before you to-night is not likely to escape the fate which, upon many occasions within my recollection, has attended the statements of scientific doctrine and of scientific tendency, which have been made in a spirit intended at any rate to be as calm and as judicial as that in which I have now laid these facts before you. I do not doubt that that fate will befall me which has befallen better men, and that I shall have to bear in patience the reiterated assertion that doctrines such as I have put before you have very evil tendencies. I should not wonder if you were to be told by persons speaking with authority—not, perhaps, with that authority which is based upon knowledge and wisdom, but still with authority—I should not wonder if you were told that my intention in bringing this subject before you is to lead you to apply the doctrine I have stated to man as well as to brutes, and it will then certainly be further stated that the logical tendency of such a doctrine is Fatalism, Materialism, and Atheism.

Now, let me ask you to listen to another product of that long experience to which I referred. The logical consequences are very important; but, in the course of my experience, I have found that they were the scarecrows of fools and the beacons of wise men. Logical consequences can take care of themselves. The only question for any man to ask is this: "Is this doctrine true, or is it false?" No other question can possibly be taken into consideration until that one is settled. And, as I have said, the logical consequences of doctrines can only serve as a warning to wise men to ponder well whether the doctrine be true or not, and to test it in every possible direction. Undoubtedly, I do hold that the view I have taken of the relations between the physical and mental faculties of brutes applies in its fullness and entirety to man; and if it were true that the logical consequences of that belief must land me in all these terrible things, I should not hesitate in allowing myself to be so landed. I should conceive that if I refused, I should have done the greatest and most abominable violence to everything which is deepest in my moral nature. But now I beg leave to say that, in my conviction, there is no such logical connection, as is pretended, between the doctrine I accept and the consequences which people profess to draw from it. Many years ago, I had occasion, in dealing with the philosophy of Descartes, and some other matters, to state my conviction pretty fully on those subjects; and, although I know by experience how futile it is to endeavour to escape from those nicknames which many people mistake for argument, yet, if those who care to investigate these matters in a spirit of candour and justice will look into those writings of mine, they will see my reasons for not imagining that such conclusions can be drawn from such premisses. To those who do not look into these matters with candour and a desire to know the truth, I have nothing whatever to say, except to warn them, on their own behalf, what they do; for assuredly if, for preaching such doctrine as I have preached to you to night, I am cited before the bar of public opinion, I shall not stand there alone. On my one hand I shall have, among theologians, St. Augustine, John Calvin, and a man whose name should be well known to the Presbyterians of Ulster, Jonathan Edwards, unless, indeed, it be the fashion to neglect the study of the great masters of divinity, as many other great studies are neglected nowadays; and I should have upon my other hand, among the philosophers, Leibnitz; I should have Père Malebranche, who saw all things in God; I should have David Hartley, the theologian as well as philosopher; I should have Charles Bonnet, the eminent naturalist, and one of the most zealous defenders Christianity has ever had. I think I should have, within easy reach at any rate, John Locke. Certainly, the school of Descartes would be there, if not their master; and I am inclined to think, in due justice, a citation would have to be served upon Emmanuel Kant himself. In such society it may be better to be a prisoner than a judge; but I would ask those who are likely to be influenced by the din and clamour which are raised about these questions, whether they are more likely to be right in assuming that those great men I have mentioned—the fathers of the Church and the fathers of philosophy—knew what they were about, or that the pigmies who raise this din know better than they did what they meant? It is not necessary for any man to occupy himself with problems of this kind unless he so choose. Life is full enough, filled amply to the brim, by the performance of its ordinary duties; but let me warn you, let me beg you, to believe that if a man elect to give a judgment upon these great questions—still more, if he assume to himself the responsibility of attaching praise or blame to his fellow-men for the judgments which they may venture to express—I say that, unless he would commit a sin more grievous than most of the breaches of the Decalogue, let him avoid a lazy reliance upon the information which is gathered by prejudice and filtered through passion. Let him go to these great sources that are open to him as to every one, and to no man more open than to an Englishman; let him go back to the facts of nature, and to the thoughts of those wise men who, for generations past, have been the interpreters of nature.

