Dissertations by eminent members of the Royal Medical Society.

Contributors

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DISSERTATIONS

DISSERTATIONS

BY EMINENT MEMBERS OF

THE ROYAL MEDICAL SOCIETY

PRINTED FOR THE SOCIETY

EDINBURGH

DAVID DOUGLAS

1892

PREFACE

THE ROYAL MEDICAL SOCIETY, not unmindful of the excellent work which it has done, and the advantages which it has, for more than a century and a half, bestowed on the students of Medicine in Edinburgh, and justly proud of the long roll of illustrious names which are to be found in the records of its membership, has resolved to publish a selection from the Dissertations which have been read at its meetings by members now deceased.

The technical expression Dissertation calls for little explanation from those who have in any way taken an interest in the Edinburgh School of Medicine, and none from those who have enjoyed the advantages—and these are manifold—of the membership of the Society. For those, however, who are not conversant with the Medical Educational Institutions of Edinburgh a short explanation of the working ways of the Royal Medical may not be inappropriate.

When a student of Medicine, or it may be a newly fledged member of the profession, desires to

enjoy the privileges of the Society, after the usual preliminaries of proposal and ballot, according to the laws, he takes his seat as an ordinary member. He is bound to attend the meetings, and is fined for absence so long as he continues to reside in Edinburgh. After a due lapse of time, varying according to the number of those who have preceded him in election, he is called upon to read to the Society an essay on some subject connected with the healing art or some of the ancillary sciences, which is called his Dissertation. The paper is open to the criticism of the members, and on many occasions has given rise to debates which have had an important effect in extending the knowledge and influencing the future opinions and practice of those who have had the benefit of hearing or taking part in them. The member who has read the Dissertation, and almost of necessity taken part in the discussion thereof, is entitled to become an Extraordinary Member, and is now freed from compulsory attendance and fines.

The Honorary Members are those members of the profession or men of science whom the Society, by the votes of the attending members, desire to honour on account of scientific reputation or professional esteem.

It is essentially to the list of Extraordinary Members that the Society has had to look for the

authors of those Dissertations which it is selecting
for publication, and there is an abundant series at
command. It is to be noted, however, that there
are several names, and some of them among the
most illustrious, of whom there are no Disserta-
tions extant. Of these may be mentioned William
Cullen, one of the founders of the Society, Joseph
Black, Haller, the Monros, Oliver Goldsmith, Mark
Akenside, Mungo Park, Sir Charles Hastings,
Thomas Addison, and Charles Darwin.

It is not to be supposed that even the latest of
these selected Dissertations will add much that is
new to our knowledge of Medicine, theoretical or
practical. To expect this would be to cast a slur
on the majestic steps in advance which our profes-
sion has made in recent years ; but the writings
temporis acti are still of much value. They form
important additions to the history of Medicine—a
study too much neglected ; they, in many instances,
reveal to us the embryonic state of opinions which,
by a process of evolution, have come to dominate
the scientific world ; they furnish us with interest-
ing additions to the biographies of men whose
memories we delight to honour ; and lastly, though
not least, they furnish, in their rough and ready
ways of working, and their imperfect means of ob-
servation, a salutary lesson to the enthusiastic dis-
coverer of the present day, armed with all his

modern refined appliances that *vixere fortes ante Agamemnona multi.*

The present members of the Society desired that the Preface to this issue of selections should be written by one of the older members. Gratitude for the benefits which I derived from it in my student days, for the honour of occupying its Chair, and for the honorary membership which it bestowed on me, were sufficient reasons for my acceding to the request that I should write the Preface. It is not, however, without misgivings that I have assented, because I know that there are many, both among the past and present members, who could have written a better Preface than myself. But as regards the being an old member, I fancy that I am sufficiently qualified, as I have been in its ranks now for sixty-two years, and have never lost anything of my interest in the Royal Medical, or my desire for its continued prosperity.

DOUGLAS MACLAGAN.

March 1892.

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EDITORIAL NOTE

THE Editorial Committee desires to state that in many cases the Dissertations published in this volume, as originally written were too long for reproduction *in extenso*. They have been abbreviated by the omission of such matter as lengthy quotations from other authors, detailed clinical histories and bibliographical references; but in no case has the writer's text been interfered with. Many of the original MSS. contained plates and diagrams, which it was found impossible to reproduce.

The Committee has to express its indebtedness to PROFESSOR SIR DOUGLAS MACLAGAN, PROFESSOR CHIENE, and DR. JOHN WYLLIE for their kind assistance and advice in the selection, from among several thousand Essays, of those contained in this volume.

MEMBERS OF EDITORIAL COMMITTEE. — *Convener*—ALEXANDER MILES, M.D.; *Secretary*—A. B. GILES, M.B.; R. J. A. BERRY, M.B.; GERALD FITZGERALD, M.B.; E. BARNARD FULLER, M.B.; A. N. S. CARMICHAEL; R. P. COCKBURN, M.B.; D. C. BREMNER; LIM BOON KENG; J. R. HIGSON; G. L. K. PRINGLE; ROBERT HUTCHISON; S. G. DAVIDSON; R. M. LESLIE.

DANIEL RUTHERFORD

1749-1819

OF APOPLEXY

Read 1770

A GENTLEMAN about sixty years of age, of a sanguine temperament and very corpulent, subject to attacks of the gout and nephritic affections, otherwise healthy, though accustomed to a full rich diet, yet of late has indulged more freely in the use of wine than formerly, having for some days now and then complained of headache and vertigo, suddenly while at table dropped down from his chair and lies as if in a profound sleep, without appearance of sense or motion, at least of the left side—for this cannot be made to contract by the application of severe stimuli, though the part shows some degree of feeling. His breathing is natural, pulse full and strong, though slow, has been in the above state above a quarter of an hour.

MR. PRESIDENT,

From the symptoms of the above case I think it is evident our patient is affected with a stroke of the apoplexy, one of the most

A

dreadful diseases in its manner of attack and event of any that befall the human body.

It is comprehended by Dr. Cullen under the first order of Neuroses, the Comata, and is thus defined, *Motus involuntarii fere omnes imminuti cum sopore plus minus profundo.*

Sauvages distinguishes it from the Carus and Cataphora by the stertor or *respiratio sonora* which enters his definition of apoplexy. Dr. Cullen with great propriety omits this symptom, and considers them as species of the same genus.

It is known from epilepsy by the relaxation of the muscles, for though there are sometimes convulsive motions of the muscles of one side in apoplexy, yet they are to be considered as entirely accidental, and are never so violent as those which happen even in a slight epileptic fit.

It differs from syncope in the way it attacks, which is for the most part very sudden, whereas in syncope the strength more gradually fails, with excessive sickness, tinnitus aurium, cold sweats, etc., besides, the pulse is for the most part always, in the sanguine apoplexy, fuller and stronger than in syncope.

It has a much nearer affinity with paralysis than any other disease—indeed, they commonly run into each other. I know no certain symptoms by which they can be distinguished at the first attack; the most likely are the almost universal and entire loss of sense and motion, and profound sleep that attend apoplexy.

It has been alleged by some that apoplexies are vastly more frequent now than they were anciently. This notion arose from a passage in Celsus, whence it

might be concluded that the ancients considered a resolution of the nerves rather as a case which could, but never did happen ; but by comparing this with other passages from Celsus, and other authors either before or after his time, there is the greatest reason to believe the whole spurious, and the opinion quite without foundation. It is true that apoplectic and sudden deaths have been much more frequent at one time than at another, but at the same time it seems certain that these occur under atmospheric circumstances which are universally allowed to be the occasional causes of apoplexy, such as sudden changes in the temperature of the air, etc.

Physicians have commonly marked two cases of apoplexy, very different from each other, both with respect to the subject, the attack, and more especially with regard to the method of cure—the sanguine and serous. The first, those of plethoric habit, have short necks and signs of topical determination to the head. During the fit the face is flushed, the pulse full and strong. The serous, on the other hand, attacks those of a lax phlegmatic habit, and who seem more disposed to dropsy. Besides, there is a third kind, that may attack all temperaments indiscriminately, which proceeds from the introduction or application of matters which seem to destroy the nervous power itself without any sensible effect upon the state of the body, such as some poisons, mephitic air, *e.g.* ; we can even imagine the state of the nervous power which induces sleep, carried to a higher degree, occasion all the symptoms of apoplexy. But as my patient's disease is entirely of the first kind, viz. the sanguine, I shall confine myself solely to that.

Though we must suppose the proximate cause of all apoplexies to be one and the same, as it seems to be purely an affection of the nervous fluid, there is little hope of our ever determining with precision what that may be, since people are cut off by particular apoplexies where we cannot observe the smallest deviation from the natural texture of the brain ; we must be contented to take the preternatural appearance within the cranium for the proximate cause of sanguine apoplexy. There are few diseases that afford so good an opportunity for discovering the causes by dissection as the present.

There is commonly found an effusion of blood or serum between the meninges of the brain, into the ventricles or some preternatural cavity in the substance of the brain, though in some few cases there is nothing uncommon to be observed, except an immoderate distension of the vessels, and accumulation of blood in them. I would therefore assign as the proximate cause of the apoplexy an accumulation or effusion of blood within the encephalon, and, from thence, compression of the brain.

A very great many cases may contribute to produce this effect. They seem to be referable to two heads : first, whatever tends to increase the quantity or accelerate the motion of the blood in general, or particularly in the vessels of the brain ; second, whatever retards the free return of the blood by the veins from the brain.

To the first head belong all the causes of plethora, and particularly high living and indolence, as in the present patient ; compression of any of the large arteries on any other part of the body ; particular conformation of the body to favour the ascent

of blood to the brain, as a large head, short neck. In people who have died of apoplexy there have been only six cervical vertebræ found, unnatural division of the arteries, as the vertebral coming from the aorta in place of the subclavian.

Those which belong to the second head are more frequently the causes of apoplexy. The particular distribution of the veins of the brain may by itself perhaps tend to produce accumulations in them, since by the firm texture of the sinuses and the tendinous threads which run across and connect their sides together, these cannot suffer any dilatation. The smaller veins which run throughout the brain must be more subject to it.

The advanced stage of life to which our patient has arrived comes under this head, for at this time congestion in the veins, or venous plethora, is most apt to take place.

Obstructions to the motion of the blood through the right ventricle of the heart belong thus likewise to this head.

Also any obstructions to the free passage of the blood through the lungs, as happens commonly in all cases of difficult respiration ; hence whatever hinders the descent of the diaphragm, as a great accumulation of fat in the abdomen, as is probably the case in our patient. The over-distension of the stomach proves a cause of apoplexy. Violent strainings, in which we commonly make a full inspiration, and then confine the air for a long time, sometimes occasion it. Cold air, by repelling the blood from the surface, and accumulating it in the lungs, and thereby obstructing respiration ; and, on the other hand, the air becoming hot of a sudden, by rarefying and

expanding the blood, are also mentioned as causes of it.

Compression of the jugular veins, or lying with the head very low, are apt to bring it on, for by barely stooping for a short time headache, sidomia, and other symptoms that commonly precede an apoplectic fit, are induced.

The causes that for the most part more immediately excite it are those which occasion an increased action of the heart and arteries, such as anger, hard drinking, etc.

The symptoms are all easily explicable from the supposition of a diminution, almost a cessation, of motion in the nervous fluid. The only difficulty is to explain how the heart and arteries continue to act when the rest of the body is almost inanimate. Some have attempted to account for this by deriving the nerves of the heart from the cerebellum, which they say is never affected in apoplexy. But I think we can scarcely subscribe to this opinion when we observe that other organs supplied with branches of the same nerves are equally deprived of sense and motion with the rest of the body. Nor is it a fact that the cerebellum is never affected. It seems more probable that this phenomenon is caused by the greater quantity or greater power of the vessels, inside of the vital organs, than of the other parts of the body, by which they are enabled to perform their functions for a long time, though in a great measure deprived of the *vis nervea*.

With respect to the prognosis, the apoplexy is for the most part fatal, and always dangerous. There are no symptoms, as far as I know, that certainly prognosticate a cure; the danger is chiefly

to be estimated according to the degree and universality of the loss of sense and motion. When these are great, and the fæces voided, it is commonly fatal in a few minutes.

The apoplexy agrees so very much with active hemorrhages, that the cure of the present fit, and the preventing a return of it, are conducted upon the same plan in both diseases.

The recovery from a fit is attempted by taking off the congestion from the head. It is evident that blood-letting is the only means that can be employed here, for any other evacuations, as by vomiting or stool, however desirable they may seem, by the straining that attends their operations, are likely to aggravate the cause of the disease.

A great quantity of blood should be taken, and that speedily too. Authors have differed with regard to the vessels from whence it should be drawn. Arteriotomy would answer very well in this case, were it not that the evacuation is very slow this way. Opening the jugular vein seems to be the most proper, and it ought to be performed on the soundest side of the body, if there be any difference, as most nearly connected with the side of the brain that is compressed, for whichever side of the body is chiefly struck, the opposite side of the brain is found to be affected. Mead and some others strongly recommend the application of cupping-glasses to the occiput, having previously taken care to make the scarifications very deep.

If the patient is not roused from his fit by the blood-letting, there will be very little chance for a cure. We should then try blisters applied to the head, and some purgative either by the mouth or

by way of clyster. Hot, stimulating applications are manifestly improper in such a case, yet if death seems inevitable otherwise, these must also be tried. Perhaps pediluvia might cause a revulsion from the head.

Were we so fortunate as to restore him to his senses, I think small doses of tartar emetic, so as to procure a diaphoresis, would be the most effectual means of carrying off the remainder of the fit.

From this time his diet ought to be extremely poor, and every means used to keep him very low; upon the slightest attack of headache, etc., it would be proper to take a little blood. He ought to abstain entirely from wine, and use a great deal of exercise, though very cautious not to fatigue or overheat himself, lest that might occasion a relapse.

II

JAMES GREGORY

1753-1821

THE *MODUS OPERANDI* OF OLEUM RICINI. IN WHAT DISEASES IS IT USEFUL?

Read 1772

MR. PRESIDENT,

The medicine of which I am now to treat has of late been pretty generally used where a very mild laxative was required, for which purpose it has been found to answer very well. As I have had but little opportunity to observe its effects myself, I must content myself with relating what I have learnt concerning it from others. In particular, I think it proper to declare that what I have to say concerning it is almost entirely taken from a dissertation of Dr. Canvane of Bath, written expressly on the virtues of the Oleum Ricini. Perhaps he is too great an enthusiast in behalf of his favourite medicine, and ascribes virtues to it greater than many would be disposed to allow. But I must observe that few can be so well qualified to judge of its virtues as he is, for he says he used it constantly for seven years in the West Indies, and very frequently for twelve years after he returned to England.

This oil is obtained by expression from the kernel of the fruit of the Palma Christi, as it is commonly called. This plant Linnæus has placed in the 9th Order (Monadelphia) of his 21st class (Monœcia). As I never yet could learn anything from a botanical description myself, I despair of being able to give such a description of this plant as may be of use to others; and if I could, I believe it would hardly be worth my while, as many members of this Society have seen a specimen of this plant already, and those who have not may see it whenever they please. The specimen I mean is that in the Botanic Garden, which is perhaps the finest in Europe. Dr. Hope, I believe, has procured a considerable quantity of the oil from the seeds of this plant. The tree has obtained the name of Palma Christi from a fancied resemblance between the palm of the hand and its leaves. It was called Ricinus by the Romans because its seed resembles an insect of that name which infests horses and other cattle. The French in the West Indies gave it the name of Agnus Castus, from a supposed virtue of moderating venereal appetite, which they ascribed to it; and that name of Agnus Castus was corrupted by the English into that of Castor Oil, by which it is now very commonly known. It is said that this plant was known to the ancient Jews under the name of Keh, and to the Greeks under the name of Κροτών, but it does not appear exactly in what manner they used it. In modern times it was first used in the island St. Christopher's, I believe after the example of the Indians. It is now very commonly used, and Dr. Canvane very strongly recommends it in all cases where a mild laxative is indicated.

In Fevers. It is said to answer very well as a laxative in all fevers where it is required to keep the belly open, and at the same time where it would be improper or dangerous to use any stimulating medicine. It is recommended particularly in the yellow fever of the West Indies after premising an emetic. Dr. Canvane gives us a caution in respect to its use in the nervous fever, where he says it did not answer.

Colica Pictorum or Dry Belly-ache. It is said to be a very valuable remedy in this disease, and to remove the constipation and consequent symptoms easily, safely, and effectually. It is said in particular that those cured by this remedy had no paralytic affections of their extremities; which, as is well known, used frequently to follow on the cure of the original disease.

Bilious Disorders. It is said to be of considerable service in carrying off and clearing the *primæ viæ* from a load of bile which is sometimes poured upon them, as in cholera. Where the bile is obstructed, as in jaundice, it is of use as a laxative to remove the costiveness which attends that disease.

Arthritis. As a mild laxative it may often be used with propriety in this disease.

Nephritis and calculous complaints in general. In the nephritis it may be used to considerable advantage as a mild laxative. But Dr. Canvane observes that it has an effect in promoting the discharge of gravelly matter, and preventing the formation of calculus. He observes :—1st. That as often as he has taken this oil it occasioned a discharge of sandy matter ; 2nd. That by the use of this oil he has continued free from nephritic complaints,

to which he was formerly subject; 3d. That in these islands where this oil is much used, the inhabitants in general are not so subject to calculous complaints as in other places; 4th, That though it generally acts as a purgative, yet it sometimes fails of that effect, and proves a very powerful diuretic; 5th. That its colour and smell are sometimes communicated to the urine of those who take it.

Dropsy. It is said to be useful in this disease, and to have occasioned a great and sudden discharge of urine when well rubbed over the abdomen. Dr. Canvane thinks it useful in the tetanus, in the gonorrhœa and fluor albus; but as he used other more powerful medicines along with it, it is difficult to determine how far the good effects were owing to the oil.

Aphthæ. The Oleum Ricini is said to be of considerable service when given occasionally in the course of this disease.

Dysentery. The importance of mild laxatives in this disease is well known, and perhaps none bid fairer to be of use than the Oleum Ricini.

It is to be observed that the effects of the oil differ greatly from those of the kernel from which the oil is expressed, which is generally the case with expressed oils. The Oleum Ricini is of a mild, unctuous, nauseous taste, but may be taken internally to the quantity of two or three ounces without its producing any other effect but that of an easy laxative. The kernel, too, is very mild to the taste, but if taken internally, even though in very small quantities, occasions sometimes violent vomiting and purging. On this account an emulsion of these kernels, if it is to be used at all, must be

exhibited very cautiously. It is said that an emul-
sion of one kernel only will occasion vomiting and
purging. Perhaps if the oil could be obtained
perfectly pure, and without any mixture of the
other parts of the kernel, it might have no more
cathartic property than oil of olives or oil of almonds.
At least it appears clearly that the chief activity
does not reside in the oil. Perhaps, too, from this
circumstance, the oil may be of very different degrees
of strength and efficacy, as greater or less force has
been used to separate it from the kernel. As we
have it here, it may be given from a half to two
ounces for a dose. Its taste is not disagreeable to
some people, but with many it is apt to excite
nausea; to such therefore it may be disguised with
a sufficient quantity of peppermint-water or brandy
(which last I am informed answers the purpose very
well), or honey or sugar in the form of an oleo-
saccharum or emulsion, or with gum-arabic, etc.
These mixtures would perhaps be rendered more
agreeable by the addition of any aromatic essential
oil.

It may be given with advantage in clysters to
the quantity of three ounces. To children it may
be applied in the way of friction or embrocation.
It is said that when rubbed on the abdomen of
children it will exert its cathartic effects.

I forgot to mention that this oil seemed to be
one of the best remedies to prevent habitual costive-
ness, even when not attended with any other disease.
It may seem somewhat extraordinary that I have
taken no notice of the first part of the question, to
wit, the *modus operandi* of castor-oil. I have all
the inclination in the world to give a good account

of this, but must confess it is beyond my abilities. I believe it acts by stimulating the intestines like other cathartics, but I do not know by what peculiar property it produces that effect more than other expressed oils.

III

GILBERT BLANE

1749-1834

WHAT IS THE NATURE OF ANTISEPTICS, AND HOW DO THEY OPERATE?

Read 1775

MR. PRESIDENT,

This question is both curious as a subject of natural inquiry, and important as an object of the Medical Art. I shall endeavour to consider it in both views, at as great length as the extent of a paper of this kind will allow.

By antiseptics are meant substances that retard or prevent the progress of putrefaction, or correct it when it has taken place. The progress of putrefaction is one of the most important operations in the economy of nature. The life of all organic beings, according to the present constitution of the world, is circumscribed within certain limits of duration. The scheme of nature requires that when deprived of life their organisation should be broken down, both to free the surface of the earth from the inconvenience that would arise from their accumulation, and to afford matter for a new succession of the same forms. All vegetable and animal matter has

a decay and a revival, and putrefaction is the middle stage through which they must all pass, and to which they are all spontaneously prone. But however useful an instrument this may be in nature, it is often a desirable object, both in the economy of life and in medicine, to prevent and arrest its progress, since it disqualifies alimentary substances for the purpose of food, and in the living body is altogether incompatible with health and life. Of however much consequence it may be in all these views, it was long before it was attended to as it ought. Lord Bacon saw the importance of it, and recommended it to the inquiry of physicians. It has accordingly been prosecuted lately by some of the ablest hands, and we have a considerable collection of facts and reasonings upon the subject. Of these I shall endeavour to give a synoptical view, as concise as the great extent of the subject will admit.

In the inquiry I shall observe the following method. I shall first consider the nature and circumstances of putrefaction. Secondly, the means of correcting and preventing it in the dead body. Lastly, the means of preventing and correcting it in the living body.

I.—NATURE OF PUTREFACTION.

In treating of the first article it may be observed that in order that putrefaction take place, it is necessary that a certain degree of moisture, heat, and air be present. These are circumstances that usually occur in nature, and therefore putrefaction is nevertheless called a spontaneous operation. That moisture should be necessary is what we should naturally expect, for no chemical change

can take place among bodies that are absolutely solid. It is necessary that their aggregation be broke either by solution or fusion, that the elementary particles may be free to unite or separate in order to form new combinations; for in this we suppose putrefaction as well as every other change in the forms of matter to consist.

Heat is no less requisite, for it is the great principle of activity in nature, and is particularly favourable to chemical attraction. It favours putrefaction according to its intensity from the freezing-point to 100° or 110°. From this to the boiling heat it has less and less effect in promoting this process. We may perhaps affirm the same from this to the degree in which the substance is decompounded. All other chemical processes are promoted in proportion to the degree of heat; and why this should be an exception is what I cannot perhaps explain satisfactorily, and will not detain you with conjectures.

The third requisite to putrefaction is air. It seems to be necessary to this operation on the same foundation that it is necessary to combustion. How it is so in either case is a difficult question. It seems to be by its affinity with some of the principles of the body, by its attraction for which, it facilitates the decomposition of the substance. The fact here has been a matter of some dispute, but it is now beyond a doubt that putrefaction does not proceed in a receiver that is absolutely exhausted. It has been objected indeed that it proceeds fastest in close vessels. But there is a fallacy here, for though it proceeds faster at first, it is found to be sooner at a stand than if it had been exposed to the free

air ; and the hasty progress at first is owing to the putrid effluvia being retained, and acting as a ferment on the rest of the substance. But when the air enclosed comes to be thoroughly charged with these effluvia the putrefaction is stopped, notwithstanding of the increased force of the ferment. The exhausting of air in part, does indeed promote it, and the condensation of air retard it, but this is to be ascribed merely to the diminution and increase of the incumbent weight.

These three circumstances may in some sense be called the causes of putrefaction. But they are so only in a secondary and remote manner, and we would choose to inquire more closely into this operation in order to understand its nature. It is seldom however we can carry our researches so far with success, for the intimate nature of bodies, and the minute motions taking place among the ultimate indivisible particles in producing the various forms of things, is all an unknown world to us. It is upon such minute changes that the great operations of nature depend ; but the shape, arrangement, and various affinities of the primitive particles of matter are slight and impalpable, and only their distant effects are the objects of our senses. The doctrine of corpuscular attraction does indeed carry us a certain length in explaining the phenomena of chemistry, and we may apply this to the present subject. There is in the formation and dissolution of organic bodies, a separation and combination of particles that seems referable to the various affinities of constitutional parts, in like manner as the mixtures and precipitations of chemistry. There are however in these processes several peculiarities that

seem hardly referable to other chemical changes, as we shall see in the sequel of our reasonings.

Animal bodies are substances of a peculiar nature, and seem to be the most elaborate and exquisite of nature's works. Organs, in order to answer their purposes, must possess a certain degree of hardness or softness, flexibility, and elasticity, according to their use in the system, and these varied and adjusted with a nicety that is not required in the other parts of nature. In order to perfect this, very subtle principles must be combined in the most artful and delicate manner in the stomach, intestines, and blood-vessels, and again unite by accretion into various forms of aggregation in the fibres forming bone, muscles, and membranes. The constituent parts of the animal mixt being thus nicely adjusted, are in a continual effort to separate, and are held together by an unknown and wonderful power peculiar to life. The natural heat of the body is about that degree which is most favourable to putrefaction, yet it resists it; but as soon as life is gone, runs into it in a much lower temperature. The excretion of effete and putrid matter by the skin and lungs, the constant repair from aliment, and the constant motion from the contraction and dilatation of vessels, go a certain length in accounting for this antiseptic power of life.

There is nothing more evident than that putrefaction consists in decomposition, for all the elementary principles in nature appear in the course of it. The aqueous, the saline, the inflammable, the aerial, and the earthy principles are severally evolved. The evolution of water is evident from the greater fluidity of putrefying substances. As to the saline,

there is some doubt of the presence of an acid, but that of the volatile alkali cannot be denied. I think it highly probable that an acid is also sometimes at least extricated. 1. It is presumable, because vegetables are the nourishment of animals, and they are well known to be acescent. 2. Decoctions of the flesh of young animals have been observed to turn sour. 3. It is now proved that animal matter is subject to the various fermentations, for the inhabitants of Siberia and the north of Tartary have a method of making an intoxicating liquor by the maceration of fish in holes dug in the earth. It is probable, therefore, that such substances may pass through the other stage of fermentation when in proper circumstances. Lastly, I conclude that an acid is generated from the septic power of absorbents. As to the aerial principle, it is proved by the experiments of Cavendish and Pearson that both fixed and inflammable air are extricated. The residuum, after the putrid process is complete, and all the volatile parts exhaled, is purely earthy; for putrefaction volatilises the salts more than even fire, converting the fixed alkaline into the volatile, and the acid into that of the phosphoric kind.

A gentleman, to whom we are indebted for several ingenious suggestions relating to the present subject, has supposed that the cohesion depended on the fixed air. But I apprehend that the mode of aggregation in all mixed bodies is the result of an habitual relation subsisting between all the principles, whereby the attraction on which cohesion depends is mutual, and that as soon as a body is decompounded by losing any one of its principles, its properties as an aggregate and its nature as a mixt is destroyed.

If air was the principle first extricated, or the only one, there might be some appearance of foundation for this opinion. But it is proved by the experiments of my ingenious friend Dr. Pearson, that the saline and inflammable principles are as soon extricated as the fixed air. Dr. Alexander has even found that putrefaction can take place without any extrication of air at all.

It has been doubted by some if putrefaction could properly be called fermentation. But it is so analogous to what is called fermentation in vegetables that it seems unnatural to deny them the same appellation. They are both instances of the dissolution of organic bodies; and, what is more essential, they are both excited by ferments. It is this that most distinguishes these processes from all others in chemistry, and there seems hardly anything less understood than their mode of operation. I am one of those who think that fermentation is a process very analogous to inflammation. 1. The principal phenomenon in both is an evolution of phlogiston. 2. The presence of air is necessary to both, and seemingly for the same reason. 3. They both resolve bodies into much the same principles. 4. They both produce actual heat. Lastly, I would infer this analogy from the nature of ferments. For in like manner as inflammable bodies are put into a state of combustion by the application of another body in the state of ignition, we may conceive a fermentable substance to be put into intestine motion by the application of a small quantity of matter actually in a state of fermentation.

They are, in short, both active conditions of matter communicable from the smallest masses to the greatest.

II.—ANTISEPTICS IN RELATION TO INANIMATE MATTER.

Having said so much of the nature of putrefaction, I proceed now to consider the means of counteracting it. These are either such as prevent putrefaction, or such as restore soundness. It is the former that have been commonly understood by antiseptics, since it is but lately we are brought acquainted with the latter. Concerning them I will venture to advance a proposition more general than has hitherto been offered on the subject, in saying that everything with which water can be impregnated is of an antiseptic nature. We have mentioned before that moisture is an indispensable circumstance in putrefaction, and it would appear that pure water is the fittest for this purpose. This is, indeed, what we would expect from the general analogy of chemistry, where we find that changes can take place only in bodies that are in some degree of fusion or solution. Now, it is well known that water unimpregnated with anything is the best menstruum for those bodies for which it has an attraction; and it is proved by the experiments of Dr. Robinson that pure water has a stronger affinity with the animal fibre than any impregnation of it. By being the solvent of the animal mixt, therefore, it gives the elementary particles of the constituent parts that free motion upon each other, which is necessary to the various attractions and repulsions resulting from their several affinities with each other. It is true there are some corrosive salts that seem to possess a solvent power superior to pure water. Of this kind are the simple salts

and some metallic salts. But these seem to act by decomposition, in consequence of a particular affinity with some of the constituent parts, and not by an attraction for the impregnated parts, as is the case with pure water. Though they have a solvent power, therefore, yet they are at the same time antiseptic, by changing the relation of the constituent parts to each other, by supporting what may be called the equilibrium of attraction, and thereby taking off that proneness to separate which they have in the state of the animal mixt.

Having thus stated the arguments *a priori* in favour of this opinion, let us next see how far it is supported by fact. It is now sufficiently proved by the experiments of Sir John Pringle, who has enlarged our ideas upon this subject, that all saline bodies in some quantity or other are antiseptic.

Alkalies from their solvent power, and because they were the product of putrefaction, were supposed to be septic, but this has been clearly disproved by the above-mentioned experiments. It is well known that all impregnations of vinous spirits are antiseptic, and it has been found by a number of experiments that all vegetable decoctions and infusions are so. All the different species of factitious air have been found to possess this power. From this induction of facts we seem sufficiently justified in the proposition before laid down, 'that every impregnation of water is antiseptic.' It need hardly be mentioned that ferments and fermentable bodies themselves are excepted from the general rule. If the impregnation be of a fermentable nature it is itself a subject of putrefaction, and will communicate this state to the mass with which it is in contact.

It will do so much more if it be applied in this state. But if the intestine motion has ceased, and the putrid process be complete, it will even have an antiseptic effect. It is the want of attention to this last circumstance that has produced some fallacy in Dr. Alexander's inferences from these experiments. Those putrid substances which he found antiseptic were such as had undergone the complete putrefactive process, and his facts do not invalidate the common opinion of the septic power of putrefying bodies. We must distinguish therefore between the putrefying and the putrid states.

What we have hitherto said relates only to the matter of antiseptics. We may also take some notice of the means. These are chiefly suggested by the circumstances necessary to putrefaction, which we have before enumerated, and are therefore the want of the requisite degrees of heat, moisture, and air. We have an example of both the two first in the practice of cold climates, where they preserve their meat from corruption by allowing it to freeze in the open air. We have an instance of the effect of drying in portable soup, and many other culinary preparations. As to the third circumstance we have already mentioned the effects of a vacuum. But this is not applicable to any useful purpose in life. There are other methods, however, of excluding the air, which may be turned to use, such as the pouring wax or grease about substances we wish to preserve. To this also may be referred the well-known method of preserving eggs. To the head of means we may add compression, which Dr. M'Bride has found to be strongly antiseptic. The division of a substance into small masses is another means of preventing

fermentation. We have a familiar instance of this in the juice of an orange, which is preserved long from corruption in the small vesicles of the fruit, but soon ferments when effused. Does not this show that the sphere of the corpuscular attraction is of a very sensible extent ?

The other class of antiseptics is of those that restore sweetness. They may be divided into two orders : those that remove the fœtor and prevent farther intestine motion, and those that restore firmness and soundness. Of the first kind are the simple salts, and of the other kind is fixed air. The former seems to act by uniting with the volatile phlogistic principle upon which the fœtor depends, and they check the farther evolution of it by their common antiseptic power. The other seems to act by restoring a principle which had escaped during the septic process.

I would willingly enlarge on this subject, and also enter into the consideration of septics. But the limits of a paper of this kind will not admit it, and the most important part of the subject yet remains to be considered. With regard to septics, I shall satisfy myself with saying that they may be reduced to three classes,—small quantities of some neutral salts, absorbent earths, and putrid ferments. I shall only add that these salts alone seem to be septic which have an earthy basis ; for Dr. Percival has found that Epsom salt is the most septic of all, and that sea salt is only septic in so far as it contains a portion of that salt.

III.—ANTISEPTICS IN RELATION TO THE
LIVING BODY.

We proceed to consider what are the antiseptics of the living body, and what is their manner of operating. In order to be intelligible on this subject, I must premise some general considerations from the animal economy. I need not mention to gentlemen so enlightened in the doctrines of physic, how much pathology, as well as the other branches of medicine, has been enlarged and improved of late by attending to the powers peculiar to life in opposition to those of a chemical and mechanical nature, which so long occupied the attention of physicians. Nor need I mention to whom we are indebted for so great an improvement. It is to him whose name I foretell will make an era in the history of our art for the many new and important truths he has established, and for the singular success with which he has applied them to the practice of medicine. Let us keep constantly in view that the human body is an animated system, subject to laws totally different from those of a mere automaton or of a chemical compound. To be sensible of this we need only reflect on the various phenomena of sense and motion, the irritable contractility of fibres, the generating power of heat,—circumstances in common with no other matter. Let us not be rash therefore in applying what has been said in the former part of this paper to the cure of diseases. We shall find that the principles laid down are in some degree applicable, but that in general we must conduct ourselves by an experience of a different kind, considering how fallacious it is, however

plausible it may be, to transfer to the living body those experiments and reasonings that are instituted on dead matter. The power of resisting putrefaction is as distinguishing a peculiarity in the animal economy as any we have mentioned. We would naturally expect that the causes of putrid diseases would, in many cases at least, operate on this antiseptic power of life independent of any direct effect on the matter of the body itself. We shall find in the nature of this inquiry that there are causes of both kinds, though very different in their power and their mode of operation.

Putrid diseases are either chronic or acute. By the first we mean the scurvy, and under the other we comprehend putrid fevers and gangrene. The scurvy arises from such causes as we might naturally expect to affect the fluids, and it is perhaps the only disease whose proximate cause consists primarily in an affection of the fluids. The occasional causes of it are, an excessive quantity of animal food, or a vitiated state of it, and a cold and moist atmosphere. The first tends to dissolve the texture of the blood, the other suppresses the excretion by the skin, which is one of the principal outlets to the effete and putrid matter continually generating in the mass of fluids. We can make some application of what has been said in the first part of this paper to the cure of this disease. There are, however, many of the antiseptic substances there enumerated which are unavailing, and many of them hurtful. It is only those that restore sweetness which have been found of any service. From these the alkalies must be excepted, as they are highly noxious, both by their stimulant and

dissolving power. Acids are exceedingly useful, especially as preservatives. Their use, however, has been magnified beyond what experience warrants. As in this disease there was supposed to be an alkali present, it was imagined that acids must be infallible by neutralising it. But it is more probable that they operate, either by coagulating the bile, as ingeniously explained by Dr. M'Lurg, or by becoming a constituent principle in the gluten of the blood. This last opinion seems to be confirmed by the superior efficacy of the vegetable acid to any other, for it is well known that this can enter into the mixture of the animal fluids. It is found that nothing will cure this disease but fresh vegetables, or fermentable substances analogous to them. We cannot sufficiently applaud the ingenuity and humanity of Dr. M'Bride in discovering and confirming this practice. He has pointed out a succedaneum to fresh vegetables which bids fair to be of the most extensive use in preserving the life and health of seamen. Such substances seem to operate merely as aliment in producing a total change in the crasis of the fluids. Their effect seems to depend on a particular mode of fermentation in the stomach, consisting in the easy extrication and reabsorption of fixed air. This has appeared so probable that Dr. Priestley and Dr. Percival have proposed to substitute water impregnated with fixed air. The particular advantage of this is that the materials for producing it are very portable, and of an incorruptible nature. Experiments are now making under public authority to determine the expediency of this practice, and we shall suspend our judgment till the result of these appear.

We come now to consider the acute putrid diseases ; but as I am already beginning to exceed the usual limits of exercises of this kind, I must satisfy myself with a few general observations, nowise proportioned to the difficulty and importance of the subject. Putrid fevers belong chiefly to warm climates and seasons, and it has been imagined that heat produced them in the living body as it does putrefaction in the dead body. But it is found, by observations on the thermometer, that the heat of the body is the same in all climates. It acts in a more indirect manner, by producing a debility and relaxation which predisposes to disease by carrying off the more fluid part of the blood by perspiration, and by generating more copiously those human and marsh effluvia which are the remote causes of fever. It can, indeed, be pretty clearly evinced that the putrid state is a consequence and not a cause of such fevers. This appears not only from the general doctrine that all fevers are primarily affections of the nervous system, but from the absence of putrid symptoms in the beginning even of putrid fevers. We are to ascribe the putrid state, therefore, either to the sedative power of the remote cause, concurring with the debilitating and relaxing power of external heat, in diminishing that energy of the vital motions which enables the body to resist the spontaneous tendency to putrefaction ; or to the contagious matter acting as a ferment, and multiplying itself by assimilating the fluids to its nature. The existence of this last is sufficiently proved by the multiplying of contagion, but the first has probably a greater share in the putrid symptoms ; for, in the remittent fevers of

warm climates, proceeding from marsh effluvia, and in tropical gangrenes, the putrefaction advances very fast without any suspicion of a ferment. For this, as well as other reasons, I cannot help disagreeing with so great an authority as Sir John Pringle, who thinks that in putrid fever, as in scurvy, there is a primary affection of the fluids, that these diseases only differ in the putrid acrimony being more slowly generated in the latter than the former, and that the latter assumes a chronical form by the symptoms being gradually habituated to this vitiated state of the fluids. The purpose of this reasoning is to show that few indications in putrid fevers are to be drawn from what has been said concerning the antiseptics of dead animal matter. Since the morbid condition of the system depends primarily on the state of motion, it is to this the means of cure must be directed. This theory is confirmed by experience, which ought to be the touchstone of all our reasonings; for those antiseptics which are at the same time tonic have been found most serviceable in the diseases in question. The bark and other tonics do indeed possess a strong antiseptic power in relation to dead matter; but this is not to be wondered at, since every impregnation of water does so. If the effect of medicines in putrid fevers was in proportion to their simple antiseptic powers, we should find camphor, ardent spirits, and essential oils preferable to all others, and alkalies would be as useful as acids. But the contrary of all this is known by experience. The operation of acids seems indeed to consist, in a considerable degree, in a purely antiseptic power; but this chiefly by sweetening the putrid contents of the

intestines. Much may depend also on their effects on the bile, which is remarkably acrid and copious in putrid fevers. The vegetable acid, as a nutritious substance, may, as in scurvy, have a good effect in correcting the crasis of the fluids. I need not insist on their refrigerent and diaphoretic powers, which render them applicable in all fevers.

I should now proceed to consider gangrene, but I find I have grasped a too extensive subject, and must leave this part altogether untouched. My unexpected attendance this session has obliged me to give in this dissertation at a much earlier period than I thought of, having intended to reduce it to a more concise form, and to have determined some of the ambiguous points by experiment. I have now only to crave indulgence for the crude form in which it appears, sensible that it is not answerable to the expectations of those who may know that the subject was assigned me at my own particular desire.

ROBERT JAMESON

1774-1854

IS THE HUTTONIAN THEORY OF THE EARTH CONSISTENT WITH FACT?

Read 1796

MR. PRESIDENT,

The celebrated theory of Doctor Hutton has, for several years, attracted the attention of geologists, not more from the ingenuity with which it is supported, than the vast collection of facts which it contains, rising in this respect far superior to all former conjectures. His arrangement is extensive, and would require more time than the Society could spare, for a complete examination would include a thorough review of the most interesting mineral phenomena. It would be preposterous then to investigate all the general principles he has laid down; I shall therefore only examine that position which appears to me to be the basis of the theory; that is, that all the strata of the globe have been consolidated by means of heat, and hardened from a state of fusion.

To explain this position two principles have been assumed, viz. Insuperable Compression and Slow Cooling. The consideration of these will conse-

quently contain what I have to say in these few notes with regard to this theory.

INSUPERABLE COMPRESSION.

This wonderful principle of compression, we are told, prevents the escape of elastic vapours, fuses silex, etc. And, in proof of this, various mineral phenomena are produced, which are said to be inexplicable upon any other principle. Thus, silex is said to be insoluble in water, consequently, must have been crystallised, etc., from fusion. All this, however, must be rejected, when we find that silex is soluble in water, and that it is probable it can form crystals, by extremely minute division, as is the opinion of Kirwan, Macie, and Chaptal. We are next told that the flinty nodules, found in chalk-beds, have been in a state of fusion, and ejected from some other place to their present situation, then the strata have been raised from the bottom of the sea. Unluckily, in this instance, he allows, notwithstanding immense compression, that these flinty nodules can be tossed from one place to another.

The only other fact I shall consider is the petrifaction of wood. This, we are informed, is produced by the liquid flint being injected into the body of the wood under immense compression. To this curious piece of hypothesis I may answer by referring to the consideration of specimens of petrified wood, where one half is silicified, the other in its natural state. What becomes of the vegetable matter that disappears? It must be converted into carbonic acid; and this, we are told, cannot take place under insuperable compression.

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These facts militate against this opinion more than the Doctor appears to have been aware of, and are more supported from the consideration of the phenomena that must have occurred in the consolidation of the globe. Thus, he tells us that this world is the ruins of a former, which had gradually been deposited at the bottom of the sea in a loose form, but, by the application of heat, under immense compression, was hardened. Here then the particles are much approached to each other, and the solid materials are formed, consequently air and water must be separated. This being allowed (which, I think, cannot be denied) it follows that all crystallised bodies should be destitute of water of crystallisation ; in short, that all the materials of the globe should be in a glassy form.

From what has been now said, it appears that notwithstanding insuperable compression, carbonic acid can be formed, flints tossed from one part to another, and immense quantities of air and water separated. All these, according to the Doctor's own statement, must have occurred ; therefore, it will not be thought presumptuous if the idea of insuperable compression be rejected till further proof be brought of its existence. This being set aside, the whole theory appears to fall to the ground. I am, however, aware of the possibility of considerable controversy about this ; I shall therefore examine the theory a little further, to endeavour to discover something satisfactory. In doing this, I am naturally led to the consideration of the second principle, that is,

SLOW COOLING.

This idea was first started by the ingenious Sir

James Hall, one of the most able advocates for this theory. In reflecting upon the formation of granite by fire, it immediately occurred to him that although insuperable compression was present, yet still it followed that the quartz and felspar would form a homogeneous mass. Much about this time a curious appearance was observed in the cooling of glass, which he mentions in the following terms :—

'A quantity of common green glass having been allowed in a great mass to cool slowly, it was found to have lost all the properties of glass, being opaque, white, very hard and refractory, and wholly composed of a set of crystals which shot into some cavities in a determinate form. When a piece of this substance was melted by the violent heat of a blow-pipe, and was allowed to cool instantly, it recovered all the properties of glass. We may conclude from this example,' says he, 'that if the glass produced by the fusion of granite had been allowed to cool with sufficient slowness, it might have crystallised, producing a granite similar to that which was originally melted.'

Even reasoning *a priori* from such a phenomenon, I should never have adduced it as an irrefragable proof of the Huttonian theory; for the term slow cooling is here evidently another way of expressing a chemical change; if so, it cannot be applied to explain the formation of granite. That this is the true explanation we find from his own words, when he tells us that the glass had lost all its properties, was opaque, white, very hard and refractory. Will any peculiar arrangement of particles cause a sub stance to lose all its properties, and assume new ones? If not, it follows that this change must

depend upon the abstraction, or addition, of some matter; consequently this explanation must be rejected.

To be convinced more certainly of the truth of what I have now said, I made the following experiments :—

Experiment 1*st*. Exposed a quantity of green glass to a strong heat, in a wind-furnace, for some hours; suddenly exposed it to the cold air, and found that it was completely converted into a whitish mass.

Experiment 2*nd*. Exposed equal quantities of green glass, and what Sir James Hall calls slowly cooled glass, to heat in a smith's forge; when I observed that the slowly cooled glass (as it is called) required *greater heat to fuse it*.

Experiment 3*rd*. Exposed a quantity of green glass to a strong heat, fused it, then cooled it suddenly; and observed the surface to have somewhat of a zeolitiform appearance.

Experiment 4*th*. Melted a considerable quantity of green glass in a large crucible, and allowed it to cool slowly (that is, by allowing the fire to be gradually extinguished); when I observed several zeolitiform masses, which were slightly opaque, had the usual hardness and fragility of glass, with the same glassy fracture.

Experiment 5*th*. Filled several crucibles with green glass, then put them into a wind-furnace, continued the application of heat for several hours, taking out the crucibles at different periods to observe the successive changes of the glass at different temperatures; when I found that as the heat was continued the glass grew opaque, hard, and at

last was reduced to a substance much resembling frit, being the siliceous matter combined with a very small proportion of alkali.

After having made these experiments, I was told by a gentleman that glass made from metallic oxides, would, by slow cooling, become opaque, etc. To ascertain the truth of this, I made the following experiment :

Experiment 6*th.* Took certain proportions of pure silex and red oxide of lead, and melted them together ; allowed the materials in one crucible to cool slowly ; but the other was instantly immersed in cold water : both presented the same transparent orange-coloured glass.

These experiments show us that the change of the properties of glass does not depend on slow cooling, but upon the application of certain degrees of heat, which extricates from it some substance, in greater or less quantity, according to the continuance of this heat. The nature of this substance, I apprehend, is at once evident : it is plainly the alkali of the glass, which is demonstrated from its opacity and infusibility. This is also further proved from the experiment with the glass made of lead, where no such change could be produced.

To all this it may be objected, as is done by several eminent chemists, that opaque masses are found in the centre of masses of glass, where the alkali possibly could not be separated. In answer to this it must be inquired : Are these masses always of the same nature ? If they are not (which is the case), it follows that the explanation of their formation must be different. That these masses are some-

times crystallised cannot be disputed; but have they lost any of their properties? Have they not the same fragility as glass? The same fracture and degree of fusibility? They have, however, very different appearances; sometimes they are the neutral salts used in the formation of glass, which have been prevented from rising to the surface; they may be what the workmen call tears, which are part of the pots vitrified; or particles of sand, which, in the operation of fritting, have been precipitated by the too sudden application of heat, and consequent separation of alkali. What conclusion, then, are we to draw with regard to this slow-cooling? I apprehend it is this:—That the opacity, infusibility, etc., of the glass does not depend at all upon slow cooling; but, on the contrary, it depends on the continued application of a certain degree of heat, which favours the escape of the alkali, thus changing the chemical nature of the glass.

