nin, and others, which would place the central terminations of the optic tracts in the occipital lobes. They are not, I think, in harmony either with physiological experiment, or, as far as I can discover, with the facts of human pathology.

Though many speculations exist, to some of which I shall refer, as to the symptoms dependent on lesions of the occipital lobes, it does not as yet appear, as the following cases will show, that there are any symptoms, attaching to disease of these lobes, so definite and constant as to establish any direct causal relationship between them, or indicate the functions which these regions subserve. Lesions of the occipital lobes are, as a rule, *latent*.

M. Vauttier* records a case of yellow softening of the right occipital lobe, and, to a larger extent, of the internal aspect of the left (quadri-lateral lobule). No affection of motion or sensation existed; and, with the exception of considerable hebetude, there were no other symptoms of cerebral affection.

Pitres† relates a case, in which, in consequence of a fall on the head, an abscess, the size of a billiard-ball, formed in the postero-inferior aspect of the brain. There was no paralysis of motion or sensation, and mental obtuseness was the only indication of cerebral lesion.

Sir W. Gull‡ has recorded a case of abscess of the left posterior lobe, also without any objective symptoms.

A similar case, in which the abscess was situated in the right occipital lobe, is recorded by Rodocalat.§

Other cases, reported by Bleyenie, Hébread, Martinet, Merriman, Ogle, Parrot, Ballinger, etc., are cited by M. Pitres in his before mentioned work,|| to which I would refer the reader.

It may be said, in reference to these cases, that the lesions being unilateral, and of slow growth, the absence of symptoms may be explained by functional compensation by the same or the opposite hemisphere. There are, however, some cases on record of traumatic lesions of the occipital lobes, also negative as regards objective symptoms.

Marcé¶ records a case of contusion, with effusion into the meninges, and softening of the cortex in the right occipital lobe, without any symptom as regards sensation or motion.

In addition to Vauttier's case already mentioned, a very important case of bilateral lesion of the occipital lobes has been put on record by Sestié.** In Sestié's case, there was an abscess in each occipital lobe, without any objective symptom; although the patient's memory was somewhat defective, there was nothing very remarkable in his mental condition.

Leger†† relates a case of tumour invading both occipital lobes, in which, beyond general mental obtuseness and headache, there were no objective symptoms. Sight was not impaired in this case, notwithstanding the existence of a cerebral tumour in this region. Except, therefore, as regards the fact of latency of lesions of the occipital lobes, these cases do not give us much positive material for generalisation.

Joffroy‡‡ attempts to establish a relation between lesions of the occipital lobes and the occurrence of acute sloughing of the sacrum; and thinks that the occipital lobes may be trophic centres. He quotes three cases in support of this hypothesis. One, a case of general paralysis, in which, though the cerebral lesions were not confined to the occipital lobes, yet the sacral sloughing was greater on the side opposite the lobe most affected. In the other two, there was only slight unilateral lesion: in the one case, a small focus of hæmorrhage; in the other, of softening, with sloughing on the opposite side of the sacrum.

But, in reference to this hypothesis, it needs only to be remarked that sacral sloughing occurs in connection with hemiplegia where no affection of the occipital lobes has been detected; and lesions of the occipital lobes are not even commonly associated with such results. Cases have occurred, and been carefully examined, in reference to this point since the publication of M. Joffroy's views, without confirming them. One such case is reported by Sazic,§§ and another by Dreyfus-Brisac.|||| These may be taken in conjunction with the other cases mentioned; for it is not likely that such a serious objective affection would have been overlooked had it actually existed.

In connection with softening of the occipital lobes, M. Charcot has occasionally noted, in addition to headache, etc., cutaneous formication and similar subjective sensations, but no true anæsthesia.

