

Smith, throughout his work, corroborates a very important practical fact, which I was, I believe, the first to point out and elucidate—viz., that the discharges, mucous or purulent, which issue from the cervical canal are generally secreted by the cervical canal, and not by the uterine cavity. In other words, I believe that I was the first who demonstrated that nearly all that had been previously written by French and English pathologists on endo-metritis, or inflammation of the lining membrane of the uterine cavity, had been written in error as to the seat of disease, and as to the origin of the morbid discharges. Instead of proceeding from the uterine cavity, as was generally supposed, in the great majority of cases, they proceed from the cervical cavity or canal only, the uterine mucous membrane being, comparatively, seldom the seat of disease and of morbid secretions. These views were developed at great length in the second edition of my work on Uterine Inflammations, 1849; and it is gratifying to me to find them so thoroughly corroborated by Dr. Tyler Smith's more recent researches.

Recognising, as Dr. Tyler Smith does, fully, the pathological importance of uterine lesions, ulcerative and other, he agrees with me as to the absolute necessity of their removal by local as well as by general treatment. He adopts the more simple means of local treatment which I recommend—astrinents, injections, local depletion, applications of the nitrate of silver, &c., but repudiates and strongly condemns the more energetic surgical agencies, such as the acid nitrate of mercury, and other mineral acids, potassa fusa, the actual cautery, &c. Dr. Tyler Smith must not think me discourteous if I once more appeal to time and to his own increased experience. These will, in my opinion, inevitably do away with all disagreement between us, by proving to him the absolute necessity of the more potent surgical agencies which he now repudiates. Dr. Tyler Smith is too sensible a man, too clever a physician, to leave in the hands of his fellow-practitioners means of treatment which are occasionally indispensable in order to entirely remove important morbid conditions. When additional experience has shown him, that there are patients, especially in private practice where cases can be followed, who can only be restored to health by the instrumentality of the vitality-modifying agents which he now condemns; and that if he does not therewith cure them, others will; I predict that he will pass the "rubicon," and become a convert to the vitality-modifying doctrine, as he has become a convert to the ulcerative doctrine.

Should that day come, however, as I believe it will, I shall have a right to ask Dr. Tyler Smith to publicly acknowledge his conversion, and not in a second or third edition of his work, to act by this question, as he has done in the first, by the ulceration one. Although compromised, as we have seen, by the expression of very decided opinions, in the controversy on the existence and frequency of ulceration of the neck of the uterus, Dr. Smith, in his work on Leucorrhœa, never even alludes to his having formerly entertained other opinions than those which he enumerates; but quietly describes ulceration as if its pathology had never been questioned, either by himself or by anyone else. I may be allowed to add, that many practitioners who formerly denounced me loudly for using too energetic surgical means in the treatment of uterine disease, have, since then, taught by experience and by my example, adopted these very means, and are now quietly and tacitly employing them, thereby gaining credit and honour in practice. Such a course may be admissible in a private practitioner, but it is certainly not justifiable in a public man, in one who claims to teach and to lead professional opinion.

The term leucorrhœa, if retained at all, ought, in sound pathology, it appears to me, to be reserved for those forms of passive mucous hypersecretion of the vaginal, cervical, and intra-cervical mucous membrane which often temporarily exist independently of inflammatory lesions, and independently of uterine ailment. These fleeting and passive conditions of hypersecretion, really and truly, are the reflex of general condition of health, and seldom come under the eye of the profession as distinct morbid states.

Grosvenor-street, 1856.

THE "MIRROR" OF HOSPITALS IN FRANCE.—A monthly medical journal has just been started in the south of France, called "*L'Union Médicale de la Gironde.*" We are glad to find that our original idea of publishing a synthetic view of the practice of the hospitals, which has been taken up by some of our British contemporaries, is also carried out by the new foreign periodical. This addition is of much importance, and we feel proud that so useful a feature originated with THE LANCET.

Annual Address

DELIVERED BEFORE THE

FELLOWS OF THE ROYAL MEDICAL AND
CHIRURGICAL SOCIETY,

ON SATURDAY, MARCH 1ST, 1856,

BY CÆSAR HENRY HAWKINS, Esq.,

PRESIDENT OF THE SOCIETY, AND SURGEON TO ST. GEORGE'S HOSPITAL.

