usher in paralysis or spasm in connection with lesions of the motor centres, rather than look upon them as projected central sensory irritation.

Hemiplegia, complete from the first and permanent, is not, however, the most common type of paralysis depending on lesion of the cortex or subjacent medullary fibres. More frequently, paralysis of cortical origin is fractional or dissociated, or is a succession of dissociated paralysees or monoplegiae. In cortical affections, we frequently find a hemiplegia, at first complete, resolving itself into a monoplegia; or a monoplegia becoming a hemiplegia by progressive advance of the disease to other motor centres. This latter is a significant indication of cortical disease. Paralysis of voluntary motion of the arm and leg; of the arm and face, or this combined with aphasia, if the lesion be in the left hemisphere; or paralysis of the inferior facial region; of the arm alone; or of certain movements of the hand and arm; or of the leg alone; without affection of sensation, and without qualitative or quantitative changes in electrical contractility, or direct impairment of nutrition, may be looked upon as depending on lesions of the cortex or subjacent medullary fibres. Monoplegia is very often associated with monospasm or early rigidity of the paralysed limb, or of the muscles governed by the centres surrounding the lesion. Sometimes the paralysed limb may remain motionless while convulsions occur in the others. Cortical paralysis is frequently erratic and transitory, more especially in connection with superficial meningo-encephalitis; appearing and then vanishing, first on one side and then on the other. According as the lesion is superficial, or deep, and invading the whole depth of the cortex and subjacent medulla, we get transitory paralysis, or a paralysis which remains permanent, and is followed by descending sclerosis and late rigidity.

Whereas early rigidity is of frequent occurrence in cortical disease, it is rare in central cerebral disease; and then particularly, it would seem, when the medullary fasciculi of the fronto-parietal region are irritated—a condition generally found in connection with hæmorrhagic effusions into the lateral ventricles. Consciousness is less frequently lost in cases of sudden cortical lesion than when similar disease occurs in the central ganglia. This is to be accounted for by the special tendency in the latter case to sudden displacement of the cerebro-spinal fluid, and, through this, general disturbance of the cerebral circulation, in the manner indicated by Duret.

As an accessory element in the diagnosis of cortical lesions, may be taken the fact, noticed by Callender* and others, that cortical lesions are more frequently accompanied by localised pain in the head; and I have frequently observed that, even when pain is not spontaneously complained of, it may be brought out by percussion over the seat of lesion.

While we cannot be quite certain of the position or extent of a cortical lesion causing a sudden and complete hemiplegia, we may take a monoplegia of the leg, or of the arm and leg, as an indication of lesion of the upper extremity of the ascending convolutions close to the longitudinal fissure; brachial monoplegia, as a sign of lesion of the upper part of the ascending frontal convolution, or, if the paralysis affect the hand more particularly, of the ascending parietal convolution; brachio-facial monoplegia as indicating lesion of the mid-fronto-parietal region; while facial and oral monoplegia, or this combined with aphasia, indicates lesion of the lower part of the ascending frontal convolution, where the third frontal unites with it.

SURGICAL MEMORANDA.

CATGUT DRAINS.

MR. PLAYFAIR is, I have no doubt, correct in his criticism upon my views as to the physics of capillarity, and I am quite ready to accept his statement; but he will allow me to say that the experiment to which I referred was one likely to lead to the view I took of the subject. More explicitly, the experiment referred to was of the following nature. One end of a skein of catgut, not tied together, and therefore with the separate strands at some distance from one another, was placed in an urine-glass filled to the brim with water. The other end hung over the side of a second glass of the same height. In a little time, the water began to run over; but after about an hour the flow completely and finally ceased. The water in the full glass had sunk about a quarter of an inch. I do not venture to argue the physics of the question; but such are the facts. This experiment was performed in one of the wards of the infirmary, and was watched by several of the students.

S. MESSENGER BRADLEY, Manchester.

* Barth. Hospital Reports, 1869.

CLAUDE BERNARD:

A LECTURE

Delivered to the Senior Class of Physiology at the Physiological Laboratory, New Museums, Cambridge.

