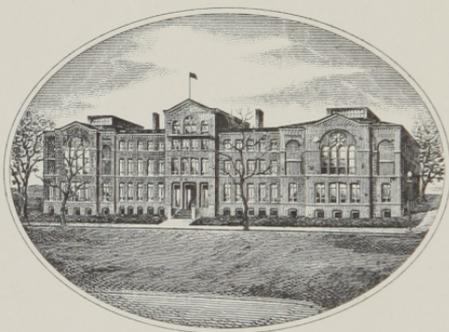


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Caldwell
on
Febrile Miasms

Caldwell (C.)

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ARTICLE I.—*Thoughts on Febrile Miasms, intended as an answer to the Boylston medical prize question, for the year 1830, "Whether Fever is produced by the decomposition of animal or vegetable substances, and if by both, their comparative influence?"* By CHARLES CALDWELL M. D.

Strenue, sed modeste conandum est.

[Extract of an official letter from Dr. Hayward, of Boston, to Professor Caldwell of Lexington, Kentucky.]

Boston, August 5th, 1830.

DEAR SIR,

It gives me great pleasure to state that the Boylston Committee on Prize Questions, at their annual meeting held yesterday, unanimously awarded to you their prize of fifty dollars, or a gold medal of that value, for your Dissertation on the question, "Whether Fever is produced by the decomposition of animal or vegetable substances; and if by both, their comparative influence?" with the motto, "*Strenue, sed modeste conandum est.*"

The Committee desired me to say, that though they felt bound by

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precedent not to ask any successful author to publish his Dissertation, it would still give them great pleasure to see yours in print.

GEORGE HAYWARD,
*Secretary of the Boylston
 Committee on Prize Questions.*

CHARLES CALDWELL, M. D.

AS the importance of the subject of this memoir is as universally known and acknowledged, as that of any belonging to the science of medicine, it would be superfluous to consume time, at present, in illustrating that of which no one is ignorant, or in exhibiting proof of what no body doubts. To say nothing of the evidence to that effect, deducible from its having been made the subject of a "prize question," by the corporation of an ancient and distinguished university, its importance is fearfully attested by the fact, that it is the source of not only the most common, but the most extensive and formidable calamities, that befall the human family. While war and famine, inundations and earthquakes destroy their thousands, febrile miasms bring millions to the grave. And the amount of suffering which they produce, in a given number of fatal cases, surpasses, not a little, that of death from most other causes. Even when death does not occur, the effects of fever are often more painful and grievous than those of mortal hurts from mechanical violence.

Could this memoir, then, be rendered worthy of its subject, its value would be incalculable. An essay throwing competent light on the true causes of fever, as respects their origin, nature, effects, and collateral relations, would confer higher benefits on man, and redound more to the glory of its author, than any other discovery in medicine. Even that of the circulation of the blood would not equal it in either brilliancy or usefulness. Nor would it be comparable to it in the extent and multiplicity of its scientific connexions. By disclosing the proper modes of both prophylaxis and removal, the discovery alluded to, should it ever be made, will either prevent fever entirely, or teach the measures requisite to destroy it in its birth. In either case, old

age and accident would become, immeasurably more than they ever have been, the chief outlets of human life.

But such a memoir must not be looked for. To say nothing of the writer's incompetency to produce it, success in the attempt is inhibited to every one, by obstacles arising out of the present condition of medical science. Facts are wanting to inform us of all that it concerns us to know of febrile poisons. The chief object that will be aimed at, therefore, in this article, will be to collect such scattered lights as are faintly glimmering from various quarters, and endeavour, by embodying them, and offering a few comments on them, to augment their lustre.

Nor is it my intention to treat of all the miasms productive of fever, with the fulness requisite even to such an elucidation of them as might be presented. An effort like this would compel me to traverse a field of enquiry by far too extensive, as well as too multifarious in its productions, to be compassed and duly explored, within the limits to which I must confine myself. My purpose is to attempt a free examination of only one of the most important of them; and on that I shall enter, after a few further preparatory remarks.

Febrile miasms are of two kinds, *contagious* and *infectious*. The contagious are the product of living matter. They are generated by morbid secretory action, in some part of the human system, when labouring under disease. A contagious miasm, which may be called febrile, is also secreted by the salivary or mucous glands of the rabid dog. The infectious are produced by changes in dead matter, presumed to be of a chemical character. But the evidence to this effect is *only* presumptive. It is not known by what peculiar action nature forms those infections; nor are they the offspring of any process that the chemist can institute. In no laboratory but that of nature, have they yet been generated. All that can be safely asserted of their origin is, that they are the result of some mutation in dead matter, produced, most probably, by the united influence of the two processes of decomposition and recombination.

I have said that contagious miasms are generated by morbid

secretory action, in living matter. But they are not always thus generated. They sometimes derive their origin from dead matter, and then attacking the human system, propagate their kind, by the peculiar action which they there induce. This is the case with small-pox, when it prevails as an *epidemic*, as it lately did in Philadelphia, and other parts of the United States. Under these circumstances it becomes an atmospherical disease, and often attacks individuals who have not been exposed to contagion from the sick; who have not even been in the neighbourhood of those affected by it, nor near to any other local source of the complaint. As the atmosphere itself is contaminated, it cannot be imagined that the poison which loads it is the product of secretion. Whence it comes we know not. But we must not, on that account, refer it to an inadequate source. The cause of epidemic small-pox, moreover, is as well known to us as that of any other epidemic. We are ignorant of the origin of every complaint of the kind. But we know something of their laws and peculiarities, as distinguished from other diseases, and can usefully avail ourselves of this knowledge in practice.

When small-pox prevails epidemically, it is more malignant and fatal, than when it depends, for its propagation, on secreted contagion alone; and it often attacks those who have suffered from it before. The reason of this is obvious. The constitution of the atmosphere, subduing and modifying the human constitution, predisposes to the complaint; and the secreted virus produces it, by acting on the system in this weakened condition. It is thus that during the prevalence of a pestilential constitution of the atmosphere, miasm from local sources generates disease much more readily, and gives to it more malignity, than at any other time.

It may be said to constitute a part of our creed in medicine, that small-pox was introduced into Europe, by the soldiers of the Cross, on their return from Palestine. The opinion *may possibly* be true; but it is deficient in proof. It resembles that which derives Lues Venerea from the continent of America; and is probably no better founded. There is reason to suspect that

both hypotheses are the result of that principle of our nature, which induces us to refer new diseases to remote sources, and to vindicate our country, as well as ourselves, from the imputation of producing an evil of any kind. From this principle arose, among the physicians of the United States, the late and long continued controversy respecting the origin of yellow fever.

Its whole history would seem to proclaim, that when small-pox first occurred in Europe, it spread *as an epidemic*. On secreted *contagion alone* its propagation does not appear to have depended. Its progress was, by far, too rapid for that, and the sphere of its prevalence, in a given time, too extensive. It passed from city to city, and from country to country, like a general pestilence, carried by the atmosphere; not like an insulated disease, propagated, by a secreted poison, from the sick to the well. Nor did the crusaders first open the intercourse between Europe and Asia. By means of commerce, it had been open for ages. Why, then, had not small-pox been previously introduced? Had its introduction depended on the importation of contagion alone, it ought to have been effected at a much earlier period. But a suitable constitution of the atmosphere was wanting. That having occurred, in sufficient strength, contagion was not necessary; not necessary, I mean, to the mere commencement of the complaint; although it would subsequently contribute to its progress and perpetuation. From whence, it might be asked, did Asia derive small-pox? Or is there any thing, in that quarter of the globe, to generate it, more than in Europe? No pathologist will hazard his reputation, by giving to this latter question an affirmative reply. To the former we know the Asiatics replied, that the disease was a production of the interior of Africa. And to what source did the Africans attribute it? To this interrogatory no satisfactory answer can be rendered, for want of an acquaintance with the speculations and dogmas of those sable philosophers. We must not be surprised, however, at being shortly informed, by one of the numerous travellers in Africa, that the wise men of that continent import the complaint from some fabled region

still further to the south. In fact, the search, by physicians and others, after the origin of small-pox, is as fruitless as that of the poet after the north;

“Ask, where’s the north?—at York, ’tis on the Tweed;

“In Scotland, at the Orcades; and there,

“At Greenland, Zembla, or the Lord knows where.

All things considered, it is highly probable, that, instead of being related to each other as cause and effect, the wars of the Cross, and the first visitation of Europe by the small-pox, were related only as contemporary events, each being in a different line of causation. This probability is the stronger from the consideration, that the disease had not existed in Asia very long before the period of the Crusades. Having been produced there, by a favourable constitution of the atmosphere, that constitution required some time to travel westward, cross the Hellespont, and carry the complaint along with it into Europe.

Were the present a suitable occasion to enter on the discussion, it might be made appear highly probable, if not certain, that the *primitive origin* of all contagious poisons must be referred to changes in dead matter. There would seem indeed to be no other source to which it can be referred. Such poisons must have had an existence before they could, by attacking the system of man, or any other animal, have reproduced their kind. And that existence must have been originally derived from some change in dead matter.

To the class of contagious miasms productive of fever belong those of small-pox and kine-pox certainly, and *possibly* that of measles. I have used the word *possibly*, because I am far from being convinced, that measles are contagious. On the contrary, the arguments that may be adduced against such a belief appear to preponderate. I am aware that the charge of heterodoxy will be almost universally preferred against this sentiment. But such accusations never move me. To make the worst of it, heterodoxy means nothing more than a difference in opinion from the majority. And although the custom and constitutions of our country authorize us to settle, by numbers, all matters of a political nature, I know of no such authority, as relates to

science. Besides, I have not forgotten the time, when the charge of heresy was loudly and angrily urged against all who denied the contagion of yellow fever. Yet there is scarcely now in the United states, a physician of any eminence who believes in it. I repeat, then, that, for reasons which might be easily and abundantly adduced, were the occasion an expedient one, I am inclined to deny the contagion of measles.

On the contagious nature of chicken-pox I decline giving an opinion. As far as my knowledge extends, authentic facts are wanting to support one. But, that that disease always begins and spreads by a secreted contagion, cannot be maintained. That it generally, if not always, *commences* from an *infectious* influence, and is thus also communicated, in most instances, to those who have had no intercourse with the sick, is a position which will not, I think, be controverted. It rests on facts that are innumerable, and as well established as any connected with medical science.

To infectious miasms belong those of influenza, scarlatina, pertussis, typhus fever, peripneumonia typhoides, pestis vera, and the entire family of bilious affections, from yellow fever, to the mildest intermittent. The ephemera sudatoria of England was also infectious, not contagious. Of dengue the same may be affirmed.

Nor does the catalogue of infectious miasms terminate here. All endemic and epidemic diseases arise from poisons of this description; all, I mean, except small-pox, which, although generally propagated by contagion alone, appears at times, as already represented, in an epidemic form—But, that I may, throughout this discussion, be the more clearly and certainly understood, I perceive the necessity of giving, before proceeding further, definitions of a few terms, which I shall occasionally employ.

An endemic is a complaint of limited extent, arising from causes connected with localities. In some instances, it is connected also, more or less, with season. Thus goitre and cretinism are the perennial products of deep and unsunned val-

lies; while intermitting fever is the annual growth of low and level alluvial situations.

An epidemic is a disease whose range may be unlimited. It is the product of causes that have no necessary connexion with either time or place, and prevails, therefore, at all seasons, and in all situations. Of this description are influenza, measles, scarlatina, and peripneumonia typhoides. Although measles and scarlatina are often denominated vernal diseases, they appear and spread at all times of the year.

A constitution is a peculiar condition of the atmosphere, arising from causes concealed from the senses, but manifest in their effects, and producing complaints of a peculiar character. Thus one constitution gives rise to influenza, another to scarlatina, a third to intermittents, and a fourth to yellow fever. And during any given constitution, especially if it be strong, no febrile disease can prevail, except that which is congenial to it. Hence no two endemics or epidemics can coexist in the same place. As well might two portions of matter occupy at once the same point of space.

Endemic and epidemic constitutions of the atmosphere, then, are the product of infectious miasms. I repeat, therefore, that all febrile affections capable of assuming an endemic or epidemic form, must be regarded as infectious. This is as true of epidemic pleurisy, peripneumony, and catarrhal fever, which occasionally succeed autumnal remittents and yellow fever, as it is of those complaints themselves.

It is on this principle alone that we can explain satisfactorily the following phenomenon of disease, which uniformly presents itself, but has never, I think, received the consideration it deserves. The febrile complaints of winter are generally supposed to depend on the sensible qualities of the atmosphere. They are considered as the more immediate productions of its vicissitudes. Hence when the temperature of the weather sinks very suddenly from higher to lower, and disease follows, it is attributed merely to the taking of cold. Yet, under sensible qualities and vicissitudes precisely alike, very dissimilar diseases occur. One change of weather takes place, and an

epidemic pleurisy or peripneumony appears. At another time a change, in all respects similar, gives rise to an epidemic rheumatism or catarrh. A third, of the same kind, renders cyanche inflammatoria epidemic, and a fourth produces affections of the eyes. Add to this, that, at other times, vicissitudes, not perceptibly different, generate no diseases at all.

