REMARKS

ON

THE "MUSCULAR SENSE", AND ON THE
PHYSIOLOGY OF THINKING.*

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How far are these views in accordance with anatomical and pathological data? We know that in cases of locomotor ataxy, where, as we have seen, the "muscular sense" seems to be frequently impaired, the stress of the pathological lesions falls upon the posterior columns of the cord; and, to a minor extent, as Lockhart Clarke has shown, upon the posterior grey cornua, and the posterior roots of the spinal nerves. Then the posterior columns of the cord are now believed, on good anatomical and pathological evidence, to contain at least two sets of fibres—one kind passing from any given part of the spinal cord to various other parts above and below (these constituting the commissural fibres concerned in spinal co-ordination), and another kind passing upwards from the different regions of the cord to some parts of the encephalon, which have not yet been fully determined.† Now we know also from experimental evidence that these fibres of the posterior columns do not convey impressions to the brain which reveal themselves in consciousness; so that I think it very possible that the latter kind of fibres may transmit to the encephalon the unconscious impressions of the "muscular sense." And, having regard to the experimental evidence adduced in the early part of this paper, showing the effects of section of the posterior roots of the spinal nerves, I am disposed to think that these fibres have not previously emanated from the muscles or passed through the roots of the spinal nerves either anterior or posterior, but that they have proceeded more or less directly from posterior prolongations of the ganglion-cells in the anterior or motor regions of the cord. There must, of course, be some constant proportional relationship existing between the cerebral volition, the changes in the intervening spinal motor cells, and the functional activity of the particular muscles which are excited by these latter; therefore, it seems quite possible that impressions produced by changes in the spinal motor cells might well convey to the brain symbolic notions as to the functionally related changes occurring in the muscles themselves. Thus I assume it to be possible that, when molecular changes are excited in certain spinal motor cells as a result of a volitional impulse, proportional recurrent impressions may be carried along certain fibres taking origin from the motor cells, and ascending in the posterior columns of the cord; so that there may be conveyed to the brain definite impressions (not revealing themselves in consciousness) referrible to the changes induced in the state of the various muscles of a limb.

Now, then, it is easy for me to answer the question which immediately preceded this inquiry into the nature of the "muscular sense." We cannot recall ideally the impressions derivable through the "muscular sense", for the simple reasons that such impressions, in their actuality, never did reveal themselves in consciousness. Therefore, also, I am bound to come to the still further conclusion that, if words be ever revived primarily as "suppressed articulations", the only form in which these could be recoverable as such in idea would be as remembered feelings of the movements of our organs of articulation. How vague and comparatively indefinite these are, any thoughtful person will soon be able to satisfy himself; and it would certainly seem that such vague sensations would be very poor and shadowy substitutes for the definite and readily recoverable symbols with which auditory and visual impressions supply us.

Having arrived at these conclusions from a consideration of physiological facts, and from a careful examination of our conscious states, we

are in a better position for proceeding with our special inquiry into the physiology of thinking, with the view of explaining various phenomena met with in the "Aphasic" and other allied conditions.

