected during that period with our medical schools, both at home and on the continent.

PRELIMINARY REMARKS—UNCERTAINTY OF
THEORIES OF FEVER.

Fever has, at all periods, been a subject of engrossing interest to the medical practitioner. Sydenham was wont to say, that nine-tenths of the human species were sent to their graves by disorders in which fever was the most promi-

ment agent. Nor was the observation of a late celebrated professor less pertinent, that a thorough knowledge of the treatment of febrile disorders in all their varying forms, types, and anomalies, embraces the great bulk of that range of human suffering, which can be relieved by the skill, science, and assiduity of the medical practitioner. There are probably few hardy enough to contravene these propositions; and those who, in the heyday of self-sufficiency, boast of possessing a remedy for every ailment that flesh is heir to, a few short years will undeceive, and will prove to them the woful inefficiency of physic, as well as the uncertainty of even their firmest convictions.

Few subjects, therefore, relating to theoretical medicine, have excited a more lively interest in the minds of the members of the profession, than the inquiry into the nature of fever; whether with regard to its *proxi-*

mate cause, or to the nature of those various remote causes, which exert so powerful an influence in predisposing the animal body to the accession, or which, under certain specified conditions, become the excitants of the febrile disorder. The theories on the question present the formidable appearance of "legion;" and the dense phalanx of their array is still daily augmented by the speculative fancies of ardent minds, and by the positive facts of more mature judgments. The rationalist on the one hand, and the empiric on the other, claim the divided empire of the realm of medicine.

It cannot certainly be affirmed in strict language, that any of the theories of the immediate exciting cause of disease rests on a basis so secure, as to be impregnable to the assaults of new theoretical writers, or to the still more convincing refutations of broadly established facts. The converse,

indeed, can only be conceived to be compatible with a perfect state of science; and the department of medicine is more remote from the prospects of such a happy consummation, than any other branch of human knowledge; although we have theorists, as well as practical men, who are convinced of the perfection of their science, even in its present imperfect condition. Nor must we be surprised that such ideas should obtain among certain members of the profession, when we revert to the fact that, but a century ago, when the mathematical pathology reigned predominant, propositions like the following were announced for discussion: "*Proposito morbo, invenire remedium.*"

If there are phases in the cycle of opinions and doctrines, these are not the phases of a recurrent series, but of an expanding and advancing intelligence. And if, from the circumscription of our facts, we are almost localised, as it might seem,

in our philosophy of causes, the extension of our facts must consequently enlarge the sphere of the basis of our principles, and dissolve the errors arising from the almost provincial mode of our philosophical propositions, by the broad and unerring light of universal truth. Propositions, delivered as true and infallible, may be so, if applied to the partial conditions under which they are developed; but they will become absolutely false when transferred to another locality, or merged in more general and comprehensive problems.

I am, therefore, Gentlemen, strongly convinced that much of the obscurity and uncertainty, and many of the contradictory doctrines espoused by different parties, are more to be ascribed to the imperfect views taken by the respective authors, than to wilful perversion of facts, or positive ignorance of the relations of various morbid actions with the series of changed functions

which they involve, and of their direct or indirect connexion with each other. There is a homogeneity (if the expression be permitted) in the laws of disease, of which we are just now beginning to enjoy the first glimpses; and although it would be too much to allege that fever and inflammation are but one and the same morbid action, greatly diversified, no doubt, through the influence of numerous concurrent circumstances, we nevertheless know that the theory of the day which rendered a special reason of the proximate cause of the *one*, was deemed to be equally illustrative and explanatory of the other. The phenomena of fever, nevertheless, as specially distinguished from inflammation, are, I am disposed to believe, *essentially one and the same*; the simplest expression of that morbid action being delineated in the paroxysm of an ague—in the succession of its three stages, hot, cold, and sweating; and assum-

ing, under certain circumstances of climate, constitution, modes of living, &c. &c., the various forms of *typhus fever*, *plague*, *remittent fever*, *yellow fever*, and all the subordinate varieties of fever denominated *essential* by the French school.

An unfortunate prejudice attaches to almost every subject that comes within the range of the speculations of the medical philosopher, arising from the necessary uncertainty of our imperfect science; so that no proposition is mooted which does not immediately become a subject of controversy; and almost every assertion is met by a plausible counter-assertion. Such, indeed, is the unhappy state of theoretical medicine, that it may too often be said of the disputants on either side,

“ Each claiming Truth,

“ And Truth disclaiming both ;”—

so delicately poised is the balance of evidence offered by the litigants. Not-

withstanding this doubt, however, and "endless errors," in which the sons of Esculapius are apparently lost, I am emboldened to lay before you various facts and circumstances connected with the origin of intermittent fever, which are totally irreconcilable with the commonly received theory of the ordinary exciting causes of that disease, and which would seem to connect the sources of the different forms of febrile action with a more broad and more comprehensive principle than the sectional causes which are expounded in our schools, and taught in our accredited works on the practice of physic.

EXAMINATION OF THE DOCTRINES OF LANCISI.

In examining the question, whether Marsh Poison or malaria is the sole generative cause of certain forms of fever, you will recollect that this doctrine is alleged to have been first propounded to the world in the

year 1695, by the celebrated Italian physician Lancisi ; and that, from its first promulgation, till within the last thirty years, it was adopted by the almost universal consent of the whole medical world,—flourishing in the schools as the theoretical dogma of the professors, and with practical men as the sole and only cause of intermittent fever. Prevalent, however, as the doctrine has been—incorporated with all the theories on the phenomena of essential fever—and embracing the fundamental pathology of one of the most numerous and most common class of diseases in tropical as well as in other regions—it is surprising to find how few men of our profession are conversant with the works of this original writer. In truth, among a wide circle of the medical profession in various quarters of the globe, including some of the most distinguished and learned physicians in London, I have never met with a *single individual* who

has perused the original works of Lancisi; and lately, being desirous to consult his works—I must confess, for the first time—no copy could be found either in our great national library, or in the library of the College of Physicians; and I finally obtained a perusal of his works in the library of the Royal College of Surgeons of England.* This, you will admit, is passing strange, when we consider that the theory of marsh poison or malaria has influenced all our reasoning on the subject of fever, and has exercised an absolute sway over the medical world for the last 150 years.

All, indeed, seem to have formed their ideas of Lancisi's doctrines on report and hearsay; for I feel satisfied that, had his works been *read* instead of *quoted*, we would have heard less of his doctrines, or, at least, we would have been less tutored

* LANCISI, J. M., Opera Omnia, Geneva, 1718. The pages refer to his Essay "De Noxiis Paludum Effluviis."

into the abstract idea of marsh poison being one and indivisible—of its being the exciting cause of intermittent fever by its poisonous action on the nervous or the sanguiferous system—of its producing these effects in only one mode—and of its being limited to the sole generation of intermittent fever, specially and individually, as distinguished from continued fever.

On each of these points I shall now offer a few observations. I fear, Gentlemen, that some of my auditors may find the matter of our present lecture dry, though its importance will be readily admitted by all; and I can only say that, however tedious the critique, the wading through the somewhat barbarous Latin of the original author was no amusing task.

1. IS THE MARSH POISON ONE AND
INDIVISIBLE?

Lancisi observes of the effluvia: “Non

ubique et semper eadem sunt, ejusdemque materiæ;”—“They are not everywhere and constantly the same, nor is their constitution always identical.” And again:—“Varia quoque in aërem ferri corpuscula, seu particulas solutas, et casu potius quàm certâ lege societas, quæ, in nostrum corpus ingestæ, minorem vel majorem noxam inferunt, prout minùs vel minimè inter se temperatæ, *aut etiam ad veneni prope naturam* evectæ sunt (p. 37);”—“There are also carried into the air various kinds of corpuscles, or particles held in solution, and connected rather by accident than by definite laws, which, on being received into our body, produce greater or less disturbance in it, according as they have been variously tempered together, or as they may even approach the nature of a poison.”

Thus he admits, virtually and positively, variations in the nature and character of the reputed poison (but he says not a

word about unity all throughout), as in the case of other specific poisons, such as the animal poison of small-pox, or the inorganic poison of prussic acid, or that matter which is believed to be the common poison of continued fever—contagion. He further remarks in this matter:—
“ Idcirco apud scriptores legimus, hujusmodi epidemias raro inter se omnino similes fuisse, tum qualitate et gravitate symptomatum, tum malorum exitu ad salutem, aut ad mortem. Quamobrem fateri necesse est, vapores a fracescentibus lymphis assurgentes noxios utique et plerumque malignos esse, *sed singulos sub unâ eâdemque specie comprehendendi non posse*; ac propterea medicos illos a vero quàm longissimè abire, qui unam semper certamque in omnibus ejusmodi effluviis particularum naturam aut quærunt cum spe inveniendi, aut se jam invenisse existimant” (p. 35).
“ We accordingly read, that epidemics of

this description have very rarely resembled each other, either in the nature and severity of their symptoms, or in their mode of termination in recovery or in death. While, therefore, we confess that the vapours arising from the fermenting waters are indeed hurtful, and generally malignant, we are bound to allow that they *cannot all be individually comprehended in one and the same species*; and consequently that those physicians have wandered very far from the truth, who inquire with the hope of discovering, or think that they have already found out, that the nature of the particles in all these effluvia is always one and invariable."

If there be, according to Lancisi, effluvia of a poisonous character, he surely, Gentlemen, cannot be deemed the author of the theory of a special poison. This must rather be considered as the doctrine of his commentators, as delivered in the following graphic definition by the most recent writer

on this subject—Dr. Watson; who, in his admirable *Lectures on the Practice of Medicine*, says:—"The exciting cause of intermittent and remittent fevers—the *primary cause* I mean, that without which ague would not occur at all—deserves a somewhat particular attention. I need scarcely say, that it consists in certain invisible effluvia or emanations from the surface of the earth, which were formerly called marsh miasmata, but to which it has, of late years, become fashionable to apply the foreign term *malaria* The malaria is a specific poison, producing specific effects upon the human body In its medical sense, it is not simply bad air, or impure air The impure air incident to large and populous cities is prejudicial enough to health, as I formerly took occasion to show you; but it does not *generate* fever; neither continued fever, nor intermittent The effluvia, which thus form the sole ex-

citing cause of intermittent and remittent fevers, proceed from the surface of the earth The inference that they exist was not made till within the last century and a half . . . Lancisi was the first, so far as I know, to put forth distinct ideas concerning malaria in his work, *De noxiis paludum effluviis*. This is the great original work on the subject." But, as already stated, Lancisi certainly cannot be considered a special pleader for the doctrine of marsh miasm, in the sense so clearly defined by Dr. Watson. This we shall see more fully immediately.

By breaking down the unity of the cause, and therefore admitting the agency of other forms of the same cause, he tacitly confesses the probability of other causes than the miasm of marsh effluvia; and thus the whole fabric of the hypothesis of a *specific* poison falls necessarily at once to the ground; for the malaria, as such, can only be con-

sidered a specific agent so long as its individuality and indivisibility remain unquestioned. Moreover, his words just quoted, "*aut etiam ad veneni prope naturam evectæ sunt,*" distinctly imply a doubt as to the existence of any poison at all.

2. ARE WE TO SUPPOSE THAT MARSH MIASM ACTS AS THE ESSENTIAL CAUSE OF INTERMITTENT FEVER, BY ITS POISONOUS ACTION ON THE NERVOUS OR ON THE SANGUIFEROUS SYSTEM?

That there might be a poison existing in the atmosphere of marshy districts, we possess no grounds, *à priori*, for denying; but the *onus probandi* assuredly rests with those who maintain the existence of such poison, more especially as they assert that its presence is not the subject of direct demonstration, but rather inferential.

But Lancisi propounds, as part of his doctrine, that there are two kinds of poisonous emanations disengaged from waters in a state of corruption (putrefaction); one altogether inorganic in nature; the other distinctly organic in character. The former he conceives to be “an accumulation of dead and inorganic particles, with impure sulphur, and acrid and volatile salts, with other extraneous matters, which being densely evolved in the exhalations from the waters, affect, in a very unpleasant manner, the sense of smell. The other genus of effluvia is composed of a multitude of worms and *ova*, which float about in the atmosphere—a distinct host of aërial animalcula.” “Alterum quidem est congeries inorganicarum atque inanimarum particularum, impuri sulphuris, saliumque lixivio acrium ac volatilium, aliorumque exoticorum, quæ, aqueis halitibus crassè obvolutæ, ingratum etiam naribus empyreuma propinant. Al-

terum vero effluviorum genus constat multitudine vermiculorum atque ovulorum, quæ animalium quasi agmen instruuntur in aëre."

Hence it is obvious, that the constitution of the vegetable or marsh miasm is altogether of a heterogeneous and contradictory nature; for the first, or inorganic constituent, can scarcely be conceived to belong to the vegetable world at all, as the sulphur and saline elements sufficiently testify; nor is the character of the second less emphatically marked, as it has no claim whatever to the title of an *effluvium* or *poison*, being positively an atmospheric congeries or aggregation of animalcules. The application of these, therefore, to the body, must necessarily produce, if any, very diverse effects.

Confining my observations to the effects of the organic or *vitalised effluvia*, let me again quote the words of Lancisi. He observes:—

“Exploratum quidem nobis videtur, or-

ganica atque animata effluvia a paludibus prodeuntia triplici potissimum modo nostrum corpus vitare;—

Primo, 1°. Per se ipsa irritando, vulnerandoque.

Secundo, 2°. Quod forte deterius est, pravos suos succos cum nostris liquidis permiscendo.

Tertio, 3°. Denique indigenas lumbricos nutriendo, saginandoque" (pp. 53).

"It seems to be ascertained, that the organic and animal effluvia, proceeding from marshes, affect our body chiefly in a threefold manner:—

1. By mere irritation, and by the wounds they inflict.
2. What is more serious, by mingling their corrupt juices with the fluids of our bodies.
3. Lastly, by affording nourishment to the parasitic intestinal worms."

Thus, in accordance with the views of

Lancisi, it is less by a direct action on the nervous system, than by a positive agency on the fluids of the body, that the deleterious effects of marsh effluvia arise. The third mode—that of affording a supply of nourishment to the worms within, after which, he believes, the economy of the body is disturbed by these animalculæ,—is not a little remarkable, and does not in any way countenance the idea of a specific poison acting as a cause of ague, whatever other influence they may be supposed to exercise. It is, moreover, Gentlemen, an incontestable proof of the very crude and unsatisfactory nature of the ideas attached by Lancisi to the assumption of an effluvia from marshes; for, under his plastic hands, while in one instance it is purely and distinctly of an inorganic constitution, in another form it assumes not the character of a specific poison, but of a hybrid medley of organic mixture, represented by worms.

ovula, *et id genus omne*, in the action of which he perceives a source of irritation applied to the organic tissues, and a pabulum of nutrition to the worms and ova indigenous to the intestines of the human body.

In vain do we search here for the distinctive proofs of a purely vegetable poison, arising during the putrefactive process of vegetable life; in vain do we endeavour to discover those more simple forms into which the decomposing process resolves all that has lived, be it animal or vegetable; in vain do we recognise in these representations of the simple forms of animalcular life, aërial essences, whose constitution is so happily adjusted, that they rise, by the expansive influence of the solar heat, to be condensed and precipitated earthwards, on the approach of the shades of night, like any inorganic gasiform body. For it is paramount, in our consideration of the *ori-*

ginal marsh miasm theory, that we always have before our eyes a distinct perception of the physical properties of the supposed poison, as well as a notion of those matters which enter into its constitution.

3. ARE THE EFFECTS OF MARSH MIASM PRODUCED ONLY IN ONE MODE—NAMELY, BY ITS ACTION AS A SPECIFIC POISON?

We shall presently see, that the influence of marsh miasm is not solely limited to the production (according to many of the moderns) of ague alone, but that it is further effective in exciting other forms of diseased action, such as diarrhœa, dysentery, cholera, neuralgia, rheumatism, &c. The observations of Lancisi, however, do not refer to the different kinds of diseases induced by the marsh effluvia, but to the different modes in which these effluvia act as an

exciting cause of intermittent fever. His words are too remarkable to be omitted here:—"Corporibus nostris magnam afferunt labem, quia scilicet partim recrementorum perspirationem præpediunt; partim detrahunt quæ incolumitati suffragantur; partim (quod maximè advertendum) multa ingerunt, quæ sanguini spiritibusque nostris, immo etiam solidis partibus, peculiaria inferunt detrimenta;"—"They are highly injurious to our bodies, partly because they prevent the perspiration of certain excretions, partly because they withdraw those elements which contribute to safety, and partly (and this is particularly deserving of notice) because they introduce many principles of highly pernicious character into our blood and vital fluids—nay, even into our solid tissues."

Now Gentlemen, we are here obviously presented with some of the leading doctrines of the pathology of intermittent fever, as

entertained by Lancisi. It is scarcely necessary to remark, that only in the third of these propositions is there any countenance given to the idea of a poison. Nor is the description which he offers such as would lead to the doctrine of the special operation of *one* element as the efficient agent in exciting ague; for he employs the word *multa*, as expressive of the general character of the matters which contaminate the blood—a term altogether abhorrent to the idea of one agent affecting the animal economy. It would, indeed, appear, that Lancisi had not any very definite idea as to the individuality of the poison, and the individuality of its effects; for, while he assumes a contamination of the fluids and solids as one of its effects, he unhesitatingly admits the positive influence of suppression of the excretions as another result of the application of the effluvia; and furthermore attributes a negative action to their effect,

that of removing, impairing, and weakening certain states or conditions especially requisite to the perfect health of the body.

Whatever, then, modern authorities may please to assert of Lancisi, it is apparent that he was not so wedded to the idea of marsh poisons as not to appreciate, clearly and distinctly, that the existing condition of the atmosphere in malarious localities had a decided action in effecting most important changes, positive as well as negative, on the functions of the animal body. Was Lancisi acquainted with the fact of the great amount of the cuticular perspiration, and the influence of moisture in diminishing all the secretions? Into this question it is immaterial to enquire, as he sufficiently evinces the weight he attaches to the fact, mentioning suppressed perspiration as one of the leading heads (three in all) of the effects of marsh effluvia on the human body.

In proceeding farther with his observations, and in adverting to the reasons which may be given explanatory of the injurious agency of a marsh atmosphere, he writes as follows:—"E contrario palustris aër, cum crassus, minus mobilis, minusque elasticus, immo, quod pejus est, impuro sulphure acribusque salibus sit inquinatus, naturales certè functiones alterabit, liquidisque nostris admixtus eorundem crasim motusque vitiabit, fibrarum quoque inductâ laxitate" (p. 45;)—"On the other hand, a marsh atmosphere, as it is dense, less mobile, and less elastic, nay more, what is worse, as it is rendered corrupt by impure sulphur and acrid salts, will certainly affect the natural functions, and, commingling with the fluids, will vitiate their cohesion and movements, inducing also a relaxation of the animal fibres."

And again, at p. 45, he says:—"Unde fit, ut quæ nostris a corporibus perspirari deberent vel noxia, vel saltem inutilia

corpuscula, magnam partem prohibeantur effluere (cujus effectûs signum e vestigio est vitiatum in advenis color, inductaque corporis lassitudo); rursûmque deteriora propter extraneorum admixtionem effecta, in sanguinem, in præcordia, atque in cætera viscera refundantur; quibus locis eo usque accrescunt, et cumulantur, donec, naturalibus solidorum et liquidorum viribus superatis, febrem accendant, reliquis comitatum symptomatis, quibus castrenses et pestiferæ stipari solent;”—“Hence it is, that those corpuscles, which ought to be thrown off from our bodies as detrimental, or at least as useless, are in a great measure prevented from being removed (an almost immediate indication of which is the change in the complexion of strangers, and the readiness with which they become fatigued); and, rendered still more noxious by the admixture of foreign matters, they are again thrown back upon the blood, the præcordia,

and the other viscera; in which parts they increase and accumulate to such an extent, that, overcoming the natural powers of the solids and the fluids, they induce a fever, accompanied with all those symptoms which usually attend on camp and malignant fevers."

In this paragraph we have the chief action of the effluvia developed in two distinct and different circumstances—first, the checked and impeded state of the perspiration; and, secondly, the admixture or introduction of foreign matters into the course of the circulation. That our author should attribute the greater influence to the latter, was natural enough; but, in attaching no small importance to the former, he evidently proves that he was not so completely absorbed in his theories as to resist the clear evidence of his senses. Facts, or rather statements, like the above, speak strongly in favour of the candour of Lancisi; for we

have no right to lay to his special charge the notion of the impure sulphur, and the acrid salts mixed with the atmosphere; these were portions of the theory of his era, and would, therefore, naturally tinge the current of his reasonings.

But, whatever faith Lancisi may have been disposed to give to his own doctrine of the marsh miasm, as the essential cause of ague, he certainly did not believe that ague could *not* arise from other causes than this contamination. He distinctly states that breezes, from whatever quarter they blow, although most completely pure, (“*tametsi saluberrimus,*”) are adequate to induce disease, solely by the force of their current—a proposition completely heterodox to the general tenor and drift of his observations, but altogether consonant with my own experience.

4. IS THE EFFECT OF MARSH MIASM LIMITED TO THE SOLE GENERATION OF INTERMITTENT FEVER, AS SPECIALLY AND DISTINCTLY SEPARATED FROM CONTINUED FEVER ?

Singular as it may appear, Lancisi does not make that decided difference between intermittent and continued fever which obtains so generally in all our modern works, and which has received the sanction of the schools. On this topic he uses the following explicit and emphatic terms :—
“Cognitum itaque perspectumque medicis erit, ideo primum sub æstatis initium, tertianas paludum habitatores adoriri ; postea continuas ac malignas febres, deinde etiam pestilentes ; tandemque eosdem longissimis morbis implicari ;”—“It is, accordingly, an established and well-known fact to physicians, that, at the very opening of the summer, the inhabitants of marshy districts are attacked

by tertian ague; *continued* and *malignant fevers* then succeed, and also pestilential; and at length they are subject to tedious chronic diseases."

His testimony here is the more valuable, as his bias must have naturally been to restrict the effects of marsh miasm to intermittent fever, as its sole product. But it is apparent that Lancisi read the Book of Nature, as she unrolled her pages, not as he might have viewed her through the spectacles of the theoretical nosologist, leaving to a more complete and more perfect state of the science the task of apportioning each disease to its special rank and position in the nosological chart.

In his further observations on the effluvia of marshes, reference is made by Lancisi to the remarks of Varro on the same subject, who describes a host of animalcules not patent to the eye—"quæ non possunt oculi consequi,"—which, nevertheless, are suffi-

ciently adroit to insinuate themselves into the body by the mouth and nostrils, occasioning very serious diseases—"difficiles morbos." Columella he also quotes, as testifying to the same fact.

Nor were the philosophers in those days inept practitioners in the art of splitting hairs, "'twixt north and north-west side;" for, while Varro and Columella ascribe the fevers to the introduction of these animals into the system, Vitruvius, adopting a somewhat more *recherché* pathology, ascribes all the disorder to the effects of the hysteric winds and secretions of the aforesaid animalcules. The words are:—"Sed in flatibus collocat, exhalationibus et spiritibus ab iisdem insectis potissimum emissis." Referring the ague "to the winds and flatulent discharges and exhalations thrown off by the insects"!!! So much for theory!

Thus Gentlemen, you will perceive that, whatever opinions the moderns may have

ascribed to Lancisi, the perusal of his works will sufficiently prove that he had no special *pet* theory to advocate and support; as his remarks, to which I have just adverted, in describing the different characters of the effluvia evolved in marsh districts, abundantly testify. Nevertheless, the following passage is too striking to be passed over in silence, especially as it exactly corresponds with my own experience, to be subsequently noticed, of the influence of the sea-breezes, under certain conditions of the animal economy, in exciting a paroxysm of intermittent fever. The words are:—"Quamobrem quilibet ventus, tametsi saluberrimus, malum istud solâ impulsione vi producere valet,—et contra ipsemet auster interdum tueri et conservare ambientem aërem alicubi potest, quoties alio ac potissimum in contrariam plagam malignos habitus validissime transfert;" (p. 40). "Wherefore, a wind from any quarter, however salubrious it may be, is

adequate to produce this malady by the force of its current alone; and, on the other hand, the very same wind may sometimes preserve the atmosphere of any place, by the force with which it repels malignant vapours in another direction, and especially if towards an opposite region." Words cannot more definitely describe the generation of intermittent fever, independently of all vegetable or marsh effluvia.

I have thought it proper to put this testimony on record, as it must be admitted to be free from all bias, and therefore more invaluable as an adjunct in support of the doctrine which I have myself embraced. I am perfectly sensible that it might be conceived by some to prove too much; this, however, I imagine, it must be allowed to demonstrate, most clearly and explicitly, that Lancisi never entertained or promulgated the idea of a poison being the inviolable exciting cause of intermittent fever.

Upwards of a century, then, has this *reputed* doctrine of Lancisi ruled in the medical world, receiving implicit credence on every hand, while all the methods of treatment, preventive as well as curative, have been fashioned to the mould of the hypothesis, and have been lauded or condemned in proportion as they corresponded with it, rather than in proportion to the success with which they were attended.

In framing his doctrine, Lancisi obviously was influenced by the then prevailing notions of the animalcular origin of diseases; and whatever opinions the moderns, from tradition and mere hearsay, have adopted regarding it, the extracts which I have submitted to you will clearly demonstrate that, in his eyes, the cause of marsh fever was neither *simple* nor *uniform*, and in no mode whatever entitled to the name of a *specific poison*. It distinctly embraced two kinds of emanations—organic

and inorganic—the former of which was not to be distinguished as a special poison, but was a heterogeneous mixture of organic compounds, combined with the ova of vermicular animalcules.

So far, then, the authority of Lancisi cannot be adduced in favour of the doctrine of a special poison; but his authority is invaluable as proving the distinct origin of every variety of fever from the agency of moisture, cold and heat, and other atmospheric vicissitudes. In this respect, his observations become of the highest interest, and in a great measure corroborate the views which I have myself been led to adopt; first, as to the cause of these fevers, and second, as to the means of prevention and cure. His words on the effects produced by the effluvia are distinct and emphatic:—"Checking the perspiration by which the noxious and useless matters, which otherwise accumulate in the system,

ought to be carried off from the body.”
 “Unde fit, ut quæ nostris e corporibus perspirari deberent vel noxia, vel saltem inutilia corpuscula, magnam partem *prohibentur* effluere.”

I shall conclude, Gentlemen, with a brief and condensed statement of the chief views entertained by Lancisi on the subject of marsh poison and its effects. You will at once perceive, how widely his recorded doctrines are at variance with those so universally and erroneously imputed to him, more especially by the moderns. He observes:—

1. That the poisonous emanations of marshes are not of a simple nature.
2. That they are always compound.
3. That they are composed of inorganic and organic effluvia.
4. That the inorganic portion can scarcely be held of itself to be a cause of disease.

5. That the organic constituent (which, by the way, is almost wholly of an animal composition), conjointly with the preceding, occasions the fevers, &c.

6. That these emanations are consequently of a very compound character.

7. That these emanations or effluvia vary very much in their physical and chemical constitution.

8. That they are more especially influenced by the season of the year.

9. That they produce a diversity of diseases.

10. That they excite continued and remittent fevers, as well as the intermittent.

11. That they give rise to other maladies, such as diarrhœa, &c.

12. That the fevers are not always caused by marsh airs or miasma.

13. That they occasionally arise from a disturbed state of the elasticity of the atmosphere.

14. That they are materially influenced by worms taken up by the animal body.

15. That they occasionally originate where the atmosphere is in the state of highest purity; and finally,

16. Lancisi never employs, throughout his work, the word "*venenum*" or poison; and, consequently, he cannot be charged with the theory of marsh miasm being a specific poison. The diversity of effects, indeed, which he ascribes to its agency, fully negative any such proposition. In fact, as I have already stated, Lancisi has been *quoted*, not *read*.

I hold in my hand, Gentlemen, numerous other extracts from the works of Lancisi, bearing out the views I have just submitted to you, and no less striking than those which I have already quoted. Our time, however, will not admit of my reading them, nor is it necessary.

LECTURE IV.

PREVAILING DOCTRINES ON MALARIOUS FEVERS—PERIODICITY NOT CONFINED TO INTERMITTENT FEVER—PARALLEL BETWEEN GOUT AND AGUE—REVIEW OF THE CONFLICTING OPINIONS AS TO THE SOURCE, NATURE, AND PROGRESS OF THE ALLEGED MARSH POISON—SUMMARY OF THE VIEWS OF STANDARD AUTHORS.

GENTLEMEN,—I have now to direct your attention to a brief consideration of the prevailing doctrines or opinions on those forms of fever of which marsh poison is the (supposed) special cause. For the term marsh poison, the more general, though not less equivocal term, *malaria*, has been recently substituted. Nor has this change in the term been altogether fortuitous, or produced by mere fashionable caprice, as sug-

gested by Dr. Watson, but rather the result of an extension of our knowledge of the conditions under which the fever originates; for, in the term malaria, we have eventually expressed a doubt as to the invariable origin of the fever from emanations arising through the decomposition of vegetable matters in marshy districts. I have already noticed the clear and concise definition of Dr. Watson, and shall now present you with the general foundation of the doctrine that marsh miasm and ague stand to each other in the relation of cause and effect, as laid down in a modern work of standard character—the *Cyclopædia of Practical Medicine*.

“ We infer the existence of the matter designated by one or other of these terms (malaria or marsh poison), as we do that of contagion in cases of disease which do not furnish a material transferable by inoculation, from certain effects on the animal

economy; and we trace its origin to marshes, from its having been observed, for a series of ages, that such effects are produced only in the vicinity of marshes, or at least are more abundant when vegetable matter and water are so situated with respect to each other, that chemical reaction between them *is possible*; and when we perceive the more familiar of them, particularly intermittent and remittent fevers, we infer its existence, and endeavour to discover its sources, which may not be very manifest."

Medical observation, Gentlemen, is in truth a most fallacious and error-leading subject; nor does this charge apply to the discussion of medical questions alone, for error, fallacy, and doubt, beset every subject which the mind of man compasses. Moreover, when we consider how much the condition of the sciences pertaining to the philosophy of nature has been altered and

changed within the last and since the opening of the present century, we would not feel much inclined to repose unlimited confidence in this evidence "of ages;" especially when it is stated of a fundamental element in the theory, that it "is possible." In contemplating such questions, we should remember that the old doctrine of phlogiston has not long disappeared before the searching inquiries of modern chemistry; and that scarcely more than seventy years have elapsed since the compound constitution of the atmosphere was demonstrated. When, therefore, we consider the brilliant discoveries which have since illumined the walks of the chemist and the histologist, it is not unreasonable to suppose that the more complex nature of animal organisation, and the various agents by which it is affected *for good or for evil*, must have been reciprocally illustrated, and rendered more definite in their characters, and pre-

cise in their relations, in so far as the present age is supplied with new elements and new methods of investigation for the elimination of truth, which were altogether beyond the reach of our predecessors.

IS PERIODICITY A DISTINCTIVE FEATURE IN CERTAIN DISEASES ?

Much importance has been attached by modern authorities to *periodicity*, as serving to distinguish the various diseases originating in marsh poison. Now, with reference to this law, I cannot help observing, that the explanations offered by different authors afford an admirable illustration of that convenient and laudable art by which we are enabled to conceal our ignorance under a cloud of scientific and well-sounding phrases, which, at first sight, would appear to convey something very like sense; but whose intrinsic value, when they are analysed, is found

to amount to "Vox et præterea nihil." I apprehend, indeed, that the law of periodicity, or the disposition to remission and exacerbations, at certain intervals, will be *found to apply more or less distinctly to all human diseases*—to those arising in the most opposite conditions of the animal economy, and determined by morbid agents apparently the most dissimilar and opposite in their nature as well as in their results. For example, what conditions, within the whole range of human nosology, can be seemingly more dissimilar and opposed than the high animal development, full plethoric constitution, and robust manifestations of vital power, evinced in the indolent, overfed, and gouty denizen of British corporate life, as compared with the pale, sickly, aguish, and care-worn countenance, the ill-nourished and emaciated frame, of the peasant of the Roman Campagna?

PARALLEL BETWEEN GOUT AND AGUE. In

our great cities, the wealthy citizen ordinarily indulges in the most substantial food, the richest cookery, and the most stimulating wines. His habits, pampered, indolent, and luxurious, react with certainty on the mind, which soon loses tone, and becomes unfit for healthy action. The higher attributes become gradually blunted; the animal propensities are exalted; and the expression of the countenance is unfavourably modified. Concomitant with these, and at an earlier or later date, according to the constitution and predisposition of the individual, other changes are developed. The gastro-intestinal mucous membrane suffers; the digestive functions fail; the animal and mental energies give way; the secretions and excretions, which have long ceased to be efficiently eliminated, are changed in quantity and altered in quality; and their accumulation in the system is revealed by derangement of the circulation, and by their deleterious

impression on the vital powers, and on the various organs of animal and organic life. At length, oppressed nature makes an effort to remove the evil, and a paroxysm of gout, more or less regular, is the immediate consequence.

Let us now observe the results on the animal economy, and the depressed state of the constitutional powers, incident to the individual exposed to labour disproportioned to his strength, scanty or unwholesome diet, and insufficient clothing, and residing in low and humid localities. Sooner or later, according to constitution and predisposition, the influence of these conditions will be manifest, as in gout, first, in impaired organic nervous power, producing inadequate performance of the respiratory actions, derangement of the heart and blood-vessels, and direct or indirect affections of the secreting, excreting, and assimilative functions, as well as of the mental manifestations.

Hence arise congestion of the large vessels, and of the internal viscera, and contamination of the circulating fluids, from diseased or deficient secretion and elimination of their various constituents; followed, when the accumulation of morbid principles has reached a certain height, by reaction, or an effort of nature to efface the morbid impression on the different organs, to restore the blood to its normal condition, and to re-establish the energetic and healthy action of the different secreting and excreting organs. This effort will be revealed in the peasant by a paroxysm of ague, as in the alderman by a paroxysm of gout; and here commences the law of periodicity in both.