By MICHAEL FOSTER, M.D., F.R.S.,
Trinity Prælector in Physiology.

GENTLEMEN,—It has long been a heavy burden on my conscience that, while you are receiving your instruction in physiology here, you are told but little of the history of science—that your attention is only rarely and incidentally called to the various steps by which we have reached our present outlook. I fear that such historical statements as do reach you are sadly limited to the controversies of the last decennium. The names you hear quoted are too often those of the authors of the most recent memoirs and of the last new contradiction; and I shall probably be going not far wrong when I say that, were your knowledge limited to what is uttered here, you would be almost justified in supposing that, except for the changes embodied in about the last dozen volumes of Pflüger's *Archiv*, Physiology might have been, like man, created in its present form in the Garden of Eden. This state of things, though undesirable, is, in large measure, unavoidable. Our time is so short, and the body of knowledge we have to traverse so great, that it is absolutely impossible for us to devote an hour's study to the history of our science without foregoing a corresponding quantity of the science itself; and, on the whole, we do well to choose the knowledge itself rather than the story of how we have come into possession of it. The recent death of a great physiologist, Claude Bernard, has, however, determined me to spend the whole of to-day's lecture in pointing out to how large an extent the modern doctrines of physiology have been influenced by his brilliant investigations.

It is not my purpose to dwell upon his personal history; indeed, there is happily but little to tell. Men of science are fortunate when their personal histories are uneventful; they live in their work and not in their lives. And Claude Bernard was in this sense fortunate. All I know of him is that he was born in the sunny South of France, at Saint-Julien, near Villefranche (Rhône), on July 12th, 1813; and that, when nineteen years of age, he came to Paris as an aspirant for literary fame, carrying in his pocket an unaccepted tragedy and a comedy which had enjoyed an obscure provincial success. Arrived in Paris, he was told by a judicious friend (Saint-Marc Girardin) that "poetry was a bad trade, but might be made a profitable amusement".* Other lovers of poetry in general, and of their own in particular, have at divers times received the same valuable advice. Bernard was in some respects singular, in that he obeyed that advice to the letter. The transition from dramatic poetry to the dissecting-room is great and startling, but he did not shrink from it; he became a medical student. In 1839, he became *interne des hôpitaux*. So far, he had given no token of his future eminence; but, in 1841, he was made assistant to Magendie, and then it was that he found his calling.

It is impossible to understand Bernard's life unless the character and influence of Magendie be rightly appreciated; and here, again, I fear that, save in the matter of the functions of the roots of spinal nerves, the name of Magendie conveys no particular ideas to your minds; and yet it occupies a most honourable place in the history of physiology. Magendie was born in 1783 and died in 1855, so that the lives of himself and his pupil Bernard extend over just about a century. During that century, the progress of physiology has been immense; and if I were to ask any of you what has been the distinguishing token of the physiology of this nineteenth century, you would answer "Experimental investigation, the application of chemical and physical knowledge and methods to the solution of biological problems". Now, Magendie was *par excellence* an experimentalist; his whole life was a prolonged effort to unravel physiological phenomena, by the help of chemical and physical methods, applied in a direct experimental manner. He is, in a certain sense, the father of modern experimental physiology. You must remember that, in his time, Germany did not possess the institutions and appliances of which she is now justly proud, and which

* Paul Bert, Preface to *La Science Expérimentale*, par Claude Bernard. Paris, 1878.