GENTLEMEN,—An address from the President is inserted amongst the standing rules of the Society, as part of the agenda at its anniversary meeting, and therefore I am compelled, however unsatisfactorily, to fulfil this duty.

In reference to the present state and condition of the Society, the several reports which have been read, and the discussion which has taken place, are sufficient to show the desire of the Council to promote the welfare of the fellows. The expenditure upon our rooms has, I trust, tended considerably to the comfort and even the health of the fellows. The usefulness of our excellent and extensive library will, I hope, be much increased by the formation of the new catalogue, which the Council have thought right to offer to every resident fellow who is desirous of possessing a copy, although the expense is necessarily considerable. I cannot refrain, however, from adding my meed of praise to the great care and labour which have been bestowed upon its formation by our present sub-librarian, Mr. Wheatley, by whom every volume has been examined and entered without reference to the former imperfect catalogue, and also by our librarians, one of whom, Mr. Dixon, whose valuable services we have lost to-day, has not allowed a single word to pass through the press unrevised by himself. Our meeting at the present hour has also arisen from the wish of the Council to study your convenience and comfort; whether correctly or otherwise, it is for the fellows to decide.

My task will now be confined to the customary notice of the fellows whose deaths we have had to deplore since our last anniversary, of whom three were non-resident.

One of these gentlemen, Dr. Ifil, formerly practised in London, but died at Barbadoes on the 9th of March, having for several years relinquished medical pursuits.

The second, Dr. Alex. Russell Jackson, was a staff-surgeon of the 1st class on the Bengal establishment, and was entrusted by the East India Company with the care of their depôt at Warley, where he died on July 28th.

I was personally acquainted only with Mr. Wm. James Wilson, of Manchester, who died on the 19th of July, and whose gentle and amiable manners and professional intelligence must have been well known to many of the fellows. Born at Leeds, and early left by the death of his parents to the kindness of relatives and to his own exertions, Mr. Wilson took the surest road to success, by procuring professional knowledge from every possible source when he began his studies in London. Assistant to a surgeon at Islington, he enjoyed the opportunities of acquiring experience, which a large workhouse has afforded to several other eminent surgeons, while he also laboured at St. Bartholomew's Hospital, and at the Eye Infirmary in Charter-house-square, and afterwards as dresser at the London Hospital. Having then settled at Manchester, his energy procured the establishment soon afterwards of an ophthalmic hospital, to which and to a lying-in hospital he was attached, and afterwards for twenty-eight years as surgeon to the General Infirmary, each of which establishments assisted in procuring for him an extensive practice in all branches of our profession. A friend and pupil of my own, who had been Mr. Wilson's dresser, informs me that he enjoyed the confidence and esteem of a wide circle of his medical brethren during his forty years' residence amongst them, and was an especial favourite with the junior members of our profession, whom he was always desirous of assisting in their claims to any merit for judicious treatment. The only publication of Mr. Wilson, I believe, was a short paper on Purulent Ophthalmia, just after he became a member of the College of Surgeons, chiefly designed to show the efficacy of bark, as he had recently

seen it employed at the Eye Infirmary, in arresting the sloughing of the cornea. The last opportunity of conversing with Mr. Wilson which I enjoyed was when he presided at the annual festive meeting of the fellows of our College, of which I had then the honour of being president, and I was much struck by the fears he expressed to me, least any observations made by him, a provincial fellow, should excite some supposed latent feeling of party spirit between them and their metropolitan brethren. I ventured to hope that his apprehension must be without foundation anywhere, but that I could affirm with confidence that such sentiments were not entertained by London surgeons with regard to their provincial brethren. May I not appeal to the selection of my immediate surgical predecessor in the chair of this Society, also a distinguished provincial surgeon, as a proof that eminence, wherever shown, will always be welcome to us metropolitans, and that the only rivalry we desire is in the advancement of medical and surgical science in all parts of the world.