AN ADDRESS

ON

RECENT ADVANCES IN ANATOMY AND PHYSIOLOGY.

Delivered in the Section of Biology at the Annual Meeting of the British Association for the Advancement of Science, 1874.

By PETER REDFERN, M.D.,

Professor of Anatomy and Physiology in Queen's College, Belfast.

GENTLEMEN,—It has occurred to me that, in attempting to give a stronger impulse and a more systematic direction to scientific inquiry, the time ordinarily devoted to an Introductory Address could not be more profitably occupied than by bringing into as great prominence as possible some of the great revolutions in our knowledge of anatomy and physiology which have taken place in my own time, and under my own immediate observation. I remember, as if it were yesterday, the elucidation in the Museum of the Royal College of Surgeons of Edinburgh of the newly discovered cell-theory, by the late distinguished professor of anatomy, John Goodsir; his account of the production of ulceration by cell-growth, of the character of the corpuscles of bone, of the structure of lymphatic glands, and of the germinal centres of basement membranes, as they were then understood.

This was the time when the teaching of histology was first established in Great Britain. Two students, of whom I was one, formed the first class, under the most enthusiastic of teachers, my old friend Dr. Hughes Bennett. The University of Edinburgh had just passed through what was probably the most brilliant period in its history. The race of the last of the Monroes was well-nigh run. The great discoverer of the difference in the motor and sensory nerves, Sir Charles Bell, was still living. The aristocracy of Scotland had only just ceased to crowd the class-room and witness the brilliant and successful experiments of Dr. Hope. The day of Cullen, and Home, and Duncan, and Macintosh was over; but there still remained in the university the most loved and revered of teachers, the benevolent Dr. Alison, Sir Robert Christison, Sir George Ballingall, Mr. Syme, and Dr. Abercrombie, still practising in the city.

At this period, the great discoveries of Schleiden and Schwann seemed likely to upset all that had previously constituted physiology. The idea that all tissues were either composed of cells or had been formed of cells, that nucleated cells elaborated all the secretions, that their energy lay at the root of the foundation, the reproduction, and the function of every tissue and organ, was a revelation of such astounding simplicity as will upset men's minds, and prevent their seeing beyond. No one who did not live through that time will, I believe, ever realise the eagerness and anxiety with which every new statement of the action of cells was received and added to the previous knowledge of their amazing power; or, on the other hand, be able to judge of the feeling, half akin to disappointment, which was experienced as each attack was made on this charming thing, showing it to be really human—very human indeed. Cells were then understood to constitute the mass of all organs, the liver, spleen, kidney, and brain, and to be the main agents in the discharge of their functions: to exist and grow upon the definite membranous walls of the glandular vesicles and ducts; to be fed by blood brought to the attached surface of membranes, which seemed almost everywhere to form an absolute separation of the cellular part, the potential gland, from the non-essential blood and lymph-vessels, the nerves and framework of the organ.

It seemed almost a pity that the little microscopic deities should be hampered by the necessities of their own existence; that they should need such base things as blood-vessels, nerves, and packing materials. Now, how strangely are matters changed! What if it should turn out that these apparently independent little beings are not independent at all; that they are only the dilated endings of nerves? To this subject, I shall refer again by and by.

This great cell-theory has now given place to what, I think, is certain knowledge: that living matter may more than perform all the functions of assimilation and intrusion, and reproduce its like, without having any of the essential characters of a cell. A living mass of protoplasm may change its shape, alter its position, feed and nourish itself, and form other matter having the same properties as it has, and yet be perfectly devoid of any structure recognisable by the highest powers of the microscope.

Mr. Lister showed that the construction of pigment-cells in the skin changes the position of the pigment-granules, driving them alternately into the processes and the body of the cell. Kühne, Golubew, and Stricker observed changes of form in *amœbæ*, white blood-corpuscles, and embryonal capillaries respectively, after the application of electrical stimuli; and Brücke observed contraction in the pigment-cells of the skin of the chameleon after excitation of the sensory nerves, whilst Kühne noticed contraction in the corneal cells after excitation of the corneal nerves. Thus obvious movements in fixed cells, or masses of protoplasm, are proved to result from the operation of various stimuli, including nervous stimuli.

