is stated, "The absorption of oxygen, and the separation of a portion of carbon, (or carbonic acid,) are, therefore, the causes to which arterial blood owes its property of being the *sole stimulus of living structures*. Venous blood, which has not undergone this change, has a *poisonous action on the organs of the body, particularly on the nervous system, and annihilates their irritability*, its action being similar to that of *carbonic acid, sulphuretted hydrogen, carburetted hydrogen*, and some other gases, by which the *irritability of the organs of the body is destroyed*, and by most of which the *arterial blood is darkened in colour*." At page 345, "It appears that the quantity of gas contained in the blood amounts in the mean to 1-10th, and sometimes is as much as 1-8th of the volume of the blood itself; that the *oxygen in venous blood equals, at most, 1-4th, and often only 1-5th of the carbonic acid which the same blood contains*; while, in the *arterial blood, the oxygen equals, at least, 1-3rd, and almost one-half of the quantity of carbonic acid*."

Andral, Gavarret, Simon, and Dr. G. O. Rees, informs us, as the result of numerous experiments, that the blood in typhus fever is found to be very deficient in fibrin and albumen; it coagulates imperfectly, and often remains in a semi-fluid state; the clot is soft, friable, of a very dark, almost black-red colour, and is very rarely covered with a buffy coat, and very soon becomes putrid; and in the latter stages, the quantity of blood-corpuscles and solid contents decrease remarkably rapidly. In thoroughly-developed typhus, Dr. Armstrong found the blood of the temporal artery as dark as that of a vein; Dr. Stevens found the chloride of sodium in typhus blood very sensibly diminished; and Dr. Clanny found all the salts diminished.

It is well known that the atmosphere cannot be breathed by man with a larger proportion of carburetted hydrogen in it, than one part in ten; the proportion of *carbonic acid gas* in atmospheric air is never more than one part in a thousand; and that inhalation of sulphuretted hydrogen gas would convert the per-oxide of iron of the red globules of the blood (the oxygen carriers,) into a black sulphuret of iron; and, at once, prevent the change from venous to arterial blood.

What is malaria? It is the emanation from decomposed and decomposing vegetable and animal matter; carburetted, sulphuretted, and phosphuretted hydrogen gases with carbonic acid gas. The immediate effect of breathing an atmosphere charged with these gases, would be:—

1st. A diminished quantity of oxygen to combine with the carbon in the venous blood in the capillaries of the pulmonary artery; the blood would become loaded with carbon, and imperfect arterialization must be the consequence.

2nd. The brain and nervous system would be imperfectly stimulated by arterial blood; and as the quantity of carbon increased, the arterial would be converted into venous blood, or nearly so, which would have a poisonous action on the nervous system.

3rd. The blood circulating as arterial would not contain that quantity of free oxygen necessary to combine with the carbon and hydrogen of the metamorphosed tissues which it meets with in the general capillary

system, and therefore they could not be removed as they are in healthy circulation.

4th. The secretions would be stopped in consequence of the deteriorated quality of the blood, and loss of energy in the nervous system; and the excrementitious matter, instead of being excreted, would be absorbed and circulated in the blood.

We are all aware that every part where blood-vessels and nerves are to be found, is continually wearing away; and if the supply of oxygen, nitrogen, carbon, and hydrogen, be not constantly kept up, the individual must perish. He first loses his fat; then his muscles; and, lastly, the substance of his brain, followed by deranged intellect and death—just as we see in the advanced stage of fever. The seat of fever, then, appears to be in the capillaries; in this structure secretion, nutrition, waste and decay go on. As long as the *vital principle is greater than the chemical force*, health is the result. But when the *vital principle is deteriorated by a deficient supply of oxygen in respiration*, feverish action is the consequence.

The foregoing explanation may account for the cold or collapsed stage of fever very easily, which would terminate here, if the individual remained in so depraved an atmosphere as to be insufficient to supply oxygen enough to excite reaction. But it will be asked, how can the animal heat be so much increased as it is during the continuance of fever, if there is a deficient supply of oxygen. When *reaction* takes place, nature makes an effort to restore the normal state of the capillaries. Respiration and circulation are increased in speed; there is then a normal quantity of oxygen inhaled (if the atmosphere become pure,) with every inspiration, and the respiration and circulation are doubled; consequently the animal heat is increased nearly two-fold: The red blood-corpuscles, or oxygen carriers, again gradually resume their office; remove a portion of the decayed tissue; but, in consequence of the depraved secretion, and large quantity of carbon in the blood, it requires a certain time to overcome this derangement; and this time constitutes the *period* of the continuance of fever.