These, I say, are familiar phenomena, and must have a cause. Nor can that cause be found in the sensible qualities of the atmosphere. When those qualities are alike, the phenomena are widely different from each other; and the reverse. In the insensible qualities, then, which make up its constitution, must the cause be looked for. Nor will the search be in vain. The constitution of the atmosphere is the remote cause, forming the *predisposition*, and its sensible qualities, especially its vicissitudes, the exciting or immediately productive cause of the complaint. One constitution predisposes to peripneumony, another to catarrh, a third to rheumatism, and a fourth to ophthalmia, while the same change from heat to cold, or from dryness to humidity, is the common exciting cause of the whole. If there exist no general predisposition to disease, the change, except to valetudinarians, will be harmless. So true is it, that, were it not for the existence and operation of peculiar atmospherical constitutions, neither endemics nor epidemics could ever prevail. Such appears to be the true explanation of the phenomenon in question. Viewed in any other point of light it seems a paradox.

Shall I be told that, by this mode of solution, I push the doctrine of atmospherical constitutions to an unauthorized extent, and, instead of a reality, present to the imagination a philosophical phantom? I cannot admit the correctness of the charge. For a peculiar effect I only look for a peculiar cause; and the whole economy of nature not only justifies me in this course, but counsels me to pursue it. A fundamental principle, without which reasoning would be conjecture, and inductive philosophy but a name, is, that, under similar circumstances, similar causes produce similar effects. Of course, the converse of this must be equally true. To produce dissimilar effects, similar

causes must be under dissimilar circumstances. But it has been shown, that similar states and mutations of the weather give rise to dissimilar complaints. The inference, therefore, is plain. The circumstances under whose influence the mutations operate must be different. But it is alone in a secret constitution of the atmosphere, made up of its insensible qualities, that those circumstances can consist. Imagination can refer them to no other source.

Analogy is favourable to this mode of reasoning; and we sanction it by our daily practices. Different febrile diseases, of very strong and striking characters, we never hesitate to derive from different constitutions of the atmosphere. So urgent is our propensity to this effect, that we may safely call it an instinct. To no other source do we think, for a moment, of ascribing pestilence, yellow fever, influenza, or epidemic measles. And wherefore do we have recourse to this mode of accounting for them? The answer is obvious. We are forbidden, by every consideration that bears on the subject, to ascribe them to the sensible qualities of the air. No tangible cause of any kind presents itself. Hence, by a principle of reason, which we cannot resist, we refer them to a cause that is itself hidden from us, and whose effects alone we are permitted to see. We thus reason, I say, from a feeling of instinct, which we can no more extinguish, than we can eradicate our propensities to eat, drink, love, or indulge in any other enjoyment. Nor is the impulse of the latter more irresistible than that of the former. The process of reasoning is as much the result of a necessity of our nature, as any other that belongs to us. Besides, it is the only means by which we pursue our march from the known to the unknown; that we "look through nature up to nature's God." Extinguish in us this principle, and our gaze on nature will be vacant and unmeaning. We shall wonder, but not reason; admire, perhaps, but not philosophize. We shall neither "see God in clouds, nor hear him in the wind." An acquaintance with mere objects of sense will constitute the entire amount of our knowledge.

I have alleged that we ascribe to corresponding constitutions

of the atmosphere, as their native sources, yellow fever, plague, and influenza, because they are striking and impressive complaints, and therefore awaken much attention. But are epidemic rheumatism, pleurisy, and ophthalmia less real, merely because they are less violent and more common, and excite comparatively but little attention? Or are they, as effects, less essentially connected with appropriate causes? No one will reply to these questions affirmatively. In etiology great effects have no superiority over small ones, except merely *as* effects. Of whatever magnitude they may be, they are all the product alike of specific causes; great effects, of powerful causes; smaller ones of those that are weaker. The principle being thus established, the issue contended for is conceded. If it is admitted that powerful epidemics are rightfully attributed to hidden constitutions of the atmosphere, because its sensible qualities are inadequate to their production, a similar concession will not be withheld, as relates to other epidemics, for no other reason but because they are weaker. As respects their origin, justice requires that the strong and the feeble be treated alike. It is thus that nature treats them. We have the sanction of reason, then, in ascribing to some infectious miasm, in the atmosphere, every fever that assumes an endemic or an epidemic form. Nor do I know of any other source to which they can be referred.

I am indeed aware, that certain epidemics are attributed, by medical writers, to what they call an "influence" or "a state" of the atmosphere, independently of the presence of a material poison. But on me such an effort at explanation is lost, because it is unintelligible to me. In general philosophy, I know nothing of properties, except as they belong to substance; nor, in medical science, can I recognize any "influence," except as the result of something material. If, in its "state" and "influence," the atmosphere is different, at one time, from what it is, at another, the cause is to be found in material agency. Is it warmer?—it has more caloric. Is it less transparent?—it contains more smoke, exhalation, or dust. More humid?—it is loaded with a greater amount of moisture. Does it produce remittents, yellow fever, or oriental pestilence?—it is adulterated

by some kind of febrile miasm. Does it give rise to influenza, scarlatina, or hooping-cough?—it is contaminated by miasm of another description. Is it productive of epidemic pleurisy, rheumatism, or ophthalmia?—the effect must be attributed to a similar cause; the presence and operation of a febrile miasm. No reason can be assigned, why such a poison may not throw inflammation on the pleura, muscles, joints, or eyes, as readily as on the stomach, intestines, or skin.

Shall I be told that all this is nothing but speculation, and needs proof, before it can be recognized as genuine science? I acknowledge that it is not mathematical demonstration; nor is it offered as truth, that must not be questioned. But I contend that it rests on the same basis with other opinions, which are received as true. A belief in the existence of atmospheric miasms and their influence in producing all endemic and epidemic fevers, stands on precisely the same footing. The opinion ascribing intermitting fever to such a poison, is no better supported than that which refers to a similar source, epidemic pleurisy, ophthalmia, or rheumatism. In each case the march of inquiry is from the known to the unknown; from visible effects to an invisible cause. And the induction is as fair in one instance, as in the other.

I am prepared to apprehend, that my opinion respecting the formation of atmospherical constitutions will be deemed heterodox. It will not, I suspect, be immediately conceded, that, for the production of every different constitution, a different infectious miasm is requisite. And yet I perceive no other view of the subject, which is either rational, or intelligible. No other indeed occurs to me which is even plausible. Influenza, scarlatina, measles, and peripneumonia typhoides are all specifically different from each other, and, as epidemical diseases, have their origin in the atmosphere. From the atmosphere therefore, *in different conditions*, they must be derived. That they cannot spring from the *same* condition of it, appears from the fact, that they never co-exist in the same place. But that those different conditions can arise from any other cause than the presence of different miasms, would seem impossible.

I know not how it can be reasonably doubted, that, for the production of influenza, it is requisite that one kind of febrile miasm should take possession of the atmosphere, for the production of measles, another, for that of peripneumonia typhoides, a third, and, for that of epidemic pleurisy and rheumatism, a fourth and a fifth. And the same appears to be essential to the production of every other complaint that can assume an endemic or an epidemic form.

Will it be alleged, in opposition to the views I have submitted, that the different effects alluded to, may arise from different proportions and combinations of the common elements of the atmosphere? I answer, that facts do not countenance such an allegation. Experiments show, that, as far as its "*common elements*" are concerned, there is no difference between an epidemic atmosphere, and a healthy one. Besides, admit the hypothesis, and my views are not impugned by it. The *unusual* combination thus formed, will be the epidemical miasm, for whose existence I am contending.

I am aware that some pathologists attribute the different endemics and epidemics to differences, at the times of their prevalence, in the constitutional predispositions of those whom they attack. The inhabitants of a community or place, say they, sustain attacks of intermitting, yellow, typhus, or scarlet fever, according as they are predisposed to the one or the other. And this they call an explanation. But they misname it. It explains nothing. It announces a truism, known to every one, and goes no further. That the different predispositions referred to exist, cannot be doubted. Without them the diseases could not have being; for it is an axiom in pathology, that all attacks of different epidemics and endemics, are necessarily preceded by corresponding predispositions. But it is those predispositions, in an especial manner, that testify to the existence and influence of the atmospherical constitutions for which I am contending. Of that influence they are themselves the immediate effects. The constitutions do not, by their own direct operation, produce disease. They only predispose to it. For its actual production, an exciting cause is necessary. And this cause is often found

in the vicissitudes of the atmosphere. Hence, as already stated, similar vicissitudes prove productive of dissimilar complaints, in consequence of the existence of dissimilar predispositions. Further exciting causes are, improprieties in diet and drink, excessive indulgence in other animal gratifications, exhausting fatigue, severe exposure, inordinate watching, and paroxysms of grief, fear, resentment, and other passions.

A predisposition to disease, induced by a febrile miasm, is like a predisposition inherited from unsound ancestors. It is not the disease that is inherited. The descendant is not always positively sick. His inheritance is only a predisposition or tendency to sickness, for the real production of which the action of an exciting cause is necessary. In like manner, an atmospheric constitution does not, I say, generate actual disease. It only begets a predisposition, which is converted into disease, by the requisite excitement.

There is yet another point, respecting which my opinion will be deemed by many unfounded. It is, whether measles, scarlatina, and hooping-cough are contagious complaints? The contagion of the two latter I have positively denied, and have done virtually the same, as relates to the former. The following are some of the facts, by which I consider my opinion sustained.

1. In commencing their epidemic career, those diseases uniformly attack individuals, who have not been exposed to sources of contagion. They begin thus in interior situations, where they have not been previously for many years; and in the United States, they often, in this way, make their appearance in new settlements, where they have never been before.

2. They do not attack a single individual, nor even a single family, and spread from thence, in regular and gradual progression, from the sick to the well. They attack at once many individuals or even the families of a whole village, town, or neighbourhood; and their spread is rapid and irregular; much more so than that of diseases propagated by contagion. Nurses and attendants are not more frequently or certainly attacked, than those who shun communion with the affected. They pass over families, and even whole neighbourhoods, to attack others,

at a distance; and, returning on their path, visit those whom they had previously spared. Nor are persons having casual intercourse with the sick more liable to the complaints, than others who have had none.

3. When those diseases are prevailing epidemically, if a case of either of them be conveyed without the epidemic atmosphere, the complaint never spreads, but terminates with the recovery or death of the person thus removed. This is as true of measles as of any other febrile affection. Provided the atmosphere be free from epidemic adulteration, the complaint is no more communicable than gout or rheumatism. Such at least, is the result of the reiterated observations of the writer of this paper.

4. The diseases under consideration cannot be propagated by inoculation; nor can the contagionists designate the tissue by which the miasm is secreted. Hence the well known discrepancy of their opinions on the subject. Some point to the lungs, as the secreting organ, asserting that contagion is conveyed by the breath. Others declare the liver to be the source of mischief, and the bile, of course, to be impregnated with the miasm. A third class pronounce contagion to be secreted by the stomach, a fourth by the intestines, especially the rectum, a fifth by the skin, and a sixth by the fauces and salivary glands. And, that he might certainly include the part offending, the late Dr. Good contended, that contagion is produced by every secreting organ of the body. This warfare of opinion speaks but one language; and that may be easily and briefly interpreted. The disputants are ignorant of the subject of controversy. Such indeed is the true interpretation, as respects every disputed case, where a number of individuals equal in talents and attainments, and who have paid the same degree of attention to the point at issue, maintain each a different opinion.

5. Those diseases cannot, as already intimated, co-exist in the same place; nor, while either of them prevails, does it permit, within its sphere, the existence of any other febrile complaint. Or, if it does not extinguish other forms of fever, it assimilates them to itself. In the language of a popular writer, it "compels

them to wear its livery," and "do homage to it as the sovereign of the day."

Of these facts the explanation is obvious. The complaints in question are epidemic. They have exclusive possession of the atmosphere, by means of their miasms. No predispositions except to themselves can be created, and, of course, no febrile diseases but themselves, or such as are closely assimilated to them, can occur.

But contagious diseases, *merely as such*, never take possession of the whole atmosphere. They are confined to the *personal* atmospheres of those affected by them; or, at furthest, to the atmospheres of sick rooms and infirmaries. Hence they prevail in the same place, and at the same time, with other febrile affections. It is only when a peculiar atmospheric constitution is formed, by some kind of infectious miasm, that but one description of fever can prevail.

6. They begin and end somewhat abruptly, are rapid in their march, as already mentioned, and their duration, which is in some degree definite, is rarely protracted. Of contagious affections the reverse of this is true. Their beginning and termination are much more gradual, their progress slower, and their duration, always indefinite, is, at times, very long. It may be added, that in most, if not in all truly contagious diseases, the contagion-secreting tissues can be distinctly designated. But in the complaints which I am considering, no such tissues can be shown.

Such are a few of the reasons of my disbelief in the contagious nature of these diseases. Further arguments to the same effect might be adduced. But they are deemed unnecessary. If the foregoing are fallacious, and can be refuted, others no stronger would not be likely to have a better effect.