we equally justly envy her. In the early part of this century, there were no physiological laboratories at Leipzig or Berlin, or elsewhere. Experimental inquiry in Germany was occasional and slight; indeed, hardly more was being done then in that country than in our own. Magendie stood almost alone as a professed and systematic experimentalist; and, willing as we English always should be to acknowledge, in the fullest possible manner, the benefits which physiology has received from Germany during the last fifty years, we must at the same time not forget that France led the way. That Magendie has not left more mark than he has on the science which he cultivated, is due to two reasons. In the first place, he was to a certain extent before his time. His tools were chemistry and physics; and these had, in his time, not as yet acquired the excellence and precision necessary for his purpose. He tried to do fine work with rough instruments, and, as a matter of course, in many cases failed. Were he living now, instead of fifty years ago, with all the appliances at his command which we have at ours, his name would be in yours and everybody's mouth. In the second place, it must be admitted that, with all his brilliant talents, Magendie's mind was limited. He was essentially *doctrinaire*. Impressed with the physical and chemical aspects of vital phenomena, he did not realise the profound complexity of the molecular changes of living matter. He did not grasp the view, that the apparently simple phenomena of living beings, even if at bottom molecular phenomena, and as such subject to physical laws and capable of physical interpretation, are so transformed that they need to be studied from a stand-point of their own, as if they were phenomena *sui generis*. His sharp sceptical intellect saw through the hollowness and insufficiency of the vitalistic doctrines current in his day, but his mind was not broad enough to recognise that they were the distorted shadows of a real substance. In one word, clear and able as he was, Magendie was without genius. But what was lacking in him abounded in the young spoiled dramatic poet, who came to him first as his pupil, remained as his assistant, and finally succeeded to his chair. And this reminds me that I must return from this digression.

While assistant to Magendie, Bernard published, in 1843, his first memoir, *Recherches Anatomiques et Physiologiques sur la Corde du Tympan*, which was speedily followed by an inaugural thesis, delivered upon his becoming doctor of medicine, the title of which was *Du Sac Gastrigue et de son Rôle dans la Nutrition*. It is suggestive that his two initial pieces of work should be in the two lines along which he was afterwards destined to labour so fruitfully. In 1854, he was appointed Professor of General Physiology in the Faculty of Sciences, a chair established on purpose that he might fill it; and, in 1855, upon the death of Magendie, he was called to the Chair of Medicine, *i. e.*, of Experimental Physiology, in the Collège de France, so long held by his master. Here for many years he laboured, receiving in due time various academic and other honours. In 1868, he quitted the Faculty of Sciences to take the place, formerly held by Flourens, of Professor of General Physiology at the Muséum d'Histoire Naturelle, succeeding Flourens also at the Académie Française. In 1869, he entered the Senate. In 1876, he received the highest scientific honour which England can bestow: the Copley Medal. Having, in the course of a life not too long, won renown in the eyes of all who love science; having come to be a prophet not without honour in his own country; having passed his days loved by all who had the happiness to know him in the flesh, he was taken with illness, in the laboratory, in the midst of his work, and died, from renal disease, on the 10th of February that has just gone by.

It is not my purpose to-day to sit, as your teacher, in judgment on the scientific character of Claude Bernard. He had his faults, and these not inconsiderable ones. I said just now that he had genius; he had also some of the failings which are proverbially said to hang on genius. When you read his writings, you will not fail to be struck with a certain rashness and want of carefulness in his statements, and indeed in his deductions. Thus, in his lectures you will come upon a statement of this kind, that the stimulation of the vagus produces arrest of the heart's beat either in systole or in diastole, according as whether the application of the stimulation takes place during systole or during diastole. Now you have all known, from your physiological infancy upwards, that this is simply an error, that cardiac arrest from stimulation of the vagus is always a diastolic arrest. And in going over Bernard's works, I might point out very many errors of as grave or even graver nature. So much so is this the case, that his books are undesirable books to put in the hands of a beginner or amateur. They always need to be read with the critical faculty alert; otherwise, they are apt to mislead, and, indeed, to do harm; for, as you well know, a writer's errors are always more readily laid hold of and obstinately clung to than his truths; and I have known hangers-on of physiology build up specious quasi-physiological doctrines on a foundation supplied

by some enticing but wholly erroneous observation of Bernard. In a smaller man, these faults would be serious matters; but in Bernard's case, they fade away like slight mists before the rays of his grand discoveries. Even in his lifetime, we were content to forget them; let it be much more so after his death. Happily, the lives of men of science are unlike those of politicians, inasmuch as the good they do lives after them; it is the evil which is interred with their bones. So let it be with Claude Bernard. We have no need to dwell on his shortcomings when we remember that it was his good fortune, amid a large quantity of slighter but still useful work, to make at least the two, we may perhaps even say the three or four, most brilliant physiological discoveries of the half century during which he lived. It is to these to which I wish to call your attention for a little while. My object to-day, indeed, is not to draw up a valuation of Bernard himself, but to point out to you the bearings of his labours.