I would by no means wish to insinuate that this is the only mode in which fever is generated. It is often the consequence of animal poisons introduced through the capillaries to the nervous system; and many other causes of fever are too well known, and generally acknowledged, to require any notice here.

(To be continued.)

ON PLACENTA PRÆVIA.

(A paper read before the Medical Society of University College, London, on Friday, January 30th, 1846.)

By JOHN ELLIOT WOOD.

(Continued from page 85.)

Reflecting upon these circumstances, Mr. Kinder Wood, of Manchester, was induced to consider whether, in very urgent cases, we might not be justified in so far imitating nature as to extract the placenta before the fœtus, and by this means arrest the flow of blood. Writing in 1821, he says, "If we find so much exhaustion as to make us fear the effect of further

hæmorrhage, the first step would be to detach the placenta; by this means the hæmorrhage will be completely suppressed. The time required to separate the placenta is very short, and the loss of blood during the attempt exceedingly trifling. I have no hesitation in recommending that the placenta be separated completely, and the membranes ruptured, and that the hand be withdrawn immediately upon this being effected, leaving the child and placenta behind." Mr. Bryden, pupil of, and afterwards co-lecturer with, Dr. Radford, says that he has always been taught, and has himself insisted on, this practice in extreme cases. Mr. Wilson and Mr. Low confirm the fact that Mr. Wood taught and practised this method in 1822.* Dr. Radford, of Manchester, had a case in 1819, in which he removed the placenta, because it was hanging so low in the vagina, that no other plan could be pursued. He now recommends, in certain urgent cases, detachment of the placenta, as a preparatory means for the employment of galvanism in unavoidable uterine hæmorrhage; "for although," he says, "the practice of detaching the placenta may be a means of suppressing the bleeding, yet it will not restore the depressed powers of the woman, and on that account we still require an agent to induce such a degree of uterine contraction as will secure her from all chances of further hæmorrhage, while we have recourse to such measures as will tend to support her strength."† But it is to Dr. Simpson, of Edinburgh, that we are chiefly indebted for bringing the question of adopting or rejecting this practice most prominently before the profession; and it must be admitted that he has supported the practice he has espoused by sound reasoning and logical arguments. Thus much we may safely say, without compromising ourselves by giving an opinion as to the correctness of the premises from which Dr. Simpson's conclusions have been deduced.

It has always been admitted by the profession generally, that the operation of turning in placenta prævia is one of extreme danger both to the mother and child, especially perhaps to the former. Dr. Lee, in his lectures on this subject, in 1844, remarks, "*At the best it is a dangerous operation, and you can never tell with certainty whether or not the patient will recover after its performance, however easily it may be effected.*"‡ And although this same gentleman now denies that any great mortality exists in the usual method of treating placental presentation, we may fairly doubt whether this remarkable change of opinion is the effect of more extended observation and further reflection on the subject.

Dr. Simpson has collected one hundred and forty-one cases, in which the placenta was extracted before the child; and in these cases a remarkable fact is that nearly one out of every three children survived, or thirty-one per cent. were saved, and sixty-nine lost. After the complete detachment of the placenta, the hæmorrhage was totally arrested in a large majority of cases; it was not alarming in its extent in a great proportion of the remaining instances; and in five only out of the one hundred and forty-one labours, did it continue so profuse under the circumstances as to be

considered alarming by the attendant. Out of the five mothers one only died; the other four all recovered. On the other hand, out of three hundred and ninety-nine cases, collected by Dr. Simpson from various authors, in which the usual practice was adopted, in one hundred and fifteen the result was fatal to the mother—a mortality as great as that of malignant cholera in 1832-33, and twice as great as the average in cases of lithotomy. Or, to put the matter in another light, in order to convince Dr. Lee, who was still sceptical, Dr. Simpson remarks that Dr. Churchill collected with much care the histories of three hundred and seventy-one cases of Cæsarean section; out of these three hundred and seventy-one cases, two hundred and seventeen mothers recovered, and one hundred and fifty-four, or nearly one in every two and four-tenths, died. This is exactly, and to a fraction, the degree of maternal mortality accompanying turning in placental presentation in the cases recorded by Dr. Lee in his Clinical Midwifery. "When we see," continues Dr. Simpson, "the results of turning the child in placental presentation in the hands of such distinguished accoucheurs as Dr. Ramsbotham and Dr. Lee, what degree of success can we expect to follow it in the hands of the general mass of practitioners?"