Hughlings Jackson and Bastian are of opinion that disease of the posterior lobes is more frequently associated with mental derangement than disease of the anterior lobes or other parts of the brain. Hughlings Jackson goes somewhat further, and thinks that such derangements, particularly "defective perception", are more commonly seen when the right side is affected, and that irritative lesions here give rise to coloured vision and other subjective ocular spectra. These are not put forward, however, as other than hypotheses, and I agree with Dr. Bastian when he says: "More extended observations, however, seem needed before we can safely arrive at a positive conclusion on this difficult subject."*

Even on my own hypothesis as to the relation between the occipital lobes and the organic sensations, I should regard it as highly probable that lesions of the occipital lobes should cause profound mental disturbances. For, if our feelings are our chief motors, it might readily be allowed that lesions of the anatomical substrata of such an important element of our feelings and emotions should lead to mental derangement.

But, not to indulge further in speculation, I think we have to admit that up to the present the facts do not yet enable us to generalise with certainty as to the positive effects of lesions of the occipital lobes. It is clear, however, from the negative effects of extirpation or disease that we cannot place in the occipital lobes the central terminations of the fibres of the internal capsule which convey impressions of special sense to the cortex.

[To be continued.]

CLAUDE BERNARD:

A LECTURE

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

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IN order, however, that you may understand the beginning of this matter, I must take you back a little.

Possibly it has never occurred to you that there was a time when the plain unstriated muscular fibres of the arteries were things unknown, or at least not believed in; and yet it is less than forty years ago (1840) that their presence was first demonstrated by Henle. Just imagine what lame and mutilated answers you would send up, in your examinations, if you were forbidden to employ any reasoning which involved the existence of muscular contractility in the arteries. You would be brought to a standstill, not in questions only belonging directly to the circulation, but in almost all kinds of physiological problems. How could you treat properly the secretion of the digestive juices, the excretion of urine, the origin and distribution of animal heat, or even the problems of the central nervous system itself, if you were debarred from all reference to vaso-motor action? Try to withdraw from the web of to-day's physiology the thread of vascular contractility, and the whole tissue becomes a tangled skein. How, then, you may naturally ask, were those phenomena explained which we now so readily interpret as the results of vaso-motor action? Happy are you that you can now learn physiology without having your minds burdened with the ideas of determination of blood, nutritive force, capillary attraction, and other *Dei ex machinâ* called in to explain the facts which you unravel by help of the simple protoplasmic property of contractility.

Let me read you what the great anatomist and physiologist Johannes Müller wrote, just before Henle's discovery. I read from Baly's translation, which was not published indeed till after Henle's paper had appeared. "The old writers, and many recent physiologists, have erroneously regarded the contraction of the arteries which follow their dilatation as a muscular act, and have looked upon the fibres of the middle arterial coat as muscular fibres. But the fibres of the elastic coat of arteries differ, as we have already mentioned, from muscular fibres, both by their physical and chemical properties, as Berzelius has shown, as well as by their form. The different arguments for the existence of the pretended muscular contractility of arteries, which have

* *Essai sur le Ramolissement Cérébral Latent*, 1868.

† *Lesions du Centre Ovale*.

‡ *Guy's Hospital Reports*, 1857.

§ *Bull. Soc. Anat.*, 1870, p. 289.

|| *Op. cit.*, p. 134.

¶ *Bull. Soc. Anat.*, 1854, p. 295.

** *Bull. Soc. Anat.*, 1833.

†† *Bull. Soc. Anat.*, November 1876.

‡‡ *Soc. de Biologie, Séance December 4th, 1875.*

§§ *Bull. Soc. Anat.*, December 15th, 1876.

|||| *Bull. Soc. Anat.*, March 23rd, 1877.

* *Paralysis from Brain-Disease*, p. 239.

† Concluded from page 521 of last number.

been adduced from comparative and pathological anatomy, are of no weight." (Vol. i, page 214.)

