[The following Oration had been prepared by Mr. Hulke, but before the day of delivery he was seized with the illness which proved fatal on February 19th. The Oration was read by Mr. Bryant.]

THE HUNTERIAN ORATION

ON

JOHN HUNTER THE BIOLOGIST.

At the Royal College of Surgeons of England, February 14th, 1895.

By J. W. HULKE, F.R.S., F.R.C.S.,

President of the College.

MR. VICE-PRESIDENT, VISITORS, FELLOWS, AND MEMBERS OF THE ROYAL COLLEGE OF SURGEONS OF ENGLAND.—We meet to-day to commemorate the 166th anniversary of the birth of John Hunter, that remarkable man whose name in this College is as a living presence, who did so much and with such great success last century to raise surgery from the lower grade of an empirical handicraft, which it then too greatly resembled, to the dignity of a branch of science by basing the principles that should guide its practice on the combined foundation of anatomy, physiology, and pathology.

His great achievements as a surgeon, his life-history, and his personality have been so frequently dwelt on here, that I may pass these by, and at once take up the theme on which I would speak to you; its subject is John Hunter the Biologist, the sagacious investigator and interpreter of "organic Nature." Of John Hunter in this character I can, however, offer you only a very incomplete sketch, since even if the time at my disposal permitted, and had I the necessary ability for such a task—a gift I may not claim—the materials for a complete presentment of him as a biologist do not now exist, for ten large bound volumes of manuscript written, mostly by his assistants at his dictation, and then revised, added to, and corrected by himself, embodying the records of the labours of many years, purchased, together with his museum, by the nation, which made our College their custodian, were, about thirty years after John Hunter's death, designedly burned by his brother-in-law and executor, Sir Everard Home. When interrogated respecting this act, he alleged that he had destroyed these MSS. by John Hunter's expressed desire, as being by him considered too imperfect for the public eye. That these ten volumes of MSS. were included under the words "collections and everything belonging thereto," which John Hunter in his last will directed should be offered to the Government "in one lot," is not open to doubt; yet Sir Everard Home, shortly before the transfer of Hunter's collections from Leicester Square to Lincoln's Inn Fields, and therefore after their purchase by the Government, had them removed to his own house. The Board of Curators of the College Museum appear not to have known that they had ever existed until after their destruction.

This irreparable loss looms the larger when we study John Hunter's anatomical and physiological collections, when we ponder on the fragments of his writings rescued from destruction by the solicitude of William Clift, his devoted assistant, our first conservator; and when we read his lectures and other papers collected and published by this College, and by Palmer, Ottery and R. Owen.

The better my knowledge of the extensive series of anatomical and physiological preparations—nearly all made by John Hunter's own hands, a series which may properly be regarded as the centre around which are clustered our own now very greatly extended and in some respects unsurpassed collections—the more profoundly am I impressed with the vastness of our great master's anatomical and physiological work, which, it should be remembered, was all accomplished within the relatively short space of thirty years, broken in

upon by frequent and severe illness and by the many interruptions incidental to the life of a busy practising surgeon: the better my acquaintance with the Hunterian series, the more am I impressed with John Hunter's sagacity, for in this series of preparations we have not the bald presentments of disconnected facts, but each preparation unfolds a tale, each conveys a lesson, each is a link in a chain, and not infrequently one clears up something which is obscurely represented or only hinted at by another.

The overshadowing greatness of his zoological work, perhaps, accounts for the imperfect recognition by so many of us of how much John Hunter also occupied himself in botanical research. In one of several physiological papers, after discussing the agreements and the differences between that which he terms "common or original matter and animate matter"—or, as we now should say, between inorganic and organic substances—and affirming the derivation of the latter from the former, for the reason that "animate is found to return to inanimate matter," he proceeds to analyse the resemblances and the differences of the matter of which animals and vegetables are composed. He restricts to vegetables the power of immediately converting common—that is, inorganic—matter "into their own kind;" and from this he draws the inference that "a vegetable seems an intermediate link between common and animal matter."

In his lectures on the *Principles of Surgery* he reviews the "accord" between the physiological endowments of vegetables and those of animals. He mentions that "a self-moving power has been observed and is universally allowed in vegetables;" and he adds that "this principle seems to be as much a property in vegetables as in animals." He illustrates internal mechanical work done within the vegetable tissues by reference to Hale's notable experiments on the rising of the sap in trees; and he contrasts the magnitude of the force employed in this movement with that exerted in the heart's systole.

Having defined irritability as the power of responding to stimuli by internal and external work, he calls attention to the visible movements of "whole parts of plants" as examples of this property. He adduces the movements of the leaves of the leguminous plant, the *Hedysarum gyrans*, as an example of this phenomenon; and he comments on an apparent analogy between these movements and those of respiration in animals suggested by their periodic repetition in both divisions of organic nature. Here he is, however, careful to avoid the error of attributing to these superficial resemblances an essential correspondence of function. With characteristic cautiousness he proceeds, "This (that is, the recurring leaf movements) is an action apparently similar to breathing in animals, though perhaps it does not answer the same purpose."

