ADDRESS

IN

PHYSIOLOGY.

DELIVERED AT

THE THIRTY-SIXTH ANNUAL MEETING OF THE BRITISH MEDICAL ASSOCIATION.

BY

GEORGE ROLLESTON, M.D., F.R.S.,

Linacre Professor of Anatomy and Physiology, Oxford.

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ADDRESS IN PHYSIOLOGY.

I HOPE that you may think that the fact of my connection with this place having put me into the honourable office of giving this address justifies me in adopting for it an arrangement, and choosing for it a set of topics, which that local position suggested to me. If I were to say that I had chosen for the subject of this address the bearings of the studies which it is the business of my life to teach here, upon the interests of the medical profession, I should be giving it too ambitious a title; it is but with some and with few of these bearings that I propose or feel myself competent to deal. I shall limit myself, firstly, by selecting only such topics as, having been pressed forcibly upon my own attention in my own peculiar course of labour, have come to assume in my own eyes, at least, a considerable importance, and have seemed, in consequence, not unlikely to prove possessed of interest for others also; and I shall limit myself, secondly, by abstaining from going over ground which has, within my own knowledge, been occupied by persons who have on previous occasions stood in the position which I now occupy before you.

Let me throw the heads of my address into a few short phrases, and say that I propose, with your permission, to speak firstly of the bearing of certain portions of the very extensive range of subjects, comprised under the titles Anatomy and Physiology, upon certain points and problems which come before the attention of the medical practitioner in the course of his actual duties; and secondly, of the illustration which some of the conclusions recently come to in Biological Science cast upon the validity of certain principles which are ordinarily looked upon as authoritative canons for the regulation of the reason in medical, and, indeed, in other investigations. Under the term Biological Science are included, besides pure Physiology, Human and Microscopic Anatomy, Comparative Anatomy also; and in this place, as your visit to the Museum will have convinced you, we give considerable, but as we hope, not undue prominence to this latter branch of study. I propose to speak of the bearings of Biology on Medicine in each, but, owing to our local speciality just alluded to, specially in the last mentioned of these four departments. And I must ask you to bear in mind that the very constant reference which I shall make, if not in my address, at all events in my notes, to the works and writings of others, is in like manner to be explained by my wish to have a distinctive colouring given to this address by the local peculiarities of the great educational centre in which we are assembled. For one of the most distinctive peculiarities of this ancient University is the formation within its precincts of such a library of modern science as will shortly have no superior, and but few rivals, in the world. This we owe to the well advised administration of the funds of that famous physician, Dr. Radcliffe; and it is from a wish to make a sort of acknowledgment of the obligation which medical and other sciences owe to him and his trustees, that I shall so constantly, at least in print, refer to the chapters and pages of the innumerable books which their enlightened munificence has put here at the disposal of the student. It is not, I can assure myself, from any irritable anxiety to impugn or depreciate the work of others, that I have so constantly consulted and specified their pages; nor, I trust, have I allowed myself to be tempted into the unpardonable fault of using, or rather abusing, a great opportunity by making upon it a petty personal display. Rather have I felt it to be my duty to occupy this hour as you occupy your lives, in doing what it may

be possible to do for the good of humanity. Your presence and your example make me feel that any other course would be but impertinence; and I have therefore kept constantly before my eyes Bacon's sentence in condemnation of empty useless erudition—*Vana est omnis eruditionis ostentatio nisi utilem operam secum ducat.*

The title of Niemeyer's work on medicine, the seventh edition of which has recently appeared and come into my hands, will furnish me with an excellent text for my first head—"The Connection and Interdependence of Medicine and Physiology." That title runs thus:—"*Lehrbuch der Speciellen Pathologie und Therapie mit besonderer Rücksicht auf Physiologie und Pathologische Anatomie*, von Dr. Felix von Niemeyer. Siebente vielfach vermehrte und verbesserte Auflage. Berlin, 1868." From this work I will take my first illustration of the nexus and connection of which I have to speak; and I believe that, though I am obliged to dissent from the explanation therein given of the facts I shall refer to from its pages, the explanation which they seem to me to bear, or rather demand, shows even more clearly than the one there given, the intimacy of the alliance which is now becoming so close between the experimentalist and the practitioner. In writing (vol. ii, p. 334) of a form of neuralgia, the pain of which those who have suffered from it themselves, or, indeed, have seen others suffering from it, will allow is not exaggerated by the application to it of the words "fast unerträgliche," Niemeyer remarks that the physiologists Dubois-Reymond and Dr. Möllendorff refer its origin to the existence of a dilated state of the *arteria carotis cerebralis*. This state of dilatation these authorities explain by a reference to certain facts in the physiology of the cervical sympathetic as discovered now some seventeen years ago by Bernard, and elucidated still further by Waller, Budge, and Brown-Séquard. And in a like spirit, or at least by a reference to certain anatomical facts in the arrangement of blood-vessels, which he supposes to become dilated and distended, the great Göttingen anatomist, Henle (cited by Niemeyer, vol. ii, pp. 319-339), explains the causation of certain neuralgiæ. The neuralgia which is apt to haunt the sixth, seventh, and eighth intercostal spaces of the left side, he has suggested may be explained by the peculiar arrangement of the left or smaller azygos vein in that region; and the greater relative frequency of neuralgia of the first than of the second and third divisions of the fifth cerebral nerve, he refers to the greater quantity of dilatable veins with which it is beset in passing from the inner to the outer surface of the sphenoid. Now I venture, though with some diffidence, as I find myself in opposition to such names and authorities, to dissent from the explanations thus given of these facts, which I suppose must be acknowledged, with perhaps an exception as to the relative statistical frequency of neuralgia of the first division of the fifth, to be only too real facts. By laying the physiology of the cervical sympathetic alongside of the natural history of an attack of neuralgia, we shall be enabled, I believe, to see that there are stages in each corresponding with stages in the other, but that it is a stage of spasm in the one, and not a stage of relaxation and congestion, which corresponds with the stage of pain in the other. Stimulation of the upper cervical sympathetic produces, more or less immediately, contraction of the blood-vessels of the head and dilatation of the pupil, and diminution of the temperature. This is the first line of operation, resulting in what Brown-Séquard (*Lectures on the Physiology and Pathology of the Nervous System*, 1860, p. 142) calls "Decrease of Vital Properties." But after a while the reverse of all this takes place, and the vessels dilate. Now whether this be so in consequence of exhaustion, as is ordinarily said (*e.g.* Funke, *Physiologie*, vol. ii, p. 772) or not, as Dr. Loven, (who says in Ludwig's *Arbeiten Phys. Anat.*, Leipsig, 1867, p. 1011, that the sympathetic is not so easily tired) thinks, is of

no consequence, or of little consequence, as the fact of the sequence of events is accepted as I have stated it on all hands. Indeed, similar alternations of alteration in the calibre of vessels takes place, as is well known, spontaneously, as the phrase goes—whether rhythmically or not, still chronometrically in relation to the needs of the animal and its tissues; in the arteries of the rabbit's ear (Funke, *loc. cit.*, ii, p. 771, citing Schiff and Callenfels), in the veins of the bat's wing (Wharton Jones, *Phil. Trans.*, 1852), in the arteries of the frog's web (Lister, *Phil. Trans.*, 1858, p. 653); and the occurrence of these latter alternations makes the occurrence of the former more intelligible. Now, a similar alternation from a stage of contraction of blood-vessels, of coldness of skin, of shivering, of total absence of heat, redness, swelling, or tenderness, to one of increased circulation, swelling, heat, and tenderness, constitutes two stages in an attack of neuralgia, homologous with the two described as occurring in irritation of the sympathetic. It is rare, I believe and Dr. Anstie teaches (*Stimulants and Narcotics*, 1864), for pain, as opposed to tenderness, to persist after congestion; and pain in tissues differs as much from tenderness as remorse in a conscience differs from tenderness in that organisation; and the two things are well-nigh equally exclusive the one of the other in both cases. Perhaps the mere apposition of the two sets of occurrences side by side is sufficient to justify my conclusion that the congestive stage of the physiological experiment is not the homologue of the painful one of the morbid history, mere apposition being sometimes sufficient to decide us on more difficult homologies, at least in the negative. But I may add, that the argument from the *juvantia ac lædentia*, as the older physicians and physiologists phrased it, gives some confirmation to the view which teaches that spasm and starvation go in company with pain; relaxation and congestion only with tenderness. I will put the facts before you as premises; you will piece them together into an argument for yourselves. Chloroform is the greatest of *juvantia* in neuralgia; chloroform, indeed, and ether, in equal parts, as recommended by Mr. R. Ellis, may be safely entrusted to a safe person for self-administration; and, if taken persistently as well as prudently, may keep the attack in suspense until the enemy, from weariness or chronometric obligations, retreats or withdraws.* But this great reducer of neuralgia, this great and blessed producer of "indolence", as Locke called it, is also the great reducer of muscular spasm, as we know from its action and our employment of it in cases of hernia, and, indeed, of tetanus. Now, if it relaxes muscles, which we can see in the limbs and trunk without the aid of a microscope, we may think it not improbable that it will do the like by muscles, which we cannot see without the aid of that instrument, in the arterioles. Chloroform, secondly, has the reverse action to that of the sympathetic, in dilating the blood-vessels of the head;† as, indeed, also has alcohol, itself too a

* The anæsthetic effect of bisulphide of carbon in various kinds of headache, as pointed out by the late Dr. Kennion (*Medical Times and Gazette*, July 18, 1668, p. 77) may, perhaps, be similarly explained.

