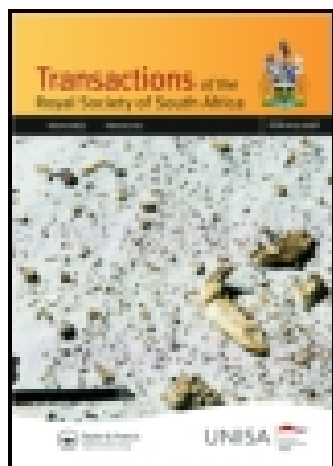


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thereby saving the higher, more expensive and dangerous portions, should such a dire contingency ever occur.

The works were finally completed in February, 1880, at a cost of £13,800.

The direct revenue at present derived from the leased new erven amounts to nearly £1,000 a year, and the indirect benefit accruing from the reservoir in having at least quintupled the spring-water supply feeding the three town furrows (thereby greatly improving the health and comfort of the inhabitants, as well as materially increasing the returns from the town erven), is estimated by some to be even greater and more important than the direct revenue above mentioned. Of one thing there can be no doubt—that with the remarkably dry five last years, with an average annual rainfall for that period of only about nine inches, Beaufort must thank the dam for meeting without stint and with great gain to the town the heavy demands made upon it.

XIV. AN ENQUIRY INTO THE CAUSE OF THE CAMP FEVER OF KIMBERLEY.—By J. H. MEIRING BECK, M.B., M.R.C.P.

[Read June 27th, 1883.]

There are probably few who have ever lived in Kimberley who will not agree with me when I assert that the "Camp Fever" has been a serious impediment to the development of the place. This being so my enquiry into the conditions causing it is important. A clear knowledge of its causation might in many cases induce people to avoid unnecessary exposure to risk of infection, or might suggest means of prevention, or might remove much of the insecurity which of necessity must exist in a community that knows not where to look for an enemy as effectually hidden from most as it is dangerous to all. Many theories have been advanced as to the cause of the camp fever, and perhaps none more "even by medical men themselves" than the bad sanitary condition of Kimberley. A moment's reflection will, however, show that this is not so. While recognising the influence of the bad sanitary arrangements existing here and there in Kimberley, in lowering the healths of individuals, and so not only predisposing them to risk of infection but also aggravating symptoms, I cannot regard this condition as the cause of the fever. The influence bad sanitation exerts on this fever is, I believe, not more than it would exert on any other fever. We can understand how bad sanitation, while unable to cause such a disease as, for example, "scarlet fever," might be able to aggravate an attack, and render it more serious. As a matter of fact, with the exception of one or two localities in Kimberley, the sanitary condition of the place is, while leaving much to be desired, not worse than the sanitary condition of many other places in the colony, certainly not worse than Cape Town; and clearly, if able to cause such serious epidemics in one place, it should

also have the same effect in others. Moreover, such diseases as typhoid fever and diphtheria are not uncommon in Kimberley, and we know sufficient of the etiology of these fevers to justify us in declaring them to be due to such bad sanitation as may exist. It is unreasonable to fether all conditions that may arise on bad sanitation, and I think I shall be able to make it clear that a far more satisfactory and feasible explanation is possible. Let me, however, not be misunderstood, and let it be not inferred from what I have just said that I under-estimate the necessity or the value of good and effective sanitation. All that I wish to lay stress upon is that, while recognising bad sanitation as a predisposing cause, I cannot regard it as the direct exciting cause of the fever. In order to convey clearly what is to follow, let me briefly indicate what I believe to be the nature of the camp fever. I believe it to be a malarious fever pure and simple. I think sufficient of my brother practitioners at Kimberley will at the present time agree with me on this point to render an exhaustive argument in support of my reasons for saying this unnecessary. I, however, wish to be clear about it, for only as recently as a year ago, while discussing the matter with one of the leading medical men at Kimberley, my suggestion as to the "malarious nature" of the fever was dismissed by him with the remark, "And where are you going to get your malaria from?" Many circumstances have induced me to regard it as such: its undoubtedly remittent type; its strict analogy to the bilious fever of India; the effect it has on organs we know to be preferred by the "malarious bacillus" (*e. g.*, the spleen and liver); its co-existence in many cases, as in India, with dysentery; the fact that it assumes often an anomalously intermittent type; the influence of certain remedies that we all regard as antidotal to malarious poison; its sequelæ, such as rheumatism, &c.; these and many other considerations have induced me come to the conclusions I have. It would take me too much out of my way to enter more fully into this matter. All I wish to indicate here is my belief, as stated above, in the malarious nature of the fever. What I propose to discuss at greater length is an explanation of the conditions giving rise to the development of the poison causing it. Personally I have entertained the theory I am about to propound ever since I first had an opportunity of studying the disease, and so convinced am I that I am right, that it has become to be regarded by me not as theory any more but as fact. Most minds associate the development of malaria with marshy places, containing organic matter such as *e. g.* decaying vegetable matter: and the total absence of undue moisture from Kimberley, as well as excessive organic matter, no doubt is the reason why for so many years no clear idea on the subject has been arrived at. Let this fallacy be once and for all dismissed from the minds of all here—for the development of malaria neither much moisture nor much organic matter is an absolute necessity. Many, and I have no doubt even some medical men, will be inclined to differ from me; I am, however, happy in being able to refer them, in support of this statement, to an admirable paper on malaria by no less an authority than "Conradi Tomasi Crudeli," which was reprinted from his official report to the Italian Government in the *Prac-*