The circling movements of tendrils, as if seeking for a mechanical support, and their twining round this when they have come into contact with it, did not escape John Hunter's notice. Neither did he overlook the remarkable circumstance which characterises the twining of the stems of certain climbing plants—namely, its constant direction for each plant. He cites the honeysuckle (*Lonicera*) and the hop (*Humulus*) as climbers, of both of which he says, "their stems turn to the left, whereas the stem of *Clitoria* (a pea) and that of convolvulus turn to the right." He instances *Dionaea muscipula* (Venus's flytrap) and the *Mimosa pudica* (sensitive plant) as plants endowed with considerable powers of movement. He remarks that "the leaf of *Dionaea*, upon being touched, closes up, and as it were confines the stimulating cause;" that is, shuts it, and so entraps the insect which, in alighting on its upper surface, has touched the little cluster of extremely irritable hairs there projecting above the general level of the cuticle.

Of the *Mimosa pudica* he observes that it bends its leaflets in response to a coarse mechanical stimulus, and also to the subtle excitation of varying quantities of light incident upon them. In connection with excitation by light he mentions the goatsbeard (*Tragopogon*) and *Calendula pluvialis*, two plants in the large order Compositae, and he says that they and many other plants close their blossoms towards night or at the approach of rain. Of this habit an indigenous *Centaurea* (*Erythraea*), once of high medicinal repute, and the scarlet pimpernel (*Anagallis arvensis*) supply familiar examples. Then he passes on to tell us that some other plants,

as certain species of "convolvulus, open their flowers in the evening, and close them at the approach of the sun." In striking contrast to these dusk-loving plants, he mentions that nearly the entire class Diadelphia (now designated Leguminosæ), which comprises, he adds, "chiefly wing-leaved plants," close their leaflets "towards night, not expanding them till morning;" and he remarks that this phenomenon had been called by Linnæus the "sleep of plants."

This reference to the great Swedish botanist is interesting, because it proves John Hunter to have been acquainted with Petrus Brema's remarkable treatise bearing the Latin title *Somnus Plantarum*. It is included in the *Amœnitates Academicæ* of Linnæus, published at Stockholm in 1759. In this instructive memoir Brema asserts that "plants possess most qualities in common with animals; they feed, they have movement and rest, they have excretions, and they celebrate their nuptials." To Brema we are indebted for an anecdote of the circumstances which first brought to the great botanist's notice the phenomenon of the folding of the leaves of certain plants at night, before unrecognised. Linnæus, he tells us, had placed in the charge of an assistant a lotus (*Ornithopodioides*), and had enjoined him to take particular care of it. The lotus blossomed. Throughout the day its conspicuous blossom attracted notice, but in the evening, when the assistant visited the plant, the blossom, to his consternation, was not to be seen. The unhappy man, conceiving that the blossom might have been surreptitiously plucked by an evilly-disposed person, watched the lotus more closely than before. Next morning a blossom again appeared; in the evening it had again vanished. Perplexed, and unable to account for the singular occurrence, but convinced that the blossom had not been stolen, the assistant hastened to Linnæus, and told him what had happened. Linnæus at once went to the lotus, and on closely inspecting it he detected the vanished blossom still actually there, only it was closed and hidden from view, mantled by the green leaves wrapt about it. Attention once aroused, this phenomenon was quickly found to be common to many other plants. That it should have so long escaped recognition, and then owe its discovery to an accident, is but one of many instances that could readily be adduced to illustrate how easily circumstances for which we are not looking pass unnoticed, even though occurring daily under our very eyes.

John Hunter made the *Mimosa pudica* the subject of a study into which he threw himself with characteristic energy. He writes: "In order to have the greatest part of the day before me, I began my experiments at 8 in the morning, while the leaves were in full expansion, and I continued them till 4 in the afternoon, as longer would not have been just, for they begin to collapse of themselves between 5 and 6 o'clock." With his peculiar aptitude for planning an experiment, he contrived a small screen upon which he could trace and measure the arc through which a selected leaflet moved in response to a certain stimulus. In this way he found that "the leaflets are less affected as they become accustomed to the stimulus;"....."that they require a stronger and quicker stimulus to produce motion after being some time accustomed to it;".....and "that they erect themselves less after a repetition of such actions." Here the analogy to the corresponding occurrences in connection with excitation of animal tissues is very obvious. Searching for the mechanism concerned in the movement of the mimosa leaflets, John Hunter discovered that "the motion is principally confined to one part, and this differing from the others in external appearance, which difference is its increased thickness and uniformity of surface." Then he locates the motor mechanism in the swelling at the base of the petiole, and in the homologous parts of the stalks of the leaflets. Next he tells us that he slit longitudinally the swelling on the "foot stalk," and also that part of "the stem on which it stands," and he is about to record what he saw in these parts; but here unhappily, as Otley notices, there occurs a blank in the manuscript which leaves us ignorant of what he actually discerned in them. I do not gather from the context that John Hunter employed the compound microscope in this investigation. The contrary appears more probable, for the use of the microscope was then only dawning, and vegetable histology had made relatively little advance since Malpighi

began to cultivate it in the latter half of the preceding century. We at our standpoint can hardly conceive the possibility of Malpighi holding concurrently the Chair of Botany and that of Zootomy in his university—Bologna, yet in both these chairs he made discoveries which gained for him enduring fame. We *medici* are wont to think of Malpighi as an anatomist only, whose honoured name has come down to us chiefly in association with certain minute bodies in the kidneys and spleen, and with a certain layer in the skin, but botanists revere Malpighi as the founder of vegetable histology. For John Hunter it may fairly be claimed that he pushed his investigations into the motor mechanism of the mimosa leaflets as far as was then practicable with the means at his command.