Behold, then, the citizen, flushed and rubicund, groaning under a regular paroxysm of inflammatory gout; the peasant, with his sallow Hippocratic face and attenuated frame, shivering in the horrors of a paroxysm of ague. Observe them again in a few

hours. Both are restored apparently—and but apparently—to their ordinary health, to undergo, after a brief space, a repetition of their previous sufferings, followed by another remission. Here, clearly, is periodicity, as distinct and well marked in the gorged and gouty alderman as in the half-starved aguish peasant. But it may be urged that the analogy is incomplete—that gout exhibits only a remission, the ague an intermission. I deny the truth of both propositions, at least in so far as relates to the distinction attempted to be established between *remission* in the one case, and *intermission* in the other. The term intermission (cessation or suspension of disease), in fact, applies correctly to neither. The animal economy does not return to its healthy condition during the interval between the paroxysms of an intermittent fever, as must be admitted by every observer who has watched its progress attentively,

or who, like myself, has been the subject of an attack. The same holds good in gout. In both diseases, the interval between the paroxysms is marked by certain feelings of *malaise*, irritability, depression of vital power, and derangement of the secretions, as truly indicative of morbid disturbance as the throbbing toe, the icy chill, or burning temples.

Thus, then, we perceive that in regular gout—a disease to which few will be disposed to assign a *malarious* origin—the law of periodicity applies no less closely than it does to ague. The analogy between masked or simulating intermittent, and atonic or anomalous gout, is too obvious to require comment.

On reflection, however, we need not feel much surprise that an analogy so striking should obtain in the phenomena of diseases which, on cursory observation, appear so dissimilar. In truth, the general features

of the internal conditions in both maladies are identical, in so far as, in both, the circulating and secreted fluids are poisoned, not by morbid matters carried into the circulation from without, but by deleterious or effete elements generated within the system, and which ought to have been eliminated.

In considering ague, as, indeed, in almost all other diseases, there are other morbid conditions to be attended to besides those which are essentially expressed in the nosological character of the malady, and which not unfrequently assume the appearance of the major element. Thus ague, in its simplest form or type, may be held as a succession of three stages—the cold, hot, and sweating. But to these may be added visceral, especially splenic and hepatic congestions ; and, in more aggravated cases, various modifications of mucous irritation arise ; so that we may have three distinct forms of morbid action connected with, and

primarily dependent on, the same exciting cause.

CAN THE SUPPOSED POISON OF AGUE REMAIN
DORMANT?

It has been urged, in proof that a special poison acts on the system in ague, that individuals, exposed only for a brief period to the influence of marsh miasmata have been seized with intermittent fever. Now, I have myself been repeatedly exposed, and have witnessed the temporary exposure of others, in robust health, and with unimpaired nervous energy, to the most concentrated emanations of marsh effluvia in various quarters of the globe; and in *no single instance* have I ever observed such temporary exposure followed by ague. It is true that, had such persons been previously subjected to the influence of long-continued heat, cold, fatigue, intemperance,

the depressing passions, &c., then, indeed, intermittent or remittent fever, rheumatism or dysentery, or some other disease, might, and probably would, ensue, according to the constitution and predisposition of the individual. Such sudden attacks I have repeatedly witnessed to supervene on such temporary exposure in marshy localities; but I have invariably found, in these instances, that the constitutional powers of the individual had previously suffered from one or other of the depressing moral or physical agencies just alluded to. Nor, in this inquiry, must we lose sight of the fact, that a long interval may elapse between the application of the *supposed* malaria and the appearance of the intermittent, as I myself frequently witnessed in 1812, while serving as resident medical officer in the general hospital at Cadiz, the garrison of that fortress being then chiefly composed of British troops, who had been engaged in

the memorable and disastrous expedition to Walcheren. Even in this country, I met with a distinguished member of the English bar, who had just recovered from a severe attack of ague, supposed to have had its origin in a night journey across the Pontine marshes, under circumstances almost identical with my own. Now, this gentleman had, in the interim—that is, between autumn and spring—enjoyed his usual health; yet did his physician, one of the most eminent in England, consider that the marsh poison had lain dormant in the system during this long interval, until called into activity by a prolonged exposure, in an open carriage, to a keen easterly wind, exactly seven months after the supposed infection! The case of a lawyer, also, under precisely similar circumstances, was reported to me while in Edinburgh; with this difference only, that the period of incubation, as the phrase goes, was con-

siderably less—about one-half—between the supposed introduction of the poison and the development of the disease. Recently, also, Mrs. C., wife to one of the British merchants at Bahia, where she had resided for some years, and whence she had just returned, after a long sea voyage, was seized with a severe attack of ague, on the evening of her arrival in Cheltenham, months after she could by any possibility have been exposed to the influence of marsh miasm.

Are we then, in these and similar cases, really to believe that a poison has entered the system, and remained dormant and unchanged for months, nay, for years?

There is something so painfully unsatisfactory in our endless succession of medical theories, that the mind, in this as in other questions, longs for a resting-place—it feels the want of a *point d'appui*, and often, I am persuaded, actuated by that feeling,

“sees a little, and jumps to a conclusion;” less from the force of the facts and cogency of the reasoning, than through the desire of enjoying some fruits for its labour, even if it be but the mere empty satisfaction of an hypothesis.

REPUTED ORIGIN OF POISON FROM MARSH MIASM.

You will recollect, Gentlemen, that the proposition of the origin of intermittent fever first opened with the statement, that a miasm from marshes was essentially requisite to its generation; and this idea was, in the main, apparently substantiated by the abettors of the theory, from the extreme prevalence of that class of diseases in the immediate vicinity of those regions. It has since, however, been discovered that the narrow theory of limitation to marshes was not so universally tenable as had been com-

monly credited; for the poison, or rather the supposed poison, (as its existence had yet to be demonstrated), betrayed its effects under conditions of the ground and the surface, to which the term marsh, in any sense of the word whatever, was totally inapplicable. Accordingly, then, as the range of observation and inquiry extended, the intermittent and remittent forms of fever were discovered to prevail frequently in the midst of low and dense brushwood—amidst reeds and grasses; and in certain instances, its source had been traced to large and open woods. The *jungle* fever, which is so proverbially known in our Eastern possessions, originates in districts of the character now described; and, in the tropical regions of Africa and America, a similar fever is prevalent under identically similar circumstances, far distant and remote from every thing in the form of a morass.

The wet meadow-lands, the grounds

alternately flooded and dried for the cultivation of rice, and the half-wet ditches of fortifications, were subsequently recognised as common sources of intermittent fever. Ponds and marshes dried up by summer heats, the dry mud exposed by the flow and ebb of tidal rivers in hot climates, the exposure of land by felling the timber—have all, at different periods, and in different historical records, been noted as the cause of ague. We are further particularly informed, on the authority of Macculloch, that the effect of turning up land which had long lain as pasture, has been productive of fever equalling in malignity the plague; the labourers actually dying on the spot in the course of twenty-four hours, if they remained there all night.

The above-mentioned have been commonly reputed the *natural* sources of the fever. The artificial decomposition of vegetable matter, as in the process of steeping flax and

hemp, is affirmed to be not an unusual source of the same malady; and Rush mentions the decomposition of coffee, potatoes, pepper, &c., as being attended with a similar effect. The leakage of sugar, the contaminating reaction of bilge-water on chips and shavings, as well as neglected sewers, have all successively been condemned as productive of intermittent and remittent fevers.

From the collective grouping of all these facts, the general inference was drawn, that the concurrence of the action of decomposing vegetable matter, along with moisture, was indispensably requisite in all cases to the generation of intermittent fever. The medical world generally was disposed to acquiesce in this idea, and, taking the hypothesis for granted, felt satisfied in tracing out the laws of the poison, in ascertaining its actual effects on the animal economy, and in determining the modifications

under which its agency comes into play, &c. &c. On these topics they have dwelt with no small degree of curious precision; and, if they have not substantially proved the entity of their poison,—the element, source, and origin of all their deductions, they have certainly raised a superstructure, no doubt attractive and ingenious, but as illusive, I believe, as the basis on which it rests. Nor have their researches been less curious than hypothetical; for, a pure assumption being the basis of their theory, a numerous progeny of fancies and hypotheses have issued therefrom, all bearing the common family likeness of misapplied observation and incomplete evidence.

That the foregoing is no exaggerated statement of the data which are current at this very hour on the etiology and semeiology of intermittent fever, I conceive that the following statements on different points connected with the origin, progress, dif-

fusion, and arrestment of marsh fever, as it is commonly named, will satisfactorily attest.

The usual source has been supposed to be a marsh; but the observations of the great mass of practical men who have actually witnessed these fevers, all agree in showing, that it is *not* when the marsh is properly entitled to that appellation that fever is most rife. On the contrary, when the rains set in, the fever usually disappears, and endemic disease subsides; and, in Africa, the natives then freely visit each other in their canoes, the danger existing when the water is dried up, the vegetation all burnt, and nothing but an arid desert remaining. In 1799, the plains of Holland did not produce any fever among our troops there, the season being wet; and it was left for the dry weather, during the fated Walcheren expedition, to testify to the malignant fevers which can arise in a situation

devoid alike of vegetation and of moisture. Spain and Portugal afford similar evidence. Again, Trichori, in the Gulf of Volo, in Greece, a dry limestone rock, is notorious for its malaria; in testimony of which we have the high authority of Dr. Macmichael: and there are many Trichories.

Dr. Watson, in his work already quoted, observes, that "malaria, the primary exciting cause of intermittent and remittent fever, without which ague would never occur, is a specific poison, producing specific effects on the human body." This definition, like all emanating from Dr. Watson, is clear and concise; but, unfortunately, the disease does not obey one single law of those attached to the histories of the so-named specific poisons. Contrast it, for example, with small-pox, or with scarlet fever, or with measles, &c. I would not, however, have this definition of a poison forced down on our opponents; but we

are justified in assuming that such is the general expression of the fact, as they are all unanimous in adopting the notion of a poison. This notion, perhaps on the theoretical ideas of Cullen, they conceive to be in a great measure substantiated by a fancied parallelism, running through the great organic kingdoms of the animal and vegetable world. Thus, as we have animal (supposed) poisons, to produce continued fever, so we have a vegetable poison which generates the intermittent fever. I would here, however, observe, that there is a very wide difference between the two conditions; for the contagion of typhus is deemed the result of a living action throughout the diseased animal tissues; whereas the poison of the malaria, if any such exist of purely vegetable character, is the unquestionable product of their decomposition—the result of a *dead* action, if that term be not inadmissible.

I take it, then, as an admitted fact, that the established doctrine of the cause of intermittent fever—the doctrine which has ruled the schools for the last century and a-half—is that of a specific poison, either applied to the animal body, or received into its interior through the medium of the first passages. This poison, then, whatever be its ultimate nature, ought to have an unity of character, so that it might produce always the same effect; and ought, it is reasonably inferred, to be as uniform and consistent in its effects as the poison of small-pox, or the inorganic poisons, as arsenic, oxalic acid, or even prussic acid; for, according to the doctrine of Lancisi, it claims the common character of an organic and inorganic poison.

Moreover, when we examine the supposed causes or conditions which affect this supposed poison, we shall find an equal discrepancy of opinion on that subject.

Sir Gilbert Blane, for example, states that the people of Boston, and of the neighbouring villages *in the midst of the fens*, were in general healthy, at a time when the fever was endemic in the more elevated situations of Lincolnshire. To a similar purport is the observation of Montfalcon, that as it (*the poison*) was carried upwards, it became more energetic. On the other hand, a more recent authority, Dr. Ferguson, alleges that it has a peculiar attraction to the soil, and consequently has no disposition to rise into the higher regions.

Not less discordant are the opinions promulgated as to the agency of water on this subtle Protean poison. A small quantity of water, it is asserted by high authority, absorbs and renders it altogether innocuous. In opposition to this, Lancisi states, that thirty people being out boating on the Tiber, the wind suddenly veered round to the south; the consequence of which was,

that twenty-nine out of the thirty were seized with fever. Equally opposed are the authorities of Sir John Lind and Sir Gilbert Blane. On the coast of Batavia, the malaria, according to Sir John Lind, was wafted out to vessels riding at anchor, some five or six miles from the shore; while, in the narrow straits of Holland, Sir Gilbert Blane tells us that none of the seamen were attacked with the fever, which was so fatal to the land forces but a few yards distant.

At Malta, it is affirmed that the effect of the poison on the spot is absolutely *nil*; but its ravages are exerted on the cliffs at a distance, so as to lead to the total abandonment of the village. Again, if we listen to Sir John Pringle and others, just to prove the happy uncertainty of every fact connected with the mazy wanderings of the pathological ghost of malaria, we find him affirming that the ground-floor of

houses where the malaria is disengaged is most deadly, while the floor above is comparatively healthy ; a statement which appears to receive support from the medical history of Up-Hill Park Camp, in the West Indies, and the stations immediately below.

Moreover, one authority asserts that a blank wall is sufficient to arrest the progress of the poison. Another, more aërial in his conceptions, says that it will mount upwards of a thousand feet above the water level, straying for miles away from its original source. A third writer, seemingly more familiar with the habitats of the marsh poison, informs us that the aqueous vapours, in which it is dissolved, are raised during the day by the heat and consequent expansion of the air, and are condensed and precipitated on the adjacent hills during the evening, there to display their pestilential action, leaving the source of their origin perfectly salubrious!

In further illustration of these opposite and irreconcilable doctrines, I shall now submit to you the opinion of two distinguished French physicians, Parent-Duchatelet, and Brachet, physician to the Hotel-Dieu at Lyons. The former, in his able work on Hygiene, amongst other subjects, acquaints us with the results of his investigations on the effects of water in which hemp has been steeped. The fountains of the town of Mans are supplied by water in which this process had been carried on, and a commission was appointed by the Minister of the Interior to examine into certain alleged inconveniences resulting therefrom; which inconveniences were pronounced to be imaginary. A similar opinion had been pronounced by M. Marc, regarding the flax-ponds at Gatteville. To inquire into this subject, on which much difference of opinion was entertained, Parent-Duchatelet zealously devoted himself for about two

years; and he came to the conclusion, that water in which hemp has been steeped is not injurious to the health of those who drink it; that it is not narcotic; and that air charged with emanations from hemp is not improper for respiration. His experiments in confirmation of these opinions were performed on himself, his wife, and family, who, as well as others, drank flax-water, and slept in rooms with damp flax, &c., with perfect impunity.

Let us now hear Brachet on the very same question. He states that the most efficient plan to convert a healthy village into a hot-bed of intermittent fever, is to furnish it with ponds, and steep hemp in them! And such is the expression of the whole line of evidence connected with the marsh miasm question.

Yet, Gentlemen, it is on evidence such as I have just laid before you, that we are called upon to admit the existence of the

malaria—the entity of an agent which affects for its localities every possible diversity of physical condition, showing the most discordant disparity in its effects and in its course; and whose laws—those assigned by its abettors—set alike at defiance the established principles of medical reasoning, and the plainest dictates of common sense. We deceive ourselves, then, wittingly with the mere figments of a word; for, however visionary not a few of the theoretical views presented to the profession have proved, there never was one which teemed with such an endless series of opposing statements and conflicting observations as that which has so long obtained in the medical world regarding the production of intermittent fever from marsh poison or malaria. I shall not attempt to reconcile such jarring elements; and I conceive that, did any person make the effort, he would at once recognise that the assumption of any one

hypothesis was utterly inadequate to explain the phenomena as resulting from one primary cause. He would, therefore, be compelled to abandon the "baseless fabric," till more certain facts could be advanced, and a more positive assurance be offered of, at least, some disposition to consistency and rationality in the explanation of the different phenomena.

After a careful perusal of the histories, and of the circumstances attendant on the general phenomena of the disease, I would observe that, while the main burden of the facts may be admitted, and that fever has manifestly shown itself under the various circumstances referred to, the state of the animal system, as affected by the varying conditions of the external elements to which it was exposed, had been scarcely at all taken into consideration. We should like to have been supplied with full information on the following and similar topics:—the

habitual employment of the individual—his exposure to fatigue—the depressing passions—intemperance—sensual excesses—insufficient or unwholesome food—insufficient or inappropriate clothing—heat and moisture—variations of temperature during the day—difference of thermometrical range between the day and the night—deposition of dew—exposure, after fatigue and excesses, to the cold night air, and the chill fogs of the morning, in gullies and valleys, in warm climates—elevation—and exposure, within the tropics, to strong, cool, and humid, though otherwise healthy, breezes. Still, the progress of inquiry has not remained stationary; and it is now pretty generally admitted, that woods, grasses, and trees, are sufficient to generate the poison, independently of marshes.

The hypothesis of vegetable decomposing matter has, indeed, been generally abandoned, in consequence of the observations

of Ferguson in the Peninsula. He states that fever always appeared similar to the worst form of fever in the West Indies, "whenever, during the *hot* season, any portion of the army was obliged to occupy the arid encampments of the level country, which at all other times were healthy, or at least unproductive of endemic fever." His inferences from these observations were, that the only condition indispensable for the production of the marsh poison, on all surfaces capable of absorption, was the paucity of water, where it had previously and recently abounded:—a condition tantamount to a rapid state of percolation, succeeded by a rapid evaporation; or, in other words, that fevers arise where drought had succeeded to moisture.

Now I have already observed, that no sufficient attention seems to have been hitherto paid to the ever-varying conditions of the mere physical frame, independent of

other important considerations, under the very different and opposite conditions where, at times, it has been exposed to the supposed sources of intermittent fever. For instance, when Ferguson speaks of the rapid evaporation of water in places where it had previously abounded, as the efficient cause of ague, he places this isolated fact before the mind, without any sufficient notice of the highly excited state of the capillary and nervous systems during exposure to the intense heat of an almost tropical climate, and the widely changed condition of the same systems during night, when the solar heat is withdrawn, and the body is subjected, unprotected, to the action of the heavy night dews. He has equally failed to distinguish the extent to which the troops had been previously exhausted by long or forced marches, unwholesome or deficient diet, insufficient clothing, inadequate camp-equipage,

the influence of depressing passions, and numerous other influences, all fertile sources of disease amongst armies in the field.

Propositions of a doubtful character, I am little disposed to moot; but, as the investigations of Bancroft show that *dead putrescent animal matters* will not generate the continued forms of fever; so I believe the same proposition may be reasonably urged against the hypothesis of a *poison* emanating from the decomposition of *dead vegetable matter*. The analogy, indeed, is almost perfect; and it would surely be passing strange, considering the beautiful harmony that pervades the universal economy of nature, if the decomposition of the matters which constitute the grand divisions of the organic kingdom, affected laws diametrically opposed to each other. As regards the poisonous emanations from decomposing vegetable matters, I am not,

indeed, aware that the question has ever been raised and examined with that scrutinizing and philosophic spirit which the late Dr. Bancroft displayed in his researches into the sources of animal poisons.

One hundred and fifty years ago, Lancisi certainly did write on the noxious effluvia of marshes; but his doctrine, though it has governed the tenets of the schools, and influenced the universal body of practitioners, appears to have been adopted, and based on evidence equally problematical with that which supported the other current medical theories of his day. Of his doctrines, I have already submitted to you a brief analysis, less with the view of substantiating my own peculiar theories on the nature of the exciting cause of ague, than with the object of showing that Lancisi's notions, divested of the technicalities and jargon of the era, were vague and complex, (for he seems to have admitted a plurality

of poisons), and decidedly at variance with the supposition of a poison purely of a vegetable character; and equally remote from the idea of a constant and unvarying agent—doctrines which the moderns, on his authority, have so generally adopted and taught.

I have, perhaps, dwelt too long on these discordant opinions; and shall, therefore, but briefly allude to one or two other topics connected with ague and its supposed primary cause. First, I would remark that the putrefactive process is undoubtedly connected with the production of odours; and as a malaria has been found, by the advocates of the doctrine of its existence, in situations so dry that vegetable putrefaction, in the ordinary sense of the term, seemed impossible, they have alleged that it is thereby proved “that the decomposition is, either in degree or in kind, different from putrefaction, though the two may

coexist." On this I would beg to observe, that the admission of a distinctive difference, in the nature of the supposed poison, from the ordinary putrefactive process, advanced by the most zealous supporters of the malaria hypothesis, is in itself another most unequivocal substantiation of the very loose and desultory evidence on which that hypothesis is based. For, even in assuming this difference, essential to their hypothesis, but insusceptible of proof, either directly or indirectly, they testify at once to the weakness of their cause, professing themselves actually ignorant of the very elements of their proposition. Nor is the subject of the origin of ague from malaria rendered less obscure by the oft-repeated statement, that whenever a wet spring is followed by a summer of unusual warmth, the intermittent and remittent fevers are observed to reappear in districts from which they had long been banished by the improve-

ments of agriculture. Surely, if the original fevers arose from an actual miasm, (the supposed genuine source of ague), no difference in atmospheric vicissitudes should have occasioned the renewal of the fever, when, as stated, the generating sources of the poison had been already removed by agricultural improvements.

Before I proceed, Gentlemen, to lay before you my own views, it may prove interesting, as well as instructive, to submit to your notice a summary of the opinions which have at different epochs prevailed among the highest authorities, as to the nature and characteristics of a poison, which has been so generally admitted as the special cause of intermittent and remittent fevers. The ideas successively prevalent thereon, it will be seen, are alike heterogeneous and contradictory, and we might well be permitted to reject some of them as altogether abhorrent to common

sense, and the first principles of sound logic. Still, however wide they may range from the real nature of things, they must all be considered as part of that progress and career, which great truths undergo, in order to the eventual elimination of what may be deemed the *constants* of medical science.

SUPPOSED ORIGIN OF THE POISON.

Varro, Columella, and Vitruvius, sixteen or eighteen hundred years ago, seem to have entertained the idea that these forms of fever could legitimately be referred to an animalcular source.

Lancisi, in 1695, passing over the long and unsatisfactory interregnum of the dark ages, ascribed the origin of these maladies to a marsh miasm, including therein effluvia both of *inorganic* and of animal constitution. This doctrine, first propounded 150 years ago, has prevailed

almost unquestioned in the medical world up to the present hour.

Elliotson attributes the origin of the malarious poison invariably to the agency of *decomposing vegetable matter*.

Ferguson denies the necessity for *vegetable decomposition*, but ascribes the disease to rapid evaporation of water in *an arid soil*.

Annesley conceives that the poison is the product of elements which exist in *a rich soil*, subjected to the conjoint agency of the sun, air, and moisture.

Watson maintains the cause to be a *specific poison*, and concurs generally with Ferguson.

Armstrong rejects altogether the hypothesis of a specific poison.

Tulloch delivers it as his opinion, that marsh miasma and ague do not stand in the relation of cause and effect to each other.

Murray (Inspector-General of Hospitals), to obscure further the cloud of statements, avers that fevers, analogous to those arising in marshy lands, frequently result from the application of intense solar or atmospheric heat.

The British and Foreign Medico-Chirurgical Review finally sums up the evidence on the debated question in the following emphatic and not very flattering language:—"We believe that we are as yet *in utter ignorance* of the nature of the agent or agencies represented by the conventional term, *malaria*, or *marsh poison*.

CHARACTERISTICS OF THE POISON.

Rises to the summits
of mountains. (MONT-
FALCON.)

Does not rise into the
atmosphere, but has a
peculiar attraction for
the earth's surface.
(FERGUSON.)

Is stopped by the intervention of a wall. (Various Authors.)

Attacks persons on the water; *e. g.*, Rome and Batavia. (LANCISI and SIR JOHN LIND.)

Has its site on dry limestone rocks; *e. g.* Trichori. (MACMICHAEL.)

Is connected with a dreadful smell. (SIR JOHN PRINGLE.)

Distinct case of ague from repeated immersion in the waters of a running stream. (Example, the River Loire.)

Is distinctly communicated by contagion. (Parisian Case. DR. FORDYCE.)

Has a great attraction for trees, and is arrested by them. (Various Authors.)

Is prevented or checked in its progress by water; *e. g.*, the dykes of Holland. (SIR G. BLANE.)

Is prevalent only where marshes are to be found. (Common Doctrine.)

A most rank and noisome odour arises from decomposing vegetable matter, but no ague. (At New Amsterdam.)

Is produced only by the poison from marshes. (General Doctrine.)

Is not infectious. (Common doctrine.)

Ague is convertible into common continued fever, and *vice versa*. (DR. FORDYCE, SIR JOHN LIND.)

Ague and continued fever are not mutually convertible. (Abstract Theorists.)

The poison is more intense in proportion to the proximity of its sources; the elevation even of a *few feet* affording some security from its effects. (JOSEPH BROWN and others.)

The poison produces no effect on the spot where it is generated! but desolates distant and elevated localities, as at Malta. (MACCULLOCH and others.)

Such are the opinions, Gentlemen, enunciated by the highest authorities, on the origin and laws of the marsh miasm, and on which they claim our assent to the doctrine of a *specific* poison, as the efficient cause of intermittent and remittent fevers.

LECTURE V.

EVIDENCE TO BE DERIVED FROM THE AUTHOR'S PERSONAL EXPERIENCE—DESCRIPTION OF CITY OF BAHIA—ITS SALUBRITY—EQUABILITY OF TEMPERATURE—PHYSICAL AND MORAL CONDITION OF THE BRAZILIANS—THE AUTHOR'S APPOINTMENT TO THE BRITISH HOSPITAL IN BAHIA—FREQUENCY OF AGUE AMONG CONVALESCENT PATIENTS—CASES IN THE HOSPITAL—CASES IN PRIVATE PRACTICE—VILLAGE OF SAN LAZARO—AGUE, INFLAMMATORY FEVER, AND TYPHUS, OCCURRING TOGETHER IN THE SAME FAMILY—THE AUTHOR HIMSELF ATTACKED WITH AGUE—INTERMITTENT FEVER PROBABLY REFERABLE TO EXPOSURE TO MOIST SEA BREEZES—CONSEQUENT PROPHYLACTIC MEASURES ADOPTED IN HOSPITAL AND IN PRIVATE PRACTICE—FAVOURABLE RESULTS.

GENTLEMEN,—The observations which I am now about to submit to you, and the comments which they suggest, rest partly on my experience as an army medical officer in different quarters of the globe, and partly on the evidence afforded by the

superintendence of the British Hospital at Bahia for twenty-three years, joined to an extensive private practice there, during the same period. A brief description of the city and the adjoining districts is therefore essential, as introductory to the new views which I have been led to adopt. The facts on which these views are based I shall distinctly narrate as they were presented to me. The inferences drawn may, like every subject not determined by the rules of mathematical evidence, be the occasion of a difference of opinion; to myself, however, they seemed to be the only rational conclusion that could be adopted; namely, that *marsh miasmata and malaria are not the efficient cause of intermittent and remittent fever.*

DESCRIPTION OF BAHIA; ITS SALUBRITY;
CAUSES OF ITS BEING HEALTHY.

The city of Bahia, the former capital of

Brazil, is situated in latitude $12^{\circ} 59'$ S., and longitude $38^{\circ} 33'$ W., at the entrance to one of the most beautiful and extensive bays in the world, extending thirty-three miles in length, from north to south, and thirty miles in width from east to west, and containing upwards of a hundred islands, several of which are inhabited. The city contains about 150,000 inhabitants; the population in 1803 being estimated at 103,000. It may be said to form two divisions, the one constituting the upper, and the other the lower city. The former division is built on a ledge of rock (chiefly gneiss, passing often into grey primitive greenstone and syenitic granite), about six hundred feet in height, and overhanging, as it were, the lower division. The latter, built on an alluvial soil, with a rocky substratum, stretches along the base of the hill, on ground gained, in part, from the bay.

The streets are irregular, ill-paved, generally narrow, and having a gutter in the middle, into which is commonly cast the filth and offal of the adjacent dwellings. The houses are unprovided with water-closets, and are otherwise ill-arranged for the purposes of ventilation and comfort. The slave population is numerous and crowded, the police inefficient, and a scavenger department unknown. The public sewers are few, the *becos* (alleys) leading from the principal streets serving as temples of Cloacina, and as receptacles for every kind of filth. In addition to this, the dead are interred within the churches and precincts of the city. I am, however, informed, that since my return from Brazil, in 1842, the state of affairs here alluded to has undergone considerable amelioration, through the advancing enlightenment of the age, and the more energetic efforts of the public authorities.

Partial, though heavy rains, occur at all seasons of the year, by which the lower city is inundated, and contaminated by the filth and offal of the upper city. In some of the most public thoroughfares, as the "Conceição," for example, there are heaps, or rather mounds, of decaying animal and vegetable matters, fermenting under the powerful rays of a tropical sun, disengaging every kind of noisome effluvia, and often of such an offensive character as to impress the sense of taste scarcely less powerfully than that of smell. The soil, too, in the immediate neighbourhood of the city, is absorbent, deep, and rich, with a substratum of clay.

Here, then, we have, accumulated, in almost unexampled abundance, all those physical conditions which are deemed, by the unanimous consent of physicians, to constitute the elements essential for the generation of the most deadly scourges of

humanity—epidemic and endemic diseases. I may further add, that I have myself, within the last twenty years, witnessed the city exposed, on three several occasions, to the combined horrors of siege and famine, with all their revolting contingencies. Yet, notwithstanding this appalling combination of physical, moral, and social evils, universally admitted as the chief agents in producing the most extensive and fatal diseases, Bahia continued healthy; and can, moreover, up to the present hour, boast the happy privilege of having escaped, since the period of its foundation, from every species of endemic or epidemic malady—*yellow fever, cholera, influenza, typhus, and dysentery*.*

* Since these pages were written, a desolating epidemic, said to be the *yellow fever*, has swept over the chief cities of Brazil. It is, I believe, generally considered by the profession to have been imported from abroad; but I trust that, ere long, we shall be supplied with full and accurate information as to its origin, progress, symptoms, and treatment. It would appear,

It is, moreover, deserving of notice, as regards the general health of the inhabitants of the upper and lower divisions of the city, that the Brazilian and Portuguese merchants and shopkeepers, who inhabit the lower division, and who rarely quit their dwellings, enjoy comparative immunity from intermittent fever; while those whose duties frequently lead them from the lower to the upper city, often suffer from this disease, as well as from continued fever (*constipacōn*), without other apparent cause than the sudden transition from the warm, but equal temperature of the lower city, to a strong, cool, and humid sea-breeze, which they encounter while bathed in perspiration and exhausted by the labour of ascending a considerable eminence.

from the writings of an old Brazilian historian, Sebastiano da Rocha Pitta, that, in the year 1686, a somewhat similar epidemic committed great ravages in the cities of Bahia and Pernambuco, and persisted for five or six years. The fact is also mentioned by Humboldt.

How, then, are we to explain this most singular salubrity of the city of Bahia? For while most of the ordinary endemic and epidemic diseases are found to occur sporadically, and sometimes in their most intense and deadly form, yet it is matter of history that they have never spread endemically.

EQUABILITY OF TEMPERATURE. I am inclined to believe that the true solution of this remarkable phenomenon is to be found in the extraordinary equability, and limited range of the temperature, the prevalence of a gentle but never-failing breeze, and the freedom from atmospheric vicissitudes enjoyed by Bahia beyond any other city with which I am acquainted. *The highest range of the thermometer in the upper city (with the exception of an occasional day,) never exceeds $82\frac{1}{2}^{\circ}$ of Fahrenheit in the summer, and the lowest in winter is 72° ; there is a further difference of two degrees*

between the upper and the lower city; and the extreme daily range is about six degrees. This unexampled uniformity of temperature is chiefly to be ascribed to the absence of any high or mountainous ranges, and of all arid and sandy deserts, aided by the genial influence of refreshing showers at all seasons of the year. It is further promoted by the perpetual verdure of the country, and by a cool and powerful monsoon, laden with moisture, and sweeping along the coast direct from the southern Atlantic. This monsoon prevails from the north-east for eight months of the year, namely, from September to April, constituting the dry or summer monsoon; and during the winter or rainy season, from May to August, it blows from the south-east. These periods are, however, liable to some irregularity.

The nights in Bahia are also usually serene and beautiful, and unattended with much deposition of dew. Unlike what

occurs in other hot regions, the delightful serenity and coolness of tropical moonlight may be enjoyed with perfect impunity, the mind being undisturbed by those visions of fever and malaria which float before the imagination in less favoured climates. Of this we have the best practical illustration in the number of foreigners, as well as natives, at Bahia, who pass a large portion of the summer nights in the open air.

Another subject of vast importance to the European in tropical climates must not be forgotten; namely, the sound and refreshing sleep which may always be enjoyed at Bahia; the nights being invariably cool, even in the midst of summer. If proper precautions be observed to avoid exposure to direct currents of air, the windows of the sleeping chamber may be left open with impunity at all seasons of the year; the terrors of malaria, and the hot, suffocating winds of other tropical climates, being

altogether unknown. I am not, indeed, aware of any circumstance which exerts a more important influence in enabling the European constitution to resist the deleterious effects of tropical climates, than the enjoyment of sound and refreshing sleep.

CONDITION OF THE PEOPLE.—Among other causes which must be held as important in modifying the general character of diseases in Bahia, and probably everywhere else, the physical, social, and moral condition of the people deserves special consideration. The native Brazilian is in general compact and well-formed, and of healthy organization, but not of an athletic frame. His intellectual faculties are acute, though little developed by cultivation. Descended from European ancestors, he has still a considerable admixture of African and native American blood. He is indolent by nature, and indisposed for active exertion or industry; but he is protected against the evil

influence of the former on his health, by a simple and abstemious diet; and the injurious consequences of the latter to his social position are obviated by the circumstance, that the four great wants of the humbler classes in Europe press but lightly on the Brazilian. Fuel he scarcely requires; of clothing, but little; his primitive habitation is simply constructed; and one day's labour will amply provide for the moderate demands of the whole week. With passions naturally quick, he is nevertheless placable; his disposition is kindly; the future never disturbs him with its doubts, nor the past with its regrets; the struggles and vicissitudes of European life are unknown. The contentions of party, the yearnings of ambition, the bitterness of fanaticism, never disturb his repose; and after gliding down the stream of time, unscathed by those tumultuous passions and harassing cares which so frequently embitter the existence and

undermine the constitution of man in other countries; he meets at length the inevitable doom, if not with philosophy, at least with resignation; satisfied of his claims to eternal felicity, in the confident assurance of an infallible Church.