But all cells are not fixed. The blood-cells, fixed as cells of organs at an early period, became free in the blood-fluid, and are hurried along by the forces which circulate it, until a second time they enter into the composition of the solid tissues by penetrating the walls of the blood-vessels, and moving along the substance of the tissues for purposes which are not yet wholly explicable. What naturalist will not at once suggest how frequently this process of alternate friction and movement of animal forms occurs low down in the scale? And yet how startling is it in man! how impossible to reconcile with our former ideas of the existence of membranous coverings of cells, of surfaces, and of gland-ducts!

But, with or without explanation, the facts must be recognised; the floating blood-cells are really the very cells which once formed the substance of the lymphatic glands, the spleen, and other organs; and they do, in fact, move through the walls of the blood-passages, and wander about freely in what are called solid tissues.

Our knowledge of this circulating fluid has marvellously increased. The duration of the life of any of its particles is but short; they die, and their places are occupied by others, as was the case with our forefathers and will be the case with ourselves. It is now a matter of observation, which commenced with Hirt of Zittau, that, after every meal, an amazing number of white corpuscles is added to the blood; breakfast doubles their number in proportion to the coloured corpuscles in half an hour; supper increases their proportion three times; and dinner makes it four times as great. They come from such solid glands as the spleen. In the blood going to the spleen, their proportion is 1 to 2.26; in that returning from the spleen it is 1 to 1.60. Every organ and every tissue changes this fluid; and, to my mind, perhaps the most stupendous miracle of organisation is the maintenance of but slightly variable characters in the living and moving blood, which is every moment undergoing changes of different kinds as it circulates through each tissue and organ in the body.

Yet, with all this change, there is an invariable transmission of the parental characters by continual descent from particle to particle as each takes the place of a former one, and thus each organ continues to discharge the same function from year to year. Animals of the same kind retain the old number of organs, the same shape of body, and similar modes of life. There is no sign of commencing life, no coming of new vital power, no production of life, out of living matter. The original life extends its limits; it operates in a more extended sphere; but it is the same life; it operates in the same way; it never fails to be recognisable in the individual by the same characters as it had when it was first known. Whatever other functions it discharges, it acts continuously in obedience to the first great law: it increases, and multiplies, and replenishes the earth.

Let us now, for a few moments, compare our former views of the structure of animal membranes with the present ones. The skin covering the outer surface of the body; the mucous membranes; the serous linings of the great internal cavities, and of blood- and lymph-vessels; and the living membranes of joints, were all alike viewed, as if formed on a definite membrane, covered on one side by cells, and on the other supplied with blood, lymph-vessels, and nerves; the membrane covering in the latter parts, and effecting an absolute separation of the cells from the vessels and nerves, which were universally believed never to penetrate into the cellular layer. The cells were regarded as the parts actively engaged in the performance of the functions, the vessels and nerves adding thereto, supplying material to be acted upon by the cells, and the nerves regulating the amount of action at particular times for special purposes. The diseased conditions, like the functions, were kept perfectly distinct; and we had one set of diseases of the epithelial or cellular parts, and another and a different set of diseases of the membranes and of the parts below.

I think the first occasion on which the public faith in these views was seriously shaken was when the late distinguished Professor of Medicine in St. Andrews, Dr. John Reid, died of what was called an epithelial cancer of the tongue. Microscopical examination showed that the disease existed in the cellular covering of the tongue. A sufficient cause for it was supposed to exist in the irritation caused by sharp