Later investigators have demonstrated that when in a young, vigorous, succulent mimosa plant a cut is made with a sharp knife into the petiolar swelling, dividing its parenchyma down to the central strand of vascular tissue, a drop of water oozes from the wound, upon which follows the well-known movement of the leaf. In the absence of this effusion of water, no movement of the leaf occurs. Julius Sachs, who in recent years has done so much to advance the study of vegetable physiology, has further demonstrated that the visible leaf movement is caused by the afflux of water in the petiolar parenchyma, distending the tissue and thus causing it to become elongated more than the less extensible axile band of vessels. By such distension of the mass of parenchyma situated above the axile vascular bundle, the upper part of the petiolar swelling is lengthened disproportionately to the lower part, and the leaf of necessity bends down; whereas when the lower mass of the parenchyma is turgid with water, the opposite occurs, and the leaf erects itself. By the device of removing first the upper and then the lower mass of parenchyma, J. Sachs was able to demonstrate that only the latter mass—that lying below the axile vessels—is endowed with this irritability. It is now known that the movements of the mimosa leaflets are attended with the production of feeble electric currents. Such currents have also been demonstrated by Professor Burdon Sanderson to attend the movements of the leaf of *Dionaea muscipula*. Dr. Kunkel, working in Sachs's laboratory, has since then demonstrated that weak electric currents accompany the movement of water. In the vegetable tissues, however, this movement is originated, and thus the generation of such electric currents proves not to be peculiar to leaf movement excited by external stimulation.

Whilst experimenting on the mimosa, John Hunter observed that when he touched the leaflets the visible effect of the local stimulation spread to the neighbouring leaflets which he saw laid down in pairs until all the leaflets of the compound leaf were folded. He noticed also that this progressive effect of the stimulus spread from the point where it was applied more readily in the direction towards the stem of the plant than in the opposite direction towards the peripheral end of the leaf.

J. Sachs and with him some others appear disposed to regard the petiolar, axile, vascular bundle as the path along which the molecular disturbance initiated by the application of the stimulus travels; but whether this or the parenchymatous tissue is the path, it seems probable that a molecular disturbance in the living, active cell protoplasm is the efficient cause of the afflux of water that produces the leaf movement. The protoplasm of adjoining vegetable cells is now known to be continuous through minute openings in the cell walls, so that we are warranted in regarding the protoplasm in living vegetable tissues as a *continuum*; and thus the propagation of a molecular disturbance to considerable distances beyond the point of application of a mechanical stimulus originating it becomes readily intelligible. John Hunter's experiments on the mimosa were not limited to the effects of mechanical stimulation. He also experimented on this plant with heat, with chemical solutions, and with ether. He tried also the effect of a tight ligature placed around the stem or branch, and he found that when the part below the ligature was cut, very slight or no movement of the leaf occurred.

Continuing his discussion of the resemblances between animals and vegetables, Hunter remarks that "vegetables

are supposed, with great reason, to have an action analogous to breathing, for the same kind of air which kills animals which do breathe certainly kills vegetables also." He touches this subject so briefly that he leaves us in uncertainty whether he had himself experimentally investigated the influence of gases on plant life, or had merely adopted the conclusions of others. This latter appears to me more probable, when we bear in mind the standpoint of the chemistry of the gases in his day. Carbonic acid gas was discovered by Black in 1754, nitrogen by Priestley in 1772, oxygen also by Priestley in 1774, and hydrogen by Cavendish in 1776, the year of John Hunter's Croonian Lecture, from which I have just quoted.

We know (as Palmer has noticed) that John Hunter and Cavendish were personally acquainted, for Hunter himself tells us that Cavendish examined for him "air" contained in certain bladders present in the intestines of a hog sent to him by Jenner. There is, then, no improbability in the supposition that John Hunter may have derived from Cavendish his knowledge of the influence of gases on vegetable life.

However this may have been, botanists have long recognised as a general principle the necessity of the presence of oxygen for vegetable life; further, that vegetables take in free oxygen from the surrounding atmosphere; and also that they are able to seize upon oxygen when this is presented to them in weak chemical combination. Of this latter action the reduction of oxyhæmoglobin to hæmoglobin in the circulating blood by the bacillus of anthrax in animals dying of cattle plague has been thought a significant example.

In the absence of oxygen plants are asphyxiated, vegetable protoplasm loses its irritability, though less quickly than does animal protoplasm, because the processes of vegetable life are less actively carried on than are those of animal life. When a plant is deprived of oxygen, as when it is placed in an atmosphere from which the gas is absent, during a short period the want of the external supply of oxygen is in some measure compensated by the atmospheric oxygen previously enclosed in the air spaces of the vegetable tissues; perhaps also some oxygen is derived by the plant from the decomposition of weak chemical combinations of oxygen normally present in certain chemical substances contained in the tissues, but these limited sources of oxygen are soon exhausted.