† I find that both Mr. Durham (*Guy's Hospital Reports*, iii, vi, 1860, p. 153) and Hammond (*On Wakefulness*, p. 25) are agreed that, in animals under chloroform, the veins of the brain become distended. I do not, however, lay any great stress upon this fact; firstly, because the veins may become distended under the influence, not so much of the chloroform, as of the more or less partial hindrance to respiration which its inhalation implies; and, secondly, because we have, as yet (Funke, vol. ii, pp. 769-773), no very distinct evidence for the production of effects on the veins by the sympathetic. But, taking the facts as given, we must allow that venous fulness, though inferior, doubtless, to arterial replenishment, is still, as the growing prostate of the aged, the rank hairs shooting up round old ulcers, and the cock's spur transplanted to the cock's comb, show, a more or less favourable condition for growth and nutrition; whereas pain is correlated always with malnutrition and ordinarily with atrophy, and is now always spoken of as a "depression" rather than as "an exaltation of function."

producer, though less directly, of the "indolence" we desire, as well as of much that we deprecate. And chloroform, thirdly, antagonises the sympathetic in its very obvious action on the pupil.

It may be bold in me to venture further in this direction; yet, as a member of the British Medical Association, and as a reader of our admirable JOURNAL, I may perhaps be allowed to say, before I return to my own more immediate subjects, that the account given by Dr. George Johnson in that periodical for March 21st, 1868, seems to me to indicate that epilepsy itself is but a frightful caricature of neuralgia, and of the results of vaso-motor irritation and contraction. The presence of dilatation of the pupil in all those sets of cases may be thought perhaps but a slight indication in the direction of identity of cause. High spirits and great vivacity are not rarely, in both diseases alike, precursors of an attack; while counterirritation, which both Schiff and Sitschenow* are agreed in considering a strong and universal reflex depressant, is not rarely, in both diseases alike, both, *ex hypothesi*, dependent on reflex vascular constriction, a preventive.†

I will now proceed to give, in the second place, an account of a physiological experiment worked out for us, from time to time, in the laboratory of Nature, which throws not a little light on a question which, I learn from Dr. Wilson Fox's work on *Dyspepsia* (p. 141), is still a matter of debate among pathologists—the question, to wit, "of the influence of perverted innervation in causing inflammatory, or sometimes even still severer morbid changes."‡ It is well known that stags, after injury

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* Setschenow's words are these (*Neue Versuche*, p. 23, 1864): "Es giebt endlich bei meinen Gegnern einen Versuch, an dessen Richtigheit ich keinen Grund zu zweifeln habe, an welchem die einseitige Trigeminus-Reizung eine starke allgemeine Reflex Depression hervorrief." These words seem to furnish something like a rationale of the picking of the nose in helminthiasis, as also of much of that counter-irritation of the fifth nerve at its periphery which so-called "nervous irritable" persons practise on themselves in the way of "tricks." Malgaigne practised similarly on his patients, as certain savage races do upon themselves with their labrets ear- and nose-rings.

† These views I came to entertain without any knowledge—or perhaps I should rather say without any conscious recollection—of those which Dr. Radcliffe had put before the world in his lectures delivered at the College of Physicians, and published in 1864 in his work on *Epilepsy, Pain, and Paralysis*. I have not altered what I had written in consequence of my consultation of this most valuable work, to which I resorted after seeing a reference to it in Dr. Anstie's book. This latter work I have already quoted in the text, and I found it most useful and suggestive to me. I believe, indeed I hope, that what I have written is more or less in accordance with Dr. Radcliffe's views. But it is as much inferior for purposes of consultation, and indeed in other points of view also, to what Dr. Radcliffe has written, and I have read of his, on the same subject, as a skull when just removed, and that in a somewhat fragmentary condition, from a barrow, is to the same skull when pieced together and reconstructed, as you may see many such skulls in the Museum, by Mr. Robertson.

‡ The following references to authorities on this vexed question I herewith append.

Lister, *Philosophical Transactions*, 1858, p. 627.
Beale, *Philosophical Transactions*, 1865, part i, p. 447.
Virchow, *Archiv*, vol. xvi, 1859, p. 428; *Cellular Pathology*, Chance's translation, pp. 311-312.
Paget's *Surgical Pathology*, Turner's edition, p. 237.
BRITISH MEDICAL JOURNAL, 1866, p. 402.
Anstie, *Lancet*, 1866, vol. ii, p. 548.
Simon, Holmes's *System of Surgery*, vol. i, p. 62.
Bernard, *Leçons Physiol. Pathol. Syst. Nerv.*, vol. ii, p. 518.
Brown-Séquard, *Lectures on the Physiology and Pathology of the Nervous System*, p. 143.
Budge, *Handbuch der Physiologie*, p. 794.
Funke, *Handbuch der Physiologie*, vol. ii, p. 776.
Donders, *Spec. Physiologie*, p. 140.
Billroth, *Die Allgemeine Chirurgische Pathologie und Therapie*, 1868, p. 72.
Niemeyer, *Lehrbuch der Pathologie und Therapie*, 1868, ii, pp. 320, 340, 428.
Handfield Jones, *Functional Nervous Disorders*, p. 11; Lectures in *Medical Times and Gazette*, 1865.
Samuel, Moleschott's *Untersuchungen*, Band ix, p. 18.

to the testes, have corresponding changes wrought out in the corresponding horns. Such a specimen I can show you from the Christ Church Museum, founded by Dr. Matthew Lee. In a curious old work dedicated to him, in company with two others of the King's Physicians, by Dr. Richard Russell,* and styled *The Economy of Nature in Acute and Chronical Diseases of the Glands* (pp. 21-24), five such experiments are recorded. In such cases as these, after the injury to the testis, the horn may or may not be shed annually, and it may never thenceforward lose its "velvet"; but it never becomes the dry lowly vascular weapon of offence which, in a fortnight or three weeks from the present time, we shall see the bucks polishing their hard leather coated horns into against shrubs and trees. It remains vascular and spongy within, and coated with hairy skin outside, which may be prolonged into pendulous outgrowths. Being sensitive and fragile, and bleeding easily, it acts as a second sexual disqualification; and, as I am speaking of this correlation of growth, I may be allowed to add, that its reality is further testified to by its absence after similar lesions in reindeer, where both bucks and does are alike horned. Now, I submit that the unilateral correspondence of malnutrition, such as we have here, is as good an instance and exemplication of Pflüger's first law of reflex action, the law of unilateral (*gleichseitig*) transmission of stimulus as any unilateral or homolateral twitching of any muscle can be in response to any one-sided stimulus. Only the reflex action shows itself in the way of nutrition—a sort of reversed hemiplegic nutrition, it is true—and not in that of movement nor in that of secretion.†

Let me, as in the former case, lay alongside of the physiological experiment a parallel to it from pathology. This I will do by the help of Budge, who, at pages 794-795 of his *Handbook of Physiology*, gives us the two following short histories, which have come under his own observation. His words run thus in translation. "After a long continuing stagnation of blood at the end of the small intestine and the beginning of the large, in consequence of which exudations and adhesions of the peritoneum ensued, the entire right half of the body became weaker than the left, was tired sooner by exertion; the right foot became cold sooner, under the same circumstances, than the left; the right ear became much more rapidly the seat of vascular dilatation than the left;

* It may be interesting to record here, in passing, that Dr. Richard Russell lived at Reading, that he was a friend of Dr. Chapman and of Dr. Frewin of this very place, and that the copy of his work to which I have referred, and which exists in the Christ Church Scientific Library attached to the Christ Church Museum, and deposited with it in the University Museum Buildings, did in 1760, eleven years before Dr. Russell's death, belong to Dr. Chapman. Now these facts and dates render it not improbable that this very specimen may have been given by Dr. Russell to Dr. Chapman, possibly together with this copy of his book. It is unfortunate that so much should be left to speculation; but this digression may be justified by the moral which it conveys, to the effect that we are bound, when receiving a specimen into a museum, to put on record forthwith, for the benefit of our successors, a note of its history and donor. Dr. Russell was the author of several other works besides the one I have quoted. Their existence has escaped the notice of Dr. Munk, in his interesting volumes, *The Roll of the Royal College of Physicians*, vol. ii, p. 132. Their titles are: 1. "De Tabe Glandulari sive de usu Aquæ Marinæ in Morbis Glandularum Dissertatio." In 1 vol. 8vo. Pret. 5s. 2. "A Dissertation concerning the Use of Sea-water in Diseases of the Glands"; to which is added an "Epistolary Dissertation to R. Frewen, M.D." In one volume, 8vo; price 5s.