We have also been deprived of four of our resident fellows, one of whom, Mr. Avery, was removed from us on March 5th, a few days after our last anniversary, after severe and lengthened sufferings, which illustrated the uncertainty of medicine, since his pulmonary disease, which was fatal, was, I understand, unsuspected, being hidden by the very severity of its effects. Mr. Avery was a very zealous and ingenious surgeon. His ardour in the acquisition of knowledge is shown by the doctor's degree granted to him in Paris in 1831, after he had finished his English studies, and by his attempted military service in the revolutionary war in Poland, where his objects in embarking in it were quickly cut short by a lengthened captivity, full of hardships, amongst his Russian conquerors. His ingenuity is evinced by several instruments which he constructed, especially one for opening the urethra in the perinæum, and for the inspection of several of the internal parts of the body, for the latter of which inventions he received the silver medal of the Society of Arts. Mr. Avery contributed several cases, or papers, to the medical journals, and to the "Transactions" of the Pathological Society, but not to our own, from which his diffidence deterred him. His favourite pursuit was the operation for fissure of the hard and soft palate, and he is said to have practised it with dexterity and success, of which he published some cases, with practical remarks, in THE LANCET.

From his official position as our treasurer, the Society has experienced a double loss by the very sudden death of Dr. Joseph Moore, on the 7th of June, who was actively engaged in our service up to that time, being an excellent man of business. He was a Master of Arts, as well as a Doctor of Medicine of the University of Glasgow, and amongst his professional appointments was that of consulting-physician to Queen Charlotte's Lying-in Hospital, to which branch of practice he was specially devoted. Dr. Moore was a member of the Ethnological Society, and he contributed a short paper on "Secondary Measles" in the twenty-second volume of our "Transactions."

The death of another of our fellows, on Nov. 7th, was almost as sudden as that of Dr. Moore, for Mr. George Pilcher had lectured, as if in perfect health, within six or eight hours of his death, although some warning signs, unheeded by himself, had been noticed by more watchful observers for a considerable time. Few learn so well as those who are called upon to teach others, and Mr. Pilcher was so fortunate as to add to the usual studies of our profession that best of all preparations for surgical practice—the duty of a lecturer on Anatomy in conjunction with his relative, Mr. Grainger, in Webb-street. He retained a taste for these pursuits, and has published some remarks on the physiology of the tympanum and on the excitatory system, in which he desired to include the ganglionic system—a novelty which, I presume, will not meet with your concurrence. An essay "On the Structure and Pathology of the Ear" procured for Mr. Pilcher the Fothergillian medal of the Medical Society of London, and it was probably the means of determining the course of his future career, and contributed much to his success, which was considerable, in this branch of surgery. Mr. Pilcher endeavoured, however, very laudably to avoid the narrow views which special practice is so apt to engender, and to combine general scientific and practical surgery with his particular attention to the maladies of one organ. Nor was he unsuccessful, if we take as a proof of considerable professional estimation his several offices of Surgeon to the Surrey Dispensary, Lecturer on Surgery in the Grosvenor-place School, President of the Medical Society, and member of the Council of the Royal College of Surgeons.

To those who are inclined to moralise on the vanity of

earthly ambition, and the transitory value of the highest professional success, an annual retrospect of the diminution of our fellows by the hand of death affords an ample text; but how much stronger is the lesson, when we consider what would have been the impression eight short years ago if my friend and colleague, Dr. William Frederick Chambers, had been then removed from amongst us, instead of surviving in retirement till the 16th of last December; and especially to those of us who remember Dr. Chambers's dignified and gentlemanly bearing in this chair—who were accustomed to meet him in professional and private intercourse, and never without kindness and consideration—when he occupied the first place amongst the practising physicians of this country, and when few persons, from the Sovereign downwards, could risk disease without invoking his aid. It would have been a pleasure to me to have dwelt upon the life of Dr. Chambers—a ripe scholar, a successful practitioner, and always a gentleman—to have learned by what means he acquired knowledge, how he attained success, what official situations he had filled, and what was his deportment and estimation amongst us; but I have been so recently and publicly anticipated, that it would doubtless be tedious to repeat what most of us have read. Amongst the instances of Dr. Chambers's great industry and desire to benefit his patients, we have been astonished to hear of Dr. Chambers's never-ceasing regularity in his daily notes of every case and of every prescription—of his sixty-seven quarto volumes, of 400 pages in each, besides numerous thinner quartos in the shape of indices! But I must confess my strong inclination to ask—Might not this self-imposed absence of that rest of body and mind which is necessary to all, but certainly not least to one so actively engaged in labour, and who, far more than others, dreaded responsibility, and suffered from anxiety, have much caused that strain upon his powers, which counselled retirement long before it was actually forced upon him, and which deprived retirement of its solace? Such unusual care must have left little leisure to instruct others by his experience, and accordingly we find no distinct work published by Dr. Chambers, except three lectures on Cholera, which are written in a very perspicuous style, and contain such a history of the disease from older writers as might be expected from a man of Dr. Chambers's classical attainments, together with the opinions he had formed from his official examination of papers and reports from 1814, when he was appointed Examining Physician to the East India Company, to the time they were delivered in 1832, the last of them having been given on the very day after the first announcement of the existence of Asiatic cholera in the metropolis, when he had just seen three cases. They were not published, however, till 1849,* when the second invasion of this frightful epidemic had given him experience, which confirmed him in the opinions which he had previously formed from the reports of others, especially that it was a fever dependent on atmospheric causes, identical with what had been sometimes witnessed before, but not propagated by contagion. Indeed, he was a staunch non-contagionist in all diseases. These lectures, and some others on Fever, in the first volume of the *Medical Gazette*, in 1828, equally clear and practical, excite regret that we have nothing further from his pen. It was, however, as a clinical teacher in the wards of St. George's Hospital, from 1816 to 1839, rather than as a lecturer, that Dr. Chambers shone. Simple and kind in explanation, clear and scientific in diagnosis, precise in verification of his views in fatal cases, and energetic in treatment, his value as an instructor in practical medicine was highly appreciated by all who studied under him.