To this general law of the necessity of oxygen for the maintenance of vegetable life certain low forms appear to offer notable exceptions. Thus the yeast plant (*Saccharomyces cerevisiæ*) can live and even increase in an atmosphere devoid of oxygen. Its highest life phase, however, requires the presence of oxygen, for spores are not produced unless the plant has access to the atmosphere. Then, also, there are certain Schizomycetes to which free oxygen seems to be positively hurtful; they die in its presence. The explanation of this singular phenomenon still awaits solution.

The final products of the oxygen taken into the tissues of the plant are carbon dioxide and water. Of these, the former is exhaled from every part of the plant's external surface. This, von Tighem says, is the most constant phenomenon of plant life; and thus in the matter of gas exchanges we find confirmed the impression mentioned by John Hunter as current in his day, namely, that a very close correspondence exists between vegetable and animal respiration.

That plants like animals have "the power within themselves of producing or generating heat" did not escape John Hunter's notice. He investigated also their power of resisting very low temperatures, employing in some experiments freezing mixtures, and noting the effects of these on succulent and on woody plants, and he found that the latter better resisted great cold. He also carried out a series of observations, prolonged over a year, on the internal temperature of trees relatively to that of the external atmosphere. He mentions that he "read his thermometers at six o'clock in the morning and again at the same hour in the evening;" and he says that he was obliged to discontinue these experiments because the sap froze in the holes bored in the trees treated for the reception of his thermometers. He records that he was careful to allow a sufficient interval to elapse between boring the holes and inserting the thermometers in order that the heat generated by the friction of the gimlet might be dissipated; and he tells us also that he enclosed in a box the part of the thermometers projecting externally beyond

the hole, and packed it in wool in order to protect it "against all immediate external influences either of heat or of cold."

Hunter also made a series of thermal experiments on vegetable seeds similar to others he had made on eggs, and he mentions his intention to record these. No trace of such record has come down to us, if actually made; it may, perhaps, have been amongst the MSS. burned by Sir Everard Home. John Hunter has left us a brief account of experiments in connection with the movement of the sap. We possess, also short statements of his views concerning the influence of light on the production of the "green colour" of vegetables, on the changes undergone by the leaf in dying, on the natural decay of the vegetable tissues, on the morphology of the bud, and on "germination and generation in vegetables."

I cannot now enlarge on his work in relation to these several subjects, nor is it necessary that I should do so, since the evidence adduced is more than sufficient to justify me in claiming for our great master that he was a very close observer, and an acute reasoner upon many of the phenomena comprised under vegetable physiology.

Leaving botany, John Hunter's title to a place in the foremost rank of original investigators in zoology, the other primary division of biology, is so universally acknowledged that more than a passing reference to his researches in the animal kingdom may seem unnecessary, particularly within these walls; yet on this occasion I may not dismiss them with a bare allusion. His memoirs on *The General Principles of the Blood*; on *The Vascular System*; on *Digestion, Absorption, and Nutrition*; and on *The Growth of Bones*, even at the standpoint we have reached, require attentive study. They show how far he was in advance of his contemporaries.

John Hunter's devotion to physiology, which had its root in his conviction of the necessity of this "discipline" for the intelligent practice of surgery, was made a reproach by empirical surgical contemporaries, who called him a theorist and not a practical surgeon; indeed, the tardy recognition of John Hunter as the leading surgeon in this metropolis was probably in no small degree due to this misconception of him. This comment has also been made by others. His memoir on *The General Principles of the Blood* is certainly one of the most important written by him. From it we learn how much patient investigation, how much concentrated thought he bestowed on the striking phenomenon of its coagulation. "This," he remarks, "is not a property of the blood as a whole, but only of one of its component parts—the coagulable lymph." Then with nice discernment he adds: "This would better be termed 'coagulating lymph,' since blood serum also contains a coagulable substance which, however, needs the addition of a chemical agent for its change from a liquid to a solid state." John Hunter considered "coagulable lymph"—fibrin, as we term it—to be the most important constituent of the blood chiefly because he found it universally present in it. He sought to ascertain the influence of temperature on the coagulation of fibrin, and he established that this latter is retarded by cold and hastened by heat.

The effect of rest and of motion on coagulation of fibrin also occupied his attention, with the result that he was led to regard rest as an important direct factor but not its immediate cause.

The final outcome of all his experiments and observations on the blood was the view of its coagulation, which I state in his own words: "That the fluid state of the blood is connected with the living vessels, which are its natural situation, and with motion; and that where there is a full power of life the vessels are capable of keeping the blood in a fluid state." These views appear to harmonise closely with those of the present day.

The white corpuscles of the blood appear to have been unknown to John Hunter, which will not surprise us if we bear in mind the imperfection of the compound microscope in his day, and also the entire absence of our numerous aids, of which I need only instance the use of differential staining agents, by which so many delicate details of minute structure have been disclosed.