He recommends, therefore, the artificial detachment of the placenta, when rupturing the membranes is insufficient, and turning is either inapplicable or unusually dangerous. As, for instance, when the os uteri is so undilated and undilatable as not to allow with safety of turning; in most primiparæ; in many of the cases in which placental presentations are connected with premature labour, and imperfect development of the cervix and os uteri; in labours supervening earlier than the seventh month; when the uterus is too contracted to allow of turning; when the pelvis or passages of the mother are organically contracted; in cases of such extreme exhaustion of the mother as forbid immediate turning or forced delivery; when the child is dead; and when it is premature and not visible." He continues, "I believe that in the above and similar cases, by the introduction of a finger, or of a common sound or bougie, the placenta might be readily and completely detached, the attendant bleeding in this way arrested, and the labour subsequently be allowed to proceed to a natural and safe termination, if it were a head or pelvic presentation; and if the child were placed transversely, a more safe and proper period could be waited for, and selected for the safe version of it."*

From these facts and reasonings he gives the following summary of results:—1. The complete separation and expulsion of the placenta in cases of unavoidable hæmorrhage is not so rare an occurrence as accoucheurs appear generally to believe.

2. It is not by any means so serious and dangerous a complication as might, *a priori*, be expected.

3. In nineteen out of twenty cases in which it has happened, the attendant hæmorrhage has either been at once arrested, or it has become so much diminished as not to be afterwards alarming.

4. The presence or absence of flooding after the complete separation of the placenta does not seem in any degree to be regulated by the duration of time

* *Provincial Medical and Surgical Journal*, Feb. 26, 1845.

† *Ibid.*, Dec. 24, 1844.

‡ Lectures p. 373.

* *Medical Gazette*, October 10, 1845.

intervening between the detachment of the placenta and the birth of the child.

5. In ten out of one hundred and forty-one cases, or in one out of fourteen, the mother died after the complete expulsion or extraction of the placenta before the child.

6. In seven or eight out of these ten casualties, the death of the mother seemed to have no connection with the complete detachment of the placenta, or with results arising directly from it; and if we do admit the remaining three cases, (which are doubtful,) as leading by this complication to a fatal termination, they would still only constitute a mortality from this complication of three in one hundred and forty, or one in forty-seven cases.

7. On the other hand, under the present established rules of practice, one hundred and thirty-four mothers died in three hundred and ninety-nine placental presentations, or about one in three.*

These statistics of Dr. Simpson's are disputed, however, by no less authorities than Dr. Ramsbotham and Dr. Lee. The former of these eminent practitioners states that, "Dr. Simpson's views are founded upon the singular and obvious fallacy, that because in a certain number of cases, (and those the most favourable for the ordinary treatment,) in which the placenta came away spontaneously before the child, and the woman recovered, the same good result should take place after the still organically adhering placenta has been torn away. This clearing away of the placenta was performed two hundred years ago by an ignorant and audacious impostor, on a lady who died in Paris, whose case is related with endorsements of the practice by Guillemeau."† Dr. Ramsbotham has omitted to inform us at what period after the operation this patient died, or indeed, whether she died from the effects of the operation at all.

Dr. Lee attacks these tables with still greater asperity. He says that, "there can now no longer be any difficulty in determining how much reliance ought to be placed on Dr. Simpson's statistics, and whether he has been sufficiently impressed with the importance of observing the most rigid accuracy, the most scrupulous regard to truth in forming statistical tables, from which it is proposed to adduce proofs in support of practice in the treatment of cases of such vital importance, and substitute another mode of treatment which is of the most dangerous character, fraught with inevitable destruction to the child, (?) and attended with no benefit to, if not actually increasing the danger to the mother."‡ He adds, that the cases of placental presentation he has met with are forty-five in number, and not forty-six as stated by Dr. Simpson; or if five cases of partial presentation be added, the whole number will be fifty. "Of this number, one woman died undelivered, and three others died, on whom the operation of turning was never performed. Of ten cases which terminated fatally after the operation of turning, three women died of uterine phlebitis long after delivery, a circumstance which could not justly be attributed to the operation. In a fourth case of turning, the uterus was lacerated, in consequence of a

great distortion of the pelvis. In a fifth, the patient was in articulo mortis before the operation was commenced. It is impossible to attribute the fatal result to the operation of turning in more than six out of these ten cases, and if the circumstances of these six be examined, it will be obvious that death could not have been averted by any other mode of treatment, and that if turning had not been practised, they would all have died undelivered."*

In spite of the positive nature of Dr. Lees' two last statements, we may be allowed to surmise that if in his fourth and fifth cases, the placenta had been detached, the result might possibly not have proved fatal; and from the facts already before us we are warranted in denying, in *toto* , the correctness of his concluding remark.