I know it is very generally contended, that measles, scarlatina, hooping-cough, and a few other complaints, are *necessarily* contagious, because the human system is liable to be attacked by them but once. The hypothesis cannot be maintained on this ground. The reason rendered in favour of it has no weight. Were I to inquire of its advocates, what necessary

connexion there is between a secreted poison and a liability to be but once attacked by it? their only true and rational answer must be, "we do not know." Fact, then, does not sustain them. Nor does even analogy. Sundry secreted poisons are known to attack the human system more than once. There is, however, in almost every poison, whether secreted or not, a tendency to weaken, if not extinguish susceptibility, as relates to itself. No poison attacks the same person so readily or severely a second or third time, as a first. This is the only ground on which the process of acclimation can take place. Without it, the requisite change in the human constitution could not be produced. Hence the liability to disease would remain. The hypothesis, then, against which I am contending, is an *error*, which, resulting from a superficial examination of things, time has sanctioned, but which observation and reason unite to overthrow.

Of the origin of the several miasms which have been noticed I have not spoken. Nor need the ground of my silence on the subject be concealed. I can say nothing respecting it satisfactory to myself, or which I could expect to be deemed satisfactory by others. I am indeed ignorant of their origin. From what source, or by what means, the atmosphere becomes impregnated, at different times, with the miasms of influenza, measles, scarlatina, hooping-cough, peripneumonia typhoides, and epidemic pleurisy, rheumatism, and ophthalmia, physicians may conjecture, but cannot discover, in the present state of medical science. And as I have no wish to add to the number of existing hypotheses, I shall not hazard an opinion on the subject.

But there remains a miasm more interesting and important to us than all the others, at whose existence and action I have hinted. Of this our knowledge is supposed, at least, to be somewhat more extensive and accurate. I allude to the miasm of marshes, to whose influence is attributed every modification of autumnal fever. Nor is its action limited to the complaints of autumn. Throughout the year it is busy, in some climates; and, even in our own, we feel its effects in spring and summer. Nor are we free, in winter, from its lingering action on those who had suffered from its influence during the preceding au-

turn. Thus does it run perpetually its circle of mischief. I proceed now to the consideration of this poison, and purpose to speak of it somewhat in detail.

Of all febrile miasms, that of marshes is the most ancient, universal, and destructive. In these respects it has no rival. If, once in a term of several years, the miasms of small-pox, measles, influenza, and scarlatina, overrun tracts of country of considerable extent, that of marshes exhibits its effects in every populated country of the globe, during a portion at least of every year. All other miasms appear, from their history, to be comparatively of modern date. The ancients had no knowledge of them. Or if they had, they have failed to leave a record of it. But the existence of marsh miasm is coeval with that of the human race. If the views entertained of its origin be correct, its commencement must have been contemporary with the first decay and dissolution of animals and vegetables. According to the present system of physical influences, its production seems to be as necessary, and as much in harmony with the laws of nature, as the descent of ponderous bodies, or the growth of plants. Since the first establishment, then, of the present order of things, it must have existed, and produced its effects.

In our most ancient writings, those effects, if not expressly recorded, are virtually alluded to. The plagues of Egypt were as much the offspring of the miasm of the Nile, in the days of Pharaoh and Busiris, as they are at present. Since the subsidence of the first inundation of that river, its banks must have been a hot-bed of this febrile poison. Wherever, in the habitations of Israel, or among the nations around them, the pestilence "walked in darkness" the virus was present.

The pestilence which desolated the Grecian camp, on the plain of Troy, is ascribed by the poet to the resentment of Apollo. But the philosopher derives it from the miasmatic exhalations of the Simois and the Scamander.

The description of the plague of Athens, by Thucydides, is a vivid and memorable record of the same miasm. So are nearly all the writings of Hippocrates. For almost every case

of disease he has described, appears to have been the product of that poison. So is every pestilence that has depopulated cities and countries, whether in Europe, Asia, or Africa, in modern times, as well as every occurrence of yellow fever, on the continent of America, and in the adjacent islands. All these, I say, are the offspring of the miasm of marshes.

Nor do they constitute the entire amount of the mischief it produces. It is the source of all the intermittents, remittents, common bilious fevers, choleras, and most of the diarrhœas and dysenteries, that destroy such myriads of the human race. It produces also bilious colic, jaundice, hepatitis, and other affections of the liver, inflammation and enlargement of the spleen, dropsy, elephantiasis, and several other chronic complaints. Some of the latter of these are the sequelæ, rather than the immediate effects of its action.

Such are the physical evils produced by this ancient, powerful, and universal poison. When to these are added its moral effects, the distress of relatives and friends on account of the sufferings and uncertain fate of the sick, the affliction of the living for the loss of the dead, the desertion of cities during the prevalence and desolation of plague and yellow fever in them, the restraints imposed on commerce by quarantine establishments, and the injury to business, and loss of property, with the consequent ruin and want, which such events and measures produce—when the entire result is thus summed up, the aggregate of the evil and misery is appalling. But, to the influence, direct and indirect, of the miasm of marshes must the whole be ascribed. A correct knowledge of that poison, then, in its origin, relations, and laws, but more especially a knowledge of its nature, could that be attained, is important to some of the weightiest regulations of civil society, and to the health and welfare of the human race. A desire to aid, however feebly, in the promotion of such knowledge, is my leading object in preparing this memoir.

Notwithstanding the immemorial existence and ravages of marsh miasm, it is not yet a century and a half, since the attention of physicians was first particularly directed to it, as an ob-

ject of inquiry. Previously to that time its very being does not appear to have been thought of, and of course, it had not yet received a name. Its devastations were gazed on with feelings of sacred horror and superstitious awe, as if they had been the visitations of an offended Deity. And to that source, or the physical influence of the heavenly bodies, were they generally attributed. But as the shadows of the dark ages passed away, and observation and rational induction took the place of mere abstraction and hypothesis, the light of truth began to shine on medicine, as well as on other branches of science. Directed by this, Lancisi, an Italian physician of great distinction, was the first to suspect the existence of marsh miasm, to make it a subject of serious investigation, and to bestow on it the name which it still retains. He was born in the year 1654, filled, for thirteen years, the chair of anatomy, in the college of Sapiencia, was then appointed ARCHIATER to Pope Clement XI, and resided afterwards in Rome.

The summer and autumnal diseases of the Campagna di Roma, and the Pontine marshes, once the seat of health, and the country residence of much of the wealth and fashion of the imperial city, had been long the terror and scourge of that part of Italy. Lancisi, with a peculiar aptitude for original observation, but delighting chiefly in the study of etiology, could not remain content with mere efforts to cure those formidable complaints. That was to act only the part of a common practitioner. But his aim was higher. He aspired to the achievements and rank of a philosopher and discoverer. Nor was he disappointed in his ambition. After a profound investigation of the subject, he ascertained, to his satisfaction, that the diseases were the offspring of an aerial poison, produced by the dissolution of dead organic substances. And, as that process was most active and abundant in marshy places, he denominated the virus thus generated "*paludum effluvium*," marsh exhalation. In his "*Dissertatio de noxiis paludum effluviis*," a work rich in matter, and otherwise of great excellence, he promulgated his discovery, and expounded his doctrine, with a full detail of the facts that support it. This was about the close of the seventeenth

century, a period not much distinguished by the true spirit of philosophy.

For a time Lancisi shared the usual fate of original discoverers and independent thinkers. He was opposed and traduced by envious contemporaries and selfish competitors, ridiculed by punsters and affected witlings, and denounced by fanatics. By the first class he was called an "impostor" and a "speculator," and by the second a "misty" and "vaporous" philosopher, while the third declared him an "enemy to the faith," because he dared to attribute to an earthly agent, complaints which they derived from Heaven, as a righteous judgment on the ungodliness of the land. But by the clearness and force of his writings, aided by the weight of his character, he triumphed over opposition, and his views of marsh miasm, received as authentic, became the settled doctrine of the medical world. If an opponent of it occasionally appeared, his influence was limited, and his hypothesis never outlived himself.

True, even at the present day, there are some who disbelieve in the existence of the poison, attributing autumnal and other fevers to the action of the sensible qualities of the atmosphere; especially to vicissitudes in its temperature and moisture. But their opinion is untenable. A solitary but well known fact completely subverts it.

When the yellow or any other form of bilious fever prevails in a seaport town, or on the coast of navigable water, mariners, who go on shore, are peculiarly liable to it. But, provided they never visit the shore, but remain constantly on board of a ship lying at anchor, only a cable's length from land, they retain their health. This fact is notorious, and easily explained. The febrile miasm from the shore does not reach them. To any difference in the sensible qualities of the atmosphere the phenomenon cannot be attributed. No appreciable difference exists. Those qualities are precisely the same on shore, and six or seven hundred feet distant from it. The presence of the poison in the former situation, and its absence from the latter, constitute, between them, the only difference, to which, in expounding the phenomenon, reason can attach the slightest weight.

Again; yellow fever is not unfrequently arrested in its progress, by the intervention of a single street. The inhabitants on one side of the street suffer from it, while those on the other escape. But the sensible qualities of the atmosphere are on both sides alike. The only difference consists in the presence of febrile miasm on one side, and its absence from the other.

Besides, how, by the sensible qualities of the atmosphere, can dissenters explain the existence of influenza, scarlatina, peripneumonia typhoides, and other epidemics? To something else than mere heat, moisture, and vicissitudes in the atmosphere these diseases must be attributed. They cannot spring from the same causes with intermittents and remittents, any more than apples and oranges can be the native productions of the same tree. They must be acknowledged to be the offspring of deleterious miasms. But if, as relates to them, this concession is necessary and admissible, it is not less so, as respects the various forms of bilious fever.

Further. In any tract of country, whose climate is variable, an extensive marsh or morass exists. A summer drought prevails, and its waters are much diminished by evaporation, leaving a large portion of its bottom nearly dry. The consequence is, the generation of marsh miasm, and the prevalence of fever. During the succeeding summer the temperature of the weather is equally high, and its vicissitudes equally numerous and great; but rain falls in abundance, and the morass is flooded. The issue is, the existence of general health. To what is the difference in the phenomena to be attributed? Not to any difference in temperature and vicissitudes. In both seasons they are alike. And, in the latter, moisture, to whose influence some pathologists ascribe fever, is much more abundant than during the former. To the presence of marsh miasm alone is disease to be attributed, during the first season, and to its absence, the prevalence of health during the last. Add to this, that in no place does autumnal fever appear, as an endemic, in which the source of its poison may not, by careful inspection, be discovered. Those, therefore, who deny the presence and influence of this miasm, in the production of fever, attribute the disease to causes

that are inadequate. Mere moisture, and vicissitudes in the temperature of the atmosphere, cannot produce it. Were this the case, no small island could be free from it; nor could a ship's crew, in tropical climates, or during the summer, in any climate, ever navigate the ocean in health. A marine atmosphere is necessarily humid; and the vicissitudes in its temperature are frequent, and sometimes great. Yet, in a clean ship, mariners are always healthy at sea; and some small islands, whose atmosphere is entirely marine, are among the most salubrious spots on earth. Autumnal fever, then, is certainly the product of an aerial poison.

But although the existence of this miasm is considered certain, nothing is known of its peculiar nature. On that point all is darkness, or, at best, conjecture. On the atmospheres of marshes, cities, and other places, where autumnal fevers prevailed epidemically, many experiments have been carefully made. But they were made in vain. No lurking poison was detected. By the test of the best eudiometers, used by the most skilful and dexterous experimenters, no difference in purity is discovered between the air of the ocean and the land, the mountain and the valley, the city and the country, the healthy champaign and the sickly morass.

But it is not to be hence inferred, that no difference exists. The contrary is unquestionably true. A difference does exist, as results demonstrate; but it has not been ascertained wherein it consists. The reason is obvious. The means employed are unsuitable. They are calculated to ascertain what proportion of oxygen the atmosphere contains, but nothing more. No test for an aerial poison has been yet discovered. Hence, even in the atmosphere of the wards of a small-pox infirmary, which persons unprotected by vaccination or otherwise, could not enter without the hazard of an attack amounting almost to certainty, no variolous miasm can be detected. There is reason to believe that metallic substances can be converted into vapour, and thrown into the atmosphere, in such a condition, as to escape detection by any experiment that can be instituted for the pur-

pose. So little do we know of the real composition of the air that we breathe.

But in lieu of *discovery*, as relates to the nature of marsh miasm, *conjecture* has been prolific. The imagination of Ovid never teemed with such a brood of metamorphoses. Nor was the offspring more incongruous and monstrous. One hypothesis identifies the poison of autumnal fever with nitrogen, another with oxygen, a third with carbonic acid gas, a fourth with carbonic oxid, a fifth with nitrous oxid, a sixth with hydrogen, a seventh with carburated hydrogen, an eighth with sulphurated or phosphorated hydrogen, and others with other aerial substances, until the resources of hypothesis have been fairly exhausted. So vague and limited, I say, is our knowledge of what the atmosphere really contains, notwithstanding the confident tone in which chemists speak and write on the subject.

In as much, then, as we are ignorant of the *nature* of marsh miasm, what is the amount of our knowledge in relation to it? In particular, what answer can be rendered to the following "Boylston medical prize-question," proposed by the corporation of Harvard college, for the year 1830?

"Whether Fever is produced by the decomposition of animal or vegetable substances; and if by both, their comparative influence?"