How narrowly John Hunter scrutinised every unusual circumstance that came before him is shown by his remarks on Liphæmia. He writes: "The serum of the blood is often wheyish, and then upon settling it often throws up a whitish

scum like cream."....."This was most probably first observed in human blood, but it is not peculiar to it." He had noticed it "more frequently in the blood of breeding women, but he had seen it in others, and sometimes in man." Examined with the microscope, he found this serum to be composed of "globular particles which are not soluble in water, and which rise to the top." This pathological condition has in recent years attracted much attention.¹

Numerous and varied experiments were made by John Hunter to determine the cause of the different colour of venous and of arterial blood. He noticed the influence of respiration on the colour, and to the objection advanced by some "that in the lungs the blood cannot come into contact with the air," he opposed the familiar fact that the bright red colour assumed by the outer surface of a bloodclot when exposed to the atmosphere "extends some depth into the clot," whence "it is evident that air can and does penetrate animal matter."

Passing to the "vascular system" we find that John Hunter notices the predominance of muscular over elastic tissue in the coats of the smaller blood vessels; and then he comments on their respective influences on the calibre of the vessels. He also notices the branching and the anastomosis of arteries, and he discusses the effects of these arrangements on the velocity of the blood current. Then he investigates the pumping force of the heart in relation to the resistance offered by the arterioles; the relative capacities of the venous and arterial systems; the retardation of the blood current in the veins; and the form, structure, and distribution of the valves in the last named vessels.

John Hunter's observations on *Digestion and Nutrition*, though much less extensive, are scarcely less instructive than those I have noticed. I shall cite one only, namely, that on "The Digestion of the Stomach by its own Juices after Death," occasioning appearances that had previously been regarded as pathological, and respecting the real nature of which much acute difference of opinion continued long subsequently to exist, notwithstanding the thoroughness of John Hunter's exposition.

In a communication to the Royal Society, made at the instigation of its President, Sir John Pringle (read on June 18th, 1772), John Hunter insists on the prime importance of a correct knowledge of the appearances produced in the tissues of the body by those changes which they naturally undergo in persons dying suddenly, as from fatal violence inflicted on them when in perfect health. He significantly remarks that, in the absence of this knowledge, appearances, collectively products of putrefaction, may easily be mistaken for others the results of disease, pathological in their nature and occurring during life; and thus confusion and misapprehension will arise. He goes on to state that "there is a case of mixed nature which cannot be reckoned a process of the living body, nor of the dead; it participates of both, inasmuch as its cause arises from the living, yet it cannot take effect till after death." He adduces the suggested fact that "animals or parts of animals possessed of the living principle, when taken into the stomach, are not affected by the powers of that viscus so long as the living principle remains. Thence it is," he adds, "that we find animals of various kinds living in the stomach or hatched or bred there; but the moment that any of these lose the living principle they become subject to the digestive powers of the stomach."

At a loss to explain these appearances, John Hunter had supposed them to have been produced during life, and to have been the cause of death; but the absence of associated symptoms, and their frequency in persons who, being in good health, had died violent deaths, occasioned him perplexity and made him, as he says, "suspect that the cause was not even imagined." He tells us that the first time he had observed this "appearance" was under circumstances that precluded its causation by disease—the man had just before his death made a hearty supper of cold meat, cheese, bread, and ale. On opening his body a large hole was found in the stomach, through which part of the ingesta had escaped into the general cavity of the belly. Doubtful as to what this might mean, John Hunter made "many experiments on digestion, on different animals, all of which were killed at different times after being fed with different kinds of food; some were not opened immediately after death, and

in some of them (he says) I found the appearance thus described in the stomach."

This memoir is worthy of study if only as an illustration of John Hunter's method of work; whenever puzzled by anything of which the explanation did not immediately present itself, he turned to experimentation for its solution.

These very incomplete references to some of his more important physiological researches prove that as an original investigator in this branch of biology John Hunter was in line with the foremost workers of his day.

How great an anatomist he was is evidenced by his published papers; by the great value in which his lectures delivered in the Windmill Street rooms were held by those who made the effort to understand them, and is told yet more eloquently by his preparations on the shelves of our museum. In anatomy, as I have already said, John Hunter was not a mere accumulator of facts, not a mere describer of figure, colour, and relative position of the organs and members of the animal body, but he sought for the explanation of these. He tried to import into the art of anatomy the character of a science. He was ever seeking the how, the why, and the wherefore of the facts disclosed by his scalpel, ever reasoning inductively from particular instances, and ever trying to deduce general laws.

But John Hunter was not only a distinguished zootomist, anthropotomist and physiologist; he also assiduously prosecuted researches in embryology previously little studied, and he reaped in it a rich harvest. How keenly, how penetratingly he observed, and how sagaciously he interpreted what he saw is apparent in the article he wrote on "The Development of the Chick." In order to secure a supply of eggs for this research he kept large numbers of fowls and also a flock of geese during several years.

John Hunter's labours were not limited to ontogeny, the development of the individual; he pressed onwards to the study of phylogeny, the evolution of the "stem." He writes, "We may observe that in natural things nothing stands alone; that everything in Nature has a relation to a connection with some other natural production or productions; and that each is composed of parts common to most others, but differently arranged. Therefore in every natural production there is an appearance of an affinity in some parts of its composition with those of some other natural production, and where there are the greatest number of these affinities, or corresponding parts, the correspondence or affinity between those of one production with those of another, the nearer are those (natural productions) allied."