I have elsewhere (*Fortnightly Review*, Jan. 1869; "Art. On Physiology of Thinking") pointed out that, as the first stage in the acquirement of language during infant-life, "we find the mere association of a particular sound with a particular set of attributes belonging to some external object, and this association becoming one of considerable constancy, so that the name invariably recalls to the child's mind the idea of the object; and the presentation of the object as promptly calls up the memory of the sound with which it has become mentally associated. Here, therefore, a *remembered sound*, or a perceptive impression revived in those portions of the cerebral hemispheres which are in remote connection with the ganglionic centres of the auditory nerve, must have been the symbols of thought made use of by the child if language of any kind entered into its thinking processes. But afterwards, as we have seen, when the child begins to make articulatory efforts, a secondary association is gradually established; viz., one between the remembered sound of the object, and the articulatory effort which the child makes in order to utter this sound for itself." To this was added the following sentence:—"Let it be granted that after a time this secondary association becomes as perfect and recurs as easily as the remembered sound, which at first was the only linguistic symbol, then the question arises: Do we, when we are, as it is termed, thinking to ourselves, still make use of the old and original remembered sounds of words as the vehicle for our thoughts—our mental symbols? or do we cast these aside, get at primarily, and make use of as our vehicle, the remembered acts of articulation necessary for the pronunciation of the said sounds?" Professor Bain, in commenting upon these views (*Fortnightly Review*, April 1869, p. 494), whilst he in part admits the truth of what is said concerning the period in the child's life previous to the ability to articulate—that at this time "the ideas of language must consist solely of sounds"—then goes on to say, although it is perfectly evident that certain name-sounds do recall to the child's mind the objects with which these have been previously associated: "Dr. Bastian too hastily assumes the inverted operation—objects recalling names a possible process, no doubt implied in the fact of association, yet at that stage serving no end, and giving no evidence of its existence. There is neither proof nor probability that the child indulges in recollections of sounds by themselves; trains of auditory ideas ending in nothing. None of its operations require such trains; and there is the greatest unlikelihood that they are indulged in previous to the stage when they can be reproduced in speech, or become a guide to action." It seems to me somewhat surprising that Professor Bain should have made these remarks. It can scarcely be questioned that, in the great majority of cases at least, where an association, such as I have assumed, does exist between certain sensory impressions—between impressions of sight and impressions of hearing, for instance—such an association is as perfect in the one direction as in the other; that the visual impression is as capable of recalling the auditory, as the auditory impression is of summoning up the visual. I was, therefore, perfectly justified, in the absence of evidence to the contrary, in assuming that the same equal relationship existed between the sights and sounds impressing themselves upon the infant's organism. If Professor Bain really thinks that such is not the case, he is wishing to establish an exception to a general rule, and therefore it lies with him to adduce the evidence in support of his opinions. When he says "there is neither proof nor probability that the child indulges in recollections of sounds by themselves", I would ask him whether there is also no proof or probability that the child indulges in recollections of sights by themselves: there certainly is just as much ground for denying the one as for denying the other; and if we deny the one we may deny them both, and thus theoretically reduce the mental life of the child which is still unable to articulate to the lowest possible level—to a hypothetical level, in fact, the utter erroneousness of which cannot fail to strike those who have watched young children in an unprejudiced manner. But as a matter of fact, I never did suppose that the child indulged in "recollections of sounds by themselves, trains of auditory ideas ending in nothing": I expressly stated, "It would seem pretty obvious that, so far as the infant thinks by means of language, it does so by the *remembered sounds* of words; these are its linguistic symbols of thought, which must, however, be mixed up inextricably in its mind with other sense-impressions, and more especially with those of sight" (p. 60). Professor Bain then goes on to state that none of the child's operations require such trains of auditory ideas; but, with the understanding just mentioned, that in the immature mind of the child such trains are probably even more intermixed with visual remembrances than they are in the adult, we are bound to admit that, if the child can think at all, however simply, it must necessarily make use in its thought of these very trains of which Professor Bain says it has no need.

* Concluded from page 463 of last number.

† See a paper on Secondary Degenerations of the Spinal Cord, by the author, in *Med.-Chir. Transact.*, 1867.

When we now come to consider the later periods—the time when the child begins to speak in articulate language, and has acquired the power—we have to return to the question which I have formerly stated: “Do words recur to the mind engaged in silent thought in their passive or in their active forms, as remembered impressions derived through the auditory ganglia, or as remembered combinations of muscular movement?” I have been induced to answer this question in favour of the passive forms of sensibility. I think words are primarily revived in the cerebral hemispheres as remembered sounds; that these afterwards, in the case of speaking aloud, automatically incite the necessary combined movements for the acts of articulation, but that a remembrance of molecular movements never does intervene as the idea of speech.* I have come to these conclusions for the following reasons.

1. I have just shown that the details of articulatory acts are never revealed in consciousness, and, therefore, obviously they could not be capable of ideal recall. All that we are conscious of is certain more or less vague and ill-defined feelings, resulting from the mere movements of our organs of articulation. These, therefore, are the only forms under which articulation can be revived in thought. The so-called “active” modes of sensation have in reality no existence as conscious impressions.

