THE CROONIAN LECTURES

ON THE

INFLUENCE OF THE CIRCULATION UPON
THE NERVOUS SYSTEM.*Delivered before the Royal College of Physicians.*

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LECTURE I.

AN essay might well be written on the consequences to medicine of the rise and expansion of the science of biology; to show how it affects medical study that the physiology of man has become a special branch of a physiology of living things. This is not the occasion for such an essay, nor even for such a remark; but the remark arises because, in beginning these lectures on the behaviour of the human brain under varying states of the circulation of the blood within it, I was of course induced, as a first step, to bring forward the natural history of the subject. But when I referred to the excellent and ably written text-book which is now universally employed as a student's manual in physiology, and searched in it for the physiology of the circulation in the human skull, I did not find a word upon the subject. I found a full and very interesting account of the falling off of rane intelligence which reveals itself when the several parts of the brain of a frog are successively cut away; but no notice of the curious and very important mechanism whereby the human brain is made secure within the skull, under the varying stress of blood-pressure that accompanies bodily or mental exertion. I observed that the biological physiologist regards the human brain as at one with the vertebral brain—a general kind of thing, more easily studied in the frog, the dog, or the monkey, on account of its simpler development in these animals. The biologist thus takes vertebrate brains at large; and it appears that to him the blood-supply of the human brain is a detail respecting one kind of brain only, and he cares nothing for its congestion, and giddiness, and insensibility; such particulars are lost sight of in the large scope of biological science. For biology tends to wideness of scope and generality of principle. Indeed, it appears not to be quite happy far out of sight of protoplasm; it goes back to protoplasm to trace up the general nervous system, and says nothing about the cerebro-spinal fluid. But it cannot be conducive to clearness of medical thought upon the circulation in the brain, that protoplasm should have displaced the cerebro-spinal fluid from works in which medical students learn human physiology. It is not as though protoplasm were better founded than the cerebro-spinal fluid. It is not so well founded. For there can be no doubt that there is such a thing as the cerebro-spinal fluid; nor is there any doubt that it fulfils vitally important functions, which greatly concern those who are called upon to be clear about the causes of giddiness or insensibility. But it is by no means certain that there is such a thing as protoplasm.

The biologist reduces the various behaviour of the nervous system to what is called the irritability of what is called protoplasm. He regards protoplasm with the same undoubting confidence in its reality as the physical philosopher has in the reality of motion, when he, on his part, reduces all the kinds of activity in unliving matter to terms of what he calls motion; so that heat is a mode of motion, and electricity a mode of motion, and light a mode of motion. Other philosophers, less restrained by fixation in the science, either of living or of unliving matter, generalise one sweep more widely, and tell us that vital motion is a mode of motion; so that we may take out all things knowable, even to human thought, in terms of motion, much in the same way as all kinds of value, even to human motive, might be taken out in terms of pence. But of course it is true that, just in the same way as all kinds of value might equally rightly be taken out in terms of shillings or of pounds as in pence; and just in the same way as all kinds of activity in unliving matter might equally well be taken out in terms of electricity, or in terms of light, as in terms of what is called motion; so also the behaviour of living matter might as rightly be taken out in terms of the brain-cell or of blood, as in terms of what is called protoplasm. For the only basis of knowledge in all these systems is equivalence in convertibility. The capitalist knows that his pound is convertible to so many

shillings, or may be expressed in terms of shillings. The physical philosopher that his foot-ton is convertible to so much electricity, or may be expressed in terms of electricity. And the biologist thinks that the nerve-cell is convertible to so much protoplasm, or may be expressed in terms of protoplasm. And the other philosopher, less restrained by fixation in the science, either of living or of unliving matter, thinks he knows that vital motion, even to human thought, is convertible to so much motion. Meantime, the physical philosophers have been led to say that heat is a mode of motion, only because mechanical motion is more universally appreciated by our senses; so that it is natural to employ it as a standard. And, for all we know, the higher animals in the planet Jupiter may be so organised that they obtain mechanical motion only by difficult experiments, as we do electricity; whilst in their hot region they have electricity always obvious to their senses. In this case, we may depend upon it they would naturally say mechanical motion is a mode of electricity; for there is no sense of reality in which electricity is not as real as mechanical motion; and it would be quite as right to adopt electricity as the standard, and say motion is a mode of electricity. In fact, electricity reveals the nature of motion, and motion reveals the nature of electricity. They mutually reveal their common nature: under certain conditions, the one becomes the other. And the physical philosopher, for purposes of exposition, chooses mechanical motion, which is the most tangible and the best known, and adopts it as the common standard for his reference, and the basis of his thought and reasoning. But the biologist, in his field, reverses this plain method of dealing, and makes his thought and reasoning rest on protoplasm, which is not the most tangible and the best known of the things with which he deals. Indeed, it is so far from being so, that it does not exist at all. There is no such thing as protoplasm. I have watched what is called protoplasm, in what are its most nearly pure forms, at work, and it was plain that its properties depended altogether on whose protoplasm it was; and that, when anything is to be done with it, or by it, you must accept it with its own peculiar powers, as it is at work before you; and that, if you act towards any living thing in terms of protoplasm, with the idea of in that way getting a product of its life and nature, you find you have got hold of the wrong end of the stick, and that your abstract idea of protoplasm does not fit to the vital faculty, which acts in, and I may say is, the living thing.

And whereas every living thing or part of a thing is held by the biologist to be a specialisation of protoplasm, in the same way as light and galvanism are held to be specialisations of motion by the physical philosopher, there is no parallel at all in the sureness of the tenets in the two cases; for, although motion, considered apart from any moving thing, is an abstraction, like protoplasm, yet it is a simple abstraction, which we can use in calculation, and verify our calculated results by experiment. But protoplasm is a very complex abstraction, theoretically endowed with all the properties of living matter; and there are no means of testing how far this complex of properties, when worked out theoretically, in combination with other simple abstractions—like mass, velocity, etc.—would be found, on experiment, to correspond with the calculated result; so that the conception of protoplasm has no stable basis, like that of motion. And by means of it we do not get any established ground wherefrom to consider the phenomena of life; and we only spin out speculations by means of it which, when finished, are no more realisable than the hypothetical conception with which they begin. Therefore, I do not think it any real gain that the biological teacher of the medical student has displaced the cerebro-spinal fluid by protoplasm. And I am begging you to fix your attention particularly on this fluid, because it is upon this fluid that a great deal of what I have to say to-day centres itself.

It has long been known that the watery fluid within the ventricles of the brain finds entry and exit from the brain into the spinal canal, so that it comes and goes from spine to brain, according as the pressure of blood in the brain is less or more; and this way there is provision for a change in the quantity of blood which can circulate in the brain. The degree in which the nervous system can exercise its special power, independently of direct action of its supplying vessels, is a question very important for the obtaining of clear views upon the complicated phenomena of nervous disorders. In these lectures, I am going to endeavour to estimate the extent of disturbance of the nervous system, which can be fairly ascribed to changes in the calibre of the blood-vessels, and in the blood-pressure within them, so as to ascertain what is the true value of the main "congestion of the brain", and to show the relation of the epileptic fit to vascular action, and of angina pectoris to high tension within the vessels.

This kind of question has been a great deal lost sight of by the pure physiologist, who seems to me, if I may humbly say so, to have let pass the consideration of some most interesting and important branches

of specially human physiology in the modern pursuit of fundamental vertebrate life, in that convenient victim and martyr, the common frog.

I am now concerned with one such branch, and it is the study of the means whereby the circulation in the human brain is made secure, so that no augmentation of pressure in the brain, either from changes in the force of the heart, or from changes in the atmospheric pressure, or from changes of posture, ever produces any ill effect. Our best modern text-book on physiology scarcely deigns to notice the group of mechanical contrivances whereby the human frame is adapted to go upright. The text-books of twenty years ago used to set forth the several points in the human organism, whereby the erect posture is made possible. Dr. Carpenter used to give a list of these, which I will not recount, but will only mention that he left out one essential to the erect posture in man, necessary, at least, to his long maintenance of it, and that is the ambition that induced him to get upon his legs. Now for this a relative increase in the size of his brain was required; so that, after vitalising and giving energy to brace up all Dr. Carpenter's mechanisms, there might be left enough nervous power to be willing to take the trouble to be upright.

But this increased size of brain necessitated an increase of blood-supply; a great increase it must be, considering how richly blood is given to nerve.

Meantime, the same erect posture placed the brain so much aloft, that blood-supply to it is less easy. For consider the difference in hæmostatic conditions which arises, when a column five or six feet long, containing fluid, is suddenly raised to the vertical, or suddenly lowered to the horizontal position. Imitate the case with a model of some spongy material soaked in liquid, and of course, as you raise the column of sponge, the liquid in it will collect below, and as you lower the column the liquid will go to both ends equally. And of course it need not be said that the physical forces which act in dead matter act equally in living; so that this tendency of the blood in the living sponge which composes our frame has to be met and counteracted, or all the mechanism for the erect posture would be of no avail.