It appears, then, that immense compression and slow cooling are, as yet, wanting of proof; consequently the pillars of the Huttonian conjecture are not well founded.

P.S.—I omitted to mention the account which Dr. Hutton has given of Natron. He tells us that 'it is a solid crystalline salt, with a structure which, upon fracture, appears to be sparry and radiated, something resembling zeolite. It contains no water of crystallisation, but melts in a sufficient heat without any aqueous fusion.'

To this account the celebrated Kirwan answers by producing specimens which contain water of crystallisation. The Doctor, however, is so little satis-

fied with this sort of explanation, that he questions whether or not he is to consider that the author is, on this occasion, consistent with himself? I can only say that from the opportunities I have had in examining this substance, which have been very considerable, I have often observed saline efflorescence, demonstrating to a certainty the presence of water.

V

HENRY HOLLAND

1788-1873

AN INQUIRY INTO THE NATURE AND ORIGIN OF THE PASSIONS IN THEIR RELATION TO THE INTELLECT AND BODILY ECONOMY OF MAN

Read 1810

MR. PRESIDENT,

It was not without hesitation that I made choice of the subject which this title purports. To any prominent novelty in its discussion I cannot lay claim, but at the same time, by a new arrangement and method of inquiry, I may possibly succeed in furnishing some deductions which have not before been expressly pointed out. In metaphysical, still more than in physical science, improvements are often the result simply of a new process of investigation applied to objects which before have been differently examined.

I am anxious that the object of inquiry should be distinctly understood. My design is, not to give a detailed account of the Passions and Feelings, or of the sources whence these individually arise; but to consider the nature of Feeling or Passion as a distinct attribute of the mind; and to survey the

relations of this principle to the intellectual powers, and to the bodily economy of man. In making use of the word Feeling, I would be understood to refer generally to all those affections of mind which we term Feelings, in contradistinction to the acts of the understanding or reason, and which are derived directly from sensible perceptions, or more remotely from sympathy and association of ideas. These affections it will be my object to reduce to one general principle, or faculty as it may be called, of the mind; a view of the subject which will appear to have experienced too much neglect from metaphysical philosophers when we consider its important relations to mental science.

The subject thus defined admits of division into four sections, the first containing general considerations of the argument; the second discussing the relation of the Feelings to the original constitution of the mind; the third their connection with the intellect; and the fourth their relation to the bodily economy of man.

GENERAL CONSIDERATIONS ON THE SUBJECT.

A circumstance greatly impeding the formation of general views on this subject has been the vague and unphilosophical character of language in relation to the feelings. Not only are various propensities and habits of action introduced into this class, but the degree likewise and minute modifications of feeling have received their distinctive appellations and separate arrangement. All this may have its convenience in matters of common life, but to the metaphysical inquirer it enhances the difficulty of obtaining general and systematic conclusions. It

will be sufficient to notice one of the many instances
which might be given of this want of precision,
arising from the defects of language.

The usage of words places before us anger and
revenge as two separate emotions of the mind. A
slight consideration will show that these are modi-
fications of the same feeling, and that the modifying
circumstance is simply the duration which the feel-
ing possesses. The action which proceeds directly
from a sense of injury we call the effect of anger or
passion; that which results from a sense of injury
for some time retained we term the act of revenge.
In these two cases it is evident that the feelings
are identical in nature, and that the only distinc-
tive circumstance is their longer or shorter continu-
ance in the mind.

A simplification of the mental feelings might
evidently be carried to a great extent, and with
manifest advantage to the perspicuity of our know-
ledge on the subject. Though prevented by the
limits of my paper from following it further into
detail, it may still be proper briefly to consider
what method or basis of inquiry will best aid us in
attaining such simplicity and precision in the treat-
ment of the subject. Considering, as is our object,
the general nature and constitution of the passions
and feelings, and how we may best simplify an
inquiry into these, our first question must regard
their most general points of resemblance or differ-
ence as they appear in the mind. Is there any
circumstance affecting uniformly one class of these
feelings in contradistinction to another class? This
question admits an affirmative answer. We may
observe one prominent distinction, affecting severally

the nature or quality of all the feelings, and giving to each its appropriate and decisive character. This circumstance is the connection of a sense of pleasure or pain with every act of feeling,—a fact highly important in its nature, and in its relation to the present object of inquiry.

Whether this sense of pleasure and pain arises from physical or other causes is a question at present immaterial to our object : the point here to be observed is its actual conjunction, in greater or less degree, with every affection of mind which can be classed under the name of passion or feeling. We are unable to bring before our minds the conception of a passion, without admitting at the same moment the conception of a pleasure or pain, co-existent and connected with it. Every mental affection attended with an emotion of pleasure or pain may, in propriety, be termed passion or feeling ; if unattended by either of these emotions, it must be referred to some other class of mental operations. It may be a simple act of memory, a judgment, or an association of ideas, but it cannot in philosophical strictness be termed a feeling.

Here then we obtain a distinction highly useful as a basis for inquiry, and which is founded not merely upon the abstract nature of these mental functions, but likewise upon the general, though not strictly defined, sentiment of mankind. Its peculiar advantages are, universality of application, and the means it affords of distinguishing the feelings from every other faculty or operation of mind—a circumstance essential to the accurate discussion of the subject. As it is our object to examine into the general nature of mental feeling, the synthetical

mode of inquiry is that we pursue, and hence the
more general the distinctions we assume, the more
important are they to our progress.

My limits prevent me from applying the charac-
teristic feature, just pointed out, to the individual
passions and feelings. The examination, however,
is open to every mind, and in its progress will con-
firm the remark I before made, that language has
had much effect in obscuring our knowledge of the
subject. We shall likewise be able, by attending to
the characteristics of pleasure and pain, to mark
the distinction between the feelings properly so
called, and those inclinations of habits of action, as
avarice, voluptuousness, which in common language
are often confounded with the former.

ON THE CONNECTION OF THE FEELINGS WITH THE ORIGINAL CONSTITUTION OF THE MIND.

Dismissing these general considerations, I shall
proceed to the question respecting the origin of the
feelings ; not meaning, however, to investigate here
the cause by which they are drawn forth in life, but
to consider more especially their relation to the
nature and constitution of the mind. And here some
important inquiries offer themselves to our notice.

The question first occurs : Are the feelings, taken
generally, to be considered as innate in the human
being, as created and born with him ? Or are we to
regard the mind as devoid of original tendency
towards any particular passion or feeling—as a
tabula rasa (to use the metaphor of Locke) upon
which these affections as well as ideas are after-
wards to be inscribed ?

To the latter opinion there can be little hesitation

in giving an assent, and some reasons for this preference I shall briefly state.

First. I may remark that as every feeling has some specific external object or cause, it would be a strange paralogism to suppose that the feeling might appear before its object or cause has any existence.

Secondly. It is a consequence of this argument that those who consider the passions innate in the mind are committing themselves to a doctrine of innate ideas, such as was maintained in the Academic school among the Greeks, by Descartes and others in more modern times, and which has since been generally discarded from the philosophy of mind; to suppose the passions innate necessarily presumes these ideas to be so likewise. It would be needless here to repeat all the arguments against this doctrine of innate ideas. Suffice it to say that they have induced the greater number of philosophers to reject the opinion as untenable in itself, and unnecessary in explaining the phenomena of mind.

Thirdly. The gradual appearance and formation of the feelings in the progress of life renders it improbable that they are instinctive in the mind. Instincts are at once perfect and complete—the feelings subject to intimate varieties, according to the situations in which the human being is placed.

Lastly. As the feelings are the great sources of moral action, the idea that any of these are innate in the mind supposes an influence of the Deity over human conduct which is scarcely consistent with the moral responsibility of man. It may be said that this difficulty occurs to all who believe in the necessity of human actions, and there is certainly

some plausibility in the statement. Arguments, however, may be adduced to prove that the cases are not perfectly parallel. The rational advocate for necessity supposes a present scheme, by which some great future moral good is to be attained. He regards as part of this scheme the direction of each individual to particular modes of thought, feeling, and action by the circumstances around him, and considers the proposition of future rewards and punishments as an exhibition of motives to modify those which the events of life may create. The advocate for the innate existence of the passions adopts an idea little reconcilable with this system of moral government. By giving to a superior agency the formation of these affections, which determine the whole course of action, he destroys the relation of man, as a moral being, to the circumstances and events around him, and throws a shade over the pleasing idea that rewards and punishments are proposed simply as motive agents in a great general system, and obscures our hope that this system is tending forwards to some state of more enlarged happiness and perfection. Such are the apparent points of difference in the two cases.

But against the general position that the feelings are not innate in the mind, may be urged the instance of Conscience or the Moral Sense. Each individual act of conscience consists, it may be said, of certain emotions or feelings, the offspring of an innate independent principle, common to all—a principle subject to none of the vicissitudes of life, constantly watching over the actions of men, and producing remorse or self-satisfaction, as these actions are vicious or the reverse. That such is the

voice of the world cannot be denied. The *forum
conscientiæ* is everywhere regarded as a divine
tribunal, infallible and certain in the dictates which
it pronounces to man. The sceptic, as to common
opinion, while admitting the general belief, will still
think it necessary to assume another position. He
will define conscience, ' an emotion or feeling conse-
quent upon certain conceptions of the reason, and
similar in its nature and origin to all other feelings
of the mind,' and, in vindication of this belief, he
will propose the following reasons :—

In the first place, the position that conscience is
an innate principle involves by direct inference the
doctrine of innate conceptions or ideas ; the improba-
bility of which, and its relation to this subject, have
before been commented upon.

Secondly. I may remark—and the argument is
an important one—that if the feelings of conscience
were innate, we might expect an almost entire
similarity of these feelings in all individuals of the
species and in every age of the world. Regarding
conscience as an inherent sense of right and wrong
—as the *mens divina* of humanity, as a principle, to
use the words of Cicero, *segregata ab omni concre-
tione mortali*, we ought to discover in it none of
those diversities which all the other qualities of
humanity display. Yet how completely is this op-
posed to observation and fact ! Looking at the
individuals around us, what varieties do we not
observe in the feelings of conscience as present in
their several minds ! Consulting our own conscious-
ness, how great is the diversity in the impression of
these feelings at different periods—a diversity not
of degree alone, but of their nature, quality, and

objects also. The contrast is still more distinct when we observe the affection of conscience, as manifested in different communities of mankind. In some countries infanticide is practised without scruple or remorse; among other nations, the practice of destroying a parent, when burdened with years, is deemed meritorious and useful; in others, again, human sacrifices are considered exalted displays of piety and virtue. Examples of this kind might be indefinitely extended, but I apprehend they must be familiar to every one who has studied the history of mankind, and their existence sufficiently proves that conscience is no uniform, unalterable principle, but varies with the circumstances of individuals and nations.

The fact seems to be, that the moral sense of every community is adapted to a certain standard—natural or revealed. With us this standard is the system of moral doctrine contained in the Scriptures, and to it each individual action being referred by the reason, the affections of mind, which we term feelings of conscience, are the result. Where circumstances have made the standard different, the feelings will vary also.

It may be objected to this opinion, respecting the nature of conscience, that it involves consequences injurious to the moral interests of man. To discuss the question, how far abstract opinions, in metaphysics or morals, affect the practical course of morality, would lead us too far from our subject; and I shall therefore shelter myself under the maxim of the philosopher, Ἀληθείαν ζήτω, ὑφῆς οὐδεὶς πώποτε ἐβλάβη.

Are we, then, to consider the nature of the

feelings as in nowise connected with or depending upon the original constitution of the mind? This position cannot, I think, be maintained. Though we have reason to believe that no individual feeling, nor even the tendency to such, can be considered instinctive or innate, yet are we by no means authorised to conclude that the general capacity for feeling is the same in all. Such an idea is strikingly contradicted by an observation of facts. We notice in children, at very early periods of life, an extreme variety in their susceptibility to those impressions which produce or constitute the feelings, and this both in the impressions which arise directly from objects of sense, and in those which originate from the complex operations of thought. In fact, it would seem that a close analogy here subsists between the feelings and the intellectual faculties, strictly so called. The mind, as far as we can contemplate its original form, enters into existence devoid of all ideas or conceptions, possessing simply capacities for the reception of external impressions, for the recollection of these impressions, and for their comparison or combination by the processes of reasoning, imagination, etc. These original capacities appear to be different in different minds, which diversity, though greatly modified by circumstances, has still a general impression upon the after-life of the man. A similar view may be taken of the metaphysical nature of the passions or feelings. These have individually no original existence in the soul; but there seems to be in the constitution of the mind a capacity of feeling varying impressions. This innate capacity, like those of the intellect, has

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no uniform standard of vigour or acuteness, and possibly is in no two cases precisely the same. Its original tone, however, influences, more or less, all the affections to which it gives rise.

Thus far, then, have the feelings a relation to the primary constitution of the mind. From this proposition may be drawn a corollary of some importance, viz. that in cases where any of the feelings are acute and strongly marked, it may be presumed with much likelihood that *all* the feelings have the same character. This deduction is further warranted by experience. We may observe in every individual a certain character and tone of feeling, by which, as a standard, are regulated all the affections of his mind. If a man is warm and irritable in his resentments, we usually find that he is capable of strong emotions of friendship and love; his moments of joy will be rapturous and exalted; his sorrows melancholy and profound.

Akin to this fact is the remark which has been made, that the most splendid virtues and infamous vices are sometimes conjoined in the same person. All the actions to which the character of virtuous or vicious can be applied have their origin in the passions; and the same warmth of feeling which, in certain situations, gives rise to actions strikingly vicious, will produce at other times the most eminent acts of heroism and virtue.

Before quitting this subject, I must remark that the original capacity for feeling is capable of infinite modification from the events of life. Situation, connection, and education affect it in a thousand ways, not merely in giving a direction to different

objects, but also in augmenting or diminishing its general acuteness. Here, too, the analogy with the intellectual faculties is strikingly marked.

ON THE CONNECTION OF THE FEELINGS WITH THE INTELLECT.

I now proceed to the third division of the inquiry,—the examination of the Feelings in their relation to the intellectual powers. Though we have considered the capacity for feeling, in general, as an independent principle of mind, yet conscious-ness must suggest a connection between each single act of feeling, and certain conceptions of the under-standing or reason. The inquiry now before us regards the nature of this connection.

It will at once be allowed that conceptions of the mind invariably attend the presence of passion or feeling, whether arising from the perceptions of the senses, or from associations of ideas. This uniform contiguity, suggesting the relation of cause and effect, we consider the conception, as in every case the immediate cause, of the feeling. Still it must be confessed that there is a difficulty in defining the precise distinction between these two acts or conditions of the mind. The thought has so far a unity and co-existence with the feeling, that they may be considered to constitute together one single state of the mind. Yet are they not identical in their nature? Thought may, and often does, exist without feeling; and when it has the effect of producing the latter, we are distinctly sensible of a change of mental condition, of something added, which was not before present to the mind.

Upon this basis have been founded the in-

quiries of philosophers into the origin of the passions. Looking to the operations of thought as the source, they have laboured thence to deduce all the complicated varieties of feeling which occur on the great theatre of life, to trace out the combinations which they assume, and to display their various and interesting relations to the character of man.

A prosecution of this particular inquiry, even if necessary, would be foreign to the subject immediately before us. A question, however, occurs on the connection between intellect and feeling, more novel in feature, and at the same time highly interesting in its nature. What cause is there why certain conceptions or thoughts should produce feeling, while other conceptions have in no degree the same effect? This question has never, I think, been stated under a perfectly definite form, though many philosophers have casually and indirectly afforded their opinions in reply to it. Of this Mr. Hume is an instance. In his essay on the Origin of Ideas we find an obvious allusion to the inquiry, though he does not follow it into detail.

The fact presumed in this question cannot be doubted. Every one must be conscious that certain impressions from external objects, or certain thoughts passing through the mind, produce emotions of feeling, while other impressions and thoughts have no similar effect. The inquiry, then, conducts us to some diversity in the ideas themselves, as the immediate cause of this fact. And here our choice is limited to a few suppositions. The distinction may consist in the vivacity of the conceptions, in the abruptness of their occurrence, or in the

arrangement or rapidity with which successive conceptions are presented to the mind. These suppositions may further be reduced by considering, as a relation between the first and second, that ideas abruptly occurring are always lively and forcible in their character. Admitting this, it is left to us to consider whether vivacity of ideas or the order or rapidity of transmission through the mind is the circumstance most essential to the production of feeling.

From the passage in Mr. Hume's writings to which I have alluded, it seems that he regarded the former of these causes as principally efficacious. And this idea is certainly not without its probability. An appeal to consciousness will show that every act of feeling is preceded by conceptions more lively and acute than those of common occurrence, and that there is a uniform ratio between the vigour of the conceptions and the vivacity of the feelings with which they are conjoined. The conceptions of danger, pain, and death, which occur to the man at whose breast the dagger of the assassin is pointed, are highly forcible, and produce a sudden and violent emotion of fear. If I attempt to imagine myself in a similar situation I cannot raise conceptions as acute as those which the reality presents, and therefore the feeling, if produced at all, is feeble and transient in its character. Some individuals possess more than others this power of drawing forth feelings by the efforts of the imagination alone. Imagination is the faculty of reviving and variously combining former perceptions ; and according to the vigour of this faculty is the vivacity of the feelings produced from this source.

Many illustrations might be adduced in further proof that the vivacity of impressions or ideas is the circumstance which renders them productive of feeling. The limits of my paper do not allow me to follow these into detail; they will, however, recur to every one who affords his attention to the subject, and accurately refers to the consciousness of his own feelings.

The other supposition we have made is, that a certain arrangement or a rapid succession in the ideas may be possible causes of the production of feeling. Here, however, we proceed with more dubious lights. Some notion may perhaps be formed of a harmonious relation between contiguous ideas, which, when it exists, produces feelings of a pleasurable nature, and of a discordant connection, from which the painful emotions result; but these impressions are too indistinct to furnish a basis for deduction or argument. The same remark may be made as to the effect produced by rapidity in the succession of ideas. In many cases of passion or feeling we find that the conceptions succeed to each other with a rapidity greater than is usual; and, coming nearer to the point, it may be said that there are instances where a mental emotion seems directly to arise from this quickness in the succession of the thoughts. Making, however, such an admission, we still cannot proceed far along this track. Rapid trains of ideas do certainly often occur without producing anything that resembles feeling, and the negative fact is sufficient to destroy any supposition founded on this basis.

Upon the whole, then, it is probable that a certain vivacity of impressions and ideas is necessary

to the production of feeling, and that other circumstances have little effect, except indirectly, in modifying this required vivacity of the mental perceptions.

We have before found reason to believe that the emotions of conscience are essentially similar to all the other feelings, and distinguished simply by their relation to some standard of morals, natural or revealed. This is further true in regard to their connection with the reasoning powers. The conceptions of the mind produce and modify these emotions as they do the other passions and feelings. It is worthy of remark that whenever such emotions are produced, the idea of future reward or punishment is always more or less directly present to the mind. This is so general a fact that its omission would scarcely be justifiable in a definition regarding the nature and origin of conscience.

ON THE CONNECTION OF THE FEELINGS WITH THE BODILY ECONOMY.

We now come to the last, but not the least interesting, part of our inquiry,—the relation of the feelings to the bodily economy of man. Whenever passion or feeling exists in the mind a simultaneous affection occurs of some one of the bodily organs, the degree of this affection being determined by that of the feeling itself. A thousand familiar instances may bring home this fact to the most casual observer. The blush of shame mantling up the cheek, the tear of sorrow or of sensibility falling from the eye, the pallid countenance of fear, are visible demonstrations of its occurrence. History furnishes more striking examples than individual

experience can afford. Instances are given where the operation of fear had been such as in a few hours to change the colour of the hair; and others are stated where life has been at once destroyed by the violent agitation of passion. These, however, appear rather like phenomena than examples. They are, in fact, the extremes of a general rule.

When first I gave my attention to this subject it seemed to me probable that the sole distinction between feeling and the simple operations of thought was the connection of the former with this affection of the bodily organs; or, in other words, that a feeling was simply some train of thought accompanied by certain corporeal sensations. A more mature consideration has led me to alter this opinion, and, admitting a distinction between thought and feeling in their connection with the mind, to suppose that this distinction determines their relation to the bodily economy, giving to the feelings an influence over the material organs, while the operations of reason, strictly so called, have no effect of this nature.

Helvetius, however, in his work *Sur l'Esprit*, seems to countenance the former opinion, and to regard the degrees of the passions as produced by a varying sensibility to corporeal pleasure and pain. The ingenious Bichat has carried the idea still further; and, in considering what he terms organic life as the exclusive seat of the passions, has given these affections only an indirect relation to the intellectual powers. I confess that there are several circumstances which give plausibility to an opinion of this nature; and though, upon the whole, I am now disposed to regard the feelings as primarily

distinct states of mind, yet I cannot consider this latter position as a truth finally, and with certainty, established. It is possible that the fact may be otherwise.

Upon the connection of the feelings with these bodily sensations I may remark, in the first place, that it is invariable and constant. Every affection of mind which we can, with a sense of the propriety of the term, denominate feeling, is thus attended; and though the corporeal changes are various in nature and degree, yet may they, in every instance, be more or less distinctly recognised by the mind.

Secondly, The corporeal sensations thus consequent upon feeling are involuntary in their nature. No act of the will is interposed between the feeling and sensation, nor is volition capable of modifying in any great degree this action upon the material organs.

Thirdly, It may be remarked that there is so far an indistinctness in these bodily sensations, as regards the circumstance of pleasure and pain, that in many cases of passion or feeling it would be difficult to determine to which class the sensations belong. It would seem, too, that the nature of particular feelings and of their attendant bodily affections may be very different in relation to the sense of pleasure and pain. It is well known that strong emotions of joy often produce sensations of a painful kind; while similar emotions, less lively in character, create those which are grateful and pleasing. Here in relation to the mind the nature of the feeling is the same in both cases; while, in respect to the body, a difference in the degree of the sensation affects essentially its nature also. The fact evidently

affords a reason for supposing that the feelings exist in the mind distinctly from the bodily affections, and that the latter are to be regarded simply as consequences or effects.

These general considerations conduct us to a further inquiry :—What is the precise nature of the corporeal changes which the passions thus produce ? The question is evidently one of much importance and extent ; and as its complete discussion would require great amplitude of detail, I must content myself with throwing out a few general hints as a basis for more extended investigation.

The remark first occurs that these bodily affections appear especially in the involuntary organs— an evident inference indeed from the fact before stated, that the will has little influence in producing such affections, or in modifying their extent. The most casual consciousness of our own sensations will confirm the truth of this remark. The muscles which, stimulated by the will, perform the various locomotive functions, are subject to no direct perceptible influence of the feelings. The presence of feeling in the mind (or rather, perhaps, the presence of those ideas which produce feeling) may indeed give rise to particular volitions, and consequent action of the voluntary muscles ; but this is only a secondary and occasional effect. On the other hand, in every case of passion or feeling we are conscious of an immediate impression upon some one of those interior organs which, independently of the will, perform functions essential to the life of man.

The fact here obtained is interesting and important in its nature, and merits a more minute attention than has yet been given to the inquiry. Some

ingenious speculation upon the subject will be found in the writings of Bichat, who applies here also the division of life into animal and organic, which he elsewhere so ably illustrates. Animal life includes the operations of the intellect and will; organic life and its various functions of circulation, digestion, secretion, etc., are exclusively subject to the influence of the feelings, or, as Bichat expresses it, are 'the sole seat of the passions.' Prosecuting also his idea that there is a separate nervous system connected with each of these great divisions of life, he considers the various phenomena of the passions as related to the gangliac system of nerves, or that which supports exclusively the functions of organic life, and supposes them to affect the animal or intellectual life only through the medium of the changes in their organic functions. Though, as I have before remarked, I differ from Bichat's opinion that the feelings have no primary relation to the mind, yet I cannot but think his remarks on their connection with the bodily economy highly ingenious and important.

To the general fact, however, that the involuntary organs are those exclusively affected by the passions, some apparent exceptions occur. One instance of such exception is the affection of the voluntary muscles of the countenance during the presence of certain feelings in the mind. Another similar instance occurs in the muscles of respiration, where passion and volition mutually hold their sway. At moments when feeling is wholly absent from the mind we can counterfeit upon the features the strongest expressions of anger or joy; we can utter forth sighs profound as those which misery itself

excites. These exceptions, however, do not proceed
far, since in every case where such bodily changes
are an effect of the presence of feeling, they are in
no degree derived from the voluntary powers. It
would seem indeed that in the organs subject to
this double influence the will has a power of modify-
ing, or sometimes of entirely repressing, the in-
voluntary affections consequent upon feeling, as
when by a strong effort of volition we counteract
those expressions of the features which the influence
of passion may create. Few questions connected
with the subject are more interesting than that
which regards this mutual relation of feelings and
the will.

But it will be asked, In which of the involuntary
organs, and in what particular mode, do these bodily
affections occur? Various and singular have been
the opinions given on this point. The ancients, as
is well known, imagined a connection between
certain passions and some of the visceral organs, as
the heart, liver, spleen, etc. Van Helmont sup-
posed the upper orifice of the stomach, Buffon and
others the diaphragm, to be the organ or part chiefly
affected by these emotions of mind, but without
offering for their opinion any very satisfactory
grounds of belief. The prevailing sentiment, how-
ever, of mankind has in all ages regarded the heart
as the centre or principal seat of these affections;
and this opinion, as it is connected with many cir-
cumstances of probability, it will be proper more
especially to consider.

That the heart and sanguiferous system are the
parts of the material frame chiefly affected by the
passions is a fact derived both from our own con-

sciousness and from the results of a more general
and extended observation. In every case where
passion or feeling occur to the mind we have a dis-
tinct sensation of something unusual about the
præcordia or region of the heart; some alteration
takes place in the action of this organ, producing a
change or irregularity in its contractions. This
affection is varied in degree, according to the viva-
city of the mental emotion: where the latter is
acute a feeling of painful oppression is often the
consequence, sometimes proceeding so far as to pro-
duce much and continued derangement. Succeed-
ing to this affection of the heart and great vessels,
and doubtless, therefore, a consequence of it, we
perceive a more diffused affection of the sanguiferous
system. In some cases, owing to an increased
frequency and tone in the contraction of the heart,
the impetus of circulation is greatly increased, the
capillary vessels become distended by the augmented
vis a tergo, redness and a sensation of heat occur
over the whole surface of the body. Striking
examples of this effect are afforded by the more
lively emotions of anger and joy. It is manifested
also, though perhaps more feebly, in the emotions of
conscience, of hope, and of love. In other cases the
due vigour of circulation is for a time suspended,
the blood recurs with considerable force to the heart,
while the external parts of the body become con-
tracted and pale. Illustrative examples of this may
be derived from the effects of fear, aversion, and
despair.

A doubt may, however, exist whether the heart
is the organ primarily acted upon by the feelings,
or whether the functions of respiration are not in

the first instance affected. I confess my inability to form a decisive conclusion on this point; but, upon the whole, I consider it more probable that the affection of the heart is that immediately consequent upon the mental feeling.

The effects produced upon the different visceral or glandular organs by the emotions of the mind form a difficult but interesting object of research. It is probable that these effects take place chiefly through the medium of the circulation. In some cases, however, it would seem that they directly result from a diffusion of the nervous irritation by which the heart itself is affected. An instance of this occurs in the stomach—which is often suddenly and violently affected by agitations of the mind, so as to produce instant nausea and vomiting. Regarding, indeed, the connections of the great sympathetic nerve, it will appear probable that no involuntary organ is wholly exempt from the direct influence of feeling—a circumstance rendering it difficult to assign the proportion which the altered state of circulation has in producing these effects.

That much, however, is to be attributed to the influence of this latter cause cannot be doubted. To it we may refer the general paleness or flushing of the surface of the body, the increase or diminution of certain secretions, and the remarkable effects often produced upon the sensorium by the presence of feeling. When the impetus of the circulation is thus increased by the influence of passion, we frequently observe a determination of the blood to particular organs, depending upon some causes which are either wholly, or in great measure, unknown. The affection of the lachrymal gland, producing an in-

crease of its secretion, is one of the most striking of these phenomena, though, from the familiarity, the fact is little dwelt upon by the casual observer. That this little organ should be thus affected by so many and such various feelings is in truth a circumstance of singular and surprising nature.   Not only is the tear drawn forth by feelings of pity and grief, but the joyful and exalted emotions of mind are likewise marked by its presence. It would seem, indeed, purposely designed in our creation, as an index to certain affections of the soul: as an evidence, open to general observation, of the presence of those feelings, which are so important and interesting in the social life of man.

I have now completed the plan which I proposed to myself in commencing this inquiry.   I must again express my regret that the limits of the paper have prevented me from pursuing many subordinate topics—as the nature and origin of the sexual feeling, the influence of sympathies, etc. ; and still more, that I have been obliged to suppress many of those illustrations which the subject not only admits, but even requires, for its completion. A regard to the time and patience of the Society must form my apology for these deficiencies, which otherwise I should have been anxious to supply.

VI

RICHARD BRIGHT

1789-1858

ON GANGRENE

Read 1813

Mr. President,

As different ideas have been attached by writers to the term Gangrene, it will be necessary, in the first place, to state the extensive signification in which I would apply it.

I consider it that state of animal matter, once living and organised, when, without being separated from the body to which it belongs, it loses its vitality.

'La Gangrène,' says M. Quesnay, 'est la morte d'une partie ; c'est à dire, l'extinction ou l'abolition parfaite du sentiment et de toute action organique dans cette partie.'

And I consider the division into gangrene and sphacelus, adopted by some authors, and into gangrene, sphacelus, and esthiomenus, adopted by others, as merely marking different stages of the same disease.

Those who, with the Arabian physicians, make use of three appellations, consider gangrene the most superficial and incipient state, esthiomenus the most

deep-seated and complete, and sphacelus inter-
mediate between the two.

Those who have employed but two terms ex-
pressive of the different stages have of course
omitted that which applies to the intermediate state.
And though in some points of view it may be ad-
vantageous to preserve the three, they will seldom
be employed in the following observations, where
I shall generally apply the term gangrene in its
extensive signification.

The predisposing causes of this disease are :—

1. That capability which exists in every part of
the body, at all times, of having its structure and
functions destroyed.

2. That state of body which favours the de-
struction of powers, and the inordinate action of the
vessels. In other words, the debility of the body,
or part of it, and the tendency to inflammation, the
two states occurring either separately or in conjunc-
tion.

Respecting proximate causes it always becomes
us to speak with great deference. And in the
present case, I only venture to suppose that the
cause of gangrene may be found in the rupture or
obliteration of small vessels, these being the most
probable results of a loss of power and action. Thus
it will appear that I am more inclined to see in this
disease a morbid state of the solids of the body
than of its circulating fluids. The connection, in-
deed, between the two, is at all times, both in
health and disease, so intimate that it is perhaps
impossible for one to be deranged without the other
participating. But I see no reason for considering
the fluids peculiarly or primarily affected in gan-

grene. Still more do I doubt the existence of any-
thing analogous to putrescency in the circulating
mass : for if anything very similar to putrefaction
ever goes on in gangrene, it is not till the life of the
part has been destroyed for some time, and the cir-.
culation through it has been completely prevented.
And, in some of those diseases where the diminished
crasis of the blood and its putrid tendency are
considered as most plainly indicated, the disposition
to gangrene has been by no means great.

The terminations of gangrene are distinctly of
two kinds, leading to the division adopted by some
authors into the moist and the dry gangrene. But
the two different states are often blended together,
or pass imperceptibly from one to the other, and it
is not easy to ascribe a cause for these very different
conditions, as the results of the same disease. That,
of two limbs suffering under very similar circum-
stances, the one shall dissolve away into ichorous
discharge and soft sloughy matter, while the other
shall dry, as if it had undergone a process of embalm-
ing, seems at first inexplicable. May not this be
owing to the mortification in one case being so
complete as to have occupied all the deep as well as
superficial parts, by which the obstruction likewise
becomes complete, and no effusion can take place ?
Whereas if the more superficial parts only be ob-
structed, the deep-seated parts still having a circu-
lation carried on in them, but being weakened, and
more particularly where in contact with the mortified
parts, effusion may well be expected to take place,
and a consequent dissolution of the dead structure.
Thus, though we frequently read of whole limbs
becoming subject to the dry gangrene, yet the

smaller parts are by far more subject to it; and not unfrequently we find a toe or a finger separating itself completely dry, while the limb is consumed by moist gangrene. Dry gangrene, again, admits of some varieties; sometimes assuming a black shrivelled appearance, while at other times, as in a case related by Hildanus, it remains free from colour, and the skin drying away, the tendons become distinctly visible through it.

As to the exciting *causes*, I shall not enumerate them now; they will be successively brought into view as I proceed to speak in order of the different cases of gangrene which occur—in doing which I have adopted an arrangement perhaps too arbitrary, but I hope sufficiently fitted for bringing under discussion the various phenomena of this disease.

Since it has been supposed, perhaps in all cases, certainly in all cases where inflammation has preceded gangrene, that erysipelatous inflammation has been present, I know of no point in this extensive subject which I may better fix upon for my commencing than those cases in which erysipelatous inflammation is obvious and undoubted. At the same time, I consider the truth of the opinion just mentioned by no means established. If erysipelas be considered a disease peculiar to the integuments and cellular membrane, we cannot for a moment support the hypothesis that it universally precedes gangrene, seeing that gangrene attacks muscles, tendons, and bones, and attacks them sometimes in preference to those integuments which lie in immediate proximity. The truth may be that the same debilitating causes acting on the system will induce inflammation to

assume the erysipelatous character on the skin, and will cause inflammation in other parts to run rapidly into gangrene. If, however, the term erysipelatous inflammation be only intended to denote a species of inflammation whose characteristic is high action tending rapidly to corresponding debility, there is little doubt that this is the nature of the inflammation which precedes gangrene. That erysipelas strongly tends to a termination in gangrene is matter of daily observation.

It might be well here to mention that gangrene which is produced by heat, and by substances capable of acting chemically upon the parts to which they are applied. All such agents seem to produce their effects by a double process :—

1. Immediately destroying some parts by disorganising them, and producing a change in their chemical composition.

2. By producing excessive inflammation, which terminates in gangrene.

As to the first of these modes of action, it admits of no other check than the removal of the cause by such means as the place of its application will admit. And the second would seem to require the strictly antiphlogistic treatment, more particularly in its local remedies; and that this is frequently efficacious I have no doubt. The observations, however, of Dr. Kentish lead to a plan of treatment somewhat different in the case of extensive burns. Instead of that assiduous application of cold, which I should conceive capable of preventing, and even anticipating, destructive inflammation, he recommends the gradual reduction of stimulus : beginning with powerful applications, and going on

to the more mild. From the ample experience of this author we have no room to doubt the utility of his practice, yet certain it is that it has failed in many cases; and its ill success led to its relinquishment in one of the hospitals of London. The inflammation which it excited was severe and dangerous. The action of arsenic and other mineral poisons upon the stomach is to promote inflammation and gangrene; and the most effectual mode of relieving this is to evacuate the contents of the stomach, and then to pursue bleeding and such other remedies as you would employ in common cases of gastritis and enteritis.

The action of caustics must be referred to the same division of the subject, but on their mode of action sufficient has just been said.

It is by no means an unusual circumstance to find the surfaces and edges of wounds assuming an erysipelatous appearance, quickly followed by gangrene, and this sometimes with a degree of rapidity which is altogether astonishing.

On what this sudden change depends it is not always possible to discover. In some cases it may be ascribed to the particular circumstances of the individual as respects age, previous habits, increased natural or morbid discharges, diseased blood-vessels, the influence of depressing passions, or the like; while in other cases it appears to be the effect of some peculiar state of atmosphere operating on many persons at the same time, so that the sores and granulating surfaces throughout a whole ward shall in one day change their appearance, lose their florid hue, assume a dull red colour, and a glazed appearance, then become brown, black, or olive

green, and pursue the regular progress of extending gangrene. This is what the French call *La pourre lieu*, and was once so prevalent in the Hôtel Dieu that it was almost expected to take place in every abscess which was opened. There can be no doubt that this may be in a great degree prevented by proper attention to ventilation and cleanliness. The beneficial powers of fresh air are not perhaps even yet sufficiently appreciated. Physicians are becoming every day more and more enlightened upon this point; in certain instances they may even carry their practice beyond the bounds of prudence. But the surgeon should likewise be instructed how little his art can do while he permits nature to be thwarted in her kindest attempts, and deprived of that assistance to which so much is intrusted, whilst the body is unconfined by disease. The rapid change of sores to an unhealthy state may likewise arise from the circulation being retarded by bandages too tightly applied, from the use of sponges, or fomentation cloths, which are not properly washed, or the too severe treatment with regard to inspection, etc.—all which causes may be most easily avoided. There are, however, independently of these causes, some circumstances which we cannot prevent. A man whose body has been constantly under the stimulus of strong drink, or whose advanced period of life has reduced his constitutional energy, may fairly be suspected to repair the injury he has sustained with less facility than one whose powers are unbroken.

The part on which an injury has been inflicted, whether a fracture, a laceration, or a contusion, will likewise have considerable influence. We know,

for instance, that parts which have but little vitality, as tendinous structures, are unable to withstand severe injuries, and in them inflammation quickly terminates in gangrene. Thus, injuries inflicted on the fingers and hands are often troublesome and dangerous, so likewise compound fractures near the joints are known to go on very frequently to gangrene.

There are certain states of body induced by previous disease which render it subject to inflammation, running into gangrene with rapidity, from causes of irritation, or contusions so slight that they would not produce the most temporary inconvenience during a state of health. This is particularly seen in advanced states of fever, where the simple pressure of the body in a recumbent posture, or the irritation of urine trickling over a part, or a pressure of one foot against the other, is sufficient to produce gangrene.

Treatment.—The few instances which I have cited sufficiently show the urgency of the mischief we are called upon to obviate, and the consequent importance of satisfying our minds as to the means of cure most likely to prove efficacious, that we may lose as little time as possible in useless attempts. In the first place, we must endeavour to prevent the occurrence of the disease by avoiding all exciting causes, and moderating those symptoms which lead to it. We must in particular moderate inflammation, and that with the greater vigour in proportion to the tendency of the part to run into gangrene. But, having failed in these attempts, the next question certainly is how far it is possible to restore parts already apparently in a state of

gangrene ?   It does not appear improbable that there may be a moment, a certain short period, when the powers of life have so far deserted some part of the human body that every vital action is at an end, that sensation and circulation and voluntary motion have ceased, and the part may be justly called gangrenous.   And yet the application of remedies may restore it to its functions. Vessalius seems to have been of this opinion when he said, ' That gangrene is the worst which corrupts the part it affects, especially when the corruption has wholly taken place, because it admits of no cure except cutting off' : evidently leading his reader to infer that some other state of gangrene does admit of cure with the preservation of the part.

For a long period it was supposed that the most effectual remedy was to scarify the part, and to surround it by incisions in such a manner as to separate it from the neighbouring sound parts. This was certainly proceeding on the idea of its being an affection of a completely local character. It was a practice inculcated in the French schools as late as the days of Petit, and persevered in by his disciples.   They, however, gradually relinquished it, finding not only its inutility, but its injurious tendency.   If you scarify the dead part only, you do little good.   If you scarify the living part, you stimulate it to an action which it cannot support.

As our science has advanced, medical men have learned to apply their remedies, not to the name but to the disease.   Hence it might occur that bleeding should be necessary in one instance, while bark and stimulants are indispensable in another; and it is the power of distinguishing these that

stamps the merit of the practitioner. It is not probable that bleeding will very frequently be requisite after gangrene really exists. It is, however, mentioned by O'Halloran and others—though the same authors afford us some striking instances of depletion being carried too far. As a preventative while the disease is threatening it may doubtless be useful; but the more useful remedies have been stimulants and tonics. With respect to stimulants, I should think them very capable of being carried to excess. In most cases it appears the indication to strengthen rather than to excite the action of weakened vessels. The urgency of the danger, however, and the rapid secession of life, will not perhaps give sufficient time to our tonics to produce their effect on the constitution. And in this emergency we sometimes find that stimulants will call forth power to oppose the progress of death.

And it is one of the most important and most difficult questions which presents itself in the treatment of this disease, Whether we shall have recourse to amputation? and if so, at what period it may be best employed? In very severe injuries, as compound fractures and gun-shot wounds, the propriety of amputation becomes a question before any symptom of gangrene makes its appearance. And though this should never be done in haste, it will often be found the most certain mode of relief; for where the injury is very great, with certain circumstances of aggravation, it is almost certain that an exhausting, if not fatal, mortification will ensue. But this is not the question most generally to be decided, but rather how far we are to assist nature after gangrene actually has taken place.

It is a well-known fact that after a time, the process of mortification being stopped, a healthy inflammation takes place in the surrounding parts, suppuration goes on, and the sloughs are separated. This process of nature is not confined to inconsiderable parts affected with gangrene, but whole limbs are not unfrequently separated in the same manner. Thus La Motte mentions a girl, seventeen years of age, who had gangrene over her whole leg, but refused amputation. The leg separated at the knee, and she did well.

The question is simply this,—Are we to interfere ? or, giving support to the body, to suffer nature to take her course ?

O'Halloran, after the relation of several cases in which nature had performed the whole, without the assistance of the knife, states two cases where death followed such interference, even after the process of separation had far advanced, and is of course inclined to judge rather harshly of the practice. His cases, however, do not strictly apply to the present question, as they were cases either of spontaneous gangrene or of gangrene after fever. Dease, in remarking on a case which healed with great difficulty after the natural separation of the arm, is inclined to reprobate delay after separation has fully commenced.

Upon the whole, O'Halloran's rules seem very judicious, though they involve some theory which we are not obliged to admit. 'Nevertheless,' he says, 'as we know there are certain cases in which life cannot be preserved without taking off the limb, we may here observe, that when the malignity is entirely deposited on a part—the leg, for instance,—

that the mortified part seems cold, livid, and sense-less; that the dead parts begin not only to separate from the sound, but that a laudable suppuration becomes established from these last.   Till it shall appear in the clearest manner absolutely impossible to restore or preserve the limb should amputation be deferred.   Not only this, but till it shall at all appear that the pulse is well established, the ap-petite restored, and health pretty well confirmed—should not the limb be taken off.'

After the amputation, we are to attempt the healing of the stump by the adhesive inflammation; which, however, will often fail.   And we must then proceed according to the usual rules of surgery.

Prognosis.—I shall neither add to nor take from Dease's observations on this subject :—' Gangrenes that seize fractured limbs, particularly if the fracture is a compound one, and near the articulation, and have come on suddenly, attended with a violent delirium, seldom admit of any relief.' ' Those that succeed gun-shot, punctured, or lacerated wounds of tense, membranous, or tendinous parts, unexpectedly coming on, are of the most alarming nature.' ' Those from long lying critical metastasis, malignant car-buncles, etc., often yield to a proper method of treatment.'

There are several other cases of gangrene, from obvious local affections and inflammations, which will merit separate attention.   And though the limits of a dissertation will not permit of the mention of all, I shall offer a few short remarks on two or three of them.

The most important viscera of the body are sub-ject to this, as a termination to the inflammation with which they are attacked.   Thus large portions

of the brain have sloughed away after accidents or the venereal disease, and that without those severe mental affections which might reasonably have been expected. The lungs likewise are said to be subject to gangrene ; but this is very rare. Huxham mentions pneumonic affections, in which such appearances seem to have presented themselves. The urinary bladder is mentioned by Le Dran ; and the neck of the uterus, in the memoirs of the Medical Society, as having been found gangrenous.

The stomach and intestines are the most frequent seat of this disease, where it forms a fatal termination of gastritis, enteritis, hernia, and the like. The symptoms by which its approach is known in the two last-mentioned organs are thus stated by Cullen :—' The tendency to gangrene may be suspected from the violence of the symptoms not yielding to the remedies employed during the first stage of the disease. And that a gangrene has already begun may be known by the sudden remission of pain, while the frequency of the pulse continues, and at the same time becomes weaker, accompanied with marks of an increased debility in the whole system.' Large and frequent bleeding, blisters, fomentations, and injections are recommended as the most probable means of preventing gangrene, but when it has once formed medicines can be of little avail.'

I will now proceed briefly to state some of those cases of gangrene which originate in constitutional affections, and do not depend on external causes acting locally. These are of three kinds, offering themselves either as symptoms of some other disease,

as the sequels of it, or as the principal feature in the disease. Amongst the first of these I might have mentioned erysipelatous affections, such as that of new-born infants, or the malignant erysipelas (but I have here anticipated myself). The gangrenous affections of the fauces, however, in cynanche maligna, and of different parts of the body, in purpura hæmorrhagica, will afford examples, and at the same time seem to point out two very different states of disease liable to this termination. It always denotes most alarming derangement in the balance of power and action, and will call for much judgment in the practitioner. In scarlatina, where the sloughs are sometimes very extensive, involving the fauces, cheeks, and tongue, and inducing caries of the bones, nitrous fumigations and stimulating gargles, even made with cayenne pepper, are recommended by Willan.

Perhaps I may include under this division of the subject those sloughing sores which supervene upon syphilis, either from the original virulence of the matter introduced, or more generally from the debility of constitution induced by scrofulous habit, or from the employment of mercury. These are frequently cases of the utmost difficulty. The syphilitic taint seems to demand the use of mercury, but it cannot be employed.

These will sufficiently serve as examples of this state of gangrene. And I shall merely add that other complaints, particularly those which are accompanied with great debility, as typhus, scurvy, some species of small-pox, etc., sometimes pass into gangrene.

Independently of any febrile attack, a gangrene will sometimes take possession of the limb, forming

the true spontaneous gangrene, of which I shall next proceed to speak.

This species of gangrene chiefly attacks old people, but it is not confined to them ; it also attacks persons of both sexes. Generally, however, it may be said to attack males advanced in age ; and Pott thinks that amongst these the rich and voluptuous, who eat to excess, are the most frequent subjects of the disease. The poor, however, are not exempted from it ; and it is not improbable that the habit of dram-drinking may go far to induce that debilitated state of circulation which either favours or causes the disease. Probably, whatever diminishes the strength of circulation, and deprives a part of the power of maintaining the regular supply of animal heat and nervous energy, are predisposing, or even exciting, causes of what we have termed spontaneous gangrene. Such irregularities of power are induced by those 'flying and uncertain pains in the feet,' mentioned by Mr. Pott, and esteemed by him forerunners of gangrene. The influence of weakened circulation is also shown in the case related by Home. It was a man subject to low feverish complaints, in whom gangrene was, after some previous threatening, immediately induced by the overaction of a purgative upon his bowels. These patients are frequently found to have been subject to coldness of the extremities, and are deficient in bodily exertion. It is no improbable supposition, though Pott considered it a mere conjecture, without foundation, that the ossified state of the arteries is frequently both the companion and the cause of this disease. Much as the pain would seem to indicate inflammation, there is little evidence of its presence.

