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THE

MOTIONS OF THE BRAIN,

WITH ILLUSTRATIVE GRAPHIC TRACINGS.

ON THE

BEHAVIOUR OF CARBOLISED CATGUT INSERTED AMONG LIVING TISSUES.

BY

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THE MOTIONS OF THE BRAIN,

WITH

ILLUSTRATIVE GRAPHIC TRACINGS.

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We have recently had numerous investigations made by physiologists as to the variations in bulk of parts of the body, due to the constantly varying quantity of blood contained in them. The applicability of the graphic method to such researches has probably determined the circumstance, that most of those lately recorded have been performed in M. Marey's Laboratory in the College of France.

First among these are the experiments by Dr Francois-Franck, upon the "Volume of organs in its relation to the circulation of the blood," published in Marey's "Travaux dù Laboratoire" for 1876. In this able paper Dr Franck gives a very complete bibliography of the subject, especially pointing out how far and in what directions the subject had been investigated by Dr Piegu, MM. Chelius, Fick, and Mosso, and others, for the details of which we must refer the reader to Dr Franck's paper. Suffice it to say, that two principal factors seem by these researches to be shown to produce change in the volume of an organ—viz., change of pressure in its contained vessels, and change of calibre of these vessels themselves. Of course, these two act and re-act on each other, so as to be in a certain sense interchangeable, but nevertheless, as we will see, they can to a great extent be studied separately. Dr Franck experimented upon the volume of the hand with the aid of his now well-known apparatus, consisting of a glass vessel of water into which the hand is introduced through an india-rubber membrane covering the top. The membrane is also perforated by a tube with a considerable dilatation upon it just after it emerges from the apparatus, one end of this tube being in the fluid, the other attached to the tube of a tambour. The dilatation does away with the oscillation of the column of liquid; and it is evident that any change of bulk in the enclosed member will alter the pressure of the air in the tambour system and permit of inscription on the cylinder. With the aid of this instrument, Dr Franck has shown that the motions thus obtained must be considered as the "summation of the changes of volume of the small vessels of the part," and that these bear the same relations to the heart's actions as do the motions of a single large vessel, both as to form and sequence.

The tracings are all dicrotic, and are influenced by mechanical and nervous causes, very much in such a way as would *prima facie* be expected.

These most interesting researches have, as an almost necessary complement, led to a similar investigation of the changes in volume of the brain; and we find the concluding paper in the same work, by M. A. Salathé, devoted to the consideration of this subject. We have also the papers of Dr Carlo Giacomini, of Turin, and Dr Mosso, which appeared about the same time; and another paper by Dr Francois-Franck, in the last *Journal de l'Anatomie et de la Physiologie.* At Dr Wm. M'Ewen's request, and with his assistance, I had been occupied during a considerable part of last winter investigating the same phenomena in two cases under his care. The following notes of the physical condition of the apertures he has kindly supplied:—

Thomas M'Donald, æt. 13, was admitted on February 9th with an aperture in the skull, which had been occasioned some months previously by a blow which he received in a coal pit. The aperture was somewhat circular, situated on the top of the head near the coronal suture, about the size of a penny, the dura mater being exposed and found at a somewhat lower level than the inner surface of the skull. On introducing the probe the dura mater was found detached from the skull for a distance of nearly ¼ inch round the aperture in the osseous plate. The dura mater was covered with granulations, from which a profuse discharge of matter exuded. The cerebral pulsations were distinctly felt through the membrane. Cardiographic tracings of the pulsations were taken on several occasions, and once whilst under chloroform, in order that a plastic operation might be performed for the covering of the aperture of the scalp.

William Hislop, æt. 16, admitted with a scalp wound on right side of head behind the ear, leading down to a fracture of the skull. The portion of bone was depressed $\frac{1}{4}$ inch. There was besides a deficiency in the osseous wall of the skull situated over the left eyebrow, which he stated had been occasioned by a fall from three storeys in height about ten years previously, for which he was treated in Edinburgh Royal Infirmary. After the recent fracture of the skull was completely healed, cardiographic observations were taken on the cerebral pulsations, which could be easily felt through the aperture in the skull. This aperture, more particularly was situated over the left eyebrow on the left side of the brow. It measured $1\frac{1}{2}$ inches in length and $\frac{3}{4}$ inch breadth in its broadest part; the widest part being situated about the middle of the length, and gradually tapering toward each extremity. It ran obliquely across the brow from below upwards and from left to right, the lowest portion being in a line with the outer canthus of the eyelids.