The problem, then, is how to prevent the sinking down of blood from the brain. That there is risk of at least inconvenience from such mere opposition of gravitation to circulation, is experienced very often by many persons in delicate health, who suffer a certain giddiness on rising from a stooping posture; a giddiness, which may be accompanied by slight loss of sight. But this occasional and trivial inconvenience to some would, of course, be constant in all, unless there were provided a large means of checking the great tendency of the blood to act in obedience to hydrostatic laws, and sink to the lowest possible level. The principal means whereby this is accomplished is doubtless the suction exerted by the practically closed cavity of the skull. Allowing all the considerations which render this cavity less securely closed than a metal globe, pierced only for entry and exit of to-and-fro pipes, would be, the cranium, nevertheless, composes a very large space inaccessible to external pressure, except, firstly, by means of the blood in the vessels which enter and leave it; and, secondly, by means of the cerebro-spinal fluid. Let us remember the relatively not very large amount of this fluid in a healthy brain, and we must allow that blood within the skull is, to a very large extent, prevented from leaving the skull by the impossibility of air or any other substance than blood finding its way in to supply the place of the blood that would go out. And, doubtless, no part of the whole system of contrivance is equal in its efficiency to this suction. But whilst it is true that by far the greatest part of the blood would then be retained by suction in the brain, it is also true that the incompleteness of the closure of the cranium does allow some amount of variation in the quantity of blood which it can hold. This variation is chiefly allowed by the variation in the amount of the watery liquid in the ventricles.

Now the part of the cerebro-spinal fluid which is within the cranium amounts in most persons to about two to three ounces, and it is, to a large extent, capable of being pushed out of the cranium or drawn into it. This gives us a variable margin of sufficient importance to call for arrangements that may meet such sudden changes in the amount of blood, as would arise in virtue of variations in this fluid. For, though it may seem that three ounces do not amount to much when compared with the whole capacity of the skull, yet there is evidence that the quantity of blood in the cranium is only capable of small changes; and that not very large changes of quantity of contents in the skull are required to produce fatal symptoms. I used to weigh the amount of blood which had escaped into the brain in cases of apoplexy; I never met with more than three and a half ounces of it. This had been sufficient to take away sense and life, that is, so far as quantity is concerned; when the tendency was to, a greater bleeding, this

showed itself in density or hardness of the clot, not in increase of it. In fact, there was no room for more, and the three ounces and a-half correspond nearly with the three ounces of cerebro-spinal liquid, especially when we observe that apoplexy occurs in anæmic brains, in which the cerebro-spinal fluid is present in more than the natural quantity. That apoplexy occurs in anæmic brains, I shall prove in my second lecture. Thus, an amount of change in the contents which bears but a small proportion to the whole capacity of the cranium, estimating that at about fifty-five ounces, is all that is observed in case of fatal pressure on the brain. It is true, that somewhat more of extraneous substance can be introduced gradually into the cranium without destroying mental life. Thus I have weighed tubercle to the extent of five ounces from the brain of a child, who showed intelligence up to the time of a fatal meningitis, which had come on in consequence of the tubercle. But it is certain that a change of about three ounces is all that can be borne suddenly by the brain; and common movements of the frame must be supposed to be quite equal to produce a change of that extent. For, when a person rises suddenly from the stooping posture, the blood which was favoured by gravitation in its flow towards the brain when the head was down, has now to run up to the brain through narrow pipes of blood-vessels, and against friction and gravitation. But if the circulation be feeble, the pressure of blood towards the cranium may be deficient. One may estimate the great and important extent to which sudden uprising tends to reduce blood-pressure towards the brain, by recalling how raising the arms will impair the power of circulation towards the hands, and how raising the legs will reduce the circulation towards the feet; facts of which the practitioner avails himself in checking hæmorrhage, and in reducing inflammation or ulceration of the extremities. And it is well known that a person may even be led to feel faint by raising both arms for a long period. Now, the brain is being constantly put into the position of a raised arm or leg; and although, by the suction to which I have referred, it is kept from such extreme changes as the hand or foot undergoes, yet the cerebro-spinal fluid allows changes to the amount of about three ounces. Now, the way by which this watery liquid enters the cranium and brain is, as I shall show, a wider and freer passage; whilst the way in which the blood reaches the texture of the brain is by the narrow passages of the capillary arteries; and if the circulation be feeble, the heart may drive the blood less forcibly to the texture of the brain than the venous pressure in the spinal canal drives the cerebro-spinal fluid into its cavities, so that, when the head is raised, and the venous blood falls away more readily from within the skull, the cerebro-spinal water may outstrip the arterial blood in the race to supply the place. And thus the ventricles of the brain may tend to fill with water more quickly than the substance of it with blood, and so the brain blanches and the person feels giddy. And this is especially the case when a permanent anæmia of the brain has led to an increase in the capacity of its ventricles. Many elderly people with poor circulations feel, during a large part of the day, more or less giddiness from this cause; and if the circulation have become, through age and high arterial tension, very inefficient, every oscillation of the head, even in ordinary walking, will, if the heart's power be at the time rather lower than usual, keep the person distressed with giddiness or a sense of insecurity, which makes him justly rather afraid of apoplexy, but which is a sign of anæmia of the brain. Apoplexy, however, occurs in anæmic brains.

This kind of giddiness, which is characteristic of age with its failing circulation, would keep the human race on all fours during the whole of existence, if it were not for certain beautiful automatic mechanisms, which tend on the one hand to automatically diminish outflow of blood from the brain; and on the other hand to automatically diminish the entry of blood into the brain, so as to automatically keep the brain under all conditions at a certain state of pressure of blood. These mechanisms depend on elasticity of artery and general goodness of venous circulation; and it is the failure of elasticity of artery and the increasing lack of free venous circulation which cause giddiness so readily to appear in old age.

First, as to the mechanism whereby the entry of blood into the brain is automatically checked. For this end, it is necessary that means of lowering the rapidity and force of the current in the vessels should be found. The researches of physiologists have shown of late years that there is such a relation between blood-pressure and the heart's action, that they must infer the existence of a self-acting mechanism which tends to preserve at a certain level the pressure of the blood. The seat of this mechanism is traced by experiment to the neighbourhood of the origin of the pneumogastric nerves. This inferential mechanism is doubtless the more fundamental and effectual of the automatic means of checking excessive entry of blood into the cranium.

But anatomists have hitherto overlooked a very interesting mechanism in the human brain which must plainly tend to check excessive entry of blood, and which, although not the most fundamental, let alone the chief or only means of automatically checking entry of blood, yet I think must be seen as a superadded contrivance necessitated by the large development of the brain in man, and the consequently large volume and large change of volume of blood to be provided for. I refer to the mass of choroid plexus of the fourth ventricle which stretches down round the side of the medulla oblongata enclosed in the lateral offset of the ventricle, which I will call its *cornu*. The cornu, as we will if you please call it, reaches down to and presses upon the roots of the pneumogastric nerve.

I said the cornu of the fourth ventricle presses upon the roots of the vagus. The proof of this pressure is interestingly given in the fact that in early life the cornu is little connected with the nerve-roots; whereas in later life, and especially in cases showing chronic distension of the ventricles of the brain, the vagus becomes firmly united to the cornu of the fourth ventricle. So much is this the case, that you will commonly find, when the brain is removed with only ordinary care from the skull, that the sides of the fourth ventricle are torn open, and the choroid plexuses protrude prominently from them. I have here a specimen of a brain removed in the ordinary way, in which the choroid plexuses are thus exposed. The very slight drag on the pneumogastric nerves, which was caused by the cutting of the roots with a sharp knife, has sufficed to pull open the lateral cornu of the fourth ventricle on each side.*

I would also here draw attention to the fact that the pneumogastric nerve, of all the cranial and spinal nerves, runs perhaps the shortest and straightest course from its origin to its escape from the dura mater. It forms, as it were, the central nerve of the whole system. Above it, all the nerves incline upwards or forwards to their exit; below it, all incline more or less downwards. But this nerve runs a straight, short, tense course; and, as it is fixed at each end, a little pressure must tell on it very directly. An observation of my late friend and colleague Mr. Hilton will serve to illustrate how tense is the root of the pneumogastric nerve, and consequently how subject to mechanical force applied to or about it. In Mr. Hilton's *Lectures on Rest and Pain*, he says: "But I suspect that if, in such cases (concussion of the brain), the base of the brain be carefully examined, the cause of death may be discovered in injury to the filaments of the pneumogastric nerve near the origin from the medulla oblongata. Some years ago, I clearly made out this lesion in a case of concussion which caused the patient's death. The only indication of local mischief which I could discover was the laceration of some of the filaments of the pneumogastric nerve." This observation of a most experienced and most skilful observer, seeing that no other part within the skull was hurt, and the pneumogastric roots were torn, shows in a conclusive way how tensely they are stretched; and, if they are tensely stretched, slight pressure upon them must be productive of not unimportant results.