Treatment.—Stimulating cataplasms, as local applications, have been supposed peculiarly indicated by the diminished power which in this case prevails. Bark has likewise been administered, under the idea that gangrene must yield under what is deemed the specific against gangrene. Pott, however, on whose observations we ever feel inclined to rely, was struck with the total inefficacy of both these remedies; particularly of bark, whether taken generally or applied locally, whatever the mode of its exhibition, or the form in which it was combined with other remedies. Chance led him to the employment of opium, and the good effects were so decided that he does not hesitate to recommend it as the most certain means of relief with which we are yet acquainted. How the opium acts in this disease I cannot pretend to explain. The effect is to relieve the pain, and Mr. Pott finds that, whatever it does, it prevents the extension of the disease. From this he is led to reprobate the use of heating and stimulating applications, employed locally, as well as the practice of scarification and the removal of parts before nature has separated them. The practice he recommends is this,—A grain of opium every third or fourth hour; keeping the bowels regular by clysters, with external applications calculated to soothe and diminish pain, such as warm milk or very soft and smooth poultices. Mr. Dease seems to doubt the efficacy of this treatment. But the only case in which he had tried it certainly formed no argument against it. Pott did not adopt this practice from the result of a single case, but from frequent experience, for he was well aware that in a disease which often extended to a certain

distance, and then stopped of its own accord, there was great room for deception in judging of the effects produced by any remedy. Others, as well as Pott, had learnt the inefficacy of the means usually employed. Le Dran tells us that he insisted on its use only because, doubtful as the remedy was, he was, as yet, acquainted with no better. And Quesnay came to the same conclusion; and, though he does not propose opium, insists much on an invigorating diet. Perhaps both bark and opium will often be found beneficial, and frequently their conjoined operation will totally fail. I do not know of any instance in which amputation has been performed in this species of gangrene. And, in general, the age and condition of the patient strongly forbid the attempt; yet doubtless cases may occur in which it would be advisable. Le Dran enjoins particular caution in amputating under this state of disease, and seems to have seen it attempted without success.

Diagnosis.—During the incipient stages of this disease it is possible to confound it with three different morbid affections of the toes: the venereal ulceration, the unhealthy sore which occupies the interstices of the toes from a neglect of cleanliness; and the inflammation attacking the gland of the nail, which often puts on an erysipelatous and sloughing character. In all of these cases the progress of the disease and the preceding symptoms will quickly enable us to distinguish with certainty. And in the meantime considerable assistance may be derived from an accurate inspection of the appearance of the sore, and something may also be conjectured from the situation which it occupies.

Thus the two former are situated deep in the interstices of the toes, and the latter generally attacks the great toe, both of which situations differ from that pointed out by Pott as the point where spontaneous gangrene commences.

Prognosis.—The prognosis is always unfavourable in this disease, as it bespeaks alarming weakness in the powers of life. Yet, as it may arise from some local vascular obstruction, or some cause really acting locally, though we know not how, or from some cause whose existence is but temporary, and which remedies may remove, we have always a right to cherish hope. The prospect of preserving the limb is indeed small, but the life must not be despaired of. Much in our prognosis will depend upon the age and constitution of the patient, and a knowledge of the particular circumstances of his situation. If, for instance, he should have been exposed to depressing mental causes, we may hope that the removal of them will restore something like the accustomed vigour to his habit, or if we find that he has been exposed to insufficient bodily support, we may still hope to see him re-established.

I shall venture to class under the present division of the subject, though with some hesitation, the very singular case related in the *Philosophical Transactions.*

In the year 1762 a mother and six children living near Bury were seized, on January 10th, 11th, and 12th, with pain in one or both of their legs or feet, which in five or six days were mortified, and went on regularly in the progress of separation, requiring very little assistance from the surgeon. The father was also slightly attacked in two fingers.

Only one, which was a child of four months old, died of the complaint, the rest continuing to enjoy good health, both at the time of the attack and during the progress of the disease. No probable cause could be assigned for this peculiar affection. The family was healthy, and exposed to no particular cold or moisture. Some little stress seems to be laid on the circumstance of the bread which they had eaten being made of injured wheat. But this was chiefly on account of the similarity which was traced between this affection and one ascribed to the eating of rye-bread made of grain affected with a peculiar disease called Ergot by the French.

Caries.—Before I draw my paper to a close, I wish slightly to mention that bone, as well as the soft parts, is subject to gangrene, and that unconnected with any surrounding gangrene. When by accident or disease the periosteum becomes inflamed, the vessels of the bone partake of the inflammation; it quickly runs into mortification, and gradually separates in lamellæ by a process called exfoliation.

Thus it would appear that gangrene may exist in the integuments, in the cellular substance, in the muscular fibre, and the tendons; in the lungs, in the coats of the stomach and intestines, in the cornea, and in bone,—in short, may probably exist in every texture of the animal body; and wherever it occurs the same general mode of treatment, somewhat regulated by circumstances, is to be adopted. All along, as inflammation indicates the tendency to gangrene, we may do our best to moderate the inflammation. When the gangrene is actually established we must support the system, and allay

every source of irritation. And when at last the gangrene has stopped, we must promote the separation of the dead part by the mildest means, carefully avoiding to inflict an injury on those parts to which the disease has not extended, and, on the contrary, endeavouring to give them healthy and vigorous action. And, when the separation is effected, we must promote the healing of the parts which remain.

VII

MARSHALL HALL

1790-1854

ON THE DISPERSIVE AND REFRACTIVE POWERS
OF THE HUMAN EYE AND ON SOME
MOTIONS OF THE IRIS

Read 1813

MR. PRESIDENT,

It is still a matter of dispute with
philosophers and physiologists whether the human
eye be perfectly achromatic. If any dispersion of
the rays of light in their progress to the retina does
really take place, it is in so limited a degree as in
ordinary vision to pass unobserved. Those, there-
fore, who consult the sense alone are convinced that
the achromacy of the eye is perfect. They have
pretended to comprehend the wisdom of Providence
in the construction of so admirable an organ as the
eye, to explain the principle on which its achromacy
depends, and to have copied nature in the invention
of the achromatic eye-glass. According to these
philosophers the different humours of the eye are so
accurately adapted to each other that the dispersion
of the rays of light produced by the agency of the
one is counteracted and remedied by the effect of

the other.   Little knowledge and consideration, however, are required to point out the fallacy of this opinion.   In the achromatic eye-glass the dispersion occasioned by the convex lens is remedied by a contrary dispersion produced by that lens which is concave.   In the human eye, however, there is no such concave lens.   The aqueous humour induces a convergency of the incident rays and a certain degree of aberration.   The crystalline lens increases the refraction, and, whatever be its dispersive power, the aberration too, and this not only at its anterior, but at its posterior surface.   For although the dispersive power of all the humours may be equal, it is certain that the refractive power of the crystalline greatly exceeds that of the other two ; and, of course, whenever an increase of refraction is produced, the dispersion must be proportionally augmented also.

Those philosophers who have relied more upon experiments made to determine the respective refractive and dispersive powers of the humours of the eye, and on calculations and deductions founded on these, contend that this organ must necessarily occasion an aberration of light, which, however, is so small as to occasion no inconvenience in vision. Dr. Brewster is of this opinion.   According to the experiments of this eminent man, the dispersive powers of all the humours of the eye are equal. Consequently there must be a dispersion of light as the effect of their combined agency.

It is with great diffidence that I presume to maintain an opposite opinion.   But it is difficult to deny the testimony of sense.   In ordinary distinct and perfect vision there is no appearance of colour.

It is mere conjecture to say that colour is produced but not discerned, especially as, in some experiments which I am about to detail, when colour is actually produced it is actually perceived also.

It appears to me that there is a part of the eye, the action of which has not been fully ascertained. The iris is supposed to regulate the quantity of light admitted to the retina, and in vision at near distances to exclude those rays which would fall too obliquely on the crystalline lens for perfect vision. These effects of the iris are undeniable. But there is another, as far as I know, not hitherto adverted to. It is the inflection and the dispersion which the rays of light which pass near its edge must necessarily suffer.

Now, is it not possible that this action of the iris may serve the purpose of the concave lens in the achromatic eye-glass? A little consideration will ascertain that the effects it must produce are precisely those induced by a concave lens. Let a house or other object be viewed through a small perforation in a card; it will be very perceptibly diminished. Let the rays of the sun be admitted through a similar perforation, a dispersion of the light will be the consequence. It is principally on the rays which pass near the edges of the perforations that these effects are produced—the very rays, of which the dispersion in vision requires to be removed. These are precisely the effects which a concave lens introduced into the eye would occasion. Those coloured rays which are the most refrangible are also the most inflectible; and thus the dispersion produced by the one may be remedied by that effected by the other.

I am not able at present to appreciate the effect which the pupil may have in correcting by inflection the dispersion of the rays of light caused by the humours of the eye. But it is remarkable that its influence in this way is greatest when most required ; namely, when intense light or very diverging rays strike the cornea. As a diaphragm preventing the rays passing too near the edge of the crystalline lens, and as a means of correcting the dispersion of the rays of light, its contraction on the approach of objects is in every way calculated to increase the effects intended to be produced.

Admitting this influence of the iris on the rays penetrating the pupil, I am inclined to think that the eye is, in distinct vision, perfectly achromatic. In this idea we have no difficulty in explaining why we do not see the colours supposed to be produced —we trust the information of the sense ; and we remove the difficulty which a calculation of the effects of the humours of the eye in dispersing the rays of light occasioned to those who regarded the achromacy of the eye as perfect, although inexplicable.

This discussion, however, relates entirely to the condition of the eye in distinct vision. In certain cases of indistinct vision, the human eye is certainly not achromatic. I shall proceed to the detail of some of many experiments which I have made to establish this point—experiments which from their novelty and importance cannot fail to excite much interest.

I may commence this division of our subject with the three following general propositions :—

1st. Whenever a pencil of rays of light whose

axis is inclined to the axes of the eyes enters the pupil, the light suffers a dispersion into the prismatic colours.

2nd. Whenever the rays of light which penetrate the pupil possess an inclination to the axis of the pencil which is different from that possessed by rays proceeding from an object to the distinct vision of which the eye is adapted, a similar dispersion of the rays is induced.

3rd. Whenever the pupil of the eye is larger than is natural for the vision of the object to which it is directed, an aberration of the rays of light proceeding from it is occasioned.

1. Let the eyes be adapted to distinct vision at the distance of 10 inches, and, retaining their conformation, let attention be paid to the appearance of an object at the distance of 6 inches. The outlines of this latter object will be tinged with the prismatic colours. If a page of a clearly printed book be viewed, the lines of the straight letters (the I's for instance) will appear double, two purple lines including a space of yellow being formed. A dot (.) becomes a purple circle with yellow in the centre. If two O's (oo) be viewed, the purple of the parts nearest each other is brought to coalesce in the manner of two penumbræ, and a spot of dark violet is produced. Other objects are also tinged with some diversity of colours unnecessary to specify.

2. Let the eyes be fixed on an object at the distance of 6 inches, and, when adapted to distinct vision, be glanced towards another object, at the distance of 10 inches : phenomena somewhat similar to those mentioned in the first experiment will be displayed.

3. It is possible after repeated trials to adapt one eye to distinct vision at any given distance, and the other eye to indistinct vision at that distance, the axes of the two eyes not meeting in the object viewed. To the first eye the object is seen without colours, by the second the decomposition of the rays of light is occasioned.

4. If when the eyes are adapted to the distinct vision of a near object one eye be pushed aside, the object seen by it will be observed to be tinged with the prismatic colours; the other remains achromatic.

These experiments, which might be much diversified, prove the first proposition. The following experiments will exemplify the second :—

1. The eyes being adapted to distinct vision, let a concave lens be interposed between one of them and the object viewed. The rays of light will be decomposed.

2. Let the same experiment be made with a convex lens; similar phenomena will be observed. Concave and convex specula and a perforated card answer the same purpose.

3. Let the eye be adapted to distinct vision through either of the lenses, and let the other eye view the object without any interposed lens, the prismatic colours will be observed.

To illustrate the last proposition, some experiments were made with the pupil enlarged by the application of belladonna, by myself and several other members of this Society.

The principal appearances with regard to colour were the following. At a great distance objects appeared fringed with purple. At a much less

distance similar or nearly similar colours to those mentioned in the first experiment (page 88) were observed. It was remarkable that when the latter colours were very evident to both eyes, they disappeared in great measure when one eye was closed. But the object being brought nearer to this eye, the colours again appeared. They were however again made to disappear by interposing a card perforated by a small hole, until the object being brought still nearer the eye, the dispersion of the light a third time took place.

It is remarkable that of six who have performed this experiment two have observed indistinct vision only without colours. The eyes of one of these gentlemen are more convex, those of the other, thought to be less convex, than natural. To the other four the colours were very manifest.

I am doubtful under what proposition the following experiment ought to be inserted. If the finger or other object be interposed between the eye and the bars of a window, those bars are fringed to a great degree with the prismatic colours, the edge nearest the finger with the blue and green, that more remote from the finger with the red, orange, and yellow.

The refractive power of the human eye is, as Dr. Wells has remarked, diminished by the application of the belladonna. Persons possessed of natural sight cannot see at the ordinary distance. A pen, for instance, cannot be mended at all, if brought so near as, in ordinary circumstances, to distinguish the point, indistinct vision being immediately produced. Persons in this situation are assisted by convex lenses, and, indeed, this experiment com-

municates a most perfect notion of the assistance derived by long-sighted persons by the use of convex glasses.

A short-sighted gentleman, to whose eyes the belladonna was applied, could no longer see distinctly with his concave glasses, but vision was as perfect without them as it usually is when performed by their means.

It is probable that this effect does not depend on the enlargement of the pupil, as it remained, in an experiment related by Dr. Wells, long after the pupil had regained its natural size.

I have already remarked that distinct vision is performed at a shorter distance by one eye than by both.   I must add another remark, which has, as well as this, escaped Dr. Wells—that objects seen by one of the eyes to which the belladonna had been applied were apparently much diminished in size.

This fact is of too singular and curious a nature to be passed over without observation. I do not know whether a similar phenomenon be observed by long-sighted persons, or persons possessed of the common, or diminished refracting power. It still continues in myself some time after the contraction of the pupil has begun.

It would appear from this fact, relative to distinctness of vision, that there is some modification induced by one eye on the other in vision under the influence of belladonna. I have observed a similar appearance, with regard to distinctness of vision, on looking, first, with both eyes, and, secondly, with one only, through concave lenses; and perhaps convex lenses may produce the same effect, objects being perfectly distinct to one eye

which were confused when viewed by both; but this may be explained.

How it is that objects appear less to one eye than to both is at present equally a mystery to me. Perhaps the particular degree of inclination of the optic axes to each other may convey to the mind some view of the distance, and consequently of the size of the object.

It has been observed that objects are seen distinctly nearer by one eye than by both, and nearer by one eye with an interposed perforated card than when the card was removed. This circumstance suggests the idea that the iris naturally contributes much to distinct vision through a greater range of distances than would otherwise occur. It is indeed difficult to separate the effect of the enlargement of the pupil in these experiments from the other change in the refractive power of the eye also observed. It is highly probable, however, that the perforated card performs the office of a contracted pupil in including those rays which proceeding from near objects would pass so near the edge of the crystalline lens as to become extraneous, and induce indistinct vision.

Some interesting experiments might be devised for the purpose of ascertaining the relation of the size of the pupil in different persons, and the natural limits of distinct vision.

I now come to the last part of my subject, the Motions of the Iris.

This muscle has been classed amongst those which are termed involuntary, possibly rather erroneously. It must always be remembered that,

in a certain sense, all muscles are involuntary. When I move my hand and fingers in writing this essay, I do not will to move a particular muscle; it is the end, the aggregate motion, not the means, the individual motions that are under volition. When I wish to change the place of vision from one more remote to a nearer, I no more *will* to move the two adductores muscles of the eyes than of the iris, whose motions are concomitant with those of the former muscles. But the adductores are termed voluntary, the iris an involuntary muscle. It may be said that the eye may be moved when the lids are closed, and therefore without willing to change the place of vision. Now even this circumstance has a parallel in the motion of the iris, for it is possible to obtain the power of moving this circular muscle at will, without fixing the eye on any subject.

1. In the first experiment (p. 88) the pupil is larger than in distinct vision at the distance of 6 inches.

2. In the second it is smaller than in distinct vision at the distance of 10 inches.

3. In the experiment under the second general proposition, similar motions of the pupils are observed.

4. In the third experiment (p. 89) under the first proposition, the pupils are of an unequal size.

5. Lastly, it is possible by much perseverance to obtain such command over the iris as to move it at will when the eyes are bent on vacancy.

The last experiment especially is conclusive with regard to the motions of the iris being capable of being urged at will. Nor are the former ones of

an ambiguous nature, for it is not necessary, as stated in the experiments, to have a real object situated at the distances mentioned; the object to be viewed is sufficient, the adaptation of the eye and the motion of the iris being performed at will, with perfect ease, after repeated trials.

A person altogether ignorant of anatomy is no more conscious of the motion of the straight than of this circular muscle of the eye in adapting it to vision at different distances. Therefore to each muscle may be, with equal propriety at least, applied the term voluntary, if this term be applicable to muscles at all. It might be less erroneous to denominate certain motions, rather than the particular muscles by which they are produced, voluntary or involuntary. However, I do not wish to contend further about the use of a word.

I shall only add, in the last place, that the experiment No. 3 (p. 89) is an instance of real strabismus. The state where the optic axes do not meet in the point oscided is not a squint. Strabismus takes place only where the optic axes meet out of the mesial plane produced.

I cannot conclude without expressing my obligations to Messrs. Bigsby, Fyfe, Wright, Sandberg, and Davy, who have all had the kindness to make some of these experiments for me, and to give me a written account of the phenomena they observed.

With regard to the whole of this paper, I must say one word in extenuation of the too manifest hurry in which it has been prepared. It was all written and composed in its present form in the space of three hours after midnight. The necessity for this was occasioned by circumstances already stated to the Society.

VIII

ROBERT LISTON

1794-1847

ON FRACTURE OF THE NECK OF THE FEMUR

Read 1820

MR. PRESIDENT,

Before proceeding to give an account of the circumstances attendant on fracture in this situation, its diagnosis and treatment, it may perhaps be proper to say something of the nature of the apparatus in the healthy state.

Anatomy.—   *   *   *   *

Fracture of the neck of the femur, as well as the separation of the epiphysis, may be produced by blows or falls on the hip, or by force applied to the distal extremity of the femur, either directly or indirectly, as in case of falls from a great height in which the person alights on his knee or foot.

Diagnosis.—The diagnosis of injuries of any part, and above all those of the hip joint, are very difficult. This difficulty arises from the swelling consequent to any injury, the great pain attendant on any examination of it, and also from a peculiar state of the ligaments and other apparatus by which a kind of crepitation is produced. In the thigh joint the feeling is moreover very deceitful, owing to the great

thickness of the immediate covering, and the superincumbent parts. Fracture of the neck of the femur can with difficulty be distinguished from fracture through, or immediately below, the trochanters, but fortunately the treatment ought in all respects to be the same. Before swelling comes on, and within a short time of the accident, distinct crepitation of the broken ends may be perceived by rolling the thigh with one hand, whilst the other is placed over the trochanter major, and the limb is slightly extended.

By bending the knee and using the leg as a lever, we shall find that in fracture the thigh is more easily rotated. The limb is seldom much shortened, not more than an inch or two (and in fact without laceration of the capsule this cannot occur to any extent); the toes are uniformly turned outwards, and this is easily understood by referring to the powerful rotators, to the weight of the foot and natural inclination of the limb. Thus it may be easily distinguished from dislocation in the thyroid hole, in which the limb is lengthened, and the toes turned out; neither can it be mistaken for dislocation backwards and upwards, in which the head rests on the back of the ilium, the limb is shortened, and the toes turned inwards.

The position of the toes will enable us to distinguish this accident from luxation of the head into the sciatic notch. In this species there is little shortening of the limb, and the toes are turned slightly inwards. It may also easily be known from dislocation of the head forwards from examination of the part in the groin. But from inflammation of the ligaments after an accident, and luxation, in

which the head points towards the cotyloid cavity,
and the trochanter major lies on the back of the
ilium with little change on the length of the limb,
and with the toes turned out, it requires an ex-
perienced eye and hand to form a proper and correct
diagnosis.

The history of the previous occurrences will
easily enable us to discriminate between morbus
coxarius, or separation of the head and shaft by this
disease from fracture of the neck.

Treatment.—With regard to the possibility of a
bony union taking place in this situation much
difference has existed amongst surgeons. Some
have even gone so far as to deny that ossific matter
can be deposited into fractures extending into joints.
Against this assertion many proofs may be brought
forward, such as the union of the patella and ole-
cranon, and of fractures extending into the joints of
all sizes and kinds in the extremities. We find
frequently also depositions of bone within the cavi-
ties, as in the humerus and ulna; and in the knee
ossified tumours connected or not with the synovial
membrane are found. Thus it is seen that the
lubrication of a part with synovia is no bar to the
formation of bone. In the fracture of the shaft of a
bone, the extravasation of fluids, and hardening of
the soft parts from this cause and inflammation to-
gether, bind the extremities closely together, and
thus favour the union ; still, however, the connecting
medium (in the first place soft and vascular,
gradually assuming the nature of cartilage, and in
which ossification takes place) is poured out from
the vessels of the broken ends. It has been supposed
that the head of the thigh-bone had not vitality

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enough to go through this process, but we know now that vessels comparatively large are destined to supply it. We find, on examining fractures in which the process of union has been interrupted by any cause, as at sea in case of rough weather, or in experiments made on animals in which bones are broken and moved repeatedly during the cure, that when nature, as it were, is balked in her first attempt to produce bony union, she covers the ends with a smooth substance, and an artificial joint is the consequence. These I have very frequently seen on animals and on the human body almost in every bone. In old subjects, where the powers of life are low, this is a very common occurrence. The same causes operate powerfully in fracture of the neck of the femur to prevent a proper union. The difficulty of producing a reunion does not depend by any means so much on the adaptation of the parts to one another as on the inefficacy of the means usually employed to retain them. So much is this the case, that some of these means have been compared to the plans adopted by anatomists in their experiments on dogs for the formation of artificial joints; and from such treatment being general, many are the beautiful preparations of artificial joints betwixt the head and neck of the femur in the museums of this country. The cases are but few in which union by ossific matter has taken place, and has been satisfactorily proved.

But it is unnecessary for me to dwell on such a well-known circumstance in a paper intended for so enlightened a body as the Royal Medical Society of Edinburgh.

Mr. Pott's method of treating fractured femora

by bending the thigh on the pelvis, and the knee on the thigh, is quite inapplicable here, as is also any plan in which flexion or motion of the joint is allowed. If a union is to be expected, it is only by keeping the broken parts firmly and accurately adapted to each other for a sufficient length of time. We have the means of ascertaining their complete adaptation, by comparing the thighs and legs with one another, and attending, at the same time, to the direction of the toes. If we are obliged from retraction, owing to the insufficiency of the apparatus employed, to replace the fracture every day or two, we shall but repeat, and that to their utmost extent, the experiments I have formerly alluded to, and the result will be the same.

Surgeons—at least many of them—are so fully impressed with the idea that union by ossific matter is out of the question, that they are not at the trouble even to make the show of trying a cure. If they do try it, the means employed are so unequal to the object that, in either case, the termination is the same. The patient is disabled by a useless limb which he is obliged to drag about for the remainder of his life. The great superabundant secretion of synovia after an injury of the hip joint has been supposed by Mr. (now Sir) A. Cooper a principal cause of the infrequency of union in cases of fracture within the capsular ligament. Laying it down as a rule that it is better for the patient to be contented with a disabled limb than to submit to the confinement necessary on the attempt to produce a bony reunion, this excellent surgeon recommends the patient to be moved out of bed after the subsidence of inflammation, and set on crutches as

speedily as possible. In this way, by the strengthening of the capsular ligament, and smoothing off of the fractured ends, the patient can, by-and-by, leave off any support, but remains with a very insecure joint, and but an unserviceable limb. I question much if the capsular ligament can be extended so much with fluid, and that suddenly, so as to force the fractured ends of the neck far from one another. We see large collections of fluid in membranes somewhat of the same nature, as in the tunica vaginalis or in the peritoneum, and even in those of the same kind, though more rarely, as in the hydrops articuli taking place in the knee, ankle, or any other joint, but these distensions are not produced in a day, nor yet in two or three; it is the business of many weeks, months, or years.

That collection takes place in the capsular ligament there can be little doubt, and if any fear is entertained that in this way the bone may be separated, the sooner means are adopted for its prevention or removal the better. I suspect Mr. Cooper will find in the inefficacy of the means employed to retain the bones in accurate apposition a better reason for the great infrequency of bony union of the neck of the femur. The means he recommends for fractures in this situation (the short splint and belts to wit) are liable to the same objections as those I have mentioned above. I hope no one would be simple enough to leave the limb without any attempt to replace it, as is recommended and practised by many surgeons in fractures, whether simple or compound, of the limbs. They are regulated by the notion of, in the first place, subduing the inflammation before reducing and

securing the fractured bones,—a very pretty fancy indeed ; and this they propose to do by laying the limb on a pillow, smooth and neat, applying evaporating lotions, and, if need be, leeches. All this is done from a superficial view of the injury. They combat an evil which it is in their power to prevent almost entirely. Whilst they are watching the symptoms, and palliating them by cool cloths, etc., they are but watching till the soft parts, irritated and torn against the fractured and unequal ends of the bone, become violently inflamed ; until the patient, with all the symptoms of inflammatory fever, becomes delirious, leaps out of bed, and stalks through the ward, to all likelihood forcing the bone through the integuments. Then the poor victim must be bled and leeched, his head shaved, and, perhaps, after profuse suppuration or gangrene of the limb, recovers of a fracture in the first place simple (if you can call it a recovery), with the loss of a limb, and what is worse, deformed by a useless and unseemly mass in lieu of it.

In my humble opinion, there is no reason why a fracture of the neck of the femur should not unite as well as any other, when put in circumstances favourable to such an occurrence.

The limb must be kept steady with regard to the trunk, and made, as it were, part of it, so that not the slightest motion can take place in any part of it. The fractured extremities should, at the same time, be firmly compressed against one another, that separation by any cause may be effectually prevented. The limb, at the same time, must be permanently extended, and in such a manner that no change of position be required. These indications, necessary

in every case of fracture, require to be observed with the greatest strictness in this case.

No means with which I am acquainted are sufficient to effect all these purposes, excepting the employment of the apparatus recommended by Desault, or what is far preferable, that used at St. George's Hospital in London.

The patient is laid on his back, the two limbs brought to the same length by the extension of the injured one, of which the toes are also directed upwards. The affected limb is then rolled equally from the toes to the groin with a moderately broad bandage. The long splint with a foot-piece, and which should extend from the heel to the short ribs, is laid along the outside of the limb. This is retained by a roller to the foot, and by slips of bandage made to apply to the limb. After it is firmly attached, a band is passed under the perineum through a slit in the top of the splint, and by the tightening of this the limb is to a certainty kept in its proper place. If swelling comes on, the bandages must, of course, be slackened a little. This apparatus must, of course, be retained six or seven weeks; afterwards, slight motion employed frequently to prevent stiffness from adhesion of the ligamentous apparatus or disuse of the muscles. When the limb is thus secured, and every joint rendered immovable, the patient can be gently raised for any evacuation or other purpose, without risk of displacement or motion of the fracture. At some future period I may present to the Museum of the Royal Medical Society a specimen of the apparatus, and, if I should be so fortunate as to obtain it, a preparation of the reunion of such a fracture.

I may here notice that another cause of the great scarcity of preparations of reunion arises from accidents of the kind occurring in old subjects generally, in which fracture in any situation almost would with difficulty unite.

IX

JAMES SYME

ON CARIES OF THE BONES

Read 1821

MR. PRESIDENT,

The truly deplorable ignorance of many surgical practitioners regarding the diseases of bones is most astonishing, especially when we consider the frequency and importance of these affections. I am convinced that the trepans and rugines of our fathers in surgery, though often misapplied, did, upon the whole, much less mischief and infinitely more good than the poultices of our brethren of the present day. Indeed, it appears that the bones occupied a much larger portion of surgical attention formerly than they do at present. It is in this way only that we can account for the present prevalent ignorance and injudicious treatment, which is the less excusable, considering the more accurate notions we now possess of the animal economy.

In the following observations I mean to direct my attention chiefly to that affection of the bones generally known by the name of caries. I should rather say, which is properly understood by this

term, for caries, to many surgeons, is synonymous with disease, and conveys no precise meaning of the particular morbid action to which it is applied in correct language. It is to this very extended use of the expression that much of the confusion and uncertainty of surgical practice in the affections of bones is to be ascribed, for it is by confounding the serious and obstinate disease of caries with really healthy actions which do not require the assistance of art, that surgeons are led to a feeble and inert practice.

It therefore appears to me that I shall do well if I succeed in characterising distinctly the carious affection ; but before making any attempt towards this desirable object, it will be necessary to take a rapid and general view of the different actions, healthy and unhealthy, to which bones are liable.

Although the diseases of bones may appear to a careless observer almost endless and complicated beyond the powers of description, they are found upon investigation to be few and extremely simple. They are all preceded by inflammation, and are the terminations of this action ; of course they vary with the degree of it, and with the constitution of the part or patient in which it occurs.

The most gentle action carried on in bones is the adhesive inflammation, or that which provides for the union of simple fractures. We know that there is one set of vessels constantly pouring out new bone, while there is another as constantly removing it. All that seems necessary in this stage of inflammation, then, is that the activity of the former should exceed that of the latter-mentioned vessels. The only irritation necessary for commenc-

ing this action seems to be the stimulus of imperfection, as it has been called—in other words, the want of due strength felt by the system.

I see no reason why the broken ends of a bone, when accurately placed, should not unite by the first intention, just as the soft parts unite directly when divided. I do not mean to say that the earthy parts will unite, but that the soft or cartilaginous basis of one end will unite with that of the other. This idea appears the more reasonable from two cases which happened to come under my immediate care.

The first of these was a transverse fracture of the humerus near the middle. Six days after the accident, the patient, who was a stout, middle-aged man, raised his arm and declared that he felt it strong. The second case was a fracture of the ulna near the elbow ; the patient, without my knowledge, removed the splint on the twelfth day and found the arm quite strong,—so much so, indeed, that I caught him using it for buttoning his clothes and sweeping the floor.

When the fractured extremities are not accurately adjusted, then a large effusion of bony matter takes place from both, so as to patch up the fracture. The ancients, and, I am sorry to say, some of the moderns too, wishing to give nature the credit of their own clumsiness, have considered this effusion as a mistake of hers, and, calling it a redundance of the callus, have attempted to restrain it by tight bandaging instead of accurate adaptation.

The same action, viz. a simple effusion of bony matter, is excited by different stimuli, either constitutional or local. In nodes we have an example

of bony effusion caused by constitutional irritation. The new matter here is not thrown out upon the surface of the old, but is placed between its layers. To satisfy ourselves of this fact, all that is necessary is to saw up a bone on which there is a node longitudinally; the laminæ of the sound part may then be traced through the enlargement. Effusion of new bone as the effect of local irritation is a very common occurrence. Any gentle stimulus, either idiopathic or symptomatic of abscesses, or death of the internal parts, is sufficient to occasion it.

The next degree of inflammation produces an effect precisely the reverse of this last—this is ulceration. The irritation acting here is generally some sort of pressure, as that of an aneurismal tumour, confined matter, or the like. This action cannot be considered morbid; it is merely an effort of nature to relieve herself, and consists simply in the activity of the absorbents exceeding that of the exhalents, the consequence of which is that the external lamina is removed, so that the cancellated structure of the bone is exposed; but the surface is healthy, and pours out new bone to repair the breach as soon as the offending cause is removed.

The next degree of action is that which has been called the suppurative inflammation, because the formation of pus is uniformly an effect of it. Any part of a bone may enter into this action : sometimes the medullary canal is the seat of it, and thus we have the disease called spina ventosa; at other times the substance of the bone is affected, and then abscesses form; lastly, the surface itself may be concerned, as often happens in the joints. After the matter has formed and is evacuated, then granu-

lations, provided the bone is not too much exhausted, spring up and repair the loss of substance which may have been caused by the disease. If two surfaces in this state are opposed to each other, as is the case in joints, they unite, hence the frequency of anchylosis.

It appears, then, that bone is formed in two ways, viz. either by the vessels themselves, their actions being excited simply, not changed; or through the medium of granulation. The first method of regeneration takes place to a greater extent in bones than any of the other tissues; the second they possess in common with all the others of the body.

The process by granulation goes on much more slowly than the other, hence the importance of healing the external wound in compound fractures directly, as by this means we lessen the risk of the bones taking on the suppurative inflammation, which they almost certainly do when the soft parts do not adhere by the first intention. But when any man writing a system of surgery declares that bones will not unite as long as they are exposed, we must suppose him to be either blind or obstinately stupid.

The next stage of inflammatory action so impairs the living powers of the bone as to incapacitate it for any healthy process without absolutely destroying it. It is this condition of a bone which the word caries properly expresses. The disease appears under many and various modifications, but is always essentially the same, follows the same course, and demands the same treatment.

Lastly, the inflammation may be so intense as

to kill the bone outright, and then we have the disease called necrosis.

From what has been said it will, I think, be evident that there are many processes carried on in bones which are really healthy, and must not be disturbed by the interference of surgical art. The carious affection is not of this description. On the contrary, it is a disease of the utmost obstinacy and malignancy, uniformly requiring the assistance of art. I wish, therefore, to characterise it so clearly that the merest surgical tradesman may never mistake it for a healthy action. While making distinctions it will not be amiss to observe that surgeons may be divided into those who practise their trade merely, and those who also study their profession. To the former already established in practice I certainly have not the presumption to expect I shall be of any use, for men who do not think, and will not be convinced by what they see, are truly in a hopeless state. But to those gentlemen who constitute the latter division, and to the rising generation of the former, the following observations may not be altogether useless.

Caries for the most part appears in young subjects, and in those particularly of a scrofulous constitution. The inflammation which precedes is generally caused by strains or other injuries of the soft parts, but occasionally appears without any assignable cause. When this is the case an abscess in the bone generally forms first, the walls of which, from the intensity of the inflammation, are left by no means disposed for a healthy action.

During the formation of the abscess the patient is racked with intolerable deep-seated pain. The

integuments suffer little change ; generally they are a little puffy. At last the matter of the abscess by its pressure causes absorption of the bone, and thus establishes an outlet for itself. The soft parts are then elevated into a tumour, an opening into which is effected either by nature or the surgeon's knife. The integuments now subside, the edges of the opening become callous, and a constant, thin, fœtid discharge issues from the morbid bone.

In the other case, that is when the disease is propagated from without, things go on much more rapidly. The abscess in the integuments forms quickly, and the destruction of bone generally occupies a larger surface, though it does not penetrate to such a depth.

Caries has been divided into different kinds, as the cancerous, worm-eaten, phagedenous, etc., but these distinctions do not appear to me at all necessary, as I believe the proper treatment of caries to be uniformly the same.

It is difficult to describe the appearance of the carious bone, as it varies with the parts in which it occurs, the cause which gives birth to it, and probably many other accidental circumstances so trivial as to escape observation.

Whenever we find a bone, which for some time previous to our examination has not been subjected to pressure, deprived of its external shell so that it feels rough, and allows the probe to be pushed into its substance, we may safely conclude that it is in a carious state, especially if it can be ascertained that an inflammatory action has been carried on within it. The grand distinction between an unhealthy and a dead bone—at least as far as regards their

diagnosis—is the want of the external lamella in the former, and the presence of it in the latter. For, as the cancellated part of a bone is much more vascular than the laminated, it is evident that the latter portion will fall a victim to such a degree of violence as would only debilitate the powers of the former, hence the frequency of necrosis in the shafts of bones and caries in their heads; but spongy bones, and the heads of long ones, have a dense external shell, which cannot resist any more than the shaft; they therefore lose this covering of theirs at the first attack of inflammation.

A carious bone, when macerated very closely, resembles a piece of sugar which has been immersed for an instant in boiling water, and then held up so as to allow the dissolved parts to drain from it. While still adhering to the body it rarely possesses this appearance; indeed, we can seldom see its structure at all, as spongy, ill-conditioned granulations fill its interstices and thus conceal it, so that all our information as to the state of the bone must be obtained from the probe.

Caries, when once fairly established, shows little tendency to put a stop to itself. It is not uncommon to find a carious bone remain in almost identically the same state for a series of years. The disease is not only indolent in itself, but resists all the efforts of the system to shake it off. The bony matter destined to supply the wants of the extremity is almost all carried to the breach, leaving the bones soft and spongy, and is there thrown out in the form of foul discharge. The system takes alarm, more and more nutritious materials are sent to the part, until nothing is retained but what

is barely sufficient for carrying on the processes of life. But all is to no purpose, for as soon as the healthy fluids enter the diseased vessels they are instantly converted into stinking sanies. Nature, finding herself foiled in her attempts to repair the breach, throws up a wall of new bone around it. Though in this way the disease is prevented from spreading, it is by no means extinguished. When of any considerable extent, the patient's health suffers cruelly from the constant and copious discharge, and, if as yet he has not attained his full size, his growth is often arrested; thus it is not uncommon to see a young man of eighteen or twenty look like a boy of ten or twelve.

Provided, however, the disease be not very extensive and the discharge consequently moderate, the system gets habituated to it, and often, as has been already stated, supports it for many years.

It does occasionally happen, though certainly very rarely, that the part of a bone which has been long the seat of caries, at last from an attack of inflammation or any other cause, dies and is cast out, immediately upon which healthy granulations spring up all over the surface of the bone, and thus a cure is accomplished speedily and effectually. This I believe to be the only way in which a natural cure of caries ever takes place, an event, indeed, which I am convinced is one of the rarest in surgery. I know very well that men possessing a considerable portion of popular confidence assert that they have seen many cases of caries get well under the action of poultices, but I think there is reason for believing that these good people have mistaken very different actions for the one under

consideration.   At least I have seen men who
erroneously supposed themselves acquainted with
their profession, and had even the art of juggling
their patients (patients indeed !) into the same way
of thinking, arguing from cases entirely uncon-
nected with caries, and resembling it only in their
confused and perplexed imaginations.

It is difficult at first sight to discover the cause
of the extreme obstinacy of caries.   The phenomena
presented by ulceration of the soft parts explain
the difficulty in a very satisfactory manner.

When a breach of continuity occurs in the soft
parts of an individual possessing a weak or un-
healthy constitution, the attempts at reparation
are always feeble, slow in their effects, and often
altogether abortive.

The same tardy progress towards a cure is
noticed when the cause productive of the injury is
of such a nature as to weaken the vital powers of
the surrounding parts.   The ulceration produced by
fire is of this kind.   Every one who has seen the
moxa applied must have noticed the extreme slow-
ness with which the resulting sore healed,—a cir-
cumstance to be explained, I should think, by
supposing that the parts surrounding the ulcer
have been stimulated beyond their powers.

We are further assisted in our inquiries into the
nature of caries by attending to the very different
effects produced on long and short bones by a
similar moderate and continued irritation.   When
an ulcer is kept up for a length of time over the
shaft of the bone, as the shin, and is irritated by
frequent injuries, it sometimes happens that the
bone exfoliates, at others gets into a carious state.

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When, again, an ulcer happens over a vascular bone, I have observed (and to the best of my knowledge the observation is new) that the bone, instead of exfoliating or getting carious, may simply have its vessels stimulated to an increased action, so that instead of displaying a loss of substance it is much increased in surface, and seems everywhere as if shooting into stalactites.

But although by transferring our reasoning on the processes of the soft parts to those of bones, we may to a certain extent account for the obstinacy of caries, the extreme pertinacity of the disease is still somewhat perplexing; hence some have supposed that a peculiar morbid action is induced, and this idea derives some support from the observation of Boyer, that any stimulus applied to a carious bone, not powerful enough to destroy it altogether, only adds strength to the disease.

To whatever cause the obstinacy of caries may be owing, there can be no doubt that it is a disease of the utmost perversity; so much indeed as to resist but too successfully the well-meant exertions of nature. Finding such to be the case, then, it becomes us to lose all reserve and act boldly and decisively, knowing that the only chance the patient has of a cure is in our hands.

The practice to be followed is exceedingly simple and obvious,—indeed, nature points it out clearly to us. Whenever she succeeds in obtaining a cure it is by putting to death the ill-disposed bone. We have therefore only to attempt the same, and if success attends our efforts, a radical cure will be the reward of our labours in every case. If the patient has been too long trifled with before we are

called in to his assistance, so that his health is ruined and his strength exhausted, amputation becomes the only, though cruel, means of relief. But here it is particularly necessary to guard against rashness; for although the strength of the patient may appear worn out, the simple removal of the diseased action is often sufficient to restore him to health and strength.

Caustics of every description have been, and still are, employed for completing the destruction of the sickly bone. Of these the lunar caustic, potash with lime and eau mercurielle, or nitric acid saturated with mercury, are the best. But it would be wasting time to dwell upon them as long as we possess that most valuable agent, the actual cautery, which is much more effectual, more easily applied, and less painful to the patient; but which at the same time, I am sorry to say, has in the hands of injudicious practitioners done infinitely more harm than good. For, when used to repress a diseased action, it uniformly strengthens it, and is never of any advantage except when pushed to such an extent as to insure the certain and complete destruction of the diseased portion.

But although the red-hot iron may often be sufficient of itself to accomplish all we wish, especially if the disease be of no great extent, it is necessary to conjoin with it other means. When we encounter the disease of greater strength, rasps, rugines, scraping, trepans, pieces of glass, and many other contrivances of the same sort have been recommended for removing the bulk of the diseased bone, so as to allow of the more effectual application of the cautery. Chisels, flat and grooved, will, I

should think, from the use I have seen made of them by my friend Mr. Liston, be found fully sufficient for digging out the rotten bone. When the metacarpal or metatarsal bones are affected, it is a great saving of trouble to remove a portion of the entire cylinder. Various instruments have been contrived for this purpose; of these may be mentioned the metacarpal saw, the half-headed trephine, chain saw, and Machel's saw. The inconveniences of these instruments are so obvious that it is hardly necessary to particularise them. The metacarpal saw can, it is very evident, be applied only to a bone which is freely exposed on all sides. Now the exposing of a metacarpal or metatarsal bone is by no means an easy matter, and necessarily implicates a very extensive separation of the soft parts. We may judge of the difficulty, I should rather say impossibility, likely to be encountered in cutting through a small irregular bone with a half-headed trephine, from the trouble which attends the use of the common full-headed trephine even on a flat bone.

The chain saw I am inclined to think, both from the testimony of others and my own experience, never succeeded in cutting through a bone ever since it made its entrance into this inventive world. And as to the saw of Machel, I am ready to allow that it is a very pretty mechanical contrivance, but how it should enter the head of any man that such an apparatus could be of any use in surgery excites my astonishment in no small degree.

The surgeon I have already mentioned, Mr. Liston, finding the inconveniences just enumerated,

bethought himself of using cutting pliers for this purpose.   He accordingly had some made of different shapes and sizes, since which time he has never experienced any difficulty in removing meta-carpal or metatarsal bones, either in whole or in part.

I have now finished the account of what appears to me the truth regarding caries, and shall next proceed to give some cases in illustration of what has been advanced.

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X

ROBERT CHRISTISON

1797-1882

ON THE CONTAGIOUS NATURE OF THE BRITISH CONTINUED FEVER

Read 1822

MR. PRESIDENT,

It was not till near the close of last century that physicians began to form precise notions of the nature and properties of contagion. About that time their attention was directed towards assembling and arranging together a series of facts relative to the diffusion of continued fever and other infectious disorders. Through a careful generalisation of these facts, and by drawing from them cautious and legitimate inferences, they were led to the discovery of certain doctrines which have since been promulgated under the title of the Laws of Contagion. More lately they have received several additions and corrections. But much yet remains to be done before they can merit the high title with which they have been dignified.

These laws of contagion, so far as they regard continued fever, I propose in the following essay to try by our most recent experience. I shall endeavour to show how far our knowledge of them may be

extended by the observations made upon the epidemic fever that not long ago laid waste the British Islands. The opportunity which has been thus afforded to judge of the tenets of our predecessors is peculiarly fitted for the purpose. For we have been furnished with careful records of its course from almost every part of Britain which it visited ; and there has scarcely ever been an epidemic whose history was described so purely from observation, whose progress was observed with so little bias from theory.

At the outset of this inquiry I shall examine the preliminary questions, whether the continued fever of the British Isles be generated by contagion, and whether contagion be its sole cause ; I shall next consider some questions that spring from these ; and lastly, I shall compare the generally received laws of contagion with what has been observed during the late epidemic.

That we may prove the contagious nature of continued fever it is not enough to quote, as many do, even innumerable instances of entire families seized with it about the same time. Such incidents indeed serve to show that the sufferers have been simultaneously exposed to some common cause of pretty general diffusion. But instead of that being an emanation from the bodies of those affected by the fever, it may be (as the opposers of the doctrine of contagion assert) a peculiar state of the atmosphere, terrestrial exhalations, or a peculiar mode of living, conjoined perhaps with exposure to cold, wet, fatigue, and the like.

It has been frequently found, however, that fever is rapidly propagated in families if those

collateral circumstances are allowed to continue by
which contagion is known to be fostered, but by
which a peculiar constitution of the atmosphere, or
any other great common cause, cannot be affected
in the least. On the other hand, it is equally well
established that the progress of the disorder will be
checked by resorting to such means as dilate, destroy,
or counteract contagious effluvia, but have no power
whatever in controlling any of the other possible
causes. These principles are admirably illustrated
by the tables of Dr. Haygarth in his work on the
prevention of infectious disorders. I may add that
our experience in the late epidemic of this city
amply corroborate them. A very large proportion
of our cases of fever proceeded from certain lodging-
houses in the Grassmarket and Cowgate, occupied
for a night only, by persons arriving in search of
employment. From these places a regular supply
was kept up, till the districts where they lay were
brought under strict superintendence, and not un-
frequently the whole inhabitants of some of them
were ill at one time. But afterwards, when they
were carefully watched, and disinfected so soon as
a single fever patient was discovered in them, the
numbers decreased greatly, and the disease in some
of them was even completely extinguished.