† See Otto, *Neue seltene Beobachtungen Samml.*, vol. ii, p. 10. Elsaesser, *Diff. Sex. Mamm. præter partes sexuales*, p. 36. Since writing the above, I have seen a note to page 22 of Mr. Paget's *Surgical Pathology*, edited by Professor Turner, in which the fact that no disturbance of nutrition is effected by mere transplantation of the testis *in cocks*, is brought forward to shew that no mere nervous disturbance can account for these alterations of nutrition. I do not think that these negative results, obtained from experiments on half a dozen *birds*, can outweigh the positive facts of unilateral correspondence in malnutrition which have been so frequently observed in mammals. (See Hunterian Catalogue, Osteological Series, vol. ii, p. 591.)

and other similar phenomena developed themselves. After a great abscess under the right gluteus maximus, and an immense loss of pus, the right hand and the entire right arm became not only evidently thinner to the eye than the left, but also actually smaller." These cases are decisive as to the interference of the nervous system in the process of nutrition; and, though organs and structures, such as the epithelial and the cartilaginous, both physiological and, I suppose, morbid, may and do exist and grow in animal bodies being as devoid of blood-vessels and nerves as though they were found in vegetables, still any arguments based upon these undoubted facts can be met at once, if so we care to meet them, with the more or less accepted physiological axioms, "the interdependence of parts augments with their development; the solidarity of organs increases and is more intimate with each superaddition of a fresh factor to the entire economy." But these cases do not, of course, touch the question of the way in which this nervous influence comes to act on the tissues, whether mediately through the blood-vessels, or immediately on the tissues with which they are supposed, in the case of the salivary glands by Pflüger, though not, as I apprehend, by Dr. Beale (*Phil. Trans.*, 1865, p. 447), to become continuous. Nor does such an experiment as that of Bidder, in which a salivary gland, under nerve-stimulation, picked out two-thirds of the entire quantity of iodide of potassium in the circulation, to one-third picked out by the substance of its fellow, not so stimulated. For a more innervated gland is also a more vascular gland; and of the two antecedents, greater nerve-current and greater blood-current, we have no right from this experiment to say that the one rather than the other is the cause of this particular consequent. And much probability will come to attach itself to Virchow's views, according to which innervation is not proven to increase nutrition directly, but works only mediately by its influence on the blood-vessels, in the minds of persons who may be averse to multiplying laws by cases such as these. We go to a case, as I suppose most of us may, like myself, have gone, and we frequently find one side of the body hot, and the other cold. This latter, the friends will tell us, is the paralysed part; we find that it is not; and Bernard's experiments, and Brown-Sequard's (*l. c.* p. 146) enable us to understand why this is so. An excellent case to the same effect, showing how increase of vital properties may take place in the entire absence of any connexion with the upper part of the cord or brain, may be given from a paper of the late Sir B. C. Brodie's, in the twentieth volume (1837) of the *Medico-Chirurgical Transactions*. "A man was admitted into St. George's Hospital, in whom there was a forcible separation of the fifth and sixth cervical vertebræ, attended with an effusion of blood within the theca vertebralis, and laceration of the lower part of the cervical portion of the spinal cord. Respiration was performed by the diaphragm only—of course, in a very imperfect manner. The patient died at the end of twenty-two hours; and, for some time previously to his death, he breathed at long intervals; the pulse being weak, and the countenance livid. At length, there were not more than five or six respirations in a minute. Nevertheless, when the ball of a thermometer was placed between the scrotum and the thigh, the quicksilver rose to 111° of Fahrenheit's scale. Immediately after death, the temperature was examined in the same manner, and found to be still the same."

The larger size of a horse's hoof, the nerves of which had been divided, should probably be similarly explained by the greater afflux of blood which would set in thither temporarily until the continuity of the nerve was re-established. (Ogle, *Med. Times and Gazette*, Nov. 3, 1866.)

And, finally, such an occurrence as the inflammation of skin, cartilage, or cornea, after its own sweet will, and not in the line of an irritated nerve passing through it or near it (Virchow's *Cell. Path.*, Chance's Translation, p. 299), seems to speak plainly enough to the self-sufficiency of animal cells to respond to what Niemeyer calls "Insülte", without appealing to any higher powers for assistance; just, in fact, as though they were as little animal as truly vegetable, and as independent of any cranio-spinal centre as the gall-producing oak or willow.

But, in spite of all this, I am inclined to think that the direct action of nerves on cells is a *vera causa;* and, even if our highest microscopic powers do succeed in proving that nerve-tissues are never continuous with any other tissues in any part of their distribution, it must still be recollected that such intervals as may be demonstrated will be, if not insensible, at all events infinitesimal; and nerve-force may well be sufficient to act across such gaps as these (See Dr. Radcliffe's *Lectures on Epilepsy*, 1864, pp. 13 and 330). I can appeal for my justification to Professor Lister's experiment, recorded in his paper on the Cutaneous Pigmentary System in the Frog (*Phil. Trans.*, 1858, pp. 636-639), in which certainly the nerve-system is shown to have some control over the molecular movements of concentration and diffusion, quite independently of the blood-vascular system. The cessation of the circulation in a frog's web entails the concentration of the pigment; therefore Professor Lister took a pale frog—*i. e.*, one in which the pigment was already concentrated; and, tying a ligature above the ankle, so as to eliminate the condition of cessation of the blood's circulation, he then eliminated the condition of nerve-influence from the cranio-spinal axis by amputation above the ligature. *Cessante causâ, cessat et effectus*; the nerve-force is removed; and the pigmentary diffusion which it had held in check, is set up and continues, until superseded by the *post mortem* concentration which ordinarily takes place, and produces that lightening of the dark hue usually seen in the frog after death. This experiment, which I have not given in full, nor in Professor Lister's own words, is a very striking one; and I hope I may remark, without offence to any representatives of the German fatherland, to which physiology owes so much, that much that has been recently written and worked at there might have been spared, had Mr. Lister's papers been as well known to them as they will be to their successors. They seem to me to mark an era in the literature and in our knowledge of the essence of inflammation.

Here, if I may be allowed to digress somewhat, I would remark that Professor Lister's suggestion made in 1858 (*loc. cit.*, pp. 619 and 640) as to the probability of the existence in the limbs of a ganglionic apparatus co-ordinating molecular and other movements at the periphery, sometimes independently, sometimes subordinately to the cranio-spinally placed nerve-centres, may seem to have found a justification in Professor Beale's demonstration in 1865 of the ending of the muscular nerves in nucleate reticular plexuses. Assuredly, the discovery of these net-works bearing nuclei does away with the necessity for any further carrying on of the apparently interminable discussions as to the existence of an "ideo-muscular" as opposed to a "neuro-muscular" contractility. But I will take this opportunity of saying, that there are not wanting purely physiological considerations, which though not by any means amounting to demonstration, do nevertheless lend some little probability to the "neuro-muscular" explanation of those movements which take place in muscles separated from all connexion with central nerve-organs. Firstly, these movements are within my experience more marked and frequent in the muscular tissues of young animals; and the history of the develop-

ment of nerves would lead us to expect to find a greater degree of independence in the peripheral nerve-system, than we should look for in the adult organism; for nerves do not grow from cells in the direction of what we know in the adult state and under low powers of the microscope as their branches; but as Von Hensen has shown (Quain's *Elements of Anatomy*, 7th edition, p. clxiv) two nerve-cells are connected by a fibre, and it is by the withdrawal of the one cell from the other, and the elongation, so to say, of the interconnecting fibre, that the peripheral and central ganglionic systems respectively assume their adult relations. And just so, I may add, in certain annelids and lamellibranchiata, we have, as we not rarely do have in the lower animals, a stereotyped though but partial adumbration of what is but a single scene in the moving diorama of the development of the higher; and we find the peripheral nerve-system studded with eyes or other sensory organs, and possessed of a prominence and importance relatively to the central nerve-ganglia which is only temporarily seen in the development of more perfect creatures.

Secondly, many of the cases of death in adults, in which this irritability is found to exist most commonly and markedly, are cases in which, from very various reasons, the functions of the intracranial nerve-centres are put into abeyance at a very early stage in the process deathward. Such are (see Nysten, cited by Brown-Séquard, *Proceedings of Royal Society*, 1862, p. 211) cases of decapitation, of asphyxia, and of sudden hæmorrhage from a large artery. Now we know that movements do continue in a portion of intestine which has been deprived of its mesentery, and we ascribe the production of these movements to the presence in the walls of the intestine of the plexuses demonstrated to us by Meissner and Auerbach; if we may ascribe like effects to like causes, we may ascribe the *post mortem* twitchings of muscles to Professor Beale's neuro-muscular apparatus. In like manner we should expect from similar reasons, and we do find, as a matter of fact, this same neuro-muscular irritability greatly prominent in the small-brained cold-blooded vertebrata, and in hibernating mammals. In all of these animals alike, the central nerve-system is small relatively to the entire mass of their bodies; whilst in birds, or at least in the more highly organised of the class—for birds, like other bipeds, differ as to the mass and use of the brain (see Parker, *Zool. Trans.*, v. 1862, p. 207)—the brain may hold a more favourable relation to the entire mass of their bodies than in any other class of animals; and in birds, as is well known, with some few reptile-like exceptions, such as the peewit, muscular irritability ceases almost with their last act of expiration.