Now, it cannot be a matter of chance that the roots of the vagus should be stretched so tightly across from origin to dura mater as to be specially picked out for laceration, and at the same time that the vagus alone of all the cranial nerves should receive a peculiar offset from the fourth ventricle; so that the ventricle puts out a finger-like process or cornu packed tightly full of a mass of blood-vessels to reach these stretched roots of the nerve, and to press upon them so hard that, as life goes on, the nerve and the finger-like process become firmly united together. Whatever may be the meaning of such an arrangement, it cannot be thought that it would exist without any signification, or service, or function. No other nerve is thus treated; and the question therefore arises, What are the effects of pressure on this particular nerve? In answer to this question, we may first observe that, when the vagus is stimulated by galvanism, the heart's beat is caused to stop thus.

Again, if the pneumogastric nerve be stimulated by digitalis, the result is to stop the heart in a similar way if the dose is large; but, if the

dose be moderate, the heart's action is not stopped, but only made slower; and, by graduating the dose of digitalis, the heart may be made to go slowly in corresponding degree.

But what will be the effect of pressure upon the vagus? An answer is furnished by an observation made by Czermak. This distinguished physician had a small osseous tumour in his neck; and he was able, by pressing the vagus against this tumour, to stop at will the beating of his heart.

We have, then, a mechanism plainly intended to do something; and what we find the thing this mechanism is calculated to do, is to check the action of the heart. Let us now see whether any beneficial result will follow from such checking of the action of the heart under the circumstances in which the mechanism is put in operation. It is clear that, as the finger-like process is stuffed with tufts of blood-vessel, the pressure it exerts will depend on the amount of blood in the vessels—the more blood, the more pressure; so that it is when the vessels of the choroid plexus become distended with blood that the finger-like cornu which contains the blood-vessels will press upon the pneumogastric nerve and check the action of the heart, and thus prevent excessive inflow into the vessels of the brain. In fact, the contrivance will act exactly like the ball-tap which controls supply to a water-cistern. Now, I am not saying that this is the only means of such check-action. Creatures unprovided with it show an automatic check upon the action of their vascular mechanism. But it is certain that the contrivance I thus describe will tend to control the entry of blood into the brain; and I think its development in the human brain bears a relation to the great size of the brain, and to the extent of its blood-supply, which requires a supplementary automatic check upon the entry of blood. Perhaps, when this check is further developed in the advancement of the race, the balance of activities between the mind and the heart may be still better regulated than is the case at present.

But I shall be asked for proof that, when there is pressure of blood into the brain, the pulse is actually checked and retarded. Evidence is not far to fetch. Let us take a simple case—one which is constant in observed results. Take the variation in rate of pulse when we change our posture from horizontal to upright; and let me cite the well-known and careful experiments of Dr. Guy. This physician found, by an extended series of observations, that the rate of the pulse when persons are standing averages 81; but, on these persons lying down, the rate of the pulse falls to an average of 67. The effect is just as if a moderate dose of digitalis had been taken. Lest it should be said that the increased rate of the pulse in the erect posture is due to the greater muscular exertion of standing, the persons under experiment were strapped to a board, and supported in such a way that they did not use any exertion to keep themselves upright; and yet, when they were caused to pass from the horizontal to the erect position by a revolving movement of the board, the same increase of pulse-rate took place. Now, in the upright position, the blood tends to drain easily away from the brain and reach it with difficulty; so that the choroid plexuses would be less distended, and the pneumogastrics less pressed upon. But, in the horizontal position, the blood goes to the brain more freely; the choroid plexuses are more distended, and the pneumogastrics more pressed upon; thus checking the action of the heart, as if the person had taken digitalis. And, indeed, it will be found true, that causes which involve too great a general flow of blood into the brain reduce the pulse. Thus, as we shall presently see, the effect of going into compressed air is to reduce the pulse. Also the first effect of active influx of blood into the skull in general inflammation of the brain is to retard the pulse. A slow irregular pulse is, of course, one of the most significant of the group of symptoms which usher in meningitis. It is true that, later in the course of this disease, the pulse becomes quick again, and indeed quicker, until it becomes impossible to count it. And it is true that, when this quickening occurs, the brain is still overfilled with blood; and, these things being true, we have to explain why now the pneumogastric is no longer pressed upon and inhibited. The answer is quite clear. Under my care, as I now write, is a child in the clinical ward at Guy's Hospital; it has passed through the early stages of tubercular meningitis, and is in the last stage, with pulse 180 in the minute. I have given, in the last two days, large doses of tincture of digitalis to this child. Now, digitalis has the power of calling out the action of the pneumogastric nerve, and so retarding the pulse. But, in the case of this child, the digitalis does not show the slightest degree of such power; the child's pulse is quicker, instead of slower. It is just as if a large dose of atropine had paralysed the pneumogastric, in which case it is known that digitalis will not retard the heart's action. We have here, then, evidence, through the inefficiency of digitalis, that the pneumogastrics are paralysed; and hence pressure on their roots in the cranium naturally produces no result, any more than do the large doses of digitalis.

* I may commend this very easy laceration of the cornu of the fourth ventricle by a slight drag on the pneumogastric to surgeons for explanation of such cases of fatal concussion of the brain as appear at first sight to offer no anatomical lesion. The shock in fracture of the base is transmitted largely towards the eighth pair of nerves; and this pair is tensely stretched; so that, if there be a severe jarring in the line of the posterior lacerated foramen, a sufficient vibration of the vagus nerve may be set up to tear open the fourth ventricle: also, as to cases of unusually free and large discharge of watery fluid from the ear in fractures of the base, it may be noticed that the damage done to the skull is apt to run through the temporal and occipital bones where they meet, and in the violent laceration and crushing action of the moment the pneumogastric nerve is liable to get a sharper pull than is given it in cutting it with a knife. If, then, at the same time a fissure be made through the dura mater and bones, reaching the tympanum and tearing its membranes—and I have traced such a fissure in more than one case—a direct road is thus opened for the flow of liquid from the fourth ventricle through the external ear.

Such being the means of checking excessive entry of blood to the vessels of the brain, let us now take into consideration the automatic mechanisms which tend to check the exit of blood from the brain. The object is to oppose an automatic impediment to the exit of blood from the cranium, which shall be efficient in proportion as the tendency of the blood to leave the cranium is exaggerated. First of all, we may notice that there is a tendency to disfavour a ready exit of blood from the brain, shown in the fact that, whereas the arteries of the brain enter the base of it, that is, enter the part nearest to the heart, thus receiving their supply with all possible directness; and whereas the arteries of the brain then run upwards over the brain, the veins do not then descend in company with them, as veins generally do, but they run for the most part just the opposite way, ascending towards the top of the brain against the attraction of gravitation. The obvious result of such a disposition of the venous current is to delay the exit of blood; so that, in case of feebleness of the current, that very feebleness would tend to cause the blood to fail to go rapidly upwards against gravitation, to reach the sinuses and escape from the skull. Now Virchow also called attention to this retardation of motion of blood in the cerebral sinuses, especially by the dilatations of calibre and projecting partitions in the walls of the sinuses. And Dusch, with reason, further ascribes a retarding tendency to the peculiar form of the canals of this portion of the venous system, seeing that, by deviating from the cylindrical form, the proportion of the surface by which the blood meets the walls of the sinus to the bulk of the blood within the sinus is increased, so that there is greater friction in proportion to volume.

But another and much more elaborate and efficient check to the exit of venous blood from the brain is seen in the curious opposition between the course of the current in the great veins at the back of the brain, and the course of the current in the longitudinal sinus. Here we shall observe a self-acting mechanism, which bears a most interestingly co-ordinated relation in its degree of efficiency to the degree of requirement which exists in each particular case; for, when the human brain is compared with the brain in allied animal forms, it is seen that the superior intelligence which characterises man is obtained through, or at least with, a backward extension of the hinder part of the brain, or more properly of the cerebrum; and, in fact, this backward growth does actually occur in the development of each human person, as may be seen in these diagrams of the brain in the later months of fetal development. Now observe the effect of this backward growth of the brain upon the course of its veins, and we see that, as the brain goes back, it carries with it the sources of the veins backwards, away from the points of delivery of the blood into the longitudinal sinus which receives their contents; for it appears that the backward growth, alike of the brain of the skull and of the dura mater, takes place chiefly behind the points of junction of the hinder cerebral veins with the longitudinal sinus. The natural result of such an arrangement is to this effect: that, just in proportion as the brain is developed backwards, with the increase of intelligence and with the erect posture which intelligence seeks, just in such proportion is the blood from the newly developed part of the brain sent forwards into the sinus which receives it. But the blood in the sinus, which thus receives a forward current, is itself going backwards, and is in fact the blood coming from the front of the brain. Hence in the sinus there is an opposition of this current from the front of the brain to the current from the back of the brain, which meet and check each other, and so diminish the freedom of exit of blood alike from the front and the back of the brain, and detaining and making less free the rush of blood in the anterior part of the brain, which the erect biped shares with lower animals, and perhaps keeping the animal down in him to some degree; at the same time, staying the blood in the whole increased mass of brain, so as to render any movements in the blood less sudden. In this way, the greater size of brain that induces the erect posture, and goes with a disposition to take up a great variety of postures with more or less brain-activity—this greatly increased brain is kept accommodated with blood by reduction of tendency to outflow of its venous current. In short, the brain is compelled to economise its supply. Such a meeting of the backward current by a forward current must have the same effect as occurs when, at the mouths of rivers, the current of the river is met by the rising of the sea, so that a rise of water in the river is a consequence, as in the production of tides; nay, the obstruction being, by means of one current actively opposing another, so direct, that it may almost be compared to a lock in the course of a stream. The result as to the outflow from the longitudinal sinus will be that, instead of a current from the veins of the brain running with a certain velocity through and out of the sinus, the forward and backward currents neutralise each other in the sinus, and the blood goes out from it, only as an overflow under the pressure existing in the veins at the time—no overflow if no pressure; and, if we trace the effect backwards upon the

state of the veins, we see that it must tend to keep the veins full under all circumstances, locking the current in when it is feeble, and so preventing too ready escape of blood in sudden change of posture, etc. On the other hand, when the current of blood through the brain is strong, the antagonism of the vein-streams would cause them to oppose and lock each other perhaps too thoroughly, so that a scheme of relief is necessary, as a sort of weir or escapement, to allow excess of blood to pass by the lock which there is in the longitudinal sinus. This weir or escapement is provided by sending the blood from the whole of the lower parts of the brain, by means of the inferior longitudinal and straight sinuses, to a point beyond the lock, which I have described; so that there is thus a free exit for some of the blood when the pressure is great.