itioner (vol. 27).^{*} To this authority I am indebted for the satisfaction of many doubts in coming to my conclusions. This is what he says, and I may add that Klebs almost unconditionally agrees with him : "We may regard as well proven facts that—1. The *Bacillus Malaria* is an organism for whose growth the air is necessary. 2. The germs or sporules of this organism may occur in soils of very different compositions, and sometimes very poor in organic substances. And what is very important—3. These malarial soils may occur in places which are not and never have been marshy."† A little further on he says :—"In all malarial soils and mud a development of the *Bacillus Malaria* into Sporiferous Bacilli, as well as a rapid succession of several generations of these Bacilli, occurs whenever they are placed under the following conditions :—*a.* A moderate temperature of about 20 deg. Centigrade (68 deg. F.). *b.* A moderate degree of permanent humidity. *c.* The direct action of the oxygen of the air upon all parts of the mass." With these facts before us, how can we apply them to Kimberley conditions ? The mine is an integral portion of Kimberley, out of it come daily thousands of loads of diamondiferous earth ; on reaching the surface this earth is carried away, and deposited in thin layers on the depositing floors. These depositing floors extend for miles in all directions, amongst houses, and when I left Kimberley a year ago were even beginning to encroach on what was supposed to be the healthiest part of Kimberley—Belgravia. As soon as deposited every particle of earth is moistened and kept moist for days together. This is done in order to encourage pulverisation of the earth, and prepare it for the washing machine, through which in due course every particle is driven, thoroughly saturated with moisture, separated from the contained diamonds, and left as debris in the "wash-up" heaps as they are called. Of these there are hundreds, dotted all over the place, many of them forming mounds of considerable size. Here, then, are all the conditions required above. It has frequently been observed in new countries (notably in Australia) that newly ploughed fields have had the power of generating malaria. Successive cultivations of these fields, it has been found, destroy this power. In this country I have myself observed that fever cases are apt to arise at isolated farms during this season of the year (ploughing season), many of which fevers I have associated with new fields that have been ploughed for the first or second time. In England it has over and over again been demonstrated that fresh railway cuttings have caused malaria. The explanation offered and accepted is that fresh earth of certain compositions, on being exposed to certain conditions, offer a favourable nidus for the development of the *Bacillus Malaria*. With all this evidence we may therefore assume that it is possible for the "fresh earth" from the mine to possess properties not unfavourable to

^{*} Malaria and the Ancient Drainage of the Roman Hills, by Conradi Tomas Crudeli, *Practitioner*, vol. 27, p. 295.

† This was noticed as far back as 1848, vide a paper published in the *Lancet* October, 21, 1848, on "The Nature of Epidemic and Malarious Fevers," by Sir Jas. Murray. Amongst the most recent of authorities, vide Fayer's "Tropical Diseases," p. 27.

the development of "malarious poison." Now this earth is spread, as I have said, on the depositing floor, and here it is subjected to the very conditions mentioned above, viz., it is *a*. Exposed to the sun's rays, which in Kimberley means a temperature often considerably higher than 20 deg. C. (68 deg. F.) (the temperature quoted above as already sufficient for the development of malaria), and we know that in India the malarious fevers become within limits directly more virulent as the places in which they occur have raised temperatures (*Vide* Fayrer's Fevers of India). *b*. It is here kept permanently and moderately moist. *c*. Free action of the oxygen of the air upon all parts is possible. So convinced am I that the cause of the fever is to be searched for here, that I believe it to be capable of experimental demonstration. I had already myself commenced to institute some experiments, in the hope of being able to verify what I have said above. An attack of fever contracted by myself, however, and my consequent departure from the place, prevented me from carrying my intention into effect. For the sake of those interested I may briefly mention the direction I intended my enquiry to take. I proposed to inoculate with earth from the depositing floor, four or five days after exposure, several bottles containing a sterilised fluid capable of supporting organic life, such a fluid as, *e.g.*, beef tea, boiled long enough to kill all contained life. These bottles I proposed after inoculation to expose for about a week or fourteen days to a temperature of about 100. F., in order to allow any organisms introduced with the earth, time and scope to multiply. At the end of the time, I intended to infect the fluid so obtained under the skin of any animal* capable of developing malarious fever, and I should have expected, if my theory were correct, to have produced in this animal an analogous fever. To this test I was anxious to submit my theory. I was, however, prevented. I need not say that I shall regard any attempt from gentlemen who have opportunities of carrying out such or any experiments that may suggest themselves, with great pleasure. But now the question arises, How is it that the same fever existed in Kimberley before the present system of disposing of the earth obtained, in other words, during the "Days of Dry Digging." The above explanation may do very well for the present time, but either it is incorrect or is insufficient to meet all the conditions under which the fever might develop. This question, I must confess, caused me for a long time to doubt the correctness of what I have advanced above, but I believe that I shall be able to offer an explanation quite consistent with what I have already said. And here again let me acknowledge the help I have obtained in coming to a satisfactory conclusion from a study of Klebs' and Tomasi Crudeli's researches. In Crudeli's paper, already referred to, he says that he found in the "Campagna of Rome" that "a very moderate degree of moisture was sufficient to produce malaria. Sometimes soils which are truly pestilential may remain inoffensive throughout a very hot and dry summer, and then all at once give rise to an explosion of malaria after