Such is, or rather was, the Brazilian. I speak of the masses, for the higher classes in all countries pretty closely approximate. But the premature, though well-intentioned, introduction of political institutions, unsuited to the material interests or intellectual advancement of a mixed population like that of Brazil, has already operated, and will probably continue to effect still more important changes on the habits and social condition of the people. These questions, however, important as they are, I must leave to the political economist and the historian. We have only to do with the past and the present, in our attempts to elucidate the causes to which Bahia has

hitherto owed her singular and happy immunity from those endemic and epidemic scourges which too frequently desolate some of the fairest regions of the earth; and where, from similarity in geographical position, the inference of similarity in disease would appear to be almost legitimate.

From the preceding account of the climate and inhabitants of Bahia, we should naturally be led, *à priori*, to conclude that disease would there present a mild and tractable character; and it is highly interesting to find that in such conclusion we are borne out by the concurring evidence of experience.

STATEMENT OF FACTS WHICH FIRST LED THE
AUTHOR TO DOUBT THE CAUSAL RELATION
OF MARSH MIASM TO INTERMITTENT FEVER.

In the year 1819, I was appointed to the British Hospital at Bahia. When I commenced the duties of this establishment, it

was reported to me that the locality was considered unhealthy, as patients received into the Hospital were afterwards subject to be attacked, during convalescence, with intermittent fever. Reports of this nature necessarily caused me uneasiness; and I accordingly investigated, with much attention, all the internal arrangements of the Hospital, as well as the immediate neighbourhood. Nowhere could I discover any obvious causes of intermittent fever. There were no marshes; and the Hospital, built on an eminence, had well-arranged and spacious rooms, and was fully exposed to the current of a strong and regular monsoon or sea-breeze. Such being the case, and my mind being fully imbued with the universally-received notions regarding the origin of intermittent fever from marsh poison, I discredited the information as totally unworthy of notice. In a few weeks, however, after I had entered on the duties of the

Hospital, my attention was unpleasantly arrested by the occurrence of a well-marked case of ague, the history of which may be here briefly related.

CASE.—A seaman, in the prime of life, and in robust health, was admitted into the Hospital, labouring under virulent gonorrhœa, which demanded the employment of very active antiphlogistic measures, including general and local blood-letting, very low diet, &c. In the course of three weeks, and when the acute symptoms had entirely subsided, the patient was suddenly seized with a violent shivering fit, which lasted some hours, and was followed by strong reaction; the paroxysm being carried off by profuse perspiration. This incident struck me forcibly at the moment. I attributed it, however, notwithstanding the protestations of the patient, to some imprudence in diet or otherwise; and, perceiving that the fever gradually subsided, I felt satisfied of the

justice of my suspicions. On the third day, however, and precisely at the same hour, the patient was seized with a paroxysm exactly similar to the former, only much more violent. In short, intermittent fever was established; and it continued, notwithstanding the administration of ordinary remedies, until the period of the patient's embarkation in his vessel, for England, about five weeks after his admission into Hospital, and twelve days after the commencement of the ague. This case caused me much embarrassment at the moment, as inexplicable by the received doctrines—the more so, as the patient had not only never suffered from ague, on any previous occasion, but had never, according to his declaration, been confined for a single day by illness during the whole course of his life.

Another case, occurring shortly after the above, afforded further grounds for reflection.

CASE.—A seaman, likewise in the prime of life, was admitted into Hospital with severe rheumatic inflammation of the shoulder-joint, brought on by exposure to wet and cold during the voyage from England. This man was of a full plethoric habit; the shoulder was red, inflamed, and considerably swollen; and there was strong constitutional disturbance. Under these circumstances, a rigorous antiphlogistic regimen, including general and local blood-letting, &c., was adopted. By these means, the local and constitutional symptoms were rapidly subdued, and the patient was pronounced convalescent on the seventh day. On the morning of the tenth day, while examining another patient in the ward, I was called to this man, and found him trembling violently, and exhibiting all the ordinary appearances of the cold stage of intermittent fever. The disease was arrested after the third paroxysm, by the exhibition of cinchona bark.

It is unnecessary to multiply cases; they were unfortunately but too numerous; and I quickly learned, by the painful tuition of experience, that, in the British Hospital, patients convalescent from any disease—more especially from rheumatism—whose strength had been much and suddenly lowered, became exceedingly obnoxious to attacks of intermittent fever, which proved almost invariably of the tertian type.

There was not a deficiency of cases of the same character in my private practice. The following may be offered as an illustration:—

CASE.—Mrs D., wife of a British merchant, lately arrived from Europe, in the prime of life, and married but a few months, had been out at a *fête champêtre* on the sea coast. She danced, and perspired, freely; and after the excitement of the day, was exposed to the cool sea breeze of the country, while returning home in her *cadeira*—a

sort of palanquin peculiar to Bahia. The result was a sharp attack of inflammatory fever, apparently *gastro-enterite*, which required a strict antiphlogistic regimen, but no blood-letting. On the seventh day, Mrs. D. was deemed convalescent; she was so perfectly free from all ailment, that I informed her husband that every source of uneasiness had now been removed. Scarcely six hours, however, had elapsed, after this declaration, when I was hastily summoned, and found Mrs. D. in a state of high febrile excitement, having had previous rigors of nearly two hours' duration. I, perhaps naturally enough, notwithstanding the strenuous denial of the patient, ascribed her relapse to some irregularity of diet or other imprudence. My friend and colleague, Dr. Silveira, physician to the Mizericordia Hospital, and subsequently physician in ordinary to the Emperor, was called in consultation, and was as much embarrassed

as myself satisfactorily to account for the sudden accession of fever. We prescribed some saline medicine; and on the following morning were gratified to find our patient, after copious perspiration, free from fever, and complaining of nothing beyond debility. We accordingly again pronounced her convalescent. On the afternoon of the following day, however, at the same hour, and almost at the same minute, an attack, almost similar to the preceding one, came on; in short, intermittent fever was duly established, and the patient was restored to health, in the course of two or three weeks, by the ordinary tonic treatment.

VILLAGE OF SAN LAZARO.—I shall now, Gentlemen, direct your attention to the history of the *Povoação*, or small village, of San Lazaro. This place is situated about two miles southward of Bahia, on the verge of the peninsula, close to the sea

shore, on a bold headland, and fully exposed to a strong and regular monsoon. The country around is very beautiful; the sea view magnificent. It is therefore chosen as the favourite evening ride of the British and other foreign residents. It is also frequently resorted to, for the day, by parties of pleasure, who return to the city in the evening. In this village there was only one good house, which belonged to a Portuguese merchant, and which, being unoccupied by the owner, was always at the disposal of such foreign visitors as might require it. The house was large, situated on the brow of a hill, and facing the sea. A spacious open verandah extended along the entire front of the building, through which the cool sea-breeze rushed with great force. Nevertheless, the house was by universal consent deemed very agreeable, as was also that of Colonel Cid, which stood on a neighbouring eminence;

while some small cottages, at the base of the hill, and sheltered in a great measure from the sea-breeze, were considered healthy.

Mr. R., one of the resident British merchants, requested my opinion regarding the character of the house in question, as he was desirous of removing his family there for the purpose of sea-bathing. Guided by the same principle as had influenced my decision regarding the healthy character of the British Hospital, I stated that there appeared no rational grounds for giving the house so bad a name; and acting on this opinion, Mr. R. and his family removed there on the following day. This gentleman's household consisted of himself, Mrs. R. his wife, Mr. M. his brother-in-law, a youth of 17 years of age, recently arrived from England, an English housekeeper, and five black servants.

On the fourteenth day after their re-

moval to San Lazaro, the state of the household was as follows:—Mr. and Mrs. R. and one of the black servants were laid up with ague; another servant with dysentery; Mr. M. a severe attack of inflammatory fever, with great determination of blood to the lungs; and Mrs. C. the housekeeper, a woman of melancholic disposition, and weak leuco-phlegmatic habit, was attacked with genuine typhus.

The whole establishment was consequently removed back to Mr. R.'s house in the city, where Mr. M. advanced quickly to convalescence under active antiphlogistic treatment, as did also the two black servants under the ordinary measures. Mr. and Mrs. R. recovered more slowly, but without any serious inconvenience; while the condition of Mrs. C. the housekeeper, became perilous in the extreme. She had a weak and rapid pulse, intense heat of skin, oppression at the chest, dry tongue

with dark fur, and sordes about the teeth and lips, tenderness of the abdomen, offensive and depraved secretions and excretions, low muttering delirium, and petechiæ on the surface of the body. This patient finally recovered, but with great difficulty, having been confined to bed during thirty-three days. My friend, Dr. Silveira, saw this case repeatedly in consultation, and admitted that he had never witnessed a more exquisitely marked case of typhus. Of this he was certainly a most competent judge, having graduated at Edinburgh, where he resided several years, and performed the duties of clinical clerk at the Royal Infirmary. He is, moreover, a gentleman of great professional talent and scientific acquirements, and well merits the high office to which he has since been appointed by the imperial government.

The case just described sufficiently proves a fact which has often been questioned, par-

ticularly by Bancroft—the possibility of the occurrence of typhus, genuine typhus, in tropical climates.* And the different diseases from the same exposure, with which the several members of the above

* In some seasons, sporadic cases of typhus—genuine typhus—are by no means rare in Brazil, and are popularly termed “febres malignas.” They commence either as ague, or as remittent fever, or as a “constipacao,” (in Portuguese), which resembles the synocha of Cullen. These cases are often fatal, and commonly run a more rapid course than the typhus of Europe. The “constipacao” or continued fever, is a frequent, perhaps the most frequent, form of fever in Bahia, and usually runs its course in from five to seven days, terminating in free perspiration. The “constipacao” is rarely or ever fatal, unless, as before stated, it should run into “maligna;” but it not unfrequently lapses into ague, which commonly assumes the tertian type, and yields readily to treatment. The so-called typhoid fever is also not unfrequent, especially at Rio de Janeiro, and has been well described by Mello Franco, (*Ensaio sobre as febres do Rio de Janeiro*, Lisboa 1822,) and subsequently with its abdominal lesions, by Sigaud, physician to the Emperor, and by Drs. Valadao and da Costa, as it appeared in their hospitals of the Mizericordia, and the Marine.

family were assailed, afford a striking illustration of the influence of secondary causes in determining the character of the subsequent disease. Mr. M., young, robust, and just arrived from Europe, had an inflammatory attack with determination of blood to the lungs; Mr. and Mrs. R., who had resided for some years in the country, and had suffered somewhat from climate, were affected with ague; Mrs. C., the housekeeper, who had also been some time in the country, and who was weak and of a melancholic and leuco-phlegmatic temperament, was attacked with typhus; and of the black servants, one suffered from ague, and another from dysentery.

On a previous occasion, while serving on the medical staff in the unfortunate expedition against New Orleans in 1814—15, I also witnessed a broad illustration of the above principle. Returning from the army, with some officers of H.M. 14th Light Dra-

goons, we were exposed for three or four days, in a frigate's launch, to cold and wet, and other privations. On reaching the fleet at Cat Island roads, all, except one, were suffering, and each from a different malady. The midshipman who commanded the boat was attacked with otitis, and became perfectly deaf; one of the officers was attacked with dysentery; another with ophthalmia; a third with fever; and myself with rheumatism, to which I have since up to the present hour been occasionally liable. And all these diseases originated in one and the same cause. In fact, you will often find that the diseased action, apt to follow on the application of any of the ordinary causes of disease, often depends less on the special nature of such cause, than on the internal condition of the animal and organic functions, both at the time of application of the cause, and for some time previously. There are few diseases, perhaps none, which

can be said to arise instantaneously; and it is more than probable that the preceding condition of the individual, and the state of more or less marked *malaise*, which is to be detected on close investigation, must essentially contribute to guide and direct (if I may use the expression) the exciting cause to the development of one form of diseased action in preference to another. Thus the type and symptoms of fever will assume different forms at different seasons and under different circumstances. At one time they will be displayed through the air-passages; at others through the nervous system, the alimentary canal, &c.; yet, strange to say, a difference in the febrile poison has even been assumed to account for these several varieties. Names and theorems, Gentlemen, have too often usurped the field of observation, setting aside the wide and unbounded expanse of nature as daily presented to our notice, ungarnished

by the sophistry of the schools, or by the dialectic craftsmen, who labour to bend, twist, and torture natural phenomena to the pet theory of the closet, and, like Procrustes, endeavour to reduce each and every form of diseased action to their own standard.

DR. DUNDAS'S CASE. In the year 1822, I became myself the subject of intermittent fever. The ague arose as follows. Our envoy to the court of Brazil having stopped for a few days at Bahia, I had occasion to accompany him on some excursions around the city; during which, in addition to my professional duties, I was much exposed to the sun. On one day in particular, after lengthened exertion and exposure, I felt exhausted, and went into a tepid bath previously to dressing for dinner. Finding the bath more than usually grateful, I continued in longer than was my custom, and

until warned by a slight sensation of cold. I then quickly dressed, and went out to dinner in my ordinary health. During the night, however, I was seized with a violent shivering, which terminated in tertian ague of the most obstinate character, resisting every mode of treatment for a period of fifteen months. My health finally became so seriously deteriorated, as to make it imperative that I should have leave of absence in order to proceed to the Organ Mountains, in the province of Rio de Janeiro, for change of climate. This I was enabled more readily to accomplish through the kindness of the commander-in-chief on the station, the late gallant Sir Thomas Hardy, who not only afforded me a passage in H.M.S. "Fly," but permitted his flag-surgeon, Mr. Neill, and subsequently Dr. Birnie, of H.M.S. "Tartar," to take charge of the British Hospital during my absence;

and, though late, I am happy in being afforded the opportunity of acknowledging my deep obligations to these gentlemen.

PROBABLE CAUSE OF AGUE. Though, up to this period (1822), I was in possession of facts sufficiently strong to raise serious doubts in my mind as to the source of intermittent fever, I nevertheless adhered to the orthodox doctrine, that ague originated only from the agency of marsh miasmata. My faith had, however, become wavering, and whilst in this state of suspense, I was called to attend the family of an intelligent Brazilian merchant, residing in the *Saude*, whose wife and two children, the latter aged respectively seven and ten years, were affected with intermittent fever. Their house was spacious, in one of the highest quarters of the city, and fully exposed to a strong sea breeze, a locality which would *à priori* be pronounced unexceptionable,

and especially exempt from all chance of *malarious agency*. The occurrence of three cases of ague under such circumstances (the more remarkable, as the female members of Brazilian families but rarely quit their habitations), afforded additional and very strong reasons for further questioning the received theory of the origin of ague from malaria. In conversation, I stated my doubts to the head of the family, who immediately replied that the house was *muito sezonatico*, (very aguish), and that he would in consequence be obliged to remove. On my demanding his reasons for considering the house "agueish," "Oh," said he, "every house in Brazil like this—*muito banhado do vento*," (literally, much bathed in wind), "is unhealthy, and especially subject to *sezoas*," (ague)—an opinion which I found fully supported by the popular voice, and which occurred to me as affording the only satisfactory explanation

of the cases of ague in my own hospital, as well as of numerous others of a similar nature.

On taking, then, a general view of the circumstances connected with the numerous cases of intermittent fever which had so unaccountably occurred to convalescents in my hospital, and in private practice, the conviction was forced upon me, in a manner altogether irresistible, that these cases *did not arise from marsh poison, but sprang directly from imprudent or too early exposure to a strong current of cool sea-air, loaded with moisture, while the system was under the debilitating influence of previous exhaustion or disease.* The capillary circulation is thus deranged, through the impaired energy of the nervous system, and the molecular and vital changes between the blood and tissues are impeded;—circumstances of paramount weight in inducing diseased action, and which the more readily

arise, from the high degree of morbid sensibility acquired by the cutaneous system in tropical climates.

I may here state, that I had already observed several most obstinate cases of ague apparently originate in the sudden influence of the depressing passions, at a time when the system had been weakened by profuse and incessant perspiration, and consequently rendered susceptible of the slightest atmospheric or *moral* vicissitude.

Having adopted the above view of the subject, I proceeded at once to act upon it, and to test its truth by the unerring criterion of experience.

PROPHYLACTIC MEASURES.—In the first place, I made several alterations in the wards of the hospital, and had those windows nailed up *which admitted directly* the strong current of the sea-breeze. At the same time, effectual measures were taken to prevent convalescents from being exposed

without sufficient warm clothing, or until they had regained a certain degree of strength. The result of these measures was immediate, and the evidence they offered was complete, and apparently free from all fallacy. From that time, intermittent fever almost completely disappeared from among the convalescents in the British Hospital.

The adoption of similar precautionary measures in my private practice was followed by equally striking and conclusive results. The sudden attacks of ague, which had formerly been so frequent among my convalescent patients, and which had caused me so much perplexity and embarrassment, ceased altogether, or rather became of rare occurrence.

In my own case, too, the result was no less conclusive. After having had my health broken up by an ague of fifteen months' duration, and having had the dis-

ease arrested by my journey to the Organ Mountains; and although I have subsequently been long and often exposed to the ordinary exciting causes; yet, by carefully guarding against exhaustion of the general system, and derangement of the cutaneous circulation, I remained free from a recurrence of this disease, so liable to return, throughout my subsequent residence of twenty years in Bahia.

In my next lecture, Gentlemen, I shall offer strong additional testimony, based on the evidence afforded by certain districts in the immediate vicinity of Bahia, to show not only that marsh effluvia and intermittent fever do not stand in the relation of cause and effect, but that, in reality, the latter has no necessary relation to the former.

LECTURE VI.

DESCRIPTION OF THE SUBURB OF BOMFIM—EXPLANATION OF ITS FREEDOM FROM AGUE—THE VICTORIA—EFFECTS OF REMOVAL OF A WOOD—EFFECTS OF COOL AND HUMID WIND ON AN EXHAUSTED CUTANEOUS SYSTEM—DR. DUNDAS'S PERSONAL EXPERIENCE IN ITALY—RELAPSES IN AGUE AFTER LONG INTERVALS—ALLEGED CAUSES EXAMINED—PROBABLE CAUSE, AN IMPERFECT RESTORATION TO HEALTH AFTER FIRST ATTACK—DR. W. F. DANIELL ON WATER TREATMENT IN FEVER—TYPES OF FEBRILE DISEASE—THEIR ESSENTIAL IDENTITY, AND MODIFICATION UNDER CERTAIN CIRCUMSTANCES OF CLIMATE, &c.—YELLOW FEVER IN BRAZIL—TRANSITION FROM BILIOUS REMITTENT INTO IT—DR. CROKER PENNELL BELIEVES THE REMITTENT AND YELLOW FEVER IDENTICAL—DR. R. PATERSON THINKS THEM DIFFERENT.

AT our last meeting, Gentlemen, I laid before you the evidence by which, in oppo-

sition to my previous views, I was led, step by step, first to doubt, and finally to reject, the received doctrines of the schools, in regard to the origin of intermittent and remittent fever from marsh poison or malaria. In my present lecture, I purpose to submit to you the additional evidence afforded by other districts in the immediate neighbourhood of Bahia, in proof of the fact that, in certain localities, you may have the most intense and concentrated effluvia of marshes, without ague; while, on the other hand, you will often find ague exceedingly prevalent under circumstances where marsh miasm is altogether out of the question. In confirmation of these propositions, I now proceed to the evidence afforded by the suburb of Bomfim.

DESCRIPTION OF BOMFIM.—Bomfim adjoins the city of Bahia to the northward. It may be described as extending about a mile and a-half from the *Agua de Meninas*,

along a broad and well-paved causeway, passing through the centre of an extensive morass, and terminated by a gentle eminence, on which stands the beautiful and, in Brazil, celebrated chapel of "Nossa Senhora do Bomfim." Along this causeway is interspersed an almost uninterrupted succession of houses and gardens, with patches of cultivated ground, gained from the wide-spreading morass which surrounds them.

The morass, in the midst of which stands the populous suburb of Bomfim, is bounded to the west by the Bay of Bahia, and to the northward and eastward, or *windward*, by a high ridge of hills, which form an imperfect semicircle, separating the morass from the South Atlantic Ocean. It is partially subject to the influence of the tide, and consequently exposed to those deleterious effects which are believed to result from the intermixture of salt and fresh

water, together with immense quantities of vegetable and animal matter, exuviae, &c., constantly acted on by the powerful influence of a tropical sun.

Now, Gentlemen, in the course of my somewhat eventful professional career, I have visited the malarious localities of the Peninsula, and the more deadly shores of Africa and the West Indies; I have bivouacked with the British army amidst the dreary swamps of Louisiana, and have traversed the more classic Pontine marshes near Rome; and I can safely assure you, that in none of these do the elements commonly deemed necessary for affording to marsh effluvia their most concentrated and deadly degree of intensity, so plentifully abound as in the district of *Nossa Senhora do Bomfim*. Yet what is the character of this pestiferous-looking swamp in regard to public health? That it is uninhabitable? Just the reverse. The suburb of Bomfim

enjoys the reputation of being, and is in reality, at certain seasons, one of the most healthy districts in Brazil. The higher classes of the inhabitants of Bahia resort to it for the purpose of sea-bathing, during the four hottest months of the year—*December, January, February, and March.* These are the months when the sun, nearly vertical, exerts its greatest power—when the swamp is partially dried up—but still a swamp; exposed to the ebb and flow of the tide, and superabounding, as already stated, in all the elements necessary to the generation of the most abundant and deleterious miasmata. Such is the season at which Bomfim is thronged with visitors; and these, moreover, in accordance with the custom of the country, pass a large portion of the night in the open air, exposed to the influence of the poisonous miasms—if such there be. Yet, during these four months, a case of intermittent fever is

almost unknown; while the towns, Taboaõ, Rio Vermelho, &c., situated on the sea-coast, at some miles to the northward and eastward—that is, to windward—of the hills bounding the Bomfim marsh, and consequently excluded from its influence, but exposed to the full sweep of the humid sea-breeze from the Southern Atlantic, are notoriously subject to ague at all seasons of the year.

How, then, are we to explain the prevalence of ague in towns lying on the sea-beach, and constantly bathed by a powerful sea-breeze; while Bomfim, situated a few miles to *leeward*, is, though placed in the midst of an extensive marsh, wholly exempt from intermittent fever, except at certain seasons? The solution, I conceive, will be found in the ridge of hills before alluded to, which breaks the force of the strong humid sea-breeze—tempering, but not excluding it; and thus secures, during the summer

season, an equality of temperature rarely met with in other regions of the globe.

Again, the history of Bomfim, during the rainy or winter season, affords the strongest additional evidence in proof of the correctness of the explanation here offered. The wet season commonly sets in about the beginning of April, with a sudden change in the monsoon, when atmospheric vicissitudes—considerable for Bahia—often ensue; the rain descends in torrents, and converts the Bomfim marsh into one entire sheet of water; and, above all, the change of wind from about north-north-east to south-south-east at once deprives Bomfim of the protection of that range of hills already so often alluded to, and exposes it to the full force of a powerful monsoon, direct from the Southern Ocean. Under such circumstances, agreeably with the received doctrines, all noxious exhalations must be effectually arrested; yet what, in reality, do

we now find to occur? We find that Bomfim, having been almost perfectly free from intermittent fever during the summer months, when it *ought* to have been absolutely uninhabitable, now becomes subject to that disease. How, then, are we to explain an anomaly so striking, and so inconsistent with the ordinary doctrine regarding the origin or production of ague? The following, I apprehend, will afford the correct solution.

In addition to the large amount of evaporation from such an extensive expanse of water, the powerful south-east monsoon, loaded with moisture, but necessarily free from any miasmatic contamination, now sweeps in, without the slightest impediment, direct from the Atlantic; and Bomfim is thus placed under precisely similar circumstances to those of Taboaõ and Rio Vermelho, the towns on the sea-coast above alluded to; and the results, as regards the

health of the inhabitants, are precisely similar, and continue until the change of the monsoon to the north-east, in October, again restores to Bomfim its former hilly screen, and, at the same time, its wonted salubrity. The few permanent residents, chiefly of the lower classes, who continue to inhabit this district throughout the year, present the ordinary characteristics—though not strongly developed—of the dwellers in low and humid localities.

It is especially worthy of remark, that, along the summit of the semicircle of hills, already described as bounding the marsh to windward, there are numerous habitations, entirely unsheltered, and constantly swept by a powerful sea-breeze, direct from the ocean. *All* these, without exception, are greatly subject to ague at *all* seasons of the year; while, as already stated, the houses placed below, in the centre of the swamp, are only affected at certain seasons,

—these seasons being precisely those during which the production of marsh exhalations must be deemed entirely suspended.

In addition to an equable temperature, I deem the uninterrupted prevalence of moderate breezes, by which a stagnant or even calm state of the atmosphere is prevented, to be of the very highest importance in obviating disease in all warm, low, and humid localities. I am, indeed, satisfied that, were the district of Bomfim subject to the calms and vicissitudes which I have witnessed on the coast of Africa, in the south of Europe, and in America, it would prove not less destructive to human life than the charnel-houses of the Coast, and the West Indies.

What impression the proofs adduced in reference to Bomfim may produce on the minds of others, I cannot tell. In the statements submitted, from which all exaggeration is carefully excluded, I can detect

none of those fallacies by which medical evidence is too often vitiated; nor can I resist the conviction that these statements, even alone, and unsupported by any other testimony, afford irresistible proof that the ordinary doctrines relating to the production and influence of marsh poison are founded in error. Numerous striking and analogous instances are, indeed, admitted by all. America—the West Indies—Italy—Sicily—are pregnant with unquestionable examples of the security afforded against intermittent fever by woods, hills, walls, &c., *to windward*. The explanation offered is, that they intercept or attract the poison. But how are we to reconcile this with another statement which the supporters of the doctrine of miasm advance with equal confidence—that, while a wood, a wall, or a tree, will at times effectually arrest the progress of marsh poison, yet the same identical poison will, on other

occasions, and under precisely similar circumstances, ascend vertically for hundreds of feet to the summit of the highest hills, and there display its deadly properties unchanged!

Again, we are told that, in some cities, malaria exists only in the low, damp, and crowded quarters of the poor; in others—as, for example, in modern Rome—the close, filthy, and crowded habitations of the Jews, adjoining the banks of the Tiber, are comparatively healthy; whilst the wide, open streets and squares, swept by the chilling tramontane winds, are notoriously subject to disease. To any one, however, who has experienced, as I have at Rome, the difference in temperature between one street and another, and even between different sides of the same street, this seeming paradox will admit of rational explanation, without any necessity for the assumption of a special poison. But I have already,

in a former lecture, entered fully into the consideration of these subjects.

In reference to that *vexata quæstio*—the comparative healthiness of ancient and of modern Rome—I am convinced, from personal feeling and observation, that, among other considerations, in a climate subject to such extreme and sudden vicissitudes, due weight has never yet been accorded to the powerful influence of his woollen clothing, in protecting the ancient Roman from those diseases so generally attributed by the moderns to malaria.

THE VICTORIA SUBURB.—Let us now direct our attention to some interesting and remarkable facts arising out of the political convulsions of the times in Brazil, which afford strong evidence in confirmation of the opinion that intermittent fever does not owe its origin to marsh miasmata.

In the immediate neighbourhood of Bahia,

to the southward, within about a mile of the city, stands the suburb of the *Victoria*, where most of the British and foreign merchants reside. Nothing more beautiful than this suburb can well be imagined. It extends about a mile, from the *Campo da Victoria* to the *Graca*, along an elevated ridge of about six hundred feet in height, and overhanging the magnificent bay. The houses are well built, spacious and elegant, placed on the brow and summit of the ridge, and surrounded by gardens, abounding in all the luxuriance of tropical vegetation. Parallel to this ridge, and at the distance of eight or nine hundred yards to windward, stood a thick and lofty woody belt, the "Graca Wood," extending in the direct course of the trade wind, and breaking its force before it reached the *Victoria*. During the war of independence in 1822—23, when the city of Bahia was besieged, or rather blockaded, by the Brazilians, the

suburb of the Victoria was taken possession of by the Portuguese troops, and occupied as an outpost; and, in order to prevent the advance of the Brazilian forces under cover of the wood, it was entirely cut down by order of the Portuguese general, Madeira. Since that period, the strong sea-breeze being no longer broken by the woody screen, intermittent fevers have been of much more frequent occurrence in the Victoria. Furthermore, in numerous instances, particular houses have suffered from similar causes:—namely, the cutting down, by order of the Portuguese general, of a tree, or the destruction of some wall or building, which had previously afforded shelter from the strong humid sea-breezes.

From a like cause, houses in elevated and exposed situations are subject to ague in Brazil, though less so when built of one story, and surrounded by a deep verandah, which breaks the force of the breeze, than

when the ordinary height and construction are adopted. Instances of this nature are numerous and well marked. So well understood and universally established are the above facts in Brazil, that no native Brazilian will build his house on an eminence, exposed, without protection, to the full influence of a strong sea-breeze.

Conversely, it is matter of common observation, that many houses in exposed and elevated situations, or built on the gorge of hills, and thus subject to a strong current of cool, damp sea-air, have, though formerly almost uninhabitable from ague, become healthy since the growth of forest timber, in the immediate neighbourhood, *to windward*, which had evidently no other effect than that of interrupting the force of the strong, cool, and damp, though otherwise healthy wind.

In estimating the influence of a powerful cool and humid wind on the animal eco-

nomy under the foregoing circumstances, we must always bear in mind, that the cutaneous system is continually relaxed, and the nervous power depressed, in tropical regions; that the difference in temperature between the sun and the shade is very great; that the slightest bodily exertion is attended by strong vascular excitement and profuse perspiration; and though, during the continuance of such exertion, little or no danger may be apprehended, that its discontinuance is usually followed by great exhaustion, and, to a certain extent, by collapse. Hence arises the paramount importance of well-sheltered situations, and the danger, in such climates, of exposure to strong damp currents of air in certain states of the system.

On these principles, too, I apprehend, we can satisfactorily explain the peculiar and unlooked-for unhealthiness of certain barracks and stations, placed in elevated and

exposed situations in the West Indies, rather than by imagining the ascent of a special morbid poison from the lower and surrounding district, whose inhabitants it leaves comparatively unscathed, to display its deadly influence, notwithstanding an extreme dilution, on the residents of the distant ridge or mountain.

The particular state of the animal economy just alluded to, is well understood by the native Brazilians, and guarded against most carefully as a chief source of *sezoens* (intermittent fever) as well as of *constipaçãoens* (continued fever). After exercise, or long-continued exposure to the sun, they invariably shut themselves up for a time in a close room; and though of temperate habits as regards spirituous liquors, they take some stimulant in order to guard against the dangerous effects of the sudden collapse which so frequently ensues.

The influence of sudden change from ex-

treme heat to comparative cold, under certain conditions of the system, in exciting an attack of a febrile type, was well exemplified in my own person, while travelling in Italy during 1839, for the benefit of my health. On the first of July, I quitted Rome for Naples, in company with my friend Mr Edwards, like myself an old resident in Brazil. At two o'clock, P.M., the thermometer stood at 87.5 in the shade, in the Piazza de Spagna, and the heat was much more oppressive than I had ever experienced in the tropics. We set off in the afternoon, preferring the danger of a night journey over the Pontine Marshes to the suffocating heat of a mid-day sun. Some time after leaving Albano, a spring of our carriage gave way, which compelled us to walk nearly a league to Gensano, where we arrived shortly after midnight. Here we found the inhabitants in such alarm, from a recent appearance of brigands in the

neighbourhood, that they refused to open their doors, and we were compelled to remain in the open street, whilst our attendant patched up the damaged carriage, which occupied upwards of an hour. Never shall I forget the intense chilliness which I experienced in the streets of Genzano, in my summer clothing, fatigued by the excessive heat of the previous day, and bodily and mentally exhausted in taking a last look at the "lone mother of dead empires." My teeth almost chattered with cold; and on reaching Velletro, at three o'clock in the morning, I had fairly entered on the first stage of an ague fit, with visions of malaria floating in my imagination. Here, however, I managed to obtain some hot coffee, together with a supply of bad brandy, which had the effect of restoring the nervous power, and setting my blood once more in circulation; and thus I was enabled to pass the dreaded Pontine Marshes

without further inconvenience. On reaching Terracina, at nine o'clock in the morning, the bright shores of the Mediterranean, aided by the less imaginative, though probably not less important influence of a good breakfast, quickly dissipated all recollection of marshes and malaria.

Now I am perfectly convinced that, had I not obtained the hot coffee and brandy at Velletro, I must—marsh poison apart—have suffered an attack of fever; the type, severity, and duration of which would have been determined rather by constitution or predisposition, than by anything specific in the original exciting cause. This exciting cause I take to have been exposure to the cold damp night air, at a time when mind and body were alike suffering from previous exhaustion. What the actual thermometrical difference might have been between Rome at three P.M. and Gensano after midnight, I cannot possibly determine; but

were I to measure it by my own feelings, I should certainly state the difference to be not less than 40 degrees; and if we take into consideration an atmosphere loaded with moisture from the adjoining marshes, a clear and cloudless sky, and the high radiating power of the rank luxuriant grasses, I am satisfied that the estimate of 40 degrees would not be far from the truth. A change so great and so sudden, when applied to an over-excited and exhausted system, will satisfactorily account for the invasion of fever, without calling in the aid of an especial poison.

There is certainly nothing intrinsic in the essential nature of intermittent fever, which absolutely precludes the idea of its arising from other causes than the reputed one of a special poison.

The question of periodicity, considered by the mass of medical authorities as almost distinctive of diseases originating in marsh

poison, I have already disposed of. I have moreover shown you, that the disease in question has been brought on simply by periodical immersion in cold water; and a singular case has been recently reported of one of those unfortunate outcasts of humanity, who prey on the feelings of the public, through the habit of feigning shivering, and who is now stated to be unable to bring his body into proper warmth—he always shivers.