I was upon the point of publishing my results, when M. Salathé's paper in *Marey's Travaux*, above referred to, came under my notice. As our method of observation and results coincided in the most marked manner, I think it will be best to give a summary of the conclusions come to by the experimenters above-mentioned, with those independent observations by which I have been able to verify most of them; to point out the few cases in which our observations seem discrepant; and to add some tracings which I have had opportunities of obtaining, and which have not been recorded by the others.

M. Salathé, experimenting on the fontanelles of infants, on patients in whom a part of the skull was wanting, and upon animals with openings into the cerebral and spinal cavities, and working with the graphic method, has arrived at the following conclusions:—

1. The modifications of the calibre of the vessels of the brain are the cause of the movements of that organ. These are related to the cardiac and respiratory actions, and produce rythmic changes in the volume of the encephalon.

2. After trephining the spinal canal, the same tracings can be obtained from it, and these are synchronous with those of the encephalon.

M. Salathé attributes much importance to the flow of the cerebro-spinal fluid from the skull to the vertebral canal. He says: "The quantity of liquid contained in a completely ossified skull is always the same; inverse variations are continually being produced between the quantity of blood and the quantity of the cerebro-spinal fluid, which, in consequence of the partial extensibility of the walls of the spinal cavity, can flow there when the volume of the encephalon augments, to return to the cranium when it diminishes."

To these variations are due the cardiac and respiratory actions. Mosso adds a third, slow and rythmical, analogous to the contraction and expansion of the individual vessels which is supposed by most physiologists to be constantly going on, and which was first pointed out by C. Bernard as easily seen in the rabbit's ear. These have been named Pulsations, Oscillations, Undulations.

In the adult man, when the respiration is calm, it is only the cardiac influence which produces variations; but the respiration being forced affects the movements proportionally, and may even mask those dependent upon the heart.

3. The movements are almost the same as those of a single artery, and the tracings are fairly comparable to a sphygmographic tracing.

4. Ansæthesia does away with the respiratory curves.

5. Artificial respiration reverses the relations of the curves, which normally rise with expiration and fall with inspiration.

6. The influence of attitude is great; when the head is lowered, a marked increase of volume taking place, and *vice versa*.

7. Absolutely similar movements can be shown as the result of similar causes in other organs, most conveniently the hand.

It now behoves us to enter at some length into the consideration of 1st, The mode of experiment by which these results have been obtained. 2nd, The theoretical considerations bearing upon them. 3rd, The records upon which they are founded.

The experiments have been performed upon—infants with the fontanelles still open; adults with loss of part of the skull from disease or accident; and animals in which part of the skull or vertebral column had been removed by operation.

In all these, except the last, the motion of the encephalon has been received by the button of a Marey's cardiograph, or similar instrument, and from it transmitted to a recording tambour writing upon a revolving cylinder.

In the case of some of the animals, instead of the receiving tambour, there has been employed a tube screwed into the hole made by the trephine, partly filled with water, and connected with the recording tambour. The perfect similarity of the methods employed by the various experimenters, even when working quite independently of each other, gives unusual facility for comparing their results, and in many cases affords almost a demonstration of their accuracy. Instances of this we shall meet with as we go on.

Some of the tracings, indeed, which the author has obtained might be *fac-similes* of others taken by different observers from different patients. This is another testimony to the value of the graphic method.