Another circumstance in the spreading of these
epidemics which establishes their contagious nature
is, that while they rage in certain parts of a great
town, they are often utterly unknown in other
districts not very remote, laid out in a manner pre-
cisely the same, and occupied by inhabitants of the
same rank and of similar habits. And as a further
proof that this cannot depend on any peculiarity of

situation, it is found that the exempted districts, when once invaded by the disease, suffer as severely from its ravages as those previously attacked.

But the most decided argument in favour of the contagious nature of the British continued fever is drawn from the circumstances under which it attacks the upper ranks of society. It is known to attack chiefly the very poorest orders, and in many parts of the country it was never seen among the richer inhabitants at all.

During the recent epidemic several very striking facts were observed with regard to its appearance in the middle ranks of this city. I shall notice minutely one of these, because I conceive it to be absolutely irreconcilable with any other doctrine but that of contagion.

While the epidemic was at its height, namely, betwixt the end of 1817 and beginning of 1820, the fever prevailed very much among the gentlemen of the resident medical establishment in our hospitals. Of six resident clerks in particular not one escaped who remained long enough, insomuch that among them and their substitutes sixteen cases of fever occurred within eighteen months. The form under which it attacked them possessed precisely the epidemic constitution that appeared among the hospital patients, and in the infected districts of the town. I mention this resemblance because it follows that both in them, and in the poorer ranks, the disease was the same, and therefore probably arose from the same common cause, whatever that cause may be.

The sum of the argument is this : that a disease identically the same with that raging among the

poor prevailed no less among a certain description
of the better ranks, who were very strongly exposed
to contagion, but in no peculiar manner to any other
supposed cause; and that all others of the same
rank, of the same habits and constitution, their own
families, their friends, their companions—provided
they were not exposed to the infection—were abso-
lutely and completely exempted.

The next point we have to examine is, whether
contagion be the sole cause of the British continued
fever. This question is still involved in obscurity.
The opinion of physicians upon it seems both
fluctuating and ill defined. Little importance has
been attached to the ordinary mode of proving that
other causes may exist besides contagion.

For example, it has been said that even during
the prevalence of an epidemic some cases cannot be
traced to contagion. But there are several sources
of fallacy which render that fact inconclusive, at
least in the way in which it is usually applied.
For in the first place, during a period of public
alarm, many illnesses, and even deaths, especially
in the better ranks, are ascribed to fever, with
which, however, they had really no connection; and
it is no wonder that then the source of infection
cannot be discovered. And secondly, many persons
in the lower ranks absolutely forget that they have
been exposed to the contagion; while others of all
classes may be exposed without their knowledge, as,
for example, in chairs and hackney coaches that have
conveyed fever-patients, and never been disinfected.

Further, it is alleged in proof that fever must
spring from more than one cause, that it is fre-
quently impossible to tell whence epidemics have

been derived. But this, though a very frequent, is also a very unnecessary subject of dispute; for in every great city it constantly lurks somewhere in sufficient force to keep itself alive till a season comes round favourable to its propagation. In Edinburgh, which is but little exposed to epidemics, fever is hardly ever wanting. It may often, indeed, be so rare that the public are not aware of its existence, but it is not the less known to those extensively engaged in practice among the poor.

There is seldom any necessity for placing the origin of an epidemic anywhere else than in the city itself where it prevails. The impossibility of finding a foreign source for it is scarcely any argument in favour of its spontaneous origin.

Lastly, it is said that fever has often been actually observed to arise from other causes, such as cold, fatigue, passions of the mind, insolation, hunger, intemperance. But it is objected that we can seldom make sure of these persons not having been also exposed to contagion; and it is added that the causes I have mentioned, whenever their action is obvious and unequivocal, engender diseases totally different from continued fever.

Yet, after all, some of these arguments possess considerable weight when viewed in conjunction with the laws recently discovered by which contagion propagates itself. Thus it will be shown that, in order to render the contagion of fever effectual, some strong, well-marked exposure is requisite; which, therefore, few can forget or unknowingly sustain. Hence if the cases untraceable to contagion be very numerous, it is almost certain that in some of them at least this has not been the

real cause. A few individuals may have been actually exposed to the extent I have hinted at, yet owing to their ignorance or forgetfulness the fact may escape our search; but it is impossible that this can hold with regard to so great a number as are unable to refer their illness to contagion. This reasoning is still more plausible if the same difficulty be encountered during an epidemic, since actual exposure is then much more readily detected. Now even while fever rages epidemically a vast number of cases are thus obscure in their origin. On consulting the tables of Dr. Welsh it will be found that the inquiry was made in 546 cases, and three-fifths only were traced to contagion.

For the same reason, when several authors tell us they have seen numerous cases of fever arising apparently from the effects of cold, fatigue, and the like, without any ascertainable exposure to contagion, we may be allowed to infer that some of them have arisen in the manner alleged, since an effective exposure cannot often escape notice or be forgotten.

An observation, which seems of some weight in determining the point, was made during the late epidemic in Edinburgh, namely, along with the more common kind of fever, manifestly contagious in its origin, there prevailed also another variety, differing in its characteristic symptoms, in its duration and event, in its phenomena under the action of remedies; and this variety could be very seldom traced to contagion.

That variety which originated in contagion was almost always a very acute synochus. It generally made its attack with great violence, often terminated soon, and suddenly, by critical sweats, or

about the end of the second week receded gradually or assumed a typhoid character, and then sometimes proved fatal. In a vast proportion it lasted only eleven or fourteen days, and very often not longer than five or seven. It was commonly accompanied by strong signs of local inflammation. At all times it was very much under the control of art, insomuch that of the hospital patients in 1811 scarcely three in a hundred died.

The other form of the disease began slowly, so that seven or eight days might elapse before the patient had to confine himself to bed. It was never acute in its character; the pulse was full, soft, and rarely exceeded a hundred; the signs of local inflammation were mild ; the appetite after the second week was good ; and about that time, too, there appeared a kind of confusion, or heavy stress, which attended the disease throughout ; a mild but very untractable diarrhœa often came on about the same time. Its course was very tedious, seldom shorter than three weeks, often more than twice as long, and, in one case I knew, it was prolonged to fifty-two days. There was never any crisis or critical evacuation, so common in the other variety, and all the customary remedies were tried without any being found which had any power to control it.

Little or no difference could be discovered in the persons attacked by these two forms of disease. Those who laboured under the slow variety were universally young, robust, and plethoric.

Now, while the ordinary fever was for the most part easily referred to contagion, this more chronic kind could seldom be tracedto it satisfactorily, and often not at all. In the autumn of 1818 many cases

occurred among the country people employed at the harvest in this neighbourhood. Several cases of the kind also occurred in the uninfected districts of the town, and could as little be traced to contagion. The strong circumstance is, that a fever of a peculiar type, differing from the usual form of the truly contagious kinds, could seldom be satisfactorily shown to spring from contagion, and very often seemed wholly unconnected with it. It is almost of necessity therefore it should have sometimes sprung from another cause.

It must be acknowledged, however, that towards the close of the epidemic it sometimes originated manifestly in contagion. I have not myself seen distinct examples of this, but in the spring of 1820 five persons of one family were admitted into the clinical wards of the Infirmary, all labouring under a slow nervous fever. The infection, too, was apparently derived from a case of the same nature. But such cases were rare, and they will hardly invalidate the conclusions I have drawn from what was observed during the rise and height of the epidemic.

Another circumstance by which this opinion might derive confirmation, would be the continuance of the slow fever after the contagious varieties disappear. I have had too little opportunity, however, of ascertaining whether or not such has been the fact.

But while the remarks just concluded render it highly probable that some kinds of fever may arise from other causes besides contagion, they by no means serve to point out what those cases are.

The researches of late authors have greatly diminished the number of the supposed exciting

causes of fever. They seem to me to have proved
in particular that no other effluvia can generate
the disease, except those from a person labouring
under it.

Again, it has been still more confidently main-
tained that exhalations from the living, healthy body,
when much concentrated and long confined, acquire
the properties of the most virulent, contagious efflu-
via. This belief, too, has been successfully combated
by Dr. Bancroft. Two very remarkable epidemics,
which occurred some time ago in England, have
been usually attributed to this cause. The first was
immediately after the Assizes at Oxford in 1517;
the other, after the Old Bailey Sessions in 1750.
They have been of late ascribed to the natural
effluvia proceeding from the prisoners previously
pent up in close, crowded rooms. Both were
violent to a degree unexampled in England; few
who attended the Courts having escaped the
disease, and few who took ill having survived the
attack. But there is no decided proof that either
of them arose in the manner alleged.

Very recently the doctrine of the identity of re-
mittent and continued fevers has been taken up by
Dr. Armstrong, and defended, not by an appeal to
direct facts, but by a comparison of the essential char-
acter of the two distempers. He affirms that if their
essential symptoms are identical the diseases must
be the same, and have one common origin. Granting
the validity of this assumption, he has founded the
proof of the identity of their symptoms on fallacious
principles; for he holds their essence to consist in
the origin of local derangement. 'The combined
affections,' says he, 'of the brain, lungs, liver, lining

of the air-passages, and alimentary canal, together
with a peculiar lassitude and languor, are the true
diagnostic signs of the remittent and continued
forms of typhus fever.' It is hardly necessary for
me to remark that in this part of the medical world
these symptoms are held to be merely fortuitous,
to constitute no part of the essence of either disease;
that their frequent occurrence in our late fever is
attributed to a particular epidemic constitution;
that when the powers producing that constitution
are not in activity the symptoms in question do not
occur. The disease is purely a general affection.

Unconnected with the generation of effluvia,
there is another class of agents, whose power to
produce fever is almost universally believed by the
vulgar. The chief of these are cold combined with
moisture; heat, especially when so applied as to pro-
duce insolation; repeated fatigue, whether of body or
mind; long-continued anxiety, and other mental
emotions; hunger, intemperance, and some others
of less consequence.

The precise operation of these agents has not yet
been investigated. They are believed by many,
however, to be no more than predisposing causes, in
which capacity their action is certainly both energetic
and unequivocal. And, moreover, it is alleged that
when they perform the part of exciting causes they
produce, not continued fever, but some one or other
of the phlegmasiæ.

We have not data enough to encourage us to
enter minutely into this inquiry. Yet I think it
may be shown that at least some of them, especially
in combination with each other, have produced
diseases which differ in no essential points from

continued fevers dependent on contagion. One
cause only may be noticed briefly by way of
example, namely, fatigue, frequently repeated, and
combined either with insolation or with mental
anxiety. The former of these combinations seemed
to be the cause of the slow fever, which, I have
observed, was prevalent in the autumn of 1818
among the shearers around Edinburgh.

The next inquiry that naturally presents itself
is the following : Supposing that continued fever
may originate in the causes I have mentioned, with-
out the aid of contagion, do the cases so originating
ever prove infectious ?

I believe we are not yet prepared with the
means of deciding this question. Dr. Bancroft,
indeed, has thought to decide it upon pure hypo-
thetical reasoning, disguised under an appeal to
common sense. He considers it unphilosophical and
incongruous to suppose that a disease propagated
by contagion can arise from any other cause. Such
a disease he calls a 'monstrous product of incom-
prehensible generation, which no one can believe
in who is accustomed to reason, and has not dis-
carded common sense.' He does not attempt, how-
ever, to show wherein the incongruity resides, and
for my own part I see none in the supposition that
a febrile disease arising from cold, fatigue, or the
like may engender certain secretions or effluvia by
which a similar disease may be excited in others.
Neither do we want the support of analogy for
such belief. There is every reason to believe that
some varieties of ophthalmia become infectious,
though they spring in the beginning from another
cause. The same remark applies even more strongly

to erysipelas, to dysentery, and perhaps also to the hydrophobia of animals.

But it is not by such reasoning that the matter will ever be satisfactorily determined. The only legitimate mode of deciding it is by selecting cases of sporadic fever, and observing what then takes place among the attendants of the sick, especially under circumstances that encourage the action of contagion.

It appears that the fever of the Oxford Assizes and that of the Old Bailey Sessions did not spread, even in a single instance, to any one who was not in the Court on the day when the disease was caught. On the other hand, however, independently of the obscurity now hanging over these events, the contagion, if any was engendered, had not full scope, for none suffered but the middle ranks, among whom we have seen contagious fever is hardly ever propagated.

As the matter stands at present, it is proper to adhere to the safest and most prevalent opinion, that fever, whatsoever its origin, may be communicated by infection. Nor should we rashly infer in any particular instance that it is not infectious because it does not spread; for not only is the concurrence of certain circumstances requisite that exposure to the contagion may be effectual, but it is likewise probable that certain terrestrial and atmospherical states are requisite to give sporadic fevers an infectious character.

I proceed, in the last place, to give a brief sketch of the laws of contagion regarding fever. They may be considered under two heads,—the

means by which it is communicated, and the powers that influence its activity.

The chief and most undoubted means of communicating it is through the application to the healthy body of certain effluvia arising from that of the sick.

It is not yet distinctly ascertained from what part of the body these effluvia proceed, or on what part their action is primarily exerted. The breath and insensible perspiration are generally most dreaded; many are likewise afraid of the urinary and alvine excretions; some attribute the most virulent power to the effluvia exhaled by newly-drawn blood; and a few believe that the infection resides only in the sensible perspiration, which must be touched before the disease can be communicated. There is, perhaps, but little reason to dread any of these sources except the breath, the cuticular exhalations, and possibly the alvine discharges. The cuticular exhalations, when long pent up, are particularly virulent. To them, at least, have been usually attributed those singular cases, where at the moment of infection the person is struck with a peculiar unconquerable fœtor, attended with sudden weakness, præcordial anxiety, and a strange consciousness of danger. This has also at times appeared to have been derived from the alvine discharges, but for obvious reasons they are much less hazardous; I even question whether they are very powerful. The water-closets of the fever wards of our Infirmary, though of a very bad construction, are daily used by the surgical patients with perfect impunity.

The lungs, as affording the most active and

delicate absorbing surface, are generally thought to be the part of the system through which the poison is introduced. There are still some persons, indeed, who believe it is conveyed with the greatest certainty by actual contact; but this opinion, once generally enough received, is now as generally abandoned, perhaps, however, without any decided proof of its incorrectness. Of course, under these ideas the air is the vehicle by which the effluvia pass from one person to another. A question here occurs, whether they are capable of being condensed upon clothes and other substances, and afterwards discharged again, still preserving their poisonous properties; in short, can the contagion of fever be transmitted by fomites?

To determine this matter, it is useful previously to make a distinction in the kind of exposure to which the supposed fomites have been subjected. Some substances are in immediate contact with the patient's body, and will therefore be deeply tainted with the cuticular excretions. These are almost universally thought capable of conveying the infection, but I believe the notion is derived not so much from fact as from analogy. Other contagious diseases, namely, of the exanthematic kind, being conveyed in this way, why should not fever also? The analogy, however, is fallacious, for these disorders cause the formation of a peculiar matter, possessing highly infectious properties, and capable of transporting them to any distance by adhering to fomites. Instances have certainly been recorded where fever has appeared to travel in the same manner, but they are for the most part unsatisfactory; and, on the other hand, there are not

wanting facts to show that many people have been
exposed to these alleged fomites without sustain-
ing injury. It is probable that in this way fever
cannot be so easily communicated as is generally
thought, and that to prove efficacious the fomes
must be applied soon after it has been infected.
Other substances never come in contact with the
skin, but are exposed at some distance to its in-
visible effluvia only. In this situation it is highly
probable they cannot be so imbued as to transmit
the disease. We know that crowds of persons are
every day liable to have their clothes so infected,
yet no one can bring forward an unequivocal
example of the disease having been conveyed in
that manner. I may mention as a strong case of
the kind, that no one ever suspected the contagion
to have been carried by the clerks of our hospitals,
even when the epidemic was most prevalent, so that
they had to spend half the day in the fever wards.

The powers that influence the activity of febrile
contagion are referable to three heads,—those de-
pendent upon the source of the infection, upon the
condition of the person exposed to it, and upon the
particular mode of exposure.

The modifying circumstances relative to the
source of infection are chiefly, the kind of fever,
the period of the disease, and the period of the
epidemic.

There are few or no observations concerning the
relative activity of the effluvia proceeding from the
different kinds of fever ; as far as we can judge,
however, the typhus gravior, now fortunately rare,
is the most active. It is a curious circumstance
that those epidemics which spread with the greatest

rapidity are also the most malignant; take, for example, the fever of the Black Assizes, or the Corunna fever. As to the potency of the contagion of the different varieties met with during the late epidemic, nothing certain is known, but it is reasonable to suppose that the most violent case would be the most infectious, because probably a larger quantity of poison would be exhaled.

On the same principle, the period of fever at which the effluvia are most virulent should be just after it is fully formed; and this seems the general opinion, though it rests upon no decided facts that I am aware of. If it be correct, we perceive the uselessness of limiting the period of inspection to a certain day, as Dr. Haygarth has done, for the fever of one may be as fully formed in two or three hours as that of another in seven, eight, or ten days. Continued fever does not appear to be, like some other disorders, infectious during convalescence. The time of day when the infection has the greatest activity is probably the evening, that is, the time of exacerbation; it is in the evening at least that the cases of sudden infection have generally occurred.

There would appear, moreover, to be a particular period in the progress of an epidemic during which the contagion is most efficacious, and this appears to be about the time when it is reaching its height. It was at this time that the gentlemen of our hospitals suffered so much. About the middle of the epidemic, when it had continued stationary for some time, many of them escaped the infection, but hardly one while it was on the rise. This tendency to decrease in activity, after continuing its ravages

for a certain length of time, seems to be one great
cause of the decline of epidemics. It would be a
most interesting object to inquire into the circum-
stances that thus temper its virulence. Some in-
fluence must without doubt be attributed to the
preventive measures put in force, and likewise to
the immunity imparted by previous attacks, but
these counteracting powers will not altogether
account for the rapid decline, and still less for the
occasional extinction, of epidemic fevers. Much will
still remain to be attributed to the mysterious
operation of unknown terrestrial and atmospherical
influences.

The next class of modifying powers relates to the
circumstances of the person exposed to the infec-
tion. These are age, sex, temperament, manner of
living, habit of exposure, and what may be called
seasoning,—the effect of a previous attack.

Age, if we may trust our experience in Edin-
burgh, operates in a way somewhat different from
common belief; advanced age offers little or no pro-
tection, infancy is certainly protected against the
contagion.

It is difficult to estimate the relative suscepti-
bility of the sexes. More women were admitted
into our hospitals than men ; but then the former, as
they remain much in the house and tend the sick,
are far more exposed to the contagion.

No temperament, or I should rather say no
habit of body, seems to import exemption. But
some are more liable than others ; and as far as I
have observed, no habit is more susceptible than one
often met with in young adults, especially in the
middle ranks, which is marked by a combination of

health and robustness, with delicacy of external sense, and acuteness of perception. There are also some individuals who, how much soever they are exposed, are insusceptible of receiving the infection.

Manner of living is universally believed to influence the activity of infection, but it is difficult to appreciate its power, and, upon the whole, if we except that part of it which consists in careful ventilation, it is less important than many have thought. This at least is certain from what I have often seen,—when the exposure is strong and decided, no manner of living will afford much protection.

It is disputed whether habit has any power in rendering the body proof against contagion. The fact of medical men being seldom attacked with fever is a fallacious one, even if it were true, for few medical men in ordinary practice are subjected to a strong exposure. It is nothing for a person to visit a number of patients for a few minutes only, and at a time too when their apartments are well ventilated. Let such a one domesticate himself in a fever hospital, and all the habit he has acquired will hardly ever preserve him. One of my companions in the Infirmary here, was taken ill after being ten months in the heart of the epidemic, and another was affected when it had begun to recede, although it had left him unharmed for nearly two years; surely if habit was of any effect, these two persons might have escaped.

The advantage of a previous attack of fever is less doubtful. It will seldom prove a complete safeguard to any one; for, if he remains long enough in an hospital, it is difficult to say how often he

may suffer.   A vast number of our patients in Edinburgh, and a still greater proportion of our nurses, had it twice.   Fourteen months after the opening of Queensberry House as a fever hospital, three of the nurses there had been ill three times, and I was myself attacked thrice in sixteen months; but we observed almost universally that after each attack a longer exposure was endured before the contagion took effect.   For example, a violent synochus of fourteen or nineteen days, with marked typhoid symptoms in the second week, appeared in general a sure safeguard, and I never knew an instance of a person having it again after the slow or nervous variety so often mentioned.

The third and last class of circumstances that affect the efficacy of contagion relates to the mode of exposure.   These circumstances are chiefly the distance of its source, and the length of exposure.

It is unnecessary to observe that those incur the greatest danger who approach nearest the sick, and that beyond a certain distance every one is safe. An important conclusion may be drawn from this law combined with what I have said on the small danger of ordinary fomites, namely, that fever wards may be safely constructed in a general hospital at no great distance from the wards of the other patients.

The practice of our clinical physicians will even show that a few fever patients may be mingled with others in one ward without danger, but if the cases of fever be numerous,—for example, if they amount to one-half,—the infection will sometimes spread, and if a single ordinary patient be put into a crowded fever ward he will almost infallibly suffer.

Unless the exposure endure for a considerable period of time very few indeed are infected. There is a singular deviation from this law, however, in the case of sudden infection, many of those who, struck down as it were with the fœtor of a patient, have been exposed to it for a few seconds only; but these cases are very rare, and in almost all, the concentration of the effluvia has made up for the shortness of the exposure.

We cannot tell what length of exposure is requisite in ordinary cases to prove effectual, but it is probably considerable. It is thus alone that we can explain why fever never spreads in the better ranks. Our hospital clerks, who generally went home when they were taken ill, gave it to none,—an excellent confirmation of what has been observed elsewhere, for there could be no doubt that the cases were of a true infectious character. Now, even in the middle ranks, the attendants are often as strongly subjected to the effluvia as they can be in an hospital; but it is only at intervals,—so that the whole duration of exposure is limited. The same remarks apply to the pupils and dressers of hospitals; the former, I am satisfied from frequent observation, may study cases of fever without risk, provided they shun those situations in which sudden infection may be caught. The clerks and nurses, on the other hand, do what they will, can seldom or never escape, because they frequent the fever wards many hours a day, and are often obliged to remain close to their patients. Even they, although they daily imbibe a large quantity of the poison, have been rarely seized till three weeks after beginning their duty. This curious circumstance cannot be wholly

accounted for by what is called the latent period, or interval between infection and seizure, for it is probable that the latent period is generally much shorter than authors imagine. When a person catches fever from a single distinct exposure, it is observed to begin soon afterwards. In those particularly in whom the act of infection is accompanied with a strong fœtor, and sense of depression, the symptoms begin either immediately, or in two or three days at the utmost, but in the more ordinary case of a person attending a fever patient, and not exposed to concentrated effluvia, they accumulate in the body by degrees, and therefore some days are required before the dose becomes large enough to be effective.

We may now see the truth of what I was obliged to anticipate in proving the diversified origin of fever, namely, that few can forget or unknowingly sustain an effectual exposure to contagion.

Before concluding, I should remark that the foregoing observations contain less than might be expected of what others have written both upon the late and upon former epidemics. This has happened partly because the late fever of Edinburgh furnishes a stock of materials which frequent reference to authors would have swelled to an unfitting bulk, and partly because the recent writers on Domestic Fever have bent their attention chiefly to its history and treatment, and have offered little new or important with regard to its contagion.

XI

WILLIAM SHARPEY

1802-1880

ON CANCER OF THE STOMACH

Read 1823

MR. PRESIDENT,

Though cancer of the stomach is neither a common disease, nor capable of being removed by any known means, yet it is not for these reasons unworthy of the consideration of the Society; for besides what interest it may possess in a pathological point of view, it is so liable to be confounded with other complaints of daily occurrence, that a thorough knowledge of its history is highly requisite for the practical physician.

Symptoms.—This, like many other chronic disorders, is slow and insidious in its attack; it most commonly commences with a feeling of uneasiness or pain in the epigastrium, which, after some time, becomes increased in severity, and is particularly aggravated on taking food. To this pain or uneasy sensation, other symptoms are soon added which indicate a disordered state of the functions of the stomach, such as acid eructations, frequent vomitings of a limpid fluid, sickness after taking food,

and for the most part a great degree of flatulence. In addition to these there is often a sense of weight or even a shooting pain in the back and loins. The patient now begins to vomit after meals, so that a considerable part of the food is rejected with little alteration, after it has remained a short time in the stomach. During this period, sometimes, though rarely, there is a diarrhœa, but in by far the greater number of cases the bowels are obstinately constipated.

After the disease has proceeded thus far, we may generally discover a degree of swelling in the region of the stomach. This swelling is hard, in some cases movable, in others fixed, and for the most part painful on pressure. It sometimes changes its position, and from its weight, or, other circumstances, sinks down in the belly, or, from adhesions taking place between it and some neighbouring part, it may even withdraw itself under the hypochondrium, and become inaccessible to the touch.

The most distressing symptoms of the disease, which hitherto may have appeared only at intervals of days, or even months, now become constant, and daily increase in severity. The stomach grows very capricious with regard to the food, some sorts being rejected, while others, though perhaps to all appearance more indigestible, are retained. The constipation continues, and the dejections, even should they be liquid, are always in small quantity.

The complaint may have reached this length without affecting the general health to any considerable degree, but now, in addition to his former sufferings, the patient rapidly loses strength, his colour changes, and becomes of a sallow or earthy

hue, his eye sinks back in the socket, and his whole features are contracted. His body also, not receiving its due supply of nourishment, becomes thin and emaciated, or sometimes from the same cause swelled and dropsical. The matters vomited, which hitherto may not have been much altered, are now mixed with a black or brown liquid, and have a most offensive smell; the dejections also are usually of the same dark colour. About this time both the vomitings and dejections sometimes contain considerable quantities of blood, and little flocculi like shreds of a membrane. A short time before death, the costiveness not unfrequently gives place to a diarrhœa, which contributes not a little to shorten the patient's existence. The pain in the stomach now becomes very severe, and the torture on taking food is extreme.

During the whole course of the disease, but especially in the later stages, the pulse is increased in frequency during the attacks of pain, or even at last a slow fever comes on which might possibly be called hectic, but true hectic fever is rarely an attendant on this disease when it goes on without complication. At length, the patient, worn out with so much suffering, and want of sufficient nourishment, dies in a state of extreme emaciation and weakness, after the disease has existed for a period which may vary in its duration from a few months to several years. Death in a few cases is preceded by convulsions or delirium, but in far the greater number of instances it takes place without commotion.

Such is the usual train of appearances attending cancer of the stomach, but it is far from presenting in every case such well-marked symptoms.

The vomiting, for example, which has commonly been reckoned one of the most constant and characteristic symptoms, sometimes remains absent for long periods, and appears only at irregular intervals. There are other instances in which it does not take place above two or three times during the whole course of the disease, and not a few cases have occurred where it was absent altogether. Nay, we have the best authority for believing that extensive scirrhus, and even open cancer of the stomach, may be found in individuals who even to their death have never had either vomitings, pains in the epigastrium, or even any dyspeptic symptom.

It is not easy to account for the vomitings being so constant and troublesome in some cases, and entirely absent in others. It has been observed that they are not so frequent when only a small part of the body of the stomach is affected, its cardiac and pyloric orifices being unaltered. This we might *a priori* have been led to expect, as here there is no obstruction to the passage of the alimentary matters ; nor are these so necessarily and constantly brought into contact with the diseased surface. M. Bourdon has given a case where the whole of the stomach, except the cardia, was scirrhous, and where there was great nausea with desire to vomit, but this could not be accomplished. He supposes that in this case the absence of vomiting was owing to the whole muscular coat being converted into scirrhus, and hence concludes that the stomach is active in vomiting. Several cases, however, are related in Magendie's *Journal* to shew that vomitings do frequently take place though the whole of the stomach is scirrhous. M. Piedaguel, who

brings forward these cases, attempts to account for the absence of vomiting in other instances by supposing that the pressure of the abdominal muscles forces the contents of the stomach through the pylorus during the efforts to vomit, for in some cases this orifice is enlarged when scirrhous, though commonly much contracted; he also thinks that when the cardiac orifice is much narrowed, the food will be forced into the intestines by the same cause. It would be interesting to ascertain the comparative frequency of vomiting as connected with these different states of the stomach; but it is evident that no accurate conclusion on this point, can be drawn from a collection of detached cases related by different individuals, as most of these are attended with some peculiarity which induces the authors of them to make them public. It is only from a series of cases admitted successively into an hospital, or occurring to the same practitioner, that a fair average can be taken.

After having thus detailed the symptoms of cancer of the stomach in general, I shall now proceed to enumerate their principal differences, according to the part of the stomach affected, so far as these have hitherto been ascertained.

When the disease occupies the cardia there is a peculiar, uneasy, or painful sensation experienced on attempting to swallow solid food; this uneasy feeling continues till the food is thrown up, which is done by eructation, or a peculiar effort, approaching nearer to that which occasions hiccup than to vomiting, and probably depending on an inverted action of the muscular fibres of the gullet. When the food is thus rejected the patient obtains relief.

There is a similar difficulty in swallowing liquids, though less in degree, and on taking a quantity of liquid into the throat, a part often passes into the stomach with a gurgling noise, while the rest is returned. Scirrhus of the cardia also is frequently attended with vomitings of a clear fluid, which is viscid and ropy like saliva. It is but rarely that we can feel any swelling. It is remarked, however, by Dr. Pemberton, that sometimes from the great emaciation attending this form of the disease, 'a tumour surrounding the cardia may be discovered by a minute investigation in the region of the stomach.' A probang may be passed down to the stricture, and thus we have an additional means of distinguishing when the cardia is affected.

When the pylorus is principally the seat of the disease, the food enters the stomach with facility, and occasions no uneasiness till after it has entered it; it is retained for a longer or a shorter time, generally from a quarter of an hour to a couple of hours or more, and when thrown up it is by vomiting, and not by the peculiar effort or eructation, already described as attending scirrhus of the cardia. The swelling may also for the most part be felt, and its position, between the cartilages of the ribs and umbilicus, may in some measure serve to point out the seat of the disease, but this is by no means to be absolutely trusted to, as the pylorus, and even the whole stomach, are often much removed from their natural situation. When a portion of the body of the stomach is affected, the pylorus or cardia remaining sound, the symptoms on the whole are not so severe, nor, as has already been remarked, are the vomitings in particular so frequent. When

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they do occur in this form of the complaint, although they may often continue through the whole course of it, yet it is observed that most commonly, they are confined to the commencement, or take place near its close, when the stomach has contracted adhesions with the neighbouring parts.

Morbid Appearances.—On opening the abdomen of those who have died of this disease, we sometimes find a quantity of serous fluid effused into its cavity; but when this is the case, the liver or some other viscus in the neighbourhood of the stomach is usually diseased. It is, therefore, in general to be regarded not so much as the consequence of cancer of the stomach itself, as the effect of complications.

The stomach is often altered in its capacity in this disease, usually being larger when the pylorus is affected, but in other cases generally smaller than natural; sometimes even its capacity is so much diminished that it is hardly capable of containing an egg. Its position too is sometimes altered, and it is often connected by adhesions to the surrounding parts.

When cut into, it is very often found to contain a blackish liquid in greater or less quantity, mixed with any alimentary matters that may be present, and exactly resembling the black matter rejected by vomiting during life; this liquid, as is observed by Bayle and Cayal, is not necessarily connected with ulceration, nor produced by it, for it is sometimes present when the disease has not advanced to ulceration, and is not always found even when the ulcer is of considerable extent. It is mentioned by Morgagni that a case of cancer of the stomach occurred to Valsalva, where there was a large ulcer

with abundance of black fluid, but on squeezing the ulcerated part, the fluid which was forced out was of a whitish colour.

A case was lately mentioned to me by a friend, in which the stomach was scirrhous, and contained a black liquid, but showed no trace of ulceration.

The part of the stomach affected is usually converted into scirrhus, and it is most commonly believed that scirrhus is the only morbid texture to be met with in this disease; Bayle and Cayal, however, assert that portions of cerebriform matter are frequently mixed with the scirrhus, besides several of what are, by them, termed accessory or accidental tissues; and M. Laennec, who, as well as these pathologists, considers the encephaloid or cerebriform matter as a sort of cancer, affirms that cancers of the stomach are sometimes entirely formed of that substance. Several considerations might be brought forward, both for, and against the opinion of the identity of cancer, and the encephaloid tumour or medullary sarcoma, as it has been called in this country. But the subject is more fitted for an essay on cancer in general than a dissertation of this sort; I shall therefore proceed to give an account of the diseased changes which constitute scirrhus of the stomach.

Upon making a section of a part of the stomach affected with scirrhus, we can generally distinguish its different tunics readily enough from one another, but these are very much changed from their natural appearance. The mucous or internal membrane is much thicker than natural, and converted into a homogeneous white substance, varying in its consistence, but most frequently lardaceous, and pre-

senting no appearance of fibres or blood-vessels;
the muscular coat is also very much thickened, and
appears to consist of a very dense yellowish or
greyish substance, intersected by membranous-
looking septa. With regard to the peritoneal or
external coat, pathologists do not agree in their
descriptions. Dr. Baillie describes it as many times
thicker than it ought to be, and having almost a
gristly hardness; the French pathologists, on the
other hand, affirm that it is very rarely affected,
except when the stomach is perforated by ulceration.
It is probable that the free exhaling surface of the
peritoneal coat is rarely affected, though the cellu-
lar membrane, on its adhering surface or between
this and the muscular coat, may often become
scirrhous.

When the disease is further advanced, this
thickened portion of the stomach is ulcerated on its
inner surface; the ulcer has hard, thick, and everted
borders, and its surface is covered with a sanious
fluid; frequently it throws out irregular fungous
excrescences, which are covered with a foul, greyish
or brownish liquid. The ulceration may have gone
so far as to perforate entirely the coats of the
stomach, and even penetrate some way into a neigh-
bouring part; thus the bottom of the ulcer is some-
times formed by the liver, spleen, or pancreas, and
the abdominal muscles or diaphragm are sometimes
destroyed to a greater or less extent; in the same
way a communication is sometimes formed between
the stomach and colon, which allows a considerable
part of the food to pass straightway from the one
viscus into the other. In all these cases, however,
the stomach is in general, previously united to these

parts by adhesions of considerable strength, by means of which the effusion of alimentary matters into the belly, and consequent death of the individual, are prevented. But although, in far the greater number of cases, Nature thus admirably guards against such a serious occurrence, her efforts are not always successful, for instances of such an effusion taking place, and death immediately following it, are recorded by various authors.

The pylorus and smaller curvature are much more frequently the seat of scirrhus than the cardia or any other part of the stomach ; when the pylorus is affected, the scirrhous degeneration not unfrequently extends a considerable way along the pyloric end of the stomach, but rarely spreads into the duodenum ; in the same way, a scirrhus of the cardia rarely extends higher than an inch into the gullet, but often affects a considerable portion of the smaller curvature. As already mentioned, there are instances where almost the whole of the stomach is converted into scirrhus. The part affected is usually well defined, though sometimes it has no very well-marked limit between it and the sound structure.

It is difficult to say in which of the textures the scirrhus commences ; some have supposed that the little bodies called *glandulæ Brunneri*, situated between the mucous and muscular coats, are the original seat of the disease. Bayle and Cayal remark that when only one of the coats is scirrhous it is almost always the muscular, the mucous membrane being very rarely primarily affected. The disease appears sometimes to have commenced by the growth of scirrhous tumours in the neighbourhood of the pancreas, and thence spread to the stomach

in consequence of adhesions between this organ and the diseased mass; it is evident that in such cases, the cancer of the stomach is to be regarded in a great measure as a consecutive complaint.

In most cases of cancer of the stomach, the lymphatic glands in the neighbourhood, particularly those lying on its lesser curvature, are enlarged, hardened, or otherwise altered in their structure.

Diagnosis.—Although the chief difficulty in detecting this disease is owing to its occasionally presenting no well-marked symptoms, yet even the presence of such symptoms is not always a sure indication of its existence, as many of these are common to other complaints which may therefore be very readily mistaken for it.

1. As most of the principal symptoms of this disease are such as indicate a disordered state of the digestion, it is very liable in its early stage to be confounded with ordinary dyspepsia unconnected with any alteration of structure; and also with various sorts of obstinate vomiting, which, though independent of any organic disease, bear a strong resemblance to it, and counterfeit some of its most striking symptoms. These vomitings may be occasioned by a variety of causes, such as irritability of the stomach, sympathy between that organ and other parts, and, according to some, on chronic inflammation of its mucous membrane.

From such affections it is to be distinguished chiefly, by the peculiar alteration of the colour of the skin, so characteristic of cancer; by the appetite frequently continuing unimpaired for a great length of time, by the advanced age of the patient, and, most certainly of all, by the swelling when it is present.

2. Various tumours in the upper part of the belly, whether they be scirrhous in their nature or not, yet if they happen, as is sometimes the case, to be accompanied by dyspeptic symptoms, may very readily be mistaken for a scirrhus of the stomach, and cases of this sort puzzle the most experienced physician. It is chiefly by a careful investigation of the history of the disease, and by considering the whole of the symptoms in their various bearings, rather than by any particular one, that we are to be guided in our diagnosis in cases of this sort.

3. The symptoms of scirrhous contraction of the cardia may be imitated in a great degree by a spasm of the gullet; but this affection is neither of long duration, nor does it necessarily occasion dyspeptic symptoms.

4. A scirrhous swelling of the pylorus often has a pulsation communicated to it, and may thus be mistaken for an aneurism. But it is clear that this mistake can hardly take place except when the scirrhous pylorus is unattended by its usual symptoms.

Causes.—Many circumstances have been supposed to have considerable effect in inducing cancer of the stomach, and hence have been regarded as exciting causes of this disease; of these the following have chiefly been enumerated: long-continued anxiety or grief, suppression of habitual evacuations, whether natural or artificial, stimulating substances taken into the stomach, particularly spirituous liquors, if their use be long persisted in.

These causes may undoubtedly assist in bringing on cancer, but it often makes its appearance, when none of them could have operated, and in innumer-

able instances they have had their full operation without occasioning it. Hence it is inferred that there generally exists a sort of constitutional predisposition to cancer, without which the various exciting causes already enumerated, though enjoying their full operation, would in most cases have no effect. In what this state of the system consists, or rather how it is to be recognised before the actual occurrence of cancer, we are not informed : various considerations, however, besides what has been already mentioned, render it probable that such a condition of the system does in many cases actually exist.

By some this condition is supposed to be hereditary, and this opinion is principally supported by the fact, that now and then several individuals of one family are affected with cancerous complaints. Yet when we consider that such complaints are by no means uncommon, and that instances of two or more of the same family being affected with them are comparatively rare, while many individuals suffer from cancer, none of whose relations, to their knowledge, ever laboured under that disease, there seems considerable reason for ascribing these supposed cases of hereditary cancer to mere accidental coincidence. This question may, however, be regarded as not yet settled, and need not detain us here, as it hardly comes within the scope of this dissertation. Most persons affected with cancer of the stomach have reached their fortieth year, hence it seems that an advanced period of life is favourable to its occurrence.

With regard to the intimate nature or proximate cause of this disease, little or nothing satisfactory is

known. Most of the opinions which have been held on this subject are so vague and improbable, as scarcely to be worth our consideration—if perhaps we except the one which regards cancer as a sort of slow inflammation, or the product of such an inflammation. But even this opinion, though somewhat more worthy of attention, is not at all satisfactory; for although the diseased textures which constitute cancer, after existing a certain time, generally occasion inflammation of the surrounding parts, or undergo a slow inflammation themselves, yet this does not take place till after these textures are actually formed, and there is no proof that the particular operation by which they are formed must be an inflammation. The belief that stimulating substances taken into the stomach sometimes occasion cancer of that organ, may be supposed to favour the opinion of its being an inflammation, but it remains yet to be proved that these substances can operate in no other way but by exciting inflammatory action; and in a number of the cases where stimuli have been supposed to act in this way, besides the cancer of the stomach, there have been found cancerous growths in other parts, where they could have had no sort of influence.

Prognosis.—It is obvious that the prognosis here must be uniformly most unfavourable, yet death happens sooner in some forms of the disease than in others. Thus when it has its seat in the cardia, the contraction of this orifice may be so great as to occasion the death of the patient from inanition, before ulceration takes place. In all forms of the complaint, if we wish to have a probable idea of the length of time the patient's life may hold out, we

must pay particular attention to the degree to which the system in general is affected, as of course the more it has suffered, the nearer, generally speaking, has the disease advanced to a fatal termination.

Treatment.—Although medicine can hold out no prospect of cure in this intractable disease, yet the aid of the physician is not therefore entirely useless, for by means of a proper regimen, we may in some measure retard the progress of the complaint, and by various medicines, we are enabled to alleviate some of its most distressing symptoms, so that at least we may render the patient's life less miserable, if we cannot prolong it.

The consideration of the various remedies, which have from time to time been held forth as specifics for cancer, need not detain us long ; indeed, the simple inspection of a part affected with cancer, and the thorough alteration which it has undergone, would almost of itself be a sufficient proof of the inefficacy of all such remedies in effecting a cure. Preparations of iron, of mercury, the muriate of barytes, and a host of other medicines, have been proposed with this view, but perhaps none ever attracted greater notice than the conium maculatum, originally recommended as an internal remedy in this disease by Baron Störck of Vienna. Störck flattered himself that he had by means of it succeeded in resolving cancerous swellings, as well as other sorts of chronic indurations, and in consequence laid the results of his experience before the public, in two or three different publications. Unfortunately, however, his specific had by no means an equal success in other hands, for from the most extensive trials by De

Haen, Fothergill, and other physicians of repute, it appeared that though hemlock is often useful in alleviating some of the symptoms of cancer, and can be used in cases where opium would be inconvenient, yet as a means of cure it is wholly inefficacious. The case is nearly the same with belladonna, hyoscyamus, and some other narcotic vegetables which have similar powers. It appears therefore, as already mentioned, that our treatment must consist chiefly in endeavouring to retard the progress of the disease, and diminish the severity of its symptoms, and this is to be attempted in the following manner :—

1. By a proper diet and regimen. All sorts of strong drink, particularly spirituous liquors, should be carefully avoided. The food should combine, if possible, the qualities of being easily digestible, and mild and unirritating—perhaps milk and the farinaceous vegetables or mild animal soups will be found on the whole most eligible. If the milk cause acidity on the stomach it may be mixed with a little magnesia or lime-water.

2. From the known effects of local blood-letting in retarding the progress of cancer in the breast, we might be led to think that taking blood from the epigastrium by leeches or cupping would be useful in cancer of the stomach ; it is evident, however, that this could be done with impunity only in the commencement of the disease, and before the strength has begun to fail, at which period, unfortunately, its existence can rarely be detected ; hence it is not astonishing that blood-letting has been but seldom recommended, and still more seldom practised, in this complaint. Blisters kept open

with savine cerate, or an embrocation of tartar emetic rubbed on the epigastrium, may be of some service, and at least their employment is attended with less risk than that of blood-letting.

3. The means just mentioned as proper to retard the progress of the disease, will also contribute to relieve the pain which is so often an attendant upon it ; this symptom, however, if it continue as it often does, in spite of the employment of these measures, is to be alleviated principally by the use of opium, or cicuta when the opium is inconvenient ; and by fomenting the epigastrium with warm water, or decoctions of anodyne vegetables.

4. For allaying the vomitings, we must trust chiefly to the effervescing draught or solid opium, or we may prescribe the tincture of opium with a little ether for the same purpose.

5. The acidity, heartburn, etc., when present, are to be removed by alkaline and earthy substances, such as magnesia, lime, potash, etc., but the best way is to avoid as much as possible all sorts of acescent food.

6. Although the bowels are for the most part exceedingly costive in this disease, we should employ only the mildest means of opening them ; for this purpose magnesia, either alone or combined with rhubarb, is perhaps the best medicine. In many cases it will be better to use clysters and suppositories than laxative medicines.

7. If in the case of scirrhus of the cardia, there is reason to fear the patient will die from want of sufficient nourishment, we must endeavour to support life by pouring nutritious liquids into the stomach through a flexible tube, or if this cannot

be done (which I should suppose most probable), the same sort of liquids must be conveyed into the intestines in the form of an injection.

Having brought this dissertation to a close, it now only remains for me to apologise for its many defects ; of these perhaps not the least is the hasty and awkward manner in which it is drawn up. For this, however, I hope that want of sufficient leisure will be considered as some excuse ; as for its other numerous imperfections, I must trust solely to the indulgence of the Society.

XII

ALLEN THOMSON

1809-1884

ON THE FORMATION OF THE EGG AND THE
EVOLUTION OF THE CHICK

Read 1829

MR. PRESIDENT,

The newly-laid egg consists of two kinds of parts. First, the external coverings, consisting of the shell and its lining membranes, which undergo scarcely any change during incubation; and second, the parts contained within these coverings, consisting of the white or albumen with the chalazæ, and the yolk with the cicatricula or germspot.

The shell or external covering of the egg gives to it its peculiar form, and is so constituted as to preserve it from external injury, and at the same time to allow of those changes taking place within it that are necessary to the development of the embryo. The substance of the shell is hard, brittle, and of a granular structure. On the outer surface are numerous indentations which might at first sight be mistaken for the openings of the foramina, but which do not extend to the inner surface of the shell, and appear rather to be marks produced by

the villi in that part of the oviduct in which the
shell is secreted. The shell consists chiefly of
carbonate of lime ; it contains also a little phosphate
of lime, phosphate of magnesia, and traces of iron
and sulphur, which ingredients are all held together
by means of a little animal matter, amounting to
two per cent.

On carefully removing the shell alone, we
expose its lining membrane, which consists very
evidently of two layers. The external adheres
firmly to the whole of the inner surface of the shell,
and is dense and tough. The interior layer is of a
softer consistence, and is smoother and thinner than
the exterior, to which when the egg is quite
recently laid, it generally adheres in every part.
These membranous laminæ, when examined in the
microscope, present the appearance of cellular or
filamentous membrane, but they are so dense as to
render their examination very difficult. While the
exterior lamina adheres closely to the shell, the
interior seems to be more properly the membrane of
the white, which it surrounds, for as the egg cools
and the evaporation of some of its more watery
parts takes place, the internal layer separates itself
from the external at the large end of the egg, so
as to leave a circular space between them, which
has received the name of air-follicle or air-space.
This separation is often to be found in eggs at the
time they are laid, and may perhaps even exist in
some before they leave the oviduct ; the air which
it contains seems to be pure atmospheric air, and is
therefore probably introduced from without. As
the length of time that eggs have been laid in-
creases, this air-space expands, and at length some-

times separates more than half of one layer of the membrane from the other. The air-space is easily seen in transparent eggs, such as those of the duck, by holding them against the sun's rays or a candle; and in most eggs its existence can be ascertained by the application of the tongue a little to one side of the obtuse end of the egg, in consequence of the shell feeling warmer at that part of the egg where this separation takes place than at any other part of its circumference.