To reply to this question fully and conclusively, may be pronounced impossible. The present condition of etiology does not admit of a result so definite. In a matter of such interest to science and humanity, our knowledge of the difference between the products of the decomposition of animal and vegetable substances, is too limited to be confidently relied on. There is, indeed, reason to believe, that, in many cases, the difference, if any exist, is exceedingly slight; so slight, as to be, for any practical purpose, wholly inappreciable.

Of a large proportion of the lower orders of animals, especially of such as inhabit the water, and burrow in the ground, the component parts are almost identical with those of vegetables. With those of some vegetables their identity may be pronounced complete; at least so far as analysis has been carried. The difference, in this respect, between vegetables and the

entire verminous and insect tribes, is very inconsiderable. It is not credible, therefore, that the difference can be great between their products, in any chemical changes they may sustain. But it is chiefly the lower orders of animals here referred to, that die, and are decomposed, in great abundance, during the summer and autumn. It is but rarely that those of higher standing perish and putrefy, in sufficient masses to do much mischief. Except on the field of battle, in besieged towns where famine and pestilence already prevail, and in ancient cemeteries, such an event is perhaps unknown. Although injury may be done by the dissolution of smaller quantities of such matter, its sphere is limited. It is with a reference to the death and decomposition of the lower orders of animals, then, much more than of the higher, that a reply must be prepared to the Boylston question.

It will be perceived that, in making these remarks, I consider the question as relating exclusively to the fevers of summer and autumn, denominated bilious; and not to typhus fever. Of the causes of this latter complaint my views are different. When it occurs in jails, hospitals, ships, and other crowded, foul, and unventilated places, its origin appears to be purely animal. And the matter producing it is that which is exhaled from the human body, converted into a poison by chemical agency. It must consist chiefly of the perspirable matter, that which issues in vapour from the lungs, and possibly of the exhalations from alvine discharges. When suffered to accumulate in a confined and stagnant atmosphere, the temperature of which is sufficiently high, these matters pass into a poison of great virulence. But it is not contagious. It cannot I mean, by the morbid action it excites, generate its own likeness. When first secreted it is innocent, but it becomes deleterious by a chemical process, which it undergoes, after its elimination from the system. But this process does not appear to be genuine putrefaction. It would rather seem to be a mode of change *sui generis*, of whose nature we have no knowledge. Nor do we know any thing of the poison it produces, except from its ef-

fects. Toward the close of this memoir I shall make it the subject of a few further remarks.

In reply to the question proposed by Harvard, I have no hesitation in expressing my belief, that marsh miasm, the remote cause of bilious fever, is the product chiefly of vegetable matter, in the process of *decomposition*, but not, I apprehend, of real *putrefaction*. The poison exists where no signs of putrefaction appear. Hence by an indiscriminate and indefinite use of that term, obscurity has been thrown on the subject, and obstacles to the ascertainment of truth created.

I have alleged that the miasm of bilious fever is produced *chiefly* by the decomposition of vegetable matter. But perhaps it is not thus exclusively produced. The decomposition of the lower orders of animals, that perish during the summer and autumn, acts probably as an auxiliary source. It cannot, however, I think, be a very fruitful one, on account of the comparatively small quantity of matter concerned in it. It cannot be doubted that the vegetable matter annually decomposed amounts to many thousand times its bulk. And as its composition is nearly the same, the quantity of miasm it produces must bear to that produced by the animal matter a similar proportion.

When, by excessive falls of rain, the erection of mill-dams, the obstruction of water-courses for the purposes of manufactures or navigation, or by any other cause, considerable tracts of land are flooded, the consequence is the destruction and decay of large quantities of vegetable matter. We know this to be true, because we see the vegetables passing into dissolution in great abundance. But, as respects the death and dissolution of animals, the case is different. Of them but few are ever seen by us. Nor are we authorized to fancy them where we do not find them.

But under the circumstances here represented, miasm is generated, and fevers prevail. Nor, in the places designated, does the poison appear, except under the circumstances just specified. As respects its origin, therefore, the inference seems plain. It is the product chiefly of vegetable matter undergoing decomposition. After the vegetables originally flood-

ed and destroyed have passed into entire decay, and been dissipated in gas, or otherwise carried off, the fever of the place ceases. Hence old mill-ponds are much less deleterious to those who live adjacent to them than new ones. Yet probably the number of worms, insects, and other animals, that annually die about their borders, and suffer decomposition, is as great as previously.

Around the margin of marshes, and in fenny districts, where, in some form, bilious affections annually prevail, decaying vegetable matter abounds. But observation teaches us that the amount of animal substance, in the same condition, is very inconsiderable. The same is true, in every district of country, where, during summer and autumn, bilious endemics prevail. In all such places, then, it is evident, that the febrile miasm is the product of vegetable much more than of animal substances. But of the exact proportion which the two kinds of matter bear to each other, no one is privileged to speak; because, I believe, no one has instituted experiments for the solution of the problem. Nor do I perceive in what way experiments to that effect can be instituted, with any prospect of success. In South Carolina the cultivation of the indigo plant formerly, and of the rice plant, at present, has been found to be productive of bilious fever. Here again, as far as observation may avail, in ascertaining facts, it is vegetable matter alone that undergoes decomposition, and generates the poison. But I know of no instance on record, nor have I ever witnessed one, where masses of putrefying animal substances, have produced, under similar circumstances, intermitting or remitting fever. If they have been suspected of giving rise to a few individual cases of disease, they have not been the cause of an endemic or epidemic. It may be received, then, as a principle, that, in the open atmosphere of the country, autumnal fever is the product chiefly of vegetable decomposition.

But how stands the case in large cities, where common bilious fever rises to a higher grade, and yellow fever occasionally prevails? Is the miasm here also the product chiefly of vegetable matter? To answer this question with definiteness is im-

possible. The filth of a large city consists of such a mixture of animal and vegetable feculence, that the one cannot be distinguished from the other; and, therefore, no inspection can discover, in many cases, which predominates. Might I indulge in mere opinion, however, I would say, that, here too, vegetable substances are most abundant.

But if we take, as our guide, the reputed origin of yellow fever, in our large seaport towns, we shall probably find it equally referable to animal and vegetable decomposition. In 1793 the commencement of that disease is believed to have been satisfactorily traced, in the city of Philadelphia, to a cargo of damaged coffee. In 1797, to a cargo of damaged West India fruit. In 1799, to a few cargoes of damaged and highly offensive hides. In 1803 the disease made its appearance, about the same time, in three distinct places, remote from each other. In each place was a large quantity of unsound oysters, to which it was attributed. Their condition rendered them, on account of their fetor, a serious nuisance to the neighbourhoods in which they lay; and nothing similar existed, at the time, in any other part of the city. In 1805 the fever was again traced to an immense pile of putrid oysters, in the District of Southwark. The stench alone, that issued from this mass of putridity, produced, in many persons, on approaching it, immediate sickness, by its intolerable offensiveness. The same was true of the putrid hides, in 1799. It is worthy of remark, that in the neighbourhoods of these nuisances, several dogs and cats sickened and died of bilious affections, previously to their attack on the human race. To those who have been observant of epidemic diseases, this is known to be a common occurrence. Such complaints are often ushered in by sickness and death among domestic animals, and sometimes among those that run wild in the forest. In 1798 the complaint appeared, in Philadelphia, during the same week, in four or five different and distant parts of the city, and was traced, in each, to a mass of putrefying matter, in some instances animal, and in others vegetable. In the summer of 1819, a most malignant fever, which, in Boston, proved destructive to a number of individuals, was clearly tra-

ced to a cargo of damaged corn. No one was attacked, who had not been in the immediate atmosphere of the vessel that contained this article; and of those who were thus exposed, but few escaped. Nor did the fever cease, in the neighbourhood where the vessel lay, until she was removed to a distance from the wharf, scuttled, and sunk.

The writer of this article witnessed once, in the city of Philadelphia, several cases of yellow fever, which seemed fairly referable to a mass of putrid fish. He would here remark, that as far as his knowledge on the subject extends, the matter of aquatic animals, undergoing the process of decomposition, has been more frequently charged with the production of malignant fever, than that of the higher orders of aerial animals. But, as respects its component parts, the former is more nearly allied to vegetable matter, than the latter. Of malignant fever said to be produced by the putrid carcases of men and horses, on the field of battle, he knows nothing, except from information. He, therefore, forbears to speak of it. Of most that he has here narrated, he has been himself a witness. Hence he speaks of it with the more confidence.

He is aware that some pathologists deny not only the production of malignant fever, but the practicability of its being produced, by the decomposition of masses of animal matter. To sustain their opinion, they adduce instances, in which such nuisances did not generate fever. Hence their inference that it cannot do it.

To sound etiologists it is scarcely necessary to observe, that this form of reasoning is not only inconclusive, but exceedingly hazardous. Its character is negative. It virtually contends, that because an event has not occurred under one set of circumstances, it cannot occur under another; that because it has not been produced at one or two particular times, it cannot be produced at all. But it should never be forgotten, that, in different states of the atmosphere, the results of putrefaction are equally different. As well may it be contended that, because a military leader has never lost a battle, he cannot be defeated. It is scarcely necessary to add, that one positive fact overbal-

ances a thousand such negatives. This topic will be recurred to hereafter.

To all the foregoing reputed causes of yellow fever objections are presented. It is said that the nuisances which have been specified always exist in large commercial cities; and it is therefore, asked, "If such are the sources of yellow fever, why does not the complaint prevail every summer, wherever they are found?" The answer is, that the heat is not always sufficiently intense and long continued; nor does there always exist an auxiliary constitution of the atmosphere. Yellow fever requires, for its production, even in sporadic cases, a certain continuance of tropical temperature; and without a congenial constitution of the atmosphere, it can never, in temperate climates, become epidemic. Were it requisite, facts in confirmation of these two positions could be easily adduced. But they must be familiar to every one competently versed in medical knowledge; especially the knowledge of the diseases of warm climates. The answer, therefore, is deemed satisfactory.

A further objection, however, presents itself. If, at the commencement of yellow fever, the nuisance to which it is attributed be removed, the disease is not eradicated. It not only continues to prevail in the place of its first appearance, but spreads to distant points, and does not disappear, until its extinction by a change of season; the actual passage of the summer and autumnal temperature into that of winter.

The fact, as here stated, is true; and the problem it presents is difficult of solution. When an epidemic yellow fever has begun its career, in one of our large commercial cities, nothing but a termination of warm weather can arrest it. Local nuisances may be removed, the inhabitants of the city may fly, man may erect all his artificial barriers, currents of water may be made to flow along the gutters, rains may fall and wash the entire streets, and the winds may blow, and change the atmosphere of the place. But all to no purpose. If the temperature of the atmosphere continues high, the epidemic mocks at resistance, until it expires under a regular change of season.

How does this comport with the belief, that it derives its ori-

gin from a local nuisance? The question, I repeat, is full of difficulty. I have but one answer to give to it, and that does not please me. The local nuisance, whether it be damaged coffee, fruit, oysters, fish, or corn, emits the febrile miasm first, because it is most matured for its production. It contains the greatest amount of the ingredients requisite to form the poison, and those ingredients are in the most suitable condition for the generative process. Hence the miasm goes forth from it, and the disease begins in its immediate neighbourhood. As the season advances, the common impurities of the streets, which also consist of animal or vegetable substances, or of a mixture of both, are brought into a similar state of preparation. In them, therefore, the same process is excited, and the same changes are produced. Hence they send out their poison, and thus aid in continuing and spreading the disease. And as no human means can remove the whole of them, nothing can eradicate the fever, or stay its progress. The impurities of the streets, now converted into actual nuisances, cover an indefinite extent of surface, and, by means of decomposition, emit every where more or less miasm, until the occurrence of cool weather extinguishes the process. Hence, until that period, the disease continues, and then disappears; as the effect always and necessarily ceases with the cause. Some pathologists have alleged, that the miasm, first emitted from the original nuisances, may act as a ferment, to excite in other masses of filth the process necessary for its own production; and that, by its operation, not on the living human body, but on dead matter, it thus contributes to its own continuance. This being only a conjecture, unsupported by fact, I simply allude to it, leaving to time, under the progress of science, to determine its worth.

To the source of yellow fever here referred to, it is once more objected, that, in large cities, those persons who are concerned in slaughter-houses, tanyards, and soap-candle and glue-factories, where there is much putrid matter, are not more subject to the disease than others who are engaged in cleanlier employments.

To this I reply, that I have often visited and examined the places here designated, with a view to the ascertainment of their

actual condition. My express object was to enable myself to determine, on the best evidence the cases afforded me, whether they were real nuisances, dangerous to the health of the community.

On these occasions I was never able to discover facts, which, in my own opinion, justified a complaint against the establishments. The odour which issued from them was, indeed, unpleasant. But it was not sickening. The animal matter accumulated in them was neither fresh nor sweet. But it was equally remote from being actually putrid. It manifested nothing of that far-gone decomposition, which I have witnessed in other masses of dead matter, suspected as the sources of febrile miasm. To suffer it to pass into such a condition, would render the article useless. In the factories, therefore, it is subjected to the processes of art to which it is destined, before the damage is so deep, as to render it dangerous.

That a slaughter-house might be converted into a nuisance injurious to health, is quite possible. But I repeat, that I have never seen one, the condition of which induced me to believe that it was so. I cannot, therefore, consider the objection valid. Some have even pronounced the exhalations from slaughter-houses favourable to health. In this opinion I cannot concur. Neither science nor experience supports it.