It only remains for me to notice the removal from our list of foreign honorary fellows, of the justly-celebrated and veteran physiologist, Magendie, by his death on the 7th of October, at the age of seventy-two. Magendie has been well known and eminent for above half a century, for although it is said that he did not learn to read and write till he was ten years old, in consequence of his father's devotion to revolution, yet that before he was twenty he became, after a successful *concours*, a teacher of anatomy under Boyer, and so great was his early reputation amongst his contemporaries, that he was elected, at the age of thirty-six, into the very limited number allotted to our profession in the Academy of Sciences, in Paris. I cannot pretend, within a brief space, to give an account of all the numerous and diversified labours of this distinguished physiologist, but some circumstances require attention, connecting his name in a peculiar manner with our own country. For myself individually, if I am not egotistical in alluding to it, it is natural, that of all Magendie's researches I should always have

* These Lectures appeared in THE LANCET of that year.

felt the greatest interest in those connected with the spinal and facial nerves, which, in their investigation, are inseparably connected with each other. A pupil of Sir Charles Bell and Mr. John Shaw, for both of whom I entertained the highest regard, having to make choice of lecturing on anatomy with my own teachers, or with Mr. Herbert Mayo, and having ultimately lectured both with Bell and Mayo, it was my lot to assist in the performance of the greater number of the experiments by all of these gentlemen on these subjects; indeed, one of the earliest records of them published at that period was partly in my own words.

I cannot but lament that hard words, and not a little insinuation, have been employed to advocate exclusive claims to discoveries for one or the other physiologist, which should, in justice, be divided amongst all parties, each contributing, in different ways, to the elucidation of what neither might probably ever have completed by himself, for the successive steps by which success was ultimately attained were numerous and complicated. For how much, then, are we indebted to Magendie for what we know of the functions of the spinal cord and nerves?

First, and above all, it is unquestionably (as between these parties) to Sir Charles Bell, that the palm of originality must be assigned. It was by him that the true method of investigation was commenced; and it is, therefore, not without some justice, that the completion of the investigation is also generally called by his name. That nerves possessed different endowments according to their several sources in the central organs, and the number of their roots; that one set were regularly double-rooted and symmetrical, and another set as constantly single-rooted and irregular, and superadded to the former; that each of the double-rooted nerves was connected with both great divisions of the brain,—with the cerebrum through its crura, the anterior columns of the spinal marrow, and anterior roots of the spinal and fifth nerves; and with the cerebellum through the crura cerebelli, posterior columns of the spinal marrow, and posterior or ganglionic roots of the spinal and fifth nerves; that with the cerebrum and its extensions were connected the nerves of motion going outwards, and the nerves of touch and sensibility going inwards from the body or viscera; and that by the cerebellum and its posterior prolongation and nerves were regulated the secret operations of the bodily frame, and the connexions which unite the parts of the body into a system;—all this formed a grand and comprehensive theory of the most attractive kind, which was tested by experiment before 1811, was then nearly dormant for ten years, and revived with renewed impetus in 1821, when I was myself a pupil. That the two roots of the spinal nerves possessed different properties was soon ascertained by these experiments, and also that motion depended on the anterior roots, and that convulsions ensued from irritation of these roots, and not of the posterior; but the imperfect knowledge of the curious effects of reflex action, as now understood, prevented the recognition of the true significance of the facts we witnessed, and made the source of sensation obscure and doubtful, and at first even appeared to confirm the ingenious though erroneous theory of Sir Charles Bell. Before these doubts were solved, Magendie performed his experiments on the roots of the spinal nerves, and published the result in August of the following year, 1822, and thereby clearly established that the posterior roots of the spinal nerves, and posterior columns of the spinal marrow, were much more connected with sensation than the anterior roots. This fact he subsequently corroborated by means of galvanism and strychnia, though he never ventured to assert, as some have said for him, that either motion or sensation belonged to either root exclusively. It is by other physiologists since his time that this has been asserted.