Let us examine the common history of a man who has suffered from an attack of ague. He is often liable, after the lapse of weeks, or months, or even of years, to a repetition of his attack on exposure to cold, or wet, or an east wind, &c. Now, what are the explanations of this remarkable phenomenon ordinarily offered by authors? One deems the law of habit to afford a satisfactory solution; another believes that the malarious poison is still lurking in the system; whilst a third, as Dr. Elliotson—

high authority—supposes that the unhappy wight, in passing through a market-place, may have come in contact with some erratic and malarious vegetable; and thus he satisfactorily accounts for the renewal of the disease.

With regard to the law of habit, I apprehend that, whatever may have been its original force, an interval of months, or years, must in all cases be deemed sufficient to annihilate its power, to say nothing of its first establishment. I also hold, as utterly untenable on chemical evidence, the doctrine of a poison remaining unaltered for years in the system, though exposed to the incessant series of organic changes and decompositions to which the human frame is subject, as well as liable to the universal law, inherent in all organised matter, of perishing, or dissolving into other forms. And the question of the malarious influence of decaying vegetable matter in the common highways and byeways,

will be, I conceive, sufficiently set at rest by the well-known fact, that Covent Garden is neither a pest-house nor a desert.

It would be important to know whether the authorities, to whose opinions I have just alluded, have ever carefully investigated the actual condition of health in these individuals who are subject to occasional attacks of ague. My own peculiar position has enabled me to make such examinations on an extensive scale; and the following are the results to which I have been led. In every case of relapse which admitted of satisfactory investigation, I found, universally, that though the first attack of ague had been subdued, and the patient restored, in common phrase, to his ordinary health and his ordinary occupations, yet his health had not in reality been perfectly re-established. If questioned under such circumstances, the reply would be, that he felt perfectly well; but on a more close examination, it would be

found that he had become more sensitive to slight atmospheric changes; that his tongue was rather white and flabby, his appetite capricious, his feelings often causelessly depressed; that he was less capable of supporting continued mental or bodily exertion; and that his urine especially had become subject to much variation, being occasionally pale and rather abundant, and at other times scanty, and depositing sediments. Still he would be considered in good health. This condition of system may, under favourable circumstances, continue for a considerable period with little change, and finally pass away altogether; or the individual being accidentally exposed, in the above state of the economy, to one or other of the usual causes of fever, as cold, wet, an east wind, &c., a fresh paroxysm of ague may be the immediate consequence.

In this mode, Gentlemen, according to my experience, is to be explained that

peculiar susceptibility to relapse in ague, which has so long and so seriously taxed the ingenuity of authors to account for satisfactorily. The influence of change of scene and climate in these cases is well known, and, in the higher latitudes especially, often affords our only resource. The mode in which this change operates so beneficially in ague, has also given rise to various conjectures and opinions. When we reflect, however, on the remarkable power exercised by change of climate over the nervous system, and on most chronic maladies unattended by organic change, we need surely resort to no very recondite hypothesis, but may at once admit the obvious fact, that the general condition of the economy had been so far modified and improved by change of scene and climate, as to be enabled to resist those ordinary exciting causes of disease to which it had previously succumbed.

I quote the following extract from one of the most recent works on the subject, as it suggests more correct views of the elements of febrile diseases in tropical regions; and the principles of treatment glanced at are unquestionably superior to the fatal routine system of venesection, calomel, and salines, so commonly pursued in these maladies. The author, however, seems unacquainted with the almost miraculous effects of quinine in large doses, in controlling every stage and form of the intermittent and remittent fevers of hot climates.

“European practitioners, in any degree conversant with the medical customs of the negroes of intertropical Africa, cannot fail to be deeply impressed with the marked attention paid by the native doctors to the due action of the cutaneous tissues, and their encouragement of this as a means for relieving disease. The Mahomedan code of laws, whose sanitary regulations are so

well adapted for the advancement of the moral and physical condition of the barbarous pagan tribes in Central Africa, strictly enjoins not only ablution, but other hygienic measures, for the promotion of cleanliness, and the proper discharge of the cutaneous functions. The inhabitants of most of the maritime localities in the Bights are fully acquainted with the importance of these views, and treat the remittent, and other fevers to which they are subject, by endeavouring to excite a long-continued and copious exudation of sweat from the cuticular pores, by the aid of heated sand-baths, ablutions of hot water, and rude attempts to imitate vapour baths. In some countries, the patient is placed to a large fire for such purposes; while, in others, he is held over it, water being slowly dropped thereon, so that the steam, as it ascends, may act on the affected portion of the body. After a careful observation of the good effects of

this remedial system, I was led to pay more particular study to the utility of its application, and, at length, to try a modified adaptation of it for the cure of those adynamic remittent fevers so destructive to Europeans. I have no hesitation in saying, that not only myself, but many others, who have experienced its efficacy by the speedy restoration to health, can vouch for its superiority over the ordinary practice of venesection, saline purgatives, and large doses of calomel," &c. (Pp. 119-20.) *

It is scarcely necessary to repeat that, notwithstanding the almost universal assumption of the hypothesis of malaria, no sufficient evidence has yet been adduced to prove its *entity*. Indeed, so far as all chemical investigations go, the most refined analysis has been altogether unable to dis-

* DANIELL, W. F., M.D. Sketches of the Medical Topography and Native Diseases of the Gulf of Guinea, Western Africa. London : 1849.

cover the slightest difference between the air of the most swampy fen, and air procured from the most pure and open sources. The actual presence of malaria is inferred from the succession of certain effects; but the question has never yet been fairly raised, whether certain *known* conditions, and states of the atmosphere, and of the individual, might not be deemed capable of explaining the phenomena more satisfactorily than the agency of a cause which is purely hypothetical, and which has never yet been brought to the test of experience.

I humbly submit that this question may be satisfactorily answered in the affirmative, on the evidence which I have adduced, and which at least demonstrates that intermittent fever does originate in numerous instances—and probably universally—from certain atmospheric conditions, in certain states of the constitution, altogether independent of organic or marsh poison.

UNITY OF FEVER.—The whole group of febrile diseases—the essential meaning of this term being a disturbance in the sanguiferous system—may be comprised in three distinct classes:—fever without local action; fever with local action; and fever with a specific action, and arising from a specific cause.

The connection of the second class or group with atmospheric vicissitudes is too palpable to demand illustration. The dependence of the last on a special cause, altogether apart from the physical influence of the elements, separates it distinctly from the others, so far as regards the cause. But the origin of the first class is undoubtedly, in numerous instances, as frequently the result of atmospheric vicissitudes as of any of the other commonly accredited causes—contagion or malaria. In all the three classes, the common feature of family resemblance is portrayed in the initial symptoms—the nervous depression, of

longer or shorter duration, the cold shivering and anserine skin, followed by the glowing heat and general perspiration.

It is, indeed, more than probable, that fever is the expression of a type of disease essentially one and uniform, but admitting of an almost endless variety of forms; the simplest being displayed in the single paroxysm of an ague. It is furthermore obvious, on the most cursory observation, that the description of fever as a disease has, in the generality of instances, been drawn for certain localities, and not from the whole group of febrile diseases, as witnessed in different parts of the world. The typhus fever of this country is superseded by the bilious remittent and intermittent in southern climates—by the plague in the Levant—and by the yellow fever within the tropics. Each of these maladies, under the special influence of climate, temperament, different modes of living, and

numerous other agencies, affects certain peculiarities in its progress; but they are all distinctly impressed by the phenomena universally characteristic of fever as a genus of disease in every clime.

Climate, indeed, and its varieties, whether permanent or temporary, always modify the animal economy, and induce, if I may be allowed the expression, an epidemic or endemic state of the human constitution exposed to its influence. This state will vary, as the atmosphere may be cold or hot, dry or humid, pure or impure, variable or equable, stagnant or the reverse: the geological character of the country or district, also, and the numerous moral and physical conditions already alluded to, powerfully contribute towards the formation of this endemic constitution in man. Moreover, the history of epidemic and endemic diseases almost universally proves that, prior to their invasion, the country or district

has been subjected, for a period more or less prolonged, to some well-marked deviation from the ordinary constitution of the climate, sufficient to modify the constitution of the inhabitants generally; so that the slightest disturbing cause, moral or physical, is followed by disease, the characteristics of which are determined as much by the constitution of the individual, as by the constitution of the atmosphere.

An extensive acquaintance with the fevers of different regions of the globe has gradually established in my mind a conviction of their essential identity. And this has been confirmed since my return to Europe in 1842, by my observation of the typhus fever of this country and of France, which, in its more intense form, differs rather in degree than in essence, from the remittent of hot climates, or the yellow fever, as I observed it during the epidemic of 1812-13 at Cadiz, while serving as Resi-

dent Medical Officer at the General Hospital for the reception of the sick of the army of Andalusia. In each, you have commonly the same tendency to remission; and there is no one symptom, even to the yellow skin and black vomit, which you will not find occasionally present in each of the three diseases; neither is there any one organ or system so constantly affected, nor any lesion so constantly detected on *post-mortem* examination, as to characterise the several maladies. There is no one morbid result, in fact, in any of these forms, that you will not occasionally find in all.

The commonly rapid course of the tropical fevers might, perhaps, be urged as sufficient to distinguish them, and to establish their claim to be placed in a distinct category: but do not other tropical diseases often run an equally rapid course, as compared with those of Europe, and yet who ever dreams of their being essentially dif-

ferent from similar maladies in colder latitudes?

The remittent, or bilious remittent, of southern climates, I need scarcely observe, is simply a more intense form of intermittent, and differs only in the severity of the symptoms, the less perfect remissions (intermission really applies to neither), and the greater disturbance of the organic functions. They are to be treated on the same principles, and are controlled, fortunately, by the same remedies.

In reference to that *veraxa quæstio*—the essential identity of the bilious remittent with the yellow fever of hot climates—I may observe that the latter disease has lately ravaged the coast of Brazil, it is said, for the first time, and has therefore given rise to much difference of opinion among the profession. Some maintain its foreign, others, as my friend Dr. Croker Pennell, of Rio de Janeiro, its indigenous

origin. In proof of the latter opinion, Dr. Pennell states (p. 19), that "It is a curious circumstance, and may perhaps tend to elucidate the origin of yellow fever in Brazil, without having recourse to a specific source of infection, that, for the last few years, the fevers of the country, evidently not infectious, but of high temperature or marsh origin, have clearly been changing their characters. The genuine remittent has been but little seen for the last three years. In 1847-48, and 49, it was replaced by a fever of its own class, popularly known by the name of 'Polka,' but in reality a remittent; and, during the present year, *it* (the Polka) has been replaced by the yellow fever, a disease also with similar features.

"Coincident with these and other changes in the diseases of Brazil, the climate, in its broad features, has altered strangely. Thunder storms, formerly of daily occurrence at a certain hour, during the summer,

are now but seldom heard," &c., &c. Dr. Pennell finally arrives at the following conclusions:—

“That bilious remittent and yellow fever exist only under similar conditions of locality and climate;

“That they are essentially the same disease;

“That yellow fever is the most intense form of bilious remittent:”—Propositions entirely in accordance with my own experience in Brazil and other countries.

On the other hand, my friend and successor at the British Hospital in Bahia, Dr. Alexander Paterson (whom I am happy to recognise now present) considers the yellow fever, which prevailed at Bahia at the same epoch as in Rio de Janeiro, to be a disease *sui generis*, and imported; and he has published some valuable and interesting evidence in support of this opinion. *

* Medical Gazette, 28th March, 1851.

Along with other reasons, he infers its essential difference from the ordinary remittent fever of the country from the fact, that quinine, which almost invariably controls the latter disease, proved powerless in the former. This evidence, however, I hold to be inconclusive. In the worst form of yellow fever, *no remedy*, Gentlemen, as I know by experience, will modify the disease—a fact, unfortunately, applicable alike to the worst form of remittent and of intermittent fever. All three may, indeed, be characterised in the terse words of a distinguished French physician, as applied to cholera—"They commence with death." And, I may add, it is impossible to distinguish, in anything essential, the more intense form of these tropical fevers from the more intense form of cholera—all commence with death; the patient never for an instant rallies; the rational and physical signs are, in all, perfectly identical;

and equally so are the post-mortem appearances.

While this sheet was passing through the press, my attention was directed to an interesting article in the last number of the *Dublin Quarterly Journal of Medical Science*, by Mr. Barton, on Hong Kong fever. Mr. B. is a believer in the theory that marsh miasm or malaria is the general exciting cause of intermittent and remittent fevers; but, as a candid and accurate observer, he does not resist the clear evidence of facts, and he therefore admits that these diseases *may* arise from other causes than marsh poison. In proof of this he gives his own case. He had resided at Hong Kong for four years in perfect health, until placed in attendance on the troops, and exposed to "unusual mental and bodily fatigue," when, after one month, he was seized with remittent fever. He however believes, (p. 347), "that a poison is imbibed into the system, remaining latent for an uncertain period, or until some exciting cause develops a febrile paroxysm;" though he admits that a combination of depressing moral and physical influences might account for the disease. He considers "that the organic nervous system is primarily affected;"—in which opinion I fully concur. He notices the "cholera form as simulating Asiatic cholera;"—there is, in fact, no distinction.

In proof of malaria being the source of fever, he notices that "the civil population at one time suffered

equally with the troops, but subsequently, on proper sanitary steps being adopted, remained comparatively healthy and the mild character of the disease in the Royal Navy, and persons living afloat." (p. 348.)

To this I reply, that were the Royal Navy deprived of their accustomed accommodation and advantages, or the civil population subjected to the same exposure and privations as the military, we should find that fever respected the one class as little as the other.

He remarks (p. 363) on the fearful amount of mortality amongst the troops as contrasted with the healthiness of the naval forces, and of the civilians; and he very rationally suggests the amelioration of the condition of the soldier as the remedy; adding, "the reputed pestilential nature of the climate is now only supported by the military inhabitants, as other individuals, untrammelled by their rules and regulations, enjoy an equal if not greater amount of health . . . than most other tropical residents;" and as a proof that the disease is not attributable to locality alone, we have the further fact, that "the officers of the 59th Regiment, quartered in the same locality as the men, but better provided, having more space allotted to their use, and not being exposed at night, escaped fever." (p. 334.)

Still Mr. Barton believes "that a poison is imbibed into the system;" and that "the system is impregnated with this subtle essence, is shown by the constant liability to relapse." He admits, however, that "suppres-

sion of perspiration is a frequent exciting cause, as instanced in the number attacked on or after night guards, from exposure to draughts of air at an open window, and the frequent relapses of the patients unavoidably placed near the doors in the hospital wards." (p. 344.)

He attributes to the concentration of malaria, or marsh exhalations in certain localities, the rapid putrefaction of dead animal matter, the rapid decay of silk, woollen, and other fabrics, and the ground floors in buildings becoming quickly rotten, &c. &c., and believes these to confirm "the theory of a noxious emanation from the soil." (p. 345). These results I have elsewhere shown to be invariably contingent on the combination of high temperature and moisture, and altogether unconnected with marsh or any other noxious exhalations.

But the evidence adduced by Mr. B. himself against the malaria hypothesis I hold as irresistible; especially when conjoined with that afforded by Brazil. Mr. B. refers to Dr. Graves of Dublin, on the question of the origin of fever in Ireland; and I am well satisfied that, had the penetrating and philosophic mind of this physician been afforded the opportunity of observing tropical fevers, the marsh poison theory would have long since disappeared.

Mr. B. tells us that "at Stanley or Chuckchoo, a military station at the south side of the island, the barracks are close to the water, and open to the sea-breeze."

A detachment of troops arrived from England in October, and were quartered here, and "*all* were attacked with fever within a fortnight after their arrival, and at a time when malaria could not be considered as exerting much influence." (p. 346.)

Again, "the south-west monsoon blowing during the summer months, and the town of Victoria being situated on the north side of the island, and under *the lee* of the highest range of mountains, it was conceived that the exclusion of the refreshing sea-breezes was one cause of sickness. A military station was accordingly formed on the south side, and barracks erected on a *peninsula open to both monsoons*, and enjoying the advantage of having *no swamp or marsh* in its neighbourhood. The mortality here exceeded the worst periods experienced at Victoria, on the north shore, and the place has been *virtually abandoned*. . . Six assistant surgeons ordered there during the past season to replace each other in succession, were all attacked with fever and removed to Victoria." (p. 346.)

"The same cause, whatever it may be, evidently gives rise, indifferently, to remittent fever, acute dysentery, or intermittent fever." (p. 346.)

Mr. B. also suspects (there is no doubt of the fact) that "causes which in a temperate climate give rise to continued fever, or inflammations of the thoracic viscera, in a tropical climate cause periodic fever, or determination to the abdominal viscera." (p. 347.) Though holding to the marsh miasm theory, he still thinks "that it is

not *altogether improbable* that periodic fever may arise independently of malaria, and be owing to heat or peculiarity of climate remotely, and, immediately, to sudden chills or any depressing agency." (p. 336.)

"The city of Victoria is situated on the northern shore, and stands close to the base of the highest ridge, the buildings extending up the face of the hill. . . . The town enjoys the advantage of being built upon a dry soil," &c. "The Regimental Hospital and military barracks are built at the foot of ravines or mountain gorges. . . . The winds during the hot season blowing through these ravines towards the military buildings."

"Whatever the cause or causes of the fever may be, one fact is incontestibly established, that the vicinity of ravines and gorges in the mountains is unhealthy. . . . Both the Murray barracks and hospital are built at the foot of ravines, and the inmates will, in my opinion, always be obnoxious to attacks of fever Men admitted for other diseases were attacked with fever Observation proved that relapses were most frequent in the eastern wards of the hospital, which was opposite a *cutting in the hill* leading from the ravine behind." (p. 338.)

Dr. Wilson's "*Medical Notes on China*" are quoted with reference to one locality which proved singularly fatal. "It is a low narrow gorge, where a line of six cottages were lately built, with high land on either side; and close to one extremity of the range there is cultivated rice ground, while there are deserted spaces

at the other. From the height of the land forming the gorge, the wind is diverted from its natural course, follows the hollow, and thus blows over the *rice land*, cultivated or waste, to the cottages. Five English gentlemen went to reside there; in a short time four died from fever, the fifth scarcely escaped with life, and the lodging has been abandoned. . . . Three large and commodious houses were erected, of which two have been razed to the ground by their proprietors as uninhabitable, owing to their insalubrity, . . . While the city of Victoria (*sheltered, recollect, by the highest ridge in the island*) enjoys almost entire immunity from fever, and the few cases that do occur are of a mild and tractable nature." (p. 341.) "The bad cases of fever always found *in exposed positions*, or in the vicinity of ravines or gorges." (p. 338.) "High temperature night and day for four or five months . . . and atmosphere very humid." (p. 343.)

But every page affords an illustration, which I would willingly quote did my space permit; and I am compelled to refer the reader—as I do most earnestly—to the paper itself, which will amply repay perusal.

The explanation, indeed, offered for the unhealthiness of the several buildings erected close to the ravines or mountain gorges, or in the valleys, is the old story of rice lands, decomposing brushwood, rank vegetation, or decayed vegetable matter carried down by the torrent, &c. But what shall we say of the

barracks at Stanley—"close to the water, and open to the sea-breeze;" or to the still more deadly, and now abandoned station—"erected upon a peninsula open to both monsoons, and enjoying the advantage of having no swamp or marsh in its neighbourhood;" whilst Victoria, built close to the base (*to leeward*) of the highest ridge on the island, is free from fever!

Let the reader compare with the above statements the evidence afforded by the city of Bahia, the Bahia Hospital, St. Lazaro, Bomfim, &c., and he will at once perceive the solution to all Mr. B.'s doubts and difficulties at Hong Kong. Mr. B. adopts the universal theory of malaria or marsh poison, and therefore considers that the vegetable products of the adjoining ravine afford a satisfactory explanation of the deadly insalubrity of his hospital and barracks; whilst Ferguson, in another hemisphere, found the Rocky Mountain gorge, devoid alike of vegetation and moisture, equally fatal, and from precisely similar diseases; and he, too, invoked a malaria! How, then, are we to reconcile such seeming anomalies? Thus: in all hot climates (from causes stated elsewhere), if your hospital, or barrack, or habitation, be placed on an unsheltered eminence, or in the current of a gorge or ravine, though such ravine, instead of decaying vegetation, shall be lined with Parian marble, and swept by the purest ocean winds—pure, but always loaded with moisture—yet will such hospital, barrack, or habitation, prove invariably unhealthy; and ague, remittent, and con-

tinued fever, dysentery, and rheumatism, the prevailing diseases.

From the height and pride of our theories, might we not cast a glance at '*ces bons Chinois*,' who ought surely to know something of their native country, and who are, besides, "subject to periodic fevers." They select *invariably* sheltered situations for their detached houses and villages, both being surrounded by *umbrageous trees* and shrubs" (p. 344), precisely as the Brazilians do under similar circumstances, in another quarter of the globe, and doubtless for the same substantial reasons.

Let us then be guided by rational principles; let us place our habitation or our hospital in some dry and *well-sheltered* locality, and avoid alike the exposed summit of the hill, or the cool and grateful, but deadly blast from the gorge or the ravine. At Hong Kong, as everywhere else, let the comfort of the soldier be equally cared for as the comfort of the civilian; let him be supplied with proper food, appropriate clothing, and commodious barracks; let over-fatigue, exposure to the mid-day sun, and night duties, be, as far as practicable, lessened or avoided; let temperance, rational amusement, and healthy exercise be encouraged; as well as all other means calculated to maintain the moral and physical energies; then drain, plant, and cultivate, by all means—by which we shall not only increase shade and shelter, and lessen moisture, but add otherwise to our personal enjoyments.

I need not here repeat, what will be fully explained elsewhere, that to the observance or neglect of the above measures, and not to visionary and occult influences, we must look for the maintenance of the health of our troops and stations in all intertropical countries. We must, in short, cease to be terrified by shadows, and attend to realities.

I greatly regret that space will not permit me to enlarge on the important questions of symptoms and treatment. I would, however, observe, that the greater gravity of the fevers, generally, at Hong Kong, as compared with those of Bahia, is explained chiefly by one single fact, viz., at Hong Kong, the thermometer ranges from 45° to 91° ; at Bahia, from 72° to 86° .

One or two more observations, and I conclude. Quinine was found *the remedy* at Hong Kong; yet, prior to Dr. Ferguson's arrival, "His predecessors reported quinine to have *signally failed*" (p. 356). "Bleeding and calomel was tried with almost uniform ill success; depletion and antimonials were equally unsuccessful" (p. 357). "One day's observation of the disease in the Military Hospital was sufficient to demonstrate that quinine in *small doses* was inadequate," &c. "The freedom from visceral disease of those cases treated with quinine at an early stage was marked," (p. 357). "Thirty to thirty-six grains commonly induced the specific action of quinine on the nervous system;" and, "in but one case, out of nearly 900 subjected to its

influence, were any bad consequences experienced :” there were “sudden sinking or collapse” (p. 355), which speedily passed off on using stimulants.

My experience, in some points of treatment, does not coincide with that of Mr. Barton ; but into this subject I am unable to enter.

LECTURE VII.

TREATMENT OF FEVER, INTERMITTENT, REMITTENT, AND
TYPHUS, BY LARGE AND FREQUENTLY REPEATED
DOSES OF QUININE—CASES OF TYPHUS FEVER TREAT-
ED IN THE LIVERPOOL NORTHERN HOSPITAL—MODE
OF ACTION OF QUININE—EXCITING CAUSES OF FEVER
—DOCTRINE OF MARSH POISON CRITICISED—REAL
CAUSE TO BE SOUGHT IN NERVOUS AND VASCULAR
EXHAUSTION—REMOVAL OF BLOOD OR OF ITS SALINE
CONSTITUENTS EXCITES DISEASE—INJURIES—MORAL
DEPRESSION—SIR JAMES CLARK'S STATEMENT THAT
AGUE IS PREVENTED BY FIRE IN ROOM—EXPLANATION
—EXEMPTION OF NEGROES FROM FEVER—MARSH
MIASM DOCTRINE SHAKEN BY FERGUSON—REASON
FOR CONTRAST IN HEALTHINESS BETWEEN DRY AND
SWAMPY DISTRICTS—CAUSE OF GRAVITY OF DISEASE
IN HOT CLIMATES—PERMANENT RESIDENTS—TEMPO-
RARY RESIDENTS—INFLUENCE OF ELECTRICITY—OB-
JECT OF PUBLISHING THESE LECTURES, TO SHOW THE
IMPORTANCE OF DISREGARDING THE IMAGINARY AND
FIXING THE ATTENTION ON THE REAL CAUSES OF

DISEASE—PROPHYLACTIC MEASURES TO BE ADOPTED
IN HOT CLIMATES—CONCLUSION.

GENTLEMEN,—I have reserved for the present lecture the few observations which I intend to offer on the treatment of the intermittent, the remittent, and the continued forms of fever; and I shall not embarrass this important question by any allusions to theoretical opinions, or to the multitudinous remedies and forms of treatment recommended by authors, but proceed at once to lay before you, in a few words, the results of my own personal experience in a field of observation of no common magnitude, and which eventually led me to the adoption of a plan of treatment at once simple, intelligible, and generally efficacious. Neither shall I occupy your time by a recapitulation of the symptoms presented by remittent and intermittent fevers; they are given with sufficient accuracy and detail in every standard work on practical medicine; and with

the latter disease you have abundant opportunities of making yourselves familiar in the wards of this hospital.

I would here premise that, in the whole history of medicine, few more pernicious or ill-founded doctrines have been inculcated in so few words, than the following maxim, laid down in a modern standard work of deservedly high authority (the *Cyclopædia of Practical Medicine*), against the administration of quinine, under certain circumstances, in intermittent fever: viz. "the intermissions being imperfect, and a recent local inflammation." Now, Gentlemen, the most ample and undoubted evidence has proved to me, that the judicious administration of quinine, under these very identical circumstances, will not only render the imperfect intermissions perfect, and cure the disease, but will also at the same time arrest the local malady. Active inflammation in ague is, indeed, of rare occurrence; though

when it does arise it will undoubtedly *disturb* the curative influence of quinine: but this should not prohibit the use of the remedy, while the local affection should be met by cupping, with, or without, the scarificator, and counter-stimulation.

TREATMENT OF INTERMITTENT FEVER.

In all cases of intermittent fever, no matter of what type, you should invariably strip your patient, and examine carefully the condition of the thoracic and abdominal organs; and if the tongue be loaded, and the secretions depraved, prescribe at once an emetic of tartarised antimony. Having ascertained the usual hour for the accession of the paroxysm, you should administer ten or twelve grains of the sulphate of quinine about two hours before the expected attack, and repeat the dose after an interval of an hour. This will generally prove sufficient to modify, if not to arrest,

the disease. Should, however, the cold stage arrive, apply external warmth, and the succeeding hot stage will be quickly terminated by the exhibition of two table-spoonfuls of the following mixture every five minutes, until perspiration is induced.

℞. Tincturæ Opii,	ʒii.
Acidi Nitrici,	ʒi.
Syrupi Simplicis	ʒi.
Aquæ Puræ,	lb.i. M.

During the period of apyrexia, give small doses (three grains) of the sulphate of quinine every three hours, until within two hours of the next expected paroxysm, when the larger doses are to be repeated as before. The smaller doses of quinine should be continued for some time after the fever has entirely disappeared. Give your patient moderate support throughout, and avoid slops and purgatives. If a purgative be required, aloes is the best.

You will, Gentlemen, find few cases of

ague, in hot or any other climates, curable by medicine, which will prove rebellious to this treatment. If it fail—as fail it sometimes will—try no other physic, but remove your patient, with all practicable dispatch, to a different locality. I have already expressed my opinion on the question of complication by local inflammations. Depletion will not cure these inflammations, but cupping—with or without the scarificator—and counter-irritation, will often prove serviceable.

TREATMENT OF BILIOUS REMITTENT FEVER. With regard to the treatment of bilious remittent fever, the sole fact which I am especially anxious to impress upon you is this:—pay no attention to the violent headache, the lumbar pains, the strong and rapid pulse, the yellow burning skin, and the intense thirst, but commence at once the administration of large doses of quinine

—ten or twelve grains every two hours. With the first dose give ten or fifteen grains of calomel, and, after the third dose of quinine, you will commonly find the severity of the symptoms abate. When you have succeeded in establishing an intermission, you must treat the case exactly as you would an ague, into some form or other of which it often lapses.

Four or five large doses of quinine will ordinarily modify the disease; but if giddiness of the head and tinnitus aurium supervene, the remedy must be suspended, or continued in smaller doses. Its curative effects are displayed in the rapid subsidence of all the more intense symptoms. Should the first four or five doses fail in checking the disease, administer tartarised antimony to the extent of causing slight vomiting; and after an interval of twelve hours recommence the quinine as before.

Should this treatment fail—which it sel-

dom does, when early resorted to—in rapidly checking the disease, I believe that no other will prove successful. You may, indeed, resort to other means, and patients may get well under or in spite of them. But with this question we have at present nothing to do. I place before you, Gentlemen, the cardinal points; they will admit of some modification; and the minor adjuvants in the treatment of febrile disease will of course be attended to by every well-informed practitioner, and no other should undertake the treatment of bilious remittent fever.

I may add, that the treatment here indicated applies more especially to those who have resided for some time in hot climates. In the robust European, lately arrived, and attacked for the first time with bilious remittent, should the febrile symptoms run high, or threaten a vital organ, one bleeding from the arm, to the

extent of sixteen or twenty ounces, may be resorted to at the *very commencement* of the attack; then the treatment already laid down should be followed up.

On the sequelæ of remittent and intermittent fever it is not my intention to enter further than to observe, that they are best met by change of scene and climate, by the various preparations of iron, in small doses, either alone or in combination with quinine, by a free administration of taraxacum, and the occasional use of extract of aloes, together with careful attention to clothing, diet, and exercise, such as I have pointed out in a former lecture.

TREATMENT OF TYPHUS FEVER. Acting on my conviction of the essential identity of the remittent and intermittent fever of the tropics with the typhus fever of Europe, and aware of the specific action of quinine *in every stage* of the former dis-

eases, I have resorted to its administration in the ordinary typhus of this country, in all its stages; and commonly with the happiest results. In these researches I have been greatly aided by my relative, Dr. Leslie, now of Rio, and formerly my house-surgeon at the Bahia Hospital, and who has himself extensively employed the treatment here laid down. In typhus, as in the remittent of hot climates, the treatment by quinine will be successful in proportion to its early administration. The doses also, as in the tropical fevers, should be large, ten or twelve grains, and repeated at intervals not exceeding two hours. Three or four doses will, in most cases, be sufficient to produce its specific influence on the nervous system, which is commonly displayed by dizziness of the head, tinnitus aurium, or deafness, or in the rapid subsidence of all the urgent symptoms. In the latter event, three or four grains of the quinine should

be administered three times a-day, and the patient supported with good beef-tea, or other light nutriment, and wine if necessary. Should the urgent symptoms return, the large and repeated doses of quinine must again be resorted to. Slops should be avoided, and purgatives also, unless obviously indicated; but an emetic of tartarised antimony will often prove useful at the commencement, and apparently renders the system more obedient to the specific influence of the remedy.

Should the urgent symptoms persist, notwithstanding the administration of five or six doses of quinine, or should dizziness and tinnitus aurium supervene, the medicine must be discontinued; and, after an interval of six or seven hours, small and repeated doses of tartar emetic should be resorted to, until full vomiting is induced. Then allow your patient to rest for twenty-four hours, and recommence the quinine as

before. When there is much restlessness and want of sleep, a full dose of liquor opii sedativus, with some drops of nitric acid, will often prove highly advantageous, and enable you to resume your treatment with more prospect of success.

Should the symptoms still resist, you may repeat the remedies successively, as above, for four or five days; and unless the beneficial effects are broadly marked within that period, we can no longer reasonably hope for success from this treatment; and it may be abandoned, or the quinine continued in smaller doses.

Still, in the vast majority of cases of *uncomplicated* typhus, taken at the commencement, you may calculate on complete and rapid success; and, in all, you will almost invariably succeed in breaking, for a time, the diseased chain of actions—no unimportant advantage in any malady. In the advanced periods of the disease, the

result will be much less certain; but, *in all stages*, the large doses of quinine may be safely resorted to, and will commonly calm your patient, cool the skin, allay the headache, and reduce the frequency and improve the character of the pulse. You must, however, bear in mind, as already pointed out, that any vital or important organ being seriously involved, will prove a disturbing cause to the curative powers of the remedy, which are clearly exerted on the nervous system, through which the blood and secretions are favourably modified, and often with marvellous rapidity.

In the history of typhus in this country, you will find numerous incidental notices, such as the following, on the epidemic of 1819:—"The disease has simulated the recurrent type. The paroxysms were marked by distinct and often violent rigors, which were succeeded by intense heat, and increased vascular energy, terminating occasionally

in profuse sweating, but more commonly in a gradual subsidence of the exacerbation, without any relaxation of the surface. The paroxysms showed no obedience to periodicity, in some cases recurring in a few hours, and in others only after the lapse of many days." * Now, here we are presented, clearly and distinctly, with the history of an irregular intermittent or remittent fever; and yet, Gentlemen, strange to say, this idea seems never once to have crossed the mind of the observer.

As I well know how distasteful an array of cases commonly proves, both to hearers and to readers, I shall only trouble you with two or three on the present subject; but these are well adapted to illustrate, beyond all rational doubt, the justness of the principles, and the efficacy of the treat-

* SHEPPARD. Edinburgh Medical and Surgical Journal, vol. xv. See, also, different accounts of the "Relapsing" fever, in this and other countries.

ment, which I have inculcated. The description of the symptoms and treatment, in these cases, is divested of all minute detail, as I am anxious to place the broad facts clearly before you, in order the more firmly to impress them on your memory, and in order that you may hereafter, as opportunity offers, submit them to large and careful experiment. I select these two cases of typhus chiefly on this account—that they were lately received into our wards, and under your own eyes. One, you will remember, was admitted under myself; the other under my colleague, Dr. Scott; the notes were taken down by our intelligent house-surgeon, Mr. Evans; and the results have been observed by most of you.