In another passage, after premising with characteristic vigour of language that "definitions are the most damnable things," he defines species as "things having the same relationship in their most essential properties, however much they differ in others." "So," he continues, "animals breeding in the full extent of that process constitute the species, although this may differ in some of their parts or other circumstances, but which differences are less essential, only constituting a variety."

He comments on the greater tendency of domestic species to variation than obtains in wild animals, and he illustrates this difference by the many diverse breeds of dogs and the few distinct races of wolves. He attributes this difference to the existence of domestic animals under other than their natural circumstances—in short, he recognises the plastic influence of environment. He perceives "in a great many species a considerable variety in the same, and from variety in this same species it becomes a doubt whether they were all original or none of them, or, if any be original, which that one is."

He alludes to the variability of species in more than one passage, and he makes the significant comment that some variations are transmissible to offspring. He tells us that "it may certainly be laid down as one of the laws of Nature for species to deviate from their type under certain circumstances;" and, he adds, "it is neither necessary, nor does it follow that all deviations from the original must be a falling off; it appears just the contrary, therefore we may suppose that Nature is improving her works, or, at least, has established the principles of improvement in the body as well as in the mind."

In these passages—laboured and somewhat deficient in per-

¹ See *Trans. Path. Soc. Lond.*, vol. xxxviii, 1883.

fect clearness of expression—we find John Hunter enunciating the principles of the “inherent variability of species;” of the “modifying influences of environment;” of the transmissibility of variations from parent to offspring; and of evolution from lower to higher life forms—in short, in those of his memoirs which we possess there is to be found abundant evidence that his mind was often and deeply engaged in the consideration of the pregnant questions comprised in the idea of evolution, around which so much and such fierce controversy has been waged in our own day.

The significance of past forms of life did not escape him; he studied their fossil remains, of which he collected a considerable number. In a memoir communicated by him to the Royal Society on a series of fossil bones from caves at Gailenreuth presented to the Society by His Serene Highness the Margrave of Anspach, we find John Hunter investigating the circumstances of their fossilisation, comparing the forms of these bones with those of extant animals, reviewing the geographical distribution of animals in past time, and speculating from the *gisements* of fossils upon the form, etc., of the earth's surface in past ages.

From these and similar considerations John Hunter inferred a duration of our earth prolonged through “many thousand centuries.” This chronology was so greatly at variance with that then universally accepted that a second memoir sent in by him to the Royal Society, in which it was stated, caused so much misgiving in its Council as led to a suggestion being conveyed to him that he should substitute years for centuries. With characteristic adherence to his convictions, John Hunter would not modify his original statement, and he withdrew the paper.

John Hunter's researches were not limited by the walls of the dissecting room, museum, and study; outside these he was a close observer of living Nature. He was fully alive to the great value of both these lines of work. In some fragmentary notes on *Natural History*, edited by R. Owen, he remarks: “Writers on the natural history of animals have been of two kinds—one (concerned) only in what they could observe externally, such as form and mode of life; the second (studying) only the internal parts, and the structure of the whole animal which was performed by the anatomist. As (the subject of) the first has an immediate connection with (that of) the second, the describers of form conjectured what the structure ought to be by consulting the anatomist; and the anatomist conjectured what the living history is, or ought to be, from the natural history of others, filling up what he conceived to be just, and fancy supplying the rest. But such union of knowledge does not properly match. It is one building built at different times, an addition to an original plan. It is no wonder therefore that the whole is imperfect.” Can we pronounce all later anatomists and writers on natural history free from this reproach?

John Hunter's remarkable memoirs on the life-history of the bee testify to his excellence as a naturalist. We find that for the convenience of close observation without molesting his bees he had hives constructed with glass windows which allowed him at all times to watch their occupants. He inquires into the causes of the death of certain bees in winter. He mentions that “there was plenty of honey in their hives; that on closely examining the dead bees he found they all died with their proboscides extended; their stomachs were full of honey, and their intestines, especially the last part, also full of excrement.” No circumstance, however minute, eluded his notice! Then we find him making observations on the heat of bees. Without warmth, he observes, they became dull, inactive, and torpid. He tells us that “on July 18th at 10 P.M., wind northerly, thermometer at 54° F. in the open air, I introduced it into the top of a hive full of bees, and in less than five minutes it rose to 82° F. I let it stand all night; at 5 o'clock in the morning it was down to 79°; at 9 on the same morning it had risen to 83°, and at 1 P.M. to 84°; and at 9 in the evening it was down to 78°. December 30th, air at 35°, bees at 73°.”