2. Then, if we turn our attention to the phenomena presented in a well marked case of Aphasia, it will be found that very valuable evidence is supplied in proof of the views which I have been advocating. We do commonly find aphasic individuals suffering from no paralysis of articulation, and unable to speak more than perhaps one or two words, who are yet able to think quite well, and whose mental faculties do not seem appreciably impaired. If such an individual think to a great extent by means of remembered articulations, how would it be possible, when his power of accomplishing the actuality was abolished, that he could still reproduce a word ideally, if this reproduction depended, as Professor Bain thinks, upon the action of the motor nerves, through the medium of the “muscular sense”? He has himself said: “It is not very easy to shew, on the other hand, that a series of actions can be repeated mentally and not bodily; for, as the mental actions are performed in the same circles, it usually needs only a volition, often the removal of a restraint, merely to bring them to the full length of actuating the muscles.” (*Senses and Intellect*, third ed., p. 346.) But I have also shown it to be almost impossible that such an ideal recall of impressions could occur alone, and without the muscular actions of which, in Professor Bain's view, they were the concomitants. The facts of aphasia, therefore, tend still further to strengthen the view which I have been advocating, as I think Professor Bain must perforce admit; so that we are bound to believe that aphasic individuals, like other people who are not deaf, must either revive verbal symbols (1) out of the auditory perceptive centres, or else (2) they must employ such indefinite counters as the mere remembered passive sensations of the movements necessary for the articulation of different words. I can only say that, in my opinion, influenced by the considerations which follow, the choice is not a doubtful one between these two possible methods of verbal recall.

3. Bearing in mind, therefore, what alone can be the real nature of an “articulate recollection” such as Professor Bain speaks of, I think it will not be difficult to show, in the first place, that what he considers to be the purest exercise of this sort of recollection is not such in reality. Professor Bain says (*Fortnightly Review*, April 1869, p. 496):—“I will

now give the instances that seem more or less to exclude auditory ideas, and to throw the stress of the acquisition on the articulate ideas. There are some strong facts of this character. Take the case of the child conning its lessons without being allowed to speak aloud. Here the only mental effects allowed, in the first instance at least, are upon the eye and upon the articulating organs. What, then, is the actual course described? The child looks at the sentence to be committed to memory, and then repeats it in a suppressed articulation: if there be a break, it refers to the book, and again repeats articulately till the string of words becomes coherent. Now, in such circumstances, one would say that the coherence takes place either in the visual circles or in the articulate, or most probably in both.” Professor Bain also adduces the case of the “student who derives his knowledge from books not read aloud”; but, he says, “still more decisive than these is the fact that the deaf have been taught to speak.” These, it appears, are the strongest cases which Professor Bain can adduce as tending to throw the stress of our acquisition of words upon remembered articulations. And it is especially worthy of note here that, in the first two instances cited, even Professor Bain is obliged to admit that the visual impressions have a very great deal to do with the assimilation of the words. So far, I entirely agree with him; only I think that in these very instances the auditory perceptive centres would have been also called into play; whilst he says there is “neither proof nor presumption” that they would be. The boy who merely cons his lesson-book without speaking aloud, receives in his visual centres certain impressions which are then translated into Perceptions of the words seen; and one almost certain result of this elaboration would be, as I maintain, the revival of activity in certain functionally related cells of the auditory perceptive centres.* In fact, the boy is now, for the most part, only reversing the association by which he was taught the meaning of the visual symbols. And in the case of words with which he has been previously unacquainted, it seems to me almost certain that he would have to conceive some suitable sound-combination to represent them; and that then he would subsequently make his articulatory efforts in accordance with the sound-conceptions. A close investigation of actual and recorded cases of “aphasia” has convinced me that many of the phenomena which they present are explicable only upon the assumption that, in the state of health, the most complete interaction and functional relationship exist between the visual and the auditory perceptive centres, but that the acts of speech are prompted almost entirely by incitations emanating from the latter. As evidence, I would call especial attention to the case of Dr. Hertz, quoted in one of my previous papers (*Brit. and For. Med.-Chir. Rev.*, Jan., p. 219; and see also April No., pp. 481-487 inclusive), though it must be confessed that a consideration of this case, and more especially of that recorded by Dr. Hunn, which I have quoted in the next page, seems to justify the opinion that the motor stimuli inciting the acts of articulation during the process of *reading aloud* may pass off directly from the visual centre.