points of the teeth, to cover which a protecting silver plate was constructed. The diseased parts were removed with the greatest skill and care by Sir William Fergusson, and subsequently by the late Dr. James Duncan, assisted by Mr. Goodsir and by Mr. Spence, now professor of surgery in the University of Edinburgh. Every conceivable care was taken by those attached friends of the poor sufferer to remove any trace of the disease; but it progressed steadily and destroyed this valuable life. At this period, no one could understand the extension of an epithelial disease through a basement-membrane, and, therefore, the affection of the adjacent lymphatic glands was explained by supposing the diseased action to have been propagated from cell to cell along the epithelial surface of the lymphatic vessels. Not long afterwards, the sternly truthful and accurate Sir James Paget declared, in terms of terrible significance, to the sufferers of the disease, that epithelial cancer takes a little longer time to do its fatal work. And it soon became thoroughly well known that the glands of the skin, the hair-bulbs, and the teeth are produced by a local development of the deep cells of the cuticle, extending far below the line of the basement-membrane or cutis, and through the position which it was supposed to occupy, as though no membrane was there to hinder them. Thus the basement-membrane, which was supposed so arbitrarily to separate the cells on one surface of membrane from the vessels and nerves on the other, gives way at once before an increased development of the cells, whether in the formation of new organs or the extension of disease. And the membranous walls of capillary blood-vessels allow the corpuscles of the blood to pass through them, much in the same way as solid particles enter into and traverse the substance of the protoplasm of an *amœba* or other mass of sarcoid.

While physiologists were engaged in these observations, the late Master of the Mint, Mr. Graham, was conducting a series of experiments of the most remarkable kind, and of the utmost importance to physiology, as well as to chemistry and physics. He found it necessary to separate two sets of substances, as crystalloids and colloids; the colloids being penetrable by the crystalloids as readily as water is; the crystalloids, such as hydrochloric acid and common salt, passing through organic membranes with great freedom; whilst many of the colloids, such as albumen and gum, will not penetrate them at all. This discovery has enabled the chemist to separate crystalloids from colloids by dialysis, even when they occur in the most minute proportions: for instance, to separate 80 or 90 per cent. of a ten-thousandth part of arsenious acid in twenty-four hours from porter, milk, or infusions of viscera—substances notoriously difficult to analyse; and it has enabled physiologists to explain how animal membranes are traversed by various substances which could not pass through them without being changed from the colloidal into the crystalloidal form. Thus the colloidal starch and albumen of our food scarcely admit of absorption until, in the process of digestion, the starch becomes sugar, and the albumen albuminose—crystalloid bodies, which pass through animal membranes with great facility. And, again, this crystalloidal albuminose, after having passed into the tissues through the membranous walls of the vessels, may become a second time a colloid, and be deposited and fixed as tissue-substance, ready in its turn to be terminated by crystalloids, either for temporary or more durable purposes in the economy. The effect of this great discovery of Mr. Graham's shows how impossible is the advance of physiology without a corresponding advance in our knowledge of chemistry and physics. If basement-membranes, the walls of blood-vessels and of cells, are made up of colloidal matter, we can easily understand how they are penetrated by crystalloids; and in like manner it is perfectly possible that they may be traversed by other substances in solid forms—as, for instance, the walls of blood-vessels by the corpuscles of the blood. No wonder that there is a continual disposition and removal of the constituents of the tissues, if so slight a change as that from the crystalloidal to the colloidal form, and the reverse, make such perfectly marvellous differences in the relation of the substances to each other.

We must look upon the tissues of an animal body as we do upon the substance of an *amœba*, and recollect how penetrable the surfaces and tissues of an animal are; then we shall cease to be startled when we see these parts become the seat of entirely new deposits, or find them traversed by migrating blood-corpuscles as freely as a colloid is penetrated by a crystalloid. It is impossible to foresee what may be the result to physiology of this great advance in our knowledge of the varying relations of substances to each other, according as they present themselves at different times in the opposite physical conditions which were described by Mr. Graham as crystalloidal and colloidal. But it is plain that we cannot continue to look upon animal membranes as forming such decided barriers against the penetration of one tissue by another, or by foreign matters, as was once supposed.