Whilst speaking of this condition occasionally found in the muscles after death, I am tempted to say a few words of the empty condition of the arteries which is almost constantly found after death. I observe that Von Bezold has explained this well-known phenomenon as being due to a last nervous impulse communicated to the small peripheral vessels *from the brain*. His words are—(*Untersuchungen aus dem Physiologischen Laboratorien in Würzburg*, Heft ii, p. 358, 359, 1867) "Sicher ist aber ein ungemein wichtiges Moment hierbei die Innervation der Muskeln in den kleinen Gefässen des Körpers. Man stelle sich vor, dass in der Agonie, in Todes-Kampfe, das Vasomotorische Central Organ im Gehirn noch in Krampf-zustande versetzt wird, welche mit Pausen der Erschöpfung abwechseln. . . . Ausserdem ist gezeigt worden, dass jenes letzte Ueberpumpen des Blutes aus den Arterien in die Venen, bei den Saugethieren wenigstens, unter dem Einfluss einer letzter Thätigkeit des Gehirns geschieht." Surely all these "Vorstellungen" would have been spared, if Professor Von Bezold had been acquainted with Mr.

Lister's papers, or even with those points in them to which I have referred. Indeed, that his view is untenable, is clear from a consideration of the fact that the circulation can be kept up, and will, like the muscular irritability, persist in a decapitated animal for a long while after death, if artificial respiration be put in play. The empty state of the arteries *post mortem* is most probably to be explained by the action of the peripheral nerve-system on the arterioles; though Dr. Alison would have explained it by the attraction *a fronte* force of the tissues around the capillaries; but Von Bezold's view of the source of the nervo-muscular action of the peripheral vessels is, I apprehend, more untenable and less plausible than most theories which have had their day and ceased to be.

The following short history seems to me to be a good instance of the action, or rather of the want of action, of the peripheral nerve-system upon the arterioles. A man, who came some years ago under my own care, had had a bullet pass through his arm just above the elbow, so as to sever the musculo-spiral nerve. The scars of exit and entrance were in the lower third of the arm. Under ordinary circumstances, the soft parts of the lower arm maintained their normal consistence; but their power of resisting changes of temperature was greatly impaired, as well of course as the sensibility of the parts supplied by the injured nerve. I recollect seeing the swollen state of the inner side of the hand one cold raw morning when the man was on sentry duty, and had his hand chilled down by the musket he had to carry. Now, I apprehend that this turgescence is to be explained by saying, that the local or peripheral nerve-system of the affected parts was competent under ordinary circumstances to regulate the calibre of the arteries; but that its activity was liable to be depressed, as under the circumstances related, into actual abeyance, in the absence of any possibility of any assistance being supplied to it from the cranio-spinal nerve-axis. Thus, under the depressing effect of cold, which seems to work here much as it does in checking the regeneration of artificially amputated parts in snails and in salamanders (Müller's *Physiology*, by Baly, 2nd edit., i, p. 444; Bonnet, *Œuvres*, tom. v, i, pp. 328, 329), the peripherally placed ganglionic system was put into abeyance; and turgescence of the vessels it ordinarily supplied with "tone" ensued. Just similarly in mammals the skin of which has been covered with an impermeable varnish, and in which death is as much due to the chilling down which the destruction of the non-conducting power of their hairy integument entails, as to the penning-in of its various acrid and volatile and other secretions, œdema and vascular congestion are to be observed in the skin, as well as in other organs (Ranke, *Physiologie*, p. 456). The flame of mammalian life, like the flame of inorganic combustion of hydrocarbon, can only be sustained at a high temperature; a certain reduction is as fatal to the one as it is to the other in the Davy lamp, and the vitality of the more exposed peripheral is more easily depressed than that of the more protected central nerve-system.

I should be paying but a poor compliment to the judgment which has provided a microscopic exhibition for the instruction and entertainment of this evening, if I were to dwell at any length upon the relations borne by Histology to Medicine and Surgery. And, secondly, if I were to dwell in the least adequately upon the importance of a knowledge of Microscopic Zoology to the diagnosis, and what is better than the diagnosis, and even than the therapeutics, the prophylaxis of diseases of all kinds from those which are considered trifling or contemptible by most men, except those who suffer from them, up to those which excite world-wide anxieties, such as trichiniasis or cholera, I should have to extend my address to a length you would shudder to think of. Upon

one single point I will make a few remarks; and the purport of these will be to show how the manipulation of such an instrument as a catheter may find, if we are to do justice to our patients, its regulative condition in the manipulation and revelations of the microscope. I had myself recently come to suspect that the determining condition of the triple phosphatic alkalescence of the urine was to be looked for and found in the presence of some of those organisms which Pasteur has proved and hygienists have believed to be the real causative agents of fermentations and putrefactions. One accepted view of the causation of this most mischievous metamorphosis is, that the coats of the bladder, in consequence of altered innervation, as after spinal injuries, act upon the urine as so much dead matter acts on blood in causing its coagulation, or as the tissues round about the capillaries act when they are in an abnormal condition upon the rows of blood-corpuscles within those canals; and by this "catalytic" agency break up the urea and throw down the ammoniaco-magnesia phosphates. Another view ascribes the like effect to the "fermentative" working of the abundant catarrhal mucus, which is in some cases flaked off from the inner walls of the bladder. Now, neither of these views suggests a *vera causa* for the effect for which they profess to account. Blood coagulates when in contact with non-vitalised matter, and blood-corpuscles arrange themselves in *rouleaux* under similar circumstances. But Mr. Lister, who has shown us so much which bears on this matter, has shown us also, and that in the last number but two (July 18, 1868), of our JOURNAL, that urine will remain for an indefinite period undecomposed in a properly constructed, which happens to mean a properly contorted, receptacle, even though that receptacle be as little vitalised as glass. And Niemeyer has shown, what I daresay many who are now honouring me with their presence have observed, but, I think, not recorded, that urine often retains its acidity through protracted cases of vesical catarrh, and in spite of cumuli of clouds of "fermentative" mucus; which are, therefore, as little of *veræ causæ* as is "catalysis" itself. But the presence of vibrios in the urine, and that before it leaves the bladder, is a *vera causa*, *i.e.*, a present condition, and therefore possibly a cause, or connected with the cause, of the phenomena to be investigated (see Beale *On the Urine*, p. 196); and the idea that the alkalescence in question depended upon them, an idea which I had not the time to find an opportunity for verifying for myself, I find has been verified for the benefit of others by Niemeyer, with the assistance of Traube and Teuffel. "In the course of last year," says Niemeyer (*l. c.* ii, 66, 1868), "I arrived, partly by means of an observation of Traube's, partly by means of experiments and observations of my own, which have been published by Teuffel in the Berlin *Klinische Wochenschrift*, at the conviction that it was not the vesical mucus, but lower organisms, which probably get into the bladder by means of the introduction of badly cleaned catheters, and excite there the alkaline fermentation of the urine." Now, whether the vibrios find their way into the bladder exclusively on dirty catheters or not, I apprehend that the addition of some carbolic acid to the oil used for lubricating these instruments, whether they be guilty or not of what is here laid to their charge, will be a piece of practice calculated to prevent the alkalescence which the vibrios cause by preventing these vibrios themselves from entering on their evil activity. Mr. Lister's paper, just alluded to, will show that this is an experiment which may very safely be tried; if carbolic acid can be safely introduced into a wounded pleural cavity, assuredly we need not hesitate about the passing it into a bladder. Thus many scientific researches, undertaken in the first instance for the elucidation of speculative truth, and for the rectification not of unsound

organs and functions but of unsound theories and explanations, and prosecuted throughout by the aid of the most refined methods and instruments, come ultimately to bear upon such matters as catheterisation and alkalescent urine. I would not, however, be thought to undervalue the worth of researches carried on at whatever cost with the sole object of procuring those correct notions as to the way in which processes, even wholly beyond our power of modification, have been and are being carried on. It is a great and positive gain when we get rid of one false hypothesis, one single false formula which by frequent repetition has attained to the dignity of a philosophic axiom, and acquired a sort of prescriptive right to "warp us from the living truth". The Chemists, as I am informed, are conspiring to effect what the old Greeks would have called a "Catalysis" of the kingdom of "Catalysis" itself, and its banishment to the Limbo of Vanity, there to herd with Phlogiston and many other and younger as well as older unsubstantial Idola Theatri; and though these alterations of theory may not as yet have affected the oxygen we breathe, nor even have enabled us as yet to regulate with any greater precision the processes of fermentation with which we have for so many ages had an empirical familiarity, they have given us at least a warning as to maintaining always that proper diffidence as to the all-sufficient validity of our theories, by whomsoever promulgated or endorsed, which is so constantly of avail in actual practical work. The phenomena, let me add, to account for which the hypothesis of Pangenesis has been recently (Darwin, *Animals and Plants under Domestication*, 1868, ii, p. 403) put forward provisionally, are and will, we may believe, always remain, beyond our control; but there will be no one, I suppose, who will not feel an interest in observing how the revelations in the all but infinite divisibility of "germinal matter", which we owe to Professor Beale, may come to bear upon the explanation of the marvellous phenomena of reproduction and hereditary transmission. Nor can I leave this subject without remarking that it is in great probability upon the self-multiplication of such infinitesimal particles as this hypothesis of Pangenesis postulates that processes, to the naked eye the very reverse of Genesis, have been found to depend; and the Blue Book on the Cattle-Plague (*Third Report of the Commissioners*, etc., p. 151), will show you that here, too, we are dependent on the employment of the very highest powers of the microscope; and it is scarcely necessary to add that its employer was in this case Professor Beale.