But admit that from the establishments, here mentioned, a gas somewhat deleterious does arise, the mischief done by it need not be great. As is the case with regard to other miasms, those who live in the midst of it become, by degrees, so accustomed to it, that it does them no injury. Thus, acclimated inhabitants are healthy, even in a sickly region, while new comers suffer from the miasm of the place.

Here a question, of some interest, not unnaturally suggests itself. Is the miasm which produces yellow fever identical with that which produces intermittents and remittents? I answer, that although formed from the same materials, I think it is not. My reasons for this opinion are as follows.

The two miasms appear and produce their effects, under conditions of the atmosphere very different from each other.

In the climate of the United States, the miasm of yellow fever requires, for its production, a long continuance of very hot and dry weather; a month or more of tropical heat connected with drought. Under no other state of weather does it seem to be formed. But the miasm of intermittents is generated in an atmosphere cooler and more humid. In the former case the generative process is more intense, in the latter more mild. That the products, therefore, should be different, comports with reason as well as observation. The opinion is strongly supported by analogy; a source of evidence, which, in the absence of positive proof, is worthy of regard.

In other instances, different compounds are formed out of the same elements, according to the strength of the generating process. Thus, under active combustion, phosphorus unites with a larger portion of oxygen, and forms phosphoric acid. Under mild combustion, it unites with less, and the phosphorous acid is the product. But the difference between those two acids, in their strength, affinities, combinations, and general effects, is known to be great.

Of sulphuric and sulphurous acids the same is true. They are formed by processes differing in intensity, and they differ from each other in almost all their attributes. Nitrogen and carbon form also different compounds, according to the quantities of oxygen with which they combine. Of other elementary substances the same may be affirmed. Why not then also of the ingredients, whatever they may be, which enter into the composition of marsh miasm? Of facts opposed to this opinion I have no knowledge; and, I repeat, that reason and analogy support it. Even in the preparation of malt-liquor and wine, the result is different, according to the greater or less intensity of the fermentative process.

But I have yet a stronger and plainer reason for disbelieving in the identity of the miasms of yellow and intermitting fevers. They produce on the human body different effects. No two diseases are more dissimilar, than a case of malignant yellow fever, and a common tertian intermittent. Measles and scarlatina, gout and rheumatism, colic and enteritis, resemble each

other much more strongly. Yet what physician will hazard his reputation by pronouncing them identical?

Yellow fever and an intermittent are not convertible into each other. An intermittent may be changed into a remittent, and the latter into the former, because they are but different grades of the same disease. So, for the same reason, may scarlatina inflammatoria be changed into scarlatina maligna, and typhus mitior into typhus gravior; and the reverse. But, I repeat, that an intermittent cannot be converted into yellow fever, nor yellow fever into an intermittent, because they are diseases radically different.

Nor can they ever coexist in the same place. From the sphere within which yellow fever prevails, intermittents are banished, in common with every other febrile affection. But did they, as some contend, arise from the same miasm, differing only in concentration and strength, this would not be the case. The poison, near to its chief source, where its concentration and strength are greatest, would produce real yellow fever; at a given distance from that point, being more diluted and weaker, its offspring would be remittents; and at the circumference of its sphere, intermittents would be its product.

But a phenomenon like this has never been witnessed. Where its miasm is most abundant, yellow fever is certainly most malignant; at a distance from this it is less so; and near the limit of the district it occupies, it is still lighter. But it is every where yellow fever. Its access, type, and leading symptoms, except as to violence, are uniformly the same.

In Philadelphia, the phenomena of the disease have been strongly corroborative of these views. In that city, yellow fever always commences in Water-Street, where, in the nature of things, its miasm must be most concentrated and virulent; and the complaint assumes there its most malignant form. In Second-Street, about two or three hundred paces from Water Street, and on much higher ground, it is less formidable, in Third and Fourth streets lighter still, and in Fifth street, if it reaches it at all, it is a mild disease; more tractable, and much less dangerous than pleurisy, peripneumony, or a common remittent. But, as al-

ready stated, it is, throughout, yellow fever; as dissimilar to an intermittent in its mildest, as in its most destructive form.

As respects marsh miasm, there remain to be considered several points, of a more practical, and therefore more important character, than some of those that have been noticed. And as the plan of question and answer is most consistent with plainness and brevity, it shall be adopted in the discussion. Conformably to this, it may be asked first;

What are the agents requisite to the production of marsh miasm?

They are, in brief, heat, moisture, and dead organic matter, chiefly, it is believed, of a vegetable character, existing together in due quantity and proportions. But what precisely this quantity and these proportions are, has not yet been ascertained. Nor is it known in what manner the ingredients act on each other, or what peculiar changes they sustain. That a decomposition of both the moisture and organic matter takes place, seems certain. Nor is it less so that a new compound is formed. For it is not credible that marsh miasm is a simple substance. But as already intimated, real putrefaction does not appear necessary to its production. It often exists in great abundance and activity, where no signs of putrefaction can be detected.

If either of the three specified agents be wanting, marsh miasm cannot be formed. It cannot be generated either during cold weather, or under a state of perfect aridity. Nor can it be produced where there are no remains of dead organic matter.

From this view, brief as it is, of the origin of marsh miasm, the inference deducible, as to the prevention of it, is plain. Filth consists in a mixture of water with dead animal and vegetable matter. Preserve perfect cleanliness, therefore, and exemption from the poison is certain. Under that condition of things, it can no more spring up, than the orange can flourish in Siberia, or the palm-seed vegetate on a block of adamant. In the United States, it is impossible to guard against heat and moisture. They belong to our climate, and we could not subsist without them. The cleanliness, therefore, to which I allude, consists in the entire removal of dead animal and vegetable matter. Were this effected, marsh miasm would cease to

exist. Perfect cleanliness would preserve our large commercial cities from yellow fever, with as much certainty as perpetual winter. But, from the nature of things, it cannot be attained. Where human beings are closely associated in crowds and masses, filth must exist. The constitution of man forbids it to be otherwise. To some extent, therefore, marsh miasm will be generated, and fever will be the issue. The utmost man can do, and all, therefore, that he is required to do, is to prevent the excessive accumulation of filth, as the result of inattention, indolence, or neglect. When he has done this, he has performed his duty. Further preventions are beyond his control.

It is nearly ten years since Dr. Ferguson, "Inspector of Army Hospitals" in Great Britain and her Colonies, read, in the Royal Society of Edinburgh, a paper "On the Nature and History of marsh poison". The essay was published in the Transactions of that Society, Vol. IX, and republished in Vol. VII of the "Philadelphia Journal of the medical and physical sciences."

The article has attracted considerable notice, and made, perhaps, some converts to the opinion it maintains. It is doubtless of no ordinary authority, Dr. Ferguson being a physician of distinction, and having derived his knowledge chiefly from observation and experience. Many of the facts it contains are interesting and important, and some of them are announced by their author, as if they were new. They are, moreover, by perhaps a majority of physicians, received as new. But they are so received by mistake. Whatever novelty may be in the inference, which their author has deduced from them, they themselves, although individually new, are not so, in their nature. Facts precisely analogous are recorded abundantly in medical writings, and to physicians of observation and experience, in the United States, they have long been familiar. It has been, at least in the middle and southern states of the union, long and universally known, that, in the low and flat lands of rivers, bays, inlets, and lakes, in the neighbourhood of mill-ponds and marshes, and in alluvial situations generally—it has been long known, I say, that in places of this description, warm and dry summers and autumns are more productive of marsh miasm, and the fevers

which arise from it, than summers and autumns that are wet and cool. And to this only do the facts of Dr. Ferguson testify. Under a rigid analysis their import goes no further.

In August 1794, the British Army encamped on a low and level alluvial plain, in South Holland. The season was hot and dry, and the troops suffered greatly from intermitting and remitting fever. In the summer of 1799, another British army, commanded by the Duke of York, encamped, in Holland, on similar ground. The season was uncommonly wet and cool, and from intermittents and remittents the troops suffered but little. Still, however, those diseases did occur; and dysentery, which is but a modification of bilious fever, appears to have been troublesome. Here let me observe, that the same general state of things, which, in dry weather, produces remittents, throws diseased action more on the bowels, and gives rise to dysentery and cholera, when the weather is wet. This is a common occurrence, and can be easily explained. In the present case, therefore, the troops being attacked by dysentery, testifies to the existence of marsh miasm. It is not probable that wet weather alone would have created that complaint. In the summer of 1810, a third British army encamped, at Walcheren, "on a soil as similar as possible" to that of the preceding positions, "and certainly not more pestiferous." The season was again "hot and dry," and the mortality occasioned among the troops, by the endemic of the place, "was nearly unprecedented in the annals of warfare."

Our author details a number of analogous facts, which fell under his notice in Spain, Portugal, and the West India islands. His object is to show that the miasm of bilious fever is not the product of marshes. He contends that before this poison can appear, the *marshy* nature of the spot where it is generated must have disappeared, and a state of perfect dryness succeeded. "Exactly" says he "in proportion to the previous drought, and consequent dryness of soil, is the quantum of sickness. I have visited it (Salvaterra near Lisbon) upon these occasions (during its sickly season) and found it the most parched spot I ever saw." In another part of his paper, the Doctor thus expresses himself.

“One only condition, then, seems to be indispensable to the production of marsh poison, on *all surfaces capable of absorption*? and that is the *paucity* of water, where it has previously and recently *abounded*. To this there is no exception in climates of high temperature; and from thence we may justly infer, that the poison is produced at a highly advanced stage of the drying process.”—Again. “I think it may be fairly presumed that water, for as long as it can preserve the figure of its particles above the surface is innocuous, and that it must first be absorbed into the soil, and disappear to the eye, before it can produce any mischievous effects.”

That Dr. Ferguson has faithfully narrated facts, which fell under his notice, we are forbidden by his character and standing to doubt. Besides, as already mentioned, they are analogous to facts, which repeatedly present themselves to us, in our own country. But, that he has carried his opinion, as an inference from them, too far, we are amply justified in believing and asserting. It is not true that *perfect dryness* is essential to the production of marsh miasm. On the contrary, *it is true*, that under *complete aridity*, its production *is impossible*. As well may its generation from gold or silver be contended for.

From a correct history of the appearance and disappearance of marsh miasm, along the shores of the Nile, much information is derived, in relation to the production of that poison. There are two periods, at which Egypt is entirely exempt from it; when the land is *flooded*, and when it is *perfectly dry*. The season of sickness is when the country is in a state of transition from inundation to *complete aridity*. To be more circumstantial on this point.

Egypt is one of the driest countries on earth. Throughout the year, a shower of rain but rarely visits it. For all its water, as well as its fertility, it is indebted to the annual superflux of the Nile. About the middle of August, that river begins to overflow its banks. In October it attains its greatest height. During November, December, and January, its waters gradually recede within its channel; and in February or March, according to the character of the season, when the earth

is neither flooded nor dry, miasm begins to be generated, and fever to prevail. The country is now luxuriant in vegetation. In every production, whose flavour, fragrance, and colour can delight, it is a paradise. But, no refreshing showers falling, nor a cloud appearing in the heavens, to intercept even a sun-beam, as the sun advances from the south, and his fervours become more intense, all verdure and blossoms die, and the earth is parched as if by a conflagration. By the time of the summer solstice, the aridity is complete. The source of the dews is dried up, and not a particle of moisture is any where found, in all the land of Sesostris and the Pharaohs, except in the river, and in artificial reservoirs, where it is preserved for use. Neither Spain, nor Portugal, nor any other country in Europe, ever experiences so complete a desiccation.

In this condition of things, about the twenty-fourth of June, the fevers of the country suddenly cease. Why? Because the miasm productive of them no longer exists. Why does it not exist? Because there is no moisture to aid in its production. Heat and vegetable relicks still abound; but humidity has vanished. So instantaneous is now the cessation of disease, that the ignorant and superstitious inhabitants, deluded by their priests, attribute it to the interposition of a tutelary Saint. Hence public processions, thanksgivings, and rejoicings mark the occasion. This statement furnishes, as I trust, a satisfactory answer to the question, so often proposed, why the plague, which ceases, in Constantinople, only at the commencement of cool weather, should cease, in Egypt, during the hottest season of the year?

There are three causes which are equally effectual in extinguishing plague, and all other febrile diseases produced by marsh miasm: flooding, aridity, and cold weather. And they operate alike in preventing the generation of the productive poison. The latter is the agent in Constantinople, and the last but one in Grand Cairo.

One of Dr. Ferguson's opinions calls for a stricter analysis, and a more severe examination, than it has yet received. It has been already quoted, and is expressed as follows.

“One only condition, then, seems to be indispensable to the production of marsh poison, *on all surfaces capable of absorption*; and that is the *paucity* of water, where it has previously and recently *abounded.*”

“One only condition,” the surface being “capable of absorption,” and having been previously wet; and that is simply that it become dry again! No matter of what that absorbing surface is composed, pure silex, calx, alumine, or magnesia, (for all these absorb) without even a particle of animal or vegetable matter in it; only wet it abundantly, and then let it become sufficiently dry—and the drier the better; for as is “the *dryness of soil*, so is the *quantum of sickness*”—let all this be done, and the product will be a flourishing crop of marsh miasm!! Such is the mode of incubation prescribed, and such the *promised brood!* But we have it *only in promise*, and will never have it in fact. As well may we talk of raising wheat without seed, or hatching chickens without eggs. To be serious.