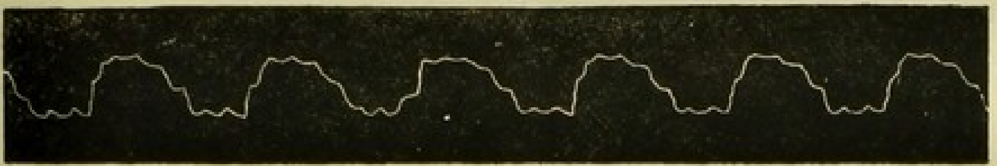
To come now to the more theoretical considerations, we must consider some of the circumstances in which the encephalon is placed. Admitting, as all must do, that the cranium is a rigid box containing no air, it has long been a question with physiologists whether or not a variation could take place in the quantity of its fluid contents. For an excellent summary of this controversy, we must refer our readers to Dr Francois-Franck's paper above quoted; suffice it to say, that although no one can now believe that the actual quantity of fluid in the cranium is liable to variation, yet it is easy remembering that we have there two fluids, separated from each other only by elastic and freely extensible membranes, to understand that the relative quantity of these fluids may largely alter, and that such an alteration implies movement of the cavities in which they are contained. Admitting, then, that the blood and cerebral fluid are, so to speak, interchangeable, when the cerebral fluid is driven from the cranium by the arrival there of more blood, it can only flow into the spinal canal. Anatomically the channel for it so to do is open, and we have only to remember that the spinal canal itself is not like the cranium, rigid nor entirely filled with the spinal cord, to enable us to understand how it can receive the overflow of cerebro-spinal fluid from the cerebrum. A certain amount of yielding probably takes place at those parts where it is more or less bounded by softer tissues, and the alternate filling and emptying of the vascular system of that packing with which it is surrounded probably permits it to receive and return a considerable quantity of fluid to the cerebrum, so as to keep up the necessary equilibrium. How this function of the spinal canal is affected by gravity, atmospheric pressure, &c., remains to be determined.

If from any of the causes above mentioned the complete closure of the skull is interfered with, we all know that the movements of the encephalon are easily seen, but it has been argued that these movements do not take place when the cranium is intact. The above considerations will I hope show that although the quantity of fluid never varies, the proportion of the two fluids does, and this entails movement of the substance in which they are enclosed, so that the motions recorded in these experiments are the same as those normally occurring in the closed skull. It is shown by M. Salathé, both by experiment upon the dead body, and by tracings from the spinal cord of animals, that such an interchange takes place, and it is evident from the tracings that the motion of the cord is due to fluid sent to it from the brain, and not to the influence upon it independently of the heart and respiration, for the amount of the variations is much greater than the relatively small bulk of the cord * compared with the brain would account for.

* The relatio n in bulk given by anatomists is about $\frac{1}{40}$.

We now come to consider the direct causes of this cerebral movement, and to enable the reader more easily to understand this I give one of my tracings in which the pulsations and oscillations are well marked.

Fig. 1.*



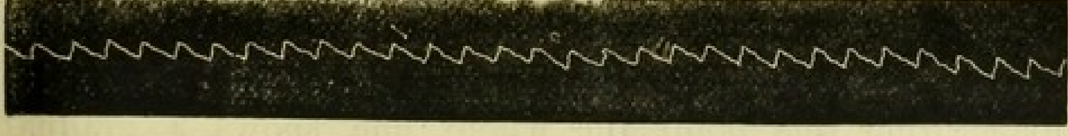
Tracing from brain of strong cat. Skull trephined Complete ether anæsthesia.

Here the large curves are respiratory oscillations, the small notches cardiac pulsations. It will be noticed that in this instance there are always eight cardiac pulsations in each complete act of respiration. The ascending part of the curve is expiration, the descending inspiration.

In this then we have a double set of curves produced by the cardiac and respiratory acts, but in which the cardiac is obscured and subordinated to the respiratory. It is impossible in the cardiac pulsations to differentiate the different parts of the curve. All we can say is that notches in the respiratory curve are produced.

On the other hand, if we study this tracing from Hislop, where the respiration curves are barely perceptible as they always are during quiet breathing, it is scarcely possible to distinguish it from a tracing of the radial.

Fig. 2.



Normal tracing from Hislop.

We have the dicrotic notch well marked, and the relations between the different parts of the curve as usual in a

* The tracings have been photographed directly from the originals on to the wood-block by Messrs Gilchrist & Fyfe, and thus great accuracy has been insured. They all read from left to right.

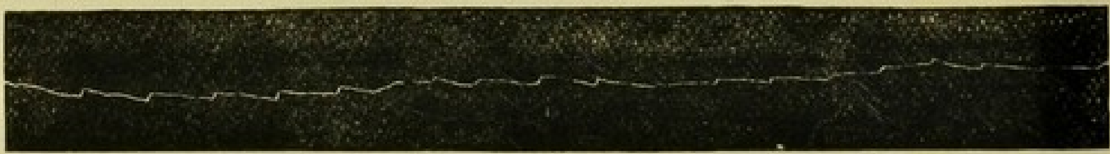
sphygmographic record. These then may be considered as examples of the types in which respectively, the respiratory and cardiac actions predominate. As to the third class of undulations described by Mosso and Francois-Franck, they are so slight and require such long tracings for their demonstration that although some of my results show them sufficiently to convince me of their reality, in common with these authors, I find it impossible to reproduce them.