If I have been short in speaking of the advantages which the histology of modern days has conferred upon its therapeutics, I might be shorter still in dealing with my third head—the dependence, namely, of the healing art upon the facts of Anthropotomy—that is to say, upon the naked-eye knowledge of the structure with which it has to concern itself. Some little, however, I must say with your permission. Some persons are inclined to think that there is some sort of antagonism between the interests of microscopic and those of naked-eye anatomy; and hints more or less obscurely expressed may be found to this effect here and there in writings even of the present day. It is in much the same spirit that persons are found to say that the sending of missionaries to the heathen abroad entails so much curtailment of similar work at home, and that others will say that the starting of any fresh charitable institution necessitates the subduction of so much from the funds available for those already on foot; and that others again will say that the encouragement of natural science is "inimical" to the progress and cultivation of literary and classical studies. Now, all these views depend upon the radically false assumption that intellectual and moral activities are limitable and measurable by certain quantitative conditions, just

as a man's expenditure is or ought to be limited and measurable by his balance in the bank. This analogy is a wholly fallacious one, but it has nipped many an excellent project in the bud. A truer analogy is furnished us by the history of those infinitesimal scraps of germinal matter of which I was just now speaking, which are hard to destroy even with floods of carbolic acid and copperas, and which possess a faculty of self-multiplication wholly unparalleled within my experience in the history of the metallic objects of which we were just now speaking. Activity and earnestness, in fact, which are some of the best things, resemble some of the worst in being eminently contagious. The example of a strenuous labourer in one field, spreads into the weedy acres of his slothful brethren on the right and left; and the improvement of the microscope has but been accompanied by a more thorough and accurate working out of human dissection. Let us leave metaphor and general statements, and come to facts. I have in my possession a work written for the use of anatomical students in the University of Edinburgh—a place then, as now, at least on a level with the most advanced centres of such education elsewhere in Great Britain. Its title is, *The Anatomy of the Human Bones and Nerves, with a Description of the Human Lacteal Sac and Duct*, by Alexander Monro, M.D., late Professor of Anatomy in the University of Edinburgh. A new edition, carefully revised, with additional Notes and Illustrations, by Jeremiah Kirby, M.D., author of *Tables of the Materia Medica*. 1810." The date of its appearance takes precedence, therefore, by a dozen years at least, of the first appearance of an achromatic combination; and if the development of microscopic zeal had really been injurious to the diffusion of thorough anthropotomical knowledge, we should find here in perfection that precision and fulness which *ex hypothesi* are the exclusive fruits of individual attention and undistracted concentration. Now, a few weeks ago, I was pursuing some anatomical researches into the homologies of the shoulder-joint muscles, and by the suggestion of one or two of my friends, amongst whom I may mention Dr. Boycott, I took up the line of argument for homological identity which innervation furnishes. Being deep in the country, I was reduced to consult, in the absence for the moment of other books, the work I have just mentioned, for a small matter in the composition and decomposition of the brachial plexus. This is what I found to satisfy my inquiry in a book expressly treating, you will please to recollect, of the nerves, and written by one of those "famous old anthropotomists" who were not distracted by "microscopische Spielereien." "The fourth cervical nerve, after sending off that branch which joins with the third to form the phrenic, and bestowing twigs on the muscles and glands of the neck, runs to the armpit, where it meets with the fifth, sixth, and seventh cervicals, and first dorsal, that escape in the interstices of the *musculi scaleni*, to come at the armpit, where they join, separate, and rejoin, in a way scarcely to be rightly expressed in words; and, after giving several considerable nerves to the muscles and teguments which cover the *thorax*, they divide into several branches, to be distributed to all parts of the superior extremity. Seven of these branches I shall describe under particular names." (P. 291.) These seven branches have the particular names of Scapularis, Articularis, Cutaneus, Musculo-Cutaneus, Muscularis, Ulnaris, and Radialis. Of such little trifles as the connexion of the second and third cervical sympathetic ganglia which give off heart-nerves, with the arm-nerves, upon which connexion the pain down the inside of the arm in heart-disease Niemeyer (ii, 338) supposes may, and the older anatomists would have said *must* depend; as the subclavius nerve and its connexion with the phrenic, and

so with the shoulder-tip pain in liver-disease, we have just as little mention made as we have of the nerves supplying those small muscles, the pectorales. Surely knowledge is not like a volcanic archipelago, where the upheaval of one mass of solid ground entails the submergence of another; rather it resembles some vast table-land which is rising, now and then at accelerated rates of progress, out of the waters, and has, in these days of the subdivision of property and labour, its broad and continuous surface seized upon, partitioned out by enclosures, and put under cultivation by various occupants so soon as ever its outlines are recognisable.

My last topic in this division of my address is the connection which Comparative Anatomy has with Medicine and Surgery, and the bearing which a cultivation of this department of Biology has, or is likely to have, upon the interests of the profession. Of the benefits which Comparative Anatomy receives at the hands of medical practitioners there is little occasion to speak; or rather Mr. Parker's volume on *The Shoulder Girdle*, just published by the Ray Society, may speak for me; it is only less vast than valuable, and will constitute the commencement of a new epoch in the science. But what I have to speak to is, not the benefits which Comparative Anatomy receives, but those which it can confer. And I believe that the educational working of this study is perhaps the particular line along which the best fruits for the profession, and for the public, may reasonably be looked for. Any study which forces its students into that most valuable knowledge—the knowledge of when a thing is proved, and when it is not—is *ipso facto* an ally of real medicine, and a deadly enemy of quackery. A person who has in any way become acquainted with what reasoning and reasons are in one subject, will be apt to look for similar reasons and similar reasoning when he has to deal with another, and especially if that other be a closely allied subject. And when natural knowledge shall have become more widely diffused through the general public, an end which we may hope to help towards accomplishing by means of our School of Physical Science, quackery, with its painful spectacles of reputation and confidence unfairly withheld and more unfairly bestowed, will cease to flourish in all its present exuberance. A worker in Biology gains reputation accordingly as he is acute enough to observe and generalise for himself, and accordingly as he is conscientious enough to make himself master of and duly acknowledge the labours of others. It cannot be said that learning, talent, and labour are equally certain to secure prominence either for a medical doctrine or a medical practitioner. The medical doctrine obtains currency, acceptance, and popularity, and the confidence of an ill-educated public, by virtue ordinarily either of the effect on the imagination which its paradoxical character secures for it, or of the effect on the ear of the alliterative ring of the phraseology in which it is embodied. The success of persons, again, in the medical profession and in some other walks of life too, may depend on personal qualities quite other than any connected with diligence, attainment, or ability—upon, say, certain peculiarities of manner, either in the way of polish or of that of roughness. The greatness of the stake, his own health, for which a man is playing when he adopts a particular doctrine or doctor of medicine, no doubt disturbs the balance of such powers of judgment as he may have, much as in Gessler's hopes the placing of an apple on the head of Tell's son disturbed the steadiness of the father's hand and eye. But habits of thought, as of other things, may be acquired by a proper course of education, and habits, like drill, steady a man under emergencies; and a scientific training enables a man to set about forming a right judgment in a right way and upon proper and legitimate grounds, even when no-

thing less than life itself is at stake. If a knowledge of such a subject as Comparative Anatomy, and of its external aspect, scientific Zoology, is a knowledge which will give the layman more power of forming right decisions, it is perhaps needless to labour long at showing that this self-same knowledge may be of the like service to the professional man. A sort of practical proof of its value is furnished us by the fact, that in Edinburgh as in Germany a dissertation on some subject of Comparative Anatomy is often accepted as a thesis for the degree of Doctor in Medicine. By such a regulation we have the obvious fact recognised, that the same sort of skill in the employment of methods, the same familiarity with organs, tissues, and functions, the same reasoning powers, are employed in investigating the problems of life wheresoever existing. The second aphorism of the *Novum Organon* applies to the one as to the other line of investigation, that of Human, and that of Brute Biology:—*Instrumentis et auxiliis res perficitur;* and alike in both, *nec manus nuda nec intellectus sibi permissus multum valet.* Comparative Anatomy finally, besides thus benefiting the public firstly, and the profession secondly, is of use to Human Biology and Medicine, as such, inasmuch as it casts so much light upon the problems which the more highly evolved organs, functions, and other relations of our own species, render in a much higher, and, indeed, sometimes in the highest degree difficult or impossible to investigate. Answers to what are riddles in Human Anatomy and Physiology are often to be found given in very simple language in the structures and functions of the lower and lowest animals. Of such hints furnished by the brute creation towards the proper solution of certain problems which concern each and all of us in dealing with our own species, I will herewith, by your permission, give a few. Of the use of rest towards the repair of injuries I presume there is little doubt, but the best established teaching is all the better for the support of a few concrete examples. Now, in what animals do we find the greatest capacity for repair of injuries, and for the reproduction of lost parts and limbs? Precisely in those in which the whole of life is carried on at the lowest rate, and in the nearest approximation to rest which is compatible with animality,—in those animals, to wit, which breathe water, and have but its scanty percentage of dissolved oxygen to sustain their animal functions. The metamorphoses which an animal may have undergone, or may have to undergo, have very little directly to do with its power of recovery from injury, or of regenerating a lost limb. No animals go through more complex metamorphoses than do many of the Crustacea, and nearly all the Echinodermata; yet assuredly no other class has a larger capacity for the reproduction of lost fragments of their bodies. Now the latter of these classes is exclusively, and the former all but exclusively, aquatic. The more perfect, again, an insect's metamorphosis, *i.e.*, its power of building up tissues and organs, the more perfect ordinarily, or rather the more profound, has been its quiescence as a pupa. Indeed, the very exception here proves the rule, and proves it to be a good one; for such hemimetabolous insects as, like May and dragon-flies, come, in their imago state, to differ almost as much from their larval forms, as the imagos of many holometabolous insects do from their larvæ, are during those preparatory stages as completely aquatic as any crustacean (Westwood, *Introduction to Entomology*, vol. ii, pp. 29, 38; Carus and Gerstaecker, *Handbuch der Zoologie*, p. 29). I am aware that there is such authority as Mr. Paget's (*Surgical Pathology*, ed. Turner, p. 123), and Mr. Darwin's (*Animals and Plants under Domestication*, vol. ii, p. 15) in favour of regarding the power of repairing injuries as standing in an inverse ratio to the amount of metamorphotic change through which an animal has gone; and I must therefore