Suppose a plain of pure silicious sand, free from the slightest relick of dead organic matter. This would be “capable of absorption” in a very high degree. It is flooded, for a week or a month, with pure water, equally free from animal and vegetable feculence, and then suffered to pass through all the stages, to the *maximum of “dryness.”* Does there exist an enlightened etiologist who will contend or believe, that, at any point of this drying process, marsh miasm will be generated? I think not. From an answer directly affirmative, Dr. Ferguson himself will shrink. He will not hazard his reputation in an attempt to maintain a position so groundless. In every locality, whether it be the waterless channel of a mountain stream, or a stream in a valley or a plain, where he has witnessed the production and ravages of marsh miasm, *alluvion* existed. Such, in the nature of things, must be the case. When swollen by rains or melting snows, mountain streams abound, in the highest degree, in animal and vegetable relicks, washed by their waters from the adjacent heights. And their currents are never so uniformly rapid, as not to be sufficiently checked, in innumerable places, to make alluvial depositions. There is not, in either Spain or Portugal, a sin-

gle brook, creek, or river, of which this is not true. I have examined mountain streams, as impetuous in their currents as any in Europe; but I have never seen one whose banks and bottoms did not contain deposits of alluvion. And I venture to say, that such is also the condition of every "hilly ravine" in Portugal, through which even a rivulet flows, as well as of the channel of the "river Guadiana." Dr. Ferguson never witnessed the production of marsh miasm, where there was nothing but dry rocks and pure earth, free from all organic relics. Such a paradox has never been witnessed by any one; nor will it be presented, until the laws of nature change.

Indeed I can scarcely conceive of a situation and general combination of things, better calculated for the production of marsh miasm, than those which the Doctor presents to us, even when he seems to wonder that the poison was produced. Let us briefly examine them, and then decide, whether I do not speak correctly.

After the battle of Talavera, fought in "the hottest weather," the British army retreated "into the plains of Estremadura, along the course of the Guadiana river, at a time when the country was so arid and dry, for want of rain, that the Guadiana itself, and all the smaller streams, had in fact *ceased to be streams*, and were no more than lines of detached pools, in the courses that had formerly been rivers; and there they suffered from remittents of such destructive malignity, that the enemy and all Europe believed that the British host was extirpated."

Such is the picture, given by Dr. Ferguson, of the condition of the country, through which his gallant compatriots passed, and such the general circumstances of the retreat. The weather was intensely hot, the troops, of necessity, greatly fatigued, and the river Guadiana, along which they moved, converted, by drought, into "a line of pools." And adjoining these "pools," as well as in other parts of the channel of the river, were, as certainly as nature works by uniform laws, depositions of alluvion. And yet the Doctor seems himself surprised that the army was sickly; and imagines that the information to that effect will surprise others. Let him reverse the matter,

and he will be right. Had the army *not* been sickly, the fact would have been surprising. According to the general views entertained on the subject, a better arrangement for the formation of marsh miasm, than that which our author has depicted, can scarcely be imagined. Each pool, in the channel of the Guadiana, resembled a little mill-pond or lake, in a season of drought, partially exhausted of its waters. Its immediate margin, therefore, to say nothing of the bottom of the river between the pools, was as excellent a laboratory for the preparation of miasm, as memory can recall, or imagination picture. And the same was true of the margins of the "stagnant pools," and the intervals between them, in the bottoms of the "hilly ravines" of Portugal. They were so many well constructed laboratories for the formation of marsh miasm. In describing them, therefore, and reporting their products and effects, Dr. Ferguson has given us no new information. The only point of singularity or surprise, in the whole affair, is, that either he, or any other enlightened physician, should have deemed new, that which is a matter of such general notoriety. The Doctor has simply told us, that he witnessed, in Spain and Portugal, the production of marsh miasm, in places where the whole world knows it is always produced; I mean moist alluvion, acted on by heat.

The experiment on a plain of silicious sand, which I have only *supposed*, is made annually in South Carolina and Georgia, as well as in some parts of Louisiana. In those states, the pine-lands, which are plains of sand, afford healthy retreats to the inhabitants of the maritime and alluvial districts, during the sickly season. Yet, on these plains water falls profusely, and is carried off again, by "drying." But no febrile miasm is produced in the process. Yet here, the sand contains some small portion of vegetable relicks; but not enough to do mischief, under ordinary circumstances. Were it perfectly pure, the security afforded by a retreat to it would be greater.

To say the least of it, the standing of Dr. Lind, as a medical writer, is not inferior to that of Dr. Ferguson. But the entire weight of his authority is opposed to the hypothesis, that the extreme of aridity is requisite to the production of marsh

miasm. As the result of his own observation, he tells us, that, at Senegal, the year is divided into two seasons, the wet and the dry. During the former, rain falls in a profusion scarcely equalled in any other portion of the globe. Yet that is the season of sickness. During the period of the greatest aridity, the region is healthy. As relates even to Holland, one of the countries in which our author made his observations, the authority of Pringle is to the same effect. He represents a certain amount of moisture as one of the requisites essential to the production of febrile miasm. And he also wrote from observation. Further concurrent testimony exists in abundance; while none, I think, of weight, can be adduced in opposition.

Dr. Ferguson is an excellent observer, but an ordinary philosopher. He therefore, reports facts much better than he expounds their causes. In his efforts at discussion, his ideas and expressions are loose and indefinite, and his reasoning feeble. When he speaks of the "putrefaction of water," he evidently has no correct knowledge of his subject. Water, as such, is not susceptible of putrefaction. Nor is any other mineral substance. Nothing can putrefy that has not possessed life. To be putrefiable, or rather to appear so, water must hold in mixture animal or vegetable substances. And it is then the dead organic matter that putrefies, not the water. Pure water is no more susceptible of putrefaction, than pure air or pure gold. Whatever changes it may undergo by stagnation, are not the result of real putrefaction. Nor will they generate febrile miasm. The offensive bilge-water of a ship, which Dr. Ferguson specifies, as an instance of aqueous putrefaction, is impregnated with much vegetable matter. And it is that which putrefies. Water is essential to the process, but is not itself susceptible of it.

I repeat, then, that, as relates to the generation of marsh miasm, our author has not recorded a fact that is new. He has given a valuable collection, well ascertained, and fairly reported, of facts such as were already known; and that is the extent of his contribution to medicine. And for that the profession is somewhat indebted to him. But his opinion, that the amount of

disease bears necessarily an exact proportion to the prevailing degree of aridity, is an error, which the *members* of the profession are called on to reject, and in every way discountenance. If I am not greatly mistaken, the following representation is true, especially in the middle and southern sections of the United States.

When the summer and autumn are moist, and moderate in temperature, intermittents prevail in the places where they are endemic. When the season is very hot, and copious showers occasionally fall, the fevers, in the same situations, are remittents. And when drought and great and continued heats combine, a more severe and malignant complaint, perhaps yellow fever, is the issue.

The history of disease in the valley of the Mississippi, during the summer and autumn of 1829, is in direct opposition to the opinion of Dr. Ferguson. That season was distinguished by an abundance of rain. It was perhaps the wettest experienced in the West since its first settlement. No near approach to aridity prevailed during any portion of it. Yet it was far from being, as, on the Doctor's hypothesis, it ought to have been, the most healthy. On the contrary, it was marked by a greater amount of bilious fever, than had existed previously for many years.

On the subject of unusually wet and dry seasons, as connected with disease, I have a few remarks to offer, which may be introduced into the present part of my memoir, without being altogether out of place. When such seasons are accompanied by an inordinate prevalence of sickness, it is uniformly attributed to the sensible qualities of the weather. In a wet season, the superabundant rain is blamed, and, in a dry one, the excessive drought.

May not the blame, in each case, be, to a certain extent, unfounded? For the fall of a superabundance of rain, as well as for the occurrence of inordinate drought, there must be a latent cause. No *perceptible* agency is sufficient to account for them. But the cause must be meteorological. It must consist in a peculiar condition of the atmosphere, the common source of en-

demics and epidemics. Does it not, then, comport with reason to believe, that the same unusual conditions of the atmosphere which, at one time, give profuse rains, and, at another, create severe drought, may also contribute to the production of disease? May they not be connected with those atmospherical constitutions, which have been already adverted to, as the source of epidemics? And may not, therefore, excessive rains and unusual sickness, in one season, and excessive drought and unusual sickness, in another, be contemporary effects of a common cause? Although I shall not myself reply to these questions in the affirmative, I would not, without greater hesitation, give a negative reply.

It may be here remarked, that both now, and throughout all time to which history extends, epidemic fevers, especially very destructive ones, have been accompanied by extraordinary phenomena, mostly atmospherical, but, on many occasions, connected also with the animal and vegetable kingdoms. Hence, in Scriptural history, pestilence, famine, the locust, and the palmer worm, are often associated in narrative, as having been contemporary. And unusual celestial appearances are stated as having frequently occurred at the same time. During some of the seasons, in which yellow fever prevailed in Philadelphia, igneous meteors are known to have been uncommonly abundant, and, during others, mosquitoes and grasshoppers, were unusually troublesome in the city and its vicinity. And we are informed, in history, that, during the prevalence of some of the plagues of Jerusalem, the "sky was so streaked, at night, by shooting stars," that the superstitious inhabitants trembled at them, as indicative of the anger of heaven. It is further remembered by the citizens of Philadelphia, that, during several of the seasons, when yellow fever prevailed in that place, there was almost an entire absence of lightning and thunder, while, during others, there was a superabundance of them.

On some occasions, when an epidemic has prevailed among the human race, domestic and other animals have been sickly, and many of the productions of the earth, in the same condition. In these instances the complaints of man have been often as-

cribed to the use of damaged provisions. But whence arose the maladies of the lower orders of creation? Is it not likely that the diseases of the human family, of the inferior animals, and of vegetables, were the offspring of a common cause? An epidemic fever is always and essentially of atmospherical origin. Nor is there any thing unreasonable in the belief, that the same condition of the atmosphere, which injures man, may injure also other kinds of living matter.

In 1793, yellow fever prevailed, in its most destructive form, in the city of Philadelphia. So diseased were the livers of hogs, brought to the market of that city, during the autumn and winter of the same year, that they could not be eaten. The disease of horses called the "yellow water" is a bilious fever, and is often contemporary with the same complaint in the human family. It is, moreover, endemial, in the same situations with bilious fever. The cats always, and the dogs sometimes, died in Philadelphia, of bilious affections, in great numbers, during the seasons in which yellow fever prevailed among the human inhabitants. So true is this, that, if, in June and July, the cats began to sicken and die, the citizens looked on the event with alarm, as a premonition of mischief to themselves. In Italy the *aria cattiva* of the Pontine marshes is highly deleterious to black cattle and sheep. It produces in them a malignant bilious fever. So certain is it, that the same atmospherical constitution which sickens man, sickens also his domestic animals. The author of the Iliad, therefore, was no less of an accurate observer than a great poet, when he said of the plague of Troy,

"On dogs and mules the infection first began,
"And last its vengeful arrows fixed in man."

But to return from this digression, and offer a few further remarks on the origin of marsh miasm. Of every fertile soil, more especially if it be *alluvial*, vegetable relicks always, and animal frequently, constitute a portion. These relicks, being greatly comminuted, are in a fit condition to undergo decomposition and change, and, under the influence of the requisite agents, to produce marsh miasm. But, as already stated, those agents are moisture and heat. Of these, the former is always present

in sufficient quantity, except under such a long and burning drought, as that which visits the Delta of the Nile. Expose, then, fairly, at any time, except when it is flooded, for a sufficient period, a tract of rich, especially alluvial soil, to the action of the summer sun, in the climate of the United States, and marsh miasm will be certainly produced. Under what degree of desiccation it will be most readily and abundantly produced, I cannot tell. But I know the soil must be neither very wet, nor robbed of the last particle of moisture. In the latter state it could no more give birth to marsh miasm, than a well burnt brick. In the former, the poison either would not be produced, or it would be absorbed by the moisture, as soon as produced. For it will appear presently, that its attraction for moisture is strong. The following fact is corroborative of the opinion here delivered.

Philadelphia stands on alluvial ground. What is now, in that city, the beautiful promenade, called Washington square, was once Potter's Field, a place set apart for the burial of strangers, and of the poor, and exhibiting an extensive and unsightly collection of neglected graves. It is about twelve or thirteen years since the improvement in it was made. The surface of the ground was uneven, being along one entire side, but more especially in one angle of the square, considerably depressed. This hollow was filled up and levelled, by earth taken from cellars, and the excavations of new streets, and otherwise collected in all parts of the city. The work was completed between the first of October, and the middle of April, of the following year. There was now presented to the sun an area of several acres, covered with fresh alluvial earth, on which his beams had perhaps never acted before. And that earth contained moisture. The consequence was serious, although not signally fatal. As the season advanced, the increasing heat, acting on this mass of humid alluvion, generated a miasm, which produced many severe and obstinate attacks of fever, in the neighbourhood adjacent to that section of the square, where the earth had been deposited. The inhabitants of the other sides of the square escaped, owing, in part, to distance, but chiefly, perhaps,

to the direction of the prevailing winds. The sickness occurred along the south and southeastern quarters of the square; and it is known that the vernal winds of Philadelphia blow chiefly from the north-east and north-west. Every other portion of the city was exempt from disease.