Now, if we consider the parts of the brain that discharge their blood beyond the lock in the longitudinal sinus, we see that they include the parts more essential to animal life, especially the great ganglia at the base of the brain in the medulla oblongata, pons Varolii, thalami, and parts of the corpora striata. Blood is sent to these essential parts of the brain immediately the arteries enter the skull, so that they get their supply at high pressure, and the blood comes away from these parts of the brain by veins that join the sinuses of exit at points beyond the lock, and near the ending of the sinuses in the jugular veins; nay, some part goes independently through the inferior petrosal sinus directly out of the skull. Thus a free circulation takes place in the vital parts of the brain, whilst those parts which subserve only the relative unessential process of thinking, have their blood-current partially locked, and are obliged to economise it; the free circulation in the vital parts of the brain relieving the block when the pressure is great.

Amongst the parts that receive their arterial supply immediately the great vessels enter the skull, are the choroid plexuses, both of the lateral and of the fourth ventricles; and I have already shown how the over-filling of a particular part of the plexuses of the fourth ventricle acts like a ball-tap in putting a direct check upon the entrance of blood into the arteries of the brain. But a very considerable easement of the stress of blood is provided by the great capacity of the choroid plexuses themselves. These plexuses will contain much blood; and, when the entry of blood into the brain is free, they fill rapidly with it; and, as they lie in the ventricles of the brain, and are bathed by the cerebro-spinal fluid, they press into and expel this fluid into the spinal canal, where it finds accommodation by pressing in its turn on the spinal vessels. The veins from these plexuses form part of the escapement system.

And the locking system which I have described does not act without telling slowly on the arteries which feed it; for these arteries, being relatively unable to empty themselves, as other arteries can and do, have to suffer greater strain; and so in time these arteries, which feed the locked circulation of will and thought, become injured, and the sum of injuries accumulates upon them; whereas the more unimpeded vessels of the base of the brain which feed the parts essential to life, whose veins form the weir, are not so much strained or so much injured, because they are unobstructed. Hence, when at length some over-strained vessel must burst or be blocked, the vessel which so suffers is, in nine cases out of ten, a branch of artery which sends blood to the locked longitudinal sinus, not a vessel which sends blood favoured by gravity to the more open lower sinuses.

Again, I think it is reasonable to reflect, further, that wearing out of the cerebral arteries would affect, first, the arteries of parts whose activity is a matter of education; and, therefore, of will and intelligence; because the activities which are organised through education are, as it were, grafted on the more stable activities that are natural to animal life. And the exaggeration of action which education compels cannot be supposed to be prepared beforehand by the animal development, in the same way as the animal development prepares beforehand a provision for the functions of purely animal life; so that educated activities act through and with a relatively less well-prepared vascular system than that with which the more natural activities are favoured. And hence, when a general difficulty overtakes the whole circulation of the brain, the less stably organised and overstrained vessels of the parts whose function is exaggerated by education, would give way first. In this manner, we may explain why it is that either the distal portion of the upper extremity—the chief subject of education—or else the organisation for speech, one or the other of these is the first injured in trivial, the last to recover in severe, arterial breakdown. I think it is from this cause that the strain on the locked system of cerebral arteries picks out for its earliest victims the most highly educated parts; so that an attack of hemiplegia nearly always lets off most lightly the limbs and parts of limbs which subserve the animal procedure of walking, and the swing of the shoulder and elbow, and so forth, and fastens itself on

parts whose education has spun out and elaborated, and relatively weakened the vascular supply of their department of the nervous centres. These flourishing but not purely natural departments fail, just as you may see often upon grafted rose-bushes, that a more vigorously growing graft, which has flourished grandly whilst the stock was sound, will be first to wither when the stock begins to fail; whereas natural shoots, which before were more moderate in their luxuriance, will commonly survive.

But I am almost forgetting in this digression that I may be asked for some proof that the arrangements which I have described as a lock in the cerebral circulation actually lead to the obstruction and delay of the venous current for which we are assuming they are designed. Now, upon this question there is a very curious and very interesting kind of evidence to show that throughout life the blood is held up at the mouths of the cerebral veins, and this evidence is in the so-called Pacchionian glands or bodies. If we look at this drawing of the venous system of the top of the brain in an elderly person, we see that around the points at which the veins enter the longitudinal sinus there are a number of knots of fibrous tissue, the nature of which has given rise to much inquiry and speculation from time to time. These bodies may be more or fewer in number, greater or smaller in size—may even be so large as to penetrate, and even perforate, the skull by their increase; but they are always there in subjects above three years old, and it is important to notice that they increase as age progresses, and are always attended by a thickened state of the pia-arachnoid membrane around the points where they take their rise in this membrane, and that they always appear about the endings of the great cerebral veins in the longitudinal sinus, and chiefly around the posterior cerebral veins. To understand the signification of these bodies, and of the thickening of the membrane around them, we may reflect that the opposition of the currents in the sinus has the same effect as the meeting of the Thames with the waters of the Channel at the rising of the tide. The consequence in the case of the Thames, or any tidal river, is that a tide is felt for a certain distance up the river, and if it be high it floods the low-lying districts, ceasing to have any influence at a certain distance from the mouth of the river. In just the same way, the quasi-tidal obstruction in the veins tells most near the mouth of the vein, and especially most on the feebler current at the side of the vein, which feebler current receives the latest small branches from the membrane, the obstruction damming up the last small branches entirely, while it merely impedes the flow in the branches that feed the freer current in the middle of the vein. Now, a chronic delay in venous blood tends to cause increase and thickening of the lower forms of texture, such as fibrous tissue. Conspicuous examples both of thickening and of tuft-like growth may be seen on the capsules of the liver and spleen in heart-disease and in cirrhosis. In some cases of very chronic and yet severe obstruction of the mitral valve, I have seen the serous covering of the liver thickened and crowded with bodies which were exactly like in all points to the "Pacchionian glands". It was this observation, in fact, that suggested to me the explanation of the Pacchionian glands that I am giving, which is that, around the mouths of the cerebral veins in the tidal area, if I may so call it, arise proofs of protracted delay of venous blood, in the form of thickening of the fibrous membrane and development of fibrous tufts, which are the Pacchionian glands whose causation has long been a puzzle to observers. No such bodies are ever found about the points of exit of the veins at the base of the brain.

I will ask you now to direct your attention to the last particular of the anatomical part of my subject, and to consider the curious opening at the apex of the fourth ventricle, which forms the means of communication between the interior of the cerebral ventricles and the subarachnoid space of the spinal canal. It is through this opening that the cerebrospinal liquid is driven or drawn to or from the cerebral ventricles by the action of the choroid plexuses, and thus a principal part of the escapement system of the cerebral circulation is put in operation. None of our works on anatomy bestow more than a trivial and passing notice on this opening. The edition of *Quain's Anatomy* from which I learnt, spoke of the opening as "generally" present, and the last edition merely says, on the authority of Magendie, that there is such an opening. But surely throughout the entire frame, and amongst all the wonderful adaptations by which the liquids and solids of the body are actuated to their mutual duties, there is no spot more worthy of close attention than this curious aperture of escapement, situated immediately above the very centre of life, and of essential service to that centre. Shall I be forgiven if I take time to describe the anatomy of the part with some care? It deserves the attention of the physiologist, even if only because the sudden change of character of the surfaces which communicate at the opening is as remarkable as the difference of the surfaces which meet at the fim-

briated opening of the Fallopian tubes. For the interior of the fourth ventricle is lined at the floor and sides with ependyma, or proper lining of cerebral ventricles, and this has upon its surface a layer of thick close-set epithelium, whose cells are of irregular cubical shape, and very conspicuous, whereas the subarachnoid space is entirely without lining membrane or epithelial covering. The change from the one character of surface to the other occurs very abruptly, the epithelial cells breaking into groups and disappearing as you trace them from the ventricle towards the space. The opening is in the middle line, and is another place of non-union of symmetrical parts: it is situated exactly over the spot where the central canal of the spinal cord joins the fourth ventricle. It is a hole with edges well defined at the sides and below, but is entirely without any definiteness above—in fact, when you are proceeding from the subarachnoid space towards the fourth ventricle, upon the roof of the ventricle you will find that there are no signs of a boundary at all. And indeed the matter is left curiously to chance, for in some subjects the choroid plexuses of the fourth ventricle wander out nearly an inch into the subarachnoid space in the most regardless manner, whilst in others they keep themselves well within the bounds that would correspond with the limits the opening reveals at the sides and floor of the ventricle. The line of demarcation between the subarachnoid space and the fourth ventricle is more than indefinite upon the roof of the ventricle; indeed, it does not exist, and the roof of the ventricle is in fact organised after the manner of subarachnoid space. This space runs forward on it to reach the valve of Viessens—in other words, the subarachnoid space is in open continuity with the fourth ventricle all along the roof of the ventricle until you reach the valve of Viessens, where a proper ependymal surface appears for the first time as you trace forwards.