The membrane of the shell consists in great part of animal matter, and, according to Dr. Prout, when burnt gives traces of phosphate of lime.

The albumen or white of the egg consists of three parts :—1st, the exterior and thinnest layer; 2nd, the interior layer, which is of greater consistence; and the 3rd, the chalazæ or tortuous bodies situated between the white and the yolk, which are the most consistent of all. The two first of these parts, the layers of the white, surround the yolk on every side. When the egg is placed on its side or upon either end, the yolk floats higher than the middle, but it never entirely breaks through its covering of albumen, there being always a thin layer of albumen left between the yolk and the membrane of the shell, a fact which is best illustrated by boiling to hardness eggs that have been placed in different positions, and then making sections of them. The structure of the two layers of the white, as altered by coagulation, seems to differ; the exterior is distinctly lamellar, while the interior, which is more dense, can be torn with equal facility in all directions. When the albumen is thrown into water its surface becomes slightly opaque, a circumstance

which probably gave rise to the idea at one time prevalent that it is inclosed by a membrane. When the albumen is left for some time in water, the superficial part becomes still more firmly coagulated, and can be inflated to three or four times its original bulk. According to the analysis of Mr. John, the white of the egg is chiefly composed of water and albumen ; the saline ingredients are soda, sulphates of soda and potash, phosphates of lime and magnesia, their carbonates, and a trace of oxide of iron.

On opening the egg at the side, we perceive floating at each end portions of albumen of an irregular shape, and of a somewhat thicker consistence than the layers of which we have just spoken. These bodies, generally known by the name of chalazæ, hang in a direction outwards and downwards from the yolk ; they are traversed by white threads, which are very tortuous, and have much the appearance of bundles of vessels. From these circumstances the chalazæ have been described as vessels by many authors, and they have been supposed to perform the office of transmitting the albumen into the yolk ; but it appears from the observations of Purkinje that they are not tubular, but merely twisted membranous bodies, which are prolongations of a layer of hardened albumen that lies immediately in contact with the membrane of the yolk. There is no evidence that the membrane of the yolk is perforated at any part,—it seems to be an entire sac, and the transmission of the white into the yellow is more probably effected by transudation through the membranes of the cicatricula. It is quite well known that that part of the yolk on which the cicatricula or germ-spot is placed always

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floats uppermost, so long as the yolk is invested with the white and chalazæ ; but when these are separated from it, the cicatricula floats indifferently in any position, which renders it probable that its original position is owing to the peculiar mode in which the chalazæ are attached to the yolk. When the egg is boiled to hardness on its side, the cicatricula is generally found on the upper side, and the chalazæ have fallen to the lower part of the egg. The upper surface of the yolk is flatter and broader than any other part, and the distance between the attachments of the chalazæ on the upper side is greater than that on the under. The same thing occurs when eggs are boiled on either of the ends. The cicatricula always tends towards the end which is uppermost, while the chalazæ hang down from the yolk apparently regulating the position of the cicatricula.

The yolk is composed of two kinds of globules swimming in a transparent fluid. These globules are of two sizes ; the largest may be easily seen with the naked eye or a common lens,—they are chiefly composed of the oily substance of the yolk, and give to this part its peculiar colour. The smaller globules,—which probably correspond with the globules of the blood,—seem to consist of albumen. The structure of the yolk, however, is not uniform throughout, the central part is of a darker colour than the rest, and when hardened by boiling, assumes the consistence of coagulated albumen. From this nucleus there rises to the cicatricula a cone of the same sort of matter, and both nucleus and cone are generally surrounded by concentric layers of a similar substance, which alternate with a

matter of a brighter yellow colour that seems to contain a larger proportion of oil. This latter substance,—which constitutes the great bulk of the yolk and all its external part,—becomes dry and brittle when boiled to hardness. The yolk is composed in great part of water, of a sweet oil, of a large proportion of albumen modified in a peculiar manner, and of some gelatine : it contains sulphur, free and in combination, a little phosphorus, some free acid, and a peculiar red matter.

On the upper part of the yolk we perceive the round spot,—to which reference has already been made,—of a lighter colour than the part of the yellow which surrounds it, to which the names of macula, germ-spot, cicatrice, cicatricula, etc., have been given. In this spot there exists the germ, embryo, or that part of the future chick which is first formed. We can, with the aid of the microscope, perceive that the cicatricula is composed of globules of different sizes, united together so as to form a soft membrane. This membrane, which has been termed the blastoderm or germinal membrane, from its containing the rudiments of the fœtus, separates into two layers after it has been macerated for a short time in water : the upper of these layers, called the serous, is thin, homogeneous in its structure, and transparent through its whole extent ; the lower, called the mucous layer, is thick and velvety, and transparent only at its centre, so that it is in it only that the transparent spot or area is conspicuous. The diameter of the cicatricula is in general about one-fifth of an inch, though its size varies much in different eggs : the transparent area of the blastoderm is about one-twelfth of an inch in diameter.

In the centre of the cicatricula Rolando, Prevost and Dumas have lately observed a dark line, which they believe to be the rudiment of the nervous system of the chick; it is situated in the serous layer of the blastoderm, and passes like a radius from the centre to the circumference of the transparent area. Malpighi is the first and only other author who has observed this line before incubation.

The cicatricula is likewise found in the unimpregnated egg, and, on a superficial inspection, seems to resemble that in the egg which is capable of being hatched. A more minute inspection shows us that the dark central spot or transparent area is entirely wanting, its place being occupied by a part of a lighter colour than the rest of the cicatricula. The areola which surrounds this generally contains a number of round spots, which seem to be perforations of the blastoderm; from this circumstance the cicatricula of the unimpregnated egg has been described by Malpighi and others as composed of a network, in the centre of which the meshes are united together so as to form a white mass. According to Prevost and Dumas no trace or rudimentary line can be found in the unimpregnated egg, and Pander asserts that the two layers of the blastoderm do not exist in it, an observation which, from the statements of later authors, appears to be incorrect.

Such is a general description of the newly-laid egg, but in order fully to understand the structure of its parts it will still be proper to say a few words respecting the manner in which they are formed.

The perfect egg is composed of the ovulum containing the yolk and germ, and of other parts which

are added to the ovulum after its formation has been completed. The principal organs of the hen concerned in the formation of these parts are—1. The ovarium, in which the ovula are developed; and 2. The oviduct, by which the accessory parts are supplied.

The ovarium of the hen is situated immediately behind the liver, and below the lungs, and is recognised by the large number of ovula which it contains in the season of laying. These are attached by membranous pedicles to a common branch, called the *racemus vitellorum*, which is itself attached to the bodies of the dorsal vertebræ. When the hen is not laying the ovarium is much smaller, and there can only be seen in its place a mass of small white globules, collected together round the vertebral column and the surrounding ribs. The small white globules are the rudimentary ovula or yolks. Their number does not appear to be limited. As the season of laying approaches some of them increase in size, and become filled with a whitish semi-transparent fluid. When the ovulum has acquired the diameter of a line it is composed, according to Purkinje, of two small vesicles, the one within the other. The exterior vesicle is what afterwards forms the yolk itself inclosed in its membrane, the interior corresponds to a small vesicle, afterwards to be noticed as being distinctly perceptible in the centre of the cicatricula. When the ovulum is the size of a pea the capsule in which it is formed becomes vascular. This capsule consists of two layers of membrane, and surrounds the ovulum on every side, except at the part where it is itself attached to the pedicle of the ovarium. The external layer is most

vascular, the vessels descend on it from the main trunk of the ovarium along the pedicle of the ovulum, and are beautifully ramified over its whole surface, except at one part which is quite free from vessels. At the part of the capsule opposite to the pedicle, there is a transparent band extending over one-third of the long circumference of the ovulum; the ramifications of vessels, on reaching the margin of this band, stop short, and are in part reflected on the internal layer of the capsule of the ovulum. This band is perceptible in very small ovula as soon as their capsules become vascular, and it is very obvious in those which have attained their full size. The internal layer of the capsule is rougher and thicker than the external one, to which it adheres firmly by prolongations of the small vessels. The fluid in the ovulum becomes of a more and more opaque white, till this body has acquired the size of a pea, when it is tinged with yellow by the deposition of globules of a yellow oil. The transition from a white to a yellow colour is gradual, but by the time the ovulum is one-third of an inch in diameter its composition seems to be nearly perfect. At this time we can perceive with ease the blastoderm or white disc of the cicatricula, the centre of which is occupied by a very small dark spot. It is in this dark spot that the vesicle, of which we have already spoken, is placed. At this period the cicatricula is about a line, and the central spot one-tenth of a line in diameter. The ovulum now increases rapidly in size, the number of oily globules becomes greater, and the vessels on the capsule are more turgid with blood. The cicatricula is seen lying below the proper membrane of the yolk; it is generally situ-

ated near the pedicle of the capsule; sometimes, also, it has been found near the clear band, but never in any other situation; it adheres firmly to the proper membrane of the yolk, so that when this is removed the cicatricula comes away along with it. When examined with care under water, the cicatricula presents the appearance of a disc composed of white globules, thicker at the middle than at the edges. In the centre of this disc we find the vesicle, which is seen to bulge out from the disc when we look at it laterally. Appearances nearly the same are seen in the unimpregnated ovulum, but in it the parts appeared to Purkinje to be weaker and thinner than in the impregnated.

Though there are two oviducts in the hen, only one of them seems to be developed for the reception of ovula and the formation of their accessory parts. This is the oviduct of the left side which, in the laying season, occupies the greater part of the back of the abdomen; it is necessarily very much convoluted, as its whole length is generally about two feet. The tube of the oviduct is enclosed between two serous membranes, which join one another at each side of it, and thus constitute a double mesentery, by which it is very tightly bound down. The upper portion of the tube opens into the cavity of the ovarium; the margins of the tube are expanded so as to form a funnel-shaped opening. A strong band passes from the upper margin of the funnel to the angle of the second left rib, while the opposite margin of the funnel is attached below to the convolutions of the oviduct by a contracted portion of the peritoneal covering, so that by this means the mouth of the oviduct is held stretched below the

ovula. The funnel leads into the Fallopian tube or entrance of oviduct; the coats of this tube are thicker and stronger than the infundibulum, and are lined internally by oblique longitudinal stripes, which appear to be folds of the inner mucous membrane, and to be of a glandular structure. The Fallopian tube becomes gradually wider till about five inches from its commencement, when it attains its greatest width, which is about two inches and a half in circumference. The folds on the internal surface are much more developed at this part; they are very distinctly glandular, and cannot be effaced by pressure, and when the tube is inflamed during the passage of the ovulum through it, vessels are seen ramifying very beautifully upon them. This part of the oviduct is about half the length of the whole; it terminates in a narrower part about three or four inches in length, from which it is separated by a slight stricture. In this part of the oviduct the spiral glands are smaller and more closely set than in the rest. In the region of the sacrum the oviduct expands into a wide cavity of a different structure and appearance from the rest: this is the cavity in which the egg receives its shell, and has, not perhaps with great propriety, been called the uterus. Before entering the uterus the tube makes a very sharp turn, and is considerably narrowed; the cavity of the uterus itself is of the size and form of the full-sized egg; the mucous membrane is studded with villi, which are so large as to have the appearance of glands; its colour is brownish red, and its consistence very soft. From the lower part of the uterus there extends another narrow tube called the vagina, which opens into the cloaca at

the side of the rectum. The parietes of the vagina are very thick and fibrous, and the sides are quite contracted together in the collapsed state of the tube.

Let us now return to trace the progress of the completely-formed ovulum suspended in its capsule from the ovarium. As the ovulum approaches to maturity the vessels of the capsule, instead of becoming more turgid with blood, diminish in size, so that the capsule appears of a lighter colour. When the ovulum is completely ripe, the expanded part of the infundibulum, in a manner which has not yet been explained, applies itself closely to it in every part, so that the free edges of the infundibulum embrace firmly its pedicle. Either by the direct pressure thus given, or by the stoppage of the blood that it occasions, the capsule gives way at the side opposite to the pedicle where the transparent band exists. The ovulum makes its exit from the capsule, and very soon passes down into the first part of the oviduct. In the meantime the infundibulum relaxes, and allows the empty and broken capsule to escape from its hold. These empty capsules are frequently observed hanging loose among the ovula; they are at first very vascular, but soon diminish in size and are absorbed. The only change which can as yet be observed in the ovulum is in the central part of the cicatricula. The vesicle that existed in the ovulum while contained in its capsule, cannot be seen in that which has passed into the oviduct.

As soon as the ovulum enters the oviduct the secretion of the albumen commences, and when it has advanced several inches into this canal the

albuminous layer is of considerable thickness. The
oviduct is expanded only at the place where the
ovulum lies, and is collapsed on either side. As
yet we see no trace of the chalazæ, unless we regard
as such a small quantity of albumen which adheres
to each side of the ovulum. As the ovulum moves
onwards layer after layer of albumen is added, and
in the meantime that part of the albumen which
immediately surrounds the yolk begins to thicken,
and is gradually condensed into a very thin mem-
brane, which applies itself closely to the membrane
of the yolk. From this new membrane arise the
tortuous cords which are afterwards seen in the
chalazæ, and which are formed by a condensation
of albuminous matter similar to that by which the
membrane itself is formed. It is to this newly-
formed membrane, according to Dutrochet and
Purkinje, that the chalazæ are attached; and it is
from the difficulty which we experience in separat-
ing this membrane from that of the yolk that the
existence of a communication between the white
and the yolk, by means of the chalazæ, has been so
universally believed.

In the egg as contained in the oviduct, the
transition from the thicker internal layer of the
albumen to the thinner external is so gradual that
they cannot be distinguished from one another as
in the recently-laid egg.

Whilst the ovulum receives the white it
gradually moves onwards, the small end proceeding
foremost, until it reaches the narrowed part between
the oviduct and uterus. It is probably in this part
that the membranes covering the albumen are
deposited, but this circumstance has not yet been

established by observation. The egg arrives in the uterine part of the oviduct, covered by the two layers of membrane, and immediately the deposition of the shell commences. On examining the egg in this state, the transparence of its membranes enables us to perceive the motions of its internal parts when the position of the egg is changed; it then appears obvious that the cicatricula has a tendency to rise to the highest part, though in one particular position this tendency seems to be greater than in any other. The structure of the membrane examined by the microscope is clearly filamentous. The outer layer is covered by small crystalline bodies, joined together in groups of two or three; these little bodies appear to be the first deposition of the calcareous matter of the shell, as they disappear with effervescence when muriatic acid is poured on them. A very short space of time is sufficient to complete the deposition of the shell, and the egg then lies perfect in the uterine portion of the oviduct, its small end being turned towards the lower opening. The canal of the vagina takes on, like the rest of the tube during the formation of the egg, an inflammatory action when the egg is about to pass through it. The egg perfected in the uterus by the addition of the shell, is in no way changed in its passage through the vagina.

The Evolution of the Chick.

*   *   *   *   *   *

Conclusion.—It would have been interesting to have traced the points of analogy and difference between the development of the fœtus of birds and of other animals, and the gradual complication of the

organs of the fœtus by which it seems to pass through the whole animal chain, at one time assuming the form of the hydatid or animalcula, till passing through its successive stages of development and complication, it arrives at the perfect state in which it comes out of the shell.

But time will not permit me to enter on these subjects, and I have only to hope that the observations of other members conversant with the subject will supply this defect as well as the other imperfections of this Essay.

XIII

JAMES YOUNG SIMPSON

1811-1870

ON THE DISEASES OF THE PLACENTA

Read 1835

Mr. President,

The diseases of the placenta, to which I have the honour of directing the notice of the Society this evening, form a subject of inquiry which appears to have hitherto attracted,—especially from British pathologists and accoucheurs,—a very incompetent share of attention. In the works of various medical authors of the last and preceding century, as in those of Bonnetus, Morgagni, Ruysch, Lietaud, and others, notices of some individual cases and forms of placental morbid lesions are occasionally to be met with, and more lately several of the diseases of this organ have been investigated with greater or less attention by different continental pathologists, as by MM. Murat, Desmoreaux, Braché, and Cruveilhier in France, and by Professors Stein, d'Outrepont, and Wilde in Germany. I am not aware, however, that the medical literature of England contains any account of the various morbid states to

which the placenta is liable, beyond what is to be
found in the casual observations and in the details
of incidental cases recorded in the obstetrical writ-
ings of Smellie, Denman, Burns, Ramsbotham, Lee,
Ingleby, Granville, and others. It is not, therefore,
without hesitation, that I venture to lay before the
Society the following general remarks upon some of
the principal forms of placental disease, as derived
partly from the data furnished by others, and partly
from the observations which I have myself had an
opportunity of making upon a few recent, and a very
considerable number of preserved specimens of mor-
bid placentæ, contained in the different pathological
museums of this country. At the same time, I may
be allowed to plead in excuse for the many imper-
fections and omissions in the following Essay, the
paucity of materials still existing upon the subject
of which it treats, the difficulty of collecting these
materials from the numerous different sources over
which they are scattered, and the haste with which
the observations it contains have latterly been
thrown together.

The human placenta, though an organ enduring
for a very limited period of time only, being destined
to serve as a mere temporary medium of communi-
cation between the mother and fœtus, and passing
through its different stages of formation, growth,
and ultimate expulsion, in the short space of seven
or eight months, yet appears to be liable during its
comparatively brief existence, to a considerable
number and diversity of morbid conditions, more
frequent, I believe, in their occurrence, and im-
portant in their effects, than seems to be generally
suspected. Nor are the morbid conditions and

lesions of this organ matters of pathological curiosity
and interest only; a more accurate knowledge of
their various phenomena appears to promise results
of no inconsiderable consequence to practical medi-
cine. The peculiarities of the placental economy of
the human female, and the nature of the relations
which exist between it and the uterus, constitute,
particularly when these relations are deranged by
placental or uterine disease, one of the great sources
of danger and fatality in human parturition.
Numerous facts also seem fully to prove that dif-
ferent morbid actions and lesions of the placenta,
exert a marked and powerful influence upon the
health and well-being of the mother during preg-
nancy; and, above all, the study of these lesions
promises to throw an entirely new light upon the
abnormalities of development, the diseases and death
of the fœtus *in utero*. Most of the latest inquiries
instituted upon these points by Geoffroy St. Hilaire,
Velpeau, and others, go to prove that several varie-
ties of malformation of the fœtus, are, in not a few
cases, attributable to morbid conditions of the
placenta as their immediate existing cause; that
the not infrequent occurrence of the death of the
fœtus, in the earlier as well as in the later stages of
pregnancy, is almost always the result of a diseased
state or states of this organ, and that the act of
abortion, occasionally the indirect cause of that
death, but much more commonly its indirect effect,
is in very many instances capable of being traced to
the same source. It seems not, therefore, irrational
to hope that by a more careful investigation into the
nature of the different diseased states to which the
placenta is subject, and by a more diligent and

rigorous observation of the symptoms which precede and accompany these states, and of the causes by which they are produced, we may be gradually led to a greater degree of facility and accuracy in their detection, and the adoption of better established principles for their prevention and treatment, and that in this way may be promoted in no inconsiderable degree those great and ultimate objects of all medical study—the preservation of human life and the alleviation of human suffering.

The two principal and (both in a pathological and practical point of view) most important diseased states to which the placenta is liable appear to be sanguineous congestion and hæmorrhage, and inflammation with its different consequences. The placentary parenchyma and the membranes investing the organ are liable to other morbid states : to hypertrophy and atrophy, to softening and induration, to cartilaginous and ossific degeneration, and the secretion or formation of other morbid products and tissues, to an abnormal or cystoid hydatiform structure, and to various forms of malformation and displacement; but in a practical point of view, most of these lesions are comparatively minor in their importance to congestion and inflammation, and the effects which these diseased conditions produce. It is to congestion and inflammation of the placenta, therefore, that I would wish first and principally to direct the attention of the Society.

By the term congestion of the placenta, I mean to imply the existence of a greater than usual quantity of blood in the vessels of that organ, taking place rapidly or more slowly, in some cases rapidly or more slowly terminating in resolution, and in

others leading to the infiltration or effusion of blood, in greater or less quantity or quantities, into the substance of the placenta, upon its uterine or fœtal surfaces, or between the membranes of the ovum. The excess of blood in the vessels of the placenta constituting congestion of that organ may, there is every reason to believe, have its seat primitively and separately either in the placental ramifications of the fœtal umbilical arteries and veins, or perhaps more frequently in the vascular prolongations which seem to be extended from the uterine vessels of the mother into the substance of this viscus; but a state of congestion in one of these systems of vessels, the fœtal or the maternal, scarcely perhaps ever continues for any considerable period of time, without giving rise to a similar state in the other.

The anatomical characters of simple congestion of the placenta may, I believe, be sufficiently illustrated by a reference to the engorged and dark coloured condition which this organ presents in cases in which the head of the child has unfortunately happened to be long and dangerously impacted in the passages of the pelvis. The appearances which the placenta exhibits on its being expelled after such cases, and more particularly if the impaction has been so great as to prove fatal to the child, are well known to every practical accoucheur. The external surface of the organ is of a more or less deep violet, and sometimes almost livid colour; its internal structure when torn or divided by the scalpel presents a deep purple hue, its vessels are everywhere loaded and distended with dark-coloured blood, the organ appears enlarged, and its substance feels heavier and more solid than natural.

In cases of abortion occurring subsequently to physical injuries of the uterine region, or to any of those causes which give rise to it by producing a strong determination of blood to the vessels of the uterus, the same congested and engorged state of the placenta, as that to which I have just alluded, is generally met with; and in these instances also the blood is not unfrequently found effused in more or fewer points from rupture of the lacerated or distended placental vessels, constituting the state of 'occult internal hæmorrhage' of Baudelocque, and the 'apoplexy of the placenta' of M. Cruveilhier, who, along with Laennec, Andral, and other Parisian pathologists, has employed of late years this term to designate the existence of an effusion of blood into the tissue of the lungs and other organs of the body, while the word literally and originally signifies one of the more striking external phenomena or symptoms only, which such an effusion of blood generally produces when it happens to occur within the cavity of the cranium.

The blood effused in placental congestion and hæmorrhage, occurring during pregnancy, may occupy different anatomical situations. In some instances it is found on the external surface of the organ, or between it and the uterus, occasionally in very large quantity, as in cases described by Mauriceau and Coloy; in other not infrequent instances the blood is situated towards the fœtal surface of the organ, or insinuated between the membranes, and in others, again, it is seen effused into the substance or proper structure of the placenta, as mentioned by Deneux and Baudelocque, and as I have myself had an opportunity of observ-

ing in a considerable number of specimens. In cases in which abortion occurs in the earlier periods of pregnancy, the extravasated blood is frequently found diffused over the whole, or nearly the whole, surface of the chorion, and between it and the amnion, in the form either of a continuous layer or layers of coagulum, or of more or less isolated nodules ; and in two instances I have seen it occupying these situations at the same time that it was extravasated in the form of roundish masses into the rudimentary structure of the placenta, and effused into the cavity of the amnion itself.

The quantity of blood effused internally in placentary hæmorrhage occurring during pregnancy is generally small, but it is occasionally also, particularly when the blood is extravasated between the uterus and placenta, towards the later stages of utero-gestation, so great as to endanger or destroy the life of the mother.

When in placentary hæmorrhage or apoplexy the blood is extravasated into the substance of the placenta itself, it most generally appears, as far as I have myself observed, under the form of roundish coagula, often accurately circumscribed ; but in other cases the form and outline of these coagula are altogether irregular, and the transition between the sound and diseased portion of placenta not very accurately marked. In some instances one or two coagula only are observed, or the extravasation is confined to one or two lobes or cotyledons only ; but generally a considerable number of coagula are seen scattered throughout different parts of the same placenta, the sanguineous extravasation having occurred at several points, either simultaneously or successively. In

one extreme case of this kind which I have met with, the section of a placenta of about the fourth month presented throughout its whole structure only one agglomerated mass of distinct and separate roundish coagula, and the organ was increased so greatly in thickness by these interstitial effusions as to infringe very considerably upon the cavity of the amnios.

The size or volume of the individual coagula or effusions is liable to great variation. In some instances they do not equal a small pea in size, and again I find in my notes two cases mentioned, in one of which a single coagulum as large as a walnut is described, and in the other case, another coagulum is alluded to as equal in volume to a pigeon's egg. Weisberg appears to have met with a placental sanguineous coagulum still larger than either of these two.

The substance of the placenta more immediately surrounding sanguineous extravasations is, particularly when these extravasations are recent, occasionally darker red than natural, from imbibition or infiltration of the effused blood into the neighbouring portions of tissue, and sometimes the surrounding placentary structure is studded with a number of dark spots or points probably originating in partial sanguineous extravasations, or in blood stagnated and coagulated in the cavities of some obstructed vessels.

The immediate anatomical seat of sanguineous effusions situated in the substance of the placenta, as well as the set of vessels from which they proceed, probably differ considerably in different cases. In several instances I have observed the extrava-

sated blood situated towards the fœtal surface of the organ, or between it and the chorion ; in others it is completely surrounded by the substance of the placenta on all sides, being probably effused into the cellular texture connecting its innumerable vessels ; or into the irregular cells or cavities which the organ contains, supposing the description of these cells or cavities given by the Hunters, Meckel, and Hildebrand to be correct. M. Deneux states that, in the case which he has given, the effused blood was situated in the interstices between the lobules or cotyledons, a circumstance which I have not had occasion to observe in any of the specimens of the disease that I have happened to examine.

When hæmorrhagic effusions take place towards the fœtal surface of the placenta, or in the earlier stages of pregnancy, between the decidua and chorion, the coagula of extravasated blood very frequently protrude these membranes before them towards the cavity of the amnion, and appear in the form of projecting eminences or elevations on the amniotic surface.

These eminences vary greatly in number and extent in different cases. In some instances almost every part of the amniotic cavity is crowded with them, in others they are seen only in that part which corresponds to the placenta. They vary in size from a very small pea to a hazel-nut and larger. Occasionally they form very slight projections, only above the general level of the surface of the amnion ; generally they appear as rounded irregularly hemi-spherical eminences ; and again I have seen them standing out from the walls of the amniotic cavity, of an elongated nipple-like shape, and nearly as large as

the first joint of the little finger. In some cases I have seen the umbilical cord at its insertion into the placenta, at the same time greatly distended with coagulated blood for a distance of six or eight lines, an appearance very correctly represented in a delineation of an aborted ovum figured by Sandifort.

In latent placentary hæmorrhage or apoplexy, the blood, after its effusion, undergoes a variety of changes interesting in themselves, and important in this respect, that a misconception of their nature has, as it appears to me, led pathologists into error in regard to the nature of some of the lesions occasionally observed in this organ. When the blood is poured out from its containing vessels into the substance or cells of the placenta, or between the membranes, it gradually coagulates and assumes a very dark purple, and sometimes (as I have seen it in two cases) almost a melanotic black colour. After a time, however, it begins to lose this tint, the colouring matter gradually becomes removed, and the coagulum successively assumes a chocolate brown—a reddish or brownish yellow hue—and latterly, if time sufficient is allowed, it presents a pale yellowish white, or straw-coloured substance, the fibrinous portion of the coagulum being then alone left. When these fibrinous and generally firm and indurated masses are divided, they, for the most part, exhibit internally a dense uniform or homogeneous tissue, but in some cases, where the individual mass or coagulum has been formed by several successive effusions of the blood occurring at the same point, and probably from the same vessel, its section shows a more or less perfect concentric

laminated structure.   M. Cruveilhier has repre-
sented an apoplectic placenta in which the structure
of the coagulum appears to be concentrically
laminated in this manner, and the more external
layers are seen to be losing their colouring matter,
while the nucleus of the effusion is composed of
darker and more recently effused blood.   I have not
had an opportunity of observing these changes in
specimens of laminated placental coagula, but have
had repeated occasion to remark the decoloration
of the more common single or homogeneous coagula
proceeding from their circumference to their centre.

I have been able to trace in the blood effused in
placentary hæmorrhage, a change still more advanced
than that of its decoloration and conversion into a
yellowish straw-coloured fibrinous mass.   In four
instances I have seen these fibrinous masses in
different parts of the same placenta, contracted to a
size considerably less than the space which they
originally occupied, and consequently appearing, as it
were, to be contained in cavities which were only but
partially filled by them.   This appearance is parti-
cularly well seen when the blood has been extra-
vasated towards the fœtal surface of the placenta, or
behind the chorion and amnion, and, when it has pro-
truded forwards these membranes, in the form of the
eminences or nipple-like projections already alluded
to.   In such cases, after the colouring matter of the
effused blood has been removed, and time is given
for the fibrinous masses which are left to contract,
the folds of the chorion and amnion which they
carried before them, being inelastic membranes,
remain in the position in which they were placed,
and present themselves when the ovum is afterwards

examined, in the form of half-empty loose bags or sacks projecting towards the cavity of the amnios. In one of the four placentæ in which I have observed this contraction or diminution at least of the fibrinous coagula, the cavities containing these contracted coagula were filled up with a limpid serum, but in the other three cases I do not find in my notes any mention of such an effusion.

In no case have I seen the complete removal of the fibrinous coagula from their containing cavities, or any appearance whatever betokening the organisation of these coagula; and I have to regret not having directed my attention particularly to the state of the placentary tissue more immediately surrounding the effusions, or observed the changes which take place in it upon the occurrence of these sanguineous extravasations. In some instances, as in those represented by Cruveilhier, the surrounding placentary structure appears to become atrophied and anæmic after the effusion has occurred, and perhaps in other cases this state may precede these effusions, but whether a cyst is ever formed around the sanguineous coagula, and under what circumstances it is formed, I have no data to determine, either from observations which I have myself made, or which I can find in the works of others.

A number of the cases of tumours and circumscribed indurations of the placenta described by Morgagni in his 48th letter, of the schirrous tubercles mentioned by Albrecht, and admitted by Murat, and of those eminences on the surface of the amnion figured by Ruysch, Sandifort, Breschet, Velpeau and Dr. Granville, and spoken of by this last author under the name of tuberculated ova, appear to me

to consist in reality of coagula of blood, which have undergone more or fewer of the changes which I have attempted to describe. The white masses in the placenta, composed of a substance resembling both fat and cartilage, recently mentioned by Professor Stein, not improbably consisted also of the fibrinous matter of effused coagula of blood. In the notes which I took of the first specimen of these yellowish fibrinous masses which I had occasion to examine, I find that I have described them as internally not unlike scrofulous or dense encephaloid matter; but since that period I have had ample opportunities of correcting this conjecture by an examination of above twenty additional specimens of this particular lesion, and I have now not only been able to trace in different placentæ the gradual transformations already described as occurring in the effused blood, from its appearance as a recent dark-coloured coagulum to its condition of a yellowish white fibrinous mass, but I have seen more than one instance in which these two states, with all their intermediate gradations of change and coloration, could be observed in different coagula which had been effused at successive periods in the same individual case.

The occasional exciting or determining causes of placental congestion may be considered as existing either on the part of the child or of the mother. With the causes existing on the part of the child we are as yet but very imperfectly acquainted, but it seems probable that whatever states and physical lesions and malformations of the fœtus and umbilical cord tend directly, or indirectly, to prevent or impede the free return of blood through the umbilical

vein, will have the effect of producing more or less marked congestion in the minuter placental ramifications of this vessel, and perhaps in some cases extravasations of blood from these ramifications. We now know that the fœtus *in utero* is liable to various febrile, contagious, malarious, and inflammatory affections, to plague, small-pox, and perhaps measles and scarlatina, to ague, and a number of acute internal inflammations, but whether the presence of these diseases in the fœtal economy is capable of exciting placental congestion or not, through the long and tortuous tract of umbilical arteries, and what effects they produce on the placental circulation in general, are points still open for investigation.

As occasional exciting or determining causes on the part of the mother of placental congestion and sanguineous effusion into the substance, and upon the surfaces of the placenta and ovum, may be mentioned physical injuries, such as blows, falls, etc., strong muscular exertions and bodily fatigue, excess of venery, morbid irritations of the uterus and neighbouring organs, acute febrile and inflammatory diseases, and in general terms all those circumstances which have a tendency to produce plethora and increased action of the whole maternal vascular system, and of the uterine system in particular. These various causes may produce placental congestion and hæmorrhage, in different ways ; certain of them, as the physical injuries alluded to, may occasionally lead to this effect by directly or mechanically rupturing, by the concussion which they create, some of the placental and utero-placental vessels and attachments ; others of them, and even these same causes at other times, may act by in-

ducing a morbid determination of blood to the uterus and placenta, or by exciting such muscular contractions of the uterus as will separate to a greater or less extent the mutual uterine and placental connections.

In respect to the mode of action of general and local plethora and increased vascular action, in producing placental congestion and hæmorrhage, I shall only venture one remark. If we admit, with some authors, that the newly formed vascular canals which, in the doctrine of the Hunters and others, are believed to pass from the uterine vessels into the placenta, are less firm and resistent in their coats or texture than the other blood-vessels in the maternal system, it will follow that when a greater than usual plethora or vascular action happens to occur in the sanguiferous system of the mother, or when, from any causes, the blood is determined in a particular manner to the divisions of the uterine arteries and veins, it will be most apt to become congested in the naturally little elastic vascular tubes and canals of the placenta, and if, further, the momentum of the whole mass of blood in the body of the mother, or of that in the branches of the uterine vessels alone, be preternaturally increased, the placental and utero-placental vessels or canals will be the first to yield or rupture under the generally increased degree of internal pressure, inasmuch as they are the least capable of affording the requisite resistance to the distending force of the contained fluid.

All the above effects will be naturally more or less promoted by the existence of any such diseased state or states of the tissue of the placenta, or of

the placental and utero-placental vessels themselves, as diminish the elasticity and power of cohesion of the component tissues of these vessels.

The symptoms produced by placental congestion and hæmorrhage are, in many instances, obscure and difficultly appreciable, but in other cases, especially in those in which sanguineous extravasation occurs, the preceding and accompanying phenomena are frequently sufficiently characteristic and well marked.

Simple placentary congestion, whether primitively seated in the fœtal or maternal vessels, seldom, perhaps, if ever, exists for any considerable period of time, without more or less speedily superinducing a degree of uterine congestion ; and again the state of congestion or determination of blood to the vessels of the uterus, when occurring as the primary pathological affection, is probably as generally and directly followed by a similar condition of the placental vascular system or systems. Placentary congestion, therefore, whether proceeding or not to extravasation of blood, usually appears to be either preceded or accompanied by those symptoms which denote an increased activity in the uterine sanguiferous vessels, as by a feeling of oppression or weight, and tension or bearing down in the uterine and pelvic viscera.

If there exists a general plethoric state of the system, various febrile phenomena may also present themselves. Occasionally the uneasy feelings in the uterine region proceed to a degree of fixed or intermittent pain, confined to one part of the uterus or uterine region only, or diffused generally over it. This pain seems frequently to extend to

the loins, and the same sensation is said in pro-
tracted cases to be sometimes even felt in the
mammæ. At the same time the morning sickness
and vomiting, with the other sympathetic affections
which so frequently attend upon the natural de-
velopment of the uterus during pregnancy, are in
most instances manifested in an increased or unusual
degree.

When placentary congestion occurs after the
period of quickening, and is very acute in its char-
acter or rapid in its appearance, the motions of the
fœtus would appear to be sometimes rendered
suddenly irregular, and at times almost convulsive ;
if the congestion is more chronic in its nature, the
motions of the fœtus occasionally become extremely
languid, or not at all appreciable, but under a timely
detraction of blood they may be, as Madame
Lachapelle has remarked, again reproduced after
having ceased for several days. How much we may
be assisted in the later months of pregnancy by
auscultation in our detection of the morbid states of
the placental and fœtal circulation, it is difficult if
not impossible at this moment to determine, but
more accurate and extensive observations upon this
subject may yet lead to some not unimportant results.

When the placental congestion has proceeded
to the extravasation of blood, besides the symptoms
already mentioned, others of a more unequivocal
character frequently present themselves. The
effused blood may, as we have already seen, be
poured out into situations in which it cannot escape
externally. Perhaps, however, in the earlier months,
at least in the larger proportion of cases, a small
quantity of it does escape *per vaginam*, and this

circumstance, when found in connection with other symptoms, particularly with lumbar and uterine pains, and with the knowledge of the previous action of any of the directly exciting causes of placentary congestion or hæmorrhage, may be considered as furnishing very unequivocal marks of the existence of these states. If a pregnant woman, for instance, immediately after receiving a fall or blow, or any sudden succussion of the body, such as that occasioned by severe coughing or retching, be seized with distensive uterine pain and more or less effusion of blood externally, little doubt can in most cases be entertained in regard to the nature of the internal morbid conditions which these symptoms betoken. One of these symptoms only, however, the uterine or lumbar pain, or the external hæmorrhage, may alone be present. The feeling of uterine pain, if occurring alone, can perhaps be but little trusted to as a diagnostic mark in the state of pregnancy, unless when taken into consideration with various collateral circumstances. In placental hæmorrhage it is probably produced, as M. Baudelocque has supposed, by the mechanical distension of the uterus with the blood effused, and this distension can, it is alleged, be sometimes actually traced externally in the enlargement of the absolute volume or bulk of the uterus. In some instances the pains remit or disappear entirely in the course of a few days or weeks, when the uterine development has again proceeded considerably in its course; and in many other instances they pass sooner or later into the intermittent and regular pains indicative of uterine contraction. The external hæmorrhage is in some instances observed to be in considerable abundance from the

beginning ; but more frequently it presents itself in the first instance as a few drops of blood, or of blood and serum only, afterwards becoming increased or not in quantity. In many cases it is seen to remit and return repeatedly after the lapse of a few days, or at the distance of some weeks.

When placental hæmorrhage occurs in the later months of pregnancy, the internal effusion of blood is sometimes so great, when arising from a separation of the placenta, to a greater or less extent, from the internal surface of the uterus, as rapidly to produce all the most alarming symptoms caused by extreme loss of blood, and under such circumstances the extravasation may even proceed to a fatal extent, as I have already mentioned, without any sanguineous discharge whatever appearing externally. In other cases of placental congestion and hæmorrhage occurring in advanced pregnancy, there is an external discharge as well as an internal effusion of blood, without, as is found on examination, any part of the placenta being placed over the os uteri—a case which I have not included, and consequently avoided all allusion to in the preceding observations.

The occurrence of sanguineous effusion from the placental, utero-placental, or exposed mouths of the uterine vessels, though often in the later and almost always in the early months of utero-gestation unattended with any great danger to the mother, and very frequently making no impression whatever upon her system, or not even betraying itself by any external discharge, must always be looked upon as a circumstance threatening the most direct and imminent danger to the life of the fœtus, for a very

small quantity of blood, whether extravasated in the earlier weeks upon the villous surface of the chorion, or at a later period, into or around the placenta, may be amply sufficient to arrest the processes of fœtal nutrition or respiration to such a degree as effectually to impede the development, or destroy the life of the embryo, and in this latter way prove an indirect cause of abortion. For my own part, I feel assured that the preparations of aborted ova contained in the pathological museums of this country fully bear out, as far as I have myself seen them, the opinion of Professor Duges, that the most frequent cause of abortion is active placental congestion, under which term he at the same time includes placental hæmorrhage. This remark, I believe, will be found in particular to apply to abortion in the earlier months.

In the greater proportion of cases which I have met with of ova aborted in consequence of internal placental hæmorrhage, I have found the embryo arrested at about the fifth or seventh week of development, or at that stage at which the extremities of the body begin to appear in the form of rounded buddings. Expulsive uterine contractions had not however supervened in many of these cases for weeks after the effusion had actually taken place, and, in some of them, the general growth of the membranes and placenta appeared to have proceeded, while in one or two cases the embyro itself had also enlarged to nearly the size of the thumb, although the individual development of its extremities, and perhaps of other organs, had been arrested.

The next series of morbid conditions of the placenta to which I have to direct the attention of

the Society, consists of inflammation and its various consequences.

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Having exhausted so very large a portion of the limits of this Essay in inquiring into the anatomical characters and products, the causes, symptoms, and effects upon the mother, and upon the child, of the two most frequent and important morbid conditions of the placenta, congestion and inflammation, I shall be obliged to confine myself in the remaining pàges to a mere enumeration rather than description of the other forms of disease to which this organ is liable.

Hypertrophy of the placenta is sometimes met with at the full term of utero-gestation. Thus Riecke speaks of placentæ of three pounds and upwards, and Stein mentions one as heavy as six pounds in weight; but it is principally in cases of death of the fœtus in the early and middle periods of pregnancy that placental hypertrophy, or a morbid disproportion in size between it and the child, has hitherto been studied, and it is admitted by Vater, Morgagni, Ruysch, Frank, and others, that the organ may not only live, but increase in growth after that occurrence has happened, and even, in some instances, after the total expulsion of the immature fœtus from the uterus.

The placenta is occasionally found atrophied. This atrophy may be partial or general. When partial, or confined to one or two lobes only, it seems in general to originate in a separation of the atrophied portions of the organ from the internal surface of the uterus. The atrophied lobes are thinner than the other parts of the viscus, and present an orange or yellowish white colour; they

<p align="center">N</p>

contain little or no blood, and appear transformed into a kind of semi-desiccated cellular or filamentous tissue. Atrophy of a whole, or of the half of a double or twin placenta is sometimes met with in those cases of plural conceptions, where one of the fœtuses perishes during the earlier periods of utero-gestation but is not expelled till the full, or near the full, term of pregnancy, along with the other perfect and generally living child. The whole placenta, or portion of placenta, belonging to the blighted fœtus, is under such circumstances almost always found greatly atrophied, and presenting the anatomical characters just now mentioned.

Cartilaginous and osseous degeneration of the placenta is not unfrequently met with.

Tubercular deposits in the placenta are mentioned by D'Outrepont as having been met with by him in one case where the mother was labouring under pulmonary phthisis.

Various instances in which sarcomatous tumours of different varieties are said to have been met with in the placenta are referred to by Voigtel and Otto in their works on pathological anatomy.

Of all the different morbid states to which the placenta is liable, none perhaps has hitherto attracted so much attention from pathologists as that to which the various names of hydatid mole, hydatid placenta, vesicular mole, acephalocystis racemosa, etc., have been given. In this form of abnormal structure the substance of the placenta is occupied, or its situation more or less completely replaced, by serous, hydatoid, cystiform bodies or vesicles of different shapes and sizes, generally co-existing in great numbers, and arranged in regular racemose or

SIMPSON] ROYAL MEDICAL SOCIETY grape-like clusters upon branching footstalks or pedicles arising from the uterine surface of the chorion. On the origin, nature, and general pathological history of this form of abnormal placental lesion I had collected a very considerable variety of facts with a view to lay them before the Society, but the unusual and unexpected length to which this paper has already extended prevents me from doing so on the present occasion. I may mention, however, as the result to which I have myself arrived in this investigation, that numerous circumstances appear to me to show that the hydatoid placenta essentially consists of the enlarged gangliform bodies and villi of the chorion, and ought not to be looked upon as a morbid degeneration, morbid transformation, or species of acquired disease, but should rather be considered as a species of primitive malformation, consisting, as many other malformations do, in an arrestment, in the first instance, of the natural changes of development, in so far that a type of structure which was intended to be temporary only, becomes permanent for the whole remaining period of intra-uterine life.

The limits of this paper necessarily prevent me also from entering here at any length into the other varieties of malformation of the placenta. The malformations by displacement of the placenta form also an interesting subject of inquiry to which I can only here allude.

I cannot allow myself, Mr. President, to bring this Essay to a conclusion without apologising to the Society, at once for its great length and great imperfections. Of its errors in both of these respects, no person, let me assure you, can be more

sensible than I myself am. But amid other more immediate and pressing avocations, and on a subject so comparatively novel, I have not been able to find leisure sufficient to condense the (for the most part) raw and scattered materials that I have had to work upon, into a form such as I could have wished. In some instances, also, I fear I may have appeared too hasty and dogmatical in the generalisations which I have ventured to draw; but if the limits of the present paper had allowed me to bring forward at the same time all the individual facts or data on which these generalisations were founded, I trust that any such impression as that I allude to would have been averted or removed. If the Essay, however, imperfect as it is, may be fortunate enough to turn in any degree the attention of the Society to the neglected but not altogether unimportant class of diseases of which it treats, I shall consider any time or labour that I may have spent in collecting and arranging the observations which it contains as fully and amply repaid.

XIV

JOHN REID

1809-1849

CAN ACQUIRED HABITS AND PHYSICAL CON-
FIGURATION OF BODY DESCEND TO THE
OFFSPRING?

Read 1836

MR. PRESIDENT,

It becomes an object of considerable
importance, not only in our attempts to modify
the breed of the domesticated animals, but also
for the better elucidation of some very interesting
points in Physiology, to endeavour to ascertain
what are the circumstances which influence the
propagation of these varieties, which may occur in
any species of animals. And as it appears to me
that in many of the discussions upon this subject,
the accidental changes upon the habits and disposi-
tions and upon the structure and appearance of the
body, by which I mean all those which have taken
place since the birth of the individual, have not
been sufficiently distinguished from the congenital—
all those with which the individual was either born
or which have appeared shortly after birth, and
apparently from causes which had been put in

operation during the growth of the fœtus; I hope that the following remarks may not prove uninteresting to the Society.

As preliminary to the observations which are to follow, I will offer a few remarks upon the terms, species, genera, and varieties. When we see a number of animals or plants agreeing in all peculiarities of structure, all of which were transmitted to them by parents similar to themselves, or when they do differ, if these differences can be accounted for by the known laws of the vegetable or animal economy, these are said to belong to the same species. Several species, though distinguished by peculiar characters, may yet have a general resemblance, by which they are more widely separated from others, may be ranged into a group, and this group constitutes a genus. From this it will be seen that the principles upon which the species may be determined are fixed and immovable, and if any doubts ever arise upon the subject, they must depend on our ignorance of facts, and not upon any difficulty in the application of them, while the formation of genera must depend upon the peculiar views, the whim or caprice, of the classifier. For one man may see a difference when another can see none, or one man, by viewing the subject from a different point, may perceive an analogy which can be seen from no other. Differences, however, may be effected upon the original organism of individuals of a species, by the influence of external circumstances, or the offspring of a perfect individual of a species may even differ in its original organism from its parents, and these constitute varieties. The latter at least, viz. where the organism is

originally different, are capable of producing individuals possessing their own peculiarities. From this a source of fallacy arises, for a variety, the history of whose origin is unknown, may at last come to be considered as a distinct species. Still, this depends entirely upon our ignorance of facts. We find botanists occasionally puzzled to decide in such cases whether a plant ought to be considered merely a variety or a distinct species. And it is very probable that many plants which are now considered as distinct species were not so originally; indeed, some have gone so far as to assert that most of the species of the same genus are, or may be, mere varieties.