Such, then, was the partial correction of Sir Charles Bell's experiments by Magendie, who was led to their performance, doubtless, by knowing the experiments on the facial nerves already begun in this country. But there is not, as it seems to me, the least ground for the imputation of unfairness, or of his having been acquainted with Sir Charles's experiments on the spinal nerves before performing his own, through either private or published information. So far from this, Mr. John Shaw himself says, in October, 1822, two months afterwards, "In the last number of the 'Journal de Physiologie,' several very curious experiments are related by M. Magendie, which are not only important in themselves, but are interesting, as they corroborate some experiments which had been previously made in this country, but of the performance of which M. Magendie does not appear to have been aware." He then gives a translation of Magendie's paper, and adds: "The importance of the facts discovered by these experiments must be evident to

everyone; and it must be gratifying to the true friends of science in this country to find that M. Magendie, whose sole object in these pursuits appears to be the promotion of physiology, has by his experiments come to the same conclusions as those which had been previously deduced by Mr. Charles Bell from observations made on the brain and spinal marrow. The truth of these deductions was also by him put to the test of experiments, the results of which, though they correspond with those of M. Magendie, were *not so conclusive*." Finally, of our experiments in Great Windmill-street, he says: "It has been difficult to ascertain which of the filaments bestows *sensibility* on the part. It was easily shown that, if only the posterior set was destroyed, the voluntary power over the muscles continued unimpaired; but the pain necessarily attendant upon the performance of the experiments, prevented us from judging of the degree of *sensibility* remaining in the part."

Why did Sir Charles, in his experiments, obtain this "not so conclusive" a result? It arose very much from a remarkable contrast between these two great physiologists. Magendie's recklessness of animal suffering, and his remarkable public apostrophe to one unfortunate victim on the table, "*taisez vous donc*," figured in pamphlets written to denounce the universal vivisection which characterized the physiology of that day, as much as chemistry and histology are used as instruments for its elucidation at the present time. Sir Charles, on the contrary, could not bear to witness suffering and torture, even for the sake of science, and it was not till Mr. John Shaw, with myself or other assistants, had prepared everything for him that our chief ventured to appear and observe, and frequently not till after the animals had been stunned and deadened to pain, nor would he desire more than three or four of the spinal nerves to be exposed for experiment. Magendie, on the other hand, was not satisfied till the whole spinal column was open to his observation, and therefore he saw more clearly.

Bell and Magendie having proved what were the functions of the separate roots of the spinal nerves, attempted also by experiment to test the properties of the columns of the spinal cord, from which they arise; but the experiments were not conclusive, nor as far as they went did pathology satisfactorily confirm the identity of the influence of the two columns with their respective roots. Many fellows of the Society are doubtless aware of some remarkable experiments of M. Brown Sequard, recently performed, which are thought by our allies (in science as in war) to overturn the discoveries of Bell and Magendie. But it must be remembered that Bell's merit consists in pointing attention to the *roots* of nerves, and that whatever may be proved as to the spinal cord, even if hyperæsthesia is produced when we should have expected anæsthesia, the doctrines of the motor and sensitive properties of the respective origins of the spinal nerves, first taught by Bell and Magendie, are not thereby invalidated.

Magendie has continued his interest in physiology even since his death. He founded in conjunction with La Place an annual prize for this science, and it is remarkable that the first prize awarded since he died has been given to M. Brown Sequard for his discoveries on the functions of the spinal cord.