The delimitation, then, between the fourth ventricle and subarachnoid space concerns only the sides and floor of the ventricle, and is entirely indefinite above. As to the sides of the ventricle about this opening, they are composed of a double membrane, the outer of which can be separated from the inner. Here is a drawing of the inner membrane after the outer has been removed. The outer membrane is the pia mater, springing from the edge of the calamus scriptorius, and reaching the under surface of the cerebellum, where it loses itself, or, at least, I always lose it. The inner membrane is a layer resembling the valve of Viessens, but thinner. It is easily lacerable, like cerebral tissue, being composed of the felted fibrils of ependyma, with a few nerve fibrils, and its inner surface is coated with the thick epithelium which I have described. It rises from the edge of the calamus scriptorius, and has pia mater at its outside, but separable from it, as I have said, and it rises up to reach the edge of the choroid plexus in the roof of the ventricle, beyond which it cannot be traced, except that it sends its epithelium over the ventricular face of the choroid plexus, but not beyond this, however, so that it does not reach into the middle line of the roof. At the verge of the opening itself, the pia mater extends in a fibrilated form one-twelfth of an inch or so beyond the free edge of the ependyma, and it is on this one-twelfth inch that the epithelium of the ependyma disappears as I have described.

I examined the opening in upwards of seventy brains, removed with especial care for the purpose, and taken from subjects at different periods of life, some very early. Thus, in a fetus 2.6 inches long, which I took to be two months old, I found the three membranes—dura mater, arachnoid, and pia mater—quite distinct, and the opening of the fourth ventricle already evident, but its relations to the cerebellum and medulla oblongata not definite on account of the undeveloped state of those parts. The choroid plexuses of the fourth ventricle were already very well developed. At yet earlier periods I found the thalami and corpora quadrigemina protuberant on the surface of the brain, uncovered by the cerebrum, so that the membranes which even here were distinct had no relations to these parts comparable to their relations in the developed brain. I observed, however, that the membranes of the brain develop very early. I have found a fringed opening of the fourth ventricle in the brains of dogs and of chickens. I could not succeed in frogs, the films of membrane were so thin, and I could not be sure of more than two membranes in the spinal cord of these animals.

At the fourth and fifth months of human foetal life the opening is quite obvious, the lips of the orifice well formed, but the edges slope from below upwards and forwards, instead of upwards and backwards, as in the adult. This appears to prove that the backward growth of the cerebellum is greater than that of the pons and medulla after the fifth month, so that the upper attachments of the edge of the orifice to the cerebellum is moved back with the growth of the cerebellum. This backward growth of the cerebellum carries back the tail of the choroid plexus often far beyond the line of the edges of the opening; the choroid plexus seems thus stretched back in linear form, to prevent its obstructing the opening.

I trust I may be pardoned this long account, because of the surprisingly slighting way in which the anatomy of this most vitally essential part of the human organisation is treated by anatomists. Not long ago, a Guy's student was seriously reprehended by an examiner at the Royal College of Surgeons for speaking of a choroid plexus in the fourth ventricle. He was informed that he ought to know that the choroid plexuses are in the lateral ventricles.

Now, considering that a large and free communication between the ventricles of the brain and the spinal canal through the end of the fourth ventricle is universal in all warm-blooded vertebrates, and is absolutely required in order to allow variation in the quantity of blood in the skull, and to adjust the pressure with which blood bears upon the brain; and, considering that our security in changes of posture, and even in the less active matter of thinking, depends largely upon this communication, I cannot help suggesting that a little more notice should be taken of it, and a little more attention given to the facts concerning it. The admirably clear and instructive text-book of the Cambridge School of Physiology does not deign to touch the curious mechanism of the varying blood-supply to the human brain, although it treats very fully of the apparent intelligence with which a frog deprived of his cerebrum will scratch himself when tickled, and will seem to take care lest he should be upset when tilted about. Meantime, I am not aware of any reasons to believe that consciousness and will must inhabit homologous parts of the brain in remote classes of vertebrates, or that light is thrown on the special functions of the human brain by experiments on frogs.

When the head is inclined downwards so as to be on a lower level than the trunk, and especially when anyone stands on his head, a sensation of great discomfort is produced, because the cerebro-spinal fluid, which is protective in natural postures, now reverses its operation, and forms a column of liquid of some length directly pressing into the ventricles of the brain, adding to the pressure which the blood produces by escaping with difficulty from the head, so that water as well as blood press forcibly upon the brain. I have never known anything more than discomfort occur from this cause; but it is well to remember the cerebro-spinal fluid when, in chloroform-syncope, etc., one is tempted to cause a flow of blood to the head by lowering it below the level of the body; for, in so lowering the head, you delay venous circulation, and send water as well as blood towards the brain, thus lessening the blood-changes there which you wish to increase.

To show how important it is that the opening in the fourth ventricle should be free, let me shortly describe the case which first fixed my attention on the results of its absence or closure. A boy of sixteen had always been a rather slow, dull boy, forgetful and hesitative, without being at all idiotic or utterly incapable. He used to go to his father's warehouse, and amuse himself in his slow dull way, without being of much use. This boy was one day not very well; but his illness appeared trivial. He stayed at home that day, and the next day went to bed early. He became worse in the night, being faint and weak; and a relative went to his bedside; then, quite unexpectedly and rather suddenly, he died. I had not seen the lad during his life, but was asked to inspect the body. Nothing whatever of a morbid kind was found in the viscera of the trunk; but, on raising the calvaria and removing the dura mater, a very curious appearance offered itself. A large bladder, just like an hydatid, was in the right cerebral hemisphere, separating the convolutions from each other; it was about two inches in diameter. I naturally thought it was an hydatid; but, on opening it, I found it extended deeply into the brain, and, in short, it proved to be an immensely dilated lateral ventricle. The ventricle of the other side was almost as much dilated, but did not reach the surface. The *post mortem* examination was made in a private house, and under the usual difficulties. I therefore took away the parts around, and including the fourth ventricle. I hardened these, and examined them. There was a growth of papilloma; in fact, a wart starting off the surface of the fourth ventricle near its apex, and running upwards to touch the roof, almost completely closing the opening of the ventricle, and stopping the way of liquid out of it. The sudden death in this case, and in several other cases of intraventricular dropsy of the brain which have come under my observation, I believe to be due to inhibition of the heart through pressure upon the pneumogastric centres.

Having thus, at what, I much fear, will have seemed tedious length, described the mechanisms whereby variations in the quantity and in the pressure of blood within the cranium are provided for, I will now proceed to inquire whether these mechanisms are of due efficacy; and I hope to show that the provisions against undue pressure of blood within the cranium will be found so far efficient that, whenever intracranial blood-pressure can be separated from asphyxia and strangulation, such pressure does not produce any ill result. In other words,

congestion of the brain, as distinguished from strangulation, does not produce symptoms; and those symptoms which are supposed to be due to congestion of the brain as a distinct disease must be referred to other causes.

But, before beginning the course of my argument, may I make clear the position intended to be taken up? I wish to contribute towards doing away with a fallacious notion which continues from one author to another, as work succeeds work on practical medicine—the fallacious notion that a quantity of blood forced into the vessels of the brain becomes itself the cause of several serious and even fatal diseases, which are described under the name of “congestion of the brain”, although in such cases there is no mechanical obstruction to the exit of venous blood from the cranium. I wish to show that no quantity of blood that can be forced into the vessels of a brain whose veins are not closed by obvious pressure does, as a matter of fact, produce any such symptoms. I believe this may be proved beyond dispute. I should like to make it clear that in doing this I am proceeding in harmony with the great principles enunciated by the greatest of pathologists, the illustrious Professor Virchow, who, with vast learning and keen original observation, showed that we are not to look to the vessels for the initiative in diseases of the textures of the body. He proved that the life of texture is in the texture itself, its pathological as well as its natural life; and that the movements of the blood in what is called irritative or inflammatory congestion are secondary to a demand upon the blood and blood-vessels, which demand is made by the proper power of the texture-elements. And these elements have power to grow, and to multiply, and to exert themselves in function, dependent on the blood only, as the labourer is dependent for his powers on his bread and cheese. And when I say that the forcing of blood into the vessels of the brain produces no symptoms, I do not mean that no symptoms will attend a fulness of the vessels when that fulness has been caused by disturbance of the brain's texture itself. There will be symptoms, but symptoms caused in the texture by the very irritation that drew the blood to the brain, and not to be ascribed to the blood that is drawn thither.