Let me now direct your attention to the present aspect of the ques-

tion, how far basement-membranes limit the distribution of vessels and nerves, and separate them from the cells of glands and membranes. Mr. Bowman, in his admirable researches into the anatomy of the organ of sense, discovered that the filaments of the nerves of smell have a remarkable structure; that they are nucleated, finely granular, contain and resemble the gelatinous nerve-fibre. The epithelial surface, too, of the olfactory region, Mr. Bowman described as differing greatly from that of the adjacent parts of the nasal mucous membrane, and as being of a dark sepia tint. Subsequent examinations by Hoyer, Max Schultze, and Lockhart Clarke, confirmed these statements; and those of Schultze demonstrated that the cells are of two kinds—one elongated and filled with yellowish granular protoplasm, exposed at the outer end of each cell, and containing a clear oval nucleus in clear protoplasm in its deeper part, which is first attenuated, and then extended into a broad flattened process, apparently connected with the connective tissue; the other cell—the proper olfactory cell—a thin rod-like body, is moniliform or varicose, connected below with the out-runners of a nerve-cell, and, in birds and amphibia, furnished with one or more hair-like processes, which at the free end come directly in contact with odorous particles. Exner, in 1872, denied the distinctness of these two forms of cells, stating that these are all intermediate forms, and that both forms are connected with a deep network continuous with filaments of the olfactory nerves.

But Dr. Newell Martin, in a paper published in the November number of the *Journal of Anatomy and Physiology*, maintains that the two kinds of cell are distinct, though their characters approximate very closely in the instance of the frog. He inclines to the belief that, as both forms of cell are distinct from ordinary epithelium, they are all olfactory cells. The only conclusion which can be drawn from these observations is, that in this situation the olfactory nerves divide into myriads of small finger-like processes, which, exposed at the free surface of the membrane, are actually engaged in feeling at the odorous particles to inform us of their characters. This single instance, so thoroughly proved, would be sufficient to destroy our former ideas that nerves are spread out under basement-membranes, and never penetrate an epithelial layer.

But this is not the only case of the kind. The general relations of the gustatory nerves to the epithelial cells of the tongue have been described by Axel Key as similar in the fungiform papillæ of the frog, and by Schwalbe and Løven in the gustatory cells of the circumvallate and of some of the fungiform papillæ in men and animals. On the protected side of the circumvallate papillæ, a peculiarity in the shape and arrangement of epithelial cells produces a series of taste-cones, the central cells of which are furnished with hair-like prolongations similar to those of the olfactory cells. In the otolith sacs and ampullæ of the semicircular canals of the ear, the nerve-filaments, having lost their white substance, become connected with peculiar auditory cells, and end in hair-like processes between the epithelial cells. In the cochlea, too, notwithstanding the complication of the examination produced by the rods of Corti, there is reason to believe that the cells supporting hairs which project beyond the epithelial surface are connected with the primitive nerve-fibrils of the plexus below.

Of the recorded instances in which nerves pass through basement-membranes to come into direct contact or continuity with the superjacent epithelial cells, none is so striking as that of the salivary and other glands, if there be the least ground for the remarkably detailed observations and suggestions of Pflüger. They are of so much importance and interest in connection with the whole process of secretion, that I offer no excuse for directing your attention to them, even though it may be proved that the act of secretion is not attended with such marvellous and extensive changes of structure as Pflüger supposes.

Up to a certain point his observations may be easily and abundantly confirmed; beyond that, there is much greater difficulty; but this meeting offers one of the most favourable opportunities for extending our knowledge, by bringing different observers into easy communication with each other, and enabling each to help the rest by stating the means by which he had overcome what seemed at first to be insuperable difficulties in the progress of an investigation.

Pflüger calls attention to the very variable character of the alveoli, the secreting cells, and the excretory ducts of the salivary glands. These parts, which are believed to have very determinative sizes and characters, he declares to differ very greatly in different parts of the same gland. The alveoli, occupied by what we understand as secreting or glandular epithelial cells and the excretory ducts, lined by columnar epithelium, he thinks he can prove to be but different stages of development of the same structure, produced on the ends of the myriad nervous filaments supplied to these glands. On this view, glandular epithelial cells must be regarded as special organs of termination of nerve-fibrils.