“CASE I. Cornelius Vincent, aged 26, was admitted on the 2d October, 1850, into ward No. 12, under Dr. Dundas. Has been ill ten days.

“*October 3.* Has severe headache; an-

xious countenance; slight delirium; hot and dry skin; the tongue is black, dry, and furred; the teeth covered with sordes. He has thirst; the urine is scanty and high-coloured; the bowels open; the abdomen painful on pressure; pulse 100; respirations 28.

“ R Quinæ Disulphatis, gr. xxx.

“ Divide in doses iii, quarum capiat i secundâ quâque horâ.

“ *October 4.* Convalescent. The pain in the head and the delirium have ceased, and the abdomen is less tender. The heat of skin is diminished; the tongue clean and moist; pulse 90; respirations 24.

“ R Infusi Quassiaë, ʒiii ter in die.

“ No further treatment was resorted to; and from this date he rapidly gained strength, and was discharged well on the 11th October.”

You are aware that our ordinary rule is, not to admit cases of typhus, but to forward them to the Fever Hospital; but the above case having been reported to my colleague,

Dr. Scott, he resolved to give the treatment by quinine a trial. An opportunity presented itself in a few days, in the following case:—

“CASE II. Edward Donald, aged 23, was admitted into ward 14, under Dr. Scott, on 25th October, 1850.

“*October 25.* He had been ill eight days. There was now great anxiety of countenance, and high delirium; dry, pungent skin; tongue dry, and coated with dark fur; sordes about the lips and teeth; great thirst; the urine was scanty and high-coloured; the bowels open; the abdomen tumid, and tender on pressure; pulse 108; respirations 30.

“℞ Quinæ Disulphatis, gr. xxx.

“Divide in doses iii, quarum capiat i secundâ quâque horâ.

“*October 26.* At the morning visit, he was found reading a book in bed! All the formidable symptoms of yesterday had disappeared.

“No further medical treatment was resorted to in this case; and he was discharged well on the 5th of November.”

Yet we are told, on high authority, that, “in the continued fevers of this country, we believe it (bark) might with great safety be erased from the list of remedies altogether:”* and another eminent authority (Dr. Pereira), in his excellent work on *Materia Medica*, lays down that “in febrile conditions of the system, attended with a hot and dry skin, and a furred and dry tongue, tonics act as local irritants and stimulants, and add to the severity of all the morbid symptoms” (p. 208); and he illustrates these principles by the action of disulphate of quina in fever! Whilst Dr. Tweedie—whom we may fairly admit as representing the opinions of the highest authorities in this country on fever—em-

* *Edinburgh Medical and Surgical Journal*, vol. xv., p. 595.

phatically states of quinine, that "its exhibition in the early stages of fever, under any circumstances, is improper, as tending, by its stimulant powers, to keep up or increase the febrile action in the system; and when there is *local complication*, it is evidently *so pernicious*, that scarcely any practitioner can be so ignorant of the common principles on which the treatment of fever should be conducted, as to think for one moment of its administration under such circumstances." *

Such, Gentlemen, may be taken as the expression of the opinions of the most distinguished men in this country on the present question. Contrast these *opinions*, not with mine, but with the facts observed by yourselves in the wards of this Hospital.

* Cyclopædia of Practical Medicine, vol. ii., p. 211. Compare these urgent doctrines of Dr. Tweedie with the results of the "quinine treatment" in the foregoing cases, and in the cases of "*typhus with complications*" in the Liverpool Fever Hospital.

Yet do not, I pray you, misunderstand me. The physicians whose names I have just now, and elsewhere, cited, are men of undoubted talent, nay, of European fame; and whose works are, deservedly, in the hands of us all, and, for this very reason have I quoted them. My object is, not to depreciate, but to warn. In the course of your professional career—and I speak from large experience—you will find the great mass of the medical world disposed implicitly to rely on authority; it saves a world of care. But, Gentlemen, though conducive commonly to our ease, and sometimes to our interest, this principle is dangerous to truth. Therefore, although I would have you to receive with all respect the opinions of eminent men, yet I would most earnestly exhort you to admit nothing on tradition; “high though his titles, proud his name,” resign blindly to no man the exercise of that reason vouchsafed to you by Provi-

dence for your own guidance, and for the benefit of your fellow-creatures.

Now, I maintain that, did the before-mentioned cases stand alone—which they do not—they would afford cogent evidence, not only in support of the quinine treatment in continued fever, but of the truth of the doctrine on which such treatment is founded; namely, the essential identity of the typhus of this country with the intermittent and remittent of the tropics, modified by climate, and numerous other influences. I hold that in the present, as in numerous other maladies (delirium tremens, for example) the results of treatment will clearly identify or disprove diseases, which the most careful observation of symptoms, aye, and of pathological appearances, have utterly failed to distinguish. In fact, Gentlemen, the history of fever must, and will ere long, be *rewritten*.*

* Subsequently to the delivery of these lectures, the

The power of quinine, when duly administered, in controlling the remittent and intermittent fever, (and the yellow fever also, as stated by Dr. Blair), is now a well-established and important fact. But I totally dissent from the received doctrine of

cinchonizing treatment has been adopted in several other cases of fever admitted into the Northern Hospital, and with equally good success. In one case, that of Ann Dobbin, one of the hospital nurses, the effect was highly instructive. Attacked with well-marked typhus, she was treated for three or four days on the ordinary routine system, by purgatives, salines, and diaphoretics. Under this treatment she became daily worse; the cerebral disturbance, the lumbar pains, the heat of skin, and the thirst, gradually became more intense; the pulse more frequent, the tongue dry and brown, and sordes began to appear about the teeth and lips. Ten grains of quinine every two hours were now given, with the result, after the sixth dose, of arresting all the unfavourable symptoms. The pulse calmed down, the tongue became moist and clean, the sordes disappeared, the heat of skin, the thirst, the cerebral and lumbar pains, all moderated, and she rapidly improved without any further medical treatment, beyond the administration of a little wine.

its specific action on the *several specific poisons* which are supposed to produce these fevers. Moreover, I have already demonstrated to you, in our own wards, that it displays an equal power in subduing the supposed animal poison of the typhus fever.

The action of quinine is clearly not that of a tonic, in the ordinary sense of the term. Its action is obviously on the nervous system, whose functions it favourably and rapidly modifies, when they are depressed or exhausted by any of the numerous moral and physical agencies before alluded to; and thus it restores to the organic nervous system its normal influence over the animal fluids and the vital phenomena. Hence the curative powers of quinine in fever, as in many other maladies. The immediate arrest of typhus by quinine will, for obvious reasons, be more signally displayed in private patients than in the ordinary run of those admitted into hospitals.

EXCITING CAUSE OF FEVER. It would, Gentlemen, be altogether remote from the object of these lectures to enter into any lengthened disquisition on the nature of the exciting cause of fever—a subject on which so many men of the first talent have laboured, and on which so much and curious speculation has been offered: yet I feel bound to submit, with great diffidence, a few observations on the idea, so rapidly seized on, especially in hot climates, of a malaria or poison acting on the animal frame, as in the supposed production of ardent, remittent, and intermittent fevers; while many of those causes are erroneously omitted, or held only as secondary, of which a most especial account ought to have been taken, and the concurrence of which, in more temperate regions, is unanimously allowed to be the efficient agent in exciting febrile diseases—at one time of the idiopathic, at another of the symptomatic form.

If we examine those instances which are especially put forward in proof of the action of a morbid poison on the system, we shall commonly find that they refer to soldiers and sailors engaged in hot climates, on detached or fatigue duty, in dockyards, on watering expeditions, &c., and exposed to the intense rays of the sun, rendered doubly oppressive by reflection from the sides of the hills, or in the deep valleys, where such operations are most usually carried on. The cutaneous system (both its nervous and its vascular elements) is brought into a state of the highest excitement by excessive heat and laborious exertion, frequently accompanied by intemperance and other excesses. Extreme exhaustion of the nervous system necessarily succeeds, and is followed by a collapse. Night comes on, with a *positive* difference in temperature, often more than thirty degrees, between the mid-day heat and the earth's surface, towards sunrise. The ner-

vous energies—animal and organic—being already profoundly depressed, and the faculty of evolving heat proportionately diminished, the men fall asleep, not only deprived of their ordinary protection, but with their light tropical clothing drenched in the chilly night-dew, and exposed to a brisk land or sea-breeze, to awake in all the horrors of ardent fever. A swamp, a morass, a fen, a something *unknown* (for, even advanced as we are in the knowledge of the laws of nature, there exists in the human mind an inherent tendency to look to some secret, hidden, and unrevealed influence), is at once accused as the sole and efficient agent; and, thus satisfied, all inquiry ceases. Now, Gentlemen, this train of argument—inferring a pre-existing cause from certain effects—may be perfectly legitimate; but the cycle of logic is not equally correct, assuming that cause to be an unknown quality or essence, while it is matter

of observation to every one, that similar effects result from causes *known* and *patent* to all.

For example, under the foregoing circumstances, the influence of cold and exhaustion, positive as well as relative, on the sentient extremities of the nerves, and the rapid depletion of the vascular system by profuse tropical perspiration, seem to be altogether forgotten. In such cases, the depressed or exhausted energies of the cutaneous nerves will lower, and occasionally even paralyse, the capillary circulation, and thus deprive the nervous centres of the vivifying influence of vitalised blood; for the activity or diminution of the circulation depends, as you are aware, less on an action in the heart than on an action in the capillaries. The energy of the peripheries of the incident nerves being thus depressed, and the respiratory movements lessened in proportion to the diminished activity of the organic ner-

vous functions, imperfect arterialisation of the blood, with deficient circulation in the pulmonary and systemic capillaries, and defective depuration, necessarily ensue; and are followed by an arrest of the evolution of animal heat, and of the normal reaction between the blood and tissues—results highly deleterious at all times, but in some constitutions, and in certain states of the system, as fatal as the most deadly poison, and often with almost equal rapidity. Surely, under such circumstances, we need not evoke the phantom Malaria, in order satisfactorily to account for the supervention of fever, or any other serious malady.

“Nec Deus intersit, nisi dignus vindice nodus.”

Moreover, as I have already informed you, I have myself been temporarily exposed, and have witnessed, largely, the exposure of others, in various quarters of the globe, to the most concentrated effluvia of marshes; yet, *in no one instance*—and I beg of you to

bear this fact in remembrance—in no one instance do I recollect having observed such temporary exposure followed by fever, in an individual whose general health was good, and where none of the depressing agencies, already alluded to, had been previously in operation.

Neither has the influence of an occasionally large and sudden subtraction of the saline constituents directly from the serum of the blood, incident to over-exertion and exposure to the sun in hot climates, been hitherto duly estimated by physicians. Physiologists teach, and pathology confirms the fact, that those elements are essential to the preservation of the circulating fluid from decomposition; for the black and dissolved condition of the blood, observed in the more rapid and fatal forms of tropical fever, is found almost invariably allied with great diminution or entire loss of its saline constituents. And this, I apprehend,

is due, not to the exhaustion of these elements by the violence of the fever, but chiefly to their removal, which precedes the invasion of the disease, and greatly influences its course.

Moreover, you must all be cognisant, from the experiments of Hunter and others, of the immediate and decided impression which the sudden loss of even a few ounces of blood exercises on the general system, and, through it, on the constitution of the entire mass of the blood itself. So, likewise, will the exhaustion of the nervous power, and the sudden depletion of the animal fluids, by the rapid and profuse perspiration incident to all laborious exertion in hot climates, often profoundly and immediately modify the whole animal economy. Thus, indeed, can we more satisfactorily explain the occurrence, from simple exposure to intense solar heat, of fevers similar in character to the most pernicious fevers of marshy districts, as observed by Dr. Murray; and

most medical officers who have served in hot climates must have witnessed, as I have, a perfectly healthy man *walked* into a pernicious fever—aye, and quickly, when the atmosphere has been humid, hot, and sultry.

In offering a solution of the above and analogous facts, we are much aided by the evidence of Sanctorius and others, who have shown that, independently of its direct depressing influence on the vital powers, a drenching perspiration (never absent under the above circumstances) will arrest, or seriously interrupt, the elimination of those digested or effete elements which are alone removed by insensible exhalation, and whose retention in the system is ever attended by the most formidable results. *

Moreover, the immediate influence of the

* Sanctorius writes as follows :—

1. "That perspiration, which is beneficial, and most clears the body of superfluous matter, is not what goes off with sweat, but that insensible steam or vapour," &c., &c. Sect. i., aphor. 21.

organic nerves, in modifying the constitution of the blood, is now placed, by direct experiment, observation, and experience, beyond all rational question; nor can it be doubted, that such morbid changes can be suddenly impressed on the blood, through the nervous system, as to transform the healthy man of to-day into a mass of disease by to-morrow—and that, too, independently of the introduction, *ab externo*, of a single particle of any morbid agent into the system. I need scarcely allude to the daily-observed fact of the influence of a serious nervous shock in morbidly modifying the animal fluids. An instructive case in point, of fatal “yellow fever” following amputation in consequence of accident, is given by Sir George Ballingall, and our own Hospital affords numerous analogous instances;

2. “Sweat is always from some violent cause; and as such, it hinders the insensible exhalation of the digested perspirable matter.” Sect. v., aphor. 3.

whilst Dr. Hodgkin, in his interesting and valuable observations,* has shown that, in certain states of the constitution, even a slight lesion is adequate to the production of similar morbid results—a fact to which I can myself bear testimony.

We know, also, on the authority of Dr. Cheyne, that the most fatal cases of typhus fever will originate solely in anxiety and mental depression, without the individual having been exposed to any contaminating influence whatever; and I have repeatedly witnessed intermittent fever, of the most intractable character, originate under precisely similar circumstances, and, on one or two occasions, apparently from accidental loss of blood.

How, also, does it occur that careful attention to all those measures which preserve the general health, sustain the nervous power, and support the capillary cir-

* Medico-Chirurgical Transactions, vol. xxxi.

culatation, will completely annihilate the virulence of marsh poison? This is clearly proven in numerous and well-authenticated instances on the coast of Africa, where, by careful attention to the general health and habits of the men, the use of woollen dresses, and protection by awnings from the sun during the day, and during night from exposure to the chill and humid atmosphere, ships' companies and boats' crews have been maintained in a state of perfect health, notwithstanding a long-continued exposure to all the ordinary exciting causes of African fever. Even so lately as July 1850, we are told on high authority, that with regard to the alleged unhealthiness of it (the coast of Africa), "so nearly have European skill, science, and care, baffled the climate, that the African station is now as healthy as the rest of our naval stations in the tropics."*

* Reports from the Lords and Commons. Edinburgh Review, July 1850.

And we are indebted to that distinguished physician, Sir James Clarke, for the following important and analogous facts—facts quite in accordance with my own experience in different quarters of the globe, but utterly inconsistent with the notion of a *specific poison*. He says, that a person may sleep with perfect safety in the centre of the Pontine Marshes, by keeping his room “well heated by a fire during the night:” and again, that “the exemption of the central parts of a large town from these fevers (malarious) is explained by the dryness of the atmosphere, and by the comparative equality of temperature which prevails there.” How, Gentlemen, does this occur? If there be a poison, it must be taken in either by the air-passages, or in deglutition, or absorbed by the skin. “Having his room well-heated” certainly cannot prevent the access of the malaria to the skin, or to the lungs; on the contrary, from the current maintained

by a fire, a greater amount of air, and consequently of the poison incorporated with it, must necessarily have access to the lungs at each act of respiration. Deglutition taking place only when we are awake, all access through this channel is necessarily denied.

Can we then believe that simply warming a room causes the poison to refuse to enter the system by the skin or by the lungs, though extensively diffused through the atmosphere?—or have we not, indeed, this obvious solution to the problem:—THERE IS NO POISON? *De non apparentibus et non existentibus eadem est ratio.* Is not the dryness of the atmosphere, as well as its warmth by means of a fire, an adequate protection against the reduced temperature of the night, and little swerving from that enjoyed during the diurnal heat? Thus are sustained, especially during sleep, the organic nervous power, and consequently the activity of the universal capillary and

respiratory functions, through which the normal reactions, so essential to health, are promoted and maintained.

Such, Gentlemen, I believe to be a true solution of the foregoing remarkable and well-established fact, and explanatory of the invasion of, or the immunity from, febrile diseases, in all warm climates. In those countries, you will please to recollect, the capillary vessels play a much more important part than in the colder latitudes, and are, consequently, more exposed to derangement from their excessive and incessant action, maintained by the permanent stimulus of high temperature. This important system, therefore, soon becomes weakened, as do also the peripheral nerves, whose energies, being exhausted or depressed from the same cause, greatly increase, if they do not indeed originate, the general mischief. Prolonged exposure to the sun, residence in humid and sultry localities,

excessive fatigue, debauch, the depressing passions, exhaustion from previous disease, &c., &c., will equally depress the organic nervous power and the capillary circulation, and thus establish a morbid sensibility and susceptibility to serious modification from such limited atmospheric changes as are caused by a strong humid sea-breeze, or other equally slight physical or moral influences, which, in a different state of the economy, would be either unattended with evil, or would pass altogether unnoticed. Moreover, in hot countries, all the internal organs sympathise largely and immediately with the slightest derangement of the dermoid membrane; and I am deeply convinced that such derangements constitute the immediate exciting cause of nineteen-twentieths of the diseases (including all the fevers) to which Europeans are subject in hot and tropical climates.*

* While this sheet was passing through the press, my

The comparative exemption from fever enjoyed by the Negro race in hot climates is another well-established fact; and its solution, I apprehend, will be found chiefly in the peculiar structure of the dermoid

attention was called to the interesting and valuable work of Dr. Bascome,* whose opinions in the following summary are, I am gratified to find, in perfect accordance with my own views and experience. "I take leave to reiterate my opinion—an opinion founded on a careful review of the foregoing history of epidemics—that all epidemic pestilences or diseases are to be accounted for on the principle of natural causes, viz., that atmospheric disturbance, consisting of variations of temperature, by hygrometric influence, atmospheric pressure, electrical tension, &c., are the exciting causes; while, on the other hand, want of light, impure air, especially from defective ventilation, in which are included malaria and all other noxious vapours, *from whatever source arising*; scanty diet, and habits induced by the irregular artificial life of many—are the predisposing causes, which by enervating and otherwise spoiling the system, render it more susceptible of external atmospheric impressions in the production of epidemic pestilence or disease."—Chap. xi.

* A History of Epidemic Pestilences from the Earliest Ages, &c. London, 1851.

membrane of the black, in the abundant and often highly-offensive sebaceous or oleaginous secretion, with which the surface of the African is naturally anointed, and which preserves him against the effects of sudden atmospheric changes. Also, the more energetic performance of the functions of the cutaneous system in the negro, as compared with the European, renders him less subject to exhaustion in the hot and humid atmosphere of his native climate.

MARSH EFFLUVIA. There is obviously, Gentlemen, something too limited in the range of the speculations of those writers who assume marsh effluvia as the cause of remittent and intermittent fever. The observations, indeed, of Ferguson, as I have already shown, tend materially to invalidate the common opinion regarding the agency of marsh miasm. He endeavours to establish the proposition, that the only condi-

tion indispensable to the supposed production of marsh poison, on all surfaces capable of absorption, is the paucity of water where it had previously abounded; a statement which, if admitted as true, removes at once all idea of a specific organic poison, and places the theory of the origin of intermittent fever distinctly within the range of natural causes, as the atmospheric vicissitudes, the heat of a mid-day sun, the precipitation of dew, the condition of the individual, &c., causes always in operation, and the efficiency of which, in exciting other febrile disorders, is universally admitted.

Still you may fairly put the question:—If we are to exclude the idea of an especial poison from marshy regions, to what agency are we to attribute the acknowledged prevalence of intermittent and remittent fever—equally in Europe as in the tropics—in all low, ill-drained, and swampy localities?

In addition to the observations which I have just submitted to you, I would further reply, that, in strong contrast to the unhealthiness of these localities, we have to place the admitted salubrity of dry and elevated regions, and their especial exemption from intermittent or remittent fever, as well as from other diseases of an asthenic character. This exemption has been attributed, and I believe justly, to the pure and dry state of the atmosphere; which, in such localities, being rarely stagnant, permits the healthy changes to be more perfectly effected in the blood, and the functions of the exhaling surfaces and secreting organs to be more energetically and more efficiently performed, so that the vital and corporeal energies are promoted and increased.

On the other hand, in low and swampy districts we have an atmosphere often stagnant, and permanently loaded with moisture, whose influence on the animal

economy is attended by opposite results, as regards the necessary changes in the blood, the secretions, the exhalations from the surface, and the organic nervous power. Hence it causes the retention in the circulation, beyond their due proportion, of elements which ought to have been eliminated, and whose retention in the system proves the predisposing, as well as the immediate cause of numerous and fatal diseases—a broad general principle in pathology, now admitted by the highest medical authorities, and supported by the irrefragable evidence of chemical research. We have, moreover, the well-established general law which determines the prevalence of fever in *all* countries, temperate or tropical, namely, the occurrence of much rain, succeeded by a calm and sultry atmosphere; with the converse fact, that a dry and cold air arrests its progress. Moisture alone will not, indeed, generate fever; as is proved by

the history of our seamen; but let the moist air become stagnant, and, if heat be super-added, how long would such immunity be maintained?—especially if over-fatigue, or any other depressing influence, be associated.

The deleterious consequences just stated will the more rapidly and readily ensue, should one of the secreting organs, as the liver, the kidneys, or the skin, be, from any cause, incompetent to the efficient performance of its healthy action, and consequently of those additional and vicarious functions by which one organ so frequently compensates the diseased or interrupted functions of another. If, again, to moisture and a stagnant state of the atmosphere be super-added high temperature, we shall find that the before-mentioned injurious agencies will be very seriously augmented, as evidenced throughout the whole circle of vital manifestations.*

* The deleterious influence of a warm, moist, and

The prevalence of the more intense forms of fever, the depression of the vital powers, and the adynamic character of disease, incidental to hot climates, have been commonly attributed to the higher concentration of terrestrial emanations, rendered more noxious by the abundant effluvia from dead animal matter, the exuviae of insects, &c., abounding in low and swampy districts. That such exhalations will render the atmosphere less pure, and deteriorate the general health, by acting on the nervous and secreting systems, I readily concede; but I do not admit that we are compelled to adopt the agency of an especial

stagnant atmosphere, is now fully recognised by our latest and best observers, as the chief cause of those direful conditions, goître and cretinism, so prevalent in some of the Alpine valleys in Europe. I have witnessed, in the mountainous valleys of Brazil, the same results, originating in precisely similar causes. In these cases, no doubt, the influence of great elevation acts as a modifying and determining agent.

morbid poison, in order satisfactorily to account for the frequency and gravity of disease in such localities. Let us for a moment reflect on the condition of the parties commonly exposed to these sources of disease.

In the permanent resident, we shall find that a low standard of health almost universally obtains; that the inhabitants are generally poor, and legibly stamped with the brand of poverty, badly clothed, badly lodged, and badly fed; an ill-developed *physique*; the abdomen prominent; the countenance care-worn, sallow, and usually indicative of suffering from some form or other of visceral derangement. In such districts, commonly low, we shall find the air warm, moist, and stagnant; uniting, in fact, all those conditions most influential in impeding and deranging the secretions and excretions, modifying the organic nervous influence, and consequently contaminating the fluids, and depressing all the mental

and bodily energies. Under such circumstances, can we wonder that any sudden or slight atmospheric change, irregularity in diet, depressing emotion, debauch, fatigue—in short, any, even the slightest, derangement of the ordinary condition of the individual, should be immediately followed by serious or fatal disease?

The temporary residents will, on the other hand, be ordinarily found to consist of soldiers or sailors—for what stranger would voluntarily sojourn in such localities?—on detached, temporary, or permanent duty. Such persons are commonly ill-lodged, often crowded, and usually deprived of their accustomed comforts, as well as amusements, and almost necessarily supplied with food unsuited to the climate; a state of things, the truth of which will be readily admitted by any medical officer who has served abroad, or in the West Indies, during the last war. Under such

circumstances, mental depression will be found pretty generally to prevail; often intemperance, harassing duties, and frequent exposure and fatigue. These deleterious agencies will be resisted for a period of longer or shorter duration, varying according to the constitution, predisposition, habits, &c., of each individual. Eventually, however, the vital powers give way—not, indeed, to the extrinsic impression on the system of a subtle, morbid poison, but to the intrinsic and united influence of all the above agencies, which depresses the nervous power, vitiates the vital fluids and solids, and thus deranges the whole animal economy, and ushers in the most formidable, dissimilar, and fatal diseases; the distinctive characters of each being determined chiefly by season, and by the constitution and predisposition of the individual. The cause is identical; the modifying and determining influences alone are different.

What influence the electrical condition exercises in this or in other constitutions of the atmosphere, the actual state of our knowledge does not enable us precisely to determine. But observation, and the records of science, and my own personal experience, sufficiently demonstrate that electrical phenomena, and the changed relations between the electrical condition of the atmosphere and that of the individual, are closely allied with the amount of health enjoyed, and with the development of disease in the human constitution. *

The foregoing evidence and statements,

* Mr. Parker, formerly surgeon to H.M. 65th Regiment, whilst in charge of the Bahia Hospital, during my temporary absence in Europe in 1829-30, engaged in an extensive series of electrical observations. The results, although not conclusive, apparently proved the powerful influence exerted by electrical phenomena over the human constitution in hot climates. Mr. Parker's attention had been previously directed to similar researches, while serving with the army in India.

Gentlemen, rest on my own personal experience of twenty-eight years in southern climates; and should they serve to direct the attention of our medical officers employed abroad, and of our colonial practitioners, to what I believe to be the essential cause of tropical fevers, and thus transfer their anxiety from the visionary terrors of malaria to the true sources of mortality in our colonies, fleets, and armies, I shall have accomplished an object which has pressed upon my mind for many years. Of its importance, many of you now present will, probably, have the opportunity of judging in the course of your medical career.

Unhappily, several of the causes alluded to are inevitable to certain of our colonial residents, to armies in the field, and to our royal and mercantile marine on different foreign stations. Still, the real sources of disease being once clearly ascertained, and

the visionary disregarded, we shall be able more efficiently to concentrate our efforts on the enforcement of such practicable measures of prevention or cure, as reason and experience may suggest in each especial emergency.

PROPHYLACTIC MEASURES. The precautions to be adopted flow so obviously from the premises, that it would be almost needless to particularise them. Still, with reference to my junior auditors, I may perhaps be permitted to indicate, and merely indicate, the more important of these. Their application, and the necessary modifications so essential in the ever-varying circumstances of individuals and bodies of men, we must refer to the judgment of those who may be entrusted with the responsibility and the power of enforcing them.

In the first place, all the ordinary and

well-established hygienic laws should be strictly attended to. The habitation, the barrack, or the camp, should be placed in a dry locality, on a moderate elevation, and well ventilated, but protected against all strong currents of wind. Lengthened or direct exposure to the sun's rays should be avoided, and all sudden vicissitudes of temperature guarded against. The use of awnings by day and by night, on board ship, should be strictly enforced. Light flannel shirts should be worn next the skin, especially when the individuals are exposed to fatigue or night duty. Sleeping in the open air, or unprotected, should, if possible, be always avoided. Clothes should be changed after exposure to rain. After exhaustion by exercise, or from any other cause, collapse must be carefully guarded against, by avoiding, for a time, exposure to the cool breeze, or by taking some slight stimulant, as coffee, wine, or a little spirits.

Spirits, otherwise, should be altogether avoided; wine should be used in great moderation, and only at dinner, and by those accustomed to its use. Generally, animal food should be used only at dinner; no supper; and no stimulating drinks, however diluted, should be taken between meals. Ripe fruit may be used in the morning, and after the middle of the day; but never after the principal meal. Temperance, *in every sense*, must be maintained. When compelled to go out early in the morning, the individual should take some support, or a dose of quinine. In warm and swampy districts, over-fatigue, or prolonged exposure to the sun, must be carefully avoided, and the use of quinine, in moderate doses, should never be neglected. The cold bath, or cold sponging every morning, on getting out of bed, should be employed. The sleeping apartment should be dry, cool, and well-ventilated, but not exposed to strong currents of air.

Of all the foregoing principles, sound and refreshing sleep is the most efficient preservative to the European constitution against the inroads of tropical disease; but, unless the above rules are observed, sound and refreshing sleep in tropical latitudes is unattainable. The *morale* must never be lost sight of; and a calm and cheerful disposition of mind must be especially inculcated.

The foregoing principles being attended to, I am satisfied that the most formidable localities of southern climates may be braved with impunity—at all events, for some years—and certainly without the slightest risk to the individual from that hitherto dreaded, but visionary enemy, marsh poison.

The question of the health of fleets and armies, in all its several details, would demand, not a passing notice, but a volume; and has, moreover, been ably treated of by

others. The preceding principles, however, constitute the basis of all rational prophylactic measures in the intertropical or warm regions of the globe, whether as applied to individuals or to bodies of men.

With these observations, I now beg leave to conclude my notice of intermittent, remittent, and continued fever, and my cursory remarks on tropical disease. I am little disposed to be the partisan of any particular dogma; indeed, the theory of marsh miasm (as formerly stated) is that to which I had almost religiously adhered as the only true and legitimate creed on this question. There is, undoubtedly, something very fascinating, and commanding withal, in the broad and sweeping generalisation developed under the hypothesis of the malaria doctrine; but it is just this precipitate adoption of general principles that has led us to so much idle warfare, and

has naturally induced us to overlook conditions from *without*, and actions from *within*, which are unceasingly in operation, and which daily and hourly, nay, every instant, display their effects; producing in one person acute inflammation, in another, continued fever, and, in a third, all the varied phenomena of intermittent, remittent, or dysentery.

But facts, Gentlemen, and facts broad and well-defined, have compelled me to adopt an opinion on the subject of malaria directly heterodox to that which reigns dominant in the republic of medicine. To detail these facts simply as they presented themselves to my notice, has been the chief object contemplated in this section of my lectures. If the inferences drawn seem to have been too large and sweeping, I may be permitted to urge, that they were such only as seemed capable of being drawn from the premises. As such, I now submit them to

you, anxious alone that the true source of the intermittent, remittent, and continued fever may be finally determined on a more precise and accurate basis than it seems hitherto to have rested on; and happy if I should have been fortunate enough to have relieved the inquiry from any of those difficulties which obviously encompass a question, as wide in its range as it is obscure in its character.

LECTURE VIII.

CAUSES OF SALUBRITY IN BAHIA—EQUAL TEMPERATURE
—COOL NIGHTS—HABITS OF THE PEOPLE—NEVER-
FAILING BREEZE—RELATION OF EPIDEMIC OR PESTI-
LENTIAL DISEASES TO A STAGNANT ATMOSPHERE—
INFLUENCE OF RECENT POLITICAL CHANGES—IN-
CREASE OF CERTAIN DISEASES—INSANITY MORE FRE-
QUENT—ELEPHANTIASIS ARABUM MORE RARE—DE-
SCRIPTION OF THE DISEASE—ELEPHANTIASIS GRÆ-
CORUM—DESCRIPTION—INCURABILITY—NOT A CON-
TAGIOUS DISEASE—MORE RARE—INCREASE OF CON-
TINUED FEVERS—OF SUPPURATIVE HEPATITIS—
IMPORTANCE OF MEDICAL HISTORY OF BRAZIL.

GENTLEMEN,—I have already had occa-
sion to comment on the peculiar salubrity
of Bahia, and its special exemption from all
serious epidemic or endemic diseases. This
would appear to depend on a variety of
causes—moral and physical.

CAUSES OF SALUBRITY IN BAHIA. In the first place, we have an EQUABILITY AND CIRCUMSCRIBED RANGE OF TEMPERATURE—a freedom from atmospheric vicissitudes, unequalled in any other region of the globe. How different is this from Italy, for example, or from America, where, while serving on the medical staff of the expedition against New Orleans, in 1814, I repeatedly observed, within a few hours, a variation of 40° of Fahrenheit; while in Bahia, as I have already stated, the thermometer rarely descends below 72° in winter, or exceeds $82\cdot5$ in summer, the daily range being about 6° . The heat, moreover, is never oppressive, being always tempered by a strong and never-failing sea-breeze, and by occasional showers at all periods of the year.

The nights, too, are at all seasons cool and agreeable, with slight deposition of dew,—the result of the limited range of temperature, and always admitting of sound

and refreshing sleep. To this latter circumstance I am disposed to attach the very highest importance, from having witnessed in other hot climates the direful results on the animal economy of a long succession of hot and sleepless nights, in predisposing the system to be impressed by the various and fatal maladies incident to climate.

Farther, the HABITS, OCCUPATION, NATURAL DISPOSITION, and TEMPERAMENT, of the inhabitants, exercise a highly important influence in warding off or modifying disease. As elsewhere stated, the Brazilians,—that is, the masses—are temperate, orderly, cheerful, impressionable, never long distressed by past, nor troubled by coming events. Chiefly occupied in agricultural pursuits, their wants are few, and those are abundantly and readily supplied. The mind is not over-exercised, and little disturbed by ambition or politics, religion or commerce. For a community thus consti-

tuted, placed in such a climate, and under such peculiar circumstances, we might legitimately infer, *à priori*, freedom from grave disease—an inference fully borne out by experience.

In addition to high temperature, the chief objection to be urged against the climate of Bahia is its great humidity. The deleterious effects, however, of this condition of atmosphere on the animal economy are tempered, if not entirely corrected, by the influence of a NEVER-FAILING BREEZE, so that a stagnant, or even a calm, state of the atmosphere, is utterly unknown. If this were otherwise, I am satisfied that Bahia, inundated with the most offensive and noxious animal and vegetable exhalations, with a total neglect of cleanliness, and absence of those police and sanitary regulations so essential to the public health in other countries, would prove a very Golgotha, modified, however, no doubt, by

the habits, constitution, and temperament of the people. Since these pages were written, however, Brazil has been desolated, for the first time in its history, by yellow fever, attributed by some to introduction from abroad, by others to epidemic influence, originating in obvious and unprecedented atmospheric changes—as increased temperature, unusual rains, failure of the accustomed sea-breeze, &c.* The comparative frequency of ague in the more elevated positions or localities of the city, as compared with the lower, I have already noticed and explained.