But the instances which Professor Bain has cited, although in his view they might be conceived to show that auditory impressions had nothing to do with the recall, do not in the least establish what he wishes to prove—namely, that we may recollect mere trains of articulations only.† The instances mentioned would simply go to establish, if his view were correct, that articulations in these cases were automatically incited by revived visual rather than by auditory impressions—a view which I should be much more inclined to admit, than that we recollected words as remembered articulatory movements simply.

And I will now endeavour to show, that in fact, in the case of the deaf who are taught to speak—the instances which Professor Bain thinks most confirmatory of his theory—precisely such a relation between visual signs and articulatory acts does exist as in ordinary individuals

* This was the definite position that I endeavoured to establish in my previous paper; it did not really concern me to show whether in Professor Bain's opinion “suppressed articulations” formed our only “ideas” of speech. I thought they never did occur as “ideas” of speech; and hence I was quite justified in criticising a passage in Professor Bain's book, in which he put this mode of revival forward as a prominent one. His charge of unfairness therefore was, I think, wholly uncalled for; and I can assure him that I did not quote the passage referred to in order to “fasten upon him the opinion that there was no other form of verbal recollection but the articulations.” I was criticising the most prominent statement which Professor Bain had made upon the subject—one which has stood in all three editions of his work, and which has been emphasised by the use of italics; it was sufficient for me therefore to show, in that paper, that I believed such a mode of revival impossible. The misrepresentation is, I think, on his side. In order to convince his readers that I had been dealing unfairly with him, he puts prominently forward (*Fortnightly Review*, April 1869, p. 493) a passage in which he does express opposite views; and although he says he has “uniformly expressed this opinion,” the passage exists only in his recently published third edition, whilst the other which I quoted had existed in all three editions. Then with the effect, at all events, of apparently still further convicting me of misrepresentation, Professor Bain employs italics to emphasise this recently introduced passage (without saying that they do not exist in his book), and gives only an imperfect and maimed reproduction of the passage I had quoted, and now suppresses the italics by which he had always emphasised this statement in his work. Thus altogether an erroneous impression is conveyed; and, worse still, there is an imputation rather than a confession of unfairness. Of course, I am glad to find from Professor Bain that there is more agreement between us than I had imagined from my study of the earlier editions of his work, though the statements at present existing in the third edition are somewhat contradictory and confusing.

* The boy would almost certainly derive an advantage from articulating the words of his lessons, instead of merely conning them. He thus supplements the visual impressions by conscious auditory impressions, and at the same time allows these sense impressions to take a firmer root, as it were, in his system, by strengthening the organic connections between his auditory and visual perceptive centres and the parts upon which the motor acts depend. But because articulation of some kind is so generally associated with reading, and because after we have once acquired the faculty of speech, we do usually embody our thoughts in motor acts, and not stop at the mere primary sense symbols, it does not seem to me so wonderful that we should be unable to remember “a passage read, either purely as a printed page seen by the eye, or purely as suggested ideas of how it would sound if pronounced.” But, notwithstanding this, under what form does Professor Bain think Aphasics are enabled to recollect what they have heard or read?