Though the offspring in general resembles the parents, yet not unfrequently varieties occur, and we have just remarked that the origin of these may depend upon two causes,—first, differences may be effected upon the original organism of an individual of a species by the influence of external circumstances, and, second, the offspring of a perfect individual of a species may even differ in its original organism from that of its parent. We have also remarked that the latter at least, viz. where the organism is originally different, are capable of producing individuals possessing their own peculiarities. And this is so well known, and is so often acted upon, as to require from us no arguments in proof of it. We will only select a few facts in illustration.

Individuals with five fingers or six toes are found to have recurred repeatedly in the same family, and in the same manner a partial ichthyosis has been present at birth which has remained ever afterwards. Scrofula, mania, and asthma are found

to occur much more frequently among particular families, and on this account these diseases have been termed hereditary. It is this fact which is constantly acted upon in the formation of new or improving of old breeds of the lower animals, when by the selection of individuals possessing some peculiarity or another, we can in general, in a short time, either produce an entirely new breed, or greatly modify the old.

Another circumstance to be considered here is that though varieties frequently arise, nevertheless, if they intermingle freely with the other individuals of the species, their peculiarities become lost in their descendants, and the general characters of the species are thus more widely maintained. This is well known to jockeys by the phrase 'breeding in and in.' If the offspring of a draught-horse and a hunter be selected, possessing the characters of each in an equal degree, this may be made in a few generations, by a proper management of the breeding, to assume the characters of the one only, those of the other being completely lost, viz. by the proper selection of hunters for the purpose, the descendants will become indistinguishable from other hunters; if draught-horses be selected, they will completely assume the characters of the draught-horse. Upon this the resemblance which runs through the mass composing a nation or tribe depends, notwithstanding that varieties are constantly springing up, and we can easily understand how this should be more apparent in a small tract of country, when the intercourse among the inhabitants is more complete. And by this we can probably also explain how the cattle and horses which

have run wild in Paraguay have almost all assumed the same appearance, while among the domesticated animals all the varieties of colour and appearance are to be found as in Europe.

It appears then to be allowed by every one that some kinds of varieties, viz. those in which the organism is originally different, can generate off-spring like themselves, and we now proceed to examine if this is equally true of the other kinds of varieties, or those in which the changes upon the original organism have been induced by the agency of external circumstances, subsequently to the birth of the individual. Upon the truth or fallacy of this position very important conclusions depend, for it is evident that if the proofs in favour of it are found inconclusive, all the attempts made to explain the varieties of the human race, or of the domesticated animals, by the influence of climate or any other external circumstance, must be totally abandoned, and in the selection of animals for propagating a breed we must carefully distinguish the varieties dependent upon original conformation from those which have been induced after birth. That external circumstances can produce astonishing effects both upon the moral and physical constitution of man, no person can for a moment deny, but it is equally undeniable that the differences between a Negro and a European are perfectly well marked in early infancy. And since their peculiarities cannot be explained by the operation of external agencies, as they have not yet been exposed to any, it is clear that the question resolves itself into the physiological law which we are about to examine, whether or not acquired changes upon the constitution of an indi-

vidual can be transmitted to his offspring. And the ascertainment of this law may enable us to set aside a great deal of discussion upon these varieties, and promises, if fully carried out, to lead to more accurate results.

For example, much has been said of the striking differences to be observed among the different castes into which, from moral and political causes, the Hindoos are divided, when the same condition of life and the same occupation are continued for successive generations. If these are only to be observed among the adults of the different castes, it only affords an excellent example of the great changes to which man is liable from varied external circumstances, the truth of which no person who has attended to the subject for a moment ever doubts. I have been unable to ascertain whether a similar difference is to be observed among the children of these castes during their early infancy, and whether the child of a Pariah (the name given to one of the lowest castes) might not differ little, if any, from that of a Brahman if they were brought up and educated together.

If we are to consider all the Hindoos as consisting of the same stock, about which I suppose there can be little doubt, it would be very desirable to ascertain this, and until that is done any arguments drawn from the differences to be found among that people ought I conceive to be totally abandoned. Supposing even that it could be rendered evident that an original difference between the offspring of these two castes actually existed at present, yet before this could be adduced as an argument in favour of acquired habits affecting the offspring,

it would require to be shown that these differences could not be explained by the constant tendency to the production of congenital varieties in all species of animals, but more particularly those which are placed under artificial circumstances, such as man and the domesticated animals.

The advocates for the all-powerful effects of climate in producing the varieties found among animals, have always assumed the possibility of the offspring being affected by causes operating upon the parent as a thing undoubted, without bringing forward any satisfactory proof in its favour. There are many and powerful facts which stand directly in opposition to this opinion. We know that various nations attempt to mould their bodies into particular shapes; some of the American tribes use bags of sand to flatten their foreheads; several savage nations elongate their ears; the Chinese compress their feet into one-third of their natural size; the Jews remove the prepuce by circumcision; and yet though these previous means have been put in practice for many centuries, it does not appear that they have had any effect in producing an exemption to the offspring from these painful and often cruel methods of distorting and maiming different parts of the body. We are in the habit of removing the ears and tails of many of our domesticated animals; the human body is frequently mutilated by accident or disease, yet we never expect that this will affect the offspring. Considerable importance has been attached by those who believe that acquired habits can descend to the offspring, to a remark made in the report drawn up by MM. St. Hilaire and Lennes, on a paper by M. Roulin in the sixteenth volume of the

Annales des Sciences Naturelles, upon the change
which the domesticated animals have undergone by
their introduction into the New World.   In that
report it is stated that the observations of M. Roulin
prove the transmission by way of generation of certain
acquired habits ; for M. Roulin has shown that the
wild horses of that country, the offspring of horses
which have been trained to an ambling pace, have
transmitted to their offspring this singular manner of
progression.   On consulting the original paper it ap-
pears to me that this is not exactly a fair inference
from the facts recorded by M. Roulin. M. Roulin
states that in that part of America where he resided
(New Grenada), the horses are early trained to an
ambling pace, and that great care is taken that they
practise no other. At certain seasons the finest of the
stallions are let loose amongst the mares running
wild, and that there results from this a breed of
horses in which the amble is the natural pace.
Now M. Roulin nowhere states whether or not the
wild horses have this ambling pace, nor does he
assert that the stallions thus let loose have no
opportunity of imparting this artificial pace to their
offspring by their example, for we have every reason
to suppose that the principle of imitation exercises
its influence upon the young of the lower animals
as well as in the human species.   Before the obser-
vations of M. Roulin can afford sufficient data to
justify the conclusions which G. St. Hilaire and
Lennes have drawn from them, it would be necessary
to show that the offspring would practise this arti-
ficial pace under circumstances where they had no
opportunity of acquiring it from others by imitation.
In a paper upon the domestication of animals by

Dureau de la Malle in the twenty-first volume of the *Annales des Sciences Naturelles*, he attempts to illustrate what he considers to be 'universellement reconnu, chez des animaux soumis à la puissante influence de l'homme, les modifications de forme, de couleur, les qualités physiques et même les qualités morales et intellectuelles sont transmissibles par la génération.'

In a paper upon a similar subject in the ninth volume of the same work by Frédéric Cuvier, we find that he is of the same opinion. There are two reasons, besides others, which may be mentioned, which lead me to believe that these authors have by no means succeeded in proving the truth of this opinion which appears to be so generally acknowledged. In the first place, F. Cuvier has himself sufficiently shown that the dispositions of the lower animals are so greatly modified by the circumstances under which they are placed, that the same animals may be rendered fierce or mild, shy or familiar. He has shown that the hyena, from having been observed under certain circumstances only, has become proverbial for its sanguinary cruelty, while the same animal, when treated with mildness, will come to the feet of his master and solicit him for caresses and food; that the dog when brought up in a sequestered place with his master attacks every other person that comes in his way; and that it is often dangerous for any person except the herdsman to approach cattle accustomed to feed in solitary places. He adduces many examples to show that the most erroneous notions have been entertained upon the dispositions of various animals from their having been examined under certain circumstances only.

These facts would induce us to place little confidence in the instances brought forward by Dureau de la Malle, and upon which he principally rests the truth of his position, and by which he attempts to show that the domesticated animals of the present day are originally of a milder disposition than in the time of the ancients. In the second place, M. Dureau de la Malle does not draw a line of distinction between the acquired and congenital dispositions of the animals, and consequently, granting that these have undergone the changes for which he contends, he has not attempted to prove that the powerful influence which man indubitably possesses of modifying the breed by limiting and directing the sexual intercourse of those individuals which best suit his purpose, may not have been sufficient to account for this. Those who maintain that acquired habits, or, as they are sometimes erroneously styled, acquired instincts, can descend to the offspring, have a host of other facts at command, all confirming, as they believe, this opinion.

It has been stated by Roulin, and triumphantly appealed to by others in support of this opinion, that the constant practice of milking cows in Europe has induced the habit in these animals of continuing the secretion of milk after the calf has been removed, while on the contrary the wild cattle of Columbia only give milk while their calves are with them.

We might have simply contented ourselves with objecting to the paucity of the details, which prevents us from judging of the data from which the narrator drew his opinion; however, the conclusion itself which is deduced from it by those entertaining the notion of the transmissibility of acquired habits,

supposing the facts related to be perfectly correct, is also liable to strong objections.

To procure the continuance of the secretion of milk in domesticated cows it is necessary the udder be daily emptied. Every dairy-maid knows the yielding of the milk by the cow is so far a voluntary act, that she can suspend it at her pleasure, and that when a cow has been long milked by one particular person, she often refuses her milk to a stranger. Now what is more likely to happen than that a wild cow should refuse to yield her milk after her calf is removed, and that the secretion of milk should in this manner be arrested? Besides, if habit had been able to superinduce upon the nature of the animal the continuance of the secretion of milk after the removal of the calf, how does it happen that this should be so suddenly lost in their descendants which have run wild?

Some, as Mr. Lyell, entertain a considerably modified view of the opinion against which we are contending. He believes 'that acquired habits derived from tuition are rarely transmitted to the offspring, and when this happens it is almost universally the case with those merely which have some obvious connection with the attributes of the species when in a state of independence.' I have seen no satisfactory proof even of this modified view. It has been a favourite opinion of some that the cultivation of the moral feelings and intellectual faculties in the parent must exert a genial influence upon the congenital aptitude for moral and intellectual excellence in the offspring, and that as the tide of civilisation rolls on, it must thus be constantly receiving fresh impulses and additions

at every stage of its progress. I am afraid that this is an opinion adopted without much previous reflection by individuals of a vivid imagination. For it remains to be shown that when a nation has been long placed under circumstances highly favourable to its intellectual and moral improvement, each successive generation is born with finer moral feelings, and a higher order of mental qualifications, than that which preceded it. Are we entitled to conclude, for example, that the present generation in this country surpass in original mental endowments those that produced a Bacon, a Shakespeare, a Milton, a Sir Isaac Newton, and others of their contemporaries, men also of commanding and colossal intellects ? And though I am not inclined to think, with others who, taking a retrospective glance at the labours of these giants in science and in literature, exclaim that the days in which we live ' are the days of little men and little things,' yet I believe that though of late astonishing progress has been made in the arts and sciences from the accumulated labours of their numerous cultivators, and though a knowledge of these is more generally diffused among the community, yet it remains to be shown that the original mental endowments of the present generation surpass the generations long gone by.

If acquired habits and mental qualifications descended to the offspring, why should the child of a Hindoo be obliged to undergo the usual drudgery of learning the trade or profession of his caste ?

Some have gone so far as to maintain that even temporary states of mind, particularly of the mother, may descend to the offspring, and they pretend to

account for the extraordinary genius and military talents of Napoleon by the circumstance that his mother, a woman of great firmness of character, when pregnant with him partook of the dangers of her husband during the civil wars of Corsica.

The acquired habits and modes of life of the inhabitants of large towns exert a very material influence upon the physical constitution of their offspring, as is well exemplified in their comparatively greater liability to various diseases than the children of the inhabitants of the country. In fact, the rate of mortality, and this particularly in children, is so great in the very large towns, as to require a constant influx of people to keep up the number of inhabitants.

In examining the bearing of this fact upon the question we are discussing, it is at once evident that it cannot be considered as an evidence that acquired habits and modes of life can be transmitted by generation; it only shows that the influence which these exert upon the constitution of the parent may affect the offspring. And in endeavouring to ascertain whether this can be considered as a change induced upon the organism after birth affecting the offspring, it is necessary to understand fully the nature of this deleterious influence exerted upon the constitution of the offspring, and the manner in which it is induced.

It is probable that the greater tendency to certain diseases in the children in large towns depends almost entirely on a weaker constitution of body; at least we can refer it to no other cause in the present state of our knowledge on the subject, and we know that everything which tends to

increase the general strength serves to diminish the tendency to disease. The production of this increased susceptibility to disease (principally scrofulous diseases) in the children of large towns may arise from two causes :—first, it may arise from their being born with a weaker structure of body, and, second, this debility may be either induced or greatly increased by their breathing a less pure atmosphere, and being frequently less perfectly nourished and clothed than children in the country. What may be the relative influences of these two causes would require extensive observation to ascertain. It is however probable that the latter exercises a greater influence in this respect than the former.

It is plain, then, that the only part of the facts now under consideration which appears to favour the opinion that acquired habits and modes of life can influence the organisation of the fœtus, is the more frequent occurrence of an originally weaker organism in the children of large towns than in those of the country. And before even this can be adduced as an argument in favour of the opinion, it would be necessary to show that the occurrence of a weaker constitution in the children of large towns is more frequent than what would be accounted for by various causes connected with the modes of life in large towns, which may affect the nutrition of the fœtus *in utero*. To inquire how far different modes of life influence the nutrition of the fœtus *in utero* would lead us into a lengthened, and I am afraid unsatisfactory discussion, at least as far as the present question is concerned. It appears to me that we have not facts sufficient to

enable us to come to any decisive conclusion. I believe, however, that I am not going too far in assuming that from the more vitiated state of society in large towns, from the derangement of the system produced by keeping late hours, and from the highly artificial modes of life in which many of the inhabitants live, it follows as a necessary consequence that the perfect organisation may be interfered with, and that there may be a greater proportion of the children born with weaker organism than when the parents live more regular lives. And since this in these cases is the original structure of the body, we can easily conceive how it can descend to the offspring. The influence of modes of life upon the offspring cannot in the present state of our knowledge afford any analogical argument in favour of the opinion that any change induced upon the individual subsequent to birth can descend to the offspring, though it may show that any cause acting upon the mother which may determine the original organisation of the fœtus may descend to the offspring. This distinction must be kept fully in view, for we shall find that they are frequently confounded with each other.

Varieties frequently spring up when animals are placed under artificial circumstances, yet we cannot perceive any connection between the kind of variety and the circumstances to which the animal was exposed. We may also remark that these varieties are confined within certain limits, and never go the length of interfering with the characters of the genus.

All the attempts made to explain the varieties

of the human race by the influence of climate or modes of life have completely failed. We can scarcely refrain from smiling even at the acute Dr. Smith, when we find him attempting to account for the dark colour of the negro by supposing that the bile becomes inspissated under the skin, as if all the dark races (as has been well remarked) laboured under an habitual jaundice; for the woolly hair of the same people by the crisping effect of great heat upon the animal textures; for the elevated shoulders, small eyes, high cheek-bones, and broad faces of some of the Mongolian tribes which inhabit the northern parts of Asia and Europe by the distortion of the features which is naturally assumed while a shower of snow is driving in the face. It is almost unnecessary to remark that we frequently find long and lank hair in tribes which are fully exposed to the operation of the causes which are assigned for the presence of the woolly hair. In the Mongolian tribes inhabiting the central parts of Asia the same configuration of the face is found as in those inhabiting the northern part, and there undoubtedly the alleged exciting causes cannot exist.

It has been argued that climate and modes of life may, after the lapse of several successive generations, effect the various changes upon the human race which we find to exist, though we cannot explain how they produce these. A supposition of the kind without some facts in its favour appears to be entitled to very little consideration, and more especially as there are numerous facts and observations opposed to it.

As far back as any records reach, the variations

among the human race appear to have been as well marked as at the present day.

From an exact copy taken by Belzoni from an ancient Egyptian tomb, the peculiarities of the Negro and Arab are quite distinctly marked. Of course, we cannot prove that sufficient time did not intervene between the creation of man and the period when these tombs were formed for allowing these changes to be effected, but it must be admitted that this fact at least throws suspicion upon that explanation. And these suspicions gain strength when we are informed that frequent instances can be adduced where individuals of different races have been exposed to similar circumstances for many years, and where all the characteristic peculiarities of each were still retained.

We would not wish to affirm that climate and modes of life have no influence whatever upon the production of varieties, but we think that we have brought forward sufficient evidence to show that varieties thus produced end in the individual, and do not extend to the offspring. We likewise would not peremptorily maintain that acquired habits and physical configuration of body never descend to the offspring, but we think that we are justified by the facts adduced in affirming that this must be to an exceedingly limited extent, and that the opinion rests upon very insufficient data. And we have not been led into the adopting of this opinion by any excessive love of generalisation, for we are fully convinced that this is a fertile source of much error and much altercation. The application of these observations is sufficiently obvious to some hypotheses of Lamarck upon the change induced in the organism

of various species of animals by the influence of
habit and modes of life, and even, according to
him, by the wishes of the animal ; and also to a
hypothesis of G. St. Hilaire, by which he attempts
to show that the present animals found on the
earth's surface may be descendants of those animals
whose remains are now only found in a fossil state,
only altered in their structure by the influence of
varied external agencies.

XV

MARTIN BARRY

1802-1855

ON THE UNITY OF STRUCTURE IN THE ANIMAL KINGDOM

Read 1836

Mr. President,

All finite existences presuppose design. This is a position which, happily, in the present day we may assume.

It has been usual to regard organic structure as manifesting design, because it shows adaptation to the function to be performed. It has also been suggested that function may be equally well considered as the result of structure; and truly so it may, yet, perhaps, we are not required to show the claim of either to priority, but may consider both structure and function, harmonising, as they always do, as having been simultaneously contemplated in the same design.

The object of the present Essay is to offer a few considerations on structure only; but the subject is so vast, and our limits are so circumscribed, that these considerations must be of the most general

character. Yet some details on development will be found indispensable.

The expression 'organic structure' includes the structure of what we call animals and plants. But while both are comprehended as beings contemplated in the same original design, while the metamorphoses presented in a realisation of this design, and the remarks that may be made on development in general, will apply equally to both, it is intended to restrict the further prosecution of the subject to animals alone.

A law, not less vast in its importance than it seems to be general in its application, may be supposed to direct structure in the animal kingdom. This law requires that a heterogeneous or special structure shall arise only out of one more homogeneous or general, and this by a gradual change. The importance of this law appears to have been insisted on chiefly by Von Baer, who arrived at it by long and attentive observation of development.

Let us then inquire, in the first place, what analogy there is in the states of germs in general at the earliest period of observation, and whether they have in common a homogeneous or general structure.

In animals presenting the most simple manifestations of life, 'in which every point of the creature is, as it were, an epitome of the whole, without any relation to or dependence on the rest, and capable therefore, when separated from the rest, of an independent existence,' maturity alone appears sufficient to produce offspring, and simple separation sufficient to constitute a new being. Such is the case with many zoophytes.

Reproduction becomes less simple as vitality grows complex, because now 'every point of the creature has a more close relation to, and dependence on, the rest than before.' When something like ova begin to be discernible, they consist of a half-fluid, throughout homogeneous, more or less granulous mass. This is the state of bodies regarded as ova, in some infusoria, some polypes, and many other zoophytes. Bodies of this kind have been called 'germinal granules.' Such imperfect ova seem to hold a middle place between 'shoots' on the one hand, and 'germinal vesicles' on the other.

The ovum of more elaborate structures, perhaps of all the rest of the animal kingdom, is a sac containing a sort of yolk, the germinal vesicle, and a layer of granules.

The yolk of ova generally is very much the same in essential character, but performing a more important part in some animals than in others; it differs much in quantity.

The germinal vesicle is an exceedingly delicate, transparent sac, measuring in diameter sometimes less than $\frac{1}{100}$th of a line, and containing a pellucid fluid. On the internal surface of the germinal vesicle there has lately been discovered an opacity— the germinal spot (*Macula germinativa*) consisting of extremely minute granules, more or less spherical in form. With a magnifying power of eight hundred diameters, that is to say, magnified 640,000 times, this spot has not yet been found to consist of other than homogeneous parts. It has been already said that it is contained within the germinal vesicle, the latter measuring in diameter sometimes less than $\frac{1}{100}$th of a line.

In some infusoria the contents of the germinal vesicle are rather a mass of granules than a fluid and a spot, perhaps corresponding parts in a less concentrated state. Indeed, may not 'shoots,' 'germinal granules,' and the contents of the germinal vesicle, be, all of them, corresponding parts in different states of concentration ?

The layer of granules (germinal layer) containing perhaps in part the rudiments of the future germinal membrane, lies immediately on the internal surface of the primary membrane that contains the germ and yolk. This layer is more or less circumscribed, and often indistinct, because of its periphery coalescing with the yolk. The germinal vesicle is found lying in the centre of this layer of granules, on the surface of the yolk, though there are reasons for supposing that originally the germinal vesicle is situated in the centre of the yolk.

We have then, *firstly*, 'shoots,' as in many zoophytes ; *secondly*, 'germinal granules,' a half-fluid granulous mass, as in some infusoria, some polypes, and many other zoophytes ; *thirdly*, the ova of some infusoria, in which the germinal vesicle contains a mass of granules ; *fourthly*, perfect ova, of more elaborate animals, consisting of the following parts, viz. *a*. germinal spot ; *b*. fluid contained in germinal vesicle ; *c*. germinal vesicle ; *d*. layer of granules, having the germinal vesicle in its centre; *e*. yolk ; *f*. primary membrane enclosing the germ and yolk ; *fifthly*, superadded, in mammals and in man, there are the Graafian vesicle and its fluid.

There are reasons for supposing that the germinal vesicle is formed before the yolk, one of which is afforded by its relatively greater size ; and, if so, the

germinal vesicle, with its contents, constitutes the primitive portion of the ovum, which in all animals where found, appears to be essentially the same.

It has thus been shown that in all classes of animals, from infusoria to man, germs at their origin are essentially the same in character, and that they have in common a homogeneous or general structure.

It appears also that essentially the manner of the metamorphosis, or metamorphoses, from a more homogeneous or general structure to one more heterogeneous or special, *i.e.* the manner of development, is universally the same.

Such a proposition seems deducible from what we know of development, not only in all the vertebrata, but also in many invertebrated animals, such as the insecta, crustacea, arachnida, and even mollusca, and Von Baer seems to have meant the observation to apply to animals in general when he spoke of development proceeding by ' a continued elaboration of the animal body through growing histological and morphological separation.'   Even zoophytes themselves, so far as their development extends, may also be included.

The layer of granules, already spoken of as having in its centre the germinal vesicle, appears, on the bursting of the latter, to contribute to the formation of the germinal membrane, though the central and most important part of the latter is perhaps constituted by the contents of the germinal vesicle.

The germinal membrane in some of the vertebrata is at first a more or less circumscribed disc, covering only a part of the yolk, and afterwards extending itself to surround and enclose the whole of it ; in others it incloses the whole of the yolk

from the first. This membrane in the invertebrata presents differences in this respect regarding which physiologists are not quite agreed.

In most vertebrated animals the embryo is at first nothing more than the exuberant growth of a part of this germinal membrane near its centre, *i.e.* in the situation occupied by the germinal vesicle, before the bursting of the latter, the exuberant part, projecting, but not being distinguishable from the rest by a well-defined border. The projecting portion becomes more and more distinct, until its growing independence is manifested in a tendency to withdraw itself from the remainder.

This separation of the central part of the germinal membrane from its periphery, and from the yolk, gives rise eventually to the appended umbilical vesicle in man and other mammals. In birds the corresponding part is taken into the abdomen. In frogs the embryo occupies from the first so large a portion of the germinal membrane, and the latter so nearly surrounds the yolk, that the yolk becomes contained in the embryo before the independence of the latter has time to manifest itself by a tendency to withdrawal.

The manner of development seems to be as follows :—The germinal membrane separates into two disjoined layers, viz. into a mucous or vegetative and a serous or animal layer, the latter being in contact with the primary membrane enclosing the germ and yolk, the former lying immediately upon the yolk itself. The vegetative layer is afterwards seen to be composed of two intimately united laminæ, viz. the proper mucous and the vascular. The animal layer also, in the embryo at least,

divides itself into two laminæ, viz. into the skin on the one hand, and into a mass containing the fleshy layer, as well as in vertebrated animals the osseous and the nervous layers, on the other. This division into layers is the *primary* separation. During the course of this separation the layers become tubes, or fundamental organs.

There occurs at the same time a separation of tissues in the substance of the layers or tubes, cartilaginous, nervous, and muscular substance, separating from each other, while a part of the mass becomes fluid. Some of the elementary parts or tissues also assume the form of laminæ, which are subordinate to the original làyers; the latter therefore (now tubes) become the central portions of systems. This separation into tissues is the *histolological* separation.

Besides the above there arise differences in outward shape, single sections of the tubes being developed into distinct forms or organs destined to perform particular functions, which functions are subordinate members of the function of the whole tube, but differ from the functions of other sections of the latter. For example, the mucous tube divides itself into the mouth, œsophagus, stomach, intestine, respiratory apparatus, liver, urinary bladder, etc., the peculiarity in the development being connected with either an increased or diminished growth. This is the *morphological* separation.

Thus, by a threefold division, the mass becomes heterogeneous; and the further back we go the more do we find, not single organs only, but histological elements united.

'Fresh parts are acquired, not by new forma-

tion, but by transformation. When, for example, the foundation of a cartilage forms, there was not previously a vacancy in the place it occupies, but a homogeneous mass, the change in which consists in the appearance of an assemblage of opaque granules, and a surrounding pellucid fluid. This is the manner of histological separation, calling forth, as it were, antitheses.'

'No part is formed that was not previously in connection with some part earlier formed; no part has an isolated origin, then adding itself to the rest. Nothing swims freely around, annexing itself here or there, as formerly was said of the whole embryo, and even lately has been conceived and taught of the spinal cord. Each organ is a modified part of a more general organ.' This is the manner of morphological separation.

It was to uniformity in the manner of the primary, of the histological, and of the morphological separations just described, that we referred in the proposition that essentially the manner of the metamorphosis, or metamorphoses, *i.e.* the manner of development from a more homogeneous or general to a more heterogeneous or special structure, is universally the same, and we have already mentioned researches which seem to warrant this conclusion.

The direction taken by development is, however, not the same precisely in any two animals, and in different classes the direction differs very widely. But of direction we shall treat more particularly hereafter.

It has then been shown that there is a great analogy in the states of germs from infusoria to

man, and we know that there are some structural characters common to all animals in a perfect state ; especially to those of the same class, as, for example, the vertebrata; there are besides resemblances between some of the more elaborate structures in certain of their embryonal phases, and many less wrought out structures in their permanent conditions, which resemblances are observable, not only between animals included in the same great class, but also, though more remotely, between animals belonging to different classes.

To sum up these important facts : If the structure of germs has been found in animals at 'both ends' of the animal kingdom, as well as in the intermediate classes, to be essentially the same; if between the homogeneous masses, forming germinal membranes, there is found no essential difference ; if the primary separation of this membrane into layers (the vegetative layer being always directed towards the yolk), and the subdivisions of these layers, incipient in the membranal, and completed in the embryonal states, are the same in character ; if the formation, not of tissues only, but of organs also, proceed in the manner just described ; and, above all, if permanent structures, among many of the less elaborate animals, resemble most obviously different degrees of histological and morphological separation, as presented in the embryonal phases of an individual destined to be more wrought out ; are we not entitled to conclude, not only that a more heterogeneous or special structure arises only out of one more homogeneous or general ; but also that, essentially, the manner of the metamorphosis, or metamorphoses—the manner of development—from

the latter to the former state is universally the same?

And are we not then led fairly to the conclusion that all the varieties of structure in the animal kingdom are but modifications of, essentially, one and the same fundamental form?

Now, seeing that not only the vertebrata, but all classes of animals, in their development must pass thus gradually from a merely animal form to the most special forms they respectively attain; further, that the manner of development may be considered as essentially the same in all, is it surprising that there are resemblances between some of the embryonal phases of very different animals; and that some of the stages in embryonal life of the more elaborate structures resemble perfect states of those that are less wrought out? Could it, indeed, have been otherwise?

Let us inquire a little more particularly into the development, firstly, of the vertebrata; and, secondly, of some invertebrated animals.

Vertebrata.—The layers into which the germinal membrane separates become, as already said, tubes. These tubes are more or less bent towards the yolk, at each extremity; but extend the whole length of the animal, including its head and tail. Therefore, out of the upper tube, constituted by a union of the laminæ dorsales, are formed the arches of the caudal, lumbar, dorsal, and cervical vertebræ, the arched cranial bones, and the soft parts covering all of these, together with the central portion of the nervous system. While out of the under tube, constituted by a union of the laminæ ventrales, are formed the ribs, the soft parts of the thorax and

abdomen, the hyoid bone, and all that portion of the neck, anterior (or inferior) to the vertebræ, the lower jaw, and some other parts, both osseous and fleshy, of the face.   The bodies of the vertebræ, and the base of the cranium, are formed out of a portion of the animal layer of the germinal membrane, common to the upper and the under tube.

The central portion of the nervous system in different animals may, in its ultimate elaboration, produce very different structures,—all grades between the splendid cerebral hemispheres in man, and the mere rudiments of hemispheres in fishes.   The nervous portions of the organs of sense are, in all the vertebrata, processes of the central portion of the nervous system, through the laminæ dorsales ; so that, though so varied in different animals, not only all parts of the central portion of the nervous system, but all processes from the latter, with a common origin, and the same manner of development, may well bear a general resemblance to each other in the perfect states of the less, and the embryonal states of the more, elaborate animals.

The muscles of the trunks of different animals of the class vertebrata are but modifications of the fleshy portions of the laminæ dorsales and ventrales, and the muscles of their extremities are only similar metamorphoses of those portions of the latter that are carried out with the osseous (or at first cartilaginous) foundation of the extremities themselves.

All the resemblances in the vascular system of different animals are, in like manner, referable to a common origin, and the same manner of development, and its varieties to various modifications in direction and degree.

P

The mucous tube originates as processes the mouth, œsophagus, stomach, respiratory apparatus, liver, urinary bladder, and other organs; in part also, and in conjunction with the vascular tube, the genital organs; which parts, in all their varieties, bear a general resemblance to corresponding parts in different animals.

In the substance of the fleshy portion of the lamina dorsalis and ventralis of each side, there is formed an osseous arc constituting the radical portion of the extremities of the superior maxillæ, etc.; and, from a point near the middle of this arc, there issues a process corresponding to the middle and terminal members of the latter. Now, it is obvious that with this common origin, and the same manner of development, corresponding parts in different animals of the class vertebrata, whether arms, legs, wings, fins, maxillæ, etc., are likely to retain a general resemblance, though the absence of the middle members, or modification of the whole extremity, etc., may render them very dissimilar in their details.

Corresponding parts of structure may, however, in different animals, perform very different functions. Thus, besides the extremities just mentioned, many other examples might be given, such as a fact pointed out by Geoffroy St. Hilaire, that certain parts of the hyoid bone in the cat correspond to the styloid processes of the temporal bone in man; and the different functions of the generative organs in the two sexes afford a still more remarkable example.

In development, germs, and even embryos, belonging to different groups of the same great class,

may long be indistinguishable, and still longer those that are more nearly allied. But those belonging to different great classes begin to diverge sooner, or rather, the angle of divergence being greater, a difference is appreciable at an earlier period, and in proportion to the angle of divergence in a germinal, are the structures unlike in a perfect state. Just as in a tree, those branches that have been given off nearest to its root become most widely separated in their terminating twigs.

In different classes, development, though it proceeds in the same manner, yet, taking thus different directions, attains, with materials perhaps essentially the same in primordial structure, very different ends.

Thus it proceeds in the vertebrata or osteozoa with especial reference to the central portion of the nervous system; in the arthrozoa (which include, besides the articulata, some zoophytes), havin gfor its chief object the organs of locomotion. In both of these classes, therefore, it is the serous or animal layer of the germinal membrane that is seen first advancing, and out of this, in these two classes, there is thus produced a very different system of organs.

In the gastrozoa (*i.e.* the mollusca and most zoophytes), on the other hand, the organs of nutrition are especially the object, and in them, therefore, development proceeds chiefly in the mucous or vegetative layer.

To these priorities in development, and to the important influence they have on the direction which development takes in other parts of the system, are referable the leading characters of
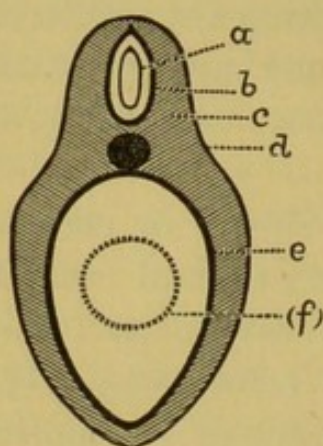
classes. Yet it is in direction only that development can be said to differ in different animals; in manner it remains the same.

Invertebrata.—The following diagrams will illustrate different directions of development, though the manner be the same :—

IDEAL TRANSVERSE SECTIONS SHOWING THE STRUCTURES FORMED OUT OF THE ANIMAL LAYERS.

OSTEOZOA (Vertebrata).

ARTHROZOA.

FIG 1

FIG 2
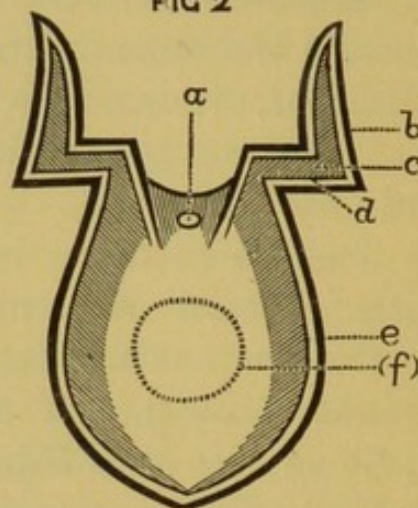
FIG. 1.—OSTEOZOA.

Upper tube—
- *a,* Central portion of the nervous system, situated in the upper part of the animal layer.
- *b,* Arches of the vertebræ, some of the cranial bones, etc. (part of the internal skeleton).
- *c,* Fleshy layer.
- *d,* Skin.

Under tube—
- *e,* Ribs, lower jaw, etc. (part of internal skeleton); the other parts of this tube as *c* and *d* of upper tube.
- (*f*), Mucous tube.

FIG. 2.—ARTHROZOA.

Upper tube, incomplete, viz.—
- *a,* Situation of what there is, corresponding to the central portion of the nervous system situated in the lower part of the animal layer.

b, External skeleton secreted from the skin ⎫
c, Fleshy layer, such as it is . . . ⎬ forming an extremity,
d, Skin . . . . ⎭ a mandible, etc.

Under tube—

 e, External skeleton secreted from skin ; the other parts of this tube as *c* and *d* of upper tube.

(*f*), Mucous tube.

It is obvious from the above,

First. That in the osteozoa, the central portion of the nervous system ; in the arthrozoa, the organs of locomotion, mandibles, etc., are the especial objects in the early stages of development.

Second. That the central part of the animal layer is appropriated accordingly. Thus it may perhaps be said that parts corresponding to the laminæ dorsales of the osteozoa go to form the extremities in the arthrozoa.

Third. That the upper tube in the arthrozoa is imperfect, though there is evidently a tendency to its formation.

Fourth. That, from the direction taken by their extremities, the arthrozoa must move about with the thorax and abdomen uppermost, the relative position of the fundamental organs being reversed. The internal parts, too, or organs formed out of the mucous and vascular layers, are found to be inverted, if compared with corresponding parts in the osteozoa ; but there occurs such an adjustment in the situation of the external parts, as, for example, in that of the mouth and organs of sense ; and, as what in the osteozoa is the extensor becomes in the arthrozoa the flexor side of the body, that, so far as these are concerned, it cannot be said that the arthrozoa move about on their backs. Rather may it be affirmed, with Valentin, that 'they have no

true back, but only the tendency to form one.'
But their thorax and abdomen are certainly inverted.

Fifth. That the situation of what these animals
have of the central portion of the nervous system, is
a part of the body corresponding very nearly to that
occupied by the central portion of the nervous
system in the osteozoa, viz. it is in the former
(arthrozoa) situated in the under, in the latter
(osteozoa), in the upper part of the animal layer,
supposing each of these classes of animals to be
situated above the yolk.

Sixth. That the term 'dorsal' vessel is calcu-
lated to mislead, the part so called obviously corre-
sponding to the aorta in other animals, and,
according to the above diagram, having a truly
thoracic and abdominal locality.

Of the development of molluscous animals we
know very little; enough, however, to render it
quite safe for us to extend to them the laws, already
laid down, of the heterogeneous arising only out of
the homogeneous, and of identity in the manner of
histological and morphological separation, the man-
ner of development, whatever may be the direction
which the latter takes, and however limited the
degree.

Even to zoophytes the same laws may be applied,
—the germinal granule of the polype, a homogeneous
shapeless mass, separating into a softer portion on the
one hand, and a more rigid, horny, or calcareous sub-
stance on the other, and assuming its proper, more
or less special form. Shoots, even those, for example,
of the hydra, at first simple swellings, then cone-
like, afterwards more or less cylindrical, and
gradually funnel-shaped, like the parent; processes

then appearing wart-like at the circumference of the common cavity, and these by degrees elongating into arms.

The whole animal kingdom, then, perhaps all organised beings, may be considered as directed, in development, by the above laws ; and all animals present besides, the antithesis of an internal or vegetative, and an external or animal portion of the body.

It seems as if, with the original design to create organised beings, there had arisen a scheme of more or less complete division and subdivision, continued down to species, and including in the latter all individual forms.

One of the supposed grand divisions may have included animals ; one of the first subdivisions, the type of the vertebrata ; subordinate to which, and co-ordinate to each other, we have the types of fishes, reptiles, birds, and mammals. Each of these groups presents its families, each family genera, each genus species, and every species has its individual forms. So would the other classes admit of being referred to subdivisions of the supposed scheme.

In thus speaking of classes and other divisions of the animal kingdom, however, we by no means acknowledge the present arrangement to be perfect. The only sure basis for classification is—not structure, as met with in the perfect state, when function tends to embarrass, but—the history of development, at that period when structure presents itself alone ; and, as Von Baer has justly said, this will perhaps ' one day become the ground for nomenclature,' as it can be the only one on which to form

a correct estimate of parts in different animal forms.

Certain elements proceeding from the elements of an individual, or from the elements of two individuals of the same species, constitute, by a separate or distinct existence, another individual—a germ, destined, like its parent or parents, to undergo, by a succession of elements, continued changes in its component parts, and, by degrees, to attain a state of being represented by a form belonging to the parent-type.

These elements, while they constituted part of the parent or parents, shared the state of being peculiar to the latter. It is, then, easily conceivable that, having themselves acquired a separate or distinct existence, the new being they constitute should contain within itself properties analogous to those of its parent or parents ; and that, therefore, in its progress towards its destined state of being, it should undergo similar changes ; that it should attain the parent-type, and also more or less of individual resemblance to its parent or parents.

The elements of every germ must have innate susceptibilities of a certain definite arrangement, so that on the application of stimuli there results a certain structure. These we shall in future call innate susceptibilities of structure, or innate plastic properties. All innate plastic properties are, of course, derived from the parent or parents. If the germ be animal, its leading properties are those characterising animals in general. But it has others common respectively to the class, order, family, genus, species, variety, and sex to which the germ belongs. Lastly, it has properties that were pre-

viously characteristic of its parent or parents, in which, indeed, all the others are included. But no innate properties, except those merely animal, are at first, to our senses at least, apparent in the structure of the germ.

The sum of these innate plastic properties determines the direction taken in development, determines, therefore, the structure of the new being.

The general direction taken in the development of all the individuals of a species is the same; but there is a particular direction proper to the development of each individual, and, therefore, a particular structure not identical with any other; for in no two individuals is the sum of the innate plastic properties in all respects the same.

As the elements of an individual cease in turn to be constituent parts of the same, the identity of that individual must be continually changing, can exist, indeed, at no two periods of time, inasmuch as new elements are continually entering into its constitution, while old ones are departing.

Hence individual peculiarities in structure must, in their turn, become hereditary to succeeding sets of elements, continually renewed, as we have just asserted these elements to be. Nay, more, there must continually present themselves fresh peculiarities, and, in their turn, these also must be inherited by sets of elements succeeding.

For the same reasons the first set of elements constituting a germ, proceeding, as already said, from the elements of a parent or parents, must possess properties that were characteristic of the latter at the moment when their separation took

place ; and can, indeed, possess no others, since the
elements of the parents are continually changing.

Hence it is that the sum of the innate properties
can be in no two individuals the same ; hence the
particular direction of development proper to each
individual ; and hence individual peculiarities of
structure.

The innate plastic properties include, as already
said, some that are characteristic of animals
generally, and others common to all the animals
contained in that division of the animal kingdom
to which the species is subordinate. Now the pro-
perties characteristic of the parent or parents at the
time of the separation of the germ must include all
of those transmitted to the latter.

This assists us to understand why properties of
the same kind should all, in a modified form, reappear
in the development of the offspring ; and, indeed,
since it is plain that ' every step in development is
possible only through the condition preceding,' that
' becoming depends upon having become,' we see
why those properties can reappear in a certain order
only—viz. in the order of their generality in the
animal kingdom.

Thus, in development, the structure character-
istic of the vertebrata only, cannot manifest itself
until there has been assumed, essentially, a struc-
ture common to animals, of which the vertebrata are
but a part, and to whose type the type of the verte-
brata is subordinate. In like manner, structures
subordinate to the type of the vertebrata cannot
manifest themselves until after a modified appear-
ance of the general type, of which they are but
partial metamorphoses. More and more special

forms are thus in succession reached, until the one most special is at length attained.

To the law requiring that a more fundamental type shall uniformly manifest itself before the appearance of one more subordinate, is perhaps referable the formation of parts that seem to answer no other purpose than the fulfilment of this law—viz. parts that either continue rudimentary through life, or, not being used, disappear.

An example of the first occurs in the appendix vermiformis of the caput cæcum coli in the human subject; of the second, in the embryonal gills of land- and air-vertebrata, which latter, having at no period an aquatic respiration, can never use gills. Development proceeds to a certain point—though this point may differ in different animals—in obedience to the law requiring that a more fundamental type shall uniformly manifest itself before the appearance of one more 'subordinate; so that the special purpose to create birds, quadrupeds, and even man himself, is, as it were, subordinate to the more general purpose, to create a vertebrated animal. This explanation will perhaps apply to all parts present in a rudimentary state alone.

It has thus been shown that a heterogeneous or special structure arises only out of one more homogeneous or general, and this by a gradual change ;—that the manner of the change is probably the same throughout the animal kingdom, however much the direction and degree of development may differ, and thus produce variety in structure, which, however, there is good reason to believe, is in essential character fundamentally the same ;—that no two individuals can have precisely the same innate susceptibilities

of structure or plastic properties, and therefore, that though all the individuals of a species may take in their development the same general direction, there is a particular direction in development proper to each individual ;—that structures common to a whole class must, in a modified form, reappear in individual development ; and lastly, that they can reappear in a certain order only—viz. in the order of their generality in the animal kingdom.

It has been our endeavour throughout this paper to limit the idea of fundamental unity of structure to essential character alone, specific, and even individual, peculiarities, however inappreciable, forbidding more. Each germ, even when presenting the merely animal type, must do so in a modified and peculiar form, on which the nature of its future metamorphoses depends ; and if in the course of embryonal life there occur resemblances in certain parts of structure to corresponding parts in other animals, they are no more than resemblances, since individualities cannot be laid aside.

There is a danger in the present day of generalising too freely, of carrying transcendental speculation much too far, of being so captivated by 'the idea of a subjective unity that real variety may be lost sight of : as bright sunbeams veil myriads of worlds that might show to mortal man what they are compared with his world, and how little he is in the latter.'

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XVI

WILLIAM BENJAMIN CARPENTER

1813-1885

ON THE PHYSIOLOGICAL INFERENCES TO BE DEDUCED FROM THE STRUCTURE OF THE NERVOUS SYSTEM IN INVERTEBRATA.

Read 1839

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MR. PRESIDENT,

I much regret that the shortness of the time allowed me for the preparation of this Essay should have prevented me from doing that justice either to my subject, to the Society, or to myself, which each might have fairly required at my hands. I must apologise in the language of Pascal : ' I had not leisure to be briefer.'

The department of Physiology which embraces the phenomena of the nervous system, is one which is universally confessed to be so difficult that it needs all the light which can be brought to bear upon it from any quarter for its perfect elucidation. Amongst the sources of information which lie open to the student, comparative anatomy is certainly among the chief, and it is perhaps to be wondered at that more use has not been made of the facts which it supplies. It has been with the view of bringing

together the results of the investigations of various recent labourers in this extensive field, in such a form as to admit of comparison and generalisation, that the present Essay has been undertaken; and whatever merit this Essay may possess must be looked for in the comprehensiveness of the survey which has been taken, and in the probability of the inferences drawn from the facts brought under consideration.

There can be no doubt that where a nervous system exists, the vital actions of the being are influenced by it; but it would require evidence of a different character from any that has been yet adduced, to prove that they are dependent upon it.

It is obvious that just ideas on this subject should be at the very foundation of our system of nervous physiology; and they will even influence our views of its anatomical character. For in the extensive group of acrita, in which are associated the lowest of all the types of structure presented in the higher forms of the animal kingdom, the existence of a 'diffused nervous system' is commonly regarded by naturalists as the necessary alternative resulting from the want of any definite indications of its presence.

If, then, we allow any sensibility, consciousness, and voluntary power to the beings of this group—to deny which would be in effect to exclude them from the animal kingdom—we must regard these faculties as associated with nervous filaments so delicate as to elude our means of research; and, when the general softness of their textures, and the laxity of structure which characterises the nervous filaments in the lowest animals in which they can be

traced are kept in view, little difficulty need be felt in accounting for their apparent absence.

The lowest animals in which nervous filaments can be distinctly traced are the echinodermata among the radiated classes; the entozoa among the articulated series; and the tunicata among the molluscous tribes. In each of these we shall find the type of their arrangement to be different, though there are links of transition which unite the most characteristic forms of each group with one another.