Like the auditory organs, touch-corpuscles, olfactory cells, muscular fibre-cells, and the like, the relation between such structures and the nerves becomes so close that it may be difficult, perhaps impossible, to define their respective limits. Pflüger has figured the nuclei of the cells of the alveoli of the salivary glands, the salivary cells, connected with a delicate fibre, which, after piercing the surface of the cell, is in contact with the *membrana propria*, and gives the cells the appearance of being stalked.

The appearance has also been seen by Schlüter, Otto Weber, Gianuzzi, Boll, and Kölliker, and indeed the appearance which Pflüger has figured may be seen by anyone who will take the trouble to examine the salivary glands of the common cockroach (*Blatta orientalis*). The process was shown to me by my friend and pupil Mr. Charles Workman, and I have several preparations which show a similar process to that which Pflüger has observed and figured; but that it is as clearly connected with the nucleus of the cell as he describes it, I am not prepared to affirm. Pflüger says it is hollow, and often discharges a large quantity of tenacious material, which clearly proceeds from the nucleus. In the interior of the gland there are ducts lined with a thick but single layer of columnar epithelium, the cells of which are clear and nucleated near their free end, but furnished with a large number of extremely fine varicose hairs at the end connected with the *membrana propria*.

This epithelium becomes thicker as the ducts proceed towards their connection with the alveoli, and as transparent drops can be seen transuding from the ends of cells when the saliva has been made to flow by irritation of the gland, Pflüger concludes that they are important secretory organs. Such ducts frequently form loops, or bend suddenly, or possess diverticula. The epithelium of the ducts which carry the secretion out of the gland is of a different and apparently less important kind. Pflüger directs special attention to the great number of nerves connected with the alveoli. He has identified them in fresh specimens by their investments here and there by an ordinary double contoured medulla, by their being blackened by perosmic acid, by their varicosities, and by tracing them to larger and more easily recognisable nerves. He finds them branching in great numbers amongst the cells of the alveoli, and traces their fibrils to the nuclei of the cells. Some, after they have been connected with multipolar ganglion-cells or nerves, covered by medulla and sheath, and containing numerous varicose axis-cylinders, branch, enlarge, and become covered with nuclei, forming what Pflüger calls a protoplasmic foot, and supposes to be a structure intermediate in character between nervous and glandular tissue, and on the surface of the ducts lined by columnar epithelium. A nerve divides a pencil-like tuft of varicose fibrils, each of which, Pflüger says, is directly continuous with one of the processes of a columnar epithelium-cell. I have frequently seen the pencil-like tuft of varicose fibrils on the surface of ducts lined by columnar epithelium, but it is not so easy to be sure that the fibrils are connected with the processes of the cells.

However, the statement is made in the most positive way by Pflüger, who has made these glands the subject of very special and lengthened investigations; and his drawings afford very strong corroborative testimony of the value of his statements. Moreover, in independent observation on the pancreas, he has also traced the nerves to endings in the secreting cells. But Pflüger has gone greatly further than this. He has figured the hair-like processes at the attached end of the columnar cells in all stages of transition into salivary cells of new alveoli; and, having previously found the nerves connected by varicose fibres with protoplasmic masses set with nuclei, he concludes that it is possible that the salivary cells are developed on the ends of the nerves without the interference of their own nuclei; and that as a continual new formation of alveoli and salivary cells implies the atrophy and disintegration of corresponding older parts, the alveoli, with pale offshoots of various forms, which he has seen in moles, are evidence of such atrophy.

With these numerous instances, in which nerves are obliged to pass through membranes, to be connected with the cells on their surfaces, as if these were their special modes of termination, we might well be content until there is time for further investigation by independent observers. But there are yet other instances. Langerhans described, in 1868, a fine network of fibres in the skin, from the superficial part of which fine non-nodulated fibres pass out of the cutis, and end in the Malpighian layer of the epidermis. He saw in the epidermis also well-marked cells, which gave off several processes towards the horny layer, and one long slender process which passed through the Malpighian layer into the cutis. He considered these cells to be nervous, and their peripheral processes to be the terminal part of the nerves of the skin.