John Hunter also made the discovery that “the wax is not gathered by the bees from flowers as is ‘farina’ (pollen), but it is formed by the bees themselves.” “It may,” he says, “be called an external secretion of oil.” “It is formed between each scale on the under surface of the belly.” He detaches the minute flake, warms it on the point of a needle at the

flame of a candle, sees it melt and run into a bead, and then burn in the manner of wax—in short, it is wax. He describes the building of the comb and of the royal cell; the deposition of eggs by the queen, their attachment to the bottom of the cell, and their occasional transference to other cells from those in which they were originally placed; the storing of bee bread for feeding the grubs; the development of the grub, its pupal phase, and its final escape out of the cell as the imago. He notes the different life forms present in the community—the queen, the males, and “the working bee, which cannot be called either sex.” Finally he describes the anatomy of the bee, and comments on its special senses. He investigated with similar care the life-history of wasps and hornets.

We find him occupying himself, also, with the economy and anatomy of the humble bee (*Bombus terrestris*), on which subjects he has left quite a long note. With unflinching industry he examined all the occupants of a nest, and found them to comprise 157 females and 25 males; and he noted that the former had longer proboscides than the latter. He observed that the humble bee does not colonise as does the honey bee—it does not swarm, the queen does not leave the hive accompanied by a large train of followers; but “the family is begun by a single female, later assisted by her offspring.” None but young queens live through the winter. On its approach they leave their nest, and seek winter quarters in holes in dry banks and similar places, from which they emerge in the spring. Humble bees construct their hives or nests generally underground. These he describes in some detail. Then he notices the deposition of the eggs by the queen, the grubs which emerge from them, and the imago or perfect insect.

The habits of the black humble bee (*Anthrophora retusa*) and those of the leaf-celled bee (*Megachile centuncularis*) also occupy his notice; the custom these bees have of cutting pieces out of the leaves of the rose, strawberry, or dogwood, and the neatness with which they adapt these in the construction of their cells.

His attention was not limited to Hymenoptera, for every living creature that frequented his garden or that he met with in the country attracted him. Thus he has left us notes on three familiar beetles. Of the dung-beetle (*Geotrupis stercorarius*) he records that in June he found the grubs nearly ready to assume the pupal phase; and that the perfect beetle escaped from the chrysalis at the end of July or the beginning of August. He found the grubs in nests at the bottom of holes 12 to 18 inches below the surface of the ground, and usually near cow dung. He treats at some length of the common cockchafer (*Malveontha vulgaris*), giving its life-history and anatomy, and with less detail he notices the rose beetle (*Cetonia aurata*).

It was scarcely possible for him not to be interested by two common Orthoptera, and so we find him noting down the appearance and the structure of the grasshopper (*Acrida viridissima*), describing particularly its eye and speculating on its vision. He detects the predatory habits of the dragon fly (*Estrus grandis*), and records his discovery in the following very characteristic note:

“August 18th, 1778, at 8 in the evening, I saw the dragon fly flying about, making short turns, which were performed very quick. I also observed gnats flying, and what took my attention most was his making up to a gnat, and the gnat was seen no more, therefore I conjectured he was feeding on them. I caught him and opened him next morning, and could observe in the stomach the scales of some insects.”

What a picture this little anecdote gives of the acuteness of John Hunter as a field naturalist!

As a zoologist and morphologist, John Hunter could not be satisfied with the highly artificial zoological classifications of his time. He remarks that the want of an adequate knowledge of these preliminary and indispensable studies had led even the great classifier Linnaeus into some very singular arrangements in the earlier editions of his *Systema Naturæ*, of which he mentions one, namely, the placing together in one order of Mammalia, mouse, the elephant, and the bat, because in each of these the mammae are pectorally situated. Such classifying as this John Hunter caustically observes may be pertinent in respect of breasts, but not as regards animals. He did not stop at showing the defects of this

then current systems of classification, but he suggested as basis for a natural classification the arrangement of the vascular, the respiratory, and the nervous system; and he tentatively drew out the scheme of a natural classification founded on a combination of what he termed essential and circumstantial characters. Thus of the order Mammalia he gives as essential characters "a four-chambered heart; lungs confined to a proper chamber, the enlargement of which is the cause of respiration; lungs divided into small cells; respiration quick; viviparous, etc.; whilst circumstantial characters are found in the structure of the auditory organ." This illustration will suffice to prove how sound and how advanced were John Hunter's views as a systematic zoologist.

If in this sketch of John Hunter—imperfect, incomplete as I know it to be—I have in some small degree succeeded in presenting to you our great master as one of the most indefatigable workers, one of the most earnest seekers after truth, one of the very closest of skilled observers, one of the most sagacious expositors of the facts of vegetable and animal life, I shall not, I trust, have altogether failed in the design I aspired to place before you on this commemorative day—a presentment of John Hunter as a biologist in the widest and truest sense of this now much used word. For this I have made large use of John Hunter's own words, because these best show his mind, and "the mind is the man."

TWO CASES OF ENTERECTOMY IN WHICH MURPHY'S BUTTON WAS USED.

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In the two following cases Murphy's button was used as a means of end-to-end approximation of the intestine.