But it is probable that neither Bell nor Magendie would have followed out with success their experiments on the spinal nerves if they had not been assisted by investigations simultaneously conducted into the functions of the cerebral nerves. And here again it was the brilliant imagination of Sir Charles Bell which opened up this path to truth, as far as he and Magendie, or as France and England, may advance their respective claims, and which in this part of the subject also, as with the spine, induced him to theorize first and experiment afterwards—I mean with regard to the nerves of the face, and thus again to obtain results "not so conclusive." Did two nerves go to the face, each as it was thought to the muscles and to the surface? They must therefore convey different properties. One was irregular and single; it was, Sir Charles thought, the respiratory nerve of the face, connected with speech and expression in the higher animals; the other was to give motion to the same muscles and to others in the lower function of prehension and mastication. With all the beautiful movements of the elephant's trunk, which has been aptly compared with the steam-engine for its nicety and its power, and which the infra-orbitary nerve supplied, as it was supposed, with motor power, yet as connected also with the breathing organs, the trunk ought to possess a second motor nerve, the portio dura of the seventh. If the celebrated actor, Liston, was the subject of palsy of the side of the face, as far as the portio dura was concerned, his comical expressions were owing, it was supposed, to the great influence he had

acquired over the muscles of the face through the fifth governing their actions. Magendie, without any imagination, was a very accurate observer of facts. He at once recognised the error we had all fallen into regarding the infra-orbital nerve, when he saw the experiment of its section performed in Paris by Mr. John Shaw, who deserves a larger share of credit than he has received for these investigations, owing to his affection and admiration for his relative and friend. Here, too, Sir Charles Bell's humanity stood in his way, for he only divided the portio dura on one side of the face, the division of which by Mayo on both sides left no doubt that no power of motion was derived from the fifth to the muscles of the face, and it was soon acknowledged by everyone that the seventh was their sole motor nerve.

The superficial nerves and muscles being thus disposed of by the corrections of Magendie and Mayo, the next step was gained by Mr. John Shaw's division of the fifth nerve within the ganglia; for hitherto neither Bell, nor Magendie, nor Shaw, nor Mayo, understood the anatomical distribution of its two roots, though it was known to several of their predecessors; and doubly important was this step in advance, as the functions thus ascertained by dissection and experiment to belong to the motor and sensitive roots of the double-rooted cerebral nerve by the successive and combined observations of Bell, Magendie, Mayo, and Shaw, were a corroborative evidence of the same functions in the separate roots of the remaining nerves of Sir Charles Bell's original symmetrical class—namely, those of the spine. Magendie finally completed the subject by his experiments and observations on the cerebral portions of the fifth, by which, after somewhat absurd errors in relation to the special senses, which his almost entire absence of generalizing powers led him into, he fairly demonstrated the importance of the sensitive part of this nerve in connexion with the perfection and even the life of the parts supplied by it; and this discovery also threw further light upon the corresponding vital functions of the corresponding part of the spinal nerves, which pathology has taught us to be true. Such is a fair and impartial statement of the share which our late honorary fellow contributed to these investigations, after they had been commenced and nearly completed by Sir Charles Bell.

But the history of the subject would not be perfect without allusion to one remarkable circumstance. If you examine the list of presents to the Royal Society at the end of the "Philosophical Transactions" for 1820, you will find that in January of that year they acknowledged that they had lately received a treatise by Bellingheri, which was published in 1818, "On the Anatomy, Physiology, and Pathology of the Fifth and Seventh Nerves of the Head and Face," in which treatise, published three years before Bell began his experiments in this country, almost the whole is accurately pointed out which it required the united efforts of Bell and Shaw, and Magendie and Mayo, during about three years to elucidate. Even some of the errors of their observations were committed also by Bellingheri. And yet it is quite certain that not one of them could have known of its existence, and of the experiments, dissections, and pathological observations of this Italian physiologist. To suppose so (and even this insinuation is not wanting) is to imagine also that they each in succession voluntarily proffered false inferences and erroneous anatomy, in order to have their errors pointed out by a competitor and rival. The simultaneous discovery and rival claims of Le Verrier and Adams were allowed and adjusted to their mutual satisfaction; Bellingheri, undeniably and completely preceding the English and French physiologists, was not so fortunate, and his treatise was perfectly unnoticed in this country till 1834, about sixteen years after its publication; and by whom is his name known as the philosophical discoverer of the functions of the fifth and seventh nerves of the brain? Nor is this all that might be said of Bellingheri's claims, for he must have discovered much of the anatomy, and performed many ingenious experiments corroborative of Bell's and Magendie's, upon the spinal cord and its nerves, before he knew what they had done; for his memoirs were begun to be read before they published the results of their experiments, although his complete work, published in 1823, may have been altered and corrected by what he had then learned of their labours, with which he fairly acknowledges his acquaintance.