This fever prevailed during the latter part of April, and throughout the month of May, and then disappeared. Nor was the cause of its disappearance hidden. It was the same that extinguishes the plague, and other forms of bilious fever, in Egypt, about the twenty-fourth of June. I mean aridity. The season was dry, and the solar heat had so completely exhausted the alluvion of its moisture, that no more miasm could be produced. On the extinguishment of its cause, therefore, the fever ceased.

Another event, analogous to this, occurred in Natchez, in the summer and autumn of 1820. The police of the city had entered on the process of levelling, on an extensive scale. In some places streets were excavated to the depth of six or eight feet, or perhaps more; and with the alluvial earth, thence removed, hollows in other places were filled up. Thus was a very extensive area of fresh clay exposed to the burning sun and heated atmosphere of that region. The consequence was terrible. A miasm was generated, which produced one of the most malignant and desolating fevers that has visited the South.

Occurrences similar to these are common in the original settlements of almost every new and fertile country. When the inhabitants first arrive, the place is healthy. But no sooner have the axe and the mattock removed the forest timber and the underbush, and the plough-share opened to the sunbeams the bosom of the humid soil, than a miasm is generated, which produces fever. Hence emigrants from the eastern to the western states of the union, especially if they settle on a tract of land entirely new, rarely escape what is called a "seasoning." But, as cultivation covers the soil with plants, which feed on the vegetable matter it contains, and at the same time protect it from the sun, the miasm ceases to be formed, and health returns.

Although these facts concur in proving, that a marsh is not requisite to the production of the poison of bilious fever, they further prove, that a certain degree of moisture *is*. The prevalence of fever, then, is never directly as the degree of aridity. Dust perfectly dry will not produce it. Nor will pure silicious or calcareous earth, with any modicum of moisture that may be mixed with it. Vegetable or animal matter is as essential to its production, as light is to vision, or sapidity to taste.

How far does marsh miasm travel from the place where it is produced?

The science of medicine does not, at present, contain materials to furnish an answer to this question. Facts respecting the real movements of this poison are wanting, and mere conjecture is substituted in their place. Hence it often comes from an adjacent source, when it is supposed to come from a distant one. It is often generated in the rich, and humid soil of fields, open forests, and pleasure-grounds, when it is believed to be the product of a marsh, or a mill-pond, situated beyond them.

The extent it may travel from its source is regulated somewhat by the character of the surface, over which it has to pass. If the ground be hilly, the sphere of its progress will be more limited, than if it were level. Yet it ascends hills of considerable elevation, and produces disease on their summits. But its power is not so great on the opposite sides.

Instances are recorded, in which this poison is stated to have travelled several miles. I have never witnessed any such; nor am I prepared to believe in their existence. I doubt if it ever travels one mile; nor, in ordinary cases, more than half the distance. I repeat, that the belief in its moving so far from the place where it is formed, arises from intervening and nearer sources of it being overlooked.

When yellow fever prevails in a city, its progress, as already stated, is often arrested by the width of a street. It has been known to attack most of the inhabitants, who remained in their houses, on the east side of certain streets in Philadelphia, while those on the west side were nearly exempt from it. And beyond the limits of the city it never passes. To what must this

be attributed? Within a certain sphere, the febrile poison exists in abundance, as its ravages too plainly show. But a few paces beyond that sphere, there is no evidence that it exists at all. The phenomenon admits of but one explanation. The poison, in passing through an uncontaminated atmosphere, becomes immediately neutralized, or so diluted, as to lose its virulence. There is reason, however, to believe, that the form of miasm productive of intermittents and remittents, travels further from its birth-place, than that which produces yellow fever. But how much further no one can tell. It is useless, therefore, to dwell on the subject.

Is this poison capable of being conveyed through the atmosphere by the wind?—It is. Hence, in places where the summer and autumnal wind blows with steadiness, from one quarter, bilious fever prevails much less on the windward, than on the leeward side of marshes, mill-ponds, and other bodies of stagnant water. In such situations, the inhabitants on the former side are often in the enjoyment of health, while those on the latter are suffering from disease. And if the miasm does sometimes produce fever, on the windward side, it is at a much shorter distance from its source, than the bounds to which it reaches on the opposite side.

A knowledge of this fact is important in the selection of sites for human residence, whether in cities, towns, or single dwellings. Is the site to be chosen near to a copious source of marsh miasm? and is the place swept, during summer and autumn, by a prevailing wind? Let the residence be erected on the windward side. Thus, in Virginia, the Carolinas, and Georgia, where the summer and autumnal wind is from the south and west, the inhabitants, on those sides of a marsh, mill-pond, or river, are often healthy, while those on the east and north sides are subject to the endemic of the place. The same wind which carries the poison from the former, conveys it to the latter. I need scarcely add, that miasm is conducted, by the wind, to a greater distance from its source, than it can travel through a tranquil atmosphere.

Should it become necessary to establish a town, or erect a fortifica-

tion, or a single dwelling, near to a marsh, or any other body of stagnant water, and on the leeward side, how may it be best protected from the influence of the miasm?—By draining the marsh, or other stagnant water, and converting it into a meadow, or otherwise covering its surface with dense vegetation. Or, should this be impracticable or inexpedient, by suffering the forest timber and underbush, if there be any between the buildings and the marsh, to stand; and by planting trees and shrubbery there, if there be none. Whether it acts mechanically, or in some other way, a cordon of trees is one of the best safeguards against marsh miasm. In Persia, and other countries of the east, this truth has been long known, and practically applied. Hence, between the dwellings of the inhabitants and contiguous sources of febrile poison, crowded shrubbery, and dense rows of luxuriant and bushy trees are uniformly found.

At a short distance to the south of Philadelphia, lies a large tract of alluvial land, called the Neck. Originally it was marshy, containing much stagnant water, and its inhabitants were annually visited by intermitting and remitting fever. Between it and the city stood a cordon of trees. In consequence of this protection, the inhabitants of the city were secure from the miasm generated in the neck. When the British army had possession of Philadelphia, during the war of the revolution, the protecting timber was cut down by the soldiers, and consumed as fuel. The effect was serious, though somewhat instructive. For many years afterwards the southern extremity of the city, which lies contiguous to the Neck, suffered greatly from intermitting and remitting fever. Nor was it again secure, until, by cultivation, the neighbouring marshes and ponds were drained, and their sites covered with a mat of vegetation. The tract of land, once so unsightly and sickly in itself, and so deleterious to its neighbourhood, presents nothing now but rich meadows and productive gardens, from which Philadelphia is supplied with an abundance of hay, and the finest vegetables her market affords. Since agriculture and horticulture have thus done their work, the inhabitants of the Neck are as free from fever, as those of the city. In Jamaica, Guiana,

and other portions of tropical America, dwellings are, in like manner, protected from miasm by shrubbery and trees.

Which is the most dangerous, exposure to miasm by day or by night?—Exposure by night is, by far, the most dangerous. This answer rests, for its truth, on well known and important facts.

When yellow fever prevailed in Philadelphia, individuals who spent the day, in the city, in attention to business, or for other purposes, but left it before sunset, and passed the night in the country, usually escaped the disease. But those who remained in the infected atmosphere, both night and day, for the most part suffered. As relates to prophylaxis, this is a point of much importance. The writer of this article has the gratification to believe, that by giving directions to men of business, conformably to the principle involved in it, he has prevented much suffering, and perhaps saved many lives. His uniform reply to those who have consulted him has been, "If you cannot quit the city entirely, until the termination of the epidemic, at least sleep out of it every night."

When a ship is lying at anchor, a short distance from a sickly coast, the sailors that go on shore, by day, for wood and water, generally remain healthy, provided they return to the vessel, and sleep on board of her every night. But if they remain on shore a single night, they usually sicken.

The inhabitants of Rome, and strangers who visit that city, can pass the day with impunity in the Pontine marshes. But if they remain there a single night, they rarely escape the influence of the miasm. An attack by the endemic of the place may be considered almost certain. It may be further stated, as a fact which is notorious, that in the maritime and sickly districts of the southern section of the United States, those inhabitants who most carefully avoid exposure, at night, suffer least from the endemic of the country.

As marsh miasm has an affinity for moisture, it is believed to be concentrated, and rendered more deleterious, in the evening, by the descent of the dew. Hence the danger, in a sickly situation, of being exposed to the fall of that meteor. There are two falls of dew during the night. Of these, the first occurs

soon after sunset, and the other between midnight and sunrise. In case, therefore, of night-exposure being indispensable, it is least noxious between the hours of nine or ten at night, and one or two in the morning. Such appears to be the result of experience.

To what elevation above the surface of the ground does marsh miasm rise?

To this question, which possesses much interest, in a scientific point of view, and no little importance, in a practical one, no definitive answer can be given. That the poison does not ascend to a great height, we certainly know; but to what height precisely, we do not know. Our knowledge, however, on this subject, is sufficient to be useful to us, in our professional intercourse with our fellow citizens. We can found on it certain prophylactic measures, if we cannot derive from it such as are curative.

In a city, where the buildings are three stories high, and upwards, say from thirty-five to forty-five or fifty feet, the miasm does not reach the upper stories in the same state of concentration and strength, which it possesses in the lower. It is doubtful whether it reaches them at all. But it has been shown that exposure to that poison, at night, is much more dangerous than exposure during the day. Hence, when yellow fever prevails in one of the Atlantic cities, it is much best for those, who cannot retire into the country, to spend their evenings and nights in the upper stories of their houses. By this practice, health in Philadelphia has been, in some instances, preserved.

The same is true, as respects Norfolk. When yellow or high bilious fever has prevailed in that city, much less sickness has occurred among those who slept in upper stories, than those who slept in lower ones. In the low lands of the Carolinas, the same truth is familiar to every one. During the prevalence of the endemic of the country, those who sleep in the highest chambers are least subject to it. I know not that the experiment has ever been made in New Orleans. But I feel persuaded that it might be made with good effect.

When the plague appears in Constantinople, Smyrna, or Cairo, the Europeans, who reside in those cities, shut themselves

up in their houses, and hold no direct intercourse with those that are without. Their intended object is to avoid contagion; and they generally escape the disease. But why do they escape? Not because they prevent the approach of those who have been attacked by the plague, or exposed to it; but because they reside and sleep on the highest floors of their dwellings. They are above the reach of the poison. Let them pass their whole time in the lowest parts of their houses, and barricade their doors, and fumigate their letters and provisions as they may, the disease will find its way to them. Another ground of their safety is, that, from the nature of their seclusion, they avoid night exposure, and escape exciting causes. Their habits are regular, and they live temperately. All this contributes to their safety.

One of the most distressing and unmanageable of our summer complaints is Cholera Infantum. It is chiefly a disease of large towns and cities. The best and perhaps the only certain mode of prevention is to send children into the country, before the malady attacks them, and allow them to remain there throughout the summer. But the condition of a large majority of families forbids this precaution. Some other preventive measure, therefore, should be substituted. The writer of this memoir has successfully tried the following, induced by the reasons about to be stated.

Cholera Infantum, like other forms of bilious disease, is the product chiefly of marsh miasm. If children cannot be removed into the country, entirely beyond the sphere of this poison, let them be kept, as much as possible, *above* its reach. As already stated, it does not seem to rise, in its strength and virulence, to the highest parts of city dwellings. Under this belief, the upper rooms of houses have been directed to be converted into the family nurseries. In these the children slept, at night, and were kept in them throughout the day, except when taken out for exercise; and then they were conveyed immediately out of the city, to enjoy, for a short time, the pure air of the country. This experiment, tried in a number of instances, so far succeeded, as to prove satisfactorily the correctness of the princi-

ple, on which it was instituted. During the prevalence of yellow fever in one of our cities, physicians, whose profession does not permit them to fly from the danger of it, may contribute to their own safety, by sleeping in upper stories.

But although marsh miasm does not ascend to a great height in the atmosphere, it climbs to the summits of lofty hills. But this it does by moving along the surface of the ground. It has produced disease at an elevation of from three to five hundred feet above its source. Of this Dr. Ferguson has given us several instances; and others equally conclusive might be adduced. A current of air may bear it up an ascent, in the same manner as along a level surface.

Can marsh miasm travel far along the surface of water? We have what I deem satisfactory reasons to believe that it cannot. It has been already mentioned, that it cannot reach the crew of a ship lying at anchor, but a cable's length from the shore where it is generated. To prove this, facts innumerable might be adduced. The histories of commerce and war, in the West Indies, and other warm and sickly climates, abound in them.

Similar facts may be collected from the history of yellow fever, in our own country. During the prevalence of that disease in Philadelphia, many individuals, and several whole families, are known to have retreated to vessels lying not more than from two hundred to two hundred and fifty yards from the wharves, and to have remained healthy. In New York and Baltimore like instances have occurred. The intervention, then, of a narrow river, provided its own alluvion did not produce miasm, would afford ample protection from the march of that poison.