Let me first turn to the earliest and to the least important of his capital investigations; to his work on the pancreas. Bernard's investigations into the function of the pancreas were made in the year 1846, but were not published till two years afterwards. Let me briefly point out what was the state of our knowledge previous to the appearance of his memoir.

If you read, as I trust some of you sooner or later will do, the *Elementa Physiologie* of the great Haller, you will find that Father of physiology, after a long and laborious discussion of the views of others, concluding that the great use of the pancreatic secretion is to dilute the bile and to temper the acerbity of that fluid in order that it may mingle more freely with the mass of the food along the intestine, and so promote the digestion of the rest of the food. "Hence," says he, "the pancreas of the crocodile is so very large, for the bile of that creature is, indeed, most acrid."*

If you turn to the first edition of Carpenter's *Human Physiology*, published eighty years later—1842—(and which is well worthy your study as an admirable exposition of what were the advanced doctrines of physiology forty years ago), you will read on page 359, as a summary of what was known concerning the action of the pancreatic juice, the following meagre sentence. "The chyme is mingled in the duodenum with the biliary and pancreatic secretions, which effect an immediate alteration both in its sensible and chemical properties."

In point of exact knowledge, this is hardly an advance upon the teaching of Haller. In one respect, indeed, the modern doctrines had gone astray. The older writers, filled as they were with great reverence for the bile, admitted fully the importance of the intestinal digestion; the later inquirers, misled by the striking observations of Beaumont and the discovery of artificial peptic digestion, had gradually been leaning more and more to the view that stomachal digestion was all in all. You know that, in the elementary lectures, I am in the habit of insisting that peptic digestion is at the best a preparatory process, and that the great conversion of food takes place along the length of the intestine. This, the now current doctrine of digestion, dates from the publication of Bernard's memoir. It is true that Valentin† suspected, from the prominence of the pancreas in herbivora, that its secretion converted starch into sugar, and indeed Bouchardat and Sandras‡ had shown this to be the case in fowls. Leuret and Lassaigne also had come to a similar conclusion. But this most important fact cannot be said to have been fairly established, until Bernard proved that it was a constant property of pancreatic juice in all animals and under all circumstances. It is also true that Eberle had previously suggested that the pancreatic juice might have a specific effect on the fats of food; but to Bernard belongs the credit of having pointed out the emulsifying power of that fluid, and of having discovered its peculiar action in splitting up neutral fats. In this matter, as in many other points, Bernard gave way to exaggeration. He insisted that the pancreas, and the pancreas alone, was the agent for the digestion of fat, and his exaggerated views were very naturally contested by Bidder, Schmidt, and others. You know, however, that in this place, we teach that Bernard was so far at least in the right, that the pancreas must be considered as the chief digester of fats, though probably largely assisted by the bile. Lastly, it is true that Tiedemann and Gmelin§ had suggested a connection between the pancreatic juice and proteid food; but their view was a fantastic and erroneous one. They thought that "the casein (alkali-albumin) of the pancreatic secretion, since it contains a large proportion of nitrogen, yields a portion of this element to the different ingredients of the alimentary substances which contain an insufficiency of nitrogen, and thus converts them into albumin, while reducing itself to their standard". Very different from this view is

* *Elementa Physiol.*, vol. vi, p. 453 (1764).

† Lehmann, *Physiol. Chem.*, Band ii, p. 91.

‡ *Compt. Rend.*, tome xx, 1085.