Then, again: "Although it be denied that the circulation is in any way aided by an attraction between the blood and the capillaries, yet the existence of such an attraction or affinity may be admitted in the instance of the 'turgescence, turgor vitalis, or orgasm', observed to take place in certain parts of the body, which are the seat of increased vital action, independently of the action of the heart. . . . This condition of vascular turgescence may be excited very suddenly, as is seen in the instantaneous injection of the cheeks with blood in the act of blushing, and of the whole head under the influence of violent passions; in both of which instances the local phenomena are evidently induced by nervous influence. The active congestion of certain organs—of the brain, for example—while they are in a state of excitement, is a similar phenomenon." (*Ibid.*, p. 239.)

Dr. Carpenter, in writing his first edition of his *Physiology*, from which I have already quoted, was aware of Henle's discovery; and we find him using the following language. "A very important use may be assigned to this muscular coat (of the arteries), which has been generally overlooked by physiologists: that of regulating the diameter of the tubes in accordance with the quantity of blood to be conducted through them to any part." (*Op. cit.*, page 411.) And again: "The minute distribution of the sympathetic nerve upon the walls of the arteries, the known power which this has of producing contractions alike in their fibrous coat and in the muscular tissue of the intestinal canal, and various phenomena which indicate the power of certain states of mind over the dimensions of the arteries, in particular parts of the body at least, render it highly probable that the calibre of the arteries is regulated in no inconsiderable degree through its intervention." (*Ibid.*, page 412.) In these words, you will readily acknowledge an anticipation of our more modern views; and yet, in spite of this, we find Dr. Carpenter, a little further on (page 415), relapsing into an enthusiastic support of the current doctrines of determination of blood, etc.

One man indeed, a man of great honour amongst us as the author of the cell-theory, I mean Schwann (who, in a few weeks, will receive public acknowledgment of his great services in the University which he has served now for forty years), had, some few years before, not only stoutly maintained, and, as he thought, experimentally proved, the contractility of the small arteries, but offered an explanation of vascular dilatation exceedingly like our present inhibitory doctrine. To his experiments, Johannes Müller replied with the dogmatic statement, that Schwann's results "could not be due to contractility of muscular fibres, because these did not exist in arteries". (*Op. cit.*, page 218.) And the physiological world in general went astray with Müller. A few scattered adherents may, from time to time, have more or less distinctly accepted Schwann's views, or something similar; but the vaso-motor doctrines now current cannot be said to have become part of general physiological teaching until after Bernard had, in 1851, called attention to the effects of division of the cervical sympathetic. As you already know, Brown-Séquard, in the following year, arrived independently not only at Bernard's result, that section of the sympathetic caused vascular dilatation of the ear, but also at the even more important result, unknown apparently at first to Bernard, that the dilatation and blush gave place to constriction and pallor when the peripheral segment of the divided nerve was stimulated; and our own Waller did much to put the whole matter in its proper light at an early stage of the discovery.

We may note here that, in this as in many other of Bernard's exploits, the discovery was, so to speak, "in the air"—it was waiting to be made. But is not this true of all discoveries? Are they not all "waiting to be made"? If Bernard had not made the observation, Brown-Séquard would; if Brown-Séquard had not, Waller would; if Waller had not, some one else would. As a matter of fact, Bernard was the first, and to him accordingly must the credit be given.

From this simple beginning, from this slight experiment, which you have now seen so often that you think nothing of it, has grown all our large body of vaso-motor physiology. And if I needed to give you any further proof of how essentially true it is that we owe this to Claude Bernard, I should only have to call your attention to the fact that it is to him again that we are indebted for the second great step in the progress of the doctrine. In 1851, Ludwig made the memorable observation that electric stimulation of the lingual nerve (or rather of the chorda tympani fibres running in the lingual trunk) called forth the activity of the submaxillary gland; but it was Bernard who, a little later, drew attention to the fact that the stimulation of the same fibres caused dilatation of the blood-vessels of the gland. He it was who first introduced the phrase "vaso-dilator nerve"; and I need hardly remind you what a crowd of ideas, some of them perhaps conflicting, but all illuminating, these words call up in the mind of the physiological student to-day.