take the more pains to show that my explanation, to the effect that this happy power depends mainly upon the peacefulness and quiet with which the various processes of life are carried on ordinarily, and after the mutilation, is the truer one. My opponents' case would rest on such facts as these which follow. I will give them first, and then show how they really support my views. The larvæ or tadpoles of the tailless Batrachia, but not the adults, says Dr. Gunther (Darwin, *loc. cit.*, and Owen, *Comparative Anatomy of the Vertebrate Animals*, vol. i, p. 567), are capable of reproducing lost limbs. So with insects, says Mr. Darwin, *in loco*, "the larvæ reproduce lost limbs, but, except in one order" (the Orthoptera, and amongst them the Phasmidæ*), "the mature insect has no such power." There is, however, one common property which lies at the bottom of the power of repair both in the larval forms and in the perfect adult animal, both in the invertebrata and in the vertebrata specified. This common property is the *comparative insignificance of the apparatus for aërial respiration:* in all alike—in the larva of the anurous amphibia, in the larva of the butterfly, and in the orthopterous insect—the lungs or the tracheæ, as the case may be, contrast to disadvantage with those of their congeners, or adult representatives, which have come to differ from them in having lost the power of reproducing lost parts. But active respiration is a prerequisite for activity of function and rapidity of rate of vital processes; and the absence of this is, according to my argument, the cause of the presence of the reparative power. The lungs are of course all but wholly in abeyance in the tadpole, and the tracheæ have no vesicular dilatations developed upon them in the caterpillar forms of any insect, nor in the adults of the non-volant Orthoptera. In the Phasmidæ, the curious "walking-stick" insects, we observe just the same sluggishness, combined with great tenacity of life, which we observe among mammals in the Bruta. Let me add some more facts in further illustration of my position. The Myriapoda, which Mr. Newport has shown to possess this power of repair up to the time of their final moult, are so little like the more typical insects, as to have been classed with the Crustacea, by no less an authority than Von Siebold. Any one, again, who will compare the simple non-cellular lung of the adult Batrachian newt *Salamandra Aquatica*, which possesses an unlimited power of repair *as an adult*, but *not in its young stages* (Bonnet, *Œuvres Hist. Nat.* v. Pt. i, p. 294), with the lung of the adult frog, will have little difficulty in understanding how their power of repair differs out of all proportion more than the amount of the metamorphotic changes they severally go through. The land Salamander, *Salamandra Terrestris*, has, so far as I know, escaped the hands of Spallanzani and Bonnet; its adult lung being little inferior in extent and development of spongy matter to that of the adult anura, I should expect the power of regeneration to be reduced to zero as in them. If the teaching of Comparative Anatomy has forced me to differ from the teaching of Mr. Paget, there are other facts in the same region of research which, as it seems to me, put one of his other many valuable doctrines in a clearer light than even his own clear enunciation of it. "Each man's capacity," says Mr. Paget (*Lancet*, Aug. 24th, 1867), "for bearing a surgical operation may best be measured by the power of his excretory organs in the circumstances in which the operation will place him." Now, I am inclined to ascribe the very considerable, and, indeed, on my views, somewhat exceptional powers of reproduction which two sets of air-breathing terrestrial animals, the pulmonate snails and the earth-worms, possess, to the great develop-

* There seems to be some little doubt whether even a a Phasma can regenerate lost parts after its last moult, and some authorities would not consider it adult till after such ecdysis. The crustacea, however, moult many times after attaining the adult state, *i.e.*, a state in which they can reproduce *the species*.

ment of their excretory apparatus. Living, as they do very ordinarily, in atmospheres laden with carbonic acid from decaying vegetable matters, they must get rid of the products of their waste and wear in the shape of solution in fluid; and the alkaline secretion with which the bodies of both are so abundantly slimy, furnishes just the required medium. When injured or mutilated, these animals can withdraw themselves pretty completely from the atmospheric oxygen by shedding out this secretion, and it at the same time disembarrasses their system from any excess of carbonic acid which may be generated within it. Thus they can attain the most perfect possible condition for repair and regeneration, the minimum of activity of all save the excretory organs; and I submit that it is possible that these two conditions may be connected as cause and effect, just as in the reverse direction a defeat of surgical skill may be connected with the presence of a fatty kidney or liver, or the excitability of a nervous system. It is going perhaps too far to attempt to explain the much greater power of repair which Amphibia possess as compared with either Pisces below or Reptilia above them to the larger size, and consequent smaller aggregate surface and less perfect aërating power of their blood-cells, and to the transpirability of their naked skins, which execute such important depuratory work for them, and are so closely connected and correlated with their lungs, livers, and kidneys. It is curious, however, and interesting to remark that the older anatomists, in commenting on the very obvious solidarity of these latter organs, went on, in their ignorance, I imagine, to a great extent, of the nature of amyloid and other degenerative changes in such cases, to observe that it was illustrated by the "fact" that, as the lungs grew smaller, so the kidneys grew larger in phthisis.* (See Funke, *De Salamandræ Terrestris Vitâ*, 1827, and Meckel, *Pathol. Anat.*, vol. i, 613, 646.)

Verloren, as quoted by Donders ("On the Constituents of Food," translated by W. Daniel Moore, M.D., p. 24), has shown how the history of insects bears on the question which we are this afternoon to have expounded to us of the "Relation of Food to Force"; and the very title of Bischoff's and Voit's work, *The Laws of the Nutrition of the Carnivora*, shows how this subject, to which I shall no further allude, but leave it to the able handling to which it has been entrusted, is dependent on the life, and the modes of life, of the lower creation. But I would say that it was from a study of the structures of the class of animals last mentioned —viz., the Carnivora—that I first came myself to be convinced that the uterine mucous membrane would, if properly looked for, be found in all animals alike to stretch after delivery over the area previously occupied by the placenta; and assuredly there is no one of the many complex and hard to be investigated problems of human physiology to which we are more bound to be thankful for light whencesoever obtained; and this, though the light come, as, in justice to Dr. Matthews Duncan and Dr. Priestley, I must say it did, in the way of illustration and confirmation rather than in that of discovery. (See Zoological Society's *Transactions*, 1863, p. 289; Dr. Duncan's *Researches in Obstetrics*, p. 206.)

These facts of the structural and functional arrangements of the lower animals have been used recently to illustrate some other points of uterine pathology and therapeutics in our own species. In a work by Dr. F. A. Kehrer, of Giessen, in two parts, the former of which was published in 1864, and treats of "Die Zusammenziehungen des weiblichen Geni-

* For accounts of experiments as to regeneration of lost or destroyed parts, see Darwin, *Animals and Plants under Domestication*, vol. ii, p. 15, *ibique citata*. Owen, *Comp. Anat. of the Vertebrata*, vol. i, p. 567. Newport, *Phil. Trans.*, vol. cxxxiv, 1844, *ibique citata*. Paget, Surg. Path., ed Turner, p. 123. Spence Bate, *Ann. and Mag. Nat. Hist.* for the current month, Aug. 1863, citing Mr. Lloyd of Hamburg, p. 118. McIntosh, *Experiments on Carcinas Mœnas*, p. 28.