Proceeding to analyse these statements still further, he says that "in these fifty cases turning was performed only twenty-seven times, by myself in nineteen cases. Out of these nineteen there were four deaths; but one woman was moribund before the operation was undertaken; in a second, before I was called into consultation, so much blood had been lost that she died soon after delivery; a third, to whom a large quantity of ergot of rye had been given, died ten days afterwards of phlebitis; while the fourth case proved fatal in consequence of the great rigidity of the os uteri, which rendered turning impossible until she was in a state of complete insensibility from the loss of blood. The mortality, therefore, instead of being one in two and four-tenths, (as stated by Dr. Simpson,) is in reality less than one in nine. In the last sixteen cases of placental presentation which have come under my own immediate care, not one woman has died, though turning was performed in eight."

Judging simply from these tables of Dr. Lee's and Dr. Simpson's, without the opportunity of subjecting the former to that rigid scrutiny which the latter have undergone, it is evident that the mortality is much greater where the usual practice is pursued than where the placenta is detached, being in Dr. Lee's cases one in nine, and in those collected by Dr. Simpson one in forty-seven. Very many cases might be added to make this great difference still more remarkable.

Dr. Ashwell, another high authority, after premising that the mortality of one in three is too high, comments on the practice of Dr. Simpson, which he thinks has more of novelty than of safety for its recommendation, and is at a loss to understand how tearing away the placenta, through an aperture too small to admit the tip of the finger for the purpose of gentle and gradual dilatation, can be either easy or free from danger. He asks, if there is no risk that the finger nail, the common sound, or the bougie, as recommended by Dr. Simpson, may wound the developed and highly vascular cervix?‡ Dr. Ashwell's criticism would be more valuable if it had ever been recommended to pursue the practice which he condemns, which is not the case.

Mr. Newnham, of Farnham, attributes the success which has attended his practice in placenta prævia principally to his turning as soon as practicable, and distrusts the new practice; stating, however, that he can imagine cases, as in narrow pelves, or where the

* London and Edinburgh Monthly Journal of Medical Science, March, 1845.

† Medical Gazette, Sept. 19th, 1845.

‡ Ibid, October 24, 1845.

* Medical Gazette, Loc. cit.

† Ibid, November 7, 1845.

head is presenting, and it might ultimately be necessary to diminish the size of that head, in which the removal of the placenta might give room for the employment of the perforator. He concludes by saying that, "in cases of necessary hæmorrhage, the rule must still be to turn and deliver as soon as circumstances will admit; but when circumstances render this rule impracticable, it is a comfort to be able to fall back upon another practice with the conviction that it may be hopefully employed."* Mr. Newnham and Dr. Simpson, therefore, are agreed as to the value of the proposed treatment, and differ only, (if they do at all,) in the degree of importance each attaches to it.

At a meeting of the Medical Society of London, a discussion took place on the subject, Dr. Golding Bird and Mr. Crisp inclining to favour the new practice, while Mr. Dendy, Mr. Hadland, and Dr. Chowne condemned it. The latter gentleman remarked, that "though many cases were detailed in which separating the placenta and bringing it away had been followed by a cessation of the hæmorrhage, and the safe delivery of the patient; yet he did not think that the practice must be considered essentially proper and safe, merely because the dangers to which it was liable had not occurred. From the results of his own experience he was satisfied of the safety of the old practice, (!) and he did not consider that by separating the placenta we did anything to stop hæmorrhage, since this could only be averted by contraction of the uterus, over which we had comparatively no control."† And Dr. Lee, with his usual asperity, states that "he is firmly convinced that none but the most rash and inexperienced would have recourse to a practice which offers no advantage to the mother, and must be followed by the certain destruction of the child."‡