The next point to be considered is the effect of varying conditions upon the tracings, and these conditions may be conveniently summarised, as (*a*) respiratory, (*b*) circulatory. (*c*) postural, (*d*) digestive, and (*e*) mental.

Under (*a*) respiratory we, in common with the other physiologists referred to, have investigated forced respiration, arrested respiration, coughing, speaking, &c.

In tracing 3 (from M'Donald) the effect of deep breathing in increasing the respiratory curves and obscuring the cardiac is well marked, as we would, *prima facie*, expect it to be.

Fig. 3.



Deep breathing (M'Donald).

The curious effect of holding the breath, both during its continuance and subsequently, is peculiarly well shown in 4, which is from Hislop.

After the moment, as indicated in the cut, at which breathing was suspended, we have for three pulsations no perceptible effect—from this point the whole line shows a gradual elevation corresponding to an increase of tension, and at the same time each pulsation becomes longer and stronger, and the dicrotic notch more marked and generally doubled; a slight fall then takes place in the tension, probably owing to an effort of the thorax to inhale, immediately followed by a marked rise, culminating in what we may call a

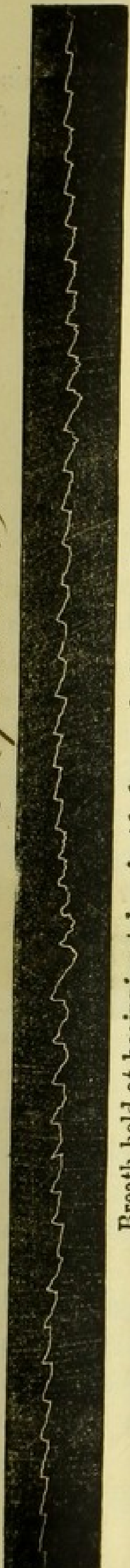


Fig. 4 (upside down)

Breath held at beginning taken just before end of trace.—Hislop.

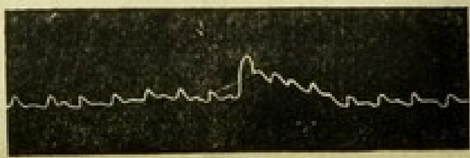
gigantic pulsation. The trace then gradually falls until breath was taken, when, after two or three irregular pulsations, it returns to the normal.

This we consider a remarkable record, and are inclined to think the production of the double dicrotism under these known conditions might help to solve the yet undecided problem as to the cause of this phenomenon. However, although in this case more than usually marked, in some of the pulsations being not only doubled, but trebled, it must be remembered that all the tracings taken from Hislop show more or less a tendency to double dicrotism. We can easily understand how the partial arrest of the venous return, due to the increased intra-thoracic tension, would tend to the exaggeration and *earlier* and even multiple occurrence of the notch, if, with Marey, we consider it due to a reflected wave; but if we can conceive a double action of the aortic valve in ordinary conditions, which, I confess, I am unable to do, it is even more improbable under the circumstances of increased intra-thoracic pressure with which we have here to deal; and therefore this seems to me an argument against this latter explanation of the phenomenon.

In Hislop's case, in which these appearances are most marked, it will be seen from Dr M'Ewen's report that a considerable thickness of tissue intervened between the brain and the instrument; and it has occurred to me that one of the notches might be due to the engorgement of the small vessels of this tissue following, as we would expect it to do, at a short interval, the dilatation of the more centrally-situated and freely-supplied deeper parts. Indeed, bearing in mind M. Francois-Franck's observations on the increase of volume of the hand,* the possible effect of this factor in the production of some obscure phenomena of double dicrotism seems worthy of attention.

The effect of coughing, as shown in the following tracing (from M'Donald), puts in black and white the physiology of the action both as affecting the respiration and circulation.

Fig. 5.