When persons are necessarily exposed to marsh miasm, can any effectual means be adopted to secure them from its deleterious effects?

I know of none. Camphor, vinegar, tobacco, and all other volatile and odorous substances, so generally resorted to, are useless. The employment of them is founded in ignorance, and ought to be rejected, as a species of quackery. It is as much the result of antiquated superstition, as amulets against witchcraft, or a belief in the performance of miracles by the relics of Saints. Nor have I much respect for the purifying process by

acid fumigation and whitewashing. The best that can be said of it is, that it removes one kind of adulteration by another not so bad. Nor does it always do even that. The only real purifiers of foul ships, hospitals, and sick-rooms, are clean water, good soap, and free ventilation. Let these be competently employed, and the danger of infection will be but a name.

Not a little has been said about the diet, drink, and general regimen best calculated to protect those who are exposed to febrile miasm. Some recommend a very moderate diet, consisting chiefly, if not exclusively, of vegetables, with water as the only drink. The object of this is stated to be, so to purify the blood, that the miasm may find in it no suitable matter on which to act. Others urge the propriety of a free, if not a full diet of animal food, with a liberal use of wine, or some other stimulating beverage, with a view, as they express it, to "live above the fever."

In the devising of neither of these plans of prophylaxis, does reason or experience appear to have been consulted. If a person exposed to the action of marsh miasm escape its effects, it is in consequence of the successful resistance which his constitution makes to it. But the stronger in constitution, and the more firmly settled in health the individual is, the more powerful and unconquerable will be that resistance.

How, then, is this soundness and vigour of health to be maintained? The answer is plain. Not by any sudden and great change in diet and drink. Such a measure must always unsettle the constitution, and, by deranging its balance, weaken it. A sudden relinquishment of old habits is always hazardous. Hence it is particularly so, during the prevalence of an epidemic, when the powers of the system should be in full vigour and harmony with each other, that they may resist, with the more certain success, the efforts of a common enemy. The rule of wisdom appears to be as follows. Let those who are exposed to a febrile miasm, persevere steadily in their usual course of diet, drink, and regimen, provided they have found it, by experience, to agree with them; I mean, provided it has secured to them the greatest amount of health, strength, and general com-

fort, of which they are susceptible. For, in proportion to these will be the resistance of their systems to the action of the poison. Any thing that may in the least derange their health, or weaken their powers, increases necessarily their liability to disease. It is like the mistake of a military commander, who changes imprudently his order of battle, in the face of an enemy ready to take advantage of it; a movement which almost certainly invites disaster. During the prevalence of an epidemic, then, let those whose habit it is to eat animal food and drink wine or spirits in moderation, continue to do so; while those whose diet has been vegetable, and their potation aqueous, should persevere in them; provided, I say, they have found them salutary. Entire health and strength being the objects at which they should aim, the means by which they have found them to be best secured, they should perseveringly employ.

During epidemic periods some physicians direct the frequent use of purgatives, tonics, or some other medicinal articles, as preventives of disease. This is also an injudicious practice. Provided health be sound, such a course is not only unnecessary, but injurious. The end of medicines is not to preserve health, when it is perfect, but to restore it when it is lost. The only genuine preservatives of health are suitable diet and drink, judiciously indulged in, and a steady perseverance in a well directed regimen, including suitable clothing. And the clothing should be such as may best maintain the action of the skin. By the use of active medicinal substances perfect health is necessarily deranged. During an epidemic, then, let them never be employed, unless called for by an evident commencement of indisposition; and then let them be taken without loss of time. Should constipation of the bowels occur, let it be removed; because it indicates derangement of an important function. But active purging, however useful, as a means of cure, is not a safe preventive. Such appears to be true prophylaxis. The very anxiety attendant on the frequent use of medicine, for the prevention of a prevailing disease, deranges health, and weakens the constitutional resistance, which would

otherwise be made. Hence timid and pains-taking individuals more frequently suffer, than those who, free from dread, pursue their usual occupations, and adhere to their habitual customs. It is to be understood, however, that, during the prevalence of an epidemic fever, fatigue and all violent exciting causes should be carefully avoided.

A few further remarks on the miasm of typhus fever shall close this memoir. There is perhaps no other febrile poison, respecting which so great a change of opinion has occurred, of late, as that which has taken place, with regard to this.

It is not long since the belief in the specific and contagious nature of typhus, was almost as universal as in that of small-pox. A denial of the correctness of this belief was not only denounced, as medical heresy, but openly scoffed at, as medical folly. The author of this article has good reason to know that he speaks truth, because, having been himself a very early dissenter from what was then deemed orthodoxy, on the subject, he has been often assailed in the manner here indicated.

But very different, with many physicians, is the present state of opinion. Those who were formerly strenuous advocates for the contagious nature of typhus fever, have not only renounced that hypothesis, but contend now, that the complaint has a common origin with intermittents and remittents; and, of course, with yellow fever and pestis vera. Such is the vibration from one extreme to the other, which, in the moral and intellectual, as well as in the physical world, so often takes place.

In the correctness of the opinion just referred to, I yet want facts to induce me to concur. That it is plausible and ably defended, cannot be denied. But it is not yet definitively established. Although long persuaded that typhus fever is the product of a miasm generated by changes in dead matter, and not by morbid secretion, I have no solid ground to believe its miasm to be the same with that which gives rise to intermittents and remittents. On the contrary, I am still inclined to consider the two poisons different. My opinion to this effect rests chiefly on the following grounds. It will be understood that I am about to speak of typhus, not as it is sometimes re-

ported to us from abroad, but as it always appears in our own country. And I am induced to believe that the diseases are not the same. I mean, that they do not arise from the same cause.

1. For reasons already given, the miasm of intermittents appears to be the offspring chiefly, if not entirely, of dead vegetable matter. But that of typhus springs from animal matter perhaps alone. As formerly stated, it seems to be the product exclusively, of changes that take place in certain secretions of the human body, after their escape from the secreting organs. All circumstances, connected immediately with its production, favour this belief. In particular, it is frequently generated in places where no vegetable matter can be found; while the poison of intermittents is produced in abundance where no animal matter appears, but never without vegetable. It may be added, that typhus fever often makes its appearance where there is no decomposable matter but human exhalations.

2. The effects of the two poisons on the system of man are very dissimilar. Few febrile diseases differ more widely from each other, in their characteristic phenomena, than typhus and an intermittent. Neither in their access, type, progress, symptoms, nor duration, have they any resemblance. They have, at least in these respects, no more of resemblance, than every febrile affection has to every other. The same arguments, therefore, which would prove them identical, would do the same, as relates to every other form of fever. If they be the same, fever is a unit. Small-pox, measles, scarlatina, and pestis vera, are, in origin, identical. But such a conclusion would be at war with just reasoning. Were it true, philosophers might no longer contend, that similar causes produce similar effects.

3. No less dissimilar are the situations in which the two miasms are produced. That of intermittents has its birth in the open air. Nor does it matter, in the slightest degree, whether the atmosphere be calm, or agitated by wind. But the miasm of typhus requires, for its production, close rooms and a stagnant atmosphere. In the open air it is never generated. Free ventilation is as fatal to it, as the north is to the plantain, or a

tropical sky to the reindeer or the martin. I allude only to local typhus, usually called jail and hospital fever. With epidemic typhus, such as our late peripneumonia typhoides, the case is different. The poison which produces it, is bred in open places, and bears ventilation. But no one will contend that it is the same with that which is the cause of intermitting fever. Such an allegation would be worse than hypothesis. It could scarcely escape the name of lunacy.

4. Tropical climates are peculiarly the birth-place of the miasm of bilious fever. But under their influence typhus miasm is unknown. It is, at least, a very rare production. It is the native of a higher latitude, and a cooler climate. It is generated, moreover, chiefly during the winter and spring. But, in temperate climates, those, for example, in which alone typhus fever ever prevails, bilious miasm is the product almost entirely of summer and autumn.

5. There is reason to believe, that typhus miasm can adhere to the body and clothes of an individual, and being, in this way, carried to a distance, escape from him, and generate disease in other persons. And the individual, from whose secretions the poison is formed, may still retain his health. Thus, prisoners taken from their dungeons to trial, themselves free from typhus, have, by the miasm carried along with them, produced that complaint in those whom they approached.

To say nothing of the reported occurrences at the Black Assizes and the Old Bailey, other facts of similar import may be adduced. If my own authority may be admitted in support of my position, I will relate one which I witnessed myself.

A criminal, who had been long confined in a small, foul, and badly ventilated dungeon, was about to be brought to trial. His council wishing to converse with him, but unwilling to enter a place so offensive, had him brought into an adjoining apartment. During this conference, he was assailed by a noisome odour, from the culprit's body, which produced, once or twice, a slight degree of nausea. In a few days afterwards, he sustained a very violent attack of typhus fever, from which he recovered with great difficulty. The place, at the time, was free

from disease; nor had the prisoner himself been sick. The poison issuing from his person and clothes, was the only cause, to which the disease of his counsel could be referred.

But, of the miasm of bilious fever, the same is not true. No instance can be adduced, where it has been thus carried, and thus communicated. Yet being so much more commonly and abundantly generated than the miasm of typhus, such instances ought to be numerous, were it capable of producing them. To the instances of typhus thus produced, they should be as ten thousand to one.

6. The miasm of typhus is much more destructible than that of intermittents. It cannot, as already represented, be generated in the open air. Nor can it, when formed, bear the action of the atmosphere, in its common state of purity, without losing its deleterious character. It cannot, therefore, pass through any considerable body of atmospheric air, and produce disease. It must act near its source, or not at all.

But very different, in these respects, is the character of marsh miasm. It is not immediately neutralized, or in any way deprived of its deleterious qualities, by admixture with unadulterated atmospherical air. It can pass to a considerable distance from its source, and still retain its virulence. Hence it propagates disease much more extensively than the miasm of typhus. In fine, typhus miasm depends on man for its production; and, that it may generate disease, its subjects must be shut up within a narrow compass. Hence its proper sphere of action is in cities, towns, crowded dwellings, and other confined places. Its product, therefore, as already intimated, has been called jail, ship, and hospital fever. But, as respects its generation, the miasm of intermittents is independent of man, and attacks him, wherever it finds him; alone or in crowds; in the city or the country. It is produced every where, and the human family is every where its prey. For these reasons, I cannot believe the poisons identical.

It has been remarked, that marsh miasm produces disease in domestic animals. Of the miasm of typhus the same is true. But the poison is generated by the animals themselves; I mean

by deleterious changes in their exhalations. Confine horses, cows, and sheep, too long, and in crowds, in badly ventilated stables and folds, and they will suffer from typhus. This is perhaps more especially true of the latter animals. The vulgar name of the disease which attacks them is the "rot." But it is a true malignant fever, of a typhus character. Nor is its source doubtful. Ventilation and cleanliness prevent its production, and extinguish it after it has been produced.

It will be observed, I repeat, that the form of human typhus here referred to, is that which arises from local sources. Although it is probably produced most readily, and marked with most malignity, when the general constitution of the atmosphere is unfriendly to health, yet it may be produced when no deleterious constitution exists. It is then purely local, the result exclusively of a poison generated by chemical changes in human exhalations. This form of fever I say, is specifically different from intermittents and remittents. It can neither be converted into them, nor they into it. Remittents do, indeed, at an advanced period, and under improper treatment, pass into what is denominated a *typhoid state*. But they do not pass into *genuine typhus*, any more than into *small-pox*. All violent and protracted febrile affections, whatever may be their original type and character, run toward the close, into typhoid action. This is very often the case with small-pox and scarlatina, and not unfrequently with measles. But those complaints are not typhus. No febrile disease changes its nature. It does not begin as one kind of fever, and terminate as another. Marked by the occurrence of a few new symptoms, it is, from beginning to end, specifically the same. New symptoms appear in many cases of small-pox, as well as of typhus. But it is small-pox still. And typhus has a character as specific and unchangeable as it has.

Of the origin of *epidemic typhus*, it is not my purpose to speak. That which, some years ago, passed over the United States, under the names of *typhus syncopalis*, *peripneumonia typhoides*, and *cold plague*, had no manifest dependence on place, season, or any of the sensible qualities of the atmosphere. It prevailed at all

times, in all situations, and during every description of weather. It was as completely atmospherical, as influenza or scarlet fever. Nor was it less different than they are from intermittents and remittents. Its origin, moreover, was equally obscure. To pronounce it the product of some terrene exhalation, would be to utter a mere conjecture. And yet the science of medicine cannot, at present, offer any thing better.

From having been an exclusive contagionist, Dr. Armstrong now derives from the same *malaria* which produces intermittents and remittents, the epidemic typhus, which lately prevailed in England and Ireland. In proof of his opinion, he adduces the prevalence of those diseases chiefly in the same places, and especially *their convertibility into each other*. He asserts that he has seen intermittents and remittents changed into typhus, and the reverse.

If this be true, they are but modifications of the same fever, and are, therefore, the product of the same miasm. But, of intermittents and remittents, and the typhus fever of the United States, whether epidemic or local, I repeat, it is not true. Those complaints are not confined to the same places; nor are they convertible into each other. They are not even most prevalent at the same season of the year. In fine, they are dissimilar in all points essential to character; nor do they seem to differ more in any respect, than in the causes which produce them.