But time is failing me; I must content myself with brief allusions to Bernard's other services.

It was in 1844 that he commenced his remarkable investigations on urari poison, though his observations were not published till 1850. The discovery of the action of this poison is perhaps typical of Bernard's work. It, like others, was a discovery "in the air"; for Kölliker, working quite independently, arrived exactly at the same result. Bernard's treatment of the question was characteristically his own. If you read his account of the matter, you will see how he gives way to exaggeration, and even to inaccuracies; and how he frequently fails to realise, or at least to show that he realised, the difficulties of his positions. In this respect, Kölliker's more cautious work contrasts favourably with his. But this must always be remembered of Bernard's apparent rashness. His speculations, even when hasty, were in most cases happy; his conclusions, even when not warranted by the evidence before him have, not once or twice, but often, proved in the end, upon fuller knowledge, to be true.

And what shall I say of the importance of this urari discovery? Letting pass on one side altogether its effect in clearing up our notions of muscular irritability and nervous impulses, let me ask you to reflect on the service which has been rendered to physiology by a knowledge of the phenomena of urari poisoning, viewed as a mere method of research. If, at this moment, we were to blot out from our modern physiological science all that has been gained, directly or indirectly, by means of urari, what would come of it?

Such are some of the more important services rendered by Bernard to the science we are studying here. If I were to attempt to tell of all his work, from that first early memoir on the chorda tympani to that last work on ferments, in the midst of which death found and took him, not a single lecture, but almost a whole course of lectures, would be needed. I have narrated to you what I take to be his most important achievements. Remove these, and there are still left basketfuls of fragment—sufficient, I might say, to establish the reputation of a whole school of young physiologists. I might speak of his important work on carbonic acid poisoning; of his many papers on the submaxillary gland, in which, besides the capital fact of which I have already spoken, he pointed out the effects of reflex stimulation, the change of colour during activity, the so-called paralytic secretion, and contended for the autonomy of the submaxillary ganglion. I might speak, too, of his labours on animal temperature, starting with that early observation on the rise of temperature in the ear, following upon division of the cervical sympathetic, and continued till his last days. As you know, I am one of those who think that Bernard's, perhaps at present unwarranted, conclusion as to the existence of frigorific and calorific nerves, is precisely one of those which future researches will probably show to be eminently happy. If so, then we must simply double, perhaps more than double, the value which we have already attached to that pioneer experiment on the cervical sympathetic.

But I must stop. To tell of all Bernard's work, I should have to traverse the whole range of physiology. There remains, however, one feature of the man of which I must say one word. I began by telling you that he arrived at Paris a poet: he remained a poet to his death. A poet I mean, not in the sense that all scientific discoverers are poets, that it needs as fine a frenzy to pierce the darkness and unravel the perplexities of the entangled phenomena of what we call matter, as to fret about the troubles and longings of what we call spirit; but a poet, in the lower and more usual sense, that he was a phrase-maker. When you read his works, you will find yourself carried along by the flow of an easy diction, which seems to make everything plain; and every now and then you will come upon a phrase, the introduction of which is in itself equal almost in value to a discovery. You remember how, in our lectures here, we insist on the blood being regarded as the great "internal medium" on which, and in which, and by which the tissues live; you know that our teaching here is largely built up upon this conception. This picturesque and far-reaching phrase we owe, too, to Bernard. What, again, can be happier than his description of carbonic oxide poisoning as "a paralysis of the blood-corpuscles"? Other phrases of a like nature you will find scattered over Bernard's writings, some of them luminous in the highest degree, all of them happy and instructive.

Such is the story of Claude Bernard. I have not attempted to hide from you those things in which he fell short. I trust I have led you to feel how great was the work he did. Without exaggeration, we may say that his labours are *par excellence* the physiological labours of the middle third of the present century; and fortunate will be physiology if, when the century closes in, she can point to yet other later physiologists as having laboured with equal power, with equal diligence, and with equal fruit.