Had Magendie done nothing in regard to the nervous system, he would nevertheless have been unquestionably a great physiologist. His beautiful and conclusive observations on the mechanism and causes of vomiting, his ingenious and remarkable experiments on absorption by the veins, and those on the cerebro-spinal cord and the uses of the epiglottis, would alone

have sufficed to establish his legitimate claims to this character. Many of Magendie's investigations had even a more directly practical character, such as those on the entrance of air into the veins; on the action of emetics; of strychnia and other poisons injected into the blood; on the nutritive properties of different articles of food. The latter researches changed the dietary of some of the hospitals in Paris, and his report on quarantine led to the removal of many vexatious regulations. In fact, a long list of Magendie's writings—essays, papers, and some complete works, above all, his "Journal de Physiologie" and "Précis de Physiologie"—have made a knowledge of his labours essential to everyone who pretends to any acquaintance with the science of physiology and its connexion with the healing art. With no pretensions to comprehensiveness of view, or genius for combining and generalizing his facts, Magendie's unwearied industry and painstaking, his accuracy of observation, and, above all, his faithful and trustworthy record of all he did and saw, and his readiness to overcome objections, not by argument, but by renewed inquiry, are evident throughout his life; and it has been said by one who appears to have known him, that it was sufficient to gain his esteem to prove to him that he had adopted a wrong view. It must be confessed, I fear, that so much truthfulness, candour, simplicity, and straightforwardness have not been so well marked in every man of science, and they must have contributed not a little to the great influence which Magendie exercised over the Government, as well as in the world of science.

OBSERVATIONS ON
THE ARCUS SENILIS,
OR
FATTY DEGENERATION OF THE CORNEA.*

By EDWIN CANTON, Esq., F.R.C.S.,

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PART VII.

HAVING taken into consideration, especially, those cases where the arcus senilis has become established at a period of life when its presence is ordinarily to be anticipated,† and, likewise, those instances in which this sign of age has prematurely developed itself,‡ I pass to an account of examples of corneæ invaded by fatty degeneration independent of the occurrence of similar change in other parts of the body. I have endeavoured to show, that of the existence of analogous alteration of internal textures the arcus may be regarded as an exponent, where its formation has not succeeded to injury of the eye, nor supervened upon disease of that organ.

It is, then, to this latter class of cases that I propose, for the present, to confine my attention, and I believe that I cannot adopt a more eligible course in pursuing the inquiry than that of first citing a well-marked example of inflammation of the eyes giving origin to the establishment of broad circles of fatty degeneration in the ordinary situation of the "senile zones." There are, however, so many features of interest in this case besides the one to which I am more particularly referring, that I shall not hesitate to give the account of it unabridged, and in the words in which it is quoted—as an instance of intermittent ophthalmia—by Dr. Mackenzie, in his admirable "Practical Treatise on Diseases of the Eye." (Case 299, p. 622, fourth edition):—"A gentleman came from a distance to consult me,

* "C'est une grande loi de l'économie en vertu de laquelle tous les fois qu'un organe tend à s'atrophier une matière grasse vient à se sécréter autour de cet organe, ou à la place de ces molécules."—Andral. Anatom. Patholog., vol. ii., p. 597.

† "The fatty degeneration in senility is best shown, as a general occurrence, in the increasing obesity which some present at the outset of old age, and in the general fact that there is more fatty matter in all the tissues, and most evidently in the bones, than there is in earlier life; while as local senile fatty degeneration, we find the *arcus senilis*, or, fatty degeneration of the cornea, and the accumulating fatty or atheromatous degeneration of arteries."—Lectures on Surgical Pathology. J. Paget, F.R.S. Vol. i. p. 98.

‡ "Much of early decay may be traced, doubtless, to the impairment of nutrition by slow disease; the malady vanishes, but its effects never. The young as to age become old as to structure."—On Fatty Degeneration. F. Barlow. Page 10.

"Under whatever circumstances the fatty transformation occurs, it is obviously a process of degeneration or degradation to a low scale of animal or even vegetable life."—Principles of Medicine. Dr. Williams. Page 374.