There is one power by which blood can be sent into the vessels of the brain with certainty and with any degree of force that may be desired, and that power is atmospheric pressure. It needs no experiment to show that great increase of atmospheric pressure must drive the blood away from the surface of the body, and into any parts that are accessible to blood and not to air: such parts are the interior of the cranium and spinal canal. Even moderate degrees of pressure, such as an addition of half or three-quarters of an atmosphere, have been noticed by Pétrequin and Sandahl to cause the disappearance of a tolerably considerable hyperæmia of the conjunctiva; but such minor degrees of pressure have had no ill results upon the nervous system. Although such results have been carefully watched for in air-chambers constructed for the relief of asthmatic persons, the result, as given by Sandahl, who carried out observations on an extended scale, is thus described: “Upon the nervous system, the pressure seemed to exert an indirectly soothing effect; whereas, on sudden reduction of the pressure, nervous phenomena made their appearance”. The only marked consequence of the pressure was retardation of the pulse.

But, in fulfilling some of the demands of civilisation, and especially in the construction of bridges over great rivers, workmen have had to go to great depths below the surface of the water. And the human frame, in some instances, has been exposed to degrees of pressure vastly greater than any that could be applied in the therapeutic air-chamber. The effects produced by such great degrees of atmospheric pressure have been recorded by many observers in numerous memoirs, one of the best of which is in the *Annales de Chimie et de Physique* for 1841, by M. Triger, who worked at a depth of sixty feet in the valley of the Loire; but the greatest depth which I can find to have been reached was upwards of ninety feet, in the construction of the great bridge across the Mississippi at St. Louis. A good account of the effects of the tremendous pressure at this great depth was published by the chief engineer, Captain Eade, and is printed in the *Medical Times and Gazette* for 1871. Like all who have witnessed and experienced the effects of such pressure, Captain Eade dwells long on the severe pain in the ears which is caused by the force exerted upon the tympanic membrane until air enters by the Eustachian tubes into the tympanic cavity. No other result is noticed until the Captain speaks as follows: “When the depth of sixty feet had been attained, some few of the workmen were affected by a muscular paralysis of the lower limbs; this was rarely accompanied with pains, and generally passed off in the course of a day or two. As the penetration of the pier progressed, the paralysis became more difficult to subdue; and in some cases the arms were involved; and in a few cases the sphincter muscles of the bowels. The patients also suffered much pain in the joints when the symptoms were severe; an average of at least nine out of ten of

those affected suffered no pain whatever, but soon recovered, and generally returned to the work... The total number of men employed in the air-chamber of this pier was 352; of this number, about thirty were seriously affected... twelve of the cases proved fatal... nearly or quite all of these deaths happened to men unaccustomed to the work. Several died who had worked but one watch of two hours... In contrast to this, is the fact that a very large number of the men (certainly one-half of those constantly employed) commenced with the work at its inception, and remained throughout its continuance entirely without injury or inconvenience..... Considering that the air-chamber was briefly visited by thousands of persons, including many delicate ladies, even after it had reached the bed-rock, some remaining as long as an hour in it, without any of them experiencing the slightest ill effect from the pressure, and that no cases of any importance whatever occurred amongst the workmen after the watches were reduced to one hour, Captain Eade feels satisfied that this is the true cause of the paralysis; and that, by lessening still more the duration of the watches, a depth considerably greater can be reached without injury to the workmen. Too long a continuance in the air-chamber was almost invariably followed by symptoms of exhaustion and paralysis." It is necessary to note that the symptoms appear only after re-entry to the ordinary atmosphere. It is necessary to note that all the accounts of this extreme pressure agree in showing that no ill results appeared during its continuance, and that the grave and sometimes fatal symptoms which occurred on its removal affected the lumbar portion of the spinal cord, producing paraplegia, which would not correspond to lesion of the brain; in fact, the brain showed a strange immunity from injury under this great pressure.

My next step will be to endeavour to estimate the extent to which blood is forced into the brain during exposure to the atmosphere of the caissons, that I may compare this forcing of blood into the brain with such stress of blood as the intrinsic powers of the circulation are able to exert upon the brain. For this purpose, we will reflect as follows. Each act of ordinary breathing exerts a force of about forty-two millimètres of mercury. Now the effect of each breath is to increase the arterial pressure to a degree equal to at least twice the pressure caused by one beat of the heart. But when the body is exposed to atmospheric pressure of forty-two millimètres of mercury, blood is driven into the closed cavity of the skull with as great force as air drives blood into the chest in ordinary breathing. Hence within the cranium there would be for each forty-two millimètres a rise of pressure as great as that expressed by two beats of the heart (really more, because the pressure in the carotid, which I have taken as the standard of comparison, is the whole pressure within the chest, lessened by the direct pressure on the arterial blood). But the pressure at ninety feet depth of water is about fifty times forty-two millimètres of mercury; so that the intracranial pressure produced in the caissons is as great as that of one hundred heart-beats.

Now, if we observe an inhibition trace, to see how many heart-beats restore the normal tension after it has been brought down by inhibition to the line of no tension, we see that about ten heart-beats suffice to bring up the full normal tension. And the atmospheric pressure of the caissons was equivalent to 100 heart-beats, so that the caisson-pressure would raise normal arterial tension ten times. But, as to the highest morbid arterial tension observed with the sphygmograph, Drs. Burdon Sanderson and Galabin, kindly answering my inquiry, say that nine to ten and a half ounces make the highest morbid tension they have recorded, which is about three times the normal tension as observed with the sphygmograph. But the caisson-tension is ten times the normal, and hence is three times the greatest abnormal tension from intrinsic causes. So far as this comparison is valid, the general conclusion is, that the caisson-pressure raises the whole vascular tension within the cranium by an amount which is as great as three times the greatest morbid arterial tension that has been observed to be due to the intrinsic powers of the circulation.

I dwell upon this kind of congestion of the brain, because the flow of blood to and from the brain is meantime free, and this congestion is one purely due to fulness of the vessels; so that we have an active congestion of the brain affecting only the condition of the circulation, and not starting in any variation of behaviour in the nervous tissue. In this example, we can consider the effects of pressure within the vessels apart from the conditions of strangulation, which condition has had a very detrimental effect, as lending a seeming support to assumptions of independent congestion of the brain. And I have thus endeavoured to show that the pressure to which Captain Eade's workmen were exposed must have had the effect of filling the vessels of the brain with a current of blood under greater stress than would be possible through any action of the intrinsic forces of the circulation, whilst normal entry and exit of blood from the brain was allowed. Whether the compari-

son of pressures I have made is valid or not (and I am aware that there are questions regarding the respiration-curve in the manometer-tracing which are unsettled), it still remains true that the effects on removal of the pressure are so peculiar and terrible as to show how great is the degree of stress from which these symptoms form the recoil.

It is evident that the physical conditions of the circulation in the nervous centres, upon removal of great atmospheric pressure, are unknown, and probably very complicated; and I will not attempt to speculate upon the question of their nature, but only ask you first to observe that, whatever may be the cause or causes of them, they affected only the lower end of the spinal cord; and, second, to observe that the tremendous positive pressure into the vessels of the brain did not produce upon it any effects whatever. Under the positive pressure, there were no effects; and, under the unknown conditions arising through sudden relief from pressure, the tip of the spinal cord was the part which showed itself to be suffering.

I will ask you to remember the negative effects of the great pressure of blood into the brain for the argument of my next lecture, and now will digress for the remainder of this lecture to inquire why the after-effects of atmospheric pressure should involve only the lower part of the spinal cord. And then I will point to some inferences from my explanation, which may have a bearing on obscure points in clinical medicine.

Here I must take the liberty of drawing your attention to some very well-known anatomical points, by which the curious fact that only the legs suffer in the caisson disease is, I think, quite clearly explained.

I need scarcely remind you that, whilst the spinal canal extends down into the sacrum, the spinal cord does not extend below the lower border of the first lumbar vertebra. But the spinal cord gives off nerves all the way down on each side, and the spinal canal on its part is pierced on each side with a series of openings all the way down, even to the lowest part of it, at pretty even intervals; the openings serving to let out the nerves that arise from the cord. The consequence of this arrangement is, of course, that the distance between the place of origin of the nerves of the cord and their place of exit from the canal is short as to the upper nerves, but increases much as to the lower nerves, so that the nerves run nearly horizontally to the neck and upper limbs; whereas, to the loins and legs, they run a long way down before reaching the dura mater, extending, indeed, for many inches, and forming a bunch of long loose threads, something like a horse's tail, and known as the cauda equina. Now the spinal cord is suspended within the spinal canal in subarachnoid fluid, which entirely insulates it. And meantime, surrounded by this liquid, and insulated by it, the spinal cord itself is out of reach of any blood-supply, except such as can come to it from the brain above, or else along the nerve-roots at the sides. And, in fact, the supply of this important part becomes, if I may so speak, one of nature's difficulties. Let us see how the difficulty is met. The blood-supply to the spinal cord is carried out by slender vessels which come from the vertebral arteries within the cranium. There are three of these arteries, one on the front, and two on the back of the cord; they are very slender, and yet have to run along its whole length. No arteries so small as these run so great a length elsewhere in the body, and pressure falls rapidly in minute arteries as the length of pipe increases, so that it becomes necessary to reinforce these slender vessels wherever possible, and advantage is taken of the nerve-roots to send up little reinforcing arteries along these.