A. M., an unmarried female, aged 30, had enjoyed very good health until about eighteen months before her admission into the Liverpool Royal Infirmary. She then began to suffer from attacks of constipation, which were relieved by purgatives and dieting. During the intervals between these attacks she was quite well. As time went on the attacks became more frequent and more severe, each succeeding one lasting a longer time and being more difficult to get over. Moreover a certain amount of pain and discomfort began to be experienced in the region of the cæcum. She lost flesh, and, during the attacks, there was not only constipation but vomiting. At last complete obstruction came on. As her life was in serious danger, she was transported from North Wales, where she lived, to Liverpool. When she arrived she was in an extremely emaciated condition and greatly exhausted. The obstruction had been complete for a fortnight; nevertheless, the abdomen was not much distended, because the patient had been fed on the smallest possible amount of easily absorbable liquid food, and had been kept free from pain by opium. Being so thin as she was the action of the small intestine could be plainly seen through the abdominal wall struggling to force a passage through some obstruction, but where that obstruction was could not be ascertained. The only guide was the patient's statement that most of her pain and uneasiness were felt in the region of the cæcum, but nowhere could any tumour or swelling be felt. As she was obviously nearing her end unless relief were given, the abdomen was opened in the middle line so as to permit of an inspection of all parts of the cavity. Presently distended and hypertrophied small gut was found, which, being followed down to the cæcum, led to the cause of obstruction. This was a thickened and strictured condition occupying about two inches of the bowel, and intensely hard to the touch. At the moment its hardness induced us to believe it malignant, but subsequently microscopical examination showed it to be mostly fibrous. This could not have made any difference in the treatment, for nothing short of removal would have been of any avail.

The difficulties in connection with it were two: (1) That the ileum on the proximal side of the stricture, by reason of its severe and protracted exertion, had become enormously hypertrophied and increased in calibre, so that it was quite as large as great intestine, while the ileum between the

stricture and the ileo-cæcal valve was atrophied, and not larger than the small bowel of a child. (2) That the last-mentioned piece of bowel was extremely short, being only about three-quarters of an inch in length, and when the stricture was cut out became even shorter by retraction of its muscular coat. It is difficult to imagine more difficult circumstances under which to attempt union of intestine. As something, however, had to be done, I rapidly removed the stricture, and proceeded to fit the Murphy's button. The difficulty with the proximal end was that it was far too big for the upper half of the button, and had to be very much crumpled up; while, on the other hand, the distal end was so short and small that I could not get enough to cover the button until I had divided one flap of the ileo-cæcal valve and thrust it into the cæcum. All this had to be done in such a hurry, owing to the weak condition of the patient, that it was done in a manner which I felt was unsatisfactory and imperfect, but there was no help for it, and we were glad to get her back to bed alive. She lived for thirty hours, but never properly rallied. At the *post-mortem* examination it was found that semi-fluid fæces had passed abundantly through the button, so that there was no longer any obstruction; also that the bowel was well united and soundly glued together, except at one small point, and at that point a little fæcal extravasation had taken place. Commenting upon the case one would say that the real cause of failure was the exhausted condition of the patient, so great that she would have died under all circumstances, whether the button had held completely tight or no. The only other method I could have employed would have been to have united by an end-to-side approximation the proximal end of the ileum with the ascending colon, closing up the short end of ileum which led into the cæcum. Theoretically this would have been the best thing to do. Practically the condition of the patient did not permit of more than was done, and that was done in such a hurry that it was done imperfectly. Nevertheless, the untoward result of the case did not prejudice me against the use of the Murphy's button. On the contrary, I at once recognised in it such valuable qualities that I resolved to use it the very next chance I had. This occurred within a few weeks, the facts of the case being briefly thus.

On Saturday evening, December 29th, 1894, I was asked by my friend, Dr. Holmes, of Anfield, to see at his own home, A. H., a schoolboy, 7 years of age. His mother told us that he had for a long time been in the habit of bolting his food. For some weeks before his present attack he had gone as usual to the watercloset after his hastily-swallowed breakfast, but had come down complaining of griping pains in his belly, which passed off after a little while. On Sunday, December 23rd (six days before I saw him), he had been taken with pains all over him, and remained in the house. Next day (Monday) he lay on the sofa, "generally ill all over." In the evening he had a dose of castor oil, which was said to have operated twice in the night. From that time onwards he had complete obstruction. On Tuesday he had frightful abdominal pains, and vomited everything, and this continued all Wednesday and Thursday. On Thursday evening he had a simple enema, and on its return it was accompanied by the passage of a considerable quantity of blood. On Friday and Saturday he was comparatively free from pain, and lay in a quiet, dreamy state. The abdomen was not much distended, but all that could be felt was that there was a feeling of fulness to the right of the umbilicus. A diagnosis was made of intussusception, chiefly owing to the passage of blood. It was agreed that if no relief came during the night the child should be brought to the Royal Infirmary next day (Sunday, December 30th). This being done the abdomen was opened in the middle line, and an intussusception of the ileum, situated about 18 inches from the cæcum, came readily up to the incision. The bowel was black and gangrenous at the site of constriction, and for some distance downwards from it. Points were selected quite free of the mischief, the bowel divided at them, and its ends approximated by a Murphy's button. The abdominal wound was completely closed. The boy soon began to get over his shock. On the nineteenth day the button was passed, and on the thirty-eighth day he was sent home. Here a correct diagnosis was made. One was prepared for what was coming, and the operation was done with the greatest ease and rapidity.