† In his article (*loc. cit.*) at p. 495, Professor Bain is contending with a shadow. He seems to suppose that I have denied the possibility of an adhesion between muscular acts—a possibility which I never doubted in the least. I only denied that we were conscious of all the details of muscular action by which an utterance was effected; of course, in the articulate utterance of any moderately long word, I know that cell-connections permitting “truly muscular adhesive groupings” must have been formed. But that is a very different thing from believing that whole strings of these combinations can, as it were, call themselves into being, and ideally constitute our verbal symbols.

obtains between auditory signs and articulatory acts; so that these cases, instead of favouring his view, are really strongly confirmatory of mine. The deaf having lost the use of their sense of hearing, the sense of sight takes its place; so that in them revived sight-impressions are the verbal symbols which automatically incite the muscles of articulation, as they have previously done those of the hands and fingers. I have lately visited the London Asylum for the Deaf and Dumb, and, through the courtesy of the Principal, Mr. James H. Watson, I was made acquainted with the method of tuition, and heard many very interesting results of it. The pupil must, as a preliminary, be able to interchange ideas with his teacher by means of the finger-language; and the pupil is also mostly able to write himself, and to read the writing of others. There are perfect means of communication, therefore, through visual symbols. The pupil understands that he is to be taught an additional mode of communicating his own thoughts, and of receiving those of other people. In order to utter a given sound, he is instructed (through the sense of sight, by watching his teacher) how to place his organs of articulation, lips, tongue, etc., in order himself to produce such a sound, which, he is led to understand, corresponds with a certain previously known and well understood visual symbol. This is the essence of the process—the building up of new combinations between known and commonly used visual impressions and certain muscular acts which the pupil is taught to consider as the equivalents of these. The pupil's education is carried on, of course, on *phonetic* principles; so that he has to be instructed that, although certain words are spelt quite differently, and have different meanings, they must be articulated in precisely the same way. In this manner the education is slowly and laboriously proceeded with, until the most perfect adhesion exists between the several visual symbols with which the deaf child thinks and the muscular acts necessary for their articulation. The education, however, is much slower and more laborious than that of an ordinary child, where articulatory acts are incited by auditory impressions. Any why is this so? Simply because the child lacks the auditory sense which would enable him easily to modulate or correct the sounds produced, and possesses instead, as his only means of knowing what he has achieved and of guidance, nothing but those much more vague and indefinite feelings of movement emanating from his articulatory organs. Thus the difference in facility of educating a deaf child and of educating one who is equally intelligent, but not deaf, comes to be a sort of gauge of the relative superiority of auditory impressions over these other more indefinite symbols derived from the mere feelings of movement.

Then a very convincing reply can be made also, from an observation of deaf persons, to the argument adduced by Professor Bain, to the effect that, because in thinking we do often mutter or make use of suppressed articulations, this is of itself an evidence that such articulation, ideal or actual, is an essential part of the process. The reply is this: Where, as in the case of the deaf, the mental symbol is more dissociated from the muscular act destined to give the word a physical expression (because the two are not so frequently and so habitually conjoined), then the processes of thought do seem to go on entirely by means of the visual symbols, and it is quite a rare thing for the mere thought to give rise unconsciously to either of its accustomed physical manifestations: it is neither associated with finger-movements nor with muttered articulations. This is perfectly in accordance with opinions which I have previously expressed concerning the significance of the suppressed articulations which occasionally accompany thought, and of the finger-movements that were seen during dreams in the case of Laura Bridgeman. These are to be regarded as accidental, and not as necessary accompaniments.

Thus Professor Bain's most decisive example in favour of "articulate" modes of verbal recollection in reality affords no proof of his hypothesis whatever—visual* symbols simply take the place of audi-

tory symbols, and the articulations themselves are still secondary and automatic.

But yet another argument against the view that words are recalled and retained in memory as "suppressed articulations" is to be drawn from the case of the deaf. It is now a well known and undisputed fact that, if a child of five or six years old become absolutely deaf, it soon loses the power of speaking altogether, however good the power of speech may have been before. Mr. Watson assured me that such a result would inevitably follow, if no special education were resorted to. And then the child is reduced practically to the same condition as the child who has been deaf from birth, and he can be re-instructed to speak only in precisely the same way as the congenitally deaf child. Now this fact is quite in harmony with my view of the physiology of speech; but it does not seem very easy to make it accord with the doctrine that words are remembered even in part as articulations. The capacity for receiving impressions of the "feelings of movement" remains unimpaired; these must, moreover, be capable of ideal recall as much as they ever were; and yet the child gradually ceases to talk, precisely as it would do if these feelings were not at all concerned in the revival of the verbal symbols. And then it cannot be supposed that this gradually diminishing power of speech is owing to the lack of guidance in the acts of articulation usually derivable through the ear; because, when these same individuals have afterwards been instructed to build up their articulatory efforts upon sense-stimuli of a different kind—those of sight—they do succeed in speaking through the sole guidance of these feelings, which would have been supposed to be inadequate for the purpose.