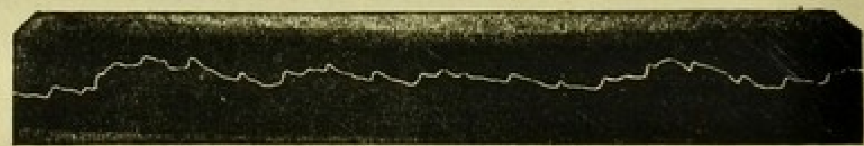


A cough.

We have the long breath, followed by the sudden expiration, at once increasing the vascular tension and the rapidity of the cardiac beats, and then both tension and rapidity gradually coming to the normal.

In speaking, we have less sudden changes, but marked effects on the respiratory oscillation, reacting on the cardiac curves.

Fig. 6.



Speaking.

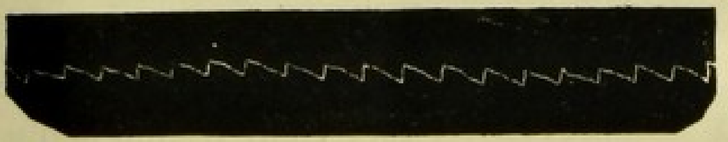
M. Francois-Franck has shown that "straining" produces an enormous augmentation of the volume of the brain, with increased frequency and dicrotism of the pulsations.

Circulatory.—Effects of interference with the circulation. Numerous experiments have been tried on the effect of compression of various blood-vessels by all the physiologists referred to above, and they have arrived at very similar results. We ourselves have investigated the effect of the general excitement of the circulation produced by the slight

* Francois-Franck. Travaux du Laboratoire, 1876.

exercise of walking quickly, in the case of Hislop, as shown by the following tracings:—

Fig. 7.



Before exercise.

Fig. 8.

After exercise.

Here we have marked increase in the force of the pulsations, and in the respiratory oscillations, but only slight increase in frequency.

Compression of the Femorals.—The continental observers whose works I have been able to obtain, do not give a record of the effect of compression of the femorals upon the brain; but Francois-Franck gives a trace of the effect of this manœuvre upon the volume of the hand, and the following trace, taken from M'Donald, is interesting, coinciding so closely as it does with Francois-Franck's trace (Travaux du Laboratoire, 1876, p. 32), taken from the hand under like conditions.

Here we see the increase in the bulk of the encephalon, produced by diverting the large quantity of blood which goes to the lower limbs and confining it to the trunk and head. This may have practical bearings as shewing the feasibility of the plan of treatment of cerebral anæmia, in which it has been proposed to increase the blood supply to the brain by compressing the great trunks. Dr Francois-Franck has shown, in the case of the hand, that a converse result may be produced by engorging the lower extremity with blood by means of a suitable aspirating apparatus; but that in the case of the brain, although a similar effect is produced, it is comparatively slight. The relations before dwelt upon of the cranial and spinal cavities probably explain this.

The difficulty of compressing the carotid in man without interfering with the nerves and veins with which it is so closely associated, has prevented very clear results being obtained from experiments upon it, although Mosso has succeeded, and asserts that the changes are the same as those produced upon the volume of the hand after the compression of the brachial—that is to say, an absolute diminution of volume and also of extent of movement. "After the cessation of the compression, the cerebral pulsations present during a certain time an exaggerated amplitude—as if the vessels, unaccustomed to the interior pressure, allowed themselves afterwards to be passively distended. Their normal tonicity is only recovered little by little, and its restitution restores the amplitude of the pulsations to their initial value."—(Francois-Franck. Journal d'Anatomie, loc. cit.). Several theories have been advanced by these authors, amongst which the effect upon the heart itself and a change in the resistance of the vascular walls, probably due to temporary arrest of their nutrition, have seemed the more probable. Indeed, from many considerations the last hypothesis alone is probably sufficient to account for the phenomena. An attempt to compress the carotids in M'Donald's case gave us results so contrary to this statement, and so inexplicable on any other hypothesis, that we must consider them as untrustworthy. The uniform effect seen in them is great augmentation of the cerebral bulk, and, indeed, a tracing closely resembling that produced by compression of the jugulars in the case of the brain, and of the venous circulation in the case of the hand.

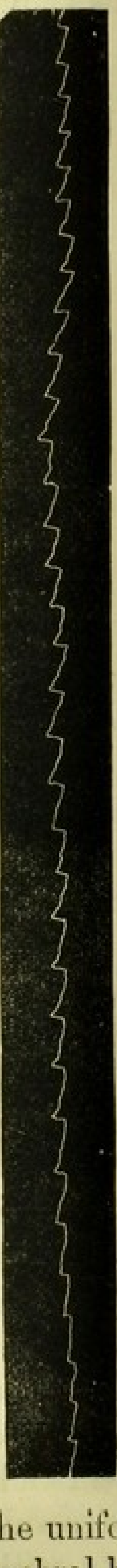


Fig. 9.

No doubt it is conceivable that this may be due to an effect either on the heart or vessels, produced by action of the nervous centres set up by the disturbance in their circulation; but further information is wanting upon this subject, and we are glad to know that Francois-Franck proposes soon to publish the results of investigations upon it.

Compression of the jugulars is shewn to produce, as might be expected, augmentation in the bulk; but it is noteworthy, that although this augmentation is of the same character as the increase of volume of the hand when the venous circulation is interrupted, nevertheless it is of much less extent, and is not accompanied by the change in the form of the cardiac curves, which is so marked in the case of the hand. These differences are obviously due to the impossibility of controlling the whole venous return from the head, as can be done with the hand, and to the compensating effect of the motion of the cerebro-spinal fluid so often insisted upon.

Posture.—The effect of posture is recorded by all the observers as nearly the same. Those positions, which tend from gravity to increase the quantity of blood and cerebro-spinal fluid in the brain, increases its bulk. Up to a certain point, and if, as in the recumbent position, they facilitate the influx of blood, they increase the force of the pulsations as well; but if the conditions are such as to cause more or less hernia of the cerebral substance through the opening, they diminish or even altogether do away with the motions communicated, the protruding part being, as it were, strangulated.

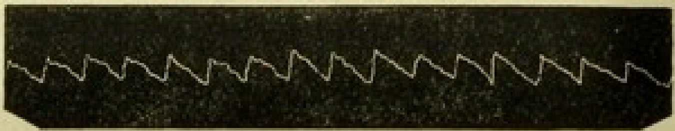
The elevation of the arms produces a marked increase in volume, as might have been expected.

Food and stimulants.—None of the papers I have seen give any experiment upon the effect of gastric conditions. I have fortunately obtained some good tracings of the effects of food and drink, and think that the following illustrations show that the effect of food is a marked increase of the extent of the cardiac pulsations. Compare the tracing Fig. 10, taken twenty minutes later, with tracing Fig. 2, taken just after dinner.

As soon as this tracing was completed, the lad, who was quite unaccustomed to the use of stimulants, took eleven ozs.

of Bass' beer. Tracings were then taken at short intervals, and the size of the pulsations and respiratory curves was found

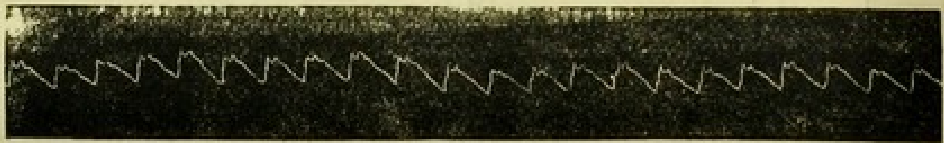
Fig. 10.



Twenty minutes after dinner. Compare with Fig. 2.

gradually to increase, while their duration, especially that of the pulsations, was lengthened. This condition of matters seemed to reach its acme about ten minutes after the beer was swallowed, at which time the following record was obtained.

Fig. 11.



Ten minutes after a pint of beer, and half-an-hour after dinner.

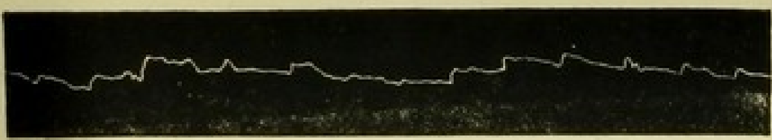
Of course, in observations extending over such comparatively long periods of time, it is impossible to record the actual effect on brain volume; but when I come to sum up, I will endeavour to show that they are probably to be accounted for by a diminution in the whole bulk of the encephalon, perhaps due to the amount of blood drained off for the performance of the digestive function.

Mental Conditions.—There only remains to us now to consider the effect of mental states—and of these sleep, anæsthesia, and brain-work have all afforded various tracings, both in the hands of my foreign fellow-workers and my own.

It is not difficult to understand, however, that these conditions (except anæsthesia) lend themselves less readily to experiment than many of those already recorded.

As to sleep, we have had unusual opportunities of obtaining tracings during this condition, chiefly owing to the fortunate circumstance that the opening in M'Donald's head was situated almost in the centre of the cranial vault, which permitted very easy application of the instruments, and to the fact that the boy was a very deep sleeper. We have taken advantage of these circumstances, and on several occasions obtained numerous tracings from his brain, not only during profound sleep, but also in various stages of wakening. As to the conclusions to be derived from these tracings, we cannot agree altogether with either M. Mosso or M. Salathé. In the first place, M. Salathé states that the respiratory curves are absent. This, we found, depended not upon the condition of sleep, but upon the condition of respiration, as, indeed, we think might have been expected. We are all aware that during sleep the respiration may vary from the most complete tranquillity to stertor; and, indeed, our tracings seem to show that respiratory oscillations are produced by less disturbances of the respiration during sleep than when awake. The following trace during profound sleep, when the breathing might be called "firm," but not forced, illustrates this:—

Fig. 12.



M'Donald asleep—breathing firmly.

Subsequently, during the same experiment, as will be seen in the tracings below, after his position was altered and the breathing became perfectly tranquil, the respiratory curves disappeared, to again recur on another change in the breathing.

The cardiac pulsations, on the other hand, were, in direct contradiction of the results of the observations of the two physiologists above mentioned, uniformly depressed, as seen in the subjoined, Fig. 13, which is a typical trace selected almost at random from a considerable number taken upon different occasions. It is indeed possible that the marked depression of the curves may be due to the button of the cardiograph having been applied with greater or less force than in the waking state; but as exactly the same arrangement was used in all cases, and the chance of this error foreseen and guarded against, I am not inclined to

Fig. 13.

Sound asleep—breathing quiet.

accept this explanation. Indeed, on one occasion the tambour was applied when the boy was awake. He went to sleep with it *in situ*, and a tracing was then obtained, with exactly the same characters.

These results are not in accordance with the others mentioned above, and point rather to a condition of congestion than anæmia of the brain—the condition usually supposed to exist during sleep. However, it is worthy of notice that in M'Donald's case the normal, as well seen in the first few curves of No. 5, is not very high, although here the dicrotic notch is well marked, while it is nearly absent in the sleeping pulse.

It is very desirable that further observations be made upon this subject, and, I believe, M. Mosso is engaged in the investigation.

Anæsthesia removes the respiratory curves, and in M'Donald's case at least, the chloroform increased the height of the pulsations in a marked manner.

The effect of brain work is also difficult to record, chiefly because the result of fixing the attention produces an involuntary change in the character of the respiration. M. Francois-Franck gives a tracing in which during calculation the volume of the cerebrum seems increased, but little if any change being produced in the cardiac curves, but as a synchronous tracing shows that the respiration was at the same time much affected, he justly places little reliance upon this record.

In the subjoined tracing the rising of the whole line is slight if any, but the marked diminution of the height of the cardiac curves points to increased cerebral volume.

The similarity of this to the sleep tracing, Fig. 13, makes one wonder if the boy could have been dreaming.

Fig. 14.

Puzzled by question in multiplication.

In conclusion I have only to point out that these tracings afford important indications of three different things. 1st. The volume of the whole brain. 2d. The influence of respiration upon it. 3d. The influence of the heart beats. I have endeavoured to show that the first—the volume of the encephalon—depends upon the relative amounts of blood and cerebro-spinal fluid contained in the cranium. That this is influenced by the amount of distention of the vessels. The second—viz.—respiratory action, alters the volume through its effect upon the circulatory system; and the third, the cardiac pulsation, is dependent as far as extent goes upon the volume.

Thus, if the cranial contents are in a state of tension the change in the bulk which we have recorded as a pulsation is less than when they are in a less tense, and therefore more mobile condition; for the same reason that the vibrations of a stretched cord are of less extent, the greater the force with which it is extended.

The important factors then in producing cerebral motion, are the alternate aspiration and expulsion of blood by the inspiratory and expiratory motions of the thorax; the greater or less force of the ventricular contraction; the increased or diminished quantity of blood available to be sent to the brain, and the action of gravity upon the cerebro-spinal fluid.

I have to thank Dr M'Ewen for directing my attention to the investigation and putting the patients at my disposal, and to both him and his house surgeon, Mr Borland, I have been indebted for much valuable assistance in the prosecution of the research.

ON THE BEHAVIOUR OF CARBOLISED CATGUT INSERTED AMONG LIVING TISSUES.

(Reprinted from the *Lancet*, 1876.)

THE absence of definite knowledge of what becomes of carbolised catgut when introduced among the living tissues, recently shown by the speakers at a debate in a distinguished London scientific society, suggested to me the expediency of trying to clear up the question by experiment.

Two ideas seem now to hold sway in the profession upon the subject. Some consider that the catgut is absorbed—that is, dissolved, taken up, and carried away, without leaving any trace behind; in the same way as a soluble crystalline substance introduced into the tissue would be. Others maintain that it is organised, converted into tissue, and becomes a living, active part of the body. In a modified form of this latter I conceive my experiments show the truth to lie.

The mode of experimentation adopted passed through various phases. At first I simply inserted carbolised catgut with a needle below the skin of a dog's back, putting fresh pieces in new places at definite intervals. A month after the first piece had been introduced the dog was killed, and no trace of any of them could be found, except in two places where abscesses had formed around portions which had only been in a few days. This was the only instance in all my experiments in which this complication ensued, and was probably due to the use of a dirty needle.

This complete disappearance of the catgut rendered it necessary to adopt some plan of marking its exact position, and for this purpose external means, such as silver sutures put in the skin and nitrate of silver applied externally, were found inefficient. Under these circumstances I adopted the plan of attaching silver wire to the catgut, by encircling with it, in a spiral form, about an inch and a half in the centre of the gut. This plan answered perfectly until I came to cut the sections, when the wire interfered, and if pulled out disturbed the very piece of tissue to be investigated. I now substitute for the wire carbolised silk, applied much in the same way—that is, passed through the gut in two places near its centre, about an inch apart, and the ends tied. The advantages of this are that it can be closely applied to the catgut, is little altered in the tissues, seems to set up no irritation, and can easily be cut with the razor and recognised in the preparation. Having then taken a piece of carefully-prepared thick catgut about six inches long and attached the silk over about an inch and a half in the centre, I thread the gut into a sharp aneurism needle, push it well into the tissues, and bring the point out say three inches from the place of puncture. The gut is caught, the needle freed and withdrawn, and the part of the catgut bearing the silk being adjusted so as to occupy the middle of the track the ends are cut short, and by putting the skin on the stretch they slip below it, thus making the whole piece subcutaneous. Of course the needle, surface of the skin, &c., are previously rendered antiseptic.

In this manner, then, I have performed about twenty experiments upon dogs and rabbits, latterly with the addition that I killed the animal by bleeding, and injected the abdominal aorta with two per cent. watery solution of Bruké's soluble Prussian blue. I then cut out the pieces of catgut with some of the tissues in which they were imbedded, hardened them in spirit or froze them, and made sections in the usual way.

The results of these experiments show that a gradual softening takes place from without in, the catgut breaking down and becoming infiltrated with cells, probably leucocytes. This part of the process takes from five days to about twenty, varying with the specimen of catgut, the tissue amongst which it is situated, and the age and vitality of the animal. Next the pultaceous mass into which it has been converted begins to metamorphose, and is soon permeated with blood channels, and ultimately may be described as a cast of the catgut in a kind of granulation tissue freely supplied with blood vessels, *which in many of my sections are very fully injected.*

If, then, we admit these conclusions, we can easily account for the different results obtained by the use of catgut in different hands. We see that it is in reality merely a temporary ligature, because when in the softened stage we cannot consider it to have any constricting effect.

Whether this temporary condition lasts long enough to produce embolic occlusion of the vessels depends upon the *sample of catgut* and *the vitality* of the patient.

In conclusion, my experiments seem to demonstrate that an aseptic, dead, foreign animal body may, under appropriate conditions, become by a process of softening, absorption, and redeposition, changed into or replaced by a living, vascular, and comparatively highly organised animal structure.

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