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ON

The

Etiology of Malaria.

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BY

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To

W. K. Bowlin M.D.

Professor of Institutes and

Practice of Medicin

The following Pages

are

Gratefully Inscribed

By

The Author.

The Aetiology of Malaria.

The subject of Malaria is one that has been of interest to the physician for hundreds of years. By the term malaria we understand an effluvia that contaminates the atmosphere of certain localities; rendering it deleterious to the health of man, producing certain train of phenomina to which various appelations has been given; such as Chills and Fever, Bilious fever, Intermittents &c, &c. Malaria is the great bain of all warm climates. Many

spots of the world might be rendered as the garden of Eaden for its loveliness, and, too, as fertile as the land of Egypt, yet it is made desolate by the ruthless hand of this fell destroyer. When certain train of phenomina are produced so palpable to the senses, it is but reasonable that the minds of cientific phisitians should be engaged in making in making research to acertain the cause that produced these phenomina. Thus it was previous to the time of Lancisi.

Previous to his time we find no ideas distinctly set forth concerning the origen of this poison. Living in Raam and observing that near the Pontine marshes malarial fever was previlent; also observing that in the marshes vegitation grew luxuriently;

very naturally he came to the conclusion that it must in some way depend upon the decomposition of vegetable matter. The opinions of Lancisi seemed to have satisfied the minds of the medical world for a century.

All the investigations made within that time seemes to have been made with the design of confirming the opinions set forth by Lancisi, in fact all observations made during that period were confined to marshy districts; sections in which malarial fevers are usually previlent. But such as were disposed to observe for themselves, having their minds biased by the general opinions, and making their observations under circumstances similar to those under which the observation of

Lancisi were made, it could not be expected that they should come to any conclusions different from his.

But from the evidence that we have before us, we do not believe that Malaria depends upon the decomposition of vegetable matter. Nay, we do not believe that the decomposition of vegetable matter has either part or lot in the production of this poison.

This proposition, we think, we are able to prove, at least satisfactorily to our own minds. We will first consider vegetation under circumstances favourable decomposition yet producing no malaria. Secondly, the existence of malaria independant of vegetation. Thirdly the products of vegetation decomposition, and their effects upon the human

system. First, it is manifest to every one, who has any knowledge of the health of different localities, that the decomposition of vegetable substances, can and does take place without the production of malaria. Where is there more vegetable substance undergoing decay than in the forests of our southern and south western country?

When we leave the marshes, get out where the face of the country is undulating, we look not for a chill. We have, too, the agreeable satisfaction of knowing that we were correct in our anticipations; for there a chill is a stranger. The pine forest of our southern country is powerfully healthy. "Pine settlements where there is plenty of vegetation (says Dr Dickson) are absolutely

exempt from the effects of Malaria".

To the truth of this assertion every one can testify, who has either lived in the pine woods, or has taken the trouble to make observation in these sections. So exempt are the inhabitants of the pine forest, that it is a popular opinion that it is owing to some exhalation from the pine counteracting the deleterious effect of the malarial poison. But it is evident there is no such exhalation from the pine, for as we approach the hammocks we have malarial fevers in its most malignant type even in the pine lands.

But it is maintained by some, in order that this poison may be generated, there must be a supply of water as well as vegetable substance.

Secondly, As proof that malaria can and does exist independent of the decomposition of vegetable matter we will compair the difference in respect to health between dry and wet Summers.

It is well known not only to the medical profession, but to the whole people, who have learned it from sad experience, that hot and dry summers are more productive of fevers than wet. Does the heat and dryness of the summer promote the decomposition of vegetables? Nay, rather preserves them from decomposition, even arresting it after decomposition has already commenced. Wet summers promotes the decomposition of vegetables; and are proverbially healthy. Here we have both vegetable matter and water;

and with them health.

We find in Wattons practice of Physic extracts, notes taken by Dr Fergeson a surgeon of the British army. Judging from the position he occupied, and the opportunities he possessed of making extensive observations in different and distant sections of country makes his opinions the more readily received.

He states that in the year 1795 in a campaign through Holland, that the British army encamped at Rasendael and Oosterhout it being hot and dry. "The soil in both places was a level and sandy, with a perfectly dry surface, where no vegetation existed or could exist, but stunted heath plants"

Here the troops suffered greatly from

intermittent fever. We are also told that in Walcheren, the soil is of a pure white sand, that during a very hot and dry summer the British army suffered to such an extent that Dr Fergerson speaks of it, as "being almost unprecidented in the annals of warfare"

We are again told that, "in the year 1809 several regiments of the British army in Spain took up encampment in a hilly ravine which had lately been a water course. Pools of water still stood here and there among the rocks, so pure that the soldiers were anxious to bivouack near them, for the purpose of using the water; several of the men were seized with violent remittent fever before they could.

leave the bivouack the next morning. 'Till then (says Ferguson) it had allways been believed among us that vegetable putrifaction was essential to the production of pestiferous miasmata; but in the instance of the half dried ravine before us, from the stony bed of which, (as the soil could never lie for the torrent,) the very existance of vegetation was impossible, it proved as pestiferous as the bed of a pond."