C. J. Eberth agrees in the main with Langerhans, and recognises

fine nerve-fibres passing from the nerves of the cutis into the deeper layer of cuticular cells, and also star- and spindle-shaped cells in the cuticle, which, he suggests, may be nervous structures, though he has not traced them in connection with nerve-fibres. On the surface of young fishes and naked amphibia, F. E. Schultze has discerned nerve-hairs arranged in the form of tufts or brushes, very much as is the case in the organ of hearing. In this instance, the brush-like endings of the nerves are probably connected with touch.

Cohnheim has described the corneal nerves as forming a superficial plexus under the anterior elastic lamina; from these, perforating branches pass perpendicularly through the lamina, and then under the epithelium, and break up into brush-like or star-shaped finer branches, which form a plexus, giving off fine nerves at tolerably regular intervals, between the deep columnar cells and the more superficial spheroidal ones, and dividing at length into their finest branches, which end by somewhat swollen extremities in the most superficial epithelial layers. Thus the exquisite sensibility of the front of the eye, like that of the olfactory or the gustatory mucous membrane, may be accounted for.

When I look upon the vast amount of research which has been applied to this department of biology for some years past, and think that the instrument which has afforded the great means for it was only perfected, so as to be capable of use for such purposes, about 1820, I cannot but congratulate the section on the abundant fruits we are reaping; and when, in addition, I contemplate the amount of certainty which physical science has imparted to physiology, by furnishing the means of examining and accurately measuring the rates of transmission of nerve currents; of obtaining tracings of the respiratory movements and of the arterial pulsations; of examining the retina in the living eye, and the larynx of a living man, almost as readily as if these parts were exposed in dissection, I cannot but conclude that this nineteenth century has been already distinguished as a very notable one for biology, and especially for physiology. Considering that so much time is required for making a single careful observation, it is very fortunate that so large an array of inquirers and so much talent are employed upon the subjects in which we are interested, and that once a year we have the admirable opportunity of listening to the results of inquiries instituted by the most eminent men in all parts of the world, and of hearing different views advocated with the greatest earnestness, and yet with perfect good humour, and a rigorous determination to rest satisfied with nothing but the truth.

CASE OF DRY GANGRENE OF THE PENIS.*

By CHAUNCY PUZEY, L.R.C.P., M.R.C.S.,
Honorary Surgeon to the Liverpool Royal Infirmary Lock Hospital.

THE subject of the following remarks is a man, aged 34, single, a smith, who, in 1869, contracted a chancre, which was followed by a suppurating bubo and by sloughing of the whole prepuce, for which he was treated in hospital. Whether the sore which afterwards sloughed had been indurated, or whether he had, a short time previously, had an indurated sore, cannot be ascertained; but he appears to have had secondary symptoms early in 1870. In September 1870, he went out to India, where he remained until May 1873. Whilst there, he was frequently laid up with attacks of intermittent fever, for which he treated himself with quinine.

He says that, from the year 1870, he did not have sexual connection, until November last, when he did so whilst intoxicated. A day or so afterwards, he noticed a small red spot near the frænum, which ulcerated and became painful. The sore gradually increased, and caused him so much distress, that he came into the Lock Hospital in the middle of December.

He was then suffering from slow serpiginous ulceration, apparently of the cicatricial tissue, which had resulted from the sloughing of the prepuce in 1869. There was no deep sloughing, but the sore was dirty, apparently creeping along superficially, and throwing off flakes of sphacelated tissue; it was covered with a fetid and acrid discharge. The whole surface was at once thoroughly swabbed with pure nitric acid, and afterwards dressed with strong opium lotion; opium, nitric acid, and cinchona with wine, and nourishing diet, formed the general treatment. The sore, however, crept on; again acid was applied, and again another time the acid permittate of mercury. Various lotions were tried—mercurial, iron, and carbolic acid—but nothing caused the slightest improvement. The ulcer gradually involved the whole of the old cicatrix.