The Echinodermata have not been usually regarded as possessing any other sense than that of touch, which seems to reside in their extensile feet, and especially in those modifications of them which are placed round the mouth, and serve especially as tentacula. Ehrenberg, however, is disposed to regard certain red spots at the extremity of the rays as rudimentary organs of vision; and states that the nervous trunk is continued towards each, and swells into a sort of ganglion where it is connected with it. This is at present very uncertain; and the only observed fact which would seem to confirm the supposition is the readiness with which they seem to perceive objects of food at a little distance from them. But this may be 'due to some modification of the general sensibility of the body, allowing of the perception of impressions in some degree allied to the sense of smell in higher animals, and related in character to the kind of sensation by which the actiniæ and other polypes are able to appreciate the presence of light, though absolutely deprived of visual organs.'

A nervous system has been detected in some of the class acephalæ. In the Beroe, for example,

a circle with diverging filaments has been traced by Dr. Grant surrounding the œsophagus as in the Echinodermata; and in some of the larger Medusæ nervous filaments have been traced, and eyes are supposed by Ehrenberg to exist at the margin of the mantle. These forms, however, approximate so closely to those already described, that little more can be said respecting them.

Feeble as are the animal powers in a great proportion of the molluscous tribes, they would seem to be almost extinct among the members of the class Tunicata or Acephala nuda.

So far as the regular vital operations are concerned, we see no indication of any voluntary actions in these animals, or even of that kind of respondence to external impressions which would lead us to suspect the existence of a connected nervous system. But in the simultaneous contraction of the whole muscular sac which is occasionally witnessed, we can scarcely fail to acknowledge the operation of nervous agency. If one of these animals be touched when its cavity is full of water, a jet of fluid is thrown out to some distance.

We shall find, accordingly, on examining into the character of the nervous system, that it is most simple in its structure and distribution, and that it only appears to be connected with the contractile sac. It usually consists of a single ganglion lying between the two orifices, and sending filaments towards each, as well as others which ramify upon the muscular sac. Such a conformation is exhibited in the nervous system of the Ascidia mammillata.

We next pass on to the Conchifera or Acephala testacea, a class which, though somewhat higher in

the scale than that just described, has a very close
affinity with it. There is a good deal of variety in
this class as to the degree of sensorial and loco-
motive power possessed by its different species;
and we shall find the structure of the nervous
system to vary in exact proportion, from a type
little higher than that of the single ganglionic
centre of the Tunicata, to the far more complex
apparatus of the Gasteropoda.

As an illustration of the lowest form of the
nervous system among the Conchifera, that of the
oyster may be selected. In the nervous system of
the oyster we observe a slight advance upon that
of the Tunicata. The principal ganglion is situated
at the posterior muscle between the branchiæ, and
hence may be called the posterior ganglion. It
obviously corresponds, both in situation and in its
relation to the respiratory organs, with the single
ganglion of the Ascidia. It sends branches to the
mantle, others to the branchia, small twigs to the
posterior muscle, and two trunks which connect it
with the labial or anterior ganglia.

In the Pecten the relative situations of the
anterior and posterior ganglia are not very dissimilar
from those which they hold in the oyster, except-
ing that the latter have partially separated so as to
form a bilobed mass, which in other instances
(*Modiola*) becomes completely double.

In the next class of Mollusca, the Gasteropoda,
we recognise a type of the nervous system essentially
the same as that which has just been described,
but modified to correspond with the conditions in
which the animals are formed to exist, and especially
with changes in the situation and development of

their locomotive and sensory organs. Although none of this class possess very active powers of locomotion, none are entirely fixed; all are more or less dependent upon the exercise of these powers for their supply of food; and the higher tribes employ them also for the perpetuation of the race, since the connection of two individuals is in them an essential part of this function.

In every division of the animal kingdom we find the development of special sensory organs to bear a close relation with that of the locomotive apparatus. This is well seen in the common snail, which, 'although at rest within the shelly covering that forms its habitation, will with great quickness perceive the proximity of scented plants which are agreeable articles of food, and promply issue from its concealment to devour them.' It is not a little curious, however, that although the general surface appears exquisitely sensitive to impressions which excite responsive movements adapted to fulfil some important office in the economy, it does not seem to be susceptible of painful impressions in anything like the same degree. This, which cannot but be regarded as a beneficent provision for the happiness of animals so helpless and so exposed to injury, would appear from the observations of various experimenters, and especially from the testimony of M. Ferussec, who says, ' I have seen the terrestrial gasteropods allow their skin to be eaten by others, and, in spite of large wounds thus produced, show no sign of pain.'

A still greater subdivision of the nervous centres is described by M. Van Beneden in the Linneus glutinosus.

From these complex forms, which show us the distinctness of parts which appear simple, we may advantageously pass on to one which exhibits the nervous system in the most concentrated aspect that it presents in this class, that, namely, of the Tritonia. In this class, the sensory apparatus, the foot, the mantle, and the branchiæ are the organs which seem to require nervous centres for the reception of impressions and the excitation of respondent motions. These centres are modified, both as to situation and development, in accordance with the situation and development of the organs which they supply; and it is from their connections only that we can judge of their character.

The class of Cephalopoda is a most interesting one in many respects, exhibiting to us the modification of the molluscous type (which is perhaps most characteristically presented in the Gasteropoda) produced by their proximity to the vertebrated division of the animal kingdom. In no organs is this modification more evident than in the nervous system; for, whilst in the lowest members of the group we find it approximating closely to the form it presents in the higher Gasteropods, its whole character and relations in the most elevated species are so like those which exist in the lowest fishes, that the analogies between their several parts may be traced with little hesitation.

In the Nautilus we observe a transverse ganglionic cord lying upon the œsophagus, swelling at its extremities into the optic ganglia, and communicating with two collars which complete the ring below. This transverse cord evidently corresponds with the cephalic ganglia of the Gasteropods, giving off not

only the optic nerves, but also filaments to the mouth and tongue (which are apparently of a sensory character) as well as branches that connect it with separate labial ganglia presently to be noticed, which, as in the Patella, lie at a considerable distance anteriorly. The anterior of the sub-œsophageal collars evidently corresponds in part with the pharyngeal ganglia of the Gasteropods, here increased in size and importance on account of the increased development of the buccal apparatus with its powerful mandibles, firm fleshy tongue, salivary glands, and contractile pharynx, and brought into close approximation with the cephalic ganglion. The greater part of the tentacula receive filaments proceeding directly from the anterior part of this collar ; but the internal labial processes are supplied in a different manner. A flattened ganglionic mass lies at their base, which is principally connected with the pharyngeal collar, but also with the cephalic ganglion, as already mentioned. This labial ganglion sends twigs to the internal labial processes, and also to the parts which are regarded as olfactory laminæ. From the posterior collar, which evidently corresponds with the sub-œsophageal ganglion of the Tritonia and other Gasteropods in which it forms but a single mass, filaments are distributed to the shell muscles, and four others arise from it which extend backwards along the vena cava. Of these, the two internal form a plexus upon the vein, whilst the two external swell into ganglia, from which ramifications are distributed to the branchiæ as well as to the digestive and reproductive organs. This distribution resembles that found in many of the higher Gasteropods inhabit-

ing spiral shells ; and the system of nerves may be termed branchio-visceral. Although the anterior sub-œsophageal ganglion has been spoken of as corresponding to the pharyngeal of Gasteropods, it must be regarded as uniting this with their palleal ganglion, since the external respiratory nerves, those, namely, which supply the muscular edges of the mantle and the muscles of the funnel, by whose movements the respiratory currents are produced, proceed from it. There is nothing surprising in this change of situation, since we have already had to notice how constantly the position of the nervous centres is governed by that of the organs they supply. The central masses of the nervous system in the Nautilus rest upon a firm cartilage, which, however, does not inclose them, but gives attachment to the powerful muscles of their neighbourhood. In the higher order this internal skeleton makes great advances towards the form which is characteristic of the Vertebrata.

We shall now take a general review of the structure and offices of the nervous system in the Mollusca, in order to deduce from the details which have been brought together such general conclusions as they may seem to warrant. It will be only by determining the characters of the different parts of the system in the highest of its forms, and tracing it downwards through its progressive stages of degradation, to the simplest indication of the molluscous type (such as is presented in the Tunicata) that we shall be able to recognise the true nature of the latter.

The supra-œsophageal mass in the Sepia evidently corresponds principally with the optic lobes in the

brain of fishes — the tubercula quadrigemina of higher Vertebrata. The anterior portion of it, however, may perhaps be regarded as the rudiment of the cerebral hemispheres, more especially as from this arise in part the peduncles which connect with the central mass the labial ganglion, whose function seems in part to minister to the sense of smell, especially in the Nautilus. If this should be regarded, therefore, as combining the characters of an olfactive ganglion with some other (to be presently inquired into); its situation will evidently resemble that of the olfactory lobes in the brains of many fishes, which are isolated by peduncles from the general mass. The infra-œsophageal mass pretty evidently represents the medulla oblongata, giving origin, as it does, to the auditory and respiratory nerves, as well as to those of general sense and motion. It is only the situation of the locomotive organs around the head that occasions the giving off of these nerves from one spot, and that the anterior portion of the œsophageal collar. We have seen in other classes how moveable the pedal ganglion is; and we can easily conceive that, if the feet had been at the opposite extremity of the body, this portion would have been removed to a distance from the remainder of the mass, and would have become the analogue of the spinal cord of Vertebrata, which (so far as relates to the extremities) is a kind of prolonged locomotive ganglion. So far, then, we have little difficulty in recognising the characters of these portions of the nervous system. But what is the nature of the branchio-visceral nerves sent off from the posterior part of the collar, and of the venous plexus? There can hardly be a doubt that

these unite the offices of the sympathetic with those of the respiratory portion of the *par vagum*; and that they convey to the ganglionic centre those impressions from the branchiæ, the stimulus of which is necessary to keep up the respiratory movements, effected by the dilatation and contraction of the mantle excited by the motor nerves proceeding from the neighbourhood. Such a union of the sympathetic and *par vagum* appears to exist through an interlacement of their filaments, to a greater extent than has been hitherto supposed, even in man and mammalia; but it is far greater in fishes.

We may next inquire into the character of the stomato-gastric system, consisting of the labial and pharyngeal ganglia, with the œsophageal plexus and cœliac ganglion. Here would seem to be conjoined in one system, and that but little dependent upon the cephalic mass, the nerves concerned in the prehension, deglutition, and digestion of food. What have we analogous to this in Vertebrata? If we contrast it with the highest forms of the latter, we should be at a loss for an analogue; but upon looking at the nervous system of fishes, we perceive the connection between the fifth pair, the glosso-pharyngeal, the *par vagum*, and the sympathetic, to be so well marked that a natural group is at once established, which is evidently analogous to the one under consideration. In the higher Vertebrata these connections still exist, being established by the interchange of fibres, although there is less evident inosculation. We may, then, with much probability, regard the labial ganglia as partly analogous to the Gasserian, and the pharyngeal to that upon the glosso-pharyngeal nerve of Vertebrata. It will

scarcely be an objection to this view that to the
labial ganglion has already been assigned a partici-
pation in the function of smell; since it is well
known that the branches of the fifth pair are
distributed to the olfactive organs, even where
their separate nerve is fully developed; and the
advantage of associating this sense with the others
immediately concerned in the supply of the digestive
system, those, namely, of touch and taste, is obvious.
By this association the impressions made upon these
sensory organs may give rise to the respondent
motions which they are adapted to excite for the
supply of the digestive organs, without the inter-
vention of the cephalic ganglion, with which their
communication is small; just as impressions made
upon the fifth pair and glosso-pharyngeal in Verte-
brata will excite respondent motions when the brain
has been removed.

There would seem much reason to believe that
ganglia are situated wherever sensory impressions
are thus destined to excite motions; and this may
probably be the rationale of the curious structure
which has been described in the arms of the Sepia.
The suckers seem capable of contracting and fixing
themselves, either in obedience to the will of the
animal, communicated to them along the non-gangli-
onic cords from the central mass, or in respondence
to a stimulus excited by contact, and acting through
the afferent and efferent nerves of the ganglion. In
like manner we may explain the structure of the
pedal ganglia of Gasteropods, as demonstrated by
Leuret.

The small size of the superior cephalic mass in
these animals, relatively to that of the inferior

portion, entirely coincides with what is known of their actions, which seem to be of that purely instinctive character to which a highly developed brain is not essential, however complex and beautifully adapted to their requirements they may be. Their movements appear immediately destined to the acquirement of food and the propagation of the race. One action of the naked species has been regarded as evincing intelligence which scarcely deserves such a character. This is the ejection of the contents of the ink-bag, which takes place when the animal is alarmed. By this means the water around it is coloured so deeply as to enable it to escape from its pursuers. Is it to be supposed, however, that the animal goes through a chain of reasoning so complex as that required if it executes this action with the intention assigned to it ? Is it not rather an involuntary or instinctive action, analogous to the expulsion of the contents of the rectum and bladder under the influence of fear, which takes place in higher animals, and which many of the human species know by experience to result from an impulse of an uncontrollable character ? This view is strengthened by the fact that the secretion of ink is really analogous to that of urine.

As we descend towards the Tunicata, in which no separate cephalic ganglion exists, we observe it gradually becoming less and less predominant, at the same time that it loses its connection with special organs of sense. It is obvious that where visual organs are developed, the impressions made upon these will determine the movements of the animal more than those of any other kind ; and it

would seem to be chiefly owing to the information which they communicate that the cephalic ganglion has such an evident presiding influence, even where smaller than others in the same system. This is, however, more the case in animals whose movements are rapid, and in which, therefore, the perception of distant objects is peculiarly important, as in the articulated classes. Even where eyes are absent, and the animal still possesses locomotive powers, the cephalic ganglia will be their principal directors, since the most delicate organs of touch are situated in connection with them, on the foremost part of the body, and therefore in the direction towards which the animal usually moves. Although, therefore, we find no separate cephalic ganglion in the Tunicata, we must regard it as consolidated with the other centres into the single ganglion which exists between the orifices of the mantle; since through it are produced the movements which result from irritation of the tentacula.

The most constant, perhaps, of all the ganglia in the Mollusca is that which is connected with the respiratory organs or the branchial ganglion. The evident respiratory motions are stimulated through a distant set of nerves; but there is reason to believe that some influence is also propagated along the efferent or centrifugal portion of the *par vagum*, though its nature has not been satisfactorily determined. In the Cephalopoda we find the respiratory movements evidently excited in a similar manner, the impressions being propagated to the sub-œsophageal mass from the branchial trunks, which produce reflex actions in the motor nerves of the mantle and siphon.

We must now direct our attention to the other great division of the Invertebrata, the Articulated series; and we shall inquire how far the inferences we have drawn from the study of the nervous system in the Mollusca are applicable to the explanation of the phenomena presented by these.

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The nervous system of the larvæ of Insects resembles almost perfectly that of the Annelida, consisting in both cases of a chain of ganglia disposed along the ventral surface, similar to one another in every respect, and of a cephalic ganglion more or less developed, according to the perfection of the sensory organs connected with it. Besides the symmetrical nerves which come off from the cord at its ganglionic enlargements, and which consist (as in the former instances) of two portions derived from its ganglionic and aganglionic columns, we find, as in the lower classes, a series of nerves given off at intermediate points, without any apparent swelling at their points of divergence. It is not easy to ascertain the true connections of these except in the thoracic region, where the ganglionic columns usually diverge laterally, especially when the metamorphosis is taking place into the pupa state. It is then seen that a third column exists on the superior or visceral side of the ganglionic tract, and that these nerves are given off from minute ganglionic enlargements upon it. Although they communicate with the nerves of the symmetrical system, they have a separate distribution, being transmitted to the muscles of respiration, especially those which govern the opening and closing of the stigmata. They would thus seem analogous to the nerves of

the gills and mantle in the Mollusca, and may be regarded as corresponding with the pneumonic portion of the *par vagum* in Vertebrata, with its associated motor nerves. It is to be recollected that the respiratory apparatus of insects is diffused throughout the whole system, and that its presiding system of nerves must be proportionally extended. It is an interesting fact that the ganglionic enlargements on its central chain are scarcely perceptible until after the animal has undergone its metamorphoses, when the function of respiration is remarkably increased both in amount and importance.

The number in variety of the reflex actions which take place in insects, etc., after decapitation is very remarkable, and they seem to have a consentaneousness proportioned to the degree of concentration of the nervous centres in the particular species. Where the ganglia appropriated to each segment remain distinct, an impression made equally upon the afferent nerves of all will produce a consentaneous action.

If we regard the ganglionic portion of the cord as sensory, and the non-ganglionic as motor, how are we to account for this reflection of impressions, since the two columns are distinct along the greater part of their course? And are we to suppose that the parts of the body of the perfect insect in which no ganglia are found are destitute of sensation while they retain their motor faculties? This would seem highly improbable, since these are the very parts in which the least active movements take place, whilst the ganglionic matter is carried to those segments which give attachment to the members whose reflex actions are so remarkable.

The stomato-gastric system of insects is in general highly developed, and its analogies to the sympathetic and *par vagum* of Vertebrata become more evident. That which may be selected as a characteristic form is the one presented in the *Gryllotalpa vulgaris.* Two distinct portions of this system are here apparent, one on the median line, and the other disposed laterally. The median system appears to originate in a small ganglion anterior to the cephalic mass, with each side of which it communicates. The ganglia of the lateral system are two on each side. The anterior pair are the largest, and meet on the median line, just behind the cephalic ganglia ; behind and externally to these lie the second pair at a distance from one another. Two cords pass backwards on each side— one derived from the anterior, the other from the posterior ganglion. They run along the sides of the œsophagus and dorsal vessel, inosculating with the respiratory system of nerves, and being finally distributed to the digestive viscera, where they assist in forming the ganglia already mentioned. The connections of these two systems would seem to indicate that the first may be regarded as analogous to the gastric portion of the *par vagum,* and the second to the sympathetic of Vertebrata. The anterior ganglion, however, also sends filaments to the mouth, which may be considered analogous to the glosso-pharyngeal.

It is scarcely necessary to extend this Essay to the consideration of the nervous system of the Crustacea and the Arachnida, since these will not supply us with any data which we have not already obtained from other sources.

XVII

JOHN BROWN

1810-1882

ON THE ADAPTATION OF THE EYE
TO DISTANCES

Read 1839

MR. PRESIDENT,

The eye has the power of distinct vision at different distances. It is said to be adapted to this end by some change in its general figure, or in the relative position of its parts. I shall shortly enumerate and explain the very various opinions as to the mechanism of this singularly beautiful adjustment.

The opinion of Kepler was, that the contraction of the processus ciliaris changes the form of the eye, and by the elongation of it places the crystalline lens at a greater distance from the retina. Descartes imagined that the crystalline humour itself became altered in its curvature by the contraction of these ligaments, or rather by its own muscular action, these ligaments constituting its tendons. Delahire maintained that there was no change in the figure of the eye, and that in order to view objects at different distances, all that is

necessary is an alteration in the size of the pupil. This opinion was held by Haller, and long after was defended by M. Le Roi. He brought forward in corroboration the fact that the nearest objects may be viewed with ease and distinctness through a small hole in a card. Moreover, employing an artificial eye, he found that when with a large aperture the images of near objects were confused and ill-defined in its retina, they became very distinct by contracting the aperture. Huygens conceived that the crystalline approached the cornea by the pressure of the external muscles, or that the lens might be made more convex by the same action. Dr. Monro made a set of experiments to prove the effect of the orbicularis palpebrarum. He first opened his eyelid wide, and endeavoured to read a book, the letters of which were so near the eye as to be indistinct. He found he could not do it. He then, keeping the head in the same relation to the book, brought the edges of the eyelids within a quarter of an inch of each other, and made an exertion to read. He then found he could see the letters distinctly. He concludes that in this action of the eye the iris, the recti muscles, the two oblique muscles, and the orbicularis palpebrarum have all their share. Dr. Porterfield, after a very full account of all previous opinions, asserts 'that the change in our eyes consists in the motion of the crystalline,' that 'the ligamentum ciliare performs this change,' and likewise increases the convexity of the cornea. His experiments are very numerous, and described with great minuteness. They prove most satisfactorily the incorrectness of Delahire's theory, while they establish the important fact

that the change in the conformation of the eye always follows a similar motion in the axes of vision, with which it has been connected by use and custom.

Dr. Juvin's supposition is that the uvea at its attachment to the cornea is muscular, and that the contraction of this ring makes the cornea more convex. He says that the fibres of this muscle may as well escape our observation as those of the muscle of the interior ring. Musschenbrock conjectures that the relaxation of the ciliary zone, which appears to be nothing but the capsule of the vitreous humour, where it receives the impressions of the ciliary processes, permits the coats of the eye to push forward the crystalline and cornea. We now come to the inquiries of Dr. Thomas Young upon this curious and vexed question. In them he has exhibited his characteristic mastery of details, simplicity of experiment, and comprehensiveness of conclusions. I shall shortly analyse his two papers in the *Philosophical Transactions*—the one in 1793, the other in 1801. In the first, after a rapid sketch of the theories already mentioned, with his objections to each as unsatisfactory, he takes up Descartes' notion of the muscularity of the crystalline lens. He states that before examining the structure of the lens he had been led by reasoning, and by the observation of Dr. Porterfield that those who have lost the lens by couching for cataract have no longer the power of accommodating the eye to different distances, ' to conclude that the rays of light emitted by objects at a small distance could only be brought to foci on the retina by a nearer approach of the crystalline to a spherical form.' Such a power, he

imagined, could only reside in the muscularity of part or the whole of its capsule. Accordingly, he says, then, 'In closely examining with the naked eye, in a strong light, the crystalline of an ox, turned out of its capsule, I discovered a structure which appears to remove all the difficulties with which this branch of optics has long been obscured. On viewing it with a magnifier this structure became more evident. The crystalline lens of an ox is an orbicular, convex, transparent body, composed of a considerable number of similar coats closely adherent. Each of these coats consists of six muscles intermixed with a gelatinous substance, and attached to six membranous tendons. Three of the tendons are anterior, three posterior. Their length is about two-thirds of the semi-diameter of the coat. Their arrangement is that of three equal and equidistant rays meeting in the axis of the crystalline, one of the anterior is directed towards the outer angle of the eye, and one of the posterior towards the inner angle, so that the posterior are placed opposite to the middle of the interstices of the anterior, and planes passing through each of the six, and through the axis would mark in either surface six regular equidistant rays.' After a great deal more minute description of the microscopic appearances, he says : 'Such an arrangement of fibres can be accounted for on no other supposition than that of muscularity.' When a person wishes to view an object at a small distance, he conceives that the influence of the mind is conveyed through the lenticular ganglion to the orbiculus ciliaris, which may be considered as an annular plexus of nerves and vessels, and thence by the ciliary processes to the muscle of the crystal-

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line, which by the contraction of its fibres becomes
more convex, and collects the diverging rays to a
focus in the retina. The disposition of fibres in each
coat is admirably adapted to produce this change,
for, since the least surface that can contain a given
bulk is that of sphere, the contraction of any surface
must bring its contents nearer to a spherical form.
He then goes on to inquire whether these fibres can
produce an alteration in the form of the lens suffi-
ciently great to account for the known effects, and
after an elaborate calculation, he declares they can.
He alludes to Descartes's supposition, but shows that
he did not apply it so much to change in the lens
itself as to the elongation of the eye's axis by the
action of the muscular lens in the sides of the eye.
Leeuwenhoek also had described the course of the
fibres of the crystalline in a variety of animals, and
has even called it *crystallinum musculum alias hu-*
morem crystallinum dictum, but he has not applied
this structure to the explanation of the problem
of the eye's adaptation to distances. Such is the
substance of his first paper. In the other, which
formed the Bakerian lecture for 1800, he maintains
the same doctrine, to which John Hunter had been
led by some physiological observations. His second
dissertation is much more elaborate in its experi-
ments, and more general in its subjects than the
first. After a general consideration of the sense of
vision, he enumerates a number of dioptrical pro-
positions, and gives a description of an instrument
for ascertaining the focal distance of the eye, which
he calls the optometer. He then investigates the
dimensions and refractive powers of the human eye
in its state of rest, and the form and magnitude of

the picture which is delineated in the retina, and
concludes by inquiring what and how great are the
changes which the eye admits, and relating a
variety of experiments confirmatory of the different
suppositions. We shall confine ourselves to what
is strictly connected with the present subject. His
experiments for ascertaining if there be any change
in the length of the axis of the eyeball are very
ingenious and simple. He considers it necessary, in
order to account for the power of the eye in adapting
itself to the distance of objects, that the diameter
should be enlarged one-seventh, its transverse diame-
ter diminished one-fourteenth, and the semi-diameter
one-thirtieth of an inch. To determine this he fixed
the eye, and at the same time forced in upon its
ball the ring of a key so as to cause a phantom
accurately defined to extend within the field of
perfect vision : then looking to objects at different
distances, he expected if the figure of the eye were
altered, that the spot caused by the pressure would
be altered in shape and dimensions ; he expected
that instead of an increase of the length of the eye's
axis, the oval spot caused by the pressure of the
key resisting this elongation should have spread
over a space at least ten times as large as the most
sensible part of the retina : but no such effect took
place, the power of accommodation was as great as
ever, and the oval phantom remained unchanged in
size and shape. Again, he placed two candles so as
exactly to answer to the extent of the termination
of the optic nerve ; he marked accurately the point
to which the eye was directed ; he then made the
utmost change in its focal length, expecting that if
there were any elongation of the axis the external

candle would appear to recede outward upon the
visible space; but this did not happen, the apparent
place of the obscure part remaining unchanged.
From these two experiments he inferred that in the
act of adjustment there is no change in the shape of
the body of the eye. He next investigates the
condition of the cornea. One of his experiments for
this purpose is so beautiful, that we give it in his
own words :—' I take out of a small botanical
microscope a double convex lens of eight-tenths
radius and focal distance, fixed in a socket one-fifth
of an inch in depth, securing its edges with wax. I
drop into it a little water nearly cold till it is three-
fourths full, and then apply it to my eye so that the
cornea enters half-way into the socket, and is
everywhere in contact with the water. My eye
immediately becomes presbyopic, and the refractive
power of the lens, which is reduced by the water to
a focal length of about 16 tenths, is not sufficient to
supply the place of the cornea rendered inefficacious
by the intervention of the water; but the addition
of another lens restores my eye to its natural state,
and somewhat more. I then apply the optome-
ter, and I find the same inequality in the horizontal
and vertical refractions as without the water; and I
have in both directions a power of accommodation
equivalent to a focal length of four inches as before.
At first sight, indeed, the accommodation appears to
be somewhat less, and only able to bring the eye from
the state fitted for parallel rays to a focus at five
inches distance, and this made me once imagine that
the cornea might have some slight effect in the
natural state, but considering that the artificial
cornea was about a tenth of an inch before the

place of the natural cornea, I calculated the effect
of this difference sufficient to account for the
diminution of the range of vision.' This may be
considered a crucial experiment. Nothing can be
more simple and unavoidably conclusive. He then
'inquires into the pretensions of the crystalline
lens,' and in doing so, he first disposes of the grand
objection to the efficacy of a change in its figure,
derived from the experiments in which those who
have been couched for cataract have yet appeared
to retain the faculty of accommodation. It is need-
less to enumerate every particular experiment made
by him for settling this point, but the universal
result was contrary to his expectation that in an eye
deprived of the crystalline lens, the actual focal
distance is wholly unchangeable. Having therefore
cleared his subject, he goes fully into the develop-
ment of his opinions regarding the lens. He begins
by stating two experiments which he thinks come
very near to a mathematical demonstration of the
existence of an internal change in its figure, and
moreover explains its origin and its mode of opera-
tion. If a point be beyond the furthest focal
distance of the eye, it appears like a star, that is,
its centre is considerably the brightest part. But
when the focal distance is shortened, matters are
reversed. The imperfect image is enlarged, its
centre is faint, and its margin brilliant ; if the
slider of the optometer is applied, the shadows of
the slits, while the eye is relaxed, are perfectly
straight, but when the accommodation takes place
they instantly become curved. The same phenomena
occur when the effect of the cornea is neutralised by
immersion in water. The only account he can give

of these appearances is that the central parts of the lens acquire a greater degree of curvature than the marginal. From his investigations he abandons the opinion he formerly held, that the external coats of the lens have anything to do in this change, and enters with great keenness into the proof that nothing but the muscularity of the substance of the lens can suffice for this end. He then passes to the physiological argument. The first reference to the muscular nature of the lens, he says, was made by Dr. Pemberton, but he seems to have obscured the subject by intricate calculations. He argued for a partial change of the figure of the lens. Albinus favours this theory, Camper likewise, and Reil. Dr. Young then details a series of experiments which he made, at the suggestion of John Hunter, to ascertain if the crystalline could be stimulated to contraction by any artificial means. After great care and repeated attempts, they entirely failed to produce any sensible change by galvanism, or any other stimulus. He confesses also his inability to demonstrate the existence of nerves in the lens; and, as in the first paper, he gives a most minute account of the arrangement of the radiating fibres. This most elaborate memoir concludes with a re-capitulation of the results of his investigations,— 1st. He determined the refractive power of a variable medium, and applied this to the constitution of the crystalline; 2nd. The optometer; 3rd. By immerging the eye in water he demonstrated that its adaptation does not depend on any change in the cornea; 4th. By confining the eye at the extremities of its axis he found that no material elongation of it took place; 5th. He found the inability of persons

couched to adjust their eye ; and lastly, He deduced from the aberration of the lateral rays an argument in favour of an internal substantial change in the figure of the lens.

I have been so particular in my analysis of this memoir, both on account of its containing much that explains and refutes former theories, and likewise on account of its own singular worth as a philosophical exercise. There is now only another theory to be noticed, that of Sir David Brewster, as propounded by him in his *Treatise on Optics*, and more fully exemplified in a paper read before the Edinburgh Royal Society in 1823. As it is the latest, and, in my opinion, the best account of this interesting and puzzling question, I shall briefly analyse it. He declares that, though the most profound mathematicians and the most acute anatomists have done their best, this subject is as little understood as it was in the time of Kepler. This we think somewhat too strong after such a memoir as Dr. Young's. He then runs rapidly over the different theories. Of Dr. Young he says, ' He revived the doctrine of Descartes, and sustained it with all the ingenuity which might have been expected from his profound knowledge of optics and physiology,' but he thinks that his attempt at proving the muscularity of the lens has failed, and that, even granting that it were muscular, he says that, from the fibres running in lines of contrary flexure, no sagacity could predict the actual effect of their contraction, and that those of the outer laminæ would in the act of contraction press upon the uvea, and destroy that compound gradation of density by which the aberration of sphericity is so exquisitely corrected. Having ex-

pressed his dissatisfaction with all former theories, he prepares a new one. He begins by stating that, 'when the eye adjusts itself to different distances, it has long been observed that the pupil contracts on viewing near objects, and dilates in observing distant ones.' He then notices the false view Delahire took of this fact, in combination with a fallacious experiment, and remarks that other philosophers, seeing the general fallacy of his principle, regarded this very singular fact of contraction and dilatation of the pupil as merely a concomitant effect, depending entirely upon the varying intensity of the light. He gives as an instance of this neglect, Dr. Wells's observation that in the pupil being dilated by belladonna, the eye lost its power of accommodation, and the referring this very instructive fact away from its natural cause to some paralysis of the deeper parts of the eye. He then details his own experiments. He first found, by making use of two objects at different distances, but with the same degree of illumination, that this contraction and dilatation are produced by something else than the varying stimulus of light. 'In order to ascertain what took place at the two limits of the range of distinct vision,' he says, 'I took a paper and wrote upon it the three words, ON THE EYE. Having placed a fold of paper behind the word THE, and two behind the word EYE, I fixed the piece of paper at one end of a square draw-tube, and placed my eye at the other end, so that I could read all the words by the transmitted light of a candle behind the paper. The word ON was most luminous, THE less so, EYE still less. I now brought the paper as near my eye as possible, so that I could see the word ON

with perfect distinctness. When this was done, no exertion whatever could enable me to read the word THE, and still less EYE. I then looked at them through a small aperture, which, upon Delahire's principle, ought to have given me distinct vision, but it increased the indistinctness of the two last words. By making the words THE and EYE as luminous as ON, or by bringing another candle near the eye, so as to increase the contraction of the pupil, they could be read easily.' From this experiment he draws the three following conclusions :—
1st. That the contraction of the pupil which accompanies the adjustment of the eye to near objects does not produce distinct vision by the diminution of the aperture, but by some other action that accompanies it. 2nd. That the eye adjusts itself to near objects by two actions, the one voluntary, the other involuntary, from the stimulus of light. 3rd. That when the voluntary power fails, the adjustment may still be effected by the other.

He then proceeds to inquire what takes place at the other extremity of the range of distinct vision, namely, in viewing distant objects, remarking that if contraction be necessary to adjusting the eye to near objects, dilatation ought to have the same relation in the case of distant objects. After giving up the experiment with belladonna, as it may be said against it that the whole eye is paralysed, he says, 'It occurred to me that if the dilated condition of the pupil was essential to remote vision, all short-sighted persons ought to have their sphere of vision extended in the evening. I accordingly found upon inquiry that this was the case to a great degree, and that several

short-sighted persons could count the six stars of
the Pleiades, though unable to see objects distinctly
at a moderate distance during day. This remark-
able effect of the dilatation of the pupil may be
deduced from the converse process of observation.
If we look at distant objects while the light of the
sun is reflected on the eye, the voluntary power of
adjustment is still capable of dilating the pupil, so
as to produce distinct vision; but the tendency of
the iris to contract under the involuntary stimulus
of light produces such a painful feeling in the eye
as to leave no doubt, even if the dilatations were
not visible, that the iris was under the influence of
two different and opposite actions.' He concludes
by affirming that the mechanism by which the eye
is adapted to distances depends on the action of the
parts which are in immediate contact with the ball
of the iris. This mechanism, he conceived, may
produce the adjustment in four ways :—1st. By
elongating the eye during the contraction of the
pupil; 2nd. By increasing the convexity of the
cornea; 3rd. By altering the convexity of the cap-
sule of the lens; and 4th. By increasing the distance
of the lens from the retina. The first and second
are excluded by the observations of Dr. Young.
Sir E. Home and Mr. Ramsden say that the third
mode cannot produce the effect, because the *liquor
morgagni* in which the lens floats has nearly the
same refractive power as the aqueous humour; con-
sequently he thinks it almost certain that the lens
is removed from the retina by the contraction of the
pupil.

Such are the various theories on this very curi-
ous subject. The experiments of Dr. Young dis-

proved all previous explanations, and reduced the agent in this mechanism to something connected with the crystalline lens ; and Sir David Brewster seems to have further simplified the matter by his experiments and conclusions therefrom.   One great difficulty remains unexplained, how an eye, apparently quite perfect in its structure, should be short-sighted, and have no power of adjusting itself to distant objects, and yet on placing before it a concave glass, it not only sees distant objects at a certain point, but at various points, and has in fact now all the power of accommodation that the more perfect eye has.   There seems to have been too little regard paid to the power of attention, by which the mind wills that certain objects are to be exclusively attended to.   There is a great difference between seeing and looking, the one being a state, the other an act.

XVIII

JOHN GOODSIR

1814-1867

ON CONTINUED FEVER

Read 1842

MR. PRESIDENT,

An attempt to treat of continued fever within the limits of a single paper can only be successful when the whole of so extensive a subject is submitted to a careful analysis. The result ought not therefore to be fragmentary, but ought to afford general yet precise views of the whole question. That I have been successful in my attempt, I cannot pretend to say. One thing at least I may assert, that this dissertation will contain statements and questions which will afford members of the Society an opportunity of considering and discussing subjects of paramount importance to the practical physician.

Definitions are dangerous, and specific characters have not yet been detected in disease. I shall therefore merely indicate the leading features of continued fever.

Section 1.—An individual feels languid; he is ill,

without being able to say how he is affected, and is slightly chilly, as if he were scantily clothed. He sits down before a fire, with his head supported on his hands, indifferent about everything that is going on around him. He is not very thirsty, although his mouth is lined with scanty viscid saliva; and, besides, he feels as if cold water would increase the chill. He thinks that a little food will relieve his uncomfortable sensations, but after a few mouthfuls the increased tension across his forehead, the load which rolls from side to side within his head, and the tendency he feels to vomit, soon convince him that he must desist.

He throws himself on the top of his bed, and closes his eyes, but the increasing sensation of chilliness at the small of his back, and the clammy coldness of his feet, prevent him from enjoying any relief. He now undresses, gets into bed, orders his friends to push the clothes down into his back, and requests that warmth may be applied to his feet.

He begins to shiver outright, and feels as if it were brought on by contact with the cold sheets. Time and warm applications begin to produce a change in his feelings. He feels somewhat warmer, but with a dread of the return of the shivering. After a time he believes he must have been asleep, for he has had an uncomfortable dream, and has suddenly started into consciousness of his situation.

He is now rather hot, and the flying pains in his limbs, which had for a day or two rendered him uneasy, settle down in his legs and back. These pains are so annoying that he does not know in what position to place his limbs, and his back, he thinks, would be relieved if the bed-clothes were

pushed harder into it. At every beat of his heart he hears, as well as feels, the rush of the blood through his neck and head ; and the pillow under the latter is so hot that he changes it backwards and forwards, turns himself round upon his other side, and, at last, orders another pillow to relieve the burning sensation of the under side of his face and head.

He is greedily engaged with a glass of cold water when the doctor arrives ; and now, in the course of the short examination into the nature of his case, he is reminded that for some days previously his bowels had been sluggish, his stool scanty and unsatisfactory.

An emetic is exhibited, and during the period of relaxation which follows its action he is much relieved. Instead of being dry and rough, his skin becomes moister and more pliable. After the action of a purgative he feels still better, and begins to consider himself in the way of recovery.

In a short time, however, especially if it be drawing towards evening, he relapses into his former state ; his skin becomes dry and hot, his temples throb, his thirst increases, and the restlessness is incessant. In the course of the night his attendants observe that, although sensible when spoken to, yet he talks incoherently when left to himself. In the morning he is again cooler.

He thinks he has had a little restless sleep, but is not aware that he has been talking incoherently. He attributes the relief he experienced at break of day to the influence of the evening medicine, and, therefore, very willingly takes his morning dose.

The same state of matters continues for some days. He is worse towards evening, and at night gets no sleep, dozes, and is better towards morning.

All the symptoms continue to increase steadily in intensity.

About this period of the disease a change comes over him. Instead of lying on each side alternately, he rolls over on his back. The incoherency continues more or less throughout the night and day. His countenance expresses great anxiety, and his skin becomes very dry and harsh. Slight stupor appears. He answers questions snappishly. His eyes appear stiff and glazed ; his nostrils stuffed ; his lips chapped ; his tongue dry and brown, and is protruded slowly, stiffly, and tremulously. He applies his finger to his nostrils to remove acrid mucus. Bleeding from the nose may ensue in small quantity, or so profusely as to run back into his throat.

If the ear be applied to his breast, rattles and chirping may be heard all over the cavity. It also appears that he has been coughing more or less for a day or two.

If the palm of the hand be applied to the surface of the abdomen, it may be felt to be excessively hot, and probably slightly tympanitic. The pulse, which for a day or two had been rather weak at the wrist, may be felt to be very strong and full in the abdominal aorta and femoral arteries. If the forefinger be gently but steadily pressed into the abdomen, between the crest of the ilium and the navel, over the cæcum and small intestines, and if his countenance be at the same time watched, a slight expression of uneasiness may be observed. It may now be ascertained that, instead of being confined, his bowels had, for the last day or two, been slightly loose, and that, too, without the use of medicine. A little

blood may pass by stool, and, by-and-by, a very great quantity in clots, uncoagulated, or like coffee-grounds.

A papular eruption may now appear on the chest and arms, or red spots, like flea-bites, on various parts of the body, or large and livid blotches here and there on the back and limbs.

The patient may continue in this condition for eight or ten days longer, in a state of low delirium, or approaching stupor. When there is purging and tympanites the delirium assumes another type, namely, constant irritable muttering, without stupor ; a restless, terrified, and depressed expression of countenance. He keeps his heavy, glazed eyes constantly bent on those around him, and moves his tremulous hands, as if to prevent too near an approach to him.

About this time also the lips may become livid, and the chest dull on percussion in the posterior region. The abdomen becomes more swollen and tympanitic, and the diarrhœa may increase ; but more generally those symptoms are by no means well marked. He now begins to pick his lips and teeth, and to twitch up the bed-clothes. His wrists and arms exhibit muscular starting, and spasms of a tetanic nature may supervene.

From the commencement of the disease the pulse has continued to increase in rapidity, and to diminish in fulness and steadiness.

Death may now ensue in one of four ways :—(1) Often with the involuntary discharge of urine and fæces, the formation of bed-sores, shrinking and collapse of the whole body, hiccough, cold extremities, increased stupor, thready iregular pulse, contracted

countenance, and tracheal rattle. (2) With the addition to the last symptoms of lividity of countenance, with laborious and oppressed respiration. (3) With increasing weakness unaccompanied by increase of stupor, and ultimately with *facies Hippocratica* and insensibility. (4) Often heat of belly and tympanites, sudden distension of the abdomen, intense pain on pressure, and coldness of inferior extremities up to the hips. The patient does not survive this sudden attack above two or three hours.

Section 2.—What structural lesions are observed in the bodies of those who have been carried off during continued fever ?

(1) Death may take place, and no decided morbid appearance be met with. (2) The cavity of the head may exhibit marks of inflammation and its consequences, as well as congestion and effusion of serum. (3) The cavity of the chest may present inflammatory action in the bronchial mucous membrane, and congestion of the lungs. (4) The cavity of the abdomen and its contents may be affected ; sometimes as simple gastro-enteritis, but generally as a congested and inflammatory condition of the vesicular glands of the ileum, with a deposition into these glands of a peculiar matter, which is generally thrown off by a process which resembles sloughing. This change in the vesicular glands is apt to terminate in ulceration, perforation, and peritonitis.

It may, I think, with safety be assumed that when the function of an organised body is deranged its structure or molecular arrangement is so also, even when the eye can detect no change in the physical character of the part affected. Such minute changes

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are daily becoming better known to morbid anatomists, in consequence of the improved modes of research now in use ; and ultimately, without doubt, every deranged function will be referred to its corresponding alteration of structure. We may therefore at present, when considering the pathology of continued fever, group lesions of structure in two divisions :—(1) Lesions appreciable to the senses, and capable of being connected with known symptoms ; and (2) lesions not yet recognised by the senses, unaided, or with instrumental assistance, but producing nevertheless numerous and varied symptoms.

We have seen that death may ensue in a case of continued fever, and that no decided morbid appearances may present themselves. General febrile symptoms, and even symptoms referable to the head, chest, and belly, may be met with, and yet no appreciable lesion of structure can be detected.

It has been stated that the blood is altered, and I have no doubt that it is ; but so little is known on this subject, and that little is doubtful, that I shall not consider it as of any value at present.

Continued fever, then, may run its fatal course, while the structural lesions which undoubtedly occur altogether escape our notice. It is a disease which, for the present, we may consider merely functional—that is, depending upon molecular structural lesions. The hypothesis of Rokitansky is as yet unsupported by facts, and depends for its very existence on the ingenuity with which it is framed, and the explanation it affords of some of the phenomena of the disease.

Must we then consider the congestions and in-

flammations of the head, chest, and belly which so commonly occur during continued fevers as mere secondary affections, as not necessary to the constitution of the fever? We are not in possession of data sufficient to answer this question. It is sufficient for our purpose at present to know that continued fever may run its course without local structural lesion, but that such lesions do occur in a great majority of cases.

Section 3.—From these views of the structural lesions in continued fever, it is evident that the symptoms group themselves into two great divisions: those of the fever itself, and those of the local affections which may occur during its course.

Again, the symptoms arising from the local affection group themselves into two divisions: those strictly local, and those arising from sympathetic action of other organs. Hence the great difficulty of analysing and grouping the symptoms in many cases of fever, and the great importance of being able to do so. In proceeding therefore to analyse and to group the signs and symptoms in any given case of continued fever, we must ever bear in mind the following pathological laws :—(1) Local affections may be characterised by symptoms strictly local, but may also present symptoms apparently referable to other organs, and therefore denominated sympathetic symptoms. (2) The more urgent the sympathetic symptoms are, the more subdued are the local symptoms, and the more latent the affection itself. (3) If a local affection co-exist with a general affection of the whole system, the local affection has a tendency to become latent.

Thus inflammation of the intestinal mucous membrane not only presents symptoms strictly local, such as tympanites, pain on pressure, local arterial action, etc., but also sympathetic symptoms, such as intense headache, delirium, etc., from sympathy in the brain ; cough and hurried respiration from sympathy in the lungs. Again, the more violent the headache and chest symptoms, the less is the pain on pressure of the abdomen. The insidious nature of the local suppurations in phlebitis is a good illustration of the third law.

All these reactions, and others of minor importance, concur to render the grouping of the symptoms in continued fever very difficult, but at the same time absolutely necessary for successful treatment. Thus headache in a case of fever may arise from three causes :—(1) The general febrile state ; (2) an inflammatory condition of the cranial contents ; or (3) from sympathy with an abdominal lesion.

We must ascertain from which one, or from what combination of these, the headache arises, before we can proceed to prescribe for it with becoming decision and certainty. The same kind of analysis must be undertaken for most of the important fever symptoms before they can be satisfactorily treated, and will tend to dispel much of the mystery in which the pathology of continued fever is involved.

Section 4.—If we were certain that any particular case of fever was not complicated by local lesions, the treatment would be sufficiently simple. It would resolve itself into the use of general bleeding, emetics, and purgatives during the first or

active stage of the complaint, and the use of stimulants and other measures for supporting the powers of life during the latter stages. This is the treatment suggested by common sense (a faculty of more use to the practical physician than all the science of Newton), and approved of by the experience of every age. In treating cases of simple uncomplicated fever, it must ever be recollected that it has a tendency to terminate by what has been denominated crisis at certain periods, generally at the commencement of the hot stage, or on the seventh, fourteenth, or twenty-first days.

Bearing this in mind, we may make the attempt to cut the fever short at the commencement of the hot stage by an emetic, a purge, or abstraction of blood, keeping however two things in view, first, whether the patient's constitution is such as to bear this treatment, and, secondly, whether the fever is of a kind which will reduce him so much at last as to render it necessary to husband his strength for the trial. A proper decision on these points requires on the part of the physician, that experience, tact, and *long-headedness*, without which no power of diagnosis will avail.

If not cut short at first, the case must go on till another period of crisis arrives, the practitioner using every endeavour to keep it uncomplicated so as to admit of its progress to spontaneous cure.