In the part of the cord corresponding to the neck, upper extremities, and trunk, where the nerve-roots are short, the reinforcing arteries are also short, and they reach and join and furnish blood to the spinal arteries; so that, in this part of the cord, every segment of it is supplied with blood from two directions, the anterior spinal artery bringing blood from above, and the reinforcing artery from below. But, when you approach the tip of the cord, the supply from below becomes exceedingly precarious, and even apt to fail entirely, because, upon the long strands of the cauda equina, the small arteries are too narrow and too long to reinforce the cord with any certainty; but, at the same time, the supply from above has to be furnished with greater difficulty than in the upper regions of the cord, because the original anterior spinal artery is very far away, and the reinforcing arteries, even in the lumbar region, have to run considerably longer courses than they had in the cervical region.

Thus the tip of the cord has its blood-supply only from above, and deficiently even there; whilst the upper parts of the cord have a better sustained supply, both from above and from below; and this becomes especially the case upon the cauda equina itself, for here the arteries are exceedingly minute and uncertain in size on the several nerves. Hence we see that the tip of the spinal cord, corresponding to the lower limbs and sphincters, is much more weakly organised as to its circulation than are the upper parts of the cord.

But lest this should seem a purely theoretical view, let me quote a report which Dr. Carrington, the able demonstrator of anatomy at Guy's Hospital, has kindly sent me after examining this matter for me. Dr. Carrington says: "I have examined seven cords in all, and send you the results. I have also written out the account of two which were the best of my specimens in a little more detail; and I can send you a like account of all if you wish it. 1. Our subjects are injected from the femoral artery; therefore, *à priori*, there would be a greater probability of the lower than of the upper end being well injected. The fact that it was not the case makes the results more striking. 2. In all the cords there was clear evidence that the anterior spinal artery filled from above downwards, the injection ceasing either at the lower end of the cord or above that part. It was obviously reinforced by vessels which entered along the nerves. 3. In no case were the reinforcing vessels on the nerves of the cauda equina injected. Details: Case I.—Anterior spinal artery injected in upper one-third, injection ceased at junction with middle one-third; the injected part was reinforced by branches which ran along the nerves of the upper one-third; none of the reinforcing branches were injected in the lower two-thirds on the anterior surface. On the posterior surface, the arteries which accompanied the nerves were more or less injected as far as about two inches from the commencement of the filum terminale; many were, however, uninjected. No injected arteries accompanied the nerves of the cauda equina. Case II.—Anterior spinal artery particularly well injected as far as the beginning of the filum terminale; beyond this point, the vessel could be traced empty on the filum. The injected part of the vessel was well reinforced by vessels which ran on the nerves; on the posterior surface, the arteries along the nerves were injected with varying degrees of fulness, some were uninjected as far as two inches from the end of the cord, at which point the injection ceased. The largest and lowest of these reinforcing arteries accompanied a nerve which ran for a distance of one inch within the dura mater. No injected arteries were found with any of the nerves of the cauda equina, but, *per contra*, on one of them a well marked long slender artery was found empty."

We have here convincing proof, that, under difficulties of circulation such as must arise when the body is subjected to great changes of atmospheric pressure, the tip of the spinal cord is a part which would suffer from anæmia; for it is out of reach except by vessels of great length in proportion to their calibre. Meantime, the career of these vessels is through the cerebro-spinal fluid, which forms an easy water-bed for them, that may favour the freedom of flow in their minute channels. But then, unfortunately, this cerebro-spinal fluid is itself subject to variations of pressure. Indeed, it exists as a means of conveying pressure from the cranium to the spine; and we have seen that, when there is activity of circulation within the skull, the cerebrospinal fluid is sent out to press a little hardly within the spinal canal on these smaller arteries of the spinal cord. Hence overmuch activity of the cerebral circulation would tend to compress these arteries, and interfere with the straight and narrow economy of the unfortunate end of the spinal cord. I think this is why very studious people become delicate about the caudal ends of their spinal cords; and it is, at any rate, a good reason why students should take care to let their legs have a fair share of activity every day for establishment of proper circulation in the spinal centres.

Now, whilst the tremendous pressure in the caissons is sending blood into the vessels of their brains and cords with great force, the workmen feel more lively and elastic, and go up their ladders with ease. But as soon as this tremendous pressure is taken off, the vessels of the nervous, instead of being intensely stuffed with blood, have a tendency to lose their contents in the same way as they do when exposed to a rarefied atmosphere; so that a general deficiency of supply arises in the previously well-filled channels: this difficulty tells most on the more weakly organised system of supply to the spinal cord, and hence the workmen are paralysed as to their lower extremities and sphincters when they go into the air.

But how, then, should any people escape? Have I not proved too much? As to this question, I have observed that the anastomotic reinforcing branches of artery along the nerves vary in size upon the several nerve-roots in different subjects, and a large anastomotic branch in the dorsal region would do much to save the cord. Also, it is true that the vascular tension is more efficient in some people than others; and, as a matter of fact, we can directly observe that some people's textures bear variations of blood-supply much less equally than do the textures of other people.

Further, I think it probable that, when the arterial supply of the cord fails under the caisson-pressure, the result is paralysis; but when the result of the pressure tells more on the veins, keeping them too full, the result is that pains in the limbs are complained of.

But when I thus am indicating this peculiar source of weakness in the spinal cord at its lower end, it will naturally be asked whether there is any further proof of such a weakness of this part, whether the

cord is paralysed under other conditions very trying to its peculiar circulation? Well, the answer to this question is not far to seek, seeing it is an otherwise, and I may say a hitherto, unexplained fact, that, in many cases of spinal palsy from whatsoever cause, the lower limbs suffer while the upper limbs escape: whereas, so far from the lower limbs being intrinsically weaker, it is of course true that in cerebral paralysis the lower limbs suffer less and the upper limbs suffer more. Let us take the example of common paraplegia. Everybody knows that this nearly never attacks the upper limbs, but it affects the lower extremities and lower part of the trunk, and a short experience of cases of this disease will show a remarkable uniformity in the extent to which the lower half of the body is involved: you will find almost always that the paralytic affection in common paraplegia ceases to give evidence of its presence about the lower dorsal or upper lumbar region. Numbness and paralysis are not traceable above the lower margins of the thorax, ceasing often along a rather well-defined line. The anatomical state of the substance of the cord found *post mortem* to account for this is a condition of "softening"—*i.e.*, a pathological state which is the same as that found in the brain as a consequence of impairment of nutrition by deficiency of the supply of blood. The causes of this condition are often obscure, but are in many cases evidently debilitating causes, such as in this very typical case which I met with. An Irish dock-labourer had been standing about the entrance of the docks for some days without employment, and getting little food, when he felt pins and needles run up and down his legs, and after he got home that evening he felt very weak and went to bed. In the night, being taken with diarrhoea, he went from his bed feeling very cold and feeble, and he could not return, being totally unable to move his legs. This man lived about a year, his disease showing all the characters of common paraplegia, and *post mortem* examination showed the lumbar cord in a state of "white softening." This was one of my cases when I was clinical clerk, and it puzzled me much to know why the poor man should thus suddenly suffer from so chronic a change as white softening, and why he should only have symptoms just up to the top of the lumbar cord, and never higher, in the whole course of his ailment; but I seem to know well now why it was. He was paralysed through starvation of his cord, on account of the difficulties of supply to it, and the paraplegia did not extend above the lumbar cord, because the cauda equina does not extend above it, and the upper dorsal part of the cord obtained enough blood along short arteries, through which the blood-pressure had been able to force a sufficient supply. Indeed, the very different kinds of cause which agree in producing paralysis of the lower extremities would show that there must be some such simple and general explanation of its ready occurrence.