Again we are told that after "the battle of Talavera the army retreated along the course of the Guadiana river; into the plane of Estamadura. The country was so dry and arida for the want of rain, that the Guadiana itself and all the smaller

that in fact ceased to be streams and were nomore than lines of detached pools in the course that had formly been rivers. The troops then suffered from remittent fever of such distructive malignity that the enemy and all Europe believed that the whole British host had perished" When the British army was passing through Portugal to Ciudad Rodrigo, situated on the Agueda river, a very clear stream they had to pass through an extensive plain that had been likened to the bed of a dried up lake. When more than one occation, when this low land after having been flooded in the rainy season, had become as dry as a brick ground, with its vegetation utterly burned up, there arose to the troops fevers which, for mal-

-ignity of type, could only be matched by those before mentioned on the Sandians."

Again Fergusson tells us, "that in the most unhealthy parts of Spain, one may in vain towards the close of summer, look for lakes, marshes, ditches, pools, or even vegetation. Spain then generally speaking is, though as prolific of endemic fever as Walcheren, beyond all doubt one of the dryest countries of Europe, and it is not till it has again been made one of the wettest, by the periodical rains, with its vegetation and aquatic weeds restored, that it can again be called healthy, or even inhabitable with any degree of safety."

We might quote other passages to prove that Malaria does not depend upon the decomposition of vegetable matter. We think that the instances already referred to are sufficient

to convince any unbiassed mind, that malaria can and does exist independent of vegetation; even in its most malignant forms. We will now proceed to the consideration of the third division of our subject. It is not our design to give the anamatia of all substances formed by the decomposition of vegetable matter, but to consider the four principle gases, viz. Carbonic acid, Carbonic oxide, Protocarbide of hydrogen and the vapor of water. It is by the production of carbonic acid that nature seeks to remove a large portion of debris from the system. And should that be retained, it would not only result in unpleasant, but fatal consequences. In an atmosphere containing a large portion of this gas death will result. But I conceive that death in this

case is not so much the result of inhibition of the carbonic acid as it is from the want of a proper oxidation of the blood, and a retention of that portion of gas which is elaborated within the system. We have this gas abundantly formed by the burning of wood, coal, &c, also in breweries and in all places where there is fermentation. Persons exposed to this gas, even in such places where it is generated ~~most abundantly~~, never present any of the symptoms of malarial fever. From the various modes by which carbonic acid is formed, we find that it does not exceed more than three parts in ten thousand. Can we, then with any show of reason, claim for it under such circumstances results, which it is incompetent to effect in its

most concentrated form?

Carbonic oxide, though poisonous, is produced in far too small quantities to effect the system. And under such circumstances when fossons are effected, there is no intermittent; but if sufficiently concentrated, instant death is the result.

It seems more probable that the Proto carbid of Hydrogen might be the cause that produced malarial fever than either of the two that we have considered.

The only mode, that we can immagine by which this gas can effect the human system, is by inhilation, the proto carbid of hydrogen becoming decomposed, the hydrogen uniting with the sulphur of the blood, forming the sulphid of hydrogen, the carbon being set free.

According to Leibig the sulphid of

hydrogen acts very deliterious upon the system by "producing immediate decomposition" of the blood". But does this effect upon the blood produce intermittent fever? If so, and the effect is as great as represented by that author, then in cities and in the vicinity of sulphur Spring we might expect to find intermittents prevalent. But to the reverse, we find cities exempt, and sulphur Springs resorted to for health and pleasure. But admitting that the sulphid of hydrogen can effect the system only by direct combination of hydrogen with the sulphur of the blood; then we might reasonably expect that such places as were most prolific in generating the proto sulphid of hydrogen, would be the most productive

of disease. It is universally conceded that marshes entirely covered by water do not generate malaria. Yet such places as these are more productive of the protocarbid of hydrogen from the decomposition of vegetable matter than any other. In coal mines also this gas is generated in great abundance.

The miner, if it was not for the jeopardy in which his life is often placed by explosions, would regard not this gas. If it was the cause of malarial fever, under such circumstances as these we might expect it even in its most malignant form.

But the quotations which we have made from Ferguson, proves that fever can exist under such circumstances as excludes the idea of this gas.

We have now to consider the vapor of water, the last of the products of vegetable decomposition. It is contended by some, that this, when generated under peculiar circumstances, is sufficient per se to produce all that train of phenomena, which we call intermittent fever. I confess, that, I am not able to understand in what manner the vapor of water, generated under any circumstances can effect the system so as to produce an intermittent. That the vapor of water will not produce this effect, is evident from the important part water plays in the physiology of the system. We know that every tissue of the body contains large quantities of water;

and in the blood this quantity is often in great excess. But the system by means of its emunctaries rids itself of this excess.

And this excess, whether taken in by imbibition, absorption, or inhalation cannot affect the system deliteriously as long as the emunctaries are in the full discharge of their various duties

But when water is placed under peculiar circumstances may not some chemical compound be formed, perhaps isomeric with, or differing from water in the proportions of its elements, and possessing poisonous qualities, to which we may attribute these phenomena?

This looks more plausable, than

to believe that it depended either upon the products of vegetable decomposition or the vapors of water

But of this we have no proof.

This subtle poison, whatever it may be, no chemist has ever been able to detect We can know its existence only by its effects

But that such a thing as malaria does exist, and is confined to certain localities is evident, and acknowledged by the medical world.

Although this subtle poison has eluded the ken of our predecessors, it affords a broad field for investigation. And he that should be so fortunate as to ascertain its true cause and nature

will be a benifactor to his Kind
and an honor to this profession.

Geo. T. Turner