§ Müller's *Physiology* (Baly's translation), vol. i, p. 600, 1840.

the conclusion at which Bernard arrived, that the pancreatic juice dissolves and digests the proteid matters of food, both those which have not been dissolved by the gastric juice and those which, having been thus dissolved, have been reprecipitated by the bile. In fact, before Bernard's memoir, the state of things was this. The action of pancreatic juice on starch was just beginning to be recognised, its action on fats was only just hinted at, and its action on proteids, so to speak, unknown. Bernard clearly proved the existence and pointed out the importance of all these three actions, and thereby restored intestinal digestion to its proper position. I need hardly point out to you how, by the subsequent labours of Corvisart, and especially of Kühne, our knowledge of the proteolytic action of pancreatic juice has made vast strides, and what a flood of light has thereby been thrown not on digestion (and let us hope on indigestion also) only, but on the metabolism of the whole body. The first steps of this inquiry, the ultimate worth of which it is almost impossible to exaggerate, we owe to Claude Bernard.

Far more important, however, than this pancreatic work was the next research, with the results of which Bernard startled the physiological world. I have already spoken of two at least of Bernard's discoveries as being the most important of the mid-century; one of these was his discovery of glycogen and of the glycogenic function of the liver, made known to the world in 1849.*

This discovery naturally starts from the knowledge of the presence of sugar in the blood, which, by a curious coincidence, we owe to Magendie. I need not pass in review before you the details of this great step. Suffice it to say, that the whole credit of it belonged to Bernard. Both the truth and the value of it were of course denied by some, and a long controversy, now happily forgotten, arose about it; but in the end Bernard, supported especially by Lehmann,† came out triumphant. He not only proved the glycogenic function of the liver, but was the first to extract glycogen, though this was independently, at a subsequent date, discovered by Hensen.‡ And to Bernard alone must be attributed the discovery of the diabetic puncture.§ But these matters are household words in all physiological schools; the names Bernard and glycogen are, and always will be, inseparable. You all know, too, how subsequent researches have somewhat modified the views first put forward by Bernard; at the present moment we do not, as he was at least inclined to do, regard proteid matter as the chief, much less as the only, source of glycogen. I should, however, like to spend a few moments in pointing out to you the bearings of this discovery.

Not so very long before Bernard commenced his work, physiologists almost unanimously subscribed to the dogma of Dumas, that "the plant creates, the animal only destroys". This doctrine has been in reality upset by Liebig's proof that fat was actually formed, and not merely accumulated, in the animal body; but it was a doctrine of great tenacity, capable of living in the face of obvious disproof. The discovery of glycogen may be said to have given it its *coup de grâce*. After that it could no longer hold up its head; and to you who, from the beginning of your biological studies, are indoctrinated with the view that the bodies of the complex animal and the complex plant are mere instances of the differentiation along two diverging lines of a common protoplasm, it may perchance seem strange that such a view should ever have been held. It was held though, and that most strongly; and the fact that it now belongs to past history we owe, next to Liebig, to Bernard's discovery of glycogen.

There is, moreover, a second aspect, in which this work of Bernard's acquires even greater importance. There was a time, not so far distant, when the animal body was regarded as merely a bundle, somewhat loosely tied together, of machines called organs, each of which had its special work or function; and the whole work of physiological inquiry was tacitly held to be limited to the finding out what was the function of this or that organ. Hence it came to pass, that when a "function" had been allotted to an organ, there seemed little more to be done. When, for instance, it was stated that the function of the liver was to secrete bile, it was felt that physiological inquiry had done its duty as regards that mass of tissue, and that there was nothing more to be done. Into such a view as this, the discovery of glycogen fell like a bomb-shell. That one organ should have two functions, and those so utterly different, seemed scarcely less than a scandal. And when Bernard went on to show that this same glycogenic "function" was shared by the placenta, by muscular fibres, by epithelial struc-

tures, and indeed might, under appropriate circumstances, be taken on by almost any tissue, the old mechanical theory of organ and function fell to the ground. I do not say that its downfall was not being prepared in many ways by other researches; it received a great shock when Bichat wrote on *The Tissues*; it, while retaining the old phrases, had for some years become more and more unreal; but the final death-blow was dealt by Claude Bernard. It is, I venture to think, not too much to say that if one sign of modern physiology be more hopeful than another, it is the tendency of present inquiries to attack the phenomena of living beings as the co-ordinated results of the concurrent and conflicting properties of variously differentiated living matter, and not as the mechanical outcome of a group of organic machines.