tal-canals," and the second of which was published in the present year, 1868, and treats of "Die Vergleichende Physiologie der Geburt des Menschen und der Saugethiere," I find no little light thrown upon the question of relative position, whether as cause or effect, in which early and late abortions respectively stand to imperfect involution of the uterus. And I find also *in loco* a very distinct admonition as to the inexpediency of allowing fear of decomposition to terrify us into what is called, "meddlesome midwifery." These extracts I think you may be interested to hear; I will simply quote them, and leave you, who are so well able to do it, to make the application of them for yourselves. "Finally, let it be remarked that in rabbits in the earlier stages of gestation I saw the fœtus with its fœtal envelopes protrude entirely out of the os uteri, whilst the placenta still remained firmly attached in the uterus; a phenomenon which indicates either that in the earlier stages of pregnancy the placenta materna is less lacerable, or that the motor power of the uterus is a relatively smaller one, and one which finds its analogy in the occurrences which take place in abortions and premature deliveries in the human subject." Heft i, p. 52. In his second Heft, p. 166, Dr. Kehrer, in speaking of the retention of the placenta being sometimes followed by symptoms like septic poisoning and sometimes not, has words to the following wise effect:—"What chemical changes may be set up in the retained placenta is clearly dependent hereon, whether the after-birth is shut off from contact with the air by the genitalia or not; for, if air find access to it, the membranes of the ovum putrefy; if air be excluded, a process of decomposition, probably identical with one of maceration of the fœtus, but wanting further chemical investigation, is set up. The occurrence of the one or the other eventually is so far of importance, as thereupon hangs the after-effect of a retention of the placenta upon the general health. In fact, we observe in women, just as in the animals mentioned, sometimes only insignificant symptoms, sometimes emaciation and sickness; sometimes, as after the absorption of putrilage from the decomposing membranes, a violent, even a fatal pyæmic fever. In the face of these facts, it seems to me to be rational in ruminants, in which the cotyledon can only be detached from the uterus by considerable violence, and scarcely even then, completely to *avoid all introduction of the hand into the cavity of the uterus after delivery*, just with the object of keeping it free from the ingress of air, and to leave the separation of the placenta rather to the natural forces. We shall thus best avoid putrefaction being set up in the cavity of the uterus, and so expose the animal the less to the risk of pyæmia." I have not quoted from the recently published works of Dr. Matthews Duncan and of Dr. Grailly Hewitt, though I have specified in my notes the pages of those works which bear upon what I have just quoted from a foreign source. I have forborne to do this, not because I think those works less, but because I think them more valuable, and I presume they will be in your hands as they have passed through mine.*

The human anatomist who has once seen in the lower animals the structures which represent, as it were, in exaggeration or caricature, the human costo-coracoid membrane; the tuberculum pubis, with the homologue of the clavicle which is attached to it as Poupart's ligament; or the supracondyloid process of the humerus—is not likely to forget their existence when, with either scalpel or bistoury in hand, either for the ligature of an artery or the setting free of a hernia, he has to deal with their representatives in the human frame. But, if I am right in think-

* Dr. Grailly Hewitt, *The Diagnosis, Pathology, and Treatment of Diseases of Women*. Second edition, 1868, pp. 32, 342, 393. Dr. Matthews Duncan, *Researches in Obstetrics*, pp. 276, 284, 285. Cazeaux, *Traite des Accouchemens*, pp. 334, 349.

ing that the ciliary muscle in the eye would not have secured for itself the notice which it has done of late years, had it not been for the much more obvious manifestation of a similar, if not homologous, muscular apparatus in the eye of the bird, I apprehend that I am justified in saying that every surgeon who performs Mr. Hancock's operation of cylicotomy for glaucoma is under obligations, whilst so doing, to Comparative Anatomy and Sir Philip Crampton. I need not speak of the bearing of the discovery of this muscle in the human eye by Mr. Bowman upon the physiology of its adjustment to clear vision at varying distances. Pure Physiology, again, unassisted by Comparative Anatomy, has made out much of pure function; but, much as has been attempted in the way of experiment with infusions of pancreatic substance, and with the introduction of cannulæ into the duct of the gland, I am inclined to think that a comparison of the relative size of the gland in the carnivora and the herbivora respectively, in a dog, say, and in a rabbit, points as unmistakably as any of the lines of experiment just referred to—which, by the very nature of the case, are greatly beset with several sources of fallacy—to the fact that this salivary gland is concerned as much with the digestion of albumen and fat, as with that of starchy substances. With the remark that Hyrtl's discovery (*Wien. Zool.-Bot. Ges.*, 1861, cit. Henle, *Handb. der Anat.*, ii, 310) of the diverticular character of the glomeruli in the kidney of the selachians and amphibia bears not a little upon the existence of a similar arrangement between the vasa recta and the renal arterioles in the human kidney, whereby, as by the direct communication shown by Schrœder van der Kolk to exist between the arteries and veins of the *pia mater*, the capillary circulation may be skipped, and the tissues in relation with it left at rest, I leave this part of my subject, and begin the concluding portion of my address.

In this part of my address I propose to consider, mainly by the light of recently attained biological results, the value of two great rules for the conduct of the understanding, each of which has a legitimate sphere of application, but the former of which enjoys, it seems to me, more and the latter less than its deserved prominence. The first of these two regulative principles has received the endorsement of Newton, and it stands as his first "Regula Philosophandi", at the commencement of the Third Book of the *Principia*. It was known in the days of the schoolmen as the "Razor of Occam", and in later days it has been styled the "Law of Parsimony" or "Economy". Newton enunciates it as follows:—"Causas rerum naturalium non plures admitti debere, quàm quæ et veræ sint et earum phænomenis explicandis sufficiunt. Dicunt utique philosophi: 'Natura nihil agit frustrà;' et 'Frustrà fit per plura quod fieri potest per pauciora.' Natura enim simplex est et rerum causis superfluis non luxuriat." I know that this Regula has great influence on the minds of many biologists, and I believe that this its influence is by no means always for good. This is not a subject in which authorities ought to count for much; but I may say that, while the names of Aristotle, Malebranche, Maupertuis, St. Hilaire, Goethe, Bichat, and Dugald Stewart may be quoted in approval of this rule, the names of Bacon, Mill, and De Candolle may be brought forward by those who repudiate it or curtail its application. Our motto, however, is "Nullius addictus jurare in verba magistri"; and our business is to ask, not what men have laid down, but how Nature operates. Can a phenomenon have more than one cause, or can it not? Is it possible, for example, and to put the question in a concrete and most practically interesting point of view at once, that a fever which we know can spread by infection or contagion, can also originate spontaneously? I rather incline, though but doubtfully, and after an imperfect examination of imperfect data, to anticipate that a negative answer to this latter question will turn

out some day to be the true one; but I do not know that there is anything in the analogy of Nature to compel us to incline towards this negative conclusion *à priori*. Such a phenomenon, at all events, as a living animal, is often enough produced by two or more distinct processes, within the limits of the same species: as, for example, from ova of different character, summer ova or winter ova, impregnated or unimpregnated ova; by fission or gemmation; through two different series of metamorphotic changes. And such a phenomenon as the production of a particular tissue may depend—in the case of adipose tissue, for example—upon the employment in Nature's laboratory of one or the other of two different chemical compounds. In each and all of these cases, the maxim which has many a time been sonorously enunciated in these Schools, "Entia non sunt multiplicanda præter necessitatem", would, if listened to, have closed our ears and eyes to at least one-half of the truth. That Bacon would have classed this maxim with his Idola Theatri, I think I am justified in saying, for that in the very next section (Section xlv) of the *Novum Organon* to that in which he treats of these delusive notions, I find these words:—"Intellectus humanus ex proprietate suâ facilè supponit majorem ordinem et æqualitatem in rebus quàm invenit;" and if I am told, as by Mr. Mill (*Logic*, vol. ii, p. 379, ed. 1846), that Bacon, in the actual practice of investigation, acted as though there were no such thing as Plurality of Causes, I need only answer that herein his practice did not differ from his precepts at all more widely than does the practice of many other writers, of many practising, of many teaching doctors, differ from theirs. I have a satisfaction in quoting the living De Candolle, who enjoys one of the first and best deserved scientific reputations of the day, in repudiation of Maupertuis' famous principle of "least action". De Candolle writes thus in his *Geographie Botanique*, vol. ii, p. 115—"Nous aimons à croire aux moyens simples, peut-être uniquement à cause du peu de portée de notre esprit."

What, then, is the legitimate application? where does Nature really bind herself to the observance of a "Law of Parsimony"? In, as I think, three distinct lines of her operations.

Where an organ can be diverted from one and set to discharge another function, there Nature will spare herself the expense of forming a new organ, and will adopt the old one to a new use. She is prodigal in the variety of her adaptations, she is niggard in the invention of new structures (Milne-Edwards, cit. in Darwin's *Origin of Species*, p. 232). The complicated arrangement of co-operating muscles whereby the bird's third eyelid is drawn across to moisten and wipe its eyeball without undue pressure on the optic nerve, is manufactured, if so we may express ourselves, out of the *suspensorius* muscle, which in other animals has but the simple function of slinging up the eye. The scarcely less complex and beautiful arrangement of the bird's *levator humeri* is the result of a modification of a subclavius muscle.

Secondly, where, by availing herself of the inorganic forces always at work and available in the circumambient medium, whatever that medium may be, or where, by the employment as in what is called "Histological Substitution", a lowly organised or vitalised tissue, such as elastic tissue, she can spare herself the manufacture of such expensive structures as muscle, there Nature adopts a line of practice which we call a Law of Parsimony. Where a suspensory muscle for the eye can be dispensed with altogether, as where there is a more or less closed bony orbit, as in ourselves, and an air-tight cavity formed by it together with the soft tissues lining it, there atmospheric pressure is trusted to to steady the eye in the socket, as it refixes the tooth loosened by inflammation, and holds the head of the femur in the acetabulum. The eye of the burrowing mole, on the other hand, loses its *recti* and *obliqui* before it verges

itself into total extinction; but this very *suspensorius* it retains after the wreck of its other property, as its guardian in the undivided undifferentiated temporo-orbital fossa.