An instance came under my observation so like that of my poor dock-labourer, that the similarity of effect was as remarkable as the unlikelihood of causation. It was in the person of a very well-fed and healthy female cat, which suffered, I am sorry to say, at my hands. A nest of thrushes was eaten by cats in my garden, and I thought it fair to retaliate by putting down a bowl of milk, also containing prussic acid, and I warned a relative to keep her cat in; but, to my grief next morning, the only cat in trouble was my relative's fine cat, which lay beside the bowl of poison in a curious state, quite paralysed as to the hind legs, but otherwise unaffected. The front paws were active and vigorous, so that she could pull herself about on the level ground by means of them; but the hind legs did not make any movements for three weeks. I did what I could for her, but it was four weeks before the cat could leave the basket in which she was placed, and four months elapsed before she could spring upon a wall; after this, she quite recovered, and had several large families. Now, prussic acid acts by lowering the tension of the blood, after a slight temporary rise; and, whereas it is very difficult to suppose that the prussic acid had any preferential power over the nervous centres of one pair of legs, as compared with those of the other pair, it is, I think, quite readily to be accepted that, as the hinder pair draw blood through longer and very slender pipes of vessel, the cat, lying with depressed circulation, nearly dead all night, suffered so much in the nutrition of the end of her spinal cord, that it became almost irrecoverable. Indeed, it is interesting to observe how the very temporary action of the poison should have so damaged the cord, that it was six months in recovering, whilst the damage was so adjusted that recovery was finally possible. Such an occurrence throws some light upon the curiously protracted tenacity of injury on the part of the cord, which the history of paraplegia brings so painfully forward. But it is not only prussic acid that affects the hind legs of creatures more than their fore legs. Although toxicologists have paid no attention to the difficulties of circulation in the lower end of the cord, so that no experiments have been made with the intention of developing this point, yet, in reading through a work on *materia medica*, one finds repeated examples of drugs which

are depressants of the circulation, and which paralyse the hind legs of animals without affecting their fore legs. Among these, I mention aconitia, conia, and, doubtfully, veratria and chloral; with reference to prussic acid, it is said to benumb the hind legs first.

Again, for other instances, take the very complete paraplegia of the lower extremities, which so much impressed Dr. Bristowe in the highly interesting cases of rupture of abdominal aneurysm which he recently published. Surely, if we consider the mode in which the arterial supply of the spinal cord would be reduced by the bursting of the aneurysm, the paraplegia in these cases is immediately comprehensible. Again, if we take the so-called urinary paraplegia, or functional paraplegia of any kind, including what is called hysterical paraplegia, we find these affections never troubling the upper limbs, but always the lower. And I think the point I have raised will suggest lines of investigation that may throw great light on this obscure class of nervous diseases. I believe that it is by impediment to the exceedingly and peculiarly difficult blood-supply of the caudal end of the spinal cord that all these various conditions lead to paralytic weakness of the lower limbs, and they are to be met by conditions improving the circulation, if possible.

Why the lumbar cord should be thus straitly supplied, is an interesting question. Perhaps it may be connected with a necessity that the economy should be so organised as to feel fatigue early enough in the parts which directly put forth exhausting exertion, lest strain of effort should first overtake more vital parts than secondarily share in all such exertion. In short, the cord is supplied scantily, so that we should sit down when we have walked enough, and lie down when we are tired; and, especially, that we should, if necessary, fall down when faintness overtakes us, to the end that, in the horizontal position, the circulation may be able to restore itself.

THE NATURAL HISTORY OF LEUCODERMA.

THE natural history of the rarer skin-diseases has been as yet but scarcely half investigated. How they develop themselves, and what they are in mid-course is well known; but what ultimately becomes of them has evidently been as yet insufficiently studied. A pungent proof of this was afforded by a recent debate at the Clinical Society (at which I was present), when competent men who had for long devoted their special attention to the study of skin diseases were found arrayed against one another in about equal number on the question, what ultimately becomes of keloids? some declaring that they always disappeared spontaneously; and some that they absolutely never disappeared spontaneously. In fact, it was clear that the majority of those who spoke did not know whether it was one way or the other.

As regards leucoderma, I venture to say our knowledge is not very much better. I therefore beg to contribute a well authenticated fact respecting it. My old schoolfellow H. B., has lately returned from India. His judicial and administrative functions had brought him in contact with a fairly considerable number of lepers. This had led him to take an interest in leprosy, and so, also, in the skin-diseases with which it is liable to be confounded. So strong a leaning has my friend H. B. for medical studies, that he almost thinks of sacrificing a high lay position for the sake, even now, of turning doctor. His medical knowledge is considerable, but his acquaintance with the subject of skin disease is superior to that of the majority of practitioners, I therefore accept his account of himself. He informs me that he had leucoderma, and that his leucoderma had spontaneously and completely vanished within a fairly short period. His age is forty-five. His leucoderma affected the backs of both of his hands (a favourite position for it), but affected no other part of him. It appeared when he was thirty-eight. It had disappeared completely by the time he was forty-four. It waned more rapidly than it had waxed. It did not affect his wrists or his fingers, but only the backs of his hands. It consisted, on each hand, of a single milk white patch of the aggregate size of a crown piece, and having a sinuous margin. The brown areola was sharply defined on its inner margin, but faded gradually off at its outer margin. It is clear that my friend had leucoderma. It is equally clear that his leucoderma disappeared spontaneously within a period of six years. This fact is interesting to know, but it would be of more interest to learn whether this is the usual course pursued by leucoderma. The inquiry is a difficult one, because those who would be most likely to take interest in the question, have the least opportunity of following up their cases. The most likely way of solving the point seems to offer itself in the inquiry "Do piebald negroes or piebald Hindoos ever cease to be piebald?" It ought not to be a difficult matter to come at that.

As we do not know how to cure leucoderma it would be an advantage to be able to tell our patients, if we can, that it will go away of itself even if they have to wait six years for it.

BALMANNO SQUIRE, M.B.Lond.

THE GULSTONIAN LECTURES

ON ANÆMIA.

Delivered before the Royal College of Physicians.

By SIDNEY COUPLAND, M.D., F.R.C.P.,
Physician to the Middlesex Hospital.

LECTURE II.—PART I.

Idiopathic Anæmia: Earliest Recognition of it by Addison—Records of Cases (1851-1871) by Barclay, Lebert, Wilks, Habershon, King—Gusserow—Biermer's New Departure (1871)—Growth of its Literature in the last Decade—Wide Extension given to the Term "Pernicious"—Clinical Features of "Pernicious" Anæmia—Onset—Progress—Digestive, Circulatory, and Nervous Symptoms—Hæmorrhage—Irregular Pyrexia—Duration—Termination in Recovery—In Recovery followed by Relapse—In Death—Post Mortem Appearances.

MR. PRESIDENT AND GENTLEMEN,—It is impossible to speak of idiopathic anæmia without reference to Addison, for he it was who seems to have distinctly called attention to a class of cases in which anæmia is the prevailing symptom from first to last—an anæmia not depending upon recognised causes, not associated with wasting of the tissues, but insidious in its onset and slowly downward in its progress, revealing after death no changes but those of fatty degeneration, especially of the heart. The passage in which he summed up his experience of this condition occurs early in his essay on Disease of the Suprarenal Capsules.* This passage has been so often quoted, that it is hardly necessary for me to repeat it now, much as I am tempted to do so, for it is a model of a succinct summary of clinical observation. It was written in 1855, but Addison's observations dated far before that period, and indeed he described the condition in his lectures as early as 1843.† There is no doubt also that similar cases had been observed, and a few recorded, about the same period. In 1851, Dr. Barclay published the notes of two fatal cases of anæmia (*Medical Times*, 1851, vol. i, p. 480) occurring in St. George's Hospital. One was the case of a man aged 34, admitted under Dr. Page on January 21st, 1852. He had been ailing for twelve months, and had suffered from inconstant aching pain in the loins, accompanied by scanty loaded urine. He had lately lost flesh and had a slight cough, was remarkably pallid, complained of shortness of breath and palpitation; pulse 120, and very weak; and he suffered from constipation. There was deficient breath-sound at both apices, especially the right, where it was distant and tubular; there was then no cardiac murmur. A fortnight later he was attacked by vomiting. A bellows-murmur with the systole developed, best heard in the mid-præcordia and towards the apex. He rather rapidly passed into a state of exhaustion, ending in coma, on February 11th. Mr. P. Hewett, who made the necropsy, found the body well nourished, the brain very anæmic, a few extravasations in the pia mater, cretified deposits at the apex of the right lung, emphysema elsewhere, the posterior parts being engorged with frothy serum. The heart was healthy and pale, but contained large fibrinous clots in all its cavities. The abdominal viscera were bloodless, and a few cysts were observed in the left kidney. The dark and healthy colour of the muscles was observed in contrast with the pallor of the integument, and the extremely yellow colour of the subcutaneous fat was also noted. The other case was that of a married woman with several children, admitted under Dr. Wilson on January 29th, 1851. She had not been well since her last confinement, four months before; she had been weak, but had not suffered from hæmorrhage; she had, however, been troubled with diarrhoea for two months previously. She was extremely anæmic; pulse quick, feeble, and empty; appetite bad; urine scanty and loaded; her only complaint was of general weakness. The diarrhoea was checked by remedies, but she did not gain strength. She suffered from insomnia, and had some attacks of dyspnoea, after one of which she died, about three weeks after admission. The *post mortem* examination by Dr. Ogle revealed pallor and slightly yellowish tint of

* See Addison's Collected Works (New Sydenham Society), p. 212. The passage is quoted by Dr. Pye-Smith in Virchow's *Archiv*, vol. lxx, p. 507; by Dr. S. Mackenzie, the *Lancet*, 1878, vol. ii, page 799; by Dr. Finny, *BRITISH MEDICAL JOURNAL*, 1880, vol. i, page 5; and many others.

† See Dr. S. Mackenzie's Lecture, *loc. cit.*