* Klebs has, I believe, succeeded in inducing malarious fever in the rabbit and Guinea pig. I have no authority at hand, but my impression is that he has done this.

being moistened by a "slight shower." In another part he says :— "The soil in malarious districts may remain exposed to the direct action of the air during a hot and dry season without causing any danger, and may suddenly become dangerous after a very slight fall of rain." In another paper* he remarks that "An elevated temperature may provoke an outbreak of it (malaria) in soil which is generally incapable of producing malaria in such quantities as to give infectious qualities to the local atmosphere," and to show how slight a cause sometimes may give rise to malaria, he writes as follows :—"The custom of keeping a number of plants in heated rooms may become the determining cause of malarial infection, even in localities where malaria is unknown." This fact was noticed also by another authority, Professor Eduard von Eichwald, of St. Petersburg, who mentions a case where flower pots placed in a heated drawing-room were actually demonstrated to have caused ague in a lady. Now all these facts are important inasmuch as with them before us it is not difficult to understand how fever should have developed during the days of "dry digging." During summer Kimberley is well known to be visited by severe thunderstorms. This being so there is nothing incomprehensible in the development of malaria under the then existing conditions. The whole population in those days was engaged in digging. The "fresh earth" from the mine, which at that time was not very deep, and directly exposed in all its parts to the sun's rays, must often have been moistened quite sufficiently by these thunderstorms for malaria to develop and create epidemics even of considerable severity. Moreover if so moderate a moisture and so apparently trivial a cause as a flower pot in a room can create an attack of fever, then in summer the fresh earth turned up in the confined tents in which people then lived, moistened by rain, must often have been sufficient to induce fever in susceptible individuals. Indeed it seems comprehensible to me that there might have been in the early days of the Fields even more general epidemics than at present; now-a-days many at Kimberley have occupations which reduce the risk of infection to a minimum. In the early days everybody almost was a digger, and as such was constantly in the mine and exposed to risk. All these considerations have practical bearings. If I am correct in what I have said, then clearly correct and definite data will exist for basing our attempts to prevent the periodical visitations that Kimberley has up to this been exposed to. Already I have taken up more time than I should have done, and it is not my intention consequently to enter into any great discussion of the means that have suggested themselves to me for limiting and preventing the fever, or of the curative measures that I have found of most avail. These, perhaps, I may be permitted to communicate at a future date. For the present, let it suffice for me to suggest that recognising the conditions I have laid down as necessary for the development of malaria, preventive measures should be based on methods that might

* On the Generation of Malaria in Flower Pots. *Practitioner*, vol. xxvii, page 387.

antagonise these conditions. Knowing, for example, that free exposure of the "fresh moist earth" to air and sun is one condition under which malaria might develop, means should be adopted to prevent the air and sun from getting at this earth more than is strictly necessary, such means as *e.g.* : The shade and protection of trees, and the planting of grass on all available patches of ground. Knowing that proximity to the depositing floors and wash-up heaps means proximity to infecting centres, some way of running these floors away from the populated parts of Kimberley might be resorted to. The directions of winds might be studied, and risk reduced to a minimum from germs raised from infecting centres by these winds, and carried by them to inhabited parts ; or when the railway is once as far as Kimberley, families might live away from Kimberley at Riverton, &c., and so be exposed to less danger. As I have said, however, it is not my intention to discuss this aspect of the question just now.

In conclusion, I may be allowed to express a hope that my subject may have been considered of sufficient interest to the members of the Society to justify my having brought it before them. In dealing with it I have tried to keep before myself the fact that I was not addressing a medical audience pure and simple, and I have attempted to develop it consequently more from a hygienistic than medical standpoint. I sincerely hope that I may be regarded as having succeeded in this, and that I have enlisted by this attempt the sympathy of non-medical as well as medical members.