I must now pass on to Bernard's second great discovery, that which, in reality, forms the basis of our present doctrines of vaso-motor action. It is possible, that when I use the word vaso-motor, some of you may be reminded of that gourmand who grew weary of partridges when served every day for dinner. And, indeed, if we were to mark out the history of physiology into epochs, the present might fairly be called the vaso-motor epoch. You think you hear in this place a great deal of vaso-motor nerves and vaso-motor actions. I can assure you that I, for your sake, have to read a still great deal more. Hardly a number of a physiological journal is laid upon my table which does not contain what might be called a vaso-motor memoir. This being so, am I not right in speaking of that initial observation of Bernard's upon which the whole of the modern vaso-motor doctrines are based as an event of capital importance? It was in 1851 that Bernard first observed that dilation of the blood-vessels of the head and neck followed upon the division of the cervical sympathetic. From that simple observation has sprung all the vaso-motor learning which is now poured so plenteously into your minds.

[To be continued.]

REMARKS

ON

THE RECENT OCCURRENCE OF AGUE IN YOUNG CHILDREN IN LONDON: ILLUSTRATED BY FIVE CASES.

By W. B. CHEADLE, M.D., F.R.C.P.,

Physician to the Hospital for Sick Children, Great Ormond Street; Senior Assistant Physician to St. Mary's Hospital; etc.

THE heavy rainfall of the last few seasons appears to have revived a disease which had been almost stamped out by the progress of modern improvement. Ague, with all its striking features fully developed in the acute and early stage of the primary disease, is rarely seen in London now-a-days. Every now and then a well marked case is admitted into the Seamen's Hospital at Greenwich; but in the General Hospitals of London proper the primary forms of intermittent fever are extremely infrequent. Relapses and the irregular recurrence of the later stages are indeed met with, but an initial attack rarely. Similarly, in private practice, a genuine unmitigated attack of ague in full blow, at any rate in the regular form of its earlier stages, is, I imagine, hardly ever encountered in London. Children are said to be less liable to the disease than adults. I have met with cases occasionally, at long intervals, in my practice at Great Ormond Street, during the last ten years; but none so severe, or presenting such striking features, as two out of the five which have come under my observation during the last twelve months.

CASE I. *Severe attack of Tertian Ague in a Child aged two years and nine months, developed five months after residence in an Ague District: Appearance of Rash simulating that of Scarlatina: Immediate Cure by Quinine.*—On February 28th last year, I was asked by one of my colleagues to see his child, a little girl two years and nine months old, who was suffering, as was supposed, from scarlatina. The patient was also seen by Dr. Mahomed and Dr. Broadbent, who kindly gave their assistance in the case, and were as much interested in it as myself. When I first saw the child, about four o'clock, it was in a state of high fever, with a pulse of 116 and temperature of 104 deg., and covered with bright red suffusion of the skin, closely resembling the rash of scarlatina. The throat, however, was not congested, and the tongue, although rather white, was not reddened or strawberry-like. The child was teething, and the gum over one of the double teeth was swollen and a little tender, but did not appear to give any pain or distress. I learnt that the illness had commenced two days before, about 9 A.M., with a sharp shivering fit; a hot-bath was given, after which a bright

* *Compt. Rend.*, t. xxvii, p. 514; t. xxxi, p. 572; *Archiv. Gén. de Méd.*, t. 18, p. 413; published in full in *Nouvelle Fonction du Foie*, 1853.

† *Bericht. Leipzig Gesellsch.*, Band iii, p. 130, 1850.

‡ *Verhandl. Würz. Gesellsch.*, Band vii, p. 216, 1860; Virchow's *Archiv*, xi, p. 395.

§ *Compt. Rend.*, t. xxviii, p. 393; t. xxxi, p. 574; *Gaz. Méd.*, 1852, p. 72.