On reference to history, we cannot but be forcibly struck with the fact, that, in every account of an epidemic or pestilence,

* It would, however, appear from the Portuguese historian Pitta, Humboldt, and others, that the yellow fever had already, in the sixteenth and seventeenth centuries, ravaged Brazil; and its present reappearance, after an interval of ages, affords certainly a singular anomaly in the history of disease and climate.

either of ancient or modern times, we have, in addition to the ordinary details of war, famine, &c., *one* observation almost universally recorded, namely, that all were preceded by wet seasons or inundations, followed by excessive and unwonted heat, with a *calm or stagnant state of the atmosphere*. During the pestilence which ravaged Rome in 262-3, the air is described by Eusebius as having been so infected, as to cover all objects with a mould or dew, similar to that which arises from putrid bodies; in other words, the atmosphere was exceedingly humid, and hence productive of those effects which are daily observed in all intertropical countries adjoining the sea-board, where the breeze, otherwise perfectly pure and healthy, comes loaded with moisture, and rapidly covers with a mould (cryptogamic vegetation), such as described by Eusebius, all objects exposed to its influence. With its effects

on surgical instruments, every tropical practitioner is unfortunately but too familiar.

POLITICAL AND OTHER CHANGES IN BRAZIL
—THEIR INFLUENCE ON DISEASE. Though the climate of Bahia, and the physical condition of the country, will probably never undergo any serious amount of change,* yet other great and rapid mutations are in progress, affecting the social, moral, and political relations of the people, and which appear to have already exercised some influence on the character and frequency of certain classes of diseases.

After a struggle of about two years, the establishment of Brazil as an independent empire was finally effected in 1823; and a political constitution, greatly unsuited to the intellectual and social advancement of the people, was conceded by the late emperor, Peter I. Since this period, Brazil and

* Late events render this point doubtful.

its population have undergone a series of remarkable and comprehensive political and social changes. From the strict and simple forms of despotic government, they have passed, at a bound, to one almost of licence, including household suffrage, popular legislative assemblies (imperial and provincial), open courts of law, trial by jury, local justices, and a national guard elected on popular principles. As might have been foreseen, this sudden and premature concession of political privileges to a people yet in the infancy of civilisation has been attended by great and numerous evils, mingled, it must be admitted, with many advantages.

In the intoxication of a new-born freedom, the empire has anticipated and wasted the national resources in foreign wars, and an endless succession of intestine broils, one province being arrayed against another. Luxury has largely increased; the laws

have been inefficiently or corruptly administered, and a lax morality has but too generally pervaded the whole community. On the other hand, an extensive and well-organised system of national education has been established throughout the empire; the slumbering intellectual powers of the nation have been aroused; wealth and intelligence have been developed; political and military ambition awakened; commercial enterprise created; agriculture revived; and, of all those mighty powers which advance and mould societies, the controlling influence of religion has alone remained stationary, if it has not retrograded. The priesthood, deprived of wealth, power, or influence, has utterly lost its *prestige*, unless, perhaps, with the very lowest classes of the community—a matter of curious speculation as regards the cause, and of vast importance as regards its future results on the character and institutions of the Brazilian people.

Coeval with these great and rapidly advancing changes in the social, moral, and intellectual habits of the nation, we can already discern some of those evils too commonly attendant on increased wealth, luxury, and intelligence. Anxieties, excesses, passions, are largely multiplied; and traces of that premature "wear and tear," so painfully characteristic of highly civilized society, begin to be distinguishable amongst certain ranks of the hitherto contented and indolent Brazilians. That such influences should effect some modification in the frequency and character of certain classes of diseases, we might reasonably infer; and the justness of the inference is fully established by my own experience, as well as by that of my colleagues.

Within the last ten years, diseases affecting the cerebral and nervous systems, pulmonary disease, and especially diseases of the heart and great vessels, have been of

much more frequent occurrence than they were during the first ten years of my residence in Bahia. Even while writing these lines, I have before me a letter from my successor at the British Hospital at Bahia, Dr. Paterson, intimating the deaths of three of my old friends and patients, viz. Dr. Paulo, dean of the faculty of medicine, and late secretary of state for the home department (a combination of offices which will sound oddly enough to British ears); Senhor Feital, the consul for Portugal; and Senhor Joaquim Bento de Figueiredo, late chief of the provincial treasury. The two former died of apoplexy, the latter of disease of the heart, and none of them were advanced beyond the middle age.

INSANITY, formerly a disease of rare occurrence, has presented itself, within the last few years, much more frequently. This is shown by the results of my individual

experience, and by that of my colleagues, as well as by the admissions to a private hospital for the reception of Brazilian patients of all classes, and with all diseases, which was established some years ago by myself, conjointly with my valued and talented friends, Drs. Persiani and Abbott, the latter surgeon-in-chief to the Misericordia Hospital, and professor of anatomy to the Faculty.

The most frequent character of the hallucinations bore distinctly on politics or commerce—rarely on love—and, on two occasions only, within my long experience, on religion; and one of these cases was a young Mahomedan slave. It is, moreover, sufficiently remarkable, that, notwithstanding an extensive professional intercourse with the religious orders, the convents and nunneries of Bahia, I have never yet met with a single case of religious mania, in either sex, among the numerous inmates of these insti-

tutions. Another remarkable fact is, that there does not, I believe, exist at this moment a single establishment, public or private, especially devoted to the reception of the insane, throughout the whole extent of the Brazilian empire—certainly none in the province of Bahia. The “mad doctor” is a species of the profession utterly unknown in Brazil; though I venture to predict that the history of the next twenty years will tell a different and a sadder tale.

ELEPHANTIASIS ARABUM, or the Barbadoes leg, though still of frequent occurrence, is now less commonly met with among the white population, than on my first establishment in Brazil. Its characteristic symptoms and morbid results you will find accurately portrayed in almost every work on practical medicine; and I notice it here chiefly on this account, that in Brazil it originates invariably from erysipelas, and never from inflammation of the lymphatic

system, as commonly laid down by authors. The parts most subject to attack are, first, the lower extremities; then the scrotum in males, and the mammæ in females. Animals are also apparently subject to the disease, especially the horse; and the part commonly attacked is the scrotum. In numerous instances, the attacks become periodic, recurring monthly; and occasionally, at the earlier periods of the disease, the inflammation may be suddenly translated from one region to another; in the later periods this scarcely ever occurs. The natives of all countries are liable to be attacked, though seldom (unless from constitutional predisposition), until the system has been deteriorated by climate; and when the European becomes subject to regular attacks of erysipelas, no stronger indication can exist for an immediate change of residence.

The constitutional and local symptoms,

especially in the early attacks, are often very severe; but they are purely those of erysipelas, and are to be met by the remedies appropriate to that disease. The causes are also those of erysipelas; namely, constitutional predisposition, deteriorated general health, and sudden atmospheric changes. In the latter stages of the malady, constitutional disturbance is rarely manifest; and equally rare is inflammation of the lymphatic vessels or glands of the extremity attacked; while, on the other hand, these latter affections (inflammation of the lymphatic vessels and glands, especially of the groin), are of very frequent occurrence about the change of the monsoon, in April. They originate in some obscure constitutional disturbance, and often prove exceedingly tedious and difficult to deal with, the general health suffering, and the glands, notwithstanding every effort to resolve them, going on occasionally to suppuration,

after which the health is at length re-established. In these cases there will be no local injury, nor inflammation of the skin or cellular tissue; and in no instance have I witnessed such attacks followed by hypertrophy.

In the Barbadoes leg, I have found all specific treatment, as by mercury, iodine, &c., either useless or prejudicial. You can only hope to benefit your patient by improving his general health, on general principles, so as to prevent the recurrence of the erysipelas; for each succeeding attack adds, by an additional effusion of sero-albuminous deposit, to the morbid enlargement of the part. If your patient be European, urge change of climate; this is your grand remedy, and, if resorted to in time, will be almost uniformly successful.

Though sometimes counselled in such cases, the knife can rarely be resorted to with safety or advantage. A case in point

occurred not long since, in my own practice. Senhor Luiz Joze dos S., one of the principal inhabitants of Rio de Janeiro, suffering from scrotal hypertrophy, came to London for advice, and placed himself under my care. I discountenanced an operation, for which he was anxious, and advised palliatives. This advice, I soon perceived, was unpalatable to a man who had travelled 5000 miles of ocean to have his scrotum diminished; and I recommended that he should take the opinion of Sir Benjamin Brodie, to whom I accompanied him. Sir Benjamin also decided against an operation. Senhor S. remained a short time longer under my care, and then proceeded to France, accompanied by his son, who subsequently informed me that the operation was, without hesitation, performed in Paris—and he died.

ELEPHANTIASIS GRÆCORUM, or tubercular elephantiasis. This singular and fatal dis-

ease is of common occurrence at Bahia, where a special hospital—Hospital de São Lazaro—about three miles from the city, has long been established for the reception of leprous patients. In the year 1829, I was furnished with permission from the Brazilian authorities, affording me unlimited access to the establishment and the records connected with it; and of these I fully availed myself prior to my first return to Europe, in 1830. On this occasion, I had prepared for publication a history of the disease, embracing the details of thirty cases. In my passage through Liverpool, my friend, the late Mr. William Gill, surgeon to this hospital, having expressed a wish to peruse the manuscript, I left it, with directions for its subsequent transmission to Dr. William Stokes of Dublin. The parcel, however, never reached its destination; and our most strenuous efforts to trace it, both here and in Dublin, proved altoge-

ther unavailing. The original notes being in Brazil, and no copy existing, all idea of publication at the time was necessarily abandoned. On my return to Bahia, in 1831, I found these notes, with other papers, rendered utterly illegible, by the ravages of that most destructive of tropical insects, the white ant. Subsequent bad health, and excessive occupation, prevented a fresh investigation. With this preface, I shall now submit to you a brief summary of the more prominent features of this extraordinary malady, as it appeared at the Hospital de Sao Lazaro, and in my own private practice.

The disease is known to the Brazilians under the name of Morphea, or Mal de S. Lazaro, and is not only one of the most incurable, but one of the most singular maladies to which the human race is subject. It spares the native of no quarter of the globe, whether he belongs to continents

or to islands; though I may observe, that I have never witnessed the disease in a native of Great Britain.* It is, moreover, worthy of remark—especially as confirming the observations of others—that a large majority of the patients in the Hospital S. Lazaro were from the sea-board, or districts adjoining.

* Since these pages were written, I have been consulted by an English gentleman who had long resided in hot climates, and in whom the disease is clearly developed. This gentleman had already consulted several of the most eminent practitioners in London, among whom was Mr. Erasmus Wilson, who employed an active and judicious treatment, but, I need scarcely add, without the slightest influence on the disease. I have put the patient on large doses of taraxacum, with liquor potassæ, in the Vichy mineral water, vapour baths, sedative local applications, with strict diet and regimen. Under this system—now some months in operation—the disease has simply *not progressed*; and, as the gentleman is still in the prime of life, and his constitution not seriously impaired, and as he has changed the climate and mode of living, it is *just possible* that the disease may be arrested. I am not informed whether Mr. Wilson was cognisant of the

The disease attacks alike all ages, all sexes, and all colours—the white, the mulatto, the aboriginal American, and the negro; it affects all ranks—the higher, however, more rarely; though in this there may be some fallacy, from not making due

essential character of this most singular and intractable malady, so rarely witnessed in this country. Even at the Hôpital St. Louis, in Paris, I never met with a single case, although a pretty constant visitor to the establishment, during a period of seven months, in 1842-3. Visiting Stevens' Hospital, in Dublin, in the winter of 1830, with Mr. Wilmot, I found a well-marked case in a boy, a soldier's son, about twelve years of age, and born in India. He had been in the hospital for several months, and his case had been the subject of much doubt and discussion. No other instance of true "lepra Græcorum" had, I was informed, been observed in Dublin. In the spring of 1841, I also saw a well-marked case in the Military Hospital at Milan. The patient had never been out of Italy. And, in the autumn of 1845, I met with another case—though not so distinctly marked—in the great hospital at Vienna. This patient had been in hot climates. These are the only cases of this fearful malady which I have met with in Europe.

allowance for the disproportion of the classes.

In the Hospital S. Lazaro, I found, in some instances, the child diseased, while the parents were free: again, the parents were sometimes confirmed lepers, but the children were unaffected. Sometimes the father was a leper, while the wife and children were clean; and sometimes the wife alone was leprous, and the husband and children healthy. In some instances, the grandchildren of lepers exhibited the disease in its most aggravated form, while the immediate parents were perfectly free from the malady; and, finally, in other instances, the evidence for deeming the disease distinctly hereditary would appear irresistible.

In the first glance at a leper, especially in the more advanced stages, there is that which supersedes all necessity for interrogation—a *something* in his countenance and general appearance, distinct from anything

to be found in any other disease to which humanity is doomed. The harsh and scurfy skin—the small, red, and sunken eye—the heavy, hairless, overhanging eyebrow—the wrinkled and hypertrophied forehead—the irregular tumid lips—the enlarged, misshapen ears—the swollen, tuberculated nostrils—the sunken nose—the distorted or mutilated limbs—with the husky, discordant voice and fœtid breath—reveal, at once, the presence of the most loathsome and the most incurable of human maladies. Premature old age was visible in all; and life was rarely prolonged to the sixtieth year. Generally, the progress of the disease is much more rapid. In every instance, the sexual passion was described as being at first weak; and, finally, at various intervals, totally extinguished. The *mammæ* in the females had disappeared; and in the males, without a single exception, I found the testes much atrophied, and the genera-

tive organs presenting a diminutive and shrivelled appearance. The *libido inexplibilis*, noticed by authors, is a pure fiction.

In a majority of instances, the approach of the disease is first distinctly manifested by numbness and loss of feeling in the fingers and toes, extending gradually to the extremities, but rarely to the trunk. A scurfy and unperspirable condition of the skin follows, with furfuraceous deposits beneath and around the nails of the fingers and toes, by which they are soon elevated, and often drop off. Tubercles of different sizes, and in different parts of the body, but chiefly about the face and extremities, succeed, sometimes rapidly, sometimes more slowly. Commonly, but not always, the general health suffers at the same time. The fingers and toes shrink, and become distorted by the permanent contraction of the integuments and flexor muscles. The skin cracks, and deep fissures form over the

flexures of the joints internally, on which unhealthy, fistulous, burrowing ulceration supervenes, unattended by pain or swelling. Tumefaction of the lymphatic glands follows; the smaller joints drop away, and heal; ulceration seizes the throat and fauces; the bones of the nose give way; the appetite, nevertheless, continues good. The large articulations are finally attacked; and the constitutional symptoms now make rapid progress. Emaciation, fever, palpitation, oppressed respiration, and diarrhœa, ensue, shortly putting a termination to the most hapless state of existence that human imagination can conceive.

The distinction attempted to be established by authors between *lepra tuberculosa* and *lepra anæsthesiaca* is without foundation. They are simply different stages of one and the same malady, as indisputably proven by the patients in the Hospital Sao Lazaro, in whom I found the chief charac-

teristics of both clearly developed at one epoch or other of the disease.

This disease may be justly considered as altogether unmanageable. Every remedy which science, chance, or desperation could suggest, has been successively resorted to; and, I must acknowledge, not only with no permanent advantage, but apparently without the *slightest influence* in arresting or modifying the progress of the malady, and often obviously hurrying on the fatal issue. The disease, in short, is mortal, and without hope; and the fatal termination, in the great majority of cases, is ushered in by some form or other of diarrhœa, or disease of the digestive viscera—more rarely of the lungs.

Neither has *post-mortem* research thrown the faintest ray of light on the essential nature, or proximate cause, of this fatal and singular malady. Every important organ of the body has been found in different instances affected, and with almost

every species of disorganisation; though, as might have been expected from the ordinary termination of the disease, the gastrointestinal mucous membrane, with the Peyerian and mesenteric glands, have presented the most constant evidences of disease; namely, numerous tubercles, softening and ulceration; cicatrices I have never myself observed, nor have I heard that they have been seen by others. In every case tubercles have been detected, to a greater or less extent, in the air-passages and lungs, often in the liver and spleen, more rarely in the kidneys, and still less frequently in the brain and its membranes.

Causes. Having observed the disease developed in the most opposite states of the animal economy, under the most dissimilar conditions of rank, age, sex, climate, and diet, I cannot resist the conviction, that the causes ordinarily assigned by authors are purely imaginary.

In regard to the highly important question, so often mooted—whether the disease can be transmitted to the healthy, unconnected by ties of blood?—the following fact would seem to determine the question clearly and satisfactorily in the negative.

Attached to the Hospital S. Lazaro are thirty-five servants, many of them slaves, and all in constant communication with the patients, as nurses, washerwomen, orderlies, &c.; and, although no especial precautions had ever been adopted, yet, within the records of the establishment, or within the knowledge of the officials attached to it, not a single case of infection has been recorded among the servants or officers of the institution. The important fact of their actual freedom from leprous disease, I verified myself, by repeated and careful examination, during the period of my investigations at the Hospital.

It is gratifying to know, from my own

experience, and from that of my colleagues, that, within the last twenty years, this melancholy disease has been less frequently met with; although the causes which have influenced it do not, in the present state of our knowledge, admit of any satisfactory explanation.

FEVERS of the continued type—the symptoms being displayed chiefly through the stomach and intestinal canal—are obviously increasing; they also partake more of the asthenic character, and are attended more frequently by fatal results than formerly.

HEPATITIS, terminating in suppuration, though still greatly below the average of other hot climates, has decidedly increased in frequency within the last ten years; and this opinion is also borne out by the experience of my professional brethren.

Other modifications in the intensity and frequency of certain of the diseases incident to Bahia, have appeared to me sufficiently

well marked; yet, as the evidence on which my convictions rest is in some respects incomplete, and liable, moreover, to certain sources of fallacy, I do not conceive myself justified in laying before you any statement on which a doubt may rest in my own mind. I would observe, further, that my chief motive for making the present cursory observations consists in the hope, that some of my well-informed and more talented brethren—native or foreign—now so numerous in all the provinces of Brazil, may be stimulated to fill up and extend the picture here so faintly and imperfectly traced. Judging from my own experience of professional feeling in England, I would safely guarantee that their labours would be kindly and considerately received by the European profession; and I am, moreover, satisfied that the novelty and importance attached to any medical history of Brazil would command a full measure of public interest

and support. In truth, it would be difficult to point out another subject of deeper or more legitimate interest to the great body of the profession. A climate unparalleled in the other tropical regions of the world; the inhabitants peculiar in their moral, social, and intellectual relations, and presenting the rare and interesting spectacle of a large community in the condition of rapid transition into new forms and combinations of social existence; the face of the country changing under the march of civilization and agricultural improvement; disease obviously and profoundly influenced by the climate and the character of the people; and as evidently undergoing certain and striking modifications under the influence of the changed conditions before alluded to;—all these circumstances taken into consideration, lead me to hope that, however imperfect may be the present sketch (and of its imperfections no person

can be more fully aware than myself), it may eventually prove not entirely devoid of interest and utility, as a standard, at least, of comparison, to future observers.

In my next, and concluding lecture, Gentlemen, I shall present you with a slight sketch of the medical institutions of Brazil, including those points in which they differ so remarkably from our own.

LECTURE IX.

RECAPITULATION OF LAST LECTURE—MEDICAL EDUCATION IN BRAZIL—COLLEGES AT BAHIA AND RIO DE JANEIRO—CONSTITUTION—PROFESSORS AND SUBSTITUTE PROFESSORS, THEIR DUTIES—DEGREES, HOW OBTAINED—PHARMACEUTICAL STUDENTS—MEDICAL SERVICES ALWAYS PAID FOR—BRAZILIAN CONSULTATIONS—METHOD OF HOLDING THEM—PROMINENT STATUS OF PHYSICIANS IN BRAZIL—THEIR CHARACTER—TONE OF PROFESSIONAL FEELING—THE AUTHOR'S RECEPTION AND TREATMENT—OFFICES OF STATE FILLED BY PHYSICIANS—DR. LINO COUTINHO, HIS POLITICAL CAREER AND CHARACTER—QUESTIONABLE POLITICAL CAREER OF SOME OF THE PROFESSORS—INSURRECTION OF 1838-9—CONCLUSION.

IN my last lecture, Gentlemen, I submitted to you some observations on the peculiar salubrity of the climate of Bahia, and its especial exemption from all serious endemic

or epidemic diseases; and ascribed this salubrity to a variety of moral and physical causes;—to the singular equability and circumscribed range of temperature, and a freedom from atmospheric vicissitudes, unequalled in any other region of the globe—the daily range of Fahrenheit's thermometer being about 6° , whilst in other countries we often witness a variation of 40° in the space of a few hours;—to the fact, that the nights are, at all seasons, cool and refreshing, with slight deposition of dew;—and to the never-failing breeze, which renders a stagnant, or even calm, state of the atmosphere altogether unknown. I directed your attention to the social, moral, and political character of the Brazilian people, and its modifying influence on their diseases. I also glanced at those changes which increased wealth, luxury, and intellectual development, appear to have exercised in determining the character and the frequency of

certain maladies; such changes being especially manifested in the increased frequency of diseases of the cerebral and nervous systems, pulmonary disease, and disease of the heart and great vessels. Fevers had partaken more of the continued type and asthenic character, and had been more frequently attended by a fatal result. I pointed out that insanity, though still rare, as compared with other countries, had largely increased; and in pointing out the remarkable fact, that, up to the present moment, there did not exist a single establishment, public or private, especially devoted to the reception of the insane, throughout the whole extent of the Brazilian empire, I ventured to predict that the next twenty years would tell a different and a sadder tale.

In the hope that the subject may prove of some interest, not only to you, but to my professional brethren in general, I shall

conclude the present series of lectures with a few brief observations on the national medical institutions of Brazil, and on the position and standing of, and the tone of feeling which pervades, the medical profession in a community placed under circumstances so widely different from our own.

MEDICAL COLLEGES IN BRAZIL. In the entire Brazilian empire, there are two national faculties of medicine, termed *Escola Imperial de Medecina*; one established at Rio de Janeiro, the other at Bahia—the present and former capitals of Brazil. Both are constituted exactly alike in laws, forms, number of professors, modelled, with very trifling difference, after the constitution of the *Ecole de Médecine* of Paris. Each college consists of fourteen professors, and six substitute-professors, with a director and a vice-director, answering to our own dean and vice-dean of the faculty. The

latter are named by Government, from a triple list sent up by the professors every third year, and discharge the ordinary duties of their chairs, being only exempt from attending the examinations. They possess a limited controlling power over their college, and constitute the official channel of communication with Government and public bodies, on all matters relating to public health, prisons, &c. The duties of professor-substitute are explained in the name. When illness, or public employment—the latter not unusual in Brazil—interferes with the duties of the professor, his chair is supplied by the substitute: both are appointed, as in France, by *concours*. Most of the older members have graduated in Portugal, Scotland, France, or Italy. Both classes receive a fixed income from the State, and derive no emolument whatever from pupils and examination fees, &c., which are applied to public purposes connected with the

college. The income of the professor was fixed at twelve hundred *mil-reis* per annum (about three hundred pounds), when first established; and that of the professor-substitute at eight hundred *mil-reis*. Both enjoy the right of retirement on their full salary, after twenty years' service, or when incapacitated by age or infirmities. A travelling professor is elected by *concours*, by the faculty, every four years, for the purpose of investigating, in the different countries of Europe, the latest improvements and discoveries in medicine and the collateral sciences, an account of which he regularly transmits, in formal reports, to his college. His expenses are defrayed by the State.

The medical faculty consists of the following chairs:—1, physics; 2, botany; 3, chemistry; 4, anatomy; 5, physiology; 6, external pathology; 7, internal pathology; 8, *materia medica*; 9, hygiene; 10, legal

medicine; 11, operations; 12, midwifery; 13, clinical medicine; 14, clinical surgery.

In addition to the professors, there is a secretary (medical), treasurer, librarian, and chemical assistant—all elected by the faculty.

The order of study is as follows:—1st year, medical physics and medical botany; 2d year, chemistry, and general and descriptive anatomy; 3d year, anatomy and physiology; 4th year, external pathology, internal pathology, pharmacy, and materia medica; 5th year, operative medicine and midwifery; 6th year, hygiene, history of medicine, and legal medicine.

All examinations are public, and the subjects are drawn by lot.

The titles conferred by the faculty are only three, viz., Doctor in Medicine, Apothecary, and Midwife. The latter is specially educated and examined.

In each chief city there are commonly

three or four large hospitals—the Mizericordia, or Civil Hospital, possessed of ample funds from endowments, legacies, and certain taxes; the Military and Naval Hospitals; and, in Rio, Bahia, and Pernambuco, Leper Hospitals. There are also infirmaries attached to convents. Private subscriptions to institutions are utterly unknown.

The academical session lasts for eight months—from 1st March to 30th October—lectures being delivered daily (with some exceptions) by the professors or their substitutes. The professors of clinical medicine and surgery have the right of selecting their cases from the Mizericordia Hospital.

The student, previous to matriculation, must take his degree in arts; and the curriculum is the same for all, viz., six years to obtain the degree of Doctor in Medicine. The examinations are conducted as in Paris.

For the degree of Doctor in Surgery—which, however, is not essential—a subsequent and special examination must be undergone, as in France.

All students are classified, on entering college, into *medical* and *pharmaceutical*; and both are obliged to obtain the degree in arts before they can be matriculated, and to have completed their sixteenth year. The pharmaceutical student obtains his diploma of pharmacy after three years' study; while that of medicine can only be obtained after six years. The student of pharmacy is obliged to repeat the courses of medical physics, botany, chemistry, pharmacy, and materia medica; while one course only of each is required from the medical pupil. The pharmaceutical student is obliged to attend for three years in a pharmacy, after the conclusion of his academical studies. He then undergoes an examination by the faculty, and publicly

defends a thesis to obtain his diploma. His duty afterwards, as apothecary, is strictly limited to the sale of drugs, and the compounding of prescriptions. He is never consulted professionally; and, did he attempt to apply a remedy for the cure of any disease, he would be immediately fined 50 *mil-reis* by the municipality, for the first, and an increasing fine for every subsequent offence; and, did he still persist, his licence would be withdrawn. On the other hand, the medical practitioner is strictly prohibited from the compounding or sale of medicines, in any shape or form.

STATE OF THE MEDICAL PROFESSION IN BRAZIL. The professors and substitutes are, like others, engaged in private practice, and not uncommonly hold some other public or medical office, of which there are a considerable number, and all paid—

honorary medical appointments being *utterly unknown* in Brazil.

So deeply, indeed, is the system of payment for medical services (under what circumstances soever rendered) interwoven with public opinion in Brazil, that, when the medical officers of an hospital—the Mizericordia or Hôtel-Dieu for example—deem it necessary to call a consultation, the regular consultation fee is invariably transmitted to the consultants; and I have myself repeatedly received it, through the treasurer of the hospital, who requires an authenticated receipt, in order to its formal insertion in the annual accounts of the establishment.

The system of consultations, or *juntas*, as they are termed, prevails to an almost inconvenient extent in Brazil. No serious case is ever treated without repeated *juntas*; and the number of consultants is rarely under three or four, and frequently much

more numerous. The mode of holding the consultation, too, is peculiar, and, so far as I am aware, different from that pursued in any other country. After the patient has been examined by each in turn, the consultants seat themselves, commonly in a semicircle, around his bed, while, forming an outer circle, are seated the friends and relatives of the family. Silence being obtained, each physician successively, in hearing of the patient and his friends, enters formally into the history, symptoms, diagnosis, and treatment of the case; often, in fact, delivering his opinion in the form of an oration. And, on more than one occasion, I have heard, from the surrounding auditors, the *appoiado*, or parliamentary "hear, hear," of approbation, or the *nao, nao*, of dissent from the opinions of the speaker. In the event of disagreement, the treatment is decided by a simple majority; or, should the votes prove equal, an

additional physician is frequently called in, whose vote decides the question. This system, though by no means devoid of advantages, is, nevertheless, counterbalanced by so many and obvious inconveniences, that, for the interest of the patient, it ought certainly to be abolished; and would be, probably, if the question rested entirely with the profession.

To return to the public establishments. In case of accident or sudden emergency, an officer of the hospital not being at hand, another practitioner is immediately summoned, and *paid his regular fee*. In fact, seeing that the barrister, the attorney, the priest, and every other class of the community, exacts remuneration—and large remuneration too—for time and services, the Brazilian cannot be made to comprehend the grounds on which the *doctor alone*, after long years of study and expense, can be expected to devote his time, his health, and talents,

gratuitously, to the people. The liberal and enlightened British public will, no doubt, open wide their eyes at such unanswerable evidence of barbarism and defective reasoning powers, in a nation making pretences to civilisation; and the proof will, I fear, be deemed too conclusive against my friends on the other side of the line, to admit of my advancing a single word in their defence. I may be permitted, however, to urge in mitigation, that people often view matters differently to the south of the equator. Be this, however, as it may, one thing is quite certain—that the Brazilian is a much more independent agent than the British physician.

The higher orders, too, of the medical profession, occupy a much more prominent position, *quoad* the public, than their brethren of Europe; and this, perhaps, may be accounted for by their superior education and knowledge of the world, as com-

pared with the generality of the upper classes in a country circumstanced like Brazil. The Brazilian physician, especially of the old school, is generally well acquainted with European medical literature, especially the French. He is characterised by great liberality of feeling; is little disposed to jealousy, and *altogether devoid of professional intrigue*. His habits are usually simple and inexpensive, and—perhaps another proof of Brazilian barbarism—the public never dream of measuring his talents by the extent of his establishment, or the splendour of his equipage. In addition to his private practice, he commonly holds some independent public appointment or professorship; and, through the *concours*, he seeks to rise rather by the approbation and respect of the profession, than by those humiliating practices by which, *on dit*, his European brother too often mounts to notoriety and fortune.

As regards the *tone* of feeling (a most important consideration) which generally pervades the medical profession at Bahia, my own career will perhaps afford the best, being a practical illustration. An utter stranger, inexperienced, ignorant of their customs, laws, and language, I was received by my professional brethren from the first with kindness, and in time admitted to their confidence. I subsequently derived the most solid advantages from their support; and was finally honoured by the warm and *unanimous* public expression of their approbation and friendship, after a close and extensive intercourse of three-and-twenty years.

Since the establishment of a constitution in Brazil, many of the highest offices in the state have been filled by physicians. Independently of provincial offices, the professors of the *Escola de Medecina da Bahia*

have, within my own day, supplied numerous deputies and senators to the national legislative assembly, besides two secretaries of state—one, my friend Dr. Paulo, dean of the faculty of medicine, already alluded to; the other, Dr. Joze Lino Coutinho, a man prominently distinguished in the annals of Brazil, and whom I knew long and intimately prior to, and during his elevation, and subsequently to his fall from power. On my appointment to Bahia in 1819, I found Lino Coutinho a simple physician, apparently absorbed in the duties of his profession. In February, 1821, the revolution burst forth in Bahia, and, after a brief and doubtful struggle, proved successful. The governor, Count da Palma, was deposed, and replaced by a revolutionary *junta*, to which my friend Lino Coutinho was not only secretary, but the very life and soul of the revolution. In 1823, the independence of Brazil being for-

mally acknowledged by Portugal, Dr. Lino Coutinho was elected deputy for his native province, to the general legislative assembly at Rio de Janeiro. He was subsequently nominated to the senate; and finally assumed the seals of office as secretary of state for the home department. He was long the idol of his party: like other idols, he outlived his worship, and returned to die in his native city, where, excepting his immediate household, the narrator of these brief remarks was the sole attendant on him who had so long swayed the destinies of millions.

Sprung from the humblest origin, Lino Coutinho was unquestionably no ordinary man. In religion a deist, in principle a republican, and by nature profoundly ambitious, he was eloquent, fertile in resource, and never depressed by adversity, nor elated by success. Many and grave defects he certainly had; and, as certainly, many

virtues. The present, however, is not the place to discuss either. I will, therefore, only add—what his bitterest enemy cannot deny—that, though he had long distributed the wealth and honours of the Brazilian empire with a lavish hand, yet Lino Coutinho himself—like his colleague Dr. Paulo—died poor, unpensioned, and untitled—no doubtful epitaph, I ween! and rarely to be found inscribed on the tombs of secretaries of state, from any other profession, in any country.

So much, Gentlemen, for the honour of medicine; a regard to which influenced, in part, the introduction of this brief episode. As an impartial historian, however, I am bound to record, that the professors of the *Escola Imperial de Medecina da Bahia* have figured, from time to time, in other conspicuous, but more questionable positions. Few, indeed, of the revolutionary attempts which, during the last thirty

years, have so frequently shaken the Brazilian throne, are unconnected with the name of a professor of medicine. The last frightful attempt at revolution and plunder, in 1838-9, was led, if not organised, by Senhor Sabino, a substitute professor of the *Escola de Medecina*, and a man of daring and desperate character.

The horrors of Bahia during this attempted revolution will not be readily effaced from the remembrance of those who witnessed them, and never from my own; for, during that awful period, I had charge of the Mizericordia Hospital, containing upwards of five hundred patients—most of the ordinary attendants and medical officers of the establishment (among them my friends Drs. Cabral and Abbott) having been compelled to fly for safety to the Imperial camp. The city, in a state of starvation, was blockaded by sea and land; all legitimate authority was in abeyance;

and the lives of the inhabitants were at the mercy of an infuriated black and mulatto mob. Money was valueless, as a means of procuring the necessaries of life; and, as regarded my unhappy charge in the Misericordia Hospital, we were deficiently supplied with medical stores, and had no fresh provisions, no vegetables, no flour; and even the supply of salt meat and fish, towards the last, became scanty and unwholesome. My heart sank within me as I daily approached the gates of the Hospital; and, even now, I can scarcely repress a shudder, as I recall to remembrance the human misery I was compelled to witness, without the power to aid. Happily, my professional brethren in England are beyond the reach of scenes like these!