In conclusion, on looking at this subject candidly and impartially, we cannot, I think, deny that very great praise is due to Dr. Simpson, for the manner in which he has brought it before the profession. The practice advocated by him is supported by facts, by reasoning, and so far, by experience; and if the statistical tables have not been compiled with that degree of exactness which we could have wished, but which from the very nature of the subject is unattainable, still, with every allowance for these inaccuracies, they go far to prove the value of the practice which they are brought forward to recommend. The reasoning appears to be sound and logical; and whether the theoretical views as regards the source of the hæmorrhage be admitted or not, the facts drawn from these views, whether depending on them or not is of little consequence practically, cannot be denied; while as far as our limited experience hitherto can bear any testimony, either one way or another, it speaks trumpet-tongued in favour of the safety and efficiency of Dr. Simpson's practice. Out of the many cases recorded in the medical journals within the last four months, not one, so far as I know, when treated on the recently advocated plan, has proved fatal. A few of the most interesting of these cases may be here subjoined.

In the *Northern Journal of Medicine*, August, 1845, a case is related by Dr. Maclean, where the os uteri was dilated to the size of half-a-crown, the placenta

presenting, and hæmorrhage taking place at every pain, which had produced very great faintness and collapse. The child was dead, as ascertained by the stethoscope; the placenta was removed, all hæmorrhage ceased, and the woman was out of bed in a few days.

Mr. Wilkinson, of Spalding, in the *Provincial Medical and Surgical Journal*, July 23rd, 1845, relates the case of a patient between six and seven months advanced in pregnancy; the os uteri dilated to the size of half-a-crown; the placenta presenting; hæmorrhage excessive. Stimulants were given, the placenta was removed, and all flooding ceased. The patient recovered.

In the *Monthly Journal of Medical Science*, June, 1845, there is the report of a case attended by Mr. Dix, in which the hæmorrhage was excessive, but the placenta was expelled before the child; and on total separation taking place, all bleeding ceased. The child had been dead some days; the mother gradually recovered.

In the *Lancet*, September 27th, 1845, Mr. Jones, of Llanfair, communicates another successful case. The os uteri was fully dilated; the hæmorrhage persisted; the placenta was removed, and the child expelled by the unassisted efforts of the uterus; it was dead. The patient recovered as soon as females generally do after an ordinary labour.

In the same journal, for June 7th, Mr. French, of Nantwich, relates a case in which there was profuse flooding; the placenta, however, was expelled by the uterus, and hæmorrhage at once ceased.

Dr. Walker, of Chesterfield, in the *Provincial Medical and Surgical Journal*, September 3rd, 1845, records a case where the flooding was alarming till the placenta was removed, when it entirely ceased. The child required turning afterwards, as it was an arm presentation, and was dead. The patient recovered perfectly.

In the same journal, for September 10th, Mr. Greenhow, of Newcastle, gives a case which was equally successful. The patient was in the eighth month of pregnancy; there was great hæmorrhage, the placenta being separated to a considerable extent, and protruding at the os uteri. The placenta was removed, and bleeding ceased; the child was dead; the patient did well.

In the same journal, for September 24th, a case is related by Mr. Parker, surgeon to the Bridgewater Infirmary, in which the placenta was expelled before the child. There was no hæmorrhage subsequently, and the patient did well. The child, it is presumed, was dead.

The last case to which I shall refer, is one which occurred in the practice of Mr. Wood, of Rochdale, and has not yet been reported. The os uteri was dilated to the size of half-a-crown, and easily dilatable; the hæmorrhage very great, and the patient becoming blanched and restless. The placenta was already separated for at least one third of its circumference, and the ordinary practice might readily have been adopted if it had been thought expedient. It was easily detached, and the flooding totally ceased; on a renewal of the examination, a foot was found presenting in the vagina, and the child was at once delivered. The patient suffered severely from headach for some days, but this was relieved by opiates; there was no untoward symptom; the child was dead.

* *Medical Gazette*, November 14, 1845.

† *Ibid*, Oct. 10, 1845. ‡ *Ibid*, Sep. 10, 1845.

It should, however, never be forgotten, that Dr. Simpson recommends the extraction of the placenta before the child in a limited number of cases only, and not as we might suppose, from a perusal of certain strictures on the practice, in every possible instance; and these are just the cases which have hitherto proved the most formidable in practice, and the most fatal in their results. Any plan, therefore, which proposes to diminish the risk and mortality in these most serious complications is worthy an extended trial.