Thirdly, where matter that would otherwise be wholly refuse, and to be rejected, can be utilised, there Nature exemplifies this law by her "utilisation of waste substances". The transverse colon, with its various contents, aids and ekes out the elastic recoil of the lungs in expiration; and by its near approximation to the stomach, has, as Duverney long ago pointed out, the shock of the ingestion of fresh food propagated directly to it as a warning against sluggishness in the discharge of its own function. The air we use in speech, as Mr. Paget has pointed out, we could not use for breathing.

Many other instances of the "Law of Parsimony" might be given; but I know not of any which cannot be reduced under one or other of these three heads; I know of none, that is, which can be in any way held to negative the tenability of a law of "Plurality of Causes".

The second great principle for the regulation of the understanding of which I wish to speak, is one which, I believe, possesses less currency and notoriety, and is less observed than it deserves. This canon bids us, in considering a complex phenomenon, to be most careful that we omit none of the circumstances which can by any possibility be of the essence of the case. And as the possibilities of Nature are all but infinite—as, for example, the investigator of problems of geographical distribution knows well that a "secret bond" may bind up together, and that inextricably, the interests of organisms removed as far as possible to all appearence from each other in the scale of life; as a fly or a plant may, by its increasing and multiplying, make half a continent uninhabitable or inhabitable by the highest mammals; I apprehend that in biological and medical problems, by the phrase "all the circumstances which can by any possibility be of the essence of the case", we mean practically, "*all* the circumstances of the case", without any qualifying limitation. But we will let Descartes, to whom the enunciation of this rule is usually and, so far as I know, rightly assigned, enunciate it for us in his own words. These run thus (*Œuvres*, tom. xi, 1826, ed. V. Cousin, p. 23):—"Règle Septième pour la Direction de l'Esprit. Pour compléter la science, il faut que la pensée parcoure d'un mouvement non interrompu et suivi tous les objets qui appartiennent au but qu'elle veut atteindre et qu'ensuite elle les resume dans une énumération méthodique et suffisante." Some of the very greatest advances which have been made of late in practical diagnosis have been made in the spirit of this recommendation. The application of a chemical test to the urine for information as to the expediency of giving or withholding wine in the case of a sinking life, would have seemed to Swift, could he have had any idea either of such a procedure or of the employment of a sphygmograph for the same object, more absurd than any of the follies he ascribed to the philosophers of Laputa. But as Archbishop Whately, a name to be greatly honoured here, and, indeed, wherever else liberality, and fearlessness, and ability are held in respect, has well pointed out, the absurdities of Laputan aspirations are less wonderfnl than the actual attainments of modern science. And to these results science has attained, for that her votaries have known that what may seem to Swift, and such as Swift, to be but curious and dilettante, otiose, or even disgusting, may turn out ultimately to be essential elements in problems, the solution of which promote directly and greatly the interests of man, and the glory of Him to whom nothing is common nor unclean. Could anything have seemed at first sight to be more impertinent, more otiosely curious and trifling, than to inquire during an epidemic of cholera what was the nature of the subsoil in the area it was ravaging, to what depth it was porous, and at what

level the water was, and had been previously, standing in it? Yet, as I think, Von Pettenkofer has at last fought out and won his battle on these points (see *Zeitschrift für Biologie*, Bd. I, Heft iii und iv, 1865; Bd. II, Heft i, 1866; Supplemental Heft, 1868, p. 54); and the distinguished President of our "Public Medicine" section, Mr. Simon, who is as little prone as most men of science to take up over readily with any new wind of doctrine, has told us (*Report of the Privy Council* for 1866, p. 366 and 457) that certain of his carefully observed cases of the distribution of this disease seem to illustrate and find their explanation in Von Pettenkofer's theory. I have pleasure in adding that I see, by papers published by the illustrious Professor of Munich in the *Allgemeine Zeitung* for June last, that he has been able to show that, amongst all the other circumstances of the case at Gibraltar and at Malta, there were still to be found, all guessing and objections notwithstanding, the porous subsoil and the retreating ground-water, as factors in the complex constituting an area or arena for cholera. Let us attend to and note always all the circumstances in every complex phenomenon which we have to investigate, but let us not betake ourselves over-hastily to the process of eliminating antecedents, until we be quite sure that they do not enter as factors into its causation.

I may say, in conclusion, that attention to this seventh rule of Descartes might have saved such students of Natural Science as have fallen into materialism from falling into it. The Physiologist, as such, has nothing to do with the data of Psychology which do not admit of being weighed or measured, nor of having their force expressed in inches or ounces. This language, which I long ago employed myself (*Nat. Hist. Rev.*, April 1861; *Med. Times and Gazette*, March 15, 1862), coincides with an utterance which I am glad to see in Mr. Herbert Spencer's recently issued first number of his new edition of *Principles of Psychology*. There (part i, chap. i, p. 48) Mr. Spencer says, "It may safely be affirmed that Physiology, which is an interpretation of the physical processes which go on in organisms in terms known to natural science, ceases to be Physiology when it imports into its interpretations any psychical factor, a factor which no physical research whatever can disclose or identify, or get the remotest glimpse of." But, I apprehend, if the Physiologist wishes to become an Anthropologist, he must qualify himself to judge of both sets of factors. There is other science besides Physical Science, there are other data besides quantifiable data. Schleiden, a naturalist of the very first order, compares the Physical Philosopher (Materialismus der neueren deutschen Naturwissenschaft, p. 48), who is not content with ignoring, without also denying the existence of a science based on the consciousness, to a man who, on looking into his purse and finding no gold there, should not be content with saying, "I find no gold here," but should go further and say, "there is no such thing as gold either here or anywhere else." It is interesting to note that here in Oxford, till within a few years of the present, we narrowed the application of the word "Science" in what seems now to be a curiously perverted fashion. For, ignoring all the physical world as entirely as though we had been already disembodied, we used the word to denote and connote only Logic, Metaphysics, and Ethics. By a "student of science" in my undergraduate days was meant a student of the works of Aristotle, Kant, and Sir William Hamilton. The wheel has since made somewhat of a circle; our nomenclature, like much else belonging to us, is altering itself into a closer correspondence with the usages and needs of the larger world outside; the so-called "student of science" of the year 1850 is now said to go into the "School of Philosophy;" and "the student of science," as our terminology runs in the year 1868, will be found at the Museum studying the works of

Helmholtz, Miller, and Huxley. I do not say this by way of triumph, but rather in that of regret, little disposed or used though I am, and hope always to remain, to regret or deprecate change as such. For there is a philosophy of both subjects, and a science also in both; and I would hope that both the one and the other might still retain a lien on the two words and the two things, nor suffer its rival to establish a claim for sole possession by its own default in exercising a right of usage.

Advocates of the dignity of man are wont to regard, or to profess to regard, with something like horror, doctrines which would hint that either his bodily structures or his mental faculties, his "more pure and nobler part," may have attained their perfection in the way of gradational evolution. But it is not clear to me that the horror expressed for these conclusions is much more legitimate than the arguments with which they have been assailed by Prime Ministers and others in the Sheldonian Theatre close by and elsewhere within our precincts. For dignity rests upon responsibility—a man is worthy or unworthy, accordingly as he can or cannot make a good answer when called upon by a voice, either from within or without, to account for his conduct or for his character. And just as a man is responsible for the employment of the wealth he possesses to the Government under which he is suffered to enjoy that wealth, no matter in what way he may have become possessed of it, whether by the hereditary transmission of a family estate, or in any other of several feasible and conceivable ways, so is a man responsible for the employment of his corporeal and mental faculties, howsoever he may have been allowed to become seized of them, to that larger and largest Government under which he has his being. I believe, however, that, if men would take as much and the same care in these psychological questions as the physiologist does in his experiments and observations, to overlook none of the conditions and circumstances of the entire complex of phenomena upon which they undertake to decide, they would come to see that above, and often behind, but always beside and beyond the whirl of his emotions and the smoothly fitting and rapidly playing machinery of his ratiocinative and other mental faculties, there stands for each man a single undecomposable something —to wit, himself. This something lives in his consciousness, moves in his will, and knows that for the employment and working of the entire apparatus of feelings and reasoning it is individually and indivisibly responsible. Its utterances have but a still small voice, and the turmoil and noise of its own machinery may, even while working healthily, entirely mask and overwhelm them. But if we withdraw ourselves from time to time out of the smoke and tarnish of the furnace, we can hear plainly enough that, howsoever the engine may have come together, and into its present being, the engineer, at all events, is no result of any processes of accretion and agglomeration. Science, business, and pleasure are but correlatives of the machinery in its different applications and activities; *we* are something besides all this, manifesting ourselves to others in the decisions of our will, and manifesting ourselves to ourselves in our aspirations and consciousness of responsibility.

"And e'en as these are well and firmly fixed,
In dignity of being we ascend."

I have heard this line or argumentation likened to an attempt to defend Sebastopol by balloons. "Whilst you are in the clouds, your city will be taken beneath your feet." But a position, though airy, may yet be impregnable. There are those present who will recollect how the highest placed forts of that town were never taken, but continued to the last to answer shot for shot, and shell for shell, to the Allies. The attacking forces knew not the strength of these north forts till they entered them, but when they entered them, they entered them as friends.