My friend, Dr. Paterson (now present), to whom, on quitting Bahia towards the end of the blockade, I transferred my melancholy charge, can bear ample testi-

mony to the painful reality of these reminiscences.

The foregoing brief outline will, I trust, make sufficiently apparent the broad distinction which at present obtains in the relative position and range of occupation of the British and the Brazilian physician. Changes, however, are already shadowed forth in Brazil; but these I leave to be recorded by future observers.

I must now, Gentlemen, bid you adieu in my capacity of lecturer; and I have to thank you for the attention with which you have honoured my somewhat desultory observations. At the call of duty or interest, it is more than probable, in the course of your professional career, that some of you shall visit foreign and southern climates, and be there placed under circumstances of inexperience and responsibility similar to my own. I have been, therefore,

anxious to lay down certain principles, based on personal knowledge, which may serve to guide you in the prevention and treatment of disease, until observation and experience shall have enabled you to determine these important questions for yourselves. I could have largely extended the present series of lectures by the details of numerous cases; but this I have purposely avoided, convinced, by experience, that cases are useful to you, only in so far as they may serve to elucidate and establish sound general principles; and these principles your own judgment must apply to the ever-varying modifications of individual diseases.

Should health and leisure permit, I may possibly recur, at a future day, to some of the more important questions involved in the preceding lectures; for man separates himself with difficulty from the recollections of a country in which he has passed

the most active period of his career—a country, moreover, endeared to myself by numerous associations—British and Brazilian—private and professional.

APPENDIX.

ON THE EFFICACY OF LARGE AND FREQUENT DOSES OF QUININE IN ARRESTING THE COURSE OF CONTINUED FEVER. By ROBERT DUNDAS, M.D., Physician to the Liverpool Northern Hospital; formerly Medical Superintendent (for twenty-three years) of the British Hospital, Bahia.

(Published in the "Medical Times.")

EARLY in the present year, I delivered a series of lectures on questions connected with the Brazils, and on the diseases of persons returned to Europe after long tropical residence. In the course of these lectures, I laid down certain new doctrines on the pathology and treatment of tropical fevers, embracing likewise continued fever as it appears in this country, and which I consider to be essentially the same in its nature as the fevers of tropical countries, and curable by the same agents.

As the treatment which I recommended has been tested at the Northern Hospital of Liverpool, as well as lately at the Liverpool Fever Hospital, and elsewhere, I am anxious to submit my views more generally to the profession, in order that their truth and value may be proved on a still larger field, and under every variety of circumstance.

TYPHUS FEVER OF EUROPE.

Acting on my conviction of the essential identity of the remittent and intermittent fever of the tropics with the typhus of Europe, and being aware of the specific action of quinine in every stage of the former diseases, I have for some time resorted to its administration in the ordinary typhus of this country, *in all its stages*, and commonly with the happiest results. In typhus, as in the remittent and intermittent of hot climates, the treatment by quinine will be successful in proportion to its early administration; also, as in the tropical fever, the doses should be large—ten or twelve grains—and repeated at intervals not exceeding two hours. Three or four doses will, in most cases, be sufficient to exert the specific influence of the medicine, which is displayed by dizziness of the head and tinnitus aurium, or in the rapid subsidence of all the urgent symptoms. In the latter event, three grains of quinine should be administered three or four times a-day, and the patient supported with good beef-tea, or

other light nutriment, and wine if necessary.* Should the urgent symptoms return, the large and repeated doses of quinine must be again resorted to. Slops should be avoided, and purgatives also, unless obviously indicated; but an emetic of tartarised antimony will prove useful at the commencement, and render the system more obedient to the specific influence of the remedy. Should the urgent symptoms persist, notwithstanding the administration of six or seven doses of quinine; or should dizziness of the head and tinnitus aurium supervene, the medicine must be discontinued; and, after an interval of six or seven hours, small and repeated doses of tartarised antimony should be resorted to, until full vomiting is induced. The patient should then be allowed to rest for twenty-four hours, when the quinine should be recommenced as before. If the symptoms still resist, the remedies should be repeated in succession, as above stated, for a period of four or five days; and, unless the beneficial effects are broadly marked within that time, we can no longer reasonably hope for success from this treatment, and it may be abandoned. Still, in the great majority of cases of uncomplicated typhus, taken at the commencement, *complete and rapid success* may be calculated on; and, in all, the diseased chain of actions will almost invariably be broken—no unimportant advantage in the treatment of any malady. In the advanced periods of the disease,

* In Hospital patients wine, and often brandy, will be frequently necessary from the first.

the results will be much less certain ; but, in all stages, the large doses of quinine may be safely resorted to, and will commonly calm the patient, cool his skin, allay the headache, and reduce the frequency, and improve the character of the pulse. It must, however, be borne in mind, (as I have elsewhere pointed out,)* that any vital organ being seriously involved will prove a disturbing cause to the curative powers of the remedy, which are clearly exerted on the nervous system, through which the blood and secretions are favourably modified, and often with *marvellous rapidity*.

In the history of typhus in this country, numerous incidental notices will be found, such as the following, on the epidemic of 1819 :—" The disease has simulated the recurrent type ; the paroxysms were marked by distinct, and often violent rigors, which were succeeded by intense heat, and increased vascular activity, terminating occasionally in profuse sweating, but more commonly in a gradual subsidence of the exacerbation, without any relaxation of the surface. The paroxysms showed no obedience to periodicity ; in some cases recurring in a few hours, and, in others, only after the lapse of as many days."† Now, here we are presented, clearly and distinctly, with the history of an irregular intermittent or remittent fever ; yet, strange to say,

* In the course of the foregoing work.

† Sheppard. — *Edinburgh Medical and Surgical Journal*, vol. xv.

this idea seems never once to have crossed the mind of the observer.

As I well know, by experience, how distasteful an array of cases commonly proves, I shall only relate two or three; but these are well adapted to illustrate, beyond all rational doubt, the justness of the principles and the efficacy of the treatment which I have just inculcated. The cases are divested of all minute detail, as I am anxious to place the broad facts clearly before the profession, so that they may be easily impressed on the memory, and in order that they may be submitted to large and careful experiment. The two cases now to be related were received into the Liverpool Northern Hospital, one under myself, the other under my colleague, Dr. Scott. The notes were taken by our intelligent house-surgeon, Mr. Evans, and are given in his words:—

“*Case 1.* Cornelius Vincent, aged 26, was admitted October 2, 1850, into ward No. 12, under Dr. Dundas.

“October 3. He had been ill ten days. Present state: Severe headache; anxious countenance; slight delirium; skin hot and dry; tongue black, dry, and furred; teeth covered with sordes; thirst; urine scanty and high coloured; bowels open; pain of abdomen on pressure; pulse 100; respirations 28.

“℞ Disulphatis quinæ, gr. xxx. Divide in doses iij., quarum capiat unam secunda quaque horâ.

“October 4. *Convalescent.* The pain in the head and the delirium have ceased; the abdomen is less

tender; the heat of skin diminished; the tongue clean and moist; pulse 90; respirations 24.

“℞ Infusi quassiae, ℥iii., ter in die.

“No further treatment was resorted to, and from this date he rapidly gained strength, and was discharged well on 11th October.”

It is the ordinary rule not to admit cases of typhus into the Northern Hospital, but to send them to the Fever Hospital. The above case, however, having been reported to my colleague, Dr. Scott, he resolved to give the treatment by quinine another trial. An opportunity presented itself in a few days.

“*Case 2.* Edward Donald, aged 23, was admitted on 25th October, 1850, into ward 14, under Dr. Scott. He had been ill eight days. Present state; Great anxiety of countenance, and high delirium; dry pungent skin; tongue dry, and coated with dark fur; sordes about the lips and teeth; great thirst; urine scanty and high coloured; bowels open; abdomen tumid, and tender on pressure; pulse 108; respirations 30.

“℞ Disulphatis quinæ, gr. xxx. Divide in doses iij., quarum capiat i. secunda quaque horâ.

“October 26. *At the morning visit, he was found reading a book in bed!* All the formidable symptoms of yesterday have disappeared. No further medical treatment was resorted to in this case, and he was discharged well on the 5th November.”

The same treatment has been adopted in several

other cases of fever admitted into the Northern Hospital, and with equally good success. In one case, that of Ann Dobbin, one of the hospital nurses, the effect was highly instructive. Attacked with well-marked typhus, she was treated for three or four days on the ordinary routine system, by purgatives, salines, and diaphoretics. Under this treatment, she daily became worse; the cerebral disturbance, lumbar pains, heat of skin, and thirst, became gradually more intense, the pulse more frequent, the tongue dry and brown, and sordes began to appear about the teeth and lips. Ten grains of quinine were now given every two hours, with the result, after the sixth dose, of arresting all the unfavourable symptoms. The pulse calmed down; the tongue became moist and clean; the sordes disappeared; the heat of skin, thirst, cerebral and lumbar pains, all moderated, and she rapidly improved without any further medical treatment beyond the administration of a little wine.

The following valuable communication from Mr. Eddowes, the highly-intelligent house-surgeon of the Liverpool Fever Hospital, requires no comment:—

(Copy of Letter from Dr. Dundas to Mr. Eddowes.)

“Canning-street, Sept. 13, 1851.

“My dear Sir,—As I understand from our house-surgeon, Mr. Weaver, that you have lately tested, in the fever cases at the Fever Hospital, the plan of treatment which I have recommended in my lectures, and

carried into practice at the Northern Hospital; and as I am about to submit this important question to the consideration of my professional brethren, I would feel greatly obliged if you would favour me with the results, briefly, of your experience of this plan of treatment in the typhus fever, as it appears in your hospital. I remain, &c.,

“ ROBERT DUNDAS.

“ To W. Eddowes, Esq.,

“ House-surgeon, Fever Hospital, Liverpool.”

(*Copy of Mr. Eddowes' Reply to Dr. Dundas.*)

“ Liverpool Fever Hospital, Sept. 17, 1851.

“ Dear Sir,—I most willingly furnish you, in a brief manner, with the results of the fever cases where I have administered quinine.

“ I have used it, during seven weeks, in every case of typhus, giving five grains every three hours; and the success has been most marked.

“ The day but one after its administration generally finds the patient better; the petechiæ gradually fade, and the fever leaves its unhappy victim.

“ In diet, I give milk, arrow-root, and beef tea; also wine, if necessary.

“ The superiority of your plan of treatment consists, I believe, in the simple fact, that it either *cuts the fever short, or prevents the accession or increase of the more formidable symptoms*; while, in the ordinary treatment, (the *médecine expectante*), the physician only interferes

when death is on the point of claiming the sufferer for his own.

“The cases in which I have used the quinine have been eruptive typhus—not a single case of typhoid fever.

“The quinine frequently excites vomiting of a grass-green liquid; but I do not discontinue it on that account.

“If I have been too brief in the foregoing outline, I shall be happy to furnish any further particulars. I remain, &c.,

“W. EDDOWES.

“To Dr. Dundas.”

I have subsequently had two interviews with Mr. Eddowes at the Fever Hospital, when he kindly permitted me to verify the results of the practice by a personal examination of his patients; at the same time declaring, in emphatic terms, his sense of the importance of my system, and of its vast superiority over all others heretofore resorted to in the treatment of typhus fever.

It will be remarked, that the improvement in the patients at the Fever Hospital was generally apparent only on the third day from the commencement of the remedy; whilst, in my own patients, the improvement is generally established within the first twenty-four hours. Mr. Eddowes, however, it will be observed, did not push the remedy to its full extent. He

administered only five grains every three hours, instead of ten grains every two hours, as I have recommended ; and this, I believe, will satisfactorily account for the slight difference in the results. He at the same time informed me—and allowed me to state the fact—that, from the high price of quinine, and the large number of patients in hospital, he was induced, by motives of economy alone, to try first the effect of the smaller doses. As regards the question of economy, very properly adverted to by Mr. Eddowes as a public officer, I am satisfied that if the plan of treatment in typhus, now laid before the profession, be adopted by public institutions, its superiority to all those systems hitherto practised will be at once manifested ; and not alone in the economy to human life and suffering, but—what is scarcely less esteemed in this economical age—by the vast pecuniary gain to the public, from the rapidity and certainty with which typhus may be arrested—CURED, *malgré* Pitcairn—and the patient thus restored to his ordinary occupation, ceasing to be a burden on the funds of the community.

Yet, we are told, on high authority, that “in the continued fevers of this country, we believe it (bark) might with great safety be erased from the list of remedies altogether.”* Another author of acknowledged eminence, Dr. Pereira, in his standard work on *Materia Medica*, lays down that, in febrile conditions of

* *Edinburgh Medical and Surgical Journal*, vol. xv., p. 595.

the system, attended with a hot and dry skin, and a furred and dry tongue, tonics act as local irritants and stimulants, and add to the severity of all the morbid symptoms,"—p. 208; and he illustrates his doctrine by the action of disulphate of quina in fever! Whilst Dr. Tweedie* states,—“ We hold its exhibition (quinine) in the early stages of fever, *under any circumstances*, improper, as tending, by its *stimulant* powers, to keep up or increase the febrile action in the system; and when there is *local complication*, it is evidently so *pernicious* that scarcely any practitioner can be so ignorant of the common principles on which the treatment of fever should be conducted, as to think for one moment of its administration under such circumstances.” Again, Dr. Watson, deservedly one of the highest (as well as the most recent) authorities in medicine, adopts, in its fullest extent, the well-known maxim of Pitcairn, “ You may *guide* a fever; you cannot *cure* it.”

When these high-vouched theories are measured by the facts which I have just stated, all of which have also been observed by others, I feel convinced that, however distinguished the names which have sanctioned these doctrines, they will be found to be utterly unfounded. So fatal, indeed, are they, that, when we take into account the extent and mortality of fever in all its forms, in every clime, and in every class, I question whether their application to the actual treat-

* *Cyclopædia of Practical Medicine*, vol. ii., p. 211.

ment of human disease will not outweigh, in danger to health and life, all the advantages to be derived from the more enlightened views of modern medicine.

The cases which I have related, even if they stood alone, which they do not, would afford cogent evidence, not only in support of the treatment adopted, but of the truth of the doctrine on which such treatment is founded,—viz., the essential identity of the typhus of this country with the intermittent and remittent of the tropics, modified by climate and numerous other influences. In fact, I believe that the history of fever must, and will ere long, be rewritten.

The power of quinine in controlling the remittent and intermittent fever, (and the yellow fever also, as stated by Dr. Blair), is now a well-established and important fact; although I totally dissent from the received doctrine of its specific action on the *several specific poisons* which originate these fevers. Moreover, I have demonstrated, that, in sufficient doses, it displays an equal power in subduing the supposed animal poison of the typhus fever.

The action of quinine is clearly not that of a tonic, in the ordinary sense of the word; its action is obviously on the nervous power, whose functions it favourably and rapidly modifies, when depressed or exhausted by any of the numerous moral or physical agencies which act in causing fever, and thus it restores to the organic nervous system its normal influence over the animal fluids and vital phenomena. Hence the curative

powers of quinine in fever, and in many other apparently dissimilar maladies.

Liverpool, Tuesday, 30th Sept.

On my return from London last night, I found a letter from Dr. Goolden, the talented physician of St. Thomas's Hospital, indicating the highly favourable results in his experience of the curative powers of quinine, as laid down above. He has kindly promised me a detail of facts, and I greatly regret that the time agreed on for forwarding this communication will not allow me at present to avail myself of them. In my work now passing through the press, I have entered more fully on the subject of fever, and I gladly seize this early opportunity of expressing my obligations to Dr. Leslie, now of Rio, and for many years my house-surgeon at the Bahia Hospital, for numerous valuable suggestions on the present and other medical questions.

Since the publication of the above, my attention has been directed to a valuable report on the fevers of New Orleans, by Dr. E. D. Fenner,* who points out the efficacy of large doses of quinine in *cutting short* the yellow fever. Dr. F. observes,—“The sedative powers of large doses of quinine, given during the early exacerbation of our summer and autumnal fevers—remittent, bilious, and yellow—have been proclaimed by some of

* *Edinburgh Medical and Surgical Journal* for October, 1851.

the physicians of this city, by the army surgeons, &c. We perceive that these views are gradually extending to the north but it will require some time to prepare the minds of our northern brethren for such a revolution in therapeutics, as they must effect when established. Quinine is given in five or ten-grain doses by a number of physicians in this city ; this is a great improvement, but they will go farther after a while. We saw twenty grains given at the Charity Hospital in the early stages of yellow fever." The *London Journal of Medicine* observes on the foregoing,—“The experience of Dr. Fenner certainly gives additional strength to the important practical doctrine which Dr. Dundas has brought before the profession.”—P. 1142, No. xxxvi.

ON THE ARREST OF TYPHUS FEVER BY CINCHONIZM.

By ROBERT DUNDAS, M.D., Physician to the Northern Hospital, Liverpool ; formerly Surgeon to H.M. 60th Regiment ; and late Medical Superintendent of the British Hospital, Bahia.

(Published in the “*London Journal of Medicine.*”)

IN the *Medical Times*, I recently endeavoured to impress my professional brethren with the evidence in favour of the efficacy of large and repeated doses of quinine, in arresting the course of typhus fever. My observations have called forth several valuable com-

munications from different correspondents ; to many of whom I have not the honour of being personally known. Some have fully admitted the soundness and the importance of the principles which I have laid down ; others demand more extended statistical proof ; and a third class state, that the quinine treatment had, in their experience, failed, in several instances, to arrest the disease. Such are the chief points in the correspondence with which I have been favoured.

Instead of replying to each inquirer individually, I am anxious to be allowed to make the present explanatory communication to the *London Journal of Medicine*, as an answer to all.

I have reason to think that my observations on the specific powers of quinine in typhus fever have been to some extent misapprehended. I do not believe, nor have I ever asserted, that large and repeated doses of quinine will *always* cure or arrest typhus fever. I have stated the contrary. And I now ask—Will quinine always arrest ague ? will calomel always salivate ? or will opium always induce sleep ? Assuredly they will not. Yet notwithstanding exceptional cases, who will question the general—not universal—specific action of these agents ? If we take, as an illustration of the failure of quinine in typhus, some unhappy patient from the noisome and unhealthy courts or cellars of a large city—his constitution broken down by intemperance, by “poverty, sorrow, and dirt”—what remedy, or what treatment, can snatch, with certainty,

that wretched being from the grave? What I say, and what experience will I believe confirm, is, not that quinine will certainly cure such cases as the above:—no, nor those in whom, from a fatal trifling with worse than useless remedies, the vital fluids have already become irrecoverably vitiated, or some vital organ already irrecoverably damaged;—but I do distinctly state, that quinine will, *generally*, cure every case of typhus fever curable by medicine, including a large number of those who would sink under any other form of treatment. I say that quinine, administered as I have directed, will, in the vast majority of cases, stay or avert the symptoms that threaten life,—will, in the forcible words of Mr. Eddowes, as proved at the Liverpool Fever Hospital, “*either cut the fever short, or prevent the accession or increase of the more formidable symptoms.*” And such also are the results in the hands of Dr. Goolden, the experienced physician of St. Thomas’s Hospital; of the practice at the Liverpool Northern Hospital; and in the private practice of myself and others.

The dogma so tersely laid down by Pitcairn, and adopted, probably, more from its antithesis than its truth, that “*you may guide a fever—you cannot cure it,*” was obviously based on the hypothesis, that fever originates in a specific poison, and must consequently run a specific course. What wonder, then, that the efforts founded on such premises should have so satisfactorily issued in proving the *incurability* of the disease? The prophecy ensured its own fulfilment!

Strange, too, that this generally received maxim, so far from being true, ought in fact to be precisely reversed; namely, that you “may *cure*, but cannot *guide* a fever.” No human agency can guide a fever: some of the symptoms may indeed be moderated, and death from certain complications may be averted; but the course of the disease will ever be fraught with danger, particularly if it be long continued, or if any system or any vital organ be predisposed, accidentally or hereditarily, to morbid action. Herein lies the chief danger in fever.

The methods of treatment—if, indeed, some of them deserve this name—have been legion, and of every degree of activity. Irish patients, in their cabins, have been abandoned to cold water: distinguished physicians have, under other circumstances, deliberately adopted and practised the expectant method; while the more heroic and imposing measures of bleeding, brandy, or mercury, have been pursued in clinical hospitals, or in the mansions of the rich. Typhus patients have recovered under each of these modes; but this fact only points out more strikingly the tenacity of life under the most disadvantageous circumstances.

As regards the demand for more extended evidence, and more numerous cases, I must at once confess, that, having observed how all new theories and modes of practice—from Hippocrates, the father of rational medicine, down to the visionary Hahnemann—have been ushered into life by a vast parade of successful

cases, I have long regarded this kind of evidence with suspicion, and have avoided it on the present occasion, under the impression that many others might feel as I do. Moreover, when, as in the present instance, any principle admits of ready and conclusive proof, the cumbrous accumulation of cases is a downright imposition on the time and patience of the profession,—supposing, which I much doubt, that they would be read. Is your principle sound? *one* apposite case affords the illustration. Is it false? *a thousand* will not establish its truth. Let it be tested. Perhaps I may be allowed, in imitation of a celebrated northern surgeon, to introduce one other, though somewhat an unusual medical witness. An hospital nurse, E. McCartney, had been so employed for thirty years, and to her was entrusted the administration of the quinine to the patients in the Fever Hospital. Whilst I was discussing in the ward, with Dr. Gee and Mr. Eddowes, the relative results of the treatment, this woman voluntarily observed to us,—“ I don’t know, but the patients that take the powders (the quinine) get well the fastest. I never saw the patients get well so fast before.” So much for a “ nurse’s tale.”

Influenced by the considerations above stated, I have introduced only one case on my own authority; and in that the notes were taken, and the treatment carried out, not by myself, but by Mr. C. J. Evans, then house-surgeon to the hospital, and now apothecary to the Liverpool Infirmary—a gentleman whose com-

petency few will question. I may be allowed to add, that all the witnesses whom I am about to cite were equally impartial, and thoroughly competent observers.

I shall now submit the valuable evidence of Dr. Goolden, who has also touched on some practical points, to which I shall afterwards briefly advert. In answer to my letter, requesting him to communicate to me the result of his experience of the quinine treatment in typhus fever, he replies:—

“I have only to state, that it is so satisfactory, that I should not feel myself justified in treating any serious case of typhus without it. Of eight cases admitted under my care in St. Thomas’s in one week—five adults and three children—all with the characteristics of typhus gravior, with one exception, the effect was well marked. Each dose of quinine produced a sensible effect; ten grains in solution, every two hours, to an adult, produced some giddiness and deafness in about thirty-six hours, when it was discontinued; and it was only necessary to give a few doses of nitre and a slight aperient, and nothing remained of the fever but slight debility—debility varied according to the previous duration of the disease. In one case in the hospital, there was so much headache and excitement after each dose, that after the third time it was discontinued, but there was no permanent ill effect; and I am satisfied the course is quite safe to adopt.

“I have met with several opportunities of trying it in private practice.

“ In one case, a young gentleman had been travelling with his tutor in Germany, and, when at Giessen, was observed to be out of health; he had loss of appetite, shivering, debility, thirst, and feverish nights. Still, he was able to make the journey home. When at home, he was attacked with severe typhoid symptoms, and I visited him with his medical attendant. I found him with *a black dry tongue, hot dry skin, petechiæ, pulse 140 and full, delirium, deafness, and diarrhœa*. He had been in this state several days, when I suggested the large doses of quinine. After an emetic, he took eight grains every two hours. I saw him after the *fourth dose*, and found him *sensible, but rather deaf, the skin bathed in perspiration, pulse sunk down to 80, and the diarrhœa checked*. The medicine was continued during the night, and discontinued the next morning, as the surgeon in attendance had had no experience in the quinine treatment, and did not like to carry it on upon his own responsibility. I saw him two days afterwards, and found that he had had no sleep, and the diarrhœa was returning. He took some opium, and repeated the quinine in smaller doses. After a good sleep he awoke much better, and was apparently going on well, when after some days I was sent for, in consequence of a congested state of the left lung, and slight cough. The apex of that lung was quite dull on percussion; no respiratory movement was observed on that side, and bronchophony was distinct, and much large moist crepitation, which made me fear the result,

as his mother was the only one of a large family who had not died of consumption. A blister was applied under the clavicle, and he was ordered port wine and nutritious food ; and I am happy to say that he is now quite recovered, and the lung perfectly sound. I do not attribute the congested lung to the quinine, but to the fever; but I should be wrong to omit it in making a report.

“ I requested the opinion of Mr. Hine, who has the care of the servants of the Great Western Railway at Swindon, including 2,000 families of artizans, &c., among whom typhus is prevalent,—perhaps from want of drainage in the new town,—and his report to me was, that for some time the result was most satisfactory; but latterly he had found that the head symptoms had prevented his using it. He thought that there had been some change in the type of the fever.

“ The only drawback that I see to the use of the large doses of quinine, is the necessity for frequently visiting the patients, say several times a-day, which is almost impossible in country practice, and very difficult in town when one is much occupied; and patients may consider such frequent visits to arise from the *nimia diligentia medici*, when the danger is over, and perhaps hardly apparent.”

The above statement needs little comment. Of the eight hospital cases of typhus gravior, the treatment was conclusively satisfactory in seven; the exceptional case was also valuable, as proving the perfect safety

of the remedy in those individuals where some peculiar condition or idiosyncrasy interferes with its curative power. In one case, Dr. Goolden pushed the remedy, not only with perfect safety, but with entire success, beyond what I have ever done, or found necessary; and this fact is very important.

The case of the private patient is highly instructive, as proving, beyond all rational doubt, the specific power of quinine in arresting, *within the short period of eight hours*, the most formidable and alarming symptoms incident to typhus fever: and it is especially important in another sense,—had the fever been prolonged, considering the patient's constitutional tendency, fatal disease of the lungs was inevitable. No other form of treatment, I firmly believe, could have saved this youth's life.

Though deficient in the necessary details, the report of Mr. Hine will fix attention; and I trust that this gentleman, as well as others, who enjoy such extensive and favourable opportunities for observation, will favour the profession with the results of their future experience. I must not pass over the "*only drawback*" urged by Dr. Goolden against the quinine treatment of typhus—and its practical difficulty is indisputable—namely, the necessity for frequently visiting the patient. To ensure success, he should, undoubtedly, be frequently seen, whilst the large doses are being administered; and this, as Dr. Goolden observes, is almost impossible in country practice, and very inconvenient to the well-employed

town physician. This difficulty, however, affects in no degree the intrinsic value of the quinine system, or the rationality and truth of the theory on which it is based. At the same time, it must be admitted, that the efficacy of this method of treatment cannot be fairly and fully tried, unless the practitioner, or some competent substitute, exercise a frequent, even though an inconvenient, supervision.

Neither in my own experience, nor in that of others, so far as I am aware, have the large doses of quinine caused congestion of any important organ. On the contrary, by cutting short, or moderating the febrile excitement, they prevent all such congestions, and in this consists the great value of the treatment; seeing that the vast majority of fever cases are carried off by these local affections. In the event of any important organ being involved, I have, with great advantage, resorted to extensive dry cupping, either alone, or followed by a blister, or a large mustard poultice, frequently repeated.

When the first large doses of quinine have failed to produce their usual curative effect, and the practitioner is obliged to discontinue them, an emetic ought to be administered, as formerly pointed out; and if, after this, the febrile excitement still continue, a full dose of the liquor opii sedativus, with a few drops of nitric acid, will frequently afford the most signal relief, and enable the patient to resume the quinine with every prospect of success.

I would here observe, that the large doses of quinine are not only safe, but advantageous in every stage, and in every form of typhus fever, and that the action assigned to it by Dr. Pereira and others is altogether erroneous. The presence of intense headache, quick and strong pulse, dry and burning skin, dry, chapped, and black tongue, intense thirst, hurried respiration, abdominal tenderness, and diarrhœa, do not contraindicate its use. On the contrary, under large and repeated doses, the headache will subside, the pulse calm down, the breathing become less frequent, perspiration will return, the tongue will become moist, and the diarrhœa will be checked.*

After the first decided impression has been made on the disease by the quinine, it is invariably necessary to support the patient's strength by good beef-tea, and a moderate allowance of wine. Purgatives, without some decided necessity, should be avoided. When the head continues much involved, a strong capsicum enema—a drachm of the *powder* to ten ounces of water—will often afford relief. The minor adjuvants in fever may also occasionally be resorted to with comfort and advantage.

In these observations I have confined myself to the exposition of general principles, and altogether avoided cases, as I entirely concur with that eminent physician,

* For obvious reasons, the power of quinine in arresting typhus fever will be more strikingly displayed in private patients, than in the general run of hospital patients.

Dr. William Stokes, whose *Lectures on Fever* are unexcelled in the English or any other language, "that you might as well expect to find two human beings exactly alike, as to find two cases of fever perfectly similar:"—the varieties are infinite.

I may here state, that I have repeatedly witnessed in Brazil, seasons of ague (for the disease prevails in seasons), when quinine, though always curative, appeared less efficient in controlling the disease than at others; and similar modifications will doubtless occasionally occur in the fever of this country. Modifications in the quinine treatment, and probably very important ones, will also, I am satisfied, be introduced through the more extended experience of the profession, though I believe the *principle* will be only confirmed by being tried and tested by different observers.

I shall now briefly repeat the principles I have here, and elsewhere, laid down. The evidence on which I adopted them is embodied in my *Sketches of Brazil*, now in the press, and which in a short time will be laid before the profession.

1. Ague and remittent fever do not originate in malaria or marsh miasm. The doctrine of a special marsh poison I hope to show to be altogether unfounded.
2. Intermittent, remittent, and continued fever, are mere varieties of the same disease. The intermittent constantly merges into the remittent or continued type; and continued fever assumes

still more frequently (in Brazil) the intermittent form; and all are curable by the same agents. By the same agents we can arrest them all. Could this be done if they originated in different *specific* poisons? or can we ever arrest, by any power, the course of a truly specific disease, as small-pox, &c.?

3. The notion of typhus fever being unknown in the tropics, is altogether ill-founded. The intermittent, remittent, and continued fevers of tropical climates often run into genuine typhus. In Brazil, when the disease takes this course, it is popularly termed "maligna," or "malina," and, in some seasons, is very frequent and very fatal.
4. Bark, duly administered, will generally arrest the intermittent and remittent fever; and typhus fever being essentially the same disease, bark ought to, and will, generally arrest it.
5. Ague will occasionally resist, for many days, the most judicious application of quinine—and finally yield: the same remark applies to typhus.
6. Quinine is more certain in its results in proportion to its early administration; but it is less to be depended on with the aged.
7. The administration of large doses of quinine in typhus, when not curative, is *never* followed by the slightest ill effects.
8. As typhus is, commonly, the more severe form of fever, and the subjects of its attack generally

less favourably disposed, so we shall find considerable discrepancy in the several results.

9. Typhus will occasionally resist quinine, and yield to other remedies, and the same holds good with ague; yet who ever associates the latter disease with any other remedy than quinine? And I am firmly of opinion, that the time approaches when the treatment of typhus fever, after ages of vacillation, will be established on the same sure and satisfactory basis as that on which the treatment of ague now rests.

Mr. Eddowes continues to adopt the quinine treatment *universally* with the patients in his section of the Fever Hospital; and Dr. Gee, in his wards of the same establishment, is now giving it a trial. From two such able observers, in such an extensive field, we cannot fail to obtain valuable and correct information.

Since the above was written, my attention has been called to an interesting report, in the last number of the *Medical Gazette*, by Dr. Humble, on fever, as it appeared during the last year, in the Newcastle Fever Hospital. The symptoms, indeed, are pretty much those which commonly characterise the fevers of this country; and I notice the report, chiefly, from the "relapsing" type assigned to the disease.*

After alluding to the different periods at which the

* Upon Relapsing Fever, so well described, in 1843, by Dr. Cormack, and more recently by Dr. Fenner and others, I have made some remarks in my forthcoming volume.

"relapse" occurred, Dr. H. goes on to state: "In general, this came on after convalescence had commenced, and it did so in spite of any precaution which could be devised against it. . . . A boy became convalescent, and was ordered to keep his bed until the usual period of relapse had passed over. This was accomplished with considerable difficulty, *as he felt himself perfectly well*; but on the evening of the fifteenth day, the relapse, or, as it might be called, *the second attack*, of fever came on." Alluding to the class of patients, Dr. H. states: "A large importation of Irish were huddled together in great numbers, in a few lodging-houses, situated in the lower parts of the town. . . . In one family, fever seemed to have been occasioned, or at any rate aggravated, by the want of the common necessities of life . . . two of the children presented all the symptoms described as belonging to the famine fever of Ireland. Four cases, after presenting the usual symptoms of continued fever for several days, took on the character of *regular ague*; in one after twelve days, where the type was tertian; and in three, after ten days, in which it was quotidian," precisely as continued fevers often terminate in Brazil and other southern countries.

What, I ask, would large doses of quinine have done in these Newcastle fevers? But the word "relapse" unhappily presents itself, and all reasoning ceases. Speaking of fever, generally, that most accurate of observers, Dr. Stokes, writes: "We seldom meet with

a case of typhus without morning remissions; and in some cases the symptoms are aggravated on alternate days, so as to bear some resemblance to double tertian." I have already stated what large doses of quinine would effect in *these cases*.

I now subjoin a letter from Mr. A. B. Steele, late surgeon to the Liverpool Fever Hospital, and medical superintendent of Irish Quarantine and Fever Ships in the Mersey. I had learned, at the hospital, that Mr. Steele had tested the quinine treatment in typhus, and was anxious to obtain the opinion of an authority so competent to decide on its merits. This opinion was kindly and promptly given as follows:—

“49 Russell-street, 24th Oct., 1851.

“Dear Sir,—At your request, I have much pleasure in furnishing the following brief notes of my experience in the treatment of continued fever with large doses of quinine, as recommended by you, and of which I was only informed a few days ago.