Such are the reasons against the presumed articulate mode of recurrence of verbal symbols; and I will now give those which, I think, tend to strengthen the supposition that words are recalled in thought as revived auditory symbols.

4. Auditory impressions do possess in the highest degree those characters of definiteness and intellectual retentiveness which would eminently fit them for such a function; whilst the very reverse is to be said concerning our mere feelings derived from articulatory movements.

5. Then I think it is another argument in favour of the same view, that, if we suppose the auditory impressions to be the forms under which words are revived for thought, we do suppose the continuance of a method which alone was possible (so far as words are concerned) during the first twelve months of the child's life. I am more inclined to believe that fresh acquirements after this date would take the form of additional and superadded associations, rather than that the early modes of thought should be reversed or superseded. I shall certainly not be dissuaded from this view, if Professor Bain is unable to find any better argument against it than the stigmatising of such a continuance as an instance of "blind conservatism".

6. Lastly, it is no argument against the views I have been advocating, to say that we have, ordinarily, no *consciousness* of these auditory impressions which I presume to be evolved.* If we analyse carefully what takes place when a person *improvises* music on a piano, and consider all the previous education which has been gone through, we shall be almost bound to come to the conclusion, as I have elsewhere stated, that, "by dint of long practice and habit, there must have grown up in the person that sort of knowledge, which now seems intuitive, as to how, when, and in what manner, to touch the various notes, so as to make the instrument produce the desired sounds. These muscular movements follow as rapidly, and with as much precision and delicate adjustment, as do the muscular movements of the larynx, lips, and tongue, in the acts of articulate speech. Both alike follow automatically; and both sets of muscular movements, as it seems to me, are set going by the agency of revived impressions in the perceptive centres pertaining to the sense of hearing; so that, as quickly as the person improvising music conjures up in imagination the idea of the several parts composing the piece, this translates itself into the suitable muscular movements necessary for educing the sounds; the different steps of the process following so rapidly upon one another, that the performer himself is perhaps conscious of giving no separate attention to the ideas of the several sounds as they become nascent, and which are so immediately translated into muscular movements of the greatest precision. Still it would seem that such a succession as this must occur—that the cerebral hemispheres must be necessary for such a process; and yet here, too, there is involved a reflex action precisely analogous to that occurring in the process of speaking. If it can occur in the one case, therefore, it can occur in the other. Here, however, the ideal sound-combination is, so to speak, the subject of thought itself, and therefore should arouse consciousness more as its successive parts become nascent and act as stimuli, inciting to the rapid and precise secondarily automatic move-

* After the views which I have already expressed, it is almost needless for me to say that I do not accept Professor Bain's views concerning the sense of sight. He says:—"In all that regards visible movement and visible form, the muscular consciousness, it is now contended, is the indispensable element; the optical sensations merely guiding the movements." Naked outlines, as the diagrams of Euclid and the alphabetical characters, are, to say the least of it, three parts muscular and one part optical; their retention is supposed to depend upon the adhesive property of the ocular muscles and their nerve-centres, and not upon purely optical circles." In his larger work, Professor Bain has fully expounded these views. I think he lays far too much stress upon this "muscular consciousness"; that this exists only to a very small extent; and that the impressions so derived are passive, and not "active," as he supposes. Much of the information which he attributes to "muscular consciousness," is, I think, only mediated derived through the activity of the muscles. The activity of the ocular muscles does enable us to derive notably more varied sense-impressions of a purely optical character; and all these, more or less associated with vague passive sensations, emanating directly from the muscles, and knit together by the multitude of inferences which we learn to draw instinctively during the education of this sense, give us, I think, that knowledge which Professor Bain seems to suppose we derive directly through our "muscular consciousness".