About the end of the second week of January, it was noticed that

the glans penis was wrinkled and apparently shrivelling; in a few days more, it became dusky, and was evidently dead; and now it was apparent that the skin of the whole of the penis was becoming cold and dusky; then the penis itself became smaller, and, by the end of January, a good line of demarcation had formed at its junction with the scrotum, and it was manifest that the whole of the organ was dead. All the urine was passed through the meatus until the beginning of February, when the urethra became penetrated at the line of demarcation. Action here was very sluggish; lotions, containing 25 per cent. of compound tincture of benzoin, were now used, and, by the end of February, the penis was detached with the exception of a few fibres of tough mortified tissue, which were divided with scissors. No hæmorrhage occurred at any time. The detached organ was perfectly black, dry, and hard. The man left the hospital in March.

I have brought this case forward, because it appears to me to be interesting: 1stly, on account of its rarity; and 2ndly, because it is peculiar from a pathological point of view.

First, with regard to its rarity. I have looked through a considerable number of the periodicals published for the last twenty years, and have only found two cases recorded at all resembling that which I have described. (These were cases brought before the notice of the Pathological Society, by the late Professor Partridge, on March 21st, 1865, and on February 7th, 1870.) In the *Archives Générales de Médecine* for 1870, there is a paper by M. Demarquay "On Gangrene of the Penis". He says that he has collected twenty-five cases from different journals; but I find that in this number he includes loss of the penis from sloughing phagedæna, from mechanical injuries, and all forms of moist gangrene; and he acknowledges that dry gangrene of the whole penis is very rare, and must be generally allied to some predominant general condition depressing the whole system.

This leads me to the second point: What was the cause of the peculiar train of symptoms in this case? There can be no doubt as to the form of gangrene which occurred. It was the slow, dry gangrene arising from the cutting off of the supply of blood to the affected part; and the question is, how this occurred. At first, it struck me that it might be due to syphilitic deposit in or around the arterial walls, gradually obliterating the channel of the vessels; but, considering the number of vessels supplying the penis, it is difficult to suppose that all these vessels could be affected so simultaneously. It is quite possible that they might have undergone some degeneration which assisted in the result described; but we must look for something more than that. I am inclined to look upon it as a case nearly allied to, even if we cannot actually call it, senile gangrene. And here I would draw attention to the general appearance of the patient when he first came under my observation. He was very wasted, with a thin delicate skin, and sallow complexion. His circulation was very feeble; his nose frequently blue at the tip; his fingers exceedingly taper, and his hands delicate, like a woman's. He had several pustulo-crustaceous spots about different parts of the body; the cicatrix of an abscess in the groin, and several livid cicatrices about his legs, the result of abrasions; one, especially large, the result of an incised wound. His legs looked like those of an emaciated old man.

Sir James Paget, in his *Pathology*, says:—"Senile gangrene occurs often in those who are old in structure rather than in years; it affects tissues already degenerate. . . . Some injury or disease, even a very trivial one, kills that which was already nearly dead. . . . The parts around or within it become inflamed, and in these (degenerate as they already were) the further degeneration of the inflammatory process is destructive."

"There are many cases in which explanation, except in the most general and vague terms is difficult. In some, the local death is to be attributed to defective quality of the blood. . . . Certain parts, even small and circumscribed parts, may die through defects or errors of the blood, which yet do not quite hinder it from maintaining the rest of the body."

Sir Benjamin Brodie, in his essay on *Mortification*, says that one of the worst cases of senile gangrene which he ever saw occurred, after a slight injury, in a man, 36 years of age, worn out by the operation of bad habits on a bad constitution. This man had been a soldier, and had served in India and Canada.

Billroth, in his *Surgical Pathology*, says:—"In acute (marasmic) gangrena senilis, the primary coagulation due to feeble circulation occurs in the capillaries, and thence extends backwards to the arteries."

"Very rare", he says, "are the cases where, from excessive anæmia, the arteries are so much contracted, that but little blood circulates through the smaller branches, and the nervous excitation of the heart is so slight, that its contractions are incomplete. . . . The patients, who are generally young, often suffer from rigidity of the hands and feet, fainting fits, and fatigue."

* Read before the Liverpool Medical Institution