“The first case was an Irishman, named Lawrence Connor, aged 40, living in 4 Court, Grosvenor-street, one of the very worst streets in Liverpool, and scarcely ever free from fever. I visited him here on Saturday the 18th instant, and found him labouring under the usual symptoms of the low type of continued fever; had been ill seven days; was lying on straw in a corner of the floor, destitute of all comforts, or even necessities.

“I gave an order for his removal to the Fever Hos-

pital, but he did not go. On Monday the 20th, I was again called to him. He was now so much prostrated, that I should have considered it hazardous to remove him. There was great nervous and muscular debility; skin hot and dry; suffusion of conjunctivæ; pulse small and frequent; tongue protruded with difficulty, very dry and brown; delirious, especially at night; can scarcely answer questions. I ordered one drachm of disulphate of quinine in six powders, one to be taken every two hours.

“On the 21st, I found the patient considerably better. The skin was moist; pulse softer, fuller, and less frequent; tongue readily protruded, moist, and whitish; the brown fur had quite disappeared; delirium stated to be quite removed; expresses himself better. The quinine in ten-grain doses was ordered to be continued, and wine and beef-tea to be given. These directions were not properly carried out during the following days, and a partial relapse has been the result; the tongue becoming rather dry and brown; the prostration returning; but still the patient is now (25th) in a far more favourable condition than he was anterior to the administration of the quinine.

“The second case, a woman named Galagher, 14 Collingwood-street, had been labouring under fever for several days. On the 20th, I found her in a state of great prostration, with well marked symptoms of low fever, with, I believe, pleuritic complication. The condition of the patient did not admit of auscultation, or a

minute examination. General symptoms very similar to the first case. I ordered quinine in ten-grain doses, and a blister to the chest. The next day she expressed herself much better; the symptoms had evidently given way. The quinine was continued, and she is now in a fair way of recovery.

“ I have tried the plan in a third case of low continued fever, with dry brown tongue, &c.; but unfortunately, from the first, the medicine was not given regularly, or in the quantities ordered; still, what would be considered a large quantity of quinine was taken, and with a decidedly beneficial effect, although not to the same extent as in the other two cases.

“ In the first case, the decided effect on the objective symptoms of the disease, *in twenty-four hours*, was so striking as at once to convince me of the value and importance of the remedy; and this improved condition was produced without the collateral advantages of *ventilation, cleanliness, nursing, nutritious diet, or stimulants*,—a fact which greatly enhances the value of this mode of treatment, in the hands of those who, unhappily, have to contend with the disease under the most unfavourable circumstances.

“ I have witnessed the results of various methods of treating fever, in several hundreds of cases, during the epidemic of 1847, in our Fever Hospital, and on board the Fever Ships in the Mersey, and subsequently in the town; but I have never found any remedy, or remedies, which appeared to me to cut short the

disease, or modify the symptoms, in the same decided manner in which the quinine has done, when fairly tried.

“ I hope shortly to be able to give you reports of more cases, as I shall continue to adopt your system, the importance of which cannot, I think, be overrated.

“ I remain, &c.,

“ A. B. STEELE,

“ Late Surgeon to the Fever Hospital, Medical Superintendent of the Irish Quarantine and Fever Ships in the Mersey.”

“ P.S.—I forgot to state, that Lawrence Connor had, by mistake, fifteen grains instead of ten grains of quinine for the first four doses.”

I have just received the following communication from my accomplished friend, Mr. Eddowes,* which, with his valuable cases, prevents the necessity of my advertising (as was otherwise my intention) to the very important question of *typhus with complications*. By the kindness of Dr. Gee and Mr. Eddowes, I observed these cases whilst under treatment, and to both gentlemen I am, for this advantage, very deeply indebted.

“ Liverpool Fever Hospital, Nov. 8, 1851.

“ Dear Sir,—In reply to your note of yesterday, I beg to state that my opinion of the quinine treatment is unchanged.

“ In one important practical point, I entirely differ from Dr. Goolden and yourself. I do not consider

* The zeal, intelligence, and eminently practical mind of Mr. Eddowes mark him out as one of those men who are destined to advance the profession.

that the quinine treatment requires, for its successful employment, any special supervision. For all practical purposes, *one* visit daily to the patient has been found sufficient in the hospital, where the ten-grain doses have been given every two hours, for many days, without any inconvenience.

"I quite agree with Dr. Goolden (judging from the cases under Dr. Gee, and in my own charge), that the treatment appears to be altogether free from danger.

"The quinine has been used here in fevers complicated with chest affections, &c. I enclose for your satisfaction the notes of two cases, (*ex multis aliis*), and should you desire any more, you can have them.

"I remain, &c.

"W. EDDOWES.

"To Dr. Dundas."

"CASE I. TYPHUS IN AN EPILEPTIC—ERYSIPELAS SUPERVENING—CONVALESCENCE ON THE FIFTH DAY OF TREATMENT. Richard Lewis, aged 30, a painter, has had colic and wrist-drop; has had epilepsy for seven years, having two fits a-month. The epilepsy occurred a month after the attack of colic. His general health is good.

"*Present Attack.* Has been ill more or less for three weeks, but confined to bed for five days only: he had been under treatment, and had got worse.

"*October 18. Present State.* He complains of deafness and frontal cephalalgia; is propped up in bed, which, he says, eases the great headache. He passes

restless nights; the countenance is flushed; the respiration hurried, 32; the pulse 106, jerking and weak; skin hot and dry; tongue dry, and brown in the centre, moist at the edges: there is tenderness upon firm pressure over the hepatic region. He was directed to have ten grains of the disulphate of quinine every two hours; four ounces of brandy daily; milk diet, beef-tea, and arrowroot.

“ *October 19.* He sleeps better; the head is easier than yesterday; is not propped up as before; the breathing is easier; the tongue as before; a dusky erysipelatous flush is appearing on the cheeks and forehead. He was ordered to continue the remedies, and apply flour to the erysipelas.

“ *October 20.* The headache is less, but he spent the night restlessly; the face is swollen; the breathing natural; pulse 96; tongue as before. He says that he ‘feels quite well, except the soreness of the head and face.’ To continue the quinine, &c.

“ *October 21.* The deafness is nearly gone; the tongue moist; he was restless at night; headache quite gone; pulse 80, natural; skin cool, covered with a perspiration; the erysipelas is better. The medicines were continued.

“ *October 23.* He was convalescent; and was directed to take ten grains of quinine three times a-day.

“ **CASE II. PETECHIAL TYPHUS—PLEURITIS AND BRONCHITIS—CONVALESCENCE ON THE FIFTH DAY OF**

TREATMENT. *October 23.* Mary Malony, aged 15, of good general health, has been ill five days; the skin is hot and dry, with petechiæ; she has great thirst; restless nights; slight headache; tongue coated with a white fur; loss of appetite. The pulse is 100, natural. \mathcal{R} Ipecac. gr. xv., antimon. potass. tart. gr. i., statim sumendus. Postea sumat quinae disulph. gr. v., secundis horis. To have milk diet, arrow-root, and beef-tea.

“*October 14.* She is breathing quickly; the pulse is 120, jerking and weak; the tongue white at the base; she has pain in the left inferior mammary region. In front, the chest is clear on percussion; there is sibilus on the right side, also on the left, with occasional cooing rhonchi. To the left of the cardiac region is a dry friction sound, loudest during expiration. Behind, the left base is resonant, but less so than the right; the respiratory murmur is faint throughout. The friction sound is audible from the supra-spinous fossa to the base; mucous rhonchi are audible over the whole of the right side. To continue the quina. \mathcal{R} Ung. hydrargyri fort. \mathfrak{z} ij., pulv. camphoræ \mathfrak{z} ss., p. opii. \mathfrak{z} j. M. Sæpe lateri sinistro infricandum.*

“*October 15.* The pain in the side is easier; the respiration slower; pulse 118; skin hot; friction sound behind as before; no increase of dulness; friction sound in front moister. Continue the quina.

* From a formula recommended by Dr. Blakiston.

"*October 16.* The breathing is easier; the pulse softer and more natural; the countenance improving.

"*October 17.* Pulse 112, soft and natural; the skin moist; no cough; no headache; breathing natural; petechiæ fading; she takes food, and says her tongue is sore. The quinine was continued.

"*October 18.* She is convalescent; the tongue is clear; the countenance natural; pulse 84. She says she feels well. The friction sound is still audible. She had no relapse.

"There were two other patients in the same ward with the same complication: the above treatment was adopted, and the recovery was as speedy.

"Perhaps it may not be uninteresting to you to know, that there have been two cases of typhus occurring in pregnant women. The quinine was administered—in the one case five, and the other ten grains—every two hours. They both recovered without any ill effects."

I have lately conversed with several able men of great experience—Dr. Ewing Whittle of the South Dispensary, amongst others—who have observed fever on a large scale, and who are of opinion that no reliance can be placed on the "eruptions," to which many distinguished authors attach so much value, as diagnostic of the different fevers. They consider the character of the eruption as dependent on epidemic constitutions, idiosyncrasy, and atmospheric and other influences. All are aware of the numerous "rashes"

which supervene on derangement of the digestive functions, from the use of certain medicines, articles of diet, &c., in different constitutions, and at different seasons.

As regards the rosy lenticular rash, deemed peculiar to typhoid fever, all tropical practitioners must repeatedly have observed these spots in protracted cases of dysentery; and I have myself witnessed all the eruptions described by authors as *pathognomonic* of the several fevers, displayed, in the same patient, at one period or other of his disease. There is now a patient in the Liverpool Fever Hospital, A. B., who presents an abundant "*mulberry rash*," which quite disappears on pressure.*

There is also at the present moment, another patient in the Hospital (James Moore), in whom we have conjoined the mulberry rash, the rosy lenticular rash, and "true erysipelas"—the latter classed by Dr. Watson, one of the latest and best authorities, with the contagious exanthemata—as "a specific disease, running a definite course, and attended with an eruption."

Now, in this instance we have, according to authority, three distinct morbid poisons—the typhus, the typhoid, and the erysipelatous—contending for mastery in the same unhappy individual, and all running their regular course, unchecked and unmodified in the slightest

* This patient died, the rash continuing effaceable.

degree. Does the history of other morbid poisons present us with anything analogous?

On the authority, indeed, of Carmichael, a "plurality of venereal poisons" was at one time pretty generally admitted by the profession; but was finally exploded by M. Ricord of Paris, who, on one occasion, exhibited, with a smile, to Mr. Carmichael himself, his *four* distinctive eruptions classically designed on one and the same patient.

This *argumentum ad hominem* did not, I believe, prove altogether conclusive to Mr. C., but perfectly so to every one else; and the "plurality of venereal poisons" soon disappeared.

All must admit that our lot is cast in revolutionary times. I, however, as a loyal citizen of the republic of medicine, have now discharged my duty in handing over "the quinine system," and the principles on which it is based, for trial before the "legal and constituted authorities."

Like other arch-revolutionists, the present doctrine is earnest in its *promises* to "benefit the public:" I have not, however, allowed its justification to rest solely on my own testimony to character—naturally open to challenge—but have adduced other and unimpeachable evidence, and I now await with confidence the verdict; for although the profession be a republic, and its decrees too often tinged by human infirmity, I firmly believe that its final judgments are never wanting in calmness, and justice, and truth.

An incidental interest attaches to the doctrine now advocated, namely, that it will afford an opportunity for testing the value of the infinitesimal doses of homœopathy with doses even larger than those commonly employed by regular physicians. Let an adequate number of fever cases be selected; place them side by side in the same room; let six be treated on my plan; six infinitesimally; let competent individuals (not including myself) be appointed on either side to take the notes, day by day, administer the remedies, and report the results; and on these results I am willing to stake, absolutely, my own professional reputation, and the reputation of legitimate medicine, so far as that can be staked by such a humble individual as myself. I entertain no doubt that the authorities of the Fever Hospital, or of the Infirmary, the Northern or the Southern Hospitals, or the dispensaries, will readily afford the means of testing by direct comparison, that which I believe, with the rest of the profession, to be a dangerous delusion, but which has obtained sufficient extension to render its refutation an object of public interest. To this "*experimentum crucis*" no honest homœopath can object.

Canning-street, Liverpool, November, 1851.

The following communications, recently received, will be read with interest, as emanating from gentlemen of great professional experience, and all of them enjoy-

ing extensive and especial fields of observation. I may also state that cinchonism, after careful and extensive trial, is now the established treatment in all forms, and in all complications of typhus fever, at the Liverpool Fever Hospital:—

Letter from Dr. Gee, Physician to the Liverpool Fever Hospital.

“ Liverpool Fever Hospital,

“ December 27th, 1851.

“ My dear Sir,—In reply to your inquiry respecting my opinion of the cinchonizing treatment of fever, I beg to state briefly, that, in the first place, I have uniformly found it to be a safe method, even where there are severe complications, and that no unfavourable symptoms have resulted from its use. I may go farther, and speak *positively* in favour of quinine as exerting a decided and beneficial influence on fever—this opinion has been based upon daily observations of the effects produced by the remedy in a great number of cases.

“ I can thus conscientiously and unreservedly speak most favourably of this new plan of treatment introduced by yourself to my notice. Mr. Eddowes and myself have recorded the results of our cases, and at some future time may, if spared, be tempted to lay them before the profession.—I am, dear Sir, your's very truly,

“ ROBERT GEE.

“ To Dr. Dundas.”

*Letter from Dr. J. F. Stevenson, Physician to the
Birkenhead Fever Hospital.*

“ Hamilton Square, Birkenhead,

“ January 4th, 1852.

“ Dear Sir,—In answer to your note requesting my opinion on the cinchonizing treatment of typhus fever, which you have lately introduced to the notice of the profession, the brief statement of a few cases will probably afford the most satisfactory reply.

“ The first two cases in which I tested the treatment were hospital patients, and under almost precisely similar circumstances. Eruptive typhus, and far advanced; face pale and sunken, but occasionally flushed; speech incoherent; hearing dull; delirium, with convulsive startings; great accumulation of black sordes on the lips, teeth, and gums; tongue dry and brown; pulse small and very rapid; urine and fæces passing involuntarily; one of the patients had hæmorrhage from the bowels, with great abdominal tenderness. *Prognosis.* Both patients will sink.

“ I ordered 12 grains of quinine, with 6 m. tincture opii, in half a glass of wine every three hours, and watched the effect with the greatest care. The treatment was continued for sixty hours, when I reduced the dose to three grains every four hours.

“ On the second day *all* the symptoms in both patients were improved. On the third day the eruption had disappeared, and at the close of the fourth day, I considered medical treatment no longer necessary,

the patients only requiring support. There was no relapse.

“ The two next cases presented no eruption, but had all the usual symptoms of low continued fever; skin dry; tongue brown and parched; pulse frequent; slight delirium towards evening, &c. These I treated with ten-grain doses of quinine every four hours, without stimulants.

“ On the second day every symptom showed improvement; the tongue moist and less brown, the skin softer and pulse less frequent, &c. On the evening of this day, reduced the dose to four grains, from a disagreeable sensation complained of in the head. On the third day this sensation had subsided, and the patients rapidly convalesced, evidently much more rapidly than they would have done under the ordinary routine treatment.

“ From my experience, the cinchonizing system, in continued fever, undoubtedly shortens the period of the disease; and if this prove to be the fact in other hands, there can be little question that it must become general, and that the ordinary doctrine of allowing a fever to run its course must, ere long, be exploded. I remain, Sir,

“ J. F. STEVENSON.

“ To Dr. Dundas, Liverpool.”

Copy of Letter from Smith Glazebrook, Esq., to Dr. Dundas.

“ West Derby, December 28th, 1851.

“ My dear Sir,—In reply to your note requesting to

be informed of the results of my experience of the cinchonizing system in typhus fever, I beg to inform you, that on reading your publications I resolved to give your treatment a fair trial, though I must confess that I commenced it in hesitation and fear; you will, however, be glad to hear that its success has far exceeded all that I could have possibly anticipated.

“ I first tested it in the following case—one of extreme danger, and in which I had trusted to chloric ether, carbon. ammoniæ, and liq. opii sedativus.

“ Mr. W——, aged 36, typhus, complicated with delirium tremens, fourteen days in bed, under treatment since 9th October.

“ *October 15.* Deaf; stupid when spoken to, and answers only yes and no; low delirium; extreme nervous agitation; sunk in bed; singing in the ears, and headache; skin harsh and dry; bowels confined; urine scanty; pulse 130, small, thready, and feeble; tongue dry, brown, cracked, and protruded with difficulty; lips quivered; rapid breathing. He was getting worse from day to day, and I, trusting to chloric ether, opium, and carbonate of ammonia, was waiting for a crisis. Such was the case when I changed my treatment to the following:—℞ sulph. quininæ grs. xxx. divide in doses iij, quarum capiat unam secunda quaque hora.

“ *October 16.* Was surprised at the change visible in the whole man. My anxiety ceased, and I felt a confidence in the treatment such as I had never before felt in any medicine. The tongue, the pulse, the skin,

the nervous agitation, and the intelligence, had all improved.

“ *October 17.* Reprtr. sulph. quininæ ut hori. Had slept; skin *bathed in perspiration*; tongue *moist and white*; pulse under 100, breathing natural; still headache and singing in the ears, but less severe; the tongue, lips, and hands had nearly ceased to tremble.

“ *Four o'clock p.m.* Still *bathed in perspiration*; continues to improve; continue the quinine.

“ *Ten o'clock p.m.* Singing in the ears, and headache increased; in all other respects continues better. Intr. quininæ. beef-tea, &c.

“ *October 18.* Convalescent; had a good night; urine abundant; bowels open; appearance natural. Noticing that he was dressed and shaved, I asked, ‘Who shaved you?’ To my surprise he replied, ‘I did it myself; no person would come near me, for fear of the fever.’

“ From this date my patient improved rapidly; in a few days left his bedroom, and went about and committed great excesses in diet: relapsed, and was nearly as bad as when I first visited him. I prescribed 15 grains of quinine daily, to which the symptoms yielded; though I now believe, that had I given the larger doses he would have more rapidly improved. No other treatment than cinchonism would, I am satisfied, have saved the patient's life.

“ I have tried the larger doses in numerous other cases with equally good success. One family, mother

and two daughters, attacked with fever. I shall only notice one, the other two being nearly similar. Miss ——, aged 34, ten days in bed, uncomplicated typhus.

“November 20. Skin hot and dry; pulse 120; tongue dry, brown and cracked, and protruded with great difficulty; no sleep; no food for ten days. On the third day, after having taken 60 grains of quinine, found her sitting up in bed, brushing her hair, with bright eyes, and laughing. Had slept; skin moist; tongue moist; urine abundant; bowels open, &c.

“After an extensive trial, I am satisfied that the influence of cinchonism in continued fever is an important discovery. It seems to cut short the disease; therefore, to lessen danger, to lessen mortality—in fact, to remove almost all anxiety for your patient. I have also tried the system with good effect in erysipelas.

“In this communication I have kept in mind that ‘one grain of matter-of-fact is worth one pound of reasoning.’—I remain, &c.,

“SMITH GLAZE BROOK.”

Copy of Letter from Charles Lister, Esq., Surgeon to the West Derby Union Fever Hospital.

“Fever Hospital,

“17th January, 1852.

“My dear Sir,—With reference to our conversation of yesterday, I feel great satisfaction, on the ground of humanity, in stating that I have extensively employed in this hospital the ‘cinchonizing system’ in typhus

fever, which you have lately brought before the profession, and that its vast superiority to all the modes of treatment hitherto employed in fever, in this country, was at once manifest. It effects that which has been hitherto deemed impossible by all medical authorities; it arrests the course of continued fever. The importance of this principle will be at once admitted by every one, and therefore needs no comment.

“As you have yourself seen several of the cases in my hospital, I shall not trouble you with details; but should you deem it useful or necessary, I shall have much pleasure in supplying them.—I remain,

“CHARLES LISTER.

“To Dr. Dundas, Canning Street.”

It may be well to place before the reader, briefly, and in juxtaposition with my own, the views of several distinguished physicians on the treatment of continued fever; and I cite these authorities not alone from their deserved eminence in the profession, but also from the fact that their opinions are almost universally admitted, and acted on in the treatment of fever, by the members of the profession with whom I have come in contact. These views are somewhat quaintly, but very clearly and tersely laid down by Dr. Southwood Smith:—
“Cold sponging, if the skin be hot; acidulated drink, if there be thirst; perfect quiet, a dark room, a silent nurse, affording prompt attendance, with a noiseless step, a cheerful countenance, and no words; this,

together with three tea-cups' full of thin arrow-root or gruel, in the twenty-four hours, comprises all else that will be required, or that will be useful, until the period of convalescence."

Dr. Jenner states, "In no other sense—in a large majority of cases at least—than that in which we say a surgeon cures a fracture, can we say a physician cures a fever. The latter, like the former, places his patient in favourable circumstances," &c. "A large and well-ventilated apartment, fresh air, a cool, but not cold, atmosphere, quiet, abstinence from solids, and a free supply of cold water and weak broth—these are the remedies on which, in a large majority of cases of typhus fever, the judicious practitioner relies for the safety of his patient;" and such, in substance, are the views laid down by Dr. Watson—the whole but a commentary on the text of Pitcairn,—"*You cannot cure a fever.*"

Bleeding and stimulants are, indeed, strongly urged in certain cases; but having never witnessed a case of typhus fever in which depletion appeared to me indicated, or where loss of blood appeared to have really benefitted the patient, I shall not dwell on this measure. As regards stimulants, I have, in exceptional cases, employed them to an extent far beyond that commonly recommended by practitioners. For example, in a case of typhus—Thomas Celeer—attended with extreme prostration, lately admitted into the Northern Hospital, after the first large doses of quinine, I resorted

first to wine, and then to brandy, of which he took twenty ounces in the twenty-four hours, and to which, I believe, the patient owed his life.

My general plan of treatment is to administer, first, an emetic; then give ten grains of quinine every second hour, until the urgent symptoms subside, or until tinnitus aurium, or deafness, supervene, when the remedy should be stopped. After an interval of about eight hours, give another emetic; then rest eighteen to twenty-four hours, and recommence the quinine. If there be restlessness, or loss of sleep, give a full dose of liquor opii sedativus, with some drops of nitric acid. Support your patient well, *from the very beginning*, with *strong* beef-tea; avoid slops; avoid purgatives. Wine will often be necessary, and occasionally brandy, especially in hospital practice, *immediately* after the first impression has been made on the disease by cinchonism. As regards stimulants, keep also in view the previous habits of your patient. Never bleed, and rarely, or ever cup; but free dry cupping, followed by a blister, or by repeated large and strong mustard poultices, when a vital organ is involved, will often prove highly important.

I believe that much larger doses than ten grains may be often given with safety and advantage; and, from his large experience in the Fever Hospital, Mr. Ed-
dowes is of opinion—in which I fully concur—that the supervention of deafness, or tinnitus aurium, does not always indicate a necessity for suspending the remedy.

Cinchonism has been tried in several cases of erysipelas, at the Northern Hospital, with good effect. It will also control gout.

I have lately looked over with attention the cases so carefully observed, and so fully and ably reported, by Dr. Jenner, and I must confess that to me they afford no sufficient evidence to support the doctrine of distinct poisons in the different forms of continued fever. In most of these cases, certain phenomena will strike the most cursory reader—namely, the intermissions or remissions, as indicated by the pulse; the frequent rigors; the copious perspirations; the enlarged and softened spleens. Case *thirty*, and the fatal cases, *twenty-three* and *thirty-three*, afford good illustrations. The doctrine that rigors, in the course of continued fever, “*invariably indicate*” the establishment of acute inflammation, is altogether opposed to my own experience, and case *thirty* disproves the doctrine. Case *thirty-five* has “typhoid fever,” whilst her mother, aunt, brothers, and cousins, are all suffering from “typhus fever.”

Relapsing fever—“the repetition of the rigors daily, for two or three days in succession, appears to approximate certain cases to intermittent fever.”—What else are they? I am here unwillingly compelled to close my brief allusions to Dr. Jenner’s highly interesting and valuable book.

One word on the foregoing evidence. Having experienced the extreme, perhaps the allowable, incredulity

with which my proposition to arrest the course of typhus fever by cinchonism was received by the great majority of my professional friends and brethren in Liverpool, I was anxious that the doctrine should come before the general profession, supported by such evidence as might obtain for the new system a prompt and impartial hearing. Moreover, as the doctrines advocated were so totally opposed to all the views of fever hitherto entertained by the medical authorities in this, and, I believe, in every other country, I was naturally desirous that the evidence adduced should be not only clear and conclusive, but free from all suspicion of bias.

Under this impression, I have abstained from putting forward my own experience, or that of my relative, Dr. Leslie, or that even of my friends and colleagues of the Northern Hospital. I rest the claims of "cinchonism in continued fever" to the confidence of the profession, on the positive, and necessarily impartial testimony of gentlemen, many of them personally unknown to me or to each other:—men of undoubted talent and experience, placed under widely different circumstances, in distant localities, and almost all of them in charge of fever hospitals. I know, moreover, that the majority, like Mr. Glazebrook, commenced the cinchonizing treatment "in fear and hesitation."

In my efforts to introduce the "quinine system" into practice, though often disheartened, I could not observe without interest the confirmation of a fact noticed in every age—namely, the different reception accorded to

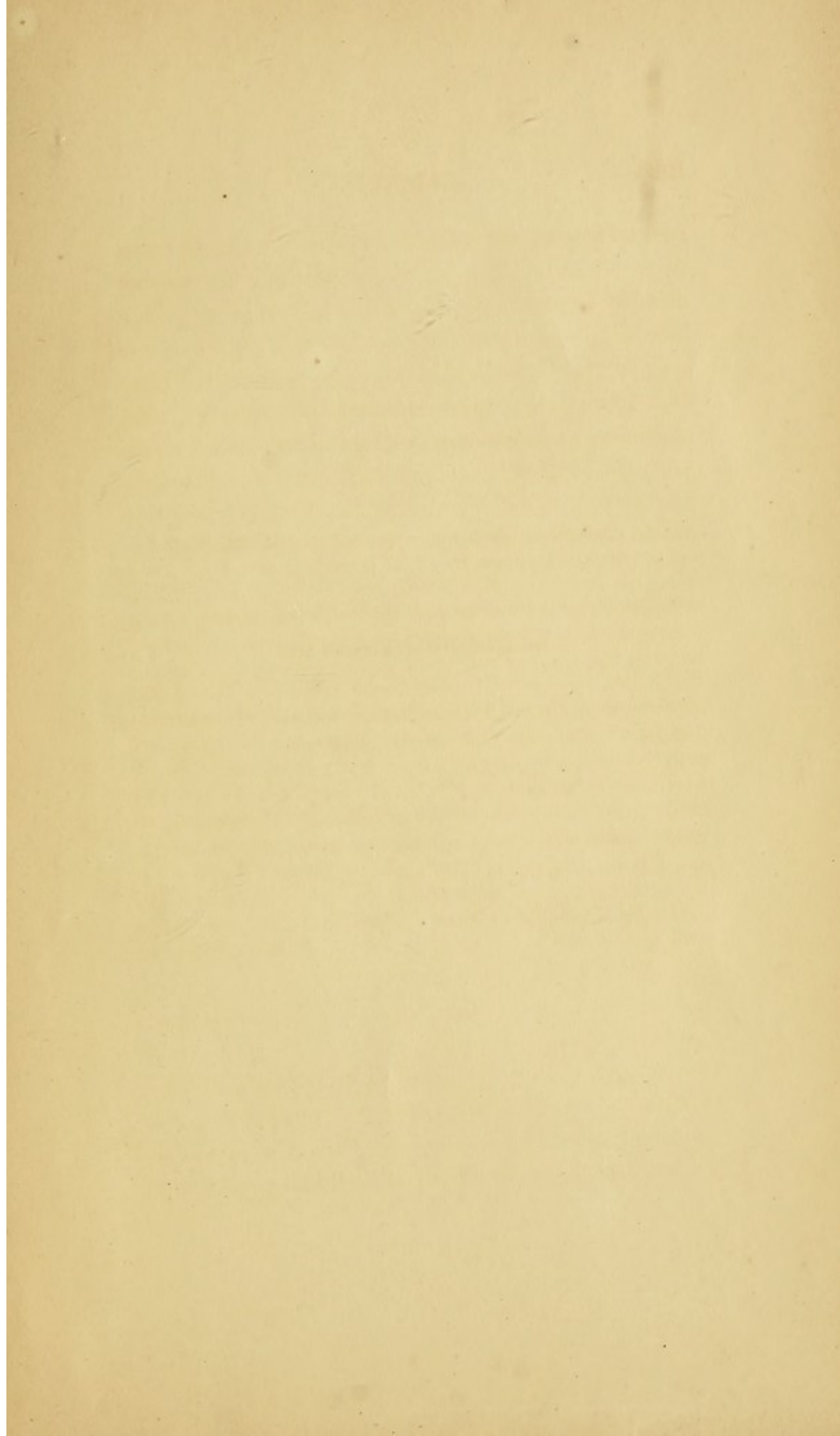
new views by the younger and by the more advanced members of all professions.

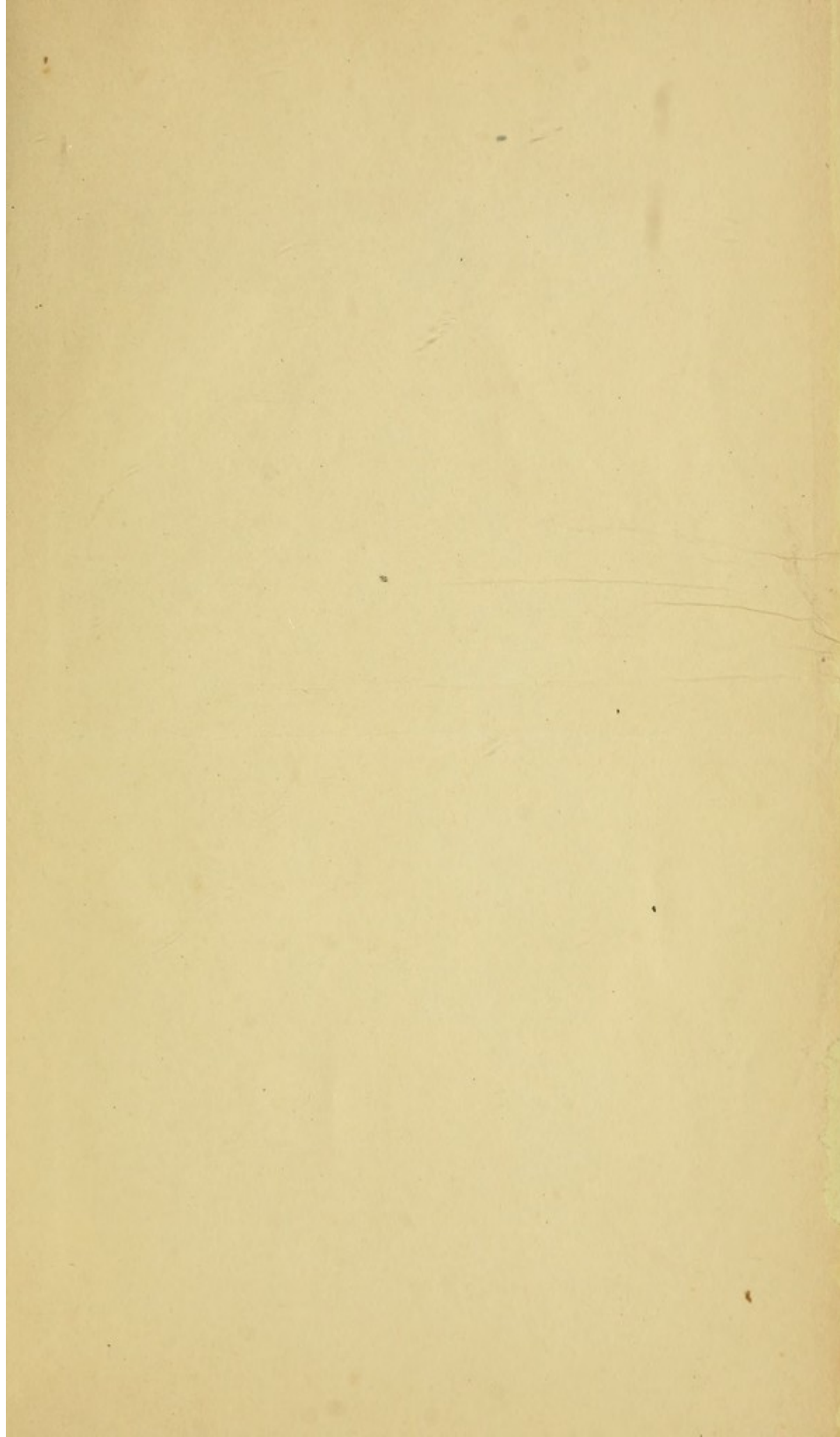
In conclusion, I feel bound to declare my conviction—a conviction founded on long, and large, and careful observation—that “cinchonism” will be found to control, generally, the continued fever of this country, *in all its forms, and in all its stages, and in all its complications.**

* The frequently *sudden* improvement which occurs in typhus, when the disease, instead of death, terminates in recovery, has been noticed by all authors. Often, after a profound sleep, the patient will awake with a soft skin—a moist tongue—the pulse moderated—the delirium gone—the conjunctivæ clear—improved strength, and some appetite. From a state of extreme peril, he has passed, at once, to convalescence! Now, the nature and importance of this fact has never yet, I believe, been correctly interpreted by physicians. No accurate observer of continued fever will question the doctrine of crisis and remissions; and close and careful calculation has satisfied me that these phenomena are as absolutely determined by the law of periodicity, as the best-marked case of tertian or quotidian ague. How often, indeed, is the so-called crisis distinctly ushered in by rigors, reaction, and perspiration, with deposits in the urine, &c.:—the *final paroxysm* in fact, (sometimes feebly, sometimes fully expressed) of the departing malady.

THE END.

C. RICHARDS, 100 ST. MARTIN'S LANE





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