* See Professor Bain's paper in *Fortnightly Review*, p. 497. Does Professor Bain know that Aphasics can add, subtract, and multiply, without articulating?

ments performed by the hands and fingers.* If, however, in these cases, the several parts of the ideal sound-combination do cause such a slight and transient mental impression as to be almost unnoticed as distinct phases of consciousness, can it be wondered at that, in the process of speech, there should be so little evidence of the consciousness of words becoming nascent as remembered sounds?"

THE SOUDAN:

HALTING-PLACES; SICKNESS BREAKS OUT; TREATMENT EMPLOYED; QUININE AND BRANDY; METEMMA; BALWAHA.

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PART IV.

THE selection of the halting-ground for the night is also of some importance. We preferred, when practicable, some rising ground—at all events, a clear, open space, where the grass was dry or burnt, free from trees, and at some distance from valleys and swamps. We never halted for the night on the banks of the few streams we crossed; and, although during the day-time we enjoyed immensely the cool shade of the well-wooded valleys of the Anseba, of the Gasch, and of the Atbara, at sun-set we moved our camp to some distance, afraid of sleeping at night too near those charming but dangerous localities. On a few occasions, when necessity obliged us to halt on some low, damp, unhealthy ground, after a few hours' rest we started in advance, allowing the camels to follow us. On other occasions, when wood was procurable, several large fires were kindled around our camp, and kept burning all night by the followers, who watched and slept in turns. This practice of night-fires is very extensively followed by the natives—not so much as a precaution against diseases, as to keep off the lions, very numerous and very daring; nevertheless, although the motive is somewhat different, the results are, all the same, favourable, and protect many from the evil effects of cold damp nights.

We avoided the midday sun as far as possible: at all events, we always protected ourselves, to the best of our ability, by following the rules which I described in the first chapter. I have explained our usual daily mode, our life, the care we took concerning the water, the precautions we adopted at night, etc. I believe that any one electing the same course, and following the same plan, will reap, under equally dangerous circumstances, the same immunity and benefits. A last recommendation before I conclude this part of my subject. Now and then, especially when our morning march led us through jungles or valleys, we took a grain or two of quinine in a wine-glass of sherry; and every third or fourth day we made our Indian servants drink half a cup of black coffee, with a few grains of quinine dissolved in it.

Up to Kassala, about half our journey, we had in our camp hardly any sickness at all. Strict orders had been given to our servants and followers not to delay, but to come to me at once on the slightest indisposition. Fear had made the order generally obeyed; and, in consequence, a few timely remedies had averted, in several cases, severe illness. But, after leaving Kassala,—with our forced halt of several days in that most unhealthy of cities, and our losing our way in the dark the evening we left that place, and having, in consequence, to encamp on a kind of island in the bed of the river Gasch, where we were overtaken by a severe storm of rain, together with the damp and green prairies we had to cross, and the greater scarcity in some places, and still more inferior quality in others, of the water—all these circumstances combined to bring on sickness: fever, diarrhoea, and dysentery made their appearance; and, before we reached Metemma, almost all our servants and followers suffered more or less from one or the other of these diseases.

On a quick march like ours, sick or well, every one must keep up with the caravan. For many, many miles, not even a roving Bedouin is to be met with, and we had no option but to carry our sick with us. This was no easy matter; we had only a few spare mules; all the camels were laden, and the camelers were very adverse to have any extra weight placed upon their beasts; but, as necessity left us no alternative, we had to make them submit, and consent to allow the sick to ride on the top of the baggage and boxes. A sharp attack of dysentery, a burning fever, are evils painful enough in themselves when the patient is surrounded by the best of comforts; what, then, must have

been the sufferings of those poor wretches, dangling backwards and forwards on the top of a camel, exposed to the sun by day, to the damp by night, often having to ride in that fatiguing position from 3 P.M. to 5 A.M., with, perhaps, in all that time, but an hour's rest! But we could not help ourselves. As we advanced, water daily became more scarce; and, even once or twice, we found none at all, and had to push on as fast as we could, contenting ourselves, sick and all, with the little turbid, nauseous fluid we carried with us.