The only disagreeable part of the subject I have purposely reserved for the last. It must be very painful to those whose object is the elucidation of truth, to observe the very intemperate tone in which this subject has been treated of by some writers, especially by Dr. Lee. If the practice advocated by Dr. Simpson implied a direct censure on Dr. Lee, the latter gentleman could not have levelled more abuse at it. Instead of regarding it as a plan of treatment put forth for the improvement of medical art, and as such, patiently testing its merits or demerits, the words "unjustifiable," "rash," "tearing away the placenta," &c., are constantly recurring. From a teacher to a pupil these expressions could scarcely be justified; from one professor of midwifery to another they are simply absurd; and this method of dealing with the subject cannot fail to have the opposite effect to that intended by Dr. Lee. People will think that there must be a strong principle of vitality in doctrines which require so powerful an acid to destroy them. Dr. Lee ought to know that had arguments are not rendered more convincing by harsh words, while good reasons are spoiled by them; and should be told that the profession will never sanction the conduct of any who would sacrifice truth before an altar—however fairly adorned, or richly decorated—raised to the false Gods, jealousy and spleen.

A CRITICAL ANALYSIS OF THE PRINCIPAL FACTS OF DISEASE.

(Continued from page 71.)

POSTSCRIPT TO INFLAMMATION.

Sympathetic pains, and convulsions of a local character, (5) are explicable on the principles above detailed.

Pain has the same seat as sensation; it is sensation in excess; it is directed by the same laws of action. The subjoined case affords a good example of sympathetic pains and convulsive movements of simultaneous origin. E. C., a female, aged 22, was a patient under the care of the writer, in the County Hospital. She had suffered during a year from pain occurring every five minutes in the left hip-joint, and descending to the foot. The pain had been accompanied by a convulsive movement of the limb for eight months. There was also pain at the sacrum, and on pressure in the left inguinal region, extending thence to the sacrum and hip. Aching pain at the hypogastrium accompanied the other pains during menstruation, which was menorrhagic, and of seven or eight days duration. All the pains, as well as the convulsive startings, diminished greatly after the menstrual period. The patient also had pain in the descending colon, and at the crown of

the head, with an opening and shutting sensation there. She recovered after a lapse of some months, when the uterine functions had become more healthy.

This case presents one of numerous instances in which all the conditions of pain existed in the uterus, while reflexed pains only were felt. This distinguishes sympathetic from radiated sensation,—a subject suggestive of extensive consideration. The state of the uterus was unhealthy, and such as to yield incident impressions to the spinal cord, and reflex pains to the sacral plexus, thence to the sciatic nerve in its entire length, as well as to branches of the hypogastric plexus belonging to the recto-colon. The only vestige of pain at the seat of the disease is produced on pressing the left ovary; but while the uterine system is engaged in the performance of its functions, it is the seat of pain; then the primary incident tract is active throughout its course to the sensorium. In the intervals the secondary incident tracts only are active, those lying between the seats of sympathetic pains and the sensorium.

It is obvious that in this case the conditions of pain, not actual pain, in the uterus, impressing the incident tract, excited reflex movements in the limb, simultaneously with the excitation of pain there. These conditions reside in the capillary system, the functions of which are under the influence of the organic nerves. These nerves act insensibly on the circulation; but when this or the nerves themselves are disordered, the ganglionic or insensible functions are enlarged, and the impressions which they remit to the incident nerves terminate in the production of spinal or cerebral phenomena, (reflex motions and sensations,) as above explained.

The nervous and capillary functions have a mutual reaction on each other. The former in excess may excite changes in the latter, or capillary disturbance may give rise to nervous in the first instance, and in turn be reflected in the manner stated. One of the most remarkable facts in neurology is that the primary source of pain should not have an impression of its conditions transmitted to the sensorium, though the intensity of such impression is sufficient to excite reflex sensations and movements. In such a case what is the condition of the direct incident tract at the point of reflex action? The power to transmit impressions is evidently suspended between a point of the cord and the brain. Is that portion of the cord exhausted from the increased demands of the ganglionic system upon it under the influence of a morbid condition of capillaries?

The analysis of erection (6) may advantageously follow in the discussion; there the same principles are brought to bear. The organic or emotional current, commencing at the brain, is transmitted to the erectile organs; increased capillary circulation follows. In this chain of phenomena is united the entire function of organic nervous structure. In erection, as in blushing, emotion on passing the ganglia sets the capillary circulation into renewed action. Within the brain, and between it and the ganglia, is emotion; below the ganglia, sensation; and the result is rapid capillary circulation.

These organic nervous phenomena persisting, incident impressions are conveyed to the spinal cord and brain; in the former exciting reflex sensations and the