He must not suppose, however, because there may be violent headache, that there must therefore be inflammatory action in the head. It may be merely the head symptoms of the febrile condition, and is to be relieved, and that only to a certain extent, by leeching, cold applications, and general

treatment.   The red suffused eye and throbbing temple must not lead him astray.   They are symptoms which may merely depend on want of sleep, and a full dose of opium will afford perfect relief.

Again, the hurried breathing may not arise from chest affection.   His ear will satisfy him of this. It may be a general fever symptom, and is to be relieved by general management of the case.

Lastly, the tongue may be in every known condition, and yet there may be no abdominal affection. Tenderness, flatulence, and distension are not surer tests.   They may all be general febrile symptoms, and may be relieved by the cautious use of a mild purgative.

When at last the patient begins to sink from continuance of the disease, the practitioner must not be afraid to give opium, wine, or ardent spirits to any extent the case may require.   The same tempered boldness which led him in the midst of doubt to bleed and purge freely at first, now induces him to prescribe stimulants to an extent no one around him would be inclined to do.   He never forgets that most important therapeutical law, that the same case may require directly opposite modes of treatment.

When it appears certain that a case of continued fever is complicated by local lesion, the following facts must be borne in mind in proceeding to treat it :—(1) Although the local lesion be removed, the fever does not necessarily cease.   (2) The persistence of the local lesion may be the cause of the protracted continuance of the fever.   (3) Local lesions may be marked by two circumstances : first, the general febrile symptoms, and, secondly, by the sympathetic

symptoms of the local lesion itself.   (4) Local lesions may be treated antiphlogistically, while powerful stimulant treatment is being applied for the general fever.   (5) The local lesion, although inflammatory, may require local stimulant treatment, in accordance with the general therapeutical law already stated.

Such are a few of the numerous considerations which must be borne in mind in treating a case of complicated continued fever. The management of such a case requires on the part of the physician a rare combination of talent : great boldness, great caution, perfect facility in taking two views of every circumstance, and last, although not least, great common sense.

The important subject of Prognosis in fever I can only hint at. Time will not admit that I should do more than merely allude to the unfavourable nature of hysterical and tetanic symptoms, and to the tedious nature of those cases in which sweating occurs.

Neither can I enter upon that most difficult question—the origin and mode of propagation of fever. I may state, however, my conviction that in time this country will be mapped out, so that the type of fever, be it cephalic or abdominal, prevailing in, or rather peculiar to, each district and town, will be ascertained by inspection of the map. Lastly, I believe that the type of fever prevailing in any district or number of districts varies according to circumstances,—social, meteorological, and geological,—with the laws of which we are not yet acquainted.

XIX

CHARLES MURCHISON

1830-1879

ON THE RED CORPUSCLES OF THE BLOOD

Read 1849

MR. PRESIDENT,

The importance in the animal economy of
the blood must be self-evident to all. The celebrated
John Hunter has justly remarked, 'Every part of
the body is formed from the blood—we grow out of it.'
And we have higher testimony than even Hunter's
to bear upon this, for in the Holy Scriptures we
find it written, ' The Blood is the Life of the Flesh.'
And not only does the blood prepare and supply
the various and numerous materials necessary for
the nutriment and growth of the bodily frame, but
it also receives and carries off these same materials,
after they have performed their peculiar functions
in the building up and subsequent disintegration
of the animal tissues, by which processes the life of
the whole body is preserved. It is therefore a
mixed fluid, or in other words is composed of two
fluids, one proceeding to the nourishment of the
tissues, the other coming from that work, and about
to be cast off from the body, or undergo certain

changes whereby it is again fitted for nourishing the tissues. These various processes would seem to be all affected by the agency of cell growth, and hence we naturally conclude that the red corpuscles (as well as the white) which are suspended in the *liquor sanguinis* constitute a very important, if not the most important, portion of the blood. What their more peculiar functions probably are we shall afterwards consider, but in the first place, as in every other physiological investigation, before considering function itself, we must have a correct knowledge of structure, of structure not only in a state of health, but also of structure as modified by disease. For this reason I think I may with propriety divide the present subject into the three following heads, viz. :—The histology, pathology, and physiology of the red corpuscles of the blood. And first their—

I.—HISTOLOGY.

Here we shall consider :—

1. The general appearance, etc., of the red corpuscles as seen in man.

I need hardly mention that the red corpuscles of the human blood are invisible to the naked eye, and require the assistance of a powerful microscope to determine their structure in a satisfactory manner. When examined under the microscope the human blood corpuscle is found to be a circular disc, the diameter of which varies from $\frac{1}{2600}$ to $\frac{1}{4000}$ of an English inch. Their thickness is about $\frac{1}{4}$ or $\frac{1}{5}$ of their transverse diameter. The corpuscles have not always the same size even in the same individual. This has been observed to be the case even in man, and

still more so in some of the lower animals. The corpuscles have the same form and volume in arterial as in venous blood. Though each blood disc is compressed from side to side, its surfaces are not flat, but considerably depressed towards the centre, as will be understood by looking at an imaginary section through the centre of one of the discs. Each disc is therefore much thinner at the centre than towards the circumference, and it is this central thinness which gives rise to the appearance of a spot, dark or light according to circumstances, in the centre of the red particle. When the corpuscles are examined under a brilliant light this central spot has often the appearance of being produced by a nucleus. A nucleus, however, is never present, and I shall afterwards endeavour to show that the corpuscles are themselves the nuclei of cells which have disappeared, differing considerably from the blood discs of reptiles, etc. As Dr. Carpenter remarks, the dark spot which is seen in the centre of the blood discs of mammalia is 'merely an effect of refraction in consequence of the double concave form of the disc.' The colour of the blood discs is generally said to be red. Other observers, however, as Professor Bennett, maintain that they are yellow, and that the red colour of the blood is produced by the immense number of these minute yellow bodies existing together, in the same way as a bottle of saffron powder appears red, though the individual grains are yellow. Certain it is, that the blood corpuscles, viewed under a microscope by transmitted light do appear to present a yellow colour, but such observers as maintain that they are yellow and not red would seem to jump too readily at conclusions, not reflecting that the phenomenon admits

of a far more easy and plausible explanation. Many
substances, we know, vary in their colour according
as they are viewed as opaque objects, or by trans-
mitted light. Take, for instance, the chromate of
silver. The crystals of this salt are dark green ;
but when viewed by transmitted light they
appear red, and such would also seem to be the
case with the blood discs. No one would dream
of saying that the crystals of chromate of silver
are red and not green ; and with as little reason
do we think, are the blood corpuscles said to
be yellow and not red. Chemical analysis shows
that the red colour of the blood corpuscles is partly
owing to the presence of iron, though its exact state
of combination has not yet been determined. Some
maintain that the colour of the blood remains even
after the removal of its iron. Berzelius believes
that iron in the colouring matter is in the metallic
state ' organically combined ' with nitrogen, carbon,
hydrogen, and oxygen, together with a small
quantity of phosphorus, calcium, and magnesium ;
Liebig again says that the iron exists in the state
of carbonate of protoxide in venous, and as hydrated
peroxide in arterial, blood. The corpuscles of the
blood are somewhat elastic ; thus they will often
regain their natural form after this has been altered
by pressure.

I now proceed to the consideration of :—

2. The differences exhibited by the blood cor-
puscles of the lower animals.

The red corpuscles of all mammalia agree with
those of man in having no nucleus. They are also
all of a circular form, except those of the camelidæ,
which are oval, but these also have no nucleus. In
mammals there is no connection between the size of

the corpuscles and that of the animals themselves in the different orders, but in the same order there is a correspondence. Thus the corpuscles of the larger ruminantia and rodentia are larger than those of the smaller species. The corpuscles of the elephant are the largest among mammalia. Those of the napu musk deer are the smallest.

Descending to birds, we find that in them, and indeed in all the lower vertebrata, the blood discs differ remarkably from those of mammalia, and that in two respects :—(1) In their form, which is seldom spherical, but generally oval; (2) In their being all possessed of a central nucleus which produces a central elevation in place of depression in each of them. In birds the long diameter of the corpuscle is rather more than double the short diameter. In a few cases the long diameter is three or four times that of the short. Those of the cassowary are the largest, and those of the humming-bird the smallest. The nucleus of the corpuscle is generally of a longer oval than the corpuscle itself. In reptiles the blood corpuscles are also of an oval form. The nucleus too is often very large and prominent. In the naked amphibia the corpuscles are generally larger than in any other animals, especially in those with permanent gills, as the siren. In osseous fishes the corpuscles have a rounded oval form, and the nuclei are nearly round. In the pike, however, the corpuscles are pointed at the ends. In the cyclostomatous fishes the corpuscles are of the same shape as in man, but slightly larger. In the amphioxus, which forms a transition to the invertebrate animals, the blood is colourless, and the corpuscles like those of lymph.

Bodies analogous to red corpuscles are also

found in the blood of invertebrate animals. In the mollusca they are generally of a pale colour, and their form is by no means regular. Among the articulata they vary remarkably in colour as well as in size. In terrestrial species they are generally of a greenish colour, as in caterpillars, whereas in the lobster and shrimp, which live in water, they are white. Corpuscles also exist in the blood of the radiata. The blood of the starfish Professor Goodsir has observed to contain organised particles of a reddish colour, as also that of the echinodermata. They may also be seen in the medusæ.

In many zoophytes, as in the genera tubularia and sertularia, the fluid, which is seen to circulate through their stem, is loaded with minute granules of nearly equal volume and shape, and endowed with a proper mobility. 'These particles,' writes Mr. Lister, ' present an analogy to those of the blood in the higher animals on the one side, and of the sap of vegetables on the other. Some of them appear to be derived from the digested food, and others from the melting down of parts absorbed.' The way in which they are produced, however, has not yet been ascertained.

Particles exactly similar may be seen in infusoria. Thus the proteus, the simplest of all infusoria, if submitted to a very high magnifying power, is found to contain an immense number of such particles, many of which, no doubt, are analogous to and perform the functions of the blood corpuscles in the higher animals.

We have thus seen that there exist in the blood of animals three different sorts of corpuscles, viz. :— (1) The non-nucleated corpuscle of man and mammalia ; (2) The nucleated corpuscle of birds, reptiles,

and fishes ; (3) The irregular granules of inverte-
brata, which also, along with the other forms, exist
in the blood of vertebrata.

In the blood of man and mammalia we have the
fully formed red corpuscle, which is always devoid
of a nucleus, and which is very different from the
corpuscle met with in birds, and other vertebrata.
These latter all possess a nucleus, and would seem
never to have reached such a high degree of de-
velopment as those of man and mammalia, but to
be referable to some earlier stage. The corpuscle
of a mammal may be thus theoretically regarded as
analogous to the liberated nucleus of the corpuscle
of a frog. And here I may mention a very interest-
ing fact in connection with, and in support of this
theory, which is, that in the very young embryo,
even of mammalia (before the development of the
liver) all the blood corpuscles possess a distinct
nucleus, and are larger than those of the adult.
They thus resemble the corpuscles of the lower
vertebrata. 'These, however,' to use the words of a
modern writer, 'disappear in the later stages of
intra-uterine life.' The mode of their disappearance
is not mentioned, neither, indeed, has it been
observed. But if we might be allowed to hazard an
explanation, it would be this, that it is not the
nucleus which disappears, but the external cell
wall which bursts and dissolves away, while the
permanent nucleus constitutes the perfect blood
corpuscle of the adult mammal. This also would
explain why the corpuscles are larger in the fœtal
than the adult state. The granules found in the
blood of invertebrata probably belong to a grade of
development lower than either of the former. What

still further strengthens the view we have taken of the different degrees of development of these bodies is the fact that we find in the blood of mammalia representatives of all the three stages, viz. :— (1) Irregular granules ; (2) Nucleated cells, or the white corpuscles, and (3) The ordinary red corpuscles, or the liberated nuclei, so to speak. From this it will at once be evident to you all that we regard the nucleated white corpuscle of the human blood as analogous to the red corpuscle of birds, reptiles, and fishes, a statement which, though at first sight it may appear somewhat startling, will, I have no doubt, after deliberate reflection, be unanimously responded to. And now comes a question which, I dare say, has already suggested itself to the minds of most of you : What relation does the white corpuscle bear to the red in the blood of man and mammalia ? Is the former the parent from which the latter is developed, or is it not ? But the answer to this question naturally leads us to the consideration of the third subject to be treated of under the present section of this Essay, viz. :—

3. The development of the blood corpuscles :—

Under this head we shall notice—(a) Their development in the embryo, or the process by which they first of all make their appearance, and (b) their development in the adult, or the process by which new corpuscles are formed to make up for the loss which the blood is constantly sustaining of these bodies by their dissolution and excretion.

(a) Their development in the embryo. The red corpuscles may be said to be developed in the embryo in two distinct localities, though both of these originally spring from the same fœtal struc-

ture, viz., the vascular or middle layer of the germinal membrane. In the larger vessels the blood corpuscles are formed from the inner layers of a mass of cells of the vascular membrane, the outer layers of which go to form the walls of the vessels themselves. In the case of the capillaries, again, the blood corpuscles are formed from other cells of the same vascular membrane. These cells send out radiating processes, the processes of contiguous cells unite and form continuous tubes, and the original contents of the cells become transformed into blood corpuscles. The embryo corpuscles are at first large, powerless, and spherical, and full of laminar particles, which are of a fatty nature. After a time their contained laminar particles become broken up and liquefy, and the whole body assumes a red colour. A nucleus, too, now becomes apparent which had before been obscured. These changes have been traced by Vogt, Kölliker, Cramner, and Paget. The nucleated corpuscle thus formed exists in the embryonic blood of mammalia, as well as of other vertebrata. In birds, reptiles, and fishes this also constitutes the permanent form in the adult, whereas in mammalia it is probable, as has been already stated, that the external cell wall dissolves, and the liberated nucleus constitutes the mature blood corpuscles. In addition to their original development Kölliker, Falconer, and Kirkes maintain that they have observed the corpuscles of mammalian embryos multiply by a process of bipartition of the nucleus, each half of which either by appropriating half the cell, or by developing a new cell around itself, becomes a corpuscle exactly similar to the parent.

(*b*) As regards the formation of new red corpuscles in the adult, few observations have as yet been made. The celebrated Leeuwenhoeck remarked that certain of the red corpuscles might occasionally be seen dividing themselves into six, which, at first very small, gradually increase to the size of their parents. Dr. Martin Barry holds a somewhat similar view. He regards the multiplication of the red corpuscles as due to the development of six young cells, which sprout from the circumference of the nucleus, being at first confined within the cell wall of their parent, but afterwards rupturing this, and becoming free. Again, Dr. G. O. Rees remarks that he has observed a constriction gradually forming at the centre of certain of the corpuscles; and that by the gradual increase of this the single corpuscle became ultimately divided into two. If this observation of the fissiparous multiplication of the red corpuscle in mammalia be correct, it affords another proof that the corpuscles are nuclei and not cells; for fissiparous division has never elsewhere been observed in animal cells. Viewing them as nuclei, this would be just an example of exogenous cell development. Dr. Copland considers the red corpuscles of the blood as formed from the white. The latter, he says, become in the process of sanguification surrounded by a coloured envelope, and thus constitute the red corpuscles. This was the view held by Hewson nearly a century ago, who also thought that the spleen was the particular locality of the circulation where this coloured envelope was manufactured. This, however, is a mere theory, one not based upon observation, and easily refuted. The white corpuscles, we should recollect, are globular,

the red are flat.   Now if the white corpuscles became
flattened out, and in addition to this surrounded by
a coloured envelope, they would be immensely larger
than we find the red corpuscles to be, seeing that
they surpass them in magnitude even in their
natural globular state.   Kirkes and Müller likewise
maintain that the red corpuscles are all formed from
the lymph and chyle corpuscles by a process of meta-
morphosis.   Professor Weber even maintains that
in the frog he had observed many of the red globules
lose their red colour and become in fact reconverted
into white globules while passing through the
capillaries.   His observations, however, have not been
confirmed, and are by no means probable.   If they
were correct, then venous blood would contain far
more white corpuscles than arterial, and would con-
sequently be of a lighter colour, neither of which
conditions, we learn from ocular inspection, hold
good.   Though the view of Müller and Kirkes may
explain the development of the red corpuscles in the
case of birds, reptiles, and fishes, it is not equally
applicable in the case of mammalia.   The red
globules of these last, as has been already stated,
are devoid of nuclei, and correspond to the nuclei
of the red corpuscles of the frog, etc. But in the blood
of mammalia we find white nucleated corpuscles, as
well as in the blood of the frog ; and arguing
analogically from the facts that the red corpuscle
of the frog is a mere modification of the white, as
shown by Müller, and that the red corpuscle of
the mammal corresponds to the nucleus of the red
corpuscle in the frog, we are naturally led to infer
that the white corpuscle in the mammal is the parent
of the red—in other words, the latter is the liberated

nucleus of the former.   The actual liberation of the nucleus, it is true, has not yet been observed, but we have often been able to trace the nucleus of the white corpuscle in man, presenting all forms of gradation from one or two almost invisible molecules adhering together, to that of a fully formed red corpuscle.   Enough, however, has for the present been said with regard to the development of the red corpuscle, as I have some intention of making it the subject of a communication to the Society at some future period.   We now proceed to consider—

4. The termination of the existence of the red corpuscles.   The exact period of the duration of the life of a red corpuscle is not known ; but this we know, that after it has performed the functions assigned to it in the animal economy, it disappears, and its place is supplied by others.   ' When a corpuscle,' says Kirkes, ' is past its perfection it degenerates, and probably liquefies.   It never forms the germs of new corpuscles.'   He describes the degenerated corpuscles of fishes and reptiles as white, pellucid, and often cracked, quite distinct from the white corpuscles.   From this we infer that the hæmatine or colouring matter is dissolved out before the corpuscle actually liquefies.   If we examine blood in the act of decomposing, we find that the walls of the globules, losing their texture, permit the outward escape of their contents, and both the hæmatine and globuline are dissolved in the serum, which thus becomes of a reddish colour.   Vogel remarks that it is highly probable that the solution of the red corpuscles after death is due to the generation of carbonate of ammonia in the blood.   Such, it is true, may be the case after death, but during

life there is no necessity for having recourse to such an explanation of the liquefaction of the red corpuscles which, obeying the laws which regulate cell growth in general, break up and dissolve into the surrounding fluid when they have reached the termination of their existence. Ecker maintains that the spleen is the organ in which the blood globules undergo their final metamorphosis. In this organ, he says, they are broken up into granular masses, which are carried by the vena cava to the liver, and thence expelled from the system. Költ- liker holds a somewhat similar view. The true function of the spleen has not yet been made out; but that it is the organ in which exclusively the red corpuscles undergo their last metamorphosis is extremely doubtful. It is far more probable that the corpuscles become dissolved generally throughout the vascular system.

II.—PATHOLOGY.

Observations are still wanting before we can have a correct idea of the relation which subsists between the red corpuscles and the various diseased states of the body. At present we have but a few scattered observations with regard to the changes exhibited by the corpuscles in a few diseases, and with the brief enumeration of these we fear we must for the present content ourselves; and, first, as to—

1. The morbid changes in their number. The red corpuscles may be increased or diminished in number in proportion to the other constituents of the blood. It should be remembered, however, that even in health they vary in number according to age, sex, temperament, etc. Thus they are more

abundant in the fœtus than in the adult, in man than in woman, in sanguineous than in lymphatic temperaments. The red corpuscles are increased in number in plethora, and also in most febrile conditions of the body. In diabetes, dropsy, chlorosis, and the true inflammations, on the other hand, they are diminished. Venesection is said to diminish the number of the red corpuscles, while it increases that of the white. Now you will observe that this is quite in keeping with the theory we have already laid down, that the red corpuscles are developed from the white. If the blood be deprived of a number of red corpuscles their place must be first supplied by white corpuscles, seeing that the latter are antecedent to the red in development : and it will take some time before the nuclei of the white corpuscles are sufficiently matured to be liberated and constitute the perfect red corpuscles.

2. The form of the blood-corpuscles may be altered. In purpura the corpuscles lose their normal shape and size, and become mixed up with granular matter. In a case of typhus fever Dr. Bennett found corpuscles presenting an irregular outline and a granular aspect. In hæmatocele corpuscles are often seen containing four or more granules, and ultimately breaking down. The blood-corpuscles which are developed in pathological epigeneses, such as tumours, have been observed to present certain peculiarities which are worthy of notice. They are of various sizes, have a rounded form, and have not the central depression observable in ordinary corpuscles. They have usually a clearly defined outline, and their diameter is generally somewhat less than in the normal state. Newly formed

corpuscles in a pathological structure are never, like those of the embryo, larger than the normal ones. They dissolve in water and acetic acid, without yielding any indication of nuclei.

3. As regards the colour of the blood-corpuscles, Haller and Damé say that it is paler in chlorosis, darker in jaundice. These are statements, however, which I have never been able to confirm ; and I think it more probable that the pale colour of the blood in chlorosis is due to a diminution of the number of the corpuscles ; while its 'dark colour in jaundice is to be attributed to the solution of bile pigment in the *liquor sanguinis.*

4. An increased attraction for one another is exhibited by the corpuscles in inflammation, and certain other conditions of the body, such as pregnancy. The corpuscles adhere to one another by their flat surfaces, forming strings which reticulate. It is this which causes the buffy coat of inflammatory blood, or, at all events, assists in its formation. The only conclusions to be drawn from these observations must be of a very general nature, viz., that in those conditions of the body in which the functions of nutrition are increased, we generally find a great number of blood-globules, while the opposite is the case when the functions of nutrition are carried on but languidly. Moreover, when nutrition is perverted, we find alterations in the number, form, and attraction (and, as some say, also in the colour and volume) of the red corpuscles.

III.—PHYSIOLOGY.

That the red corpuscles do perform a very important function in the economy of the animal

organism is a fact incontestably proved, and which
there is no denying. Indeed, as we have already
hinted in our introduction, they would seem to be
the most important constituent of that all-important
fluid in which they exist—the blood—the liquid
flesh, the source of life to all the body. As a proof
of the importance of the corpuscles in the blood, we
may state that it has been demonstrated by Milne
Edwards that the beneficial effects, which in many
cases follow upon the transfusion of blood from one
individual to another, are mainly to be attributed to
the increase of the blood-globules.

The actual importance of the blood-globules in
the animal system being thus proven, the question
now comes to be, What is the nature of that func-
tion which they perform ? And here we are obliged
to confess that to state their actual function is as
difficult as the importance of that function is certain.
Various opinions on this point are entertained by
different physiologists. We shall now endeavour to
lay these before you, afterwards mentioning what
view we consider most tenable in consistence with
known facts.

Some physiologists consider the blood-corpuscles
as carriers of oxygen and carbonic acid. Thus
Simon observes that the greater part of the carbonic
acid exhaled by the lungs arises from the red cor-
puscles, and that such substances as urea, uric acid,
and bilin may be products of active changes in the
blood corpuscles in connection with these changes.
Liebig supposes that the colouring matter of the
corpuscles of venous blood contains the carbonate of
the protoxide of iron, and that this in the lungs is,
by the action of the oxygen in the atmosphere, con-

verted into the hydrated peroxide of iron, carbonic acid being given off. This hydrated peroxide is again converted into the carbonate of the protoxide when the blood is passing through the systemic capillaries, and in this way, he says, the tissues are furnished with the amount of oxygen necessary for their growth and nutrition.

Other physiologists consider the blood-corpuscles as concerned in elaborating the crude protein principles of the chyle, and rendering these more fit for the complex process of nutrition. This is the view entertained by Kirkes. A similar opinion, viz., that the red corpuscles are floating glandular cells, is also held by Henle, Wagner, Wharton Jones, Newport, Bischoff, and others, and even by physiologists as far back as Turpin, A.D. 1700.

I believe, however, that here, as often elsewhere, the truth lies with no one of them, but rather somewhere between them all. In what remains of this Essay I shall endeavour to lay before you what, to my mind, is a plausible and also probable explanation of the functions of the corpuscles, taking into consideration their different stages of development as I have already detailed them, as also the known changes which the blood undergoes in its circulation through the system. I shall commence then with the white corpuscles, as I have already endeavoured to show that they are the parents of the red. The white corpuscles of the lymph and chyle are, as you are aware, identical with those found in the blood. Then the question comes to be, From what do the chyle corpuscles take their origin? What is the process of their development? Müller, Rudolphi, and others seem to think that the

chyle globules may be 'absorbed into the lacteals' from the chyme in the intestinal canal. The late admirable researches of Professor Goodsir, however, with reference to the structure of the intestinal villi, prove that this is a physical impossibility. The investigations of the same distinguished observer also show that the epithelium lining the lacteals undergoes a remarkable modification on entering the mesenteric glands. Its particles, from being flattened scales, are transformed into spherical nucleated cells. Now I think it exceedingly probable that these spherical nucleated cells lining the lacteals, in their passage through the glands, afterwards constitute the white corpuscles found in the chyle and blood, and on the following grounds :—(1) The epithelium of the lacteals undergoes the above-mentioned transformation evidently to serve some particular purpose. (2) As in all epithelia, so here, the individual particles must, after a certain time, be thrown off to make room for new generations of them. (3) When so thrown off, there is no alternative for them but to pass into the fluid of the lacteal vessels. (4) These nucleated particles resemble, in size and general appearance, the chyle corpuscles.

If this view be correct, it follows that the chyle cells are not formed, as some say, in an isolated fluid independently of pre-existing cells, but that their development is quite in accordance with the laws which regulate that of epithelia in general (or of gland cells). The chyle corpuscles thus formed pass along to the blood, and in process of time their nuclei enlarge, assume various shapes, and ultimately form the red corpuscles. But in order to

effect this change the cell wall by its inherent endosmotic power draws in nourishment from without, and this nourishment must be derived from the circumambient fluid portion of the chyle which has been absorbed by the lacteals. The principal protein ingredient of the chyle is shown by analysis to be albumen, but we know that in order to fit it for the nutrition of most of the animal tissues, it must be converted into fibrin. Now it is not impossible that this important change is effected at the very moment of the passage of the liquor of the chyle through the external walls of the white corpuscles, in the same way as the matters secreted by glands, as the liver, are said to be transformed at the very moment of secretion. This view would explain why in scrofula there should be an increase of albumen and diminution of corpuscles in the blood, as also other morbid changes. In process of time, in man the external cell wall bursts and liberates its nucleus, which already is, or at all events is afterwards to become, a red corpuscle. These dissolved cell walls, along with the fluid which existed between them and the nuclei, no doubt form certain of the protein principles (fibrin) of the blood, and are consumed in nutrition. The liberated nucleus, the red corpuscle, however, is destined to effect future changes in the blood. The precise nature of this change is by no means certain, but would seem to be intimately connected with the function of respiration : and here we see no reason for not adopting the theory of Liebig already mentioned, by which he endeavours to show that the blood corpuscles carry oxygen from the lungs to the tissues, and carbonic acid from the tissues to the

lungs. These changes, which are of a chemical nature, no doubt form one of the great sources of animal heat. The red corpuscles, after having performed this, or whatever other function may be allotted to them, decompose or die. The products of their decomposition pass into the *liquor sanguinis*, and they in all probability go to form the urea, uric acid, bilin, and other oxygenated principles which are excreted by the kidneys and liver. We have thus followed the blood-corpuscles from their origin to their final disappearance, from their cradle to their grave, and have endeavoured to assign to each stage of their existence its peculiar duty in effecting the various known changes undergone by the blood. Much, we are aware, in what has been stated will be said to be purely theoretical, and that such is the case we do not pretend to deny. But, theories though they be, we trust that they are theories which are not inconsistent with, but rather tend to explain, the known phenomena, while at the same time they are conformable with the general laws ('ultimate facts') which have been arrived at in physiological science.

I now, gentlemen, have finished the task allotted to me, a task which, irksome though it may to some appear to be, is certainly conducive to much useful information. I trust you will pardon the very imperfect way in which it has been performed, from the circumstances which compelled me to do it in so short a time.

Gentlemen, if the reading of this paper, or the discussion which I trust will ensue, affords to any of you the slightest degree of the benefit which the writing of it has conferred upon myself, I am more than satisfied.

<center>XX</center>

JAMES MATTHEWS DUNCAN

<center>1826-1890</center>

REFLECTIONS ON THE DURATION OF PREGNANCY, WITH REMARKS ON THE CALCULATION OF THE DATE OF CONFINEMENT

<center>*Read* 1854</center>

MR. PRESIDENT,

In the numerous elaborate Essays which have been written on the subject of the duration of pregnancy in woman and in animals, it has always appeared to me that an important source of error has lain concealed. The exposition of this will, I trust, throw some light on this interesting question, and I am sure that when it comes to be completely investigated our notions as to the duration of pregnancy will be much more definite and satisfactory than they now are. My object in the present communication is to make a few remarks on this particular point, and then briefly to discuss the general subject.

In the beginning it will be useful to define the meaning to be attached to some important terms frequently recurring in this discussion, viz. in-semination, conception, and impregnation. By the

word insemination is to be understood, simply, the injection of semen into the genital passages,—the conjunction of the two sexes. By conception is to be understood the more hidden and mysterious union of the semen and ovum; while the word impregnation implies both of these processes.

The confusion of the two former of these different processes is so general among obstetric writers that it is needless to quote authorities for the assertion. That they should always be held distinct in studying this subject will, I hope, be made apparent. For in fixing the commencement of pregnancy it is necessary to date only from the period of conception. Authors in discussing this subject have delighted to quote as crucial examples those cases where the date of a single connection, or of connections within a short and limited time, could be satisfactorily decided. But it is evident that such a date only fixes the time of insemination, and not the time of the commencement of pregnancy. For a woman cannot be said to be pregnant whose body merely contains seminal matter. Pregnancy is a state of fertility, of breeding, which, as Leeuwenhoeck long ago pointed out, cannot be said to commence till such time as may have elapsed after insemination before the union of the ovum or ova and semen has taken place. This period of time, whatever may be its possible length, must be subtracted from all these supposed crucial cases of the duration of pregnancy. The interval which they describe as pregnancy, that is, between successful insemination and parturition, must be considered as in strict language a false period; and it is so, because it contains the period between insemination and con-

ception, during which a woman is not pregnant. By this interval, then, all such cases must be curtailed.

Very little has as yet been ascertained as to the possible length of this interval. It was my intention to have attempted to make it out in regard to some of the lower animals ; but my inexperience in such investigations, and the pressure of other avocations, have hitherto deterred me from the pursuit of this subject. There is then at present no resource in this question but to facts already known. Now it has been ascertained by physiologists that semen newly expelled by the male is not essential to impregnation. Animals have been frequently impregnated by Spallanzani and others with semen which has not only been kept for some time, but has even been variously altered, in mechanical properties at least, in experiments. And there seems to be no limit to the time during which the semen may be kept without losing its virtues, except the term of the life of the spermatozoa.

That this period is not insignificant, and cannot be passed over without risk of important error—in fact, that it may extend to many days or weeks,— will appear from the following observations. We omit the facts in regard to animals so low in the scale as insects, in the females of which the semen is laid up in cavities where it retains its power for months. In regard to the dog, Leeuwenhoeck pointed out that these animalcules might live for more than seven days preserved in a glass tube, and if such be the case in a rude experiment, it may be expected that they should retain vitality considerably longer in the passages of the bitch, where they have heat and moisture supplied under peculiar

circumstances. That they do live for some days in the genital passages has been proved by abundant observations, although the possible length of this period is not certain. The decision, indeed, of this point by microscopic observations would be a very difficult matter, as it would involve the almost impossible search for spermatozoa over every part of a long tract of mucous membrane. And this search would be necessary, for we know by the experiments of Spallanzani that semen highly diluted, or, in other words, the smallest quantity of semen, is sufficient for successful impregnation.

Again, the elaborate experiments of Haighton, long ago performed, show that in the rabbit conception generally does not take place till about fifty hours, or more than two days after insemination. He found that division of the Fallopian tube earlier than this time prevented conception, and that, by waiting longer, or till the ova had descended from the ovaries, the conception was not prevented by the mutilation. It thus appeared that the conjunction of the ova and semen in the rabbit generally did not take place till more than two days after insemination. In the rabbit, then, there was found in Haighton's experiments this long interval between insemination and conception, and in some cases it is possibly much longer. In the rabbit the interval between insemination and parturition is ordinarily thirty days. The observations of Tessier upon 161 rabbits give five days as the extreme limit of the protraction of this term, a period of time which may be accounted for without any great stretch of the space during which the semen may retain its fructifying power. And in this way it may have

happened that the real period of gestation, that is, from conception to parturition, may not have been at all protracted in those cases. The cases also in which the period was less than thirty days may be explained by supposing the ova to have been further matured, or even advanced into the uterine horns before impregnation took place, so that conception may have happened very soon after insemination. And in Tessier's observations it is remarkable that in none of the rabbits did labour anticipate the usual time more than two days, the period which Haighton's experiments seem to show to be the usual interval between insemination and conception in this animal. In the present state of our knowledge, however, these explanations cannot be established.

For reasons which do not require to be stated, there is great deficiency of evidence in regard to the analogous subject in the human female. But there is every reason to believe that the circumstances of conception in her closely resemble those in the higher animals. It has of late years been shown that, in woman, at every menstrual period an ovum is matured and expelled from its Graafian vesicle, and that she is liable to conceive during its progress along the Fallopian tube. How long after its maturation the ovum can retain its vitality and susceptibility to the seminal influence is not known, but probably the time is short. But cases might be easily adduced from the works of eminent obstetricians to prove that a single insemination at any period of the interval between two menstrual periods may result in the fertilisation of the female. Of such cases those only are important, in our present point

of view, where conception has resulted from insemination shortly before the return of a period. They admit of explanation in three different ways. Either the ovum has remained up to this time entire, and susceptible of being influenced by the semen—a supposition which is very improbable as regards the ovum, and is at variance with what we know of the history of the decidua or nidus prepared for the egg's further development; or the excitement of connection may have hastened the maturation and rupture of a Graafian vesicle,—a view which is in itself improbable, and inconsistent with what we know results from similar circumstances in the lower animals. But it may also happen that the seminal animalcules may remain in the passages till the ovum is prepared and discharged from its vesicle. An objection at once appears to this explanation, namely, that these spermatozoa would be removed by the menstruation contemporaneous with the discharge of the ovum. When menstruation does supervene to a single recent coitus, this will probably happen, unless the semen have permeated the Fallopian tubes, and thus advanced beyond the scope of the menstrual flux. But the study of such cases, as recorded by authors, reveals this interesting fact, that under such circumstances menstruation often does not take place at all, or only very scantily, the uterine system, as it were, anticipating the conception, and preventing the failure which might result from a free discharge of blood. It is evident that such cases occurring in married life would be very liable to be considered cases of gestation protracted a month.

The interval between insemination and parturi-

tion is a period of the greatest importance in a medico-legal point of view. It is discussed by obstetric authors as the period of gestation, or as the term of the duration of pregnancy. We have already shown that the present state of our knowledge requires us to make a distinction between the date of insemination and that of conception; and it strongly appears to us that the full comprehension of the bearings of this distinction will go far to equalise the discordant views as to the term of pregnancy in the human female, and to account for many of the so-called cases of prolonged gestation. But with our present ignorance of the possible interval between insemination and conception, the attainment of the result is impracticable.

No reliance can be placed but upon accurately ascertained dates of parturition and of fruitful connection. In regard to the latter of these dates, no confidence can be placed in the statements of women living habitually with males, however truthful they may be, or whatever additional evidences they exhibit. We are therefore reduced to a limited class of observations, namely, those where the pregnancy resulted from a single coitus, including those where this never took place but on a single day, and those where it was removed on both sides from other similar occasions by months, or such other periods as would render it absurd to refer the parturition of a fully developed fœtus to them. With those dating from a single day we have included some dating from one or two days; but in such cases our calculations are dated from the coitus of the first day only. This statistic contains forty-six cases, which yield the period of 275 days as the

average interval between insemination and parturition. While 275 days was the average interval, it may be remarked that the largest number of cases, at any particular day, was seven, at the 274th day.

The interval between the last menstruation and parturition is a period which, for obvious reasons, can be much more easily and frequently ascertained than that last under discussion. It is one, the knowledge of which is of the greatest practical importance in the everyday life of the married female, and of the obstetric practitioner, seeing that by aid of it he attempts to predict the date of the expected confinement. In the vast majority of cases it is the only fixed point from which the calculation can be made, and hence the necessity of accurately ascertaining it, if possible.

Authors have too frequently neglected the discussion of this important period, the only one available in most cases of pregnancy. They generally decide the term of pregnancy theoretically, and upon insufficient grounds, and direct that, in calculating for the day of confinement, this term should be told off from some day after the last menses, which day they conceive to be that on which conception most frequently or most probably takes place. For instance, Montgomery states, upon the evidence of a very few cases only, that the natural period of human gestation is 280 days, and, in calculating the date of parturition, recommends this to be added to any day within a week after the last menstruation. He thus includes, between the last menses and the date of parturition, a period varying from 281 to 287 days, a period which we shall show considerably overreaches the mark.

Other authors and teachers, considering that a woman is equally liable to conceive on any day between two menstrual periods, direct that the middle day of that interval be taken, and the supposed period of gestation, 280 days, added thereto ; thus including the exaggerated space of 290 to 295 days between the last menstruation and parturition.

The exact decision of this interval, as of that last under discussion, can be obtained only by a reference to actual observations. Modern researches have shown that it is at the menstrual period that the ovum quits its Graafian vesicle and traverses the Fallopian tube on its way to the uterus. It is in the course of this passage that it encounters the semen, and conception results. This passage occupies about three days in the rabbit, and, in M. Bischoff's opinion, it occupies eight or ten days in woman. During all this time, then, the woman will be liable to conceive. It will therefore be expected that the interval of which we are at present speaking will be some days, at least, longer than the last.

The statistical calculations on this subject give, on an average, 278 days as the interval between the last menstruation and parturition ; a period less even than the 280 days which we have generally been taught in this country to be the interval between impregnation and parturition, or the duration of pregnancy.

The largest number of cases on particular days conglomerate about the 280th. Among Dr. Reid's 500 instances, 283 were within the 280 days, and 217 beyond it. So far is it then from 280 days being the ordinary duration of pregnancy, that a woman generally does not go more than 278 days

after the last menstruation is over. This period exceeds the average interval between insemination and parturition by three days ; and we may argue from this, with some little probability, that conception takes place generally a few days after menstruation is finished ; a view which is confirmed by numerous other physiological observations.

The prediction of the day of confinement is one of the functions ascribed to the accoucheur ; and apart from the comfort and convenience to the mother attending the foreknowledge of it, she often makes its failure or success a test of the more subtle acquirements of the physician. The foregoing statistics, however, will always justify the latter in never giving a decided prognosis of the day of confinement ; and if he has been guarded and careful will afford him asylum, showing as they do that with certain knowledge of the termination of the last menstruation, or even of the date of a single coitus, no safe prediction can be made unless within limits so extended as to deprive it of much of its value. At the same time, there is no doubt it will always be a desideratum to know the most probable day of confinement, and this can generally be settled with some exactness.

If the date of a single connection is ascertained, which is of course very rarely the case, then the process of deciding the probable day of confinement simply consists in telling off 275 days (the average interval between insemination and parturition) from that date. Now any nine consecutive calendar months include 275 days, if February is not in the number. If February is in the number, the nine calendar months include only 273 days, and

the correction necessary is apparent. The whole process of calculation then consists in attaching the number of the day of connection to the name of the month ninth succeeding, and adding two additional days if February is included in the interval.

In the vast majority of cases the day of confinement is predicted from the date of the termination of the last menstrual period. In many cases the calculation can be aided and corrected by comparison with former pregnancies in the same female. But when this source of information is wanting, the nearest approach to truth will be made by adding to the day of the disappearance of the menses, 278 days, the average interval between the end of menstruation and parturition. The prediction will, of course, prove erroneous in a great number—nay, in the majority of cases,—but it forms the nearest approximation which the mother can obtain to guide her. If a woman, then, knows the last day of her last period, she has only to tell the same day for the ninth month following (most mothers do so on their fingers, which thus form an admirable periodoscope) and add three days, or, if February is in the interval, five days. She thus has the most likely day of her confinement, or, perhaps better, she has the middle day of the week on which she will probably be laid up.

I have already casually shown how much this varies from the calculations ordinarily recommended by most British authors and teachers. It would be tedious to enter further on this subject. I may merely remark that a more correct plan prevails on the Continent. And from some

inquiries and observations I have made in Scotland and England, I find that popularly a more correct calculation is in use than that recommended in the schools. For instance, in Edinburgh, and some parts of Scotland, it is common to find women calculate in this way. They find the last day of menstruation, and they hold that the same day nine months after will be the day of confinement. The celebrated Harvey's opinion on this subject was also very correct, and his remarks tally with Dr. Tyler Smith's ingenious views on this subject.

Protraction of the period of pregnancy beyond the common or natural term is a phenomenon which most obstetricians are now willing to admit. But, although believing in its possibility, I am at the same time convinced that it is not so frequent in occurrence as late writers on this subject seem to think, and that most of the cases of this kind which are recorded have not sufficient evidence to support them. They are mostly based upon the signs of the disappearance of the menses, of the sympathetic phenomena of pregnancy, and of a physical examination of the uterus ; all of which, it is needless to say, are abundantly liable to create misapprehensions and fallacious reasonings, and, singly or combined, can justify no absolute conclusion from them. One great reason for discrediting the evidence of most of the cases recorded by authors is that we hear nothing of great development of the uterus, or of large size of the child, or of the placenta in such cases, results which, to say the least, might be expected. On the contrary, we find such authors stating that in these so-called cases of protracted

pregnancy the child is no bigger than usual, or is even smaller than ordinary. 'Although in some of the cases of protracted gestation,' says Dr. Montgomery, 'the child was of enormous size, it by no means follows that it should be so in all such instances ; and, in point of fact, we find it expressly mentioned in some of them that the child was smaller than usual, as happened in one of Dr. Hamilton's cases ; and Foderé says that in three instances in which gestation was evidently prolonged, the children were undersized and ill-thriven ; while, on the other hand, the largest children are often produced where no extension of the term could have taken place.' Dr. Burns also says that 'some causes which we cannot explain nor discover have the power of retarding the process (of gestation), the woman carrying the child longer than nine months ; and the child, when born, being not larger than the average size.' In further corroboration of these views the valuable observations on cows and mares by Tessier and Spencer have been cited as showing that there was no marked coincidence of increase of size and weight of the fœtus with protraction of gestation. But this reasoning from analogy between the cow and the woman appears to be very much overstretched ; and there are evident reasons for expecting, *a priori*, that the period of gestation in women should be limited on the side of protraction more than in the lower animals. Of these the strongest is based on a consideration of the adaptation of the well-developed nine-month fœtal head to the maternal passages, and the evils that are so well known to result from even slight disproportion between them. And unless it be supposed that

pregnancy is protracted for the special behoof of
small and ill-developed children, it must be admitted
that an extraordinary development of the ovum is
to be looked for in such cases. The acknowledged
absence, then, of this extraordinary intra-uterine
development is a strong evidence against the reality
of the great mass of so-called cases of prolonga-
tion. On the other hand, the presence of this sign,
in addition to others, is, in my opinion, powerfully
corroborative of the supposed protraction in any
instance. In illustration of this, I may state that
the best example I have met with of probable pro-
traction occurred in a female who had borne several
children, and who had previously been correct in
the calculation of the period of confinement from
the cessation of menstruation. On the occasion in
question she passed her calculated time four weeks,
and before confinement expressed her conviction, all
the more strongly in consequence of my incredu-
lity, that she had passed her time a month. The
labour was more tedious than usual, in consequence
of the great size of the fœtal head. The child
proved of very large size and advanced development.
It weighed 10 lbs. 4 oz. The placenta was 2 lbs.
in weight. Other cases similar to the above have
been communicated to me by professional friends,
and some are to be found recorded.

In these cases the ordinary sources of evidence
were confirmed by the evidently exaggerated de-
velopment of the ova, the result of these protracted
pregnancies. I have lately had under my care two
cases in which gestation was supposed to be pro-
longed, but which I reject from this category
because, although the ladies were in good health at

the time of falling in the family way, yet the infants born were not at all larger than their former children. The ladies were sisters, and in each of them their calculation and mine was passed by nearly a month. The data founded upon were the cessation of menstruation, and the occurrence of morning sickness. In both cases the respective nurses were residing with them for about a month before the supervention of labour.

Such cases as those of the two sisters just mentioned, and numerous other so-called cases of protraction, are easily explained by supposing simply that that menstrual flux was suppressed which should have occurred about the probable time of the eventful intercourse, or, in other words, the decidua prepared for the ovum destined to be impregnated did not as usual throw off the bloody fluid. In these cases we must suppose either that the suppression for this one period arose from some ordinary constitutional cause, or, what is more likely, that the fruitful intercourse occurring shortly before the ordinary menstrual period anticipated and prevented it. This phenomenon we believe not to be very rare, and to be sufficient to explain away many cases of protracted gestation. In further illustration of this circumstance, we must be satisfied with referring to those cases of pregnancy after a single coitus taking place shortly before menstruation, the coitus producing, firstly, the partial or complete suppression of the menses at the approaching period, and, secondly, the fertilisation of the ovum discharged in coincidence with the suppressed period. Some careful observations of this sort are recorded by Raciborski and Montgomery.

The evidence of highest value in regard to this subject which we possess is founded upon cases where pregnancy resulted from a single connection. The results of these cases go far to establish the well-founded opinion of Dr. Montgomery, that the cases most deserving of confidence are those in which the usual term is exceeded by more than three or four weeks. But the cases referred to give us the interval between insemination and parturition, a period which, I have elsewhere remarked, requires a connection which physiology has not yet enabled us to decide for the possible interval between insemination and conception. In a practical and medico-legal point of view, however, the interval obtained is of great importance. In the collection of cases of this kind the longest duration found is in one case, where the period was 293 days.

The theory of the duration of pregnancy is still unknown. Some authors, believing that labour comes on at the tenth menstrual period, explain the protraction by the female's having a longer menstrual interval than usual, ten of which will make up a period exceeding the usual term of pregnancy. Others have supposed that from some cause a female might miss the usual period and go on to what would have been the next menstrual period had she not been impregnated. Others have connected it with tardy development of the fœtus, with the influence of depressing emotions, etc., but all these are mere hypotheses.

In conclusion, we beg to state the following propositions :—

1. That the interval between conception and

parturition (the real duration of pregnancy) has not been exactly ascertained in any case.

2. That the average interval between insemination and parturition (commonly called the duration of pregnancy) is 275 days.

3. That the average interval between the end of menstruation and parturition is 278 days.

4. That the intervals between insemination and parturition, and between menstruation and parturition, have no standard length, but vary within certain limits.

5. That while absolute proof of the prolongation of real pregnancy beyond its usual limits is still deficient, yet that there is sufficient evidence to establish the probability that it may be protracted beyond such limits to the extent of four weeks.