For the medical man, every one will understand me, the difficulty was indeed great. I had to contend not only against severe and fatal sickness, but under circumstances that seemed almost to paralyse any efforts of mine. However, I did my best to make the poor sufferers as comfortable as possible; availed myself of every halt, and adopted an energetic treatment, followed, I am happy to say, with success in every case. The majority of the fever cases belonged to the simple remittent type. In a few instances, it affected a more continuous and serious form, accompanied by adynamic and ataxic symptoms, whilst in the simplest cases the fever assumed a mild intermittent type.

The fever began usually by severe aching pains in the limbs, headache, and prostration, soon followed by a more or less lasting rigor, the reaction (the intensity of which was in relation to the severity of the first symptoms) being more or less violent and persisting. The treatment which I adopted was the following. All, before starting, took a dose of quinine, varying, according to cases, from six to ten grains. At the halt, during the heat of the day, a mild emetic was given to those presenting a coated tongue—a small dose of castor-oil in such cases where the bowels did not act properly. When water was procurable, the heat of the body was reduced, and the skin's action increased by cold packing. Beef-tea or arrow-root, with port wine, was allowed to every one twice a day. In the evening, before starting, all received an ounce of brandy and another dose of quinine; and, when the night-march was long, another half-ounce of brandy, in cold tea, was given to the most debilitated. In a few severe cases, where the tongue presented a rough, red, dry appearance, a blister was applied on the epigastrium. The cry for water was indeed painful to hear; but I would not give in, and only allowed to each patient a couple of bottles of cold tea or rice-water, as the case might be; carried on the shady side of the camel; and strict orders were given to those who accompanied the sick, never to allow them to drink from any of the pools or wells they might come across.

Every one of them eventually recovered; and, by the time we reached Metemma, only three of the worst cases remained on the sick-list. After a few days' rest and a continuation of the former treatment, aided in its effects by a better diet, these also quickly improved; and, as all were only weakened by sickness, and not also by the treatment, the convalescence was, in every case, short, and with but few relapses; and, before many days after our arrival at Metemma, the whole party was in excellent health, and in good working order.

Diarrhoea, in some cases, was rather severe, accompanied by much griping, and with a tendency to pass almost at once into dysentery; in those who came at once to me, a grain or two of opium in half-an-ounce of brandy, or some thirty minims of chlorodyne in brandy, never failed to check, and soon to stop entirely, the disease. When mucus or streaks of blood had already made their appearance in the stools, or even when dysentery had actually declared itself, I adopted the following treatment. At the first halt, a mustard-plaster was applied for half-an-hour on the epigastrium, and twenty-five minims of tincture of opium were given at the same time; half-an-hour afterwards, from forty to fifty grains of ipecacuanha were administered in one dose. The result in every instance was so satisfactory that it never required a second application of the same treatment to modify entirely the character of the disease. In the evening, chlorodyne and brandy were given, as in the simple cases, to check the little diarrhoea that remained. In every case of diarrhoea or dysentery, beef-tea or arrow-root, with port wine, was given once or twice a day; and at night, or before sunrise, all received a small dose of quinine and brandy. In a few cases, where for several days afterwards a little mucus was still to be seen surrounding the faecal matters, an enema or two of simple cold water were sufficient to complete the cure. All the cases of diarrhoea and dysentery recovered even before we reached Metemma.

If now I sum up the medical portion of our journey through the Soudan, I believe I can advance that it is the only instance known of such a large party having performed that long and dangerous journey without the loss of a single life, and what was, on a mission like ours, almost as good, without having a host of invalids and broken-down followers to drag along or dismiss just when they began, by having learnt our habits, to make themselves useful. For awhile, our many precautions completely baffled sickness; and when at last it fell upon our party, situated although we were in the very midst of the most

* More than in the case of speech, where the words, when revived in the auditory centres, only come up as the symbols of thought, and would, therefore, naturally occupy an even smaller amount of attention, inasmuch as the person is engrossed with the thought itself. But in the case of the person improvising music, the several ideal sounds are at once both thought and symbol.