An experimental inquiry into the nature, cause, and varieties of the arterial pulse : and into certain other properties of the larger arteries, in animals with warm blood. / by Caleb Hillier Parry.

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AN

EXPERIMENTAL INQUIRY

INTO THE

NATURE, CAUSE, AND VARIETIES

OF THE

ARTERIAL PULSE;

AND INTO

CERTAIN OTHER PROPERTIES

OF THE

LARGER ARTERIES,

IN ANIMALS WITH WARM BLOOD;

ILLUSTRATED BY ENGRAVINGS.

BY

CALEB HILLIER PARRY, M.D. F.R.S.

MEMBER OF THE COLLEGE OF PHYSICIANS OF LONDON;
MEMBER, AND FORMERLY A PRESIDENT, OF THE ROYAL MEDICAL SOCIETY OF EDINBURGH;
HONORARY MEMBER OF THE PHYSICAL SOCIETY OF GÖTTINGEN, AND OF THE GEOLOGICAL SOCIETY OF LONDON;
ONE OF THE PHYSICIANS TO THE BATH GENERAL HOSPITAL, AND PHYSICIAN TO THE CASUALTY HOSPITAL, AND PUERPERAL CHARITY, IN THAT CITY.

"TOTOS NOS IN CONTEMPLANDIS REBUS PERSPICIENDISQUE PONEMUS, PROPTEREA QUOD ET NATURA INEST MENTIBUS NOSTRIS INSATIABILIS QUÆDAM CUPIDITAS VERI VIDENDI."

CIC. TUSC. QUÆST. LIB. I.

PRINTED BY
RICHARD CRUTTWELL, ST. JAMES's-STREET, BATH;
AND SOLD BY
UNDERWOOD, FLEET-STREET, LONDON.
MDCCCXVI.

TO THE

RIGHT HONOURABLE

SIR JOSEPH BANKS, BARONET,

K. G. C. B.

PRESIDENT OF THE ROYAL SOCIETY,

&c. &c. &c.

THE UNWEARIED AND MUNIFICENT PATRON

OF

EVERY USEFUL SCIENCE AND ART,

THIS INQUIRY

IS

WITH JUST RESPECT INSCRIBED,

BY

THE AUTHOR.

ADDENDA.

Page 30, l. 22, after the words " with regard to the," insert, " portion between the first and second ligature in the"

99. This illustration by Dr. Young, taken from the Hydraulic Ram, I admit only in so far as the finger, when pressing on the first valve, feels the impulse of the lateral current of fluid.

SYNTHETICAL VIEW

OF THE CHIEF

EXPERIMENTS,

ON WHICH THE FOLLOWING ANALYSIS IS FOUNDED; IN THE ORDER IN WHICH THEY WERE MADE.

EXPERIMENT I.

September 22, 1814, 8 a. m.

IN a Ram, both carotid arteries were laid bare by Mr. GEORGE NORMAN, in the presence of Mr. COOMBS, two others of his pupils, and myself.

Notwithstanding the animal was greatly agitated, there was not in either carotid the least appearance of dilatation during the systole of the ventricle, or contraction during its diastole: Nor, except when the animal breathed, was there any degree of loco-motion perceived in these arteries, which remained completely quiet and at rest. Nevertheless, when either artery was

compressed between the finger and thumb, the pulse was very strong and distinct.

Both arteries were adequately tied by Mr. NORMAN, each with a single ligature; after which, the ram stood and walked about well, apparently suffering little inconvenience.

EXPERIMENT II.

October 5, 1814, 8 a. m.

At my desire, and in the presence of the persons mentioned in the former experiment, Mr. GEORGE NORMAN laid bare the left carotid of a Ram, denuding the artery of its cellular membrane. The eighth or vagus nerve was seen attached to it by cellular membrane, on its posterior (upper) side. The pulse, when felt in it, was 100 in a minute; but no motion whatever was perceptible in the artery to the eye, whether of dilatation or change of place, except that it was slightly moved forwards towards the head, and back again, in respiration, accompanying the trachea. No pulse could be felt in it on mere contact, or even when with the finger it was pressed laterally against the sterno-mastoid muscle; but when it was taken between the finger and thumb, and so compressed, the pulse in it was sufficiently distinct.

The diameter of the artery was measured with a pair of fine compasses, and found to be $\frac{1}{4}\frac{1}{0}\frac{4}{0}$dths of an inch.

A ligature was then made on it, intended merely to half compress it; and this effect seemed to have been produced at first; but, on further examination, it was found to have tightened itself, so as to prevent all pulsation in the artery beyond it.

The ligature being now improper for the intended purpose of the experiment, which was to try whether the artery would contract itself beyond the part which was half compressed, it was cut off, and another applied in another part, so as distinctly to allow of a pulsation beyond the ligature.

In the part immediately before the ligature a very slight movement, of a nature which we could not define, was perceiveable.

Neither of the ligatures of ram No. I. was come away. He was in good health.

EXPERIMENT III.

October 12, 1814.

This day Mr. G. NORMAN, in the presence of the before-mentioned persons, cut down upon the femoral artery of a Ram. Without the

aid of a glass, it appeared to all of us perfectly motionless, and void of all dilatation from the systole of the ventricle, which, on pressure against the artery, we found to be 108 times in a minute. A small artery, running nearly parallel to it, and which was possibly the profunda femoris, was equally free from loco-motion or dilatation.

The right carotid was then examined. Whenever the animal ceased for a moment to breathe, or when the artery was drawn upwards out of its place, there was no movement in it of any kind.

It must, however, be remarked, that from the violent agitation of the animal, there was an almost perpetual loco-motion of the artery during respiration, which we had observed to occur in the two former rams. This motion, however, appeared not to be any proper movement of the artery; for it always ceased in the short intervals when the ram ceased to breathe, and on being accurately and repeatedly reckoned by a watch indicating seconds, was found exactly to coincide with the respirations, which were 14 in every five seconds, or 168 in a minute; while the systole of the ventricle, as counted in the same artery, was only 108 in a minute.

The arteries appeared to be always equally distended in the systole and diastole of the ventricle; and when they were compressed, the part beyond the compression remained just as full as before, though having no pulse.

The right carotid was tied with a single ligature.

October 17, 1814.

This day the ram, in Experiment I. was examined. The ligatures were come away, and the parts wounded were completely healed.

The ram, of Experiment II. was also examined. The wound appeared to be healed.

The ram No. III. was also, like the others, in good health. Soon afterwards, the ram No. III. in fighting with No. I. had the ligature on the right carotid torn out, and suffered a considerable hæmorrhage. Mr. NORMAN merely put a ligature by means of a needle round the muscular and other substances contiguous to the torn spot; and, the hæmorrhage ceasing, left the animal to his fate.

Subsequently to this period, in 1814, the carotids in one or more Sheep and Rabbits were examined; but as the results appeared exactly conformable to those above stated, they were

not recorded. No dilatation of the artery, from the impulse of the blood by the systole of the left ventricle, could, in any case, be discovered, even with the strongest magnifier, the field of which was sufficient to include the whole diameter of the artery.

EXPERIMENT IV.

April 20, 1815, 8 a. m.

This day Mr. GEORGE NORMAN, in the presence of three of his pupils and myself, made the following experiment on a Ram.

He dissected down to the carotid on the right side. The artery was very distinct. It had no dilatation or contraction whatever; but had a longitudinal motion backwards and forwards, exactly corresponding with the respirations, so that it was drawn backwards towards the thorax at each inspiration, and returned forwards into its place at each exspiration.

Its circumference, being accurately measured, was found to be $\frac{261}{400}$ of an inch; whence its diameter might be stated as being, in round numbers, about $\frac{1}{3}$ of an inch.

The left carotid being exposed in the same way for exactly $2\frac{1}{2}$ inches in length, and denuded of cellular membrane as much as possible all

round, was found to be in circumference $\frac{241}{400}$ of an inch.

In this second artery, as in the former, there was a total want of all dilatation and contraction; but the longitudinal motion, corresponding with respiration, was very conspicuous; and during every violent exspiration, the artery, which, in the part laid bare, was separated from all its adhesions, was pushed forwards with such force, as to form a segment of a circle backwards towards the posterior part of the neck, but recovered its longitudinal direction at each inspiration.

This latter artery was then tied by a single ligature, exactly in the middle of the exposed part.

EXPERIMENT V.

June 8, 1815.

One of the carotids of a Ewe was laid bare by Mr. G. NORMAN, in the presence of some of his pupils, Dr. RIGBY of Norwich, and myself. No motion of the artery could be perceived, except the longitudinal one from respiration before mentioned; nor could any pulse be felt, except when the diameter of the artery was lessened by counter pressure to that of the finger.

EXPERIMENT VI.

July 31, 1815, 3 p. m.

Present Mr. SODEN, surgeon; Mr. GEORGE KITSON, surgeon; Mr. COOMBS; two of Mr. G. NORMAN's pupils; and myself.

Mr. G. NORMAN laid bare the left carotid of a small Spaniel Dog; the pulse of which in the femoral artery, after he was laid on the table, and previously to any operation, was 72 in a minute, and considerably irregular.

The carotid was denuded for more than two inches in length. A nerve (descendens noni?) and a small vein crossed it, and the vagus and sympathetic nerve ran together in close contact with it.

After the incision, the pulse was a beat or two slower, but not more irregular than before; the dog being perfectly quiet. A motion in the artery, corresponding with the systole of the ventricle, was very apparent. It was, however, evidently for the most part a longitudinal one; the artery advancing forwards towards the head during the systole, and retreating during the diastole. Once the motion appeared to be in some degree lateral, that is, of the whole artery from side to side. It was, however, evident, after a long and attentive examination, assisted

by a strong lens, that there was nothing like dilatation and collapse, or a change in the diameter of the vessel, which always appeared equally full and extended.

EXPERIMENT VII.

Immediately after the foregoing experiment, the left carotid was examined in a Ewe; the neck and head being stretched out during the operation. Here, also, there was no dilatation or contraction, nor was any movement perceived from the systole and diastole of the ventricle; but a longitudinal movement backwards and forwards from respiration was evident, as in the preceding experiments.

EXPERIMENT VIII.

August 2, 1815, 3½ p. m.

In the presence of Mr. TUDOR, surgeon, Mr. COOMBS, three pupils, and myself, Mr. G. NORMAN laid bare the carotid of a Ewe, of which the other carotid had been examined, July 31st.

When the head was stretched out, and the neck forcibly elongated, no movement was perceptible in the carotid, except that produced by respiration; but when the head and neck were

placed in a more easy posture, so that the carotid, dissected from its attachments, became loose, then the systole and diastole of the ventricle were apparent by a movement of the artery, which, to most of us, was perceived to be mere loco-motion in different directions; as we were absolutely unable to discover, either with the naked eye, or with glasses, any change in the diameter of the artery. Mr. TUDOR was not, however, clear that he did not perceive some dilatation, though not, in his opinion, sufficient to produce the phænomenon of the pulse.

EXPERIMENT IX.

The same experiment was immediately afterwards repeated on the carotid, and femoral artery of a Ram, and with the same results. The different movements from respiration, and the action of the heart, were perceivable in both arteries when relaxed; more especially by the continuance of the movement from the heart, during different times that the animal for a few seconds ceased to breathe.

EXPERIMENT X.

August 5, 1815, 3 p. m.

In the presence of Mr. GEORGE KITSON, Mr. BROWN, surgeon, Mr. COOMBS, three of Mr.

NORMAN's pupils, and myself, Mr. G. NORMAN exposed the right carotid of the Dog which had been operated on three days before.

When the neck was stretched out, the influence of the systole of the ventricle on it was obscure, but apparent when it was bent in a somewhat natural posture, as the dog lay on his left side. This influence consisted of various modes of loco-motion in the artery, such as lateral, forwards and backwards, and longitudinal in both directions; but neither of us could discover, either with the naked eye, or a strong lens, the least change in the diameter of the artery, either of dilatation or contraction. The femoral artery on the same side was at the same time laid bare; and the same appearances were observed in it as in the carotid.

The breathing of the dog having been throughout very little hurried, the movement from it was with difficulty discernible.

EXPERIMENT XI.

August 11, 1815, 4 p. m.

Present, Mr. COOMBS, two of Mr. GEORGE NORMAN's pupils, and myself.

Mr. NORMAN having laid bare the two carotids of a Rabbit, more than half grown, we all

saw in those vessels a movement backwards and forwards, which corresponded with their pulse, but so short that it was not always visible without a magnifying glass. The movements, also, were so quick, that it was impossible to count them. We many times viewed them in the sunshine with a strong lens, but could not perceive the least change in their diameter, whether of dilatation or contraction. No difference in these movements was produced by any change of the position of the head.

The abdominal aorta was then exhibited, and denuded of the peritonæum for a considerable extent. The respirations were 72 in a minute. We could not, either with the naked eye or the lens, discover in this artery any movement, whether longitudinal or otherwise; though at the same time, when it was pressed by the finger, its pulse was firm and perfectly distinct.

EXPERIMENT XII.

August 16, 1815, 6 p. m.

Examined both carotids of a Rabbit, two-thirds grown. The dissection, as usual, by Mr. GEO. NORMAN, in the presence of Dr. DICKSON, R.N. Mr. COOMBS, two pupils of Mr. NORMAN, and myself.

There was evidently no dilatation or contraction whatever; but there were two sets of movements; one produced by respiration, and being longitudinal and much slower than the other, the movement of the adjacent parts keeping pace with it; the other depending on the systole of the heart, and being generally longitudinal, but occasionally, though rarely, lateral, or even in a certain degree rotatory. All the latter movements were, however, extremely quick and short, so as scarcely to be discoverable without a strong lens, and to be by no means capable of being counted. Even the respirations were 120 in a minute.

This was the result of our observations, in whatever posture the animal's head or body was placed.

The abdomen was then opened, and the aorta examined. Mr. NORMAN could not perceive in it any motion whatever, even with the lens; but Dr. DICKSON was not sure that he could not distinguish some trembling of it. There was, however, evidently no dilatation or contraction, though the pulse was strong on pressure. The respirations continued to be 120 in a minute.

EXPERIMENT XIII.

August 23, 1815, 3½ p. m.

Present, Mr. COOMBS, three pupils of Mr. G. NORMAN, and myself.

In a Ram, Mr. G. NORMAN cut down on the left femoral artery. It had a confused movement from respiration, not peculiar to itself, but in common with the rest of the body, from the general agitation which the rise and descent of the diaphragm, and the consequent protrusion and retraction of the abdomen, produced. When, however, the respiration for a while ceased, which was frequently the case, we could easily see the longitudinal motion of the artery, corresponding with the systole of the left ventricle of the heart. We could not in this experiment decide whether this was elongation of the artery, mere loco-motion, or both.

The right carotid was then examined, and was separated from all its adhesions. When the head was stretched out, and the neck straightened, the motion chiefly apparent was the longitudinal one from respiration, so often described; and even when the respiration for a while ceased, the motion from the systole of the ventricle was very obscure. On the other hand, when the neck was bent, and the carotid became by the

position tortuous, with its convex bend projecting outwards, each systole increased this convexity in a very sensible degree; and the vessel returned to its former state during the diastole of the ventricle. We could not, however, though assisted by lenses, discover any change of diameter in either artery. The pulse could not be perceived either in the femoral or carotid, when simply touched with the finger, without compression; but was evident enough, when, by pressure against a hard substance, the diameter was diminished.

We now killed the animal by a ligature round the trachea, clear of all the vessels.

Previously to his being killed, on examining the carotid, we found in it, in one spot, a stricture or contraction of the artery, which had not existed before, and which was not more extensive than if a fine piece of twine had been tied round it. About an inch and a half below this spot, where the artery retained its natural size, it was found, by an accurate measurement, twice tried, to be exactly $\frac{260}{400}$ of an inch in circumference.

Within five minutes after the death of the animal, the same artery being measured as nearly as possible in the same place, was found to be

in circumference only $\frac{190}{400}$ of an inch; being thus reduced $\frac{70}{400}$dths. The femoral artery was also greatly shrunk in size; but the degree was not ascertained by actual admeasurement.

August 24, $3\frac{1}{2}$ p. m.

The carotid artery was again measured, $21\frac{1}{2}$ hours after the former admeasurement, and exactly in the same spot. It was now found to be in circumference $\frac{222}{400}$ of an inch, or to have gained, since the last measurement, $\frac{32}{400}$ of an inch.

The process of putrifaction having begun in the abdomen and dissected thigh of this animal, there can be little reason to doubt that its death, and consequently its loss of tonicity, was total and complete.

EXPERIMENT XIV.

September 4, 1815, $3\frac{1}{2}$ p. m.

Present Dr. CHARLES PARRY, Mr. GEORGE KITSON, two of Mr. G. NORMAN's pupils, and myself.

Mr. G. NORMAN exposed the right carotid of a Rabbit, two thirds grown. The respirations were 72 in a minute; and when the head and neck were in a relaxed posture, it was very easy to distinguish the two different movements

from respiration and the systole of the ventricle; the former being much slower than the latter. When the mouth was stretched out in a line with the neck, the latter motion was scarcely perceptible, and only the former was distinctly observable. Both movements were longitudinal; and it was well observed by Dr. CHARLES PARRY, that the line of light, on the convex surface of the artery, when viewed with a strong lens, never changed its position; which could not have been the case, had not the diameter of the artery remained the same. The movement from the systole was very short and quick.

Mr. KITSON, who had not witnessed many of these experiments, could not feel any pulse in the carotid when he pressed it, unless some hard substance, such as the trachea, was opposed to his finger, so as to reduce the diameter of the artery.

Mr. NORMAN for a considerable time rubbed with his finger a portion of the carotid; but the artery was not dilated by the friction.

The abdomen was now opened, and the aorta, undivested of the peritonæum, was examined, chiefly above the exit of the mesenteric arteries. There was now a very distinct motion from the systole of the ventricle, but none from respiration, the exact process of which was easily

ascertained by the descent and ascent of the diaphragm. This movement of the artery simulated that of dilatation and contraction more nearly than any which I had ever before witnessed; but since, on farther examination with a powerful lens, it appeared that the artery had at the same time no lateral swell, it was evident that this was not dilatation and contraction, but merely an advance of the whole forwards towards the eye, alternating with a retreat backwards from it. With this, there was a slight union of the longitudinal motion already so often described.

EXPERIMENT XV.

September 7, 1815, $3\frac{1}{2}$ p. m.

Present Dr. CHARLES PARRY, Mr. CHAPMAN, Mr. WOOD, and Mr. FIELD, (three of Mr. G. NORMAN's pupils,) and myself.

In the Ram before mentioned, Experiment III. and which was very fat, Mr. COOMBS laid bare the left carotid. We were extremely surprised at the great superiority of size in this artery, comparatively with those which we had before examined. It had a very strong motion from the systole of the heart, as the animal lay on his back with the neck relaxed, so that it

bent itself forwards towards us with every acceleration of the current of blood from the systole of the ventricle, and receded during the slower current from the diastole. With this motion there was also a longitudinal one from the same cause, but no lateral enlargement, and therefore no increase of diameter whatever.

Two measurements of the circumference of this artery were made in the same spot, and exactly coincided, giving a circumference of $\frac{379}{400}$ of an inch.

The ram was then killed by a ligature round the trachea only; the vessels not being included.

After the animal was dead, at the end of six minutes from the first application of the ligature round the trachea, the carotid was again examined. The decrease of size was extremely conspicuous. Being measured in precisely the same place as before, it was found to be in circumference only $\frac{270}{400}$ of an inch, having suffered a diminution of $\frac{109}{400}$ of an inch.

September 8, $3\frac{1}{2}$ p. m.

The artery being again examined in the same spot, was found to be in circumference $\frac{320}{400}$ of an inch, which was an increase, subsequently to the preceding day, of $\frac{50}{400}$ of an inch.

EXPERIMENT XVI.

September 13, 1815.

Present Mr. COOMBS, Mr. NORMAN's pupils before mentioned, and myself.

Mr. G. NORMAN examined the right carotid of a full-grown fat Wether. When the mouth was stretched out, no motion was to be seen in the artery, but the longitudinal one from respiration; but when the artery became somewhat tortuous, from bending the head forwards, the vessel, completely denuded of the contiguous substances, suffered some additional flexion from the systoles of the heart. But there was no perceptible movement of dilatation or contraction. The mean of three measurements of the circumference of the artery, in the same spot, was $\frac{243}{400}$ of an inch.

The left carotid was now exposed in a similar way; and one or two attempts were made to confine the blood in a portion of it included between two ligatures; but it was found very difficult to prevent the evacuation of the insulated part through some openings within one or other of the ligatures. At last, by straining two ligatures at the same instant, a portion of the artery, of about an inch and a half in length, was left distended with blood, although it had

evidently contracted itself to a smaller magnitude than when it was first exposed. The circumference of this part was found to be $\frac{200}{400}$ of an inch.

In the part so included, when it was compressed between the finger and thumb, there was an obscure pulse, exactly corresponding with that of the portion before the ligature.

The included part being now punctured with a lancet, blood flowed from it, but with little force. It being then measured in the same part as before, was found to be in circumference $\frac{160}{400}$ of an inch.

The sheep was now killed, by opening the left carotid just before the ligature. This was at $\frac{1}{4}$ past five. The animal was five minutes in dying. Ten minutes afterwards, the right carotid being measured in the same point as before, was reduced in circumference to $\frac{170}{400}$ of an inch.

The sheep was immediately skinned, and the abdominal viscera removed.

September 14, $9\frac{1}{2}$ a. m.

The right carotid, being now measured, was found to be in circumference $\frac{186}{400}$ of an inch.

EXPERIMENT XVII.

Wednesday, September 27, $3\frac{1}{2}$ p. m.

Mr. COOMBS, in the presence of several pupils of Mr. GEORGE NORMAN, and myself, cut down to the right carotid of a stout Ram Lamb in perfect health, and six months old.

A ligature was placed round it, but not tightened, and the artery was carefully measured before and beyond the place of the ligature. The circumference before the ligature was $\frac{273}{400}$ of an inch; that beyond it was $\frac{244}{400}$ of an inch.

The ligature being then tightly drawn, the artery was again measured in the same spots as before. The circumference before the ligature was $\frac{270}{400}$ of an inch; and beyond it, was $\frac{251}{400}$ of an inch.

No pulsation could be felt, by any mode of examination, beyond the ligature; but when the artery was compressed between the finger and thumb, and the pressure was suddenly diminished, the sides of the artery struck against the finger and thumb so as to simulate a pulse.

About $\frac{1}{4}$ of an inch, or a little more, beyond the part where the ligature was made, a branch was given off by the carotid.

The left carotid artery was now exposed, and as quickly as possible tied.

Scarcely any blood flowed during the operations.

When the lamb was taken off the table, he seemed to be greatly distressed. He attempted several times to rise, but ineffectually; at last he got on his feet, his whole body and limbs trembling, and the latter bending under him, so that he stood with difficulty, and in attempting to walk, dragged his legs after him, and soon fell on his right side. His head was bathed with a cold sweat; and at various times, for half an hour, he got up and ineffectually repeated his attempts to walk, in the manner before described.

He was at length taken away and laid on straw, from which, when we last saw him, which was three quarters of an hour after the commencement of the operation, he had made no farther efforts to rise.

On the afternoon of the 28th, he stood, but with difficulty; shewing little sensibility when he was approached or even touched; and his nose being always directed downwards towards the ground. He had tried to take food; but after he had chewed it, dropped it out of his mouth.

From this period he eat nothing, but thick barley gruel was once or twice poured down his

throat. He stood always in one place, with his head to the ground, apparently unable to see, and shewing no fear, or other marks of perception, either when he was touched, or a violent noise was made close by his ear.

On the morning of the 3d of October, he died.

He was dissected by Mr. COOMBS the next morning. No nerve had been included in either of the ligatures. The parts round the arteries operated on were healed, and no inflammation existed within or without the vessels; both of which had coalesced at the points of ligature, apparently by means of coagulated fibrine; from which, in each, there was a conical coagulum of blood of $\frac{3}{4}$ of an inch in length before the ligature only, and unattached to the serous coat.

EXPERIMENT XVIII.

Immediately after the former experiment, both carotids were carefully examined, and afterwards tied, each with a single ligature, at the same instant, in a Ram two years old.

He struggled a great deal during the tightening of the ligatures; but soon afterwards, being taken off the table, he quickly raised himself on his feet, and having for a few seconds stared around in a confused manner, ran out of

the room into the yard with great strength, and without any apparent inconvenience.

No pulse was perceived in either artery beyond the ligature; nor could any dilatation or contraction from the systole and diastole of the left ventricle be observed.

EXPERIMENT XIX.

October 2, 1815, 3 p. m.

Mr. COOMBS, in the presence of three of Mr. G. NORMAN's pupils, and myself, examined the left carotid of a Ram, two years and half old, whose right carotid had been examined on the 2d of August.

The circumference of the artery, at the extremity of the incision next the heart, was $\frac{324}{400}$ of an inch; and $\frac{1}{2}$ an inch farther was $\frac{338}{400}$ of an inch. It was, however, certain, that much cellular membrane was left attached to the external coat of the artery.

A ligature was then firmly made between these two spots so measured. There was an exceedingly strong pulse in the artery immediately before the ligature, which appeared to move a short distance forwards at each systole of the heart, and to retreat during its diastole.

A slight pulsation was also perceptible to the finger, immediately beyond the ligature; but it was evidently produced by the motion of elongation or advance of the artery before the ligature, from the shock given by the blood; for it was felt only on the side of the finger and thumb next the ligature, but no farther. A little farther onwards, as half an inch, the impulse was only now and then perceivable, and then only on very slight contact; being wholly lost, if any greater pressure was made on the artery.

Another ligature was now firmly applied, at the distance of $1\frac{1}{6}$ of an inch farther. The circumstances as to the pulsation between these ligatures were exactly the same as in the former trial, just related.

No movement of any kind could be felt beyond the second ligature.

A third ligature was made at the distance of $\frac{7}{8}$ of an inch from the second. The pulsation first described was now felt between the two first ligatures, with the same modification as before; except that the pulse before the first being now somewhat less strong, that immediately beyond it was proportionably weakened.

Still no pulse existed beyond the second ligature. The portion of the artery between the

first and second ligature was now punctured, and the blood squeezed out of it; notwithstanding which, it gave the sensation of pulsation precisely in the manner first described, that is, of an impulse only against the side of the finger and thumb next the first ligature, when they were brought very near it; but in no degree along the compressing surfaces.

Hence it was evident, that it was merely a longitudinal stroke, continued on from the free portion of the artery above, and acting in a mechanical manner, but by no means arising from any dilatation of the empty portion of the artery which was so felt. In this instance, also, the longitudinal advance and retreat of the ligature, corresponding with the increase and diminution of impulse during the systoles and diastoles of the heart, were sufficiently perceivable even by the naked eye.

A ring of the artery between the two first ligatures, being now cut off at the point nearly corresponding to that last measured, was found to be extremely contracted, and on being opened and laid flat, was found to measure transversely, on its outer coat, only $\frac{1}{40}$ of an inch. It must, however, be remarked, that it had been conside-

rably denuded of its cellular membrane, subsequently to the former measurement.

Nearly the whole of the artery between the first and third ligatures was now cut away; and the wound, which was a very long one, was sewed up.

In this sheep, all the parts about the left carotid appeared much more vascular than usual, and discharged a considerable quantity of blood from cut vessels. One or two pretty large branches also arose from the carotid between the first and second ligatures. These branches were tied, before the examinations above described were made.

The right carotid, which had been exposed, but not tied, in August, was now examined by Mr. COOMBS, and tied with a single ligature.

EXPERIMENT XX.

October 3, 1815, 3 p. m.

Mr. G. NORMAN, in the presence of Dr. CHARLES PARRY, Mr. COOMBS, Mr. SEWELL, veterinary surgeon, four of Mr. NORMAN's pupils, and myself, laid bare for several inches the right carotid artery of a large old Horse, intended for the slaughterer.

The pulse, both before and after the horse was thrown, was 48 in a minute.

The circumference of the artery was $\frac{5\,16}{400}$ of an inch.

Its movement was very long and carefully examined.

No person present could discover the least degree of dilatation or contraction of the artery, during the systole and diastole of the heart.

No motion of a longitudinal kind occurred from respiration. In reality, the artery appeared altogether quiescent, except during, and for two or three seconds after, a violent struggle; in which case the artery once or twice bent itself outwards during the systole, and returned during the diastole. Then the artery became again quiescent, and without motion, as before, even when examined with a strong lens.

In order still farther to investigate whether there was any elongation, or longitudinal motion, of the artery, white thread was slightly wound round it in two places; but we could not discover any change of place, whether absolute or relative, in these threads.

A ligature was then firmly made on the artery. A slight pulsation was perceivable beyond the ligature, just as in the ram in the

preceding experiment; that is, when the artery was held between the finger and thumb, there was an impulse against that side of those parts which was next the ligature, but no perceptible effort at dilatation farther on between the thumb and finger. This phænomenon occurred to a greater distance from the ligature in this experiment, than in the ram in the preceding one.

A longitudinal movement was now observable in the artery beyond the ligature. When, however, the extent of movement was tried, by placing a piece of small thread across the artery, so as to correspond, during the diastole of the heart, with another piece placed on an adjacent muscle, which was motionless, the advance of the artery, during the systole of the heart, was not greater than the diameter of the thread.

A second ligature being made, an inch beyond the first, an impulse was capable of being felt beyond both the first and second ligature, in the manner already mentioned, and just as was described with regard to the ram in the preceding experiment, except that it extended to a greater distance.

A third ligature was now applied, more distantly from the second than the second from the first. The phænomena, as to impulse, were

precisely the same as with regard to the two former ligatures; extending themselves beyond the third ligature. All the ligatures were plainly perceived to move forwards at each systole of the heart, and to return back during the diastole.

The part included between the first and second ligature was punctured with a pair of scissars; but serum alone flowed out. A full hour had elapsed from the time of the second ligature.

The part between the second and third ligature was now punctured; and entire blood flowed from the wound. This last ligature had not been made more than a quarter of an hour.

The impulse was felt, just as before, beyond the second and third ligatures; and the first, second, and third, were affected by the systole and diastole of the heart, as before the punctures had been made.

The artery being now measured in the same spot as before, which was a little before the puncture between the second and third ligatures, was found to be in circumference only $\frac{252}{400}$ of an inch.

The horse was then ($4\frac{3}{4}$ p. m.) killed, by opening the left carotid artery.

During these experiments, the head of the horse was always in precisely the same situation with regard to his neck, as in an ordinary walking attitude.

The carotid being measured the next morning at 9 o'clock, was found to be in circumference $\frac{379}{400}$ of an inch.

In the space between the first and second ligatures, there were three small clots of black coagulated blood.

During the dissection down to the artery, a very minute branch springing from the carotid being divided, the blood sprang out to the distance of eight feet.

EXPERIMENT XXI.

October 6, 1815, $3\frac{1}{2}$ p m.

The right carotid of a Horse was laid bare by Mr. COOMBS, in the presence of Mr. SEWELL, two of Mr. G. NORMAN's pupils, and myself.

This horse was considerably stronger than the former. His pulse, after he had been thrown, was 52 in a minute; and during the dissection of the parts, previously to reaching the artery, a good deal of blood flowed.

The pulse was now, at a proper time after his having ceased to struggle, 72 in a minute,

and the respiration 24, with grunting or roaring exspirations.

The circumference of the artery, in a given part, was $\frac{636}{400}$ of an inch.

The head was placed in an easy and natural posture. We could not discover the smallest dilatation or contraction of the artery from the systole and diastole of the ventricle; but having placed on it a strait piece of white waxed thread in a line with another, on a stationary spot near, we perceived that the former line was protruded beyond the latter, at each systole of the heart, about one and a half time's the diameter of the thread which was stationary; and this distance, on accurate measurement, appeared to be about $\frac{34}{400}$ of an inch.

When the artery was firmly compressed between the finger and thumb of one person, a faint sensation of impulse could be perceived by another person, beyond the pressure, in the manner described, with regard to portions included between ligatures, in the last experiment.

In this, as in other experiments, when the neck was considerably bent, the carotid, where it was separated from its attachments, rose in a curve outwards at each systole of the ventricle.

He was now, at half-past five p.m., killed, by opening the left carotid artery.

Soon after he had ceased to move, the right carotid was measured, in the spot before examined, and found to be $\frac{390}{400}$ of an inch in circumference.

On the 7th, at 9 a. m. the circumference of the artery, in the same spot, was $\frac{460}{400}$ of an inch.

This horse laboured under no thoracic disease.

EXPERIMENT XXII.

October 11, 1815, $8\frac{1}{2}$ a. m.

Mr. COOMBS, in the presence of Mr. CHAPMAN, and myself, laid bare both carotids of a Ewe, $1\frac{1}{2}$ year old.

The left carotid was tied with two ligatures, exactly one inch asunder. No pulsation whatever could be perceived, either between them, or beyond the second ligature. The artery was then cut through half way between the ligatures, and the blood sprang out to a considerable distance from the vessel. The ends of the artery now immediately retracted themselves; and the distance between the ligatures, being measured, was found to be $\frac{502}{400}$ of an inch.

The extremities being now carefully examined, we saw the ligature next the heart propelled longitudinally forwards at each systole of the

ventricle, and retreat during the diastole. The more distant ligature also advanced towards the former by a retrograde motion, so also as to correspond with the systoles; but with this difference, that the advance was less in degree, and always a sensible, but extremely short, space of time after that of the other portion. The difference in time was indeed so small, that the advance of the more distant portion seemed to have taken place, before the retreat of the near portion was sensibly begun. No other movement of the artery, besides the longitudinal one, was at all perceptible.

During these observations, the neck of the sheep was always stretched in the same degree.

A small ring was cut off from the farther extremity of the artery.

The carotid on the right side was now tied with one ligature.

The ewe did not seem to be in any way materially affected by these operations; but died two or three days after, from inflammation and suppuration about the left side of the trachea. In the artery on the right side, there was little appearance of external inflammation round the tied part of the artery; in which, before the

ligature, there was a coagulum of blood, of about an inch in length, but none beyond the ligature.

EXPERIMENT XXIII.

October 12, 1815, $3\frac{1}{2}$ p. m.

Mr. COOMBS, in the presence of Mr. GEORGE NORMAN, Mr. CHAPMAN, Mr. HUNT, and myself, laid bare the two carotids of a Ewe, $2\frac{1}{2}$ years old.

The ewe was very small. The circumference of the right carotid was $\frac{233}{400}$ of an inch.

The left carotid was then tied with a single ligature. Five minutes afterwards, the right carotid, being again measured in the same place as before, was in circumference $\frac{257}{400}$ of an inch.

The right carotid was now tied with a single ligature; and, a few minutes afterwards, appearing to have shrunk, was again measured half an inch before the ligature, and found to be in circumference exactly $\frac{233}{400}$ of an inch, as before the tying of the opposite artery.

The left carotid, appearing to be still more reduced in size, was measured before the ligature, and found to be only $\frac{170}{400}$ of an inch in circumference.

This ewe, at the time, suffered little apparent inconvenience from the ligatures; but died on

the morning of October 14. The evident cause of death was inflammation, which had spread itself to all the adjoining parts. The parts tied had, on both sides, coalesced. In the left carotid there was, before the ligature, a thrombus of about half an inch in length; but neither any coagulum, nor even blood, in the artery beyond the ligature.

On the right side there was a thrombus both before and beyond the ligature; that beyond being much shorter and smaller than that before. Here also the artery, beyond the farther thrombus, was empty of blood.

The tied part was on neither side surrounded either by blood or fibrine; and the artery had not coalesced with the adjacent cellular membrane.

EXPERIMENT XXIV.

October 14, 1815, $8\frac{1}{2}$ a. m.

In the presence of Mr. CHAPMAN, Mr. WOOD, and myself, Mr. COOMBS laid bare both carotids of a Ewe.

The circumference of the left carotid was $\frac{225}{400}$ of an inch; of the right $\frac{222}{400}$.

After the left carotid had been denuded about half an hour, it appeared so contracted for some

length about the middle of the exposed portion, as to induce us to measure it. It was in circumference only $\frac{162}{400}$ of an inch. The parts above and below, which were only just laid bare, appeared proportionably increased in size. The circumference of the artery half an inch beyond, being measured, was $\frac{284}{400}$ of an inch. While we were about to measure the portion before the contracted part, we found it in this short space of time so reduced, that we desisted from our purpose.

The left carotid was then tied beyond the measured part.

Twelve minutes afterwards, the right carotid, measured in the same spot as before, was found to be in circumference $\frac{120}{400}$ of an inch.

The pulse in the left artery, before the ligature, was strong and distinct; but was not perceptible beyond it, except faintly, when the finger pressed against the ligature. The longitudinal advance, during the systole of the ventricle, was scarcely discernible.

The animal was then blooded in the left jugular vein in the following quantities by measure, and with the following effects:

Oz.		*Inch.*
8	Circumference of the right carotid after the bleeding, - - - - -	$\frac{258}{400}$
	Pulse in the carotid good.	
8	Circumference - - - - -	$\frac{250}{400}$
	Pulse weaker.	
8	Circumference - - - - -	$\frac{238}{400}$
	Pulse as before.	
8	Circumference of the right carotid -	$\frac{235}{400}$
	Pulse slower, and much the same as to apparent strength, but more jerking, and the longitudinal movement stronger.	
8	Circumference - - - - -	$\frac{222}{400}$
	Blood flowed very slowly. Pulse weaker. Jerking increased.	
8	Circumference - - - - -	$\frac{201}{400}$
	The artery easily compressible. The jerking less, and the pulse very weak, and to be felt only on that side of the finger and thumb which was nearest the heart.	
8	Circumference - - - - -	$\frac{191}{400}$
	Pulse quicker, and very weak.	
8	Circumference - - - - -	$\frac{161}{400}$
	Blood required to be rubbed out of the vein. Pulse very quick, small and weak.	

Oz.		*Inch.*
$7\frac{1}{2}$	Circumference immediately after death	$\frac{160}{400}$
	Convulsive and stertorous respiration now occurred, and the animal dying, no more blood could be obtained.	
	Circumference about five minutes after death, - - - - --	$\frac{213}{400}$
	Ditto - - ten minutes after death	$\frac{234}{400}$
	Ditto - - $4\frac{1}{2}$ hours after death	$\frac{233}{400}$
	Ditto - - $11\frac{1}{4}$ hours after death	$\frac{232}{400}$
	Ditto - - 23 hours after death	$\frac{232}{400}$
	Ditto - - $28\frac{1}{2}$ hours after death	$\frac{232}{400}$

Putrifaction had now begun.

EXPERIMENT XXV.

October 24, 1815, $8\frac{1}{2}$ a. m.

Present Mr. CHAPMAN, Mr. WOOD, Mr. SALMON, and myself.

Mr. COOMBS opened the left side of the thorax of a Rabbit, so as to exhibit the aorta, just beyond the curvature.

The lung on that side was perfectly collapsed on the apex of the heart, the motions of which were very distinct. As the apex shifted its place at each systole of the ventricle, the lung was strongly moved with it, and returned to its place at the diastole. The aorta was

clearly seen, in a strong sunshine, for a full inch and a half in length. For some minutes it had no apparent movement; but afterwards, as the animal approached towards death, a very slight one, scarcely distinguishable without a strong magnifier. This movement was longitudinal, and sometimes a little lateral, on one side only; but never in any degree that of dilatation or contraction. It exactly accorded with the systole of the heart, which was slow and distinct. In a short time the animal died; immediately on which the aorta became very sensibly diminished in diameter; but as the heart continued to beat, a very slight longitudinal motion still remained, corresponding with the systole.

EXPERIMENT XXVI.

October 25, 1815, $8\frac{1}{2}$ a. m.

Present Mr. CHAPMAN, Mr. WOOD, Mr. SALMON, and myself.

Mr. COOMBS laid bare the right carotid of a Ewe.

Its circumference was $\frac{210}{400}$ of an inch; but it almost immediately shrunk through the whole space which was exposed, so as to become in circumference only $\frac{188}{400}$ of an inch. At the same time a portion of the artery, before the contracted

part, and which had been more recently exposed, was $\frac{254}{400}$ of an inch.

The pulse in the dilated part was very strong and full; that in the contracted part very weak and soft.

The shrunk part being now farther reduced by a ligature of $\frac{143}{400}$ of an inch in length, the pulse beyond the ligature was very faint and weak, and still very strong in the dilated part before it.

The ligature being removed, we were about to measure the dilated part, but found that it also had begun to shrink.

The left carotid, being now exposed, appeared evidently much larger than the right; but immediately contracted itself, so as to defeat our intention of ascertaining its enlargement by measure.

On re-examining the right carotid, we now found it to be again visibly enlarged. It proved to be in circumference $\frac{193}{400}$ of an inch; but during the measurement, began to shrink, and in a few seconds, appeared to be as small as before.

The pulse was now 94 in a minute; and the respiration 42, and irregular.

EXPERIMENT XXVII.

December 11, 1815, 9½ p. m.

Mr. COOMBS, in the presence of Mr. CHAPMAN, Mr. WOOD, and myself, laid bare the right carotid of a small Ewe.

Its circumference was $\frac{220}{400}$ of an inch.

The left carotid was then exposed, and being tied with packthread in two places distant from each other, so as to intercept the flow of blood, but not break the internal and middle coat, an opening was made between the ligatures, with a view to introduce a small goosequill longitudinally into the artery. It had, however, so shrunk by having its blood evacuated, that the quill could not be made to pass up it. A crow quill was then introduced, but was not sufficiently large to fill the artery ; so that, when the farthest of the ligatures was loosened, the blood flowed out through the wounded part of the artery. The coats of the artery being firmly tied on the quill just above and below the wound, the ligature nearest the heart was removed. The pulse before the quill was good, but none could be perceived beyond it.

This experiment, however, is inconclusive, not only from the smallness of the quill, but be-

cause, when it was removed, the blood was found to have coagulated in it.

Three firm ligatures having been made round the artery, of which one was on each side of the part containing the quill, and the third beyond the second, the ligatures round the quill were removed, and the quill itself taken out.

The distance between the first and second ligature was found to be $\frac{648}{400}$ of an inch. The artery being then cut through between the second and third ligatures, the two ends immediately retracted. The distance between the first and second ligature being now a second time measured, was only $\frac{349}{400}$ of an inch.

Professional avocations prevented the farther prosecution of the experiment at this time.

December 12, 1815, $9\frac{1}{2}$ a. m.

The parts incumbent on the right carotid were found to have in some degree united.

The artery, being again exposed, was measured as nearly as possible in the same spot as on the preceding day, and found to be in circumference $\frac{222}{400}$ of an inch. This part, however, appeared to be considerably less than the adjacent portions of the artery; and the pulse in

it was much weaker and softer than in the more dilated portion immediately before it.

The ewe was then blooded from the left jugular vein, with the following effects on the circumference and other circumstances of the carotid artery, the action of the heart, and the respiration:

	Circumference of the Artery. Inch.	*Pulse.*	*Respiration.*
Before bleeding - - - -	$\frac{222}{400}$	124, and regular - -	24, chiefly by the diaphragm.
After bleeding, - - - -			
8 ounces of blood taken from the left jugular. The first 4 ounces were as florid as arterial blood; the second four ounces of a dirty red colour.	$\frac{196}{400}$	72 - - - -	24, ditto.
9 ounces taken away - - -	$\frac{178}{400}$	68 - - - -	24, and equal.
8 ounces - - - - -	$\frac{170}{400}$	80, and weaker - -	20, and unequal.
8 ounces - - - - -	$\frac{145}{400}$	100, more jerking, and apparently stronger - -	14, and irregular in every respect.
8 ounces - - - - -	$\frac{127}{400}$	116, still more bounding -	24, with quicker and harder exspiration.
8 ounces - - - - - Sheep faint.	$\frac{140}{400}$	116, as before - -	20, but irregular as to frequency and depth; and a good deal by the thorax.
8 ounces: flowing freely at the beginning, but gradually more slowly. - -	$\frac{141}{400}$	168, very bounding, and tolerably strong - -	16, but irregular; and the elevation of the thorax being often alternate with the descent of the diaphragm.
2 ounces - - - - - The jaw of the animal fell.	$\frac{145}{400}$	None - - -	Gone.
18 minutes after death - -	$\frac{158}{400}$		
27 minutes after death - -	$\frac{160}{400}$		
33 minutes after death - -	$\frac{169}{400}$		

ON THE

STRUCTURE OF ARTERIES.

THE larger arteries consist of three coats, of which the innermost is smooth within, and though very thin, is strong. Of the three it is said to be the least extensible; yet, in the living animal, it is evidently capable of bearing, without injury, a considerable degree of stretching. This coat is usually called Cuticular; but it has certain properties in common with peritonæum and other serous membranes; especially that of occasionally secreting, from inflammation, albumen or fibrine.

The next, or middle coat of arteries, is the Fibrous; which, from certain vital properties, has usually been considered as muscular. It must, however, be observed, that according to that accurate chemist BERZELIUS, fibrine, which forms a constituent part of muscle, and even of intestine, which has muscular powers, is not dis-

coverable in the coats of arteries. A similar conclusion has been formed, from chemical analysis, by Dr. YOUNG.

This middle or fibrous coat is attached to the inner by very thin cellular substance, which seems to be the seat of those bony lamellæ, so often found in the larger arteries, and which consist of phosphate of lime.

Immediately without the fibrous coat is the Cellular; which appears to be little more than consolidated cellular substance, gradually losing itself in a looser tissue of the same materials, which sometimes forms a sheath, including, not only the artery, but other near and important parts.

This is the general structure of those arteries, which are sufficiently large to become objects of accurate examination.

All these coats have their proper vessels, carrying either blood or colourless fluids. The serous coat usually exhibits the latter kind, while the others are more conspicuously furnished with those which convey red blood. During life, however, the cellular coat of arteries is so opake, that the blood circulating in its vessels is scarcely seen; but if a piece of an artery be cut out of a living animal, and suffered to dry, it

becomes more or less red, probably from the transparency, which its substance acquires from that desiccation.

The arteriæ arteriarum are rarely, if ever, derived from the trunks which they surround; but are minute ramifications of those which are more remote.

The fluids, effused by these vessels, are capable of being absorbed by lymphatics; with which, therefore, the coats themselves must be copiously supplied.

Arteries are also furnished with nerves, derived from the system of the ganglia.

ON THE

POWERS OF ARTERIES.

ARTERIES are usually supposed to be endowed with a capacity of motion derived from two sources.

The first is Elasticity; a merely mechanical power, in consequence of which, when forcibly distended, compressed, or elongated, they spontaneously return to a mean state, as soon as the force is removed. So when an artery is stretched beyond a certain length, it is said to resume its former state by elasticity alone. A similar power tends to contract it, when it is forcibly dilated, and to expand it into its usual cylindrical form, when it has been forcibly compressed.

The second is a vital faculty, presumed wholly to reside in the middle or fibrous coat; which has, therefore, been denominated muscular.

I have already remarked, that this coat is destitute of fibrine, which has been supposed to be essential to the composition of muscle; whence the learned author last quoted infers, that the only power of dilatation and contraction in arteries is derived from their elasticity. Although, however, as BICHÂT has rightly observed, parity of structure implies parity of function, it would be highly rash to assume the converse of that proposition, and to assert, that muscular power exists no where but in muscular fibres, or in substances containing fibrine. That question should be solved, not by any presumed analogy, but by a direct observation of facts; and into these and other properties of arteries, it is the object of this work, by experiment, to inquire.

With regard to the vital power of the middle or fibrous coat of arteries, physiologists have generally agreed, that it acts merely in the direction of the circumference; in consequence of which, the light, or calibre, is diminished when it operates, and to a certain extent increased, when it no longer acts. The first case, therefore, implies a shortening, and the second an elongation, of these fibres.

This power is, however, so far different from that, which, in muscles, is called Irritability, that an artery suffers no degree of contraction from a great variety of chemical and mechanical agents, called Stimuli; for none of these substances, applied to any part of an artery, will cause it to contract. Neither is an artery capable of being thus acted on by what is called the Will.

The faculty in question seems more similar in its phænomena to that which, in muscles, has been called Tonicity; and which implies a mean state of contraction, existing during health, and capable of being increased by certain causes, and of being diminished or lost by certain diseases, as hemiplegia, &c.

In arteries we see this faculty carried to a great extent, even to the obliteration of their cavity; so that in cases in which they cease to convey blood, as in those of ligatures, or of the canalis arteriosus, &c. they become impervious cords.

Bichât has given to this power the name of *Contractilité par défaut d'extension*, and considers it as independent of life.

It is, however, certain, that the contraction of an artery, to the extent of the coalescence of its parietes, takes place during life only; and

I shall hereafter endeavour to shew, that it not only does not exist after total death, but is actually counteracted by certain powers, which preponderate during that state.

Hence I have judged it convenient to give this faculty of vital contraction the name of Tonicity.

With regard to the term itself, I wish to be understood as using it to signify, not a cause, but an assemblage of phænomena, so like, that when the word is employed respecting one of these phænomena, it serves to recall to the mind the chief phænomena comprehended under it. This, so far as I can see, is the only purport of all other abstract terms; which often include many objects, in various respects extremely dissimilar, but agreeing in certain assignable properties. Thus the term Animal comprehends not only an elephant, but a thrush, a minnow, a polypus, and a flea. If, however, any one shall discover an important difference between these phænomena, and shall assign them, severally or collectively, more distinctive, and therefore better denominations, he is doubtless at liberty to employ them.

Mr. HUNTER has attempted to demonstrate the existence and degree of tonicity in arteries

in the following way. In a horse, bled to death, he cut off rings of various arteries, and having slit them in the longitudinal direction of the vessel, measured their transverse length, as they were gently laid flat. Having then stretched them out in the same transverse direction, so as to overcome their vital or tonic power, he allowed them to contract by their elasticity; and, having again measured them as before, judged that the difference between their present and first length was the measure of their tonicity.*

To this and all other similar modes, there seems to be an *à priori* objection. If the contraction by tonicity, or muscular power, is the result of life, we should be disposed to conclude, that, as soon as the parts had totally lost that faculty, the moving fibres would relax themselves, and recover their former length, especially when acted on by the elongator, or elastic cellular coat. If, also, we admit, that a ring of an artery, so treated, still retains some degree of vital power, it is evident, that as no rule can be assigned for the sum existing at any given period after apparent death, no conclusion can be formed as to that, which, in any case, existed during life. Hence this mode of calculating the

* Treatise on the Blood, &c. p. 124, 125, 126.

tonicity of arteries, though it may, under certain circumstances, prove its existence, yet, as a measure of its degree, must, as this candid author himself acknowledges, be highly fallacious.

In order to ascertain the duration of the vital, contractile power of arteries, Mr. HUNTER chose, for the subject of his experiment, those of the umbilical cord, because, says he, "I could "confine the blood in them, and keep them dis- "tended for any length of time. In a woman "delivered on the Thursday afternoon, the navel "string was separated from the fœtus. It was "first tied in two places, and cut between, so "that the blood contained in the cord and pla- "centa was confined in them.

"The placenta came away full of blood; and "on Friday morning, the day after, I tied a "string round the cord, about an inch below the "other ligature, that the blood might still be "confined in the placenta and remaining cord. "Having cut off this piece, the blood immedi- "ately gushed out, and by examining the cut "ends of the cord, I attentively observed to what "degree the ends of the arteries were open, and "the blood having now all escaped from this "portion, the vessels were left to contract with

" the whole of their elastic power, the effect of " which is immediate.

" Saturday morning, the day after this last " part of the experiment, having examined the " mouths of the arteries, I found them closed " up; so that the muscular coat had contracted " in the twenty-four hours, to such a degree as " to close entirely the area of the artery. That " same morning, I repeated the experiment of " Friday; and on Sunday morning, observed the " result of the second experiment to be similar " to that of the former.

" On this morning, Sunday, I repeated this " experiment the third time; and on Monday, " observed that the result had not been the same " as before, the mouths of the arteries remaining " open, which shewed that the artery was be- " come dead.

" These experiments shew that the vessels of " the cord have the power of contraction above " two days after separation from the body."*

In this relation there seems to be an incongruity so great, that one wonders how it could have escaped the attention of the author. How all the blood in a small artery could have immediately escaped, except by a complete contact of

* Treatise on the Blood, &c. p. 116.

its sides from elasticity, and therefore how any subsequent diminution of its area could have taken place from the same, or any other power, it is difficult to conceive.

Without, however, cavilling about words, I several years ago determined to examine into the fact, which my situation as physician to the Puerperal Charity in this city, of which the Messrs. NORMAN are accoucheurs, and in which between three and four hundred women are annually delivered, gave me almost daily opportunities of doing.

Accordingly a placenta and cord were obtained, treated in the manner described by Mr. HUNTER. No sooner, however, was the cord cut through by Mr. GEORGE NORMAN, than the blood gushed out, and the arteries contracted to an almost invisible point, evidently incapable of any farther perceptible reduction of area.

Thinking, however, possible, what I am now farther inclined to believe is true, that, after the cessation of circulation in the cord, the arteries might, by their vital power, expel part of their blood into the placenta, I suggested the propriety of making three ligatures before the separation of the cord from the fœtus; in consequence of which, the blood would be more

perfectly retained between the two which were more remote, than in the former experiment. It turned out that the arteries actually appeared more turgid with blood, though from what cause I do not presume to say. The cord being now cut through between the ligatures, the blood was forcibly expelled, and the arteries at once contracted just as before.

Both experiments were made at a much earlier period after delivery, than that stated by Mr. HUNTER; and no diminution whatever in the opening of the arteries could be perceived the next day.

In his experiment, one day had elapsed between the delivery of the placenta, and the trial to which the funis was subjected. It was, therefore, proper to conduct the experiment precisely in the same manner. Accordingly, last summer, the funis of a placenta, delivered by Mr. COOMBS at eleven o'clock on Friday morning, had two ligatures firmly made on it previously to its separation from the fœtus. The blood being thus retained, and at twelve o'clock at noon, on Saturday, a new ligature being made on the cord, at some distance from that which remained, all the vessels appeared turgid with blood. The placenta being then placed

below, and the cord held up in a perpendicular direction, so that no blood could escape by its own gravity, the cord was cut through between the ligatures. The blood immediately sprang out with considerable force, and even for some short time continued to ooze from one of the arteries, leaving the orifices of both much more patulous than in the two former experiments. Another ligature was made farther on, and the intervening part cut through, with precisely the same effects as before. All the portions thus divided were put into clean water, and laid aside until half-past twelve on Sunday. The various sections being then examined, no difference in the size of any of the arterial orifices from that of the preceding day could be perceived. So far, indeed, were these arteries from being fully contracted, that some of them still contained blood.

The result of this experiment, therefore, differed from that of the two former, inasmuch as the tonicity of the arteries seemed to have been totally or nearly lost, previously to the several sections.

These different modes appearing too indistinct or uncertain to lead to any decisive conclusion as to the facts which they were intended

to ascertain, it seemed expedient to prosecute the enquiry by other experiments.

In a late work, I stated the well-known fact, that, in the dead body, the arteries are usually found more or less deprived of their blood; and an attempt was made to explain this fact, by supposing that, after apparent or animal death, arteries were contracted by their tonicity beyond that degree which would have been otherwise permitted by their elasticity. Hence the blood would be, in a certain proportion, expelled into the veins; but the tonicity soon ceasing, with the other powers of organic life, the arteries would be dilated by their elasticity, and would therefore remain more or less robbed of their blood.*

In order to ascertain this point, the following experiments were made.

The carotid artery of a living Ram having been detached from all the surrounding parts, its circumference was accurately measured with some fine thread, and found to be $\frac{260}{400}$ of an inch. The animal was then killed by a cord passed round the trachea, without including any other part. The artery being again measured precisely in the same spot, within five minutes after apparent death, was found to be in cir-

* Elements of Pathology and Therapeutics, v. i. parag. lxiii.

cumference only $\frac{190}{400}$ of an inch. Thus the circumference appeared to have been reduced $\frac{70}{400}$ of an inch.

In order to discover whether any, and how much, of this reduction depended on tonicity, it was presumed that a subsequent period might be found, when the tonic contraction would be entirely lost, and the artery would be brought to that state, which was the result of the mechanical power of elasticity.

Accordingly, twenty-one hours and a half after the last measurement, putrifaction having already begun about several parts below the diaphragm, the artery was measured in the same place a third time, and proved to be in circumference $\frac{222}{400}$ of an inch.

Hence, of the whole contraction of the circumference, or $\frac{70}{400}$ of an inch, that by tonicity was $\frac{32}{400}$ of an inch, and that by elasticity $\frac{38}{400}$.*

In another Ram, the circumference of the carotid before death was $\frac{379}{400}$ of an inch. The animal being killed by strangulation, as before, the vessel, measured in the same spot, six minutes after the first application of the ligature, was found to be in circumference only $\frac{270}{400}$ of an inch. Being again measured nearly twenty-

* Experiment XIII.

four hours afterwards, its circumference was $\frac{120}{400}$. Here, then, the tonicity was as $\frac{50}{400}$ of an inch, and the elasticity as $\frac{59}{400}$.*

Both these animals having been in a state of perfect health and great vigour, when the first measurements of the carotid were made, and being in a state of incipient putrifaction at the last measurements, we may fairly conclude the first set to be indicative of the condition of the several arteries during the possession of the full powers of life, and the last, of the same arteries during the total loss of those powers.

If it be objected to the accuracy of these conclusions, that the arteries had possibly contracted themselves previously to the first measurements, as is well known to happen under certain circumstances of exposure; I answer, that, being well aware of this effect, care was taken by the experimenters to perform the measurements in much less time than that, which ample experience had shewn to be necessary for that effect.

In a fat Wether, full-grown, the circumference of the carotid was $\frac{243}{400}$ of an inch. The animal was bled to death by an incision in the other carotid; and in fifteen minutes after this

* Experiment XV.

incision, the first artery measured in circumference $\frac{170}{400}$ of an inch. Sixteen hours after, the circumference was $\frac{186}{400}$ of an inch. Here the contraction by elasticity seemed to be as $\frac{37}{400}$ of an inch, and that by tonicity as only $\frac{16}{400}$ of an inch.*

In this experiment, however, it is probable that an insufficient allowance was made for the tonicity, and too great an allowance for the elasticity. In reality, one might at first view be disposed to conclude, that there were some remains of tonicity at the third measurement, since not only the interval was shorter, but the abdominal viscera had been removed immediately after death, and the animal was hung up in a cool place, for the express purpose of supplying the table. Hence the carcase was perfectly fresh at the time of the third measurement, and continued so for four days afterwards; whereas, in the two former cases, from the retention of the blood and viscera, and exposure to a greatly heated atmosphere, putrifaction, as before observed, had already begun, and all tonicity was of course lost.

At a still more advanced and a cooler season, the right carotid of a large Horse was found to

* Experiment XVI.

be in circumference $\frac{636}{400}$ of an inch. He was killed by opening the left carotid; and as soon as he had ceased to move, the right, measured in the spot before examined, was in circumference $\frac{390}{400}$ of an inch. The next morning, fifteen hours and a half after the last measurement, the circumference was $\frac{460}{400}$ of an inch. Hence, of the whole first contraction, or $\frac{246}{400}$ of an inch, that by elasticity seemed to have been $\frac{176}{400}$ of an inch, and that by tonicity $\frac{70}{400}$. In this experiment, as in that last mentioned, from the shortness of the time, and the total want of putrifaction, it is possible that the tonicity was not entirely lost; and, therefore, that too great an allowance was made for the contraction of the artery by elasticity.*

In a fifth case, a Ewe, the right carotid of which, after a ligature on the left, was $\frac{320}{400}$ of an inch round, was bled to death; in consequence of which, the circumference of the artery was immediately diminished to $\frac{160}{400}$ of an inch, but afterwards rose to $\frac{232}{400}$ of an inch.†

With regard to the reasons suggested for the apparent defect of tonicity in Experiments XVI. and XXI. however specious they may at first appear, their validity is justly rendered doubtful,

* Experiment XXI. † Experiment XXIV.

by certain circumstances, which occurred in Experiment XXIV. some other results of which have just been detailed.

In this experiment it was found, that, at five minutes after apparent death, the circumference of the carotid had risen from $\frac{160}{400}$ to $\frac{213}{400}$ of an inch; and at ten minutes after death, to $\frac{234}{400}$ of an inch, which was the full dilatation which it reached, even after the commencement of putrifaction.

Now as in Experiment XVI. the first measurement after apparent death is stated as having been at ten minutes, it is not improbable that the artery had already begun to dilate by a diminution of its tonicity; and consequently, that the whole sum of the tonicity is not accounted for.

A similar deficiency may have taken place in Experiment XXI. in which no period is assigned for the first measurement after apparent death. To which may be added, that, in the latter case, the animal had undergone some previous experiments, the relation of which would not be apposite to our present purpose, but which might have had some influence on the tonicity of the artery during life.

Another point, also, deserves attention. It appears from Experiment XXIV. that in an ani-

mal, whose heart, after large blood-letting, still beats, and respiration continues, the carotid may, by that process, have been reduced to the utmost degree of which it is capable, after those functions have entirely ceased. Since also, as far as that experiment goes, it appears that the tonicity of an animal bled to death is very quickly evanescent, and since the instances of apparent defect of tonicity, which I have adduced in Experiments XVI., and XXI. were only those of animals killed by bleeding; it may be conjectured, that the dilatation by elasticity had in these cases already begun to take place, and, therefore, that the maximum of contraction had elapsed, and was not fully measured.

This conclusion receives great additional force from what occurred in Experiment XXVII., in which, after the loss of forty-one ounces of blood, the circumference of the carotid of a Ewe was reduced to the utmost degree that it could reach by apparent death: Subsequently to which period, although the ewe was still alive, the artery, after the farther loss of eight ounces of blood, dilated $\frac{1}{4}\frac{1}{0}\frac{1}{0}$ of an inch; and after the additional loss of ten ounces, which finally killed the animal, the dilatation was immediately increased $\frac{5}{4}\frac{1}{0}\frac{1}{0}$ of an inch. Eighteen minutes af-

terwards, the circumference rose $\frac{13}{400}$; in nine minutes more $\frac{2}{400}$, and in six minutes more $\frac{9}{400}$. Hence, it appears, that while, after large evacuation of blood, life remains, the tonicity of an artery may have entirely lost its preponderance over the elastic power, although some time may still elapse before the ultimate effect of the latter shall have taken place.

In most of these cases, the changes produced on the size of the arteries, at the different stages of the experiments, were so great, as to be immediately perceptible, on the slightest examination. Neither were they confined to the carotids only; for in the femoral of a sheep, and in the aortæ of rabbits, they were sufficiently conspicuous, although their degree was not ascertained by actual admeasurement.* The general result corresponds with those related above with regard to the umbilical cord; in which the contraction of the arteries was entire after a short period, but ceased at the end of twenty-four hours.

Whether the difference observed in the funis in Mr. HUNTER's experiment depended on the season, the desiccation of the arteries, or any other assignable cause, his narration does not enable us to judge.

* Experiments XI. XII. XIII. XIV. XXV.

It is, indeed, probable, that the proportion between the tonic and elastic powers may greatly vary, not only in different arteries, and in different animals, but in the same individual animal at different times. So also the distention of an artery by its contained blood may vary, with the same modifications as those just expressed; so that any single experiment, such as I have described, can only be considered as relative to those circumstances.

In the mean while, the general principle of the existence of two moving powers in arteries of warm-blooded animals is, by these experiments, illustrated; and the theory of the cause of the greater or less bloodlessness of the larger arteries after death, advanced in the work, to which I have alluded, is fully established.

The facts, which I have adduced, lead to conclusions as to the usual state, and relative proportions, of the moving powers of arteries, very different from those of preceding physiologists, and more especially Mr. HUNTER. That writer considers "the natural pervious state of an "artery, to be that to which the elastic power "naturally brings a vessel, which has been "stretched beyond, or contracted within, the "extent which it held in a state of rest. The

" stretched is that state produced by the impulse " of the blood in consequence of the contraction " of the heart; from which it is again brought " back to the natural state by the elastic power, " perhaps assisted by the muscular."* From this, and several succeeding passages in the same work, it appears to have been the opinion of Mr. HUNTER, that the natural state of an artery during life is that, to which the elastic power spontaneously brings it, when no longer forcibly distended by the systole of the heart; that is, during the diastole of that organ.

The question, whether any such difference from the systole and diastole of the left ventricle, as is assumed by Mr. HUNTER, usually obtains in a living artery, will be a subject of subsequent inquiry. In the mean while, the experiments, which I have related, demonstrate, on the contrary, that, during health, the larger arteries of a living animal, as well under the diastole as systole of the left ventricle, are in a state of distention, to which they are forcibly impelled by their contained blood, against their mechanical power of elasticity. This was in fact the case in the experiment of the arteries of the umbilical cord, described by Mr. HUNTER himself;

* Hunter on the Blood, &c. p. 116.

who admits, that the sudden expulsion of the blood, when the cord was divided between two ligatures, was the result of the operation of the elastic power, acting immediately on the contained blood, as soon as the obstacle to its action was removed.

In order farther to enquire into this point, two ligatures, distant from each other an inch and a half, were at the same instant tightly drawn on the carotid of a living sheep. This artery, measured half way between the ligatures, was found to be in circumference $\frac{200}{400}$ of an inch. The blood being suffered to flow out of this included part, by means of a puncture with a lancet, the artery in the same spot as before, measured in circumference only $\frac{160}{400}$ of an inch. Hence the circumference appeared to have been immediately reduced $\frac{40}{400}$ of an inch, by the mere evacuation of the blood contained in the artery.*

In a Horse, the right carotid artery was in circumference $\frac{516}{400}$ of an inch. After a short time had been occupied in examining its movements, it was firmly tied at some distance before the part measured. Some farther observations having been made, a second ligature was applied an inch beyond the first; and, in an hour after-

* Experiment XVI.

wards, care being taken to avoid the point measured, the portion between the ligatures was punctured. Little more than serum flowed out; the blood, as appeared from subsequent examination, having coagulated in the artery. The vessel being now measured in the same part as before, was found to be in circumference only $\frac{252}{400}$ of an inch. The next morning, sixteen hours and a quarter after the animal had been killed, by bleeding from the left carotid, the circumference of the artery, at the same point, was $\frac{279}{400}$ of an inch. Hence the elasticity was as $\frac{137}{400}$ of the circumference of the artery, and the tonicity as $\frac{127}{400}$.*

From these two last experiments, it appears, that the forced distention of the artery does not necessarily depend on the impulse of the blood from the systole of the ventricle, since it existed when the communication with the stream of blood from the heart was interrupted by a ligature.

As, during the common circumstances of health, arteries are naturally distended beyond that degree, to which they would spontaneously contract by their elastic power, so they are capable of suffering a still greater degree of

* Experiment XX.

distention, from increased fulness of blood in the general system, or from those excessive determinations of blood to particular parts which occur, on various occasions, in the animal frame. Thus it is generally admitted, that, if the passage of the blood through one arterial trunk is intercepted, the collateral branches, supplying the part, undergo an increased distention, in order fully or partly to compensate the loss.

This conclusion was rendered very probable by what occurred in the Ram, Experiment XV.; in which one carotid had been tied nearly eleven months before, and the other was found much larger than any that we had before observed.

In order, however, more accurately to investigate this point, the right carotid of a small Ewe having been found to be in circumference $\frac{233}{400}$ of an inch, the left carotid was tied; a few minutes after which, the right had gained in circumference $\frac{24}{400}$ of an inch.*

In another Ewe, the right carotid, being $\frac{222}{400}$ of an inch in circumference, was again measured twelve minutes after the left had been tied, and was found to have gained in circumference $\frac{98}{400}$ of an inch.†

* Experiment XXIII. † Experiment XXIV.

I know of no actual measurements to prove that a similar effect takes place, in inflammation and other maladies, in the larger arteries supplying the parts so affected; but we may reasonably presume its existence from the tangible, and sometimes visible, increase of size in the artery, as well as from the great perceptible enlargement of the veins leading from the diseased parts.

From various preceding considerations, it is natural *à priori* to conclude, that when, under the state not only of the usual healthy dilatation of arteries, but of that increased dilatation which often accompanies disease, the distending cause, which is a certain quantity or momentum of blood, is diminished, the elasticity will tend to contract them, so as, within certain limits, to accommodate them to the quantity of blood which they ought to convey. If, however, the elasticity alone should be inadequate to the requisite force or degree of contraction, the tonicity, or vital power, may assume the office of contraction, where it was left by the elasticity, and carry it to the necessary extent.

In order to ascertain how far this accommodation of arteries to the loss of blood from the system would reach, I suggested the Experiment

XXIV.; in which, the circumference of the right carotid in a Ewe having been first raised, by a ligature on the left, from $\frac{222}{400}$ of an inch to $\frac{320}{400}$, was successively diminished to $\frac{288}{400}$, $\frac{250}{400}$, $\frac{235}{400}$, $\frac{235}{400}$, $\frac{222}{400}$, $\frac{201}{400}$, $\frac{191}{400}$, and $\frac{161}{400}$ of an inch, by as many blood-lettings of eight ounces each, from the left jugular vein, till the death of the animal.

So, also, in Experiment XXVII., the right carotid of another Ewe, which was $\frac{222}{400}$ of an inch in circumference, was successively reduced to $\frac{196}{400}$, $\frac{178}{400}$, $\frac{170}{400}$, $\frac{145}{400}$, and $\frac{127}{400}$ of an inch, by similar bleedings from the same vein.

Hence we may understand, how readily the arteries may become more or less capacious, in proportion to the quantities of blood which may either exist in the whole system, or may be determined to particular parts.

It is, however, probable, that the relative capacities of entire arteries, or of certain parts of them, may be affected by other agents, besides the mere immediate quantity of blood which they contain. This evidently happens under certain circumstances of the exposure of arteries in living animals. Thus, in Experiment XIII., a very narrow ring of a carotid became, while it was under examination, contracted, as if a

small ligature had been half tightened around it. So, also, in Experiment XXIV., a portion of the left carotid of a Ewe, which was $\frac{225}{400}$ of an inch in circumference, after having been half an hour denuded, was found to be only $\frac{162}{400}$ of an inch; while at the same time the continuous portions before and beyond, which had been more recently laid bare, appeared, even to the eye, to be proportionably augmented. One of these portions, which was beyond the contracted part, was measured, and found to be $\frac{284}{400}$ of an inch in circumference, or to have gained $\frac{59}{400}$; but while we were preparing to measure the other dilated portion, we saw it shrink to nearly the same size as the contracted part. Again, in Experiment XXVII. there was a considerable smallness of one portion of the artery, relatively to the adjacent parts before and beyond.

It is worthy of being remarked, that this partial reduction of circumference was found in the progress of the last experiment but one, to be the utmost extent which it was capable of reaching, from the want of the distending power of the blood within the artery.

In another instance, the different changes from dilatation to contraction, and the contrary, arising under exposure, were more sudden than

any which we had before witnessed. In the same experiment, there was an increase of size in one carotid from a ligature on the other, and a spontaneous enlargement of the artery before the contracted part, just as had been observed in Experiment XXIV. In the present instance, that enlargement was capable of being tolerably well ascertained by measurement; and was found to be $\frac{44}{400}$ of an inch.*

To what immediate cause these sudden changes under exposure are owing, I am unable to suggest. In my experiments they occurred almost wholly in Ewes, and in a few cases only of these. I know not whether this was in any way connected with the colder season, at which the experiments, exhibiting those phænomena, were made. That the contraction is not owing to mere cold, is at first sight probable; since, in one experiment, a long-continued application of ice to the denuded carotid of a ram, had no effect whatever of this kind. This fact, however, affords only some degree of probability; for the difference may have originated in a less degree of excitability in the living arterial coat of the male, which was exposed to this trial, than in that of the female.

* Experiment XXVI.

These contractions were, however, much too sudden and apparent, to vitiate any of the conclusions deduced from the foregoing experiments, relatively to the changes produced on arteries, either by death, or by the evacuation of blood from the system or the part.

Neither could they, as it should seem, be in any way attributable to the mechanical power of elasticity; since they occurred in the most rapid manner, not only from enlargement to contraction, but the reverse, alike under similar states of desiccation, under the putrifaction and actual life of other parts of the system, and at the same time respectively in different parts of the same artery, exposed to the same causes of mechanical dilatation.

As, in all these experiments, no evidence exists, that the living fibres of the middle coat of arteries have, like the radiated fibres of the iris, any inherent power of elongation, from the application of any known stimulus; but, it appears, on the contrary, that their action is that of shortening or contraction, we are, so far, obliged to conclude, that, in the dilatation of an artery, these fibres are altogether passive. Hence it follows, that they are no farther concerned in that dilatation, than as, through defect of tonic exertion, they

may yield to causes of distention, from within or without.

So, also, with regard to the elasticity; it is obvious, that, as arteries are already dilated beyond the degree, which that agent, if not counteracted, would have permitted, any increase of dilatation must arise, not from an augmentation, but a diminution, of the action of that power. Hence, the dilatation, so far as respects elasticity alone, will be in the compound ratio of the momentum of blood in the vessel, and the want of resistance in its coats; so that, the contractile power of the coats being given, the dilatation will be directly as the momentum of blood in the vessel; and, on the other hand, the momentum being given, the dilatation will be inversely as the contractile power of the coats.

From these two positions taken in connection, we are, without any danger of refutation from the preceding experiments, compelled to admit as the cause of the common dilatation of the larger arteries, and still more forcibly of that which is preter natural, the mechanical distending power of the blood which they contain.

Hence it probably follows, not only that one carotid is dilated on the tying of the other, by

the mere mechanical impulse of a larger quantity of blood than it had usually received, but that, in the instance adduced of an unusual dilatation of an artery immediately before a portion, which was preternaturally contracted, and, in another instance, both before and beyond it, that dilatation was owing to an accumulation of blood, in some mode dependant on the neighbouring contraction.

This increase of diameter in adjoining portions of the same vessel does not, however, always occur, when the perviousness of the artery is completely destroyed by a ligature. Thus, in Experiment XXIII., the circumference of the right carotid, which was $\frac{133}{400}$ of an inch, and which was increased to $\frac{157}{400}$ by a ligature on the left, was reduced to its former dimensions by a ligature on the former beyond the measured part. The left carotid, also, which had been first tied, and had appeared to be nearly of the same size as the right, was now so shrunk before the ligature, as to be in circumference only $\frac{170}{400}$ of an inch.

From the various results which have been described, we are obliged to attribute to the larger arteries a much greater force and sum of tonicity than are admitted by HUNTER and

BICHÂT; who contend that, in this series, they exist in a very small degree.

In the first place, with regard to force, we have seen that it is capable of completely overcoming that elasticity, by which an artery is spontaneously dilated to a mean degree, but which ineffectually acts, until the tonicity is lost by the perfect death of the part.

Next, with regard to the sum or quantity of tonicity; it has been shewn that the degree immediately exhibited on the first abstraction of the blood, which counteracts the contraction of the artery, is nearly equal to the sum of the elasticity. If to this, also, we add the tonic power, by which, during life, an artery is farther capable of being contracted to a mere impervious cord, it must be admitted, that the tonicity greatly exceeds the elasticity.

All the conclusions, which have thus been formed as to the powers of arteries, are derived from observations made on the carotids only; but if we assent to those of Mr. HUNTER respecting the increase of tonicity proportionably to the diminution of diameter, the carotids in sheep will possess a low degree of this faculty. With some allowance, therefore, as to the relative sum of the elastic and tonic powers, the same

general principles may, doubtless, be extended to all; since the largest arteries have a considerable degree of tonicity, and the smallest, as Mr. HUNTER very properly observes, must have elasticity enough to maintain them in a state, sufficiently pervious to admit of the passage of blood through their cavities.

It is probable, that Mr. HUNTER's conclusion, with regard to the proportion of tonicity, respects the relative magnitude of vessels in the same individual animal, and not in different animals. Observation, however, has shewn me, that the aorta itself, both in the abdomen and thorax of rabbits, is capable of great visible reduction of diameter by death; although, on account of the smallness of those animals, these arteries in them appeared to be unfit objects, on which to attempt a measurement of the actual proportions of elasticity and tonicity. It is, also certain, that in the human aorta, several hours after death, a great diminution of the usual quantity of blood is often observable; whence, on the principles already laid down, we may infer, that this artery is possessed of a considerable degree of tonic power. The human aorta, also, like arteries of a smaller size, has been

found in a state of complete coalescence and imperviousness.

The facts, which have been related, afford a probable reason why, in the experiments of HALLER, the circulation of the blood proceeded, for a certain time, in microscopic arteries, after ligatures on the aorta, or even the entire separation of that artery from the heart. The larger arteries continuing to contract by their tonicity, the blood, during that period, must necessarily have gone on suffering slow propulsion from them into the smaller or capillary branches of the same system, and through that into the veins.

It is, probably, from the same cause, that, in the experiments of HALLER and SPALLANZANI, not only a puncture in any part of an artery, in which the circulation had become either languid, or even had for a short time wholly ceased, occasioned an increase or renewal of circulation; but the blood in adjacent portions of the same vessel, or in neighbouring vessels, flowed towards the puncture, sometimes in a retrograde direction, and at other times against its own gravity. For, if the whole arterial system, stretched beyond what may be termed its natural degree, were spontaneously tending towards contraction by its elasticity or tonicity, or both, and the

resistance to this contraction were any where diminished by a puncture, it is evident, that to that point the blood, if fluid, would be compelled to flow from within a certain distance all around the punctured part.

Although, however, this contraction of vessels is the probable cause of the phænomena just described, to which HALLER gives the name of "Derivation;" I am far from asserting, that it is the cause of all the motions of the blood observable in arteries, after the influence of the heart on it has been destroyed. The experiments of HALLER abundantly prove, that irregular movements of this kind may occur from mechanical agitation, from gravitation, from the attraction between the coats of the vessels and the globules of blood, and probably of these globules towards each other. All these movements are, however, most essentially different from that which arises either from the systole of the left ventricle of the heart, or from the gradual contraction of vessels by their own inherent powers, after the cause of distention has been wholly, or in part, removed.*

* See Haller; Memoire sur le Mouvement du Sang, I. chap. v. vi. Memoire II. p. 338 to 341. edit. Lausanne. Another cause of these movements has also suggested itself to me; but I have not had time to inquire into the fact by actual experiment.

The existence of such a process, in certain animals of warm blood, has been sufficiently proved by the preceding experiments. HALLER, indeed, asserts, that, in the arteries of certain cold-blooded animals, as frogs and toads, he could perceive no constriction whatever. To me, however, it seems, that to this opinion there are several powerful objections. For, first, his conclusion on this subject is partly theoretical; being apparently founded on the fact, that, in these animals, the systole of the heart produces no more dilatation of the arteries, than if they had been tubes of glass; whence he infers, that, where there is no dilatation, there can be no contraction. In this reasoning, therefore, like other writers, he confounds the occasional tonic contraction, with a supposed uniform contraction during the diastole of the ventricle.

He, however, speaks of the want of contraction, as a matter of actual observation. But even this circumstance is by no means conclusive, since, in the first place, his observations are, for the most part, made on microscopic arteries, too small to admit of any mode of measurement; and, secondly, these amphibia are so tenacious of life, that the arteries themselves, although of a larger size, might not have reached

that degree of contraction, which could be perceived by the eye, although it might be sufficient, by slow gradation, to carry on the blood, and to determine it to any aperture in the vessel. So little, indeed, does HALLER himself rely on his own observation, as to the want of contraction in the arteries of these amphibia, that he doubts whether the phænomena of derivation may not depend on "une contraction invisible et innée "des parois des vaisseaux."*

In his large work on Physiology, referring to that just quoted, he again assigns this movement of the blood after death, or after the exclusion of the arterial system from the agency of the heart, to a contraction, which depends neither on life, nor on any muscular power, but on the native elasticity of the cellular coat. "Nunc "sincerus aliud phænomenon propono, quod non "videtur absque aliqua contractione intelligi "posse. Ea vero contractio a vita non pendet, "neque musculosæ est indolis, quæ a morte "supersit, et ad nativum potius elaterem tensæ "fibræ cellulosæ spectat. Ea enim alio etiam "in exemplo, sed lentè, et multis continuis diebus "arterias calidorum animalium resectas exinanit,

* Ibid. p. 162, 163.

"diametrumque minuit, et lumen delet."* For the evidences of the truth of this opinion, he refers to vol. i. page 72. Had this illustrious author been acquainted with the more recent discoveries as to the living power of arteries, he would not have attributed to elasticity alone the facts which he relates.

SPALLANZANI admits of two instances, in his experiments, of a considerable reduction of diameter, from depletion, in the descending aorta; but of what animals he does not inform us. It is probable, that he alludes to salamanders and green frogs, ("raines vertes,") though he denies any such reduction in various other experiments.†

He, however, falls into an error similar to that of HALLER; inferring, that the strongest support of the truth of that physiologist's opinion of an actual contraction in these vessels would be the discovery of their power of dilatation; with regard to which, however, his experimental results were different on different occasions.‡

I greatly regret, that various circumstances, attendant on the advance of the season, have

* Physiologia, lib. vi. sect. i. §. xi.

† Experiences sur la Circulation, &c. Diss. III. Exp. 136, 138.

‡ Ibid. p. 383.

obliged me to postpone any experimental inquiries into the nature and order of the phænomena of arteries, in the class of animals with cold blood.

Arteries, during life and health, are in a forced state, not only of lateral distention, but also of elongation. Hence, when they are any where divided, the cut ends suffer a considerable degree of retraction. In a Ewe, two ligatures on a carotid, having been made exactly one inch distant from each other, and the artery having been divided in the middle between them, the distance between the ligatures immediately became $\frac{502}{400}$ of an inch; so that the retraction was full one fourth of the length of the artery included between the two ligatures.*

Since, however, in this experiment, the increase of distance between the ligatures, on the division of the artery, was, perhaps, the sum of the whole contraction on each side, from the divided part to the remotest attachments of the vessel, I endeavoured to obtain a more correct result in the following way. Two ligatures, distant from each other $\frac{648}{400}$ of an inch, were made on the carotid of a living Ewe. A third ligature was now made beyond the second; and between these two the artery was divided.

* Experiment XXII.

Nothing then existing to counteract the contraction of every portion of the artery, the distance between the first and second ligatures was again measured, and found to be $\frac{549}{400}$ of an inch. Whence it appears that the retraction of this portion was $\frac{99}{400}$, or somewhat less than $\frac{1}{6}$.† It is probable, however, that, in this experiment, the proportion of retraction is over-rated; not only because the blood in the part measured had been previously evacuated, but because the agitation of the parts from respiration, &c. made it very difficult to keep the artery in a strait position, after it had been divided.

In both of these experiments, care was taken to preserve the head in a similar and easy position.

That power of dilatation and contraction in arteries, which the foregoing experiments have proved, not only to exist, but to be owing to a cause partly mechanical and partly vital, leads to the consideration of the nature and cause of the Pulse in this order of vessels.

† Experiment XXVII.

THE NATURE AND CAUSE OF THE ARTERIAL PULSE.

THE cause of the Pulse in the Arteries is a subject so interesting, that, from a very early period, it has occupied the attention of physiologists. With regard to this point, they are, however, still at variance.

HALLER plainly considers this phænomenon as owing to the alternate dilatation and contraction of the artery from the impulse, and comparative retardation, of the wave of blood, corresponding with the systole and diastole of the left ventricle of the heart. Thus, in his Elementa Physiologiæ; "Mihi quidem de phænomeno "ipso dubium nullum superest. Etsi enim "utique non rarum est in vivo animale inciso

" pulsum nullum apparere, neque videri arterias " dilatari, tamen frequentes satis observationes " sunt, utique dilatationem et oculis et tangenti " digito fuisse manifestam, ut arteriæ arcum " majorem describant dum dilatantur."* Again, " Hæc alterna arteriarum micatio est quam " medici pulsum vocarunt; quo nomine dila- " tatio quidem arteriæ potissimum intelligitur."† So also, in his Memoires sur le Mouvement du Sang; " D'abord je me suis assuré que le sang, " poussé par le cœur, dilate les artères, et forme " ce battement, qu' on appelle le pouls."‡

In the first of these quotations, he acknowledges, that, even in living animals examined by dissection, no pulse, or any dilatation of the artery, is frequently to be seen; but adds, that, in many other instances, which he particularizes in both the works which I have quoted, the dilatation is sufficiently perceptible both by the eye and finger. Afterwards,§ in referring to the parts in which the dilatation is most evident, he seems, with less than his usual accuracy, to place the visible dilatation of the artery out of the question, and only infers dilatation from the sensation " digiti tangentis." This distinction

* Lib. vi. sect. II. §. 9. † Ibid. §. 10. ‡ Page 33.

§ Physiologia, lib. vi. sect. II. §. 12.

is of great consequence; because, although it be admitted, that the pulsation of the artery can in various parts of the living body be felt, it is a *petitio principii* to infer that it is felt because the artery is dilated.

Long after that period, BICHÂT, sufficiently accustomed to experiments on living animals, finding by the clearest evidence of sight, that, in arteries denuded, there was no adequate dilatation during the systole of the left ventricle, abandoned, though with apparent hesitation, that opinion; and considered the pulse as chiefly owing to a locomotion, or change of place, in the whole artery; in consequence of which, it springs against the finger during the systole, and returns during the diastole. "Il me reste "à examiner comment le cœur produit le pouls "par ce mouvement brusque et instantané. "Or, nous avons encore sur ce point beaucoup "d'obscurité à éclaircir; mais on ne sauroit "disconvenir que la locomotion du systeme "arteriel ne soit pour beaucoup dans ce phéno- "mène. A l'instant ou la masse sanguine est "poussée ainsi du cœur vers les extrémités par "un mouvement de totalité" (the simultaneous movement of a continuous column) "elle tend "inévitablement à redresser les artères, surtout

" quand elles sont flexueuses. Ce redressement " y détermine nécessairement une locomotion, " laquelle produit le battement de l' artère. " Quant à la dilatation, elle est presque nulle " dans l' état ordinaire."* He adds, that if you rest a little on the artery, the blood makes an effort to dilate it; and this effect increases the sensation of pulse. JADELOT indeed, believed, that it wholly proceeded from the latter cause.

Whatever reasons JADELOT might have given for this opinion, it seems they were little satisfactory to BICHÂT, who afterwards thus concludes. " La dilatation et le reserrement " des artères étant peu de chose, et même pres- " que nuls dans l' état ordinaire, il parôit que la " cause spéciale du pouls est dans la locomo- " tion des artères; locomotion qui est générale " et instantanée pour tout leur systeme."†

In order to illustrate the term locomotion, let us suppose a piece of flexible cane, which we may move up and down in the direction of its length, may cause to advance or recede, may bend in various ways, or may even turn on its own axis. These are all examples of locomotion or change of place in the whole or certain

* Anatomie generale; tom. ii, p. 335, 336.

† Ibid. tom. ii. p. 338.

parts of the cane; while, at the same time, its diameter, or size, is in no respect changed. In reality, the meaning of this term is so clear, that I should have conceived it to require no explanation whatever, had I not actually found certain persons, who could not readily distinguish such movements from those of dilatation and contraction, or an alternate change of diameter in an artery.

The dependance of the pulse on the alternate contraction and dilatation of the artery is maintained by DUMAS. "La proprieté générale de "ces vaisseaux d'être alternativement contractés "et dilatés, constitue ce que les medecins ap-"pellent le pouls."* This dilatation and contraction he supposes, after GALEN, to be inherent in arteries themselves, and not to depend on the mechanical impulse of blood from the heart; so that arteries do not dilate, because blood is impelled into them; but the blood enters them, because, by their dilatation, they are disposed to receive it. "Pour se convaincre pleinement que "les artères se contractent et se dilatent, en "vertu d'une proprieté inhérente à leurs parois, "il n'y auroit qu' à rèfléchir sur le but, ou la fin "de cette double action. Car puisq' elles

* Principes de Physiologie; tom. iii. p. 314.

" doivent reçevoir et pousser le sang, elles se " disposent à la réception de ce fluide par le " mouvement qui les dilate et les ouvre. Elles " le chassent ensuite par celui qui les contracte " et les referme. Dès-lors elles agissent avant " que le sang entre dans leurs cavités ; elles s'y " prêtent d'elles-méme; et ce n' est point, comme " disait Galien, parceque le sang y pénètre qu' " elles sont dilatées, mais plûtot le sang y pénetre, " parcequ' elles se dilatent."*

He acknowledges, however, that there is great difficulty in proving this dilatation by experiment ; but asserts that there are some parts of the arterial system, in which it can easily be seen ; as in the arch of the aorta, and in the carotids, which he says evidently dilate themselves, when they receive the blood from the heart. " Il est assez difficile de constater la dilatation " par experience. Cependant il y a des portions " du systême artériel, ou les yeux peuvent " aisément la saisir, comme la crosse de l' aorte,

* Ibid. p. 305, 306. A curious specimen of physiological reasoning ! " Arteries dilate and contract by their own " powers, because, in order to receive and expel the blood, " they must dilate and contract by their own powers." By similar logic, we might demonstrate life in the leather pipe of a fire-engine.

" les carotides, par exemple, qui se dilatent " évidemment lorsqu' elles reçoivent le sang " du cœur."*

Conformably to the views of the greater number of preceding physiologists, RICHERAND also attributes the pulse chiefly to an increase of diameter in an artery, from the impulse of blood by the systole of the left ventricle. " Comme " les artères sont toujours pleines durant la vie, " et que le sang y coule avec d'autant moins de " rapidité, qu' elles sont plus éloignées du cœur, " la portion de ce fluide, que les contractions du " ventricule gauche poussent dans l' aorte, ren- " contrant les colonnes antécédentes, leur com- " munique l' impulsion qu' il a reçue ; mais, " retardé, dans sa marche directe, par la résis- " tance qu' elles lui opposent, il agit contre les " parois des vaisseaux, et les écarte de leur axe. " Cette action latérale, par laquelle les artères " sont dilatées, depend dont de ce que leurs " cavités sont toujours remplies par un fluide " qui résiste à celui que le cœur y projette. " Plus considérable dans les grandes artères que " dans celles d' un moindre calibre, cette dilata- " tion se manifeste par un battement connu sous " le nom de pouls."†

* Ibid. p. 314, 315. † Physiologie, tom. i. p. 319, 320.

The two last-mentioned authors speak also of the advance of the artery during the systole of the ventricle, as forming part of the phænomenon of the pulse.

PORTAL admits of the same coincidence. "On croit géneralement que le pouls est l' effet de "la dilatation de l' artère, dependant de l' influx "du sang en elle ; cependant quelques physiolo- "gistes ont cru que le mouvement de loco- "motion se faisoit aussi ressentir."*

SOEMMERRING contends for a similar explanation of the pulse. "In corpore vivo arteriæ "—alio tempore magis, alio minus turgent. "Ubi nimirum arteriæ sanguini, qui a vi cordis "undatim intropellitur, et illarum premit pari- "etes, cedunt, supra mediam diametrum ma- "nifesto in longum et transversum extenduntur : "hic proximo temporis momento vis cordis "remittitur, arteria tum in longitudinem, tum, "et apertius quidem, in transversum, partim vi "sua elastica, partim propria vi vitali contrahitur; "et infra mediam diametrum, protrudit, deinde "denuo relaxatur, et novæ sanguinis undæ locum "concedit. Præter has in arteriis magnis "tactui et quandoque visui obvias mutationes "in partibus quibus laxæ arteriæ adhærent, simul

* Cours d'Anatomie Medicale : tom. iii. p. 126.

" fit ut, præcipué si distensioni non facile cedunt, " loco moveantur. Id omne pulsum vocant, " ictum, pulsationem vel micantem arteriarum " motum."* Under the term pulse, therefore, SOEMMERRING includes the three co-operating circumstances, in an artery, of dilatation, elongation, and locomotion.

On reference to the physiological writers of our own country, we shall find an universal coincidence in the opinion of the dilatation and contraction of arteries by the systole and diastole of the left ventricle.

Thus HALES; " Now this velocity is only the " velocity of the blood at its first entering into " the aorta, in the time of the systole; in con- " sequence of which, the blood in the arteries, " being forcibly impelled forward, with an ac- " celerated impetus, thereby dilates the canal " of the arteries, which begin again to contract " at the instant the systole ceases."† Afterwards, reasoning on the same principle, and assuming that " the systole is nearly one third " of the time between pulse and pulse," he concludes, that " the other two thirds of that " time must be spent in the contraction of the

* De corporis humani fabrica; tom. v. p. 84, 85.

† Hæmastatics; p. 22, 23.

" arteries;" and, therefore, that " the sum of the " dilatation of all the arteries in each systole is " equal to about the quantity of two-thirds of " the blood which is thrown out in each systole."*

JOHN HUNTER, so original, and usually so accurate with regard to phænomena which fell under his own observation, is, on this subject, strangely wavering and inconsistent. Thus, in his work on inflammation; " Arteries during " their diastole, which arises from an increased " quantity of blood being thrown into them, " increase much more in length than width.— " It is, however, the increased diameter that " becomes sensible to the touch.—The dilata- " tion of the artery producing the stroke is " either felt by the finger, or may be seen when " superficial; but were we to judge of the real " increase of the artery by this, we should deceive " ourselves; for when covered by the integu- " ments, the apparent effect is much greater " than it really is in the artery itself; for in " laying such an artery bare, the nearer we " come to it, the less visible is its pulsation; and " when laid bare, its motion is hardly to be " either felt or seen. The more an artery is " covered, especially with solid bodies, the more

* Ibid. p. 23, 24.

" is the pulsation to be felt or seen."* Again, " This increase of the artery is so manifest, " as to be felt or seen; and produces what is " called the pulse."†

This alternate dilatation and contraction, the author considers as a cause of equalizing the circulation.

More recently, the very learned physician and profound mathematician, to whom I have before alluded,‡ assuming as a fact the dilatation of the arteries at each systole of the ventricle, has supposed an assignable proportion of that dilatation in arteries of different orders of diameter. It is, however, true, that this acute writer has separated the supposed dilatation from the tangible pulse of arteries, which he has shortly explained nearly in the just manner.§

Lastly, the writer of the article *Circulation* in REES's new Cyclopædia observes; " We admit " a diastole of the arteries, arising from the " lateral pressure of the blood forcibly projected " into these canals." Again, under the article *Pulse*; " The pulse is well-known to consist of " the successive dilatations and contractions of " the arteries, in consequence of the successive

* On the Blood, &c. p. 175. † Ibid. p. 176. ‡ P. 48.
§ Philosophic. Trans. 1809, p. 21, 22.

" impulses given to the blood through them by " the repeated contractions of the heart, and " by their own muscular and elastic powers."

As this work is a recent one, emanating from the metropolis, the opinion expressed in it, as to the derivation of what is usually called the Pulse, from the dilatation of the artery, is probably that which prevails in the southern parts of this island.

To the theory of HUNTER, above quoted, many *à priori* objections immediately occur. For, first, if the dilatation of an artery is not to be judged of by the sight or touch, it is difficult to understand in what mode it can be judged of at all. How, also, in an artery laid bare, a dilatation, which can scarcely be seen or felt by a part like the finger, should become both visible and tangible by the simple interposition of other substances, whether soft or hard, is utterly incomprehensible. Neither is it true, in fact; for no one ever felt a pulse in a living body at all the better, because a thick garment, or a steel gauntlet, intervened between the artery and the scrutinizing finger. On the contrary, the pulse of an artery, other things being equal, is better felt, and as this able physiologist himself acknowledges, better seen, when it is super-

ficial, than when it is deep. Thus, the pulse in the radial artery is better felt than that in the carotid; that in the carotid better than that in the popliteal; the last better than that in the femoral; and so on.* And while the pulse in the first is often visible, that in the femoral cannot be seen at all. Hence it follows, that it is not the mere intervention of any substance, that makes the pulse of the artery more tangible or visible; but some other circumstance, casually accompanying this intervention.

It seems, therefore, in various physiological views, well worthy of inquiry by due experiments on living animals, whether any such dilatation, as is usually supposed, does really exist.

Nor, is this a question of pure philosophical curiosity. It involves a point of great consequence in the practice of modern surgery; of which, the treatment of arterial maladies forms an equally extensive, difficult, and important part. Were we, for example, to suppose a case, in which a young surgeon, taught to expect that he should be able to distinguish, by this pretended dilatation, an artery, which he was re-

* It is doubtless for this reason, that, in old or thin persons, the pulse is, cœteris paribus, stronger, and apparently fuller, than in those who are younger or more fat.

quired to lay bare, should, during an actual operation, unfortunately find this test to be wholly wanting; or another case, in which, a more experienced operator, about to tie a deep-seated artery, should be unable to discover that artery by the touch, and, having made his ligature, should be ignorant whether it was rightly applied, till he was assured of his success by the event; if these cases are possible, it is surely proper, that the cause of such failures should be fully ascertained, and some principles established, by which their future occurrence may, with moral certainty, be prevented.

In the first experiments made in the year 1811,* on the arteries of living sheep and rabbits, for the purpose of observing the functions of the sympathetic and vagi nerves, and the effects of ligatures, Mr. GEORGE NORMAN and I were astonished to observe, that, contrary to our preconceptions, the carotids, when exposed to view, were perfectly free from all dilatation during the systole of the left ventricle of the heart. In reality, they appeared precisely like solid dead cylinders; except that they had a longitudinal locomotion backwards and forwards, exactly corresponding with the respira-

* These experiments are not detailed in this work.

tions; so that they were drawn backwards towards the thorax at each inspiration, and returned forwards to their former place at each exspiration. This coincidence was farther proved, by counting with a watch indicating seconds; which, in one example, shewed the respirations, and, consequently, the pairs of these longitudinal movements, to amount to 84 in a minute; while, during the same space of time, the pulsations in the artery were 108.* When, also, the animal for a few seconds ceased to breathe, the longitudinal motion entirely ceased, but returned immediately with the breathing. During these short exemptions from respiration, though the pulse was going on as before, there was no dilatation, or other movement, in the artery. Precisely the same quiescence occurred, when the carotid was drawn laterally out of its place.

In a sheep, which is a very timid animal, the agitation of respiration during these operations, though much varying in different individuals, is usually extremely great. The diaphragm, which is the chief organ of that function in these quadrupeds, is moved up and down very forcibly and quickly. We can easily un-

* Experiments I. II. III. and others.

derstand, how, at each descent of this muscle in inspiration, the mediastinum, pericardium, heart, arch of the aorta, and the single trunk, which may be called arteria innominata, from which spring both vertebrals, both axillaries, and both carotids, should be drawn down, and return to their places, at each exspiration. In reality, in some of the experiments, the force of this return was so great, that when a long portion of the carotid was detached from all the surrounding parts, it bent on itself, so as to form a considerable curve outwards, towards the back of the neck. The ascent and return were, in all cases, accompanied with a corresponding movement of the vagus nerve, and other parts connected with the artery. In the femoral artery, no regular movement of this sort was produced; but the shaking of the whole body was so great, that no distinct motion of any kind could be discerned, except during the intervals of respiration, which sometimes took place.

In this respect, therefore, a sheep, though, from the magnitude of its vessels, it offers many advantages for such experiments, is less proper for them than the dog, and more especially the horse, whose respiration is less affected by the same cause.

With regard to the quiescence of arteries, except from the process of respiration, it was natural to suppose, that what, after careful scrutiny, had been uniformly found to exist, should have been a constant, and not a casual, occurrence. Although, however, the fact was, under the existing circumstances, precisely such as has been represented, farther experiments shewed that there was also another motion, corresponding with the systole of the left ventricle, which was wanting, when the artery was strained by the stretching out of the neck and head, but was sufficiently apparent, under certain circumstances, when both were loosely placed.

This motion was first observed in the carotid of a dog, in which, from an apparent apathy, no disturbance of respiration or circulation took place. The neck was in a relaxed position, and no motion, dependent on respiration, was perceivable, while that from the systole of the ventricle was very manifest.*

In a second experiment on a dog, this movement was also plainly seen; and it was suggested by Mr. TUDOR, that it depended on the position of the head, which was found to be the case; although in this, as in former experiments,

* Experiment VI.

when the jaws of the animal were stretched out in a line with the neck, the movement from respiration was alone visible.*

This rule generally obtained in numerous subsequent experiments, as by reference to them may be seen.

The mode of this movement was chiefly longitudinal; so that the whole visible portion of the artery advanced from the heart, during the systole of the left ventricle, and returned during the diastole. Sometimes, though very rarely, the whole artery moved in a lateral direction, or forwards to the eye, and backwards from it; and, still more rarely, seemed to turn, though in an almost imperceptible degree, on its own axis. It even sometimes happened, that when the neck was much bent, the carotid, if separated for some length from its surrounding attachments, formed itself into an arch, during the systole of the ventricle, in the manner already described as having taken place from exspiration. This was the result of observations made on a great number of arteries in sheep, horses, dogs, and rabbits, in the strongest light, and aided by powerful lenses.

* Experiment VIII.

Even the longitudinal movement itself, however, although so common, did not always occur; for in a horse, notwithstanding the neck was in an easy posture, and the parts in no respect embarrassed, not the smallest movement of any kind could be perceived in the carotid. Yet, at the same time, Mr. SEWELL, and others, felt the pulse strongly beating in that artery.*

So also, in two rabbits, the abdominal aorta, denuded of its peritonæal coat, was examined; but in neither could we perceive, with a strong lens, any movement from respiration, or the systole of the ventricle.† The same quiescence occurred in the beginning of an observation on the thoracic aorta of a rabbit.‡

In all the above experiments, and others, in the year 1812, in which is comprised the most careful examination of aortæ, carotids, and femorals, amounting in all to fifty-five arteries, we could not, by any mode of scrutiny, detect, in any single instance, the smallest dilatation or contraction from the systole and diastole of the left ventricle of the heart.

This is the more extraordinary, as the far greater number of the observations were made

* Experiments XX. † Exp. XI. XII. ‡ Exp. XXV.

on the carotids, which DUMAS adduces as especial exemplifications of the dilatation of arteries from the systole of the heart. How far such a dilatation may obtain in the arch of the aorta in warm-blooded animals, I am unable to say; as death immediately ensued in the only experiment of this kind, which I have seen attempted.

HALLER gives us some examples, in which he saw certain arteries actually dilated during the systole of the left ventricle, and contracted during its diastole. SPALLANZANI asserts the same of the aorta of salamanders, and land and water frogs, ("raines vertes.") But the last cases are by no means in point; since, from his observations, it appears, that, in these animals, the whole aorta has a fleshy and muscular coat; and is possessed of an independent power of alternate contraction and dilatation,* though separated from the heart, and taken out of the body. In them, therefore, this part may be considered as an accessary portion of the heart itself; and the stimulus of the blood, and other substances acting on it, may excite in it a similar contraction, from which, like the heart, it returns to a state of dilatation by its elasticity, in a mode

* Experiences sur la Circulation du Sang. Diss. III. Exp. 146, 147, 148, 149, 150, 151, 152, 157.

exactly contrary to that affection of arteries, which is supposed to produce the pulse.

Although, however, in some instances, the force of the heart's action may be sufficient to cause alternate dilatation and contraction in certain arteries, such a vibration cannot be necessary to the production of the pulse, since that phænomenon can exist, in the fullest degree, without it.

In microscopical experiments made by HALLER and SPALLANZANI, those authors tell us, that they were unable to discover any dilatation from the systole of the heart, in arteries of a very small size, such, for example, as of the 140th part of an inch in diameter. These results have, however, been judged inconclusive, because the extreme minuteness of those arteries would not admit of any measurement of an increase, which was presumed not to exceed the 160th part of their diameter.

This objection, however, will not hold good in the case of the Sheep, in which the living carotid has been observed to be $\frac{379}{400}$ of an inch in circumference, or almost one third of an inch in diameter;* and is still less valid with regard to the Horse, whose carotid has been

* Experiment XV.

found to reach $\frac{636}{400}$ of an inch in circumference, or above half an inch in diameter.* In these instances, in which the examination was aided by a lens of a considerable magnifying power, if any dilatation of the artery by the systole of the ventricle, sufficient to produce the pulse, had existed, it could not have escaped observation. Since, however, in these and all other cases, the artery, when properly felt, gave to the touch a pulse far more strong and distinct than any which can be perceived by the finger, in an artery undivested of the surrounding parts; we are unavoidably compelled to refer that pulse to some other cause, than an excess of the diameter of the artery, during the systole of the ventricle, above its diameter during the diastole.

In the course of our experiments, in which a carotid, separated from all its attachments, was fully exposed to view, and yet no movement was observable in it, we were sometimes equally surprised to find, that, when the finger was pressed against it, or even when it was removed out of its place by the force of the finger beneath it, or on one side, it was equally void of all pulse to the touch.

* Experiment XXI.

A frequent repetition of the same results led to a discovery of what appears to be the nature of the pulse itself.

The blood, in every part of the arterial system, from the mitral valves, onwards through the whole frame, to the right auricle, may be considered as a set of continuous columns, possessing little compressibility, and filling the tubes in which they are contained.

When, by the contraction of the left ventricle, the blood included in it is forcibly expelled into the aorta, all these columns receive the shock of propulsion at the same instant. But the velocity, during this systole, being greater than during the diastole, the momentum, and consequently the impulse, in every direction, is also greatest in the systole. When, therefore, an artery is compressed with the fingers, in the usual mode of feeling the pulse, the blood, in consequence of the systole, rushing into the artery with an increase of momentum, gives a stronger impulse of dilatation to the fingers, than from the less momentum which exists during the diastole, and thus produces the phænomenon of the pulse.

Hence, it appears, that the pulse is the effect, not of an extension of an artery beyond its usual diameter, but of a stronger effort, during the

systole of the ventricle than during its diastole, to restore the usual diameter of the artery, which had been diminished by compression.

Since, also, the excess of velocity from the systole extends throughout the whole of the space compressed by the fingers, it is evident, that the distending effort, producing the sense of pulsation, must also be felt throughout that space.

Before this explanation could be fully admitted, it was necessary to ascertain the cause of the converse of this state; or the reason why a pulse was sometimes wanting in an artery exposed to the view, and susceptible of any mode of examination by the touch. Reiterated trials demonstrated, that this deficiency was owing to the followng circumstances. The coats of the carotids are so firm, that when either impelled against any soft substance, or simply moved out of their place, these arteries readily recede; suffering no reduction of diameter, and, therefore, giving no sensation of a pulse. But if they are confined by any hard substance placed behind them, so as to resist a change of position from the pressure of the finger, or if they are squeezed between the finger and thumb,

so as, in either case, to suffer a certain reduction of diameter, then the pulse never fails to exhibit itself.

These latter experiments, therefore, while they shew that the pulse does not exist under the mere contact of the finger with the artery, and, therefore, completely refute the supposed dilatation of an artery by the systole of the ventricle, as an object of touch; by ascertaining the precise circumstances under which a pulse does or does not exist, demonstrate, beyond all reasonable objection, the nature of that phænomenon.

Hence we see the probable reason, why no pulse is said to have been discovered in those surgical operations, to which I have before alluded. If, however, the principle as to both states be well established, we have only sufficiently to diminish with the finger the diameter of an artery, and then, if it be of a certain size, and the course of the blood through it, from the heart, be uninterrupted, we shall not fail to feel the pulse in that artery.

These points may be illustrated by the annexed figures. Plate I.

I. Situation in which no Pulse is felt.

FIG. I.

A. B. C. D. A tube of the same size as the carotid, in a large living horse.

a. b. Diameter of the tube during the systole and diastole of the left ventricle.

c. d. e. f. The same artery moved laterally out of its place, as expressed by the dotted curved lines.

g. h. The diameter of the artery, so displaced, remaining exactly equal to that of the same artery at *a. b.*

In this case, the diameter of the artery not being diminished by the pressure, no pulse is felt in it.

II. Situation in which the Pulse is felt.

FIG. II.

A. B. C. D. A cylindrical artery, as in the former figure.

a. b. Its natural diameter during the systole and diastole of the left ventricle.

c. d. e. A given space, through which the artery is compressed by the fingers, when the pulse is felt in the usual way: expressed by the dotted line, in which are shewn the various degrees of diminution of diameter in the vessel,

Fig: 1.

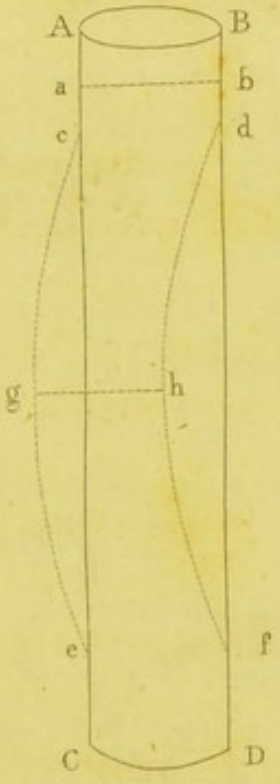
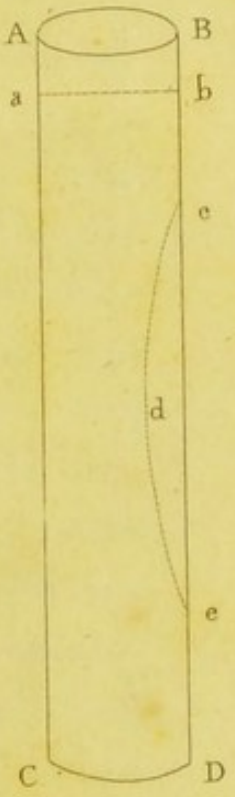


Fig: 2.



by the approach of that line, *c. d. e.* to the opposite and fixed side of the artery A. C.

In this case, in consequence of the increased velocity of the blood during each systole of the ventricle, the pulse is felt throughout the whole extent of the compressed and contracted part from *c.* to *e.*

From the facts which have been stated, we see the weakness of those arguments, by which it is inferred, that, because arteries have, under certain circumstances, a power of dilatation and contraction, therefore, that dilatation and contraction must be produced by the usual systole and diastole of the heart. The inference, in this case, is just as legitimate, as if we were to conclude, that, because a scale may be made to preponderate with the weight of a pound, *ergo* it must turn with that of a scruple. This error, however, is not uncommon with those, who, deserting the plain and direct road of fact, satisfy themselves with *à priori* reasonings. In reality, arteries during health are always in a forced state of dilatation, which the usual impulse of blood from the left ventricle is incapable of augmenting.

From the above statement, we can also understand, why the phænomenon of the pulse should be confined to vessels of a certain diameter, or at

a certain distance from the heart. In the first case, the vessel, if very small,* does not, without obliteration of its cavity, admit of that diminution of diameter, which is necessary for the production of the effect. In the second case, the difference of velocity of the blood in a distant vessel, during the systole and diastole of the heart, is so far lost, as not to give the sensation of a pulse to the finger, although, under certain circumstances, it may be sensible to the eye. Thus, in various of the microscopical observations of HALLER and SPALLANZANI, the predominance of the velocity of blood from each systole of the ventricle was very visible; and in a much more palpable instance, I have often seen the blood flow from the cephalic vein of a human being, with jets almost as from an artery; and yet, notwithstanding the turgid state of that vein, not the smallest pulse could, on the nicest examination, be discovered in it.

These facts respecting the pulse point out one cause of that phænomenon, which was the stumbling block in the way of Mr. HUNTER, and still continues to be so in that of the majority

* Haller asserts, that no pulse can be felt in an artery, which is less than one sixth of a line in diameter.

of modern physiologists. If an artery has, from the systole of the heart, no visible pulsation, when denuded; why has it so often a visible pulsation, when surrounded with its natural coverings?

In order to discover this cause, we have only to make one simple experiment. Lay bare an artery, and no dilatation is perceivable. Place behind it a hard substance, so as to prevent its recession. Press the finger against it exactly opposite to the resisting substance; and you will immediately perceive the finger propelled outwards at each systole of the ventricle, and retreat at each diastole. The experiment may be repeated in various ways, and will always, under proper circumstances, have the same result. The whole which is necessary is, that the artery should have its diameter in a certain degree reduced; and then the substance reducing it, if not too ponderous, will be driven off in leaps, which must accord with the systoles of the heart, and are often perceptible to the eye.

In this way, the pulsation of arteries may be frequently seen, when they are unusually compressed by muscles, or other intervening substances. Hence fæces in the colon, or other indurations in the abdomen, near, or in close

contact with, the aorta, by compressing that vessel, often, both to the eye and touch, simulate aneurysms; and hence aneurysms themselves, forming, for the blood, channels, which are preternaturally compressed in various directions, suffer jerks from the impulse of the ventricle, which often shake all the neighbouring parts.

It is not, then, the mere intervention between the eye or finger, and an artery, of any substances, whether hard or soft, that, conformably to the opinion of Mr. HUNTER, makes its pulsation more sensible; but it is because the substances, thus intervening, often interrupt the current of blood through the artery, and therefore receive a shock, which may be tangible or visible, from each systole of the ventricle.

It has been suggested, that the tangible pulse of an artery may arise from the impulse produced by the approximation of the more internal coats to the external, during the systole of the heart. The microscopical observations of HALLER shew, that, in the smaller arteries, there is in the same vessel, at different times, a very considerable difference between the relative thickness of the coats, to that of the light or diameter of the area. Thus, in one of his examinations of the mesenteric artery of a frog,

at the commencement of the observation, the coats were twice the diameter of the area, but at the conclusion, the thickness of the coats was only a fourth part of that diameter. This difference depended entirely on the force of circulation in the different cases.* Other instances of this variation are recorded by the same author; but in neither of them was there, at the same time, any change in the diameter of the whole artery. Since, however, he is wholly silent as to any thinning of these coats during the systole of the ventricle, and thickening during the diastole, we have a just right to conclude, that no visible change of that kind existed, and therefore, that the tangible pulse is in no degree owing to this cause.

A recent author having assumed that, "the "volume of blood is greater in a given space of "an artery at one time than at another," concludes that the calibre of the artery must be proportionably great.† Both these positions will readily be granted; but I am at a loss to see how they prove the dilatation of an artery by the usual systole of the heart; unless it could be shewn, that the quantity of blood in

* Memoires sur le Mouvement du Sang, p. 228.

† PRING's View of the Relations of the Nervous System, p. 67.

the artery is greater during the systole than during the diastole. This, however, I by no means admit. The velocity of the blood is greater, but not the quantity.

Various other *à priori* arguments are employed in favour of this opinion of alternate dilatation and contraction of arteries. Not satisfied with what really is, authors boldly decide what ought to be, the process, by which the circulation is, on all occasions, carried on through this system. They have observed, that, among the different genera in the scale of animated being, there is a sort of balance, by which an apparent defect of power in one part is supplied by some equivalent accession in another. Some animals, having no hearts, owe the circulation of their blood to vascular action alone. Others, having imperfect hearts, obtain assistance from other parts of the vascular system. Hence, they have sagely concluded, that, in all more perfect animals, the heart, unassisted by that system, is incapable of maintaining the healthy circulation. Strange analogy! Ought we not, on the contrary, from that very gradation itself, to infer, that, in animals with hearts so elaborate and perfect as in the mammalia, the assistance of the vascular system is unnecessary, in direct

proportion to that perfection? In this view, therefore, the conclusion drawn from the circulation and growth of akerious fœtuses, is a mere *argumentum ad ignorantiam*, which furnishes no proof of the mode of circulation in full grown and perfect animals, after exclusion from the womb.

Those, who maintain that the pulse is owing to the alternate dilatation and contraction of arteries, from the systole and diastole of the left ventricle, have probably deduced their conclusion from the analogy of the action of the heart itself. In this organ, they presume that each cavity is dilated in consequence of the distending power of the blood impelled into it, and then contracts itself in consequence of that distention. To me, however, it appears, that this theory, however specious, is absolutely erroneous. The different compartments of the heart are so far from expanding, in consequence of the blood which is driven into them, that, when altogether empty of blood, and even separated from the animal, they expand in a greater degree than when in their natural situation during life and healthy circulation. Hence, Mr. HUNTER himself, notwithstanding the view which he gives of the action of the heart, acknowledges

that this organ, after death, has a larger volume than while the animal is living.* The true nature of the heart's action, if urged as an analogical proof of that of the arterial system, would therefore lead to conclusions, respecting the movements of that system, very different from those which are deduced by the writers who advance it.

To the opinion of the want of visible alternate dilatation and contraction of arteries, as a cause of the pulse, it has been objected, that this may arise from faintness, or certain other circumstances of disorder in the animal, which has undergone the operation of having them laid bare by the knife.

To this objection, it might, perhaps, be sufficient to reply, that it is incumbent on those who adduce it, to prove the operation of such a cause. But, waving that argument, I presume it will be admitted, that this supposed weakness must either affect the heart, and therefore the circulation in general, or else that particular artery, which is the subject of investigation.

* In his experiment with the bellows, the heart, when not acting, became twice as large as when acting. On the Blood, &c. p. 147.

With regard to the first ground of objection, so far is the action of the heart in a sheep or horse from being impaired by exposing the carotid, that it seems to be actually increased, somewhat in frequency, and, apparently, a good deal in force. This is peculiarly the case during any struggle. At the same time, in all the above experiments, the pulse in all other tangible arteries, which lay undisturbed amidst their natural integuments, was at least as strong as before ; and on one occasion, a very minute artery, arising from the exposed carotid of a horse, having been wounded, a thread of blood sprang out with such force, as to fall on the ground at the distance of eight feet from the punctured part. Yet, under no circumstances, were we able to discover the least dilatation or contraction of an artery from the systole and diastole of the ventricle.

So much with regard to the state of the heart ; and as to that of the vessel itself, the objection is altogether nugatory, unless it can be shewn, that an exposed artery is always less filled by the systole than is natural, and that, consequently, the pulse is either wanting, or, at least, diminished in force. On the contrary, in all the experiments above related, in which

the vessel retained its full size, the pulse, when properly felt, was so far from exhibiting any weakness, that it possessed a degree of strength, of which no one, who had not examined a denuded artery, could have had any conception.

DUMAS accuses HALLER of inconsistency, in denying that the circulation of the blood depends on the action of the arteries, since his own experiments prove the power of various stimuli, applied to those vessels, to renew the circulation in them after it had become languid, or was about to cease. "Il a vu qu'on "pouvoit ranimer et retablir son mouvement "lorsqu' il faiblissait, ou qu' il alloit s'éteindre, en "portant sur les vaisseaux divers moyens d'ir-"ritation."* This statement is a mere fiction. HALLER expressly denies the power of any chemical or mechanical stimulus to excite vital contraction in an artery; and after the most careful perusal of his work, "Sur le Mouve-"ment du Sang," to which DUMAS alludes, I can find no single example of the restoration of the circulation of the blood by any operation on the arteries, except that of blood-letting. The same is true with regard to the experiments

* Physiologie, tom. iii. p. 305.

of SPALLANZANI, whom DUMAS includes in his reference.

The rationale of this effect of blood-letting, I have already attempted to shew; and I should have been far from thinking it necessary to advert to it on the subject of the pulse, had not DUMAS fancied that it illustrated his theory of that phænomenon.

On the whole, I trust that it has been proved, by sufficient force of evidence,

First, that in the larger arteries of certain animals, there is no sensible dilatation or contraction from the systole and diastole of the left ventricle of the heart; and, therefore, that the pulse cannot depend on that alternation: and,

Secondly, that the chief cause of the pulse is a strong and predominant impulse of distention from the systole of the left ventricle given by the blood, as it passes through any portion of an artery forcibly contracted within its natural diameter.

I have already spoken of the longitudinal movement, which, in the foregoing experiments, was usually, but not always, observed in arteries, from the systole of the ventricle; and I was desirous of examining, in a large animal, whether this motion was an elongation of the vessel,

or the mere propulsion of the whole artery forwards from the heart. When, however, even in the carotid of a horse, the circulation was unimpeded by a ligature, the whole movement, when visible, was so short, even in an extent of several inches, as to render abortive any mode which I could devise for its mensuration.

A firm ligature round the vessel, probably by resisting the free current of blood, greatly increased the longitudinal advance of the vessel; and, in an experiment on the carotid of a horse, I was able to ascertain that this advance, at each systole of the ventricle, was about $\frac{34}{400}$ of an inch.*

This longitudinal movement was also increased in a ewe by great loss of blood,† and by the approach towards death in the aorta of a rabbit.‡ In this latter case, it remained, after the cessation of breathing, as long as the left ventricle continued to act.

I may add, that it was also, in several animals, visibly augmented by certain irritations of the sympathetic nerve; although the detail of these experiments has been omitted, as not immediately connected with the design of this work.§

* Experiment XXI. † Exp. XXIV. ‡ Exp. XXV.

§ There seems to be some connection between this longitudinal projection of arteries, arising from hæmorrhage, and

Still, however, none of these causes of the increase of longitudinal motion produced the smallest dilatation or contraction of any of the affected arteries, from the systole and diastole of the ventricle.

With regard to the longitudinal advance, it is evidently not the result of any vital action in the artery itself, but the mere mechanical effect of the peculiar mode of systole in the left ventricle of the heart.

In the course of our experiments, I attempted to investigate the causes of error in GALEN, and some recent physiologists, who ascribe the pulse to an inherent power of dilatation in the arteries, independent on any mechanical influence of the blood on their coats. In support of this principle, they adduce certain experiments to shew that a pulse will exist in the artery of a living animal, even where the blood does not reach; and, conversely, that, under

other debilitating powers, and the fact mentioned by HALLER; who relates, that, on various occasions, when, in minute arteries, the predominance of the velocity of the blood in the systole of the ventricle above that in the diastole could not be perceived during a vigorous state of the animal, it became very conspicuous, when, from the length of the experiment, he was greatly exhausted.—Memoires sur le Mouvement du Sang.

certain circumstances, solely affecting the coats of arteries, it will not exist where the blood reaches with full force.

The truth of the first position we tried in various ways, by means of ligatures, and more especially by three ligatures, distant from each other, and completely obstructing the circulation. On compressing the artery with the finger and thumb beyond the first, second, or third ligature, we could usually, but not always, perceive a very faint movement, somewhat resembling that from the impulse of the blood through the vessel, by the systole of the ventricle. This sense of beating was, however, greater, the nearer we approached the first ligature; and, when we were at a certain distance from it, was almost wholly perceived on the side of the finger and thumb nearest the heart, without being equally continued through the whole compressed space, as happens in the case of the real pulse. A feeling of impulse, similar to that last described, occurred between the second and third ligatures, after the space included between the two first had been emptied of its blood,* and even beyond the third, after the blood had been

* Experiment XIX.

suffered to escape, by puncture, from both the spaces included between the three ligatures.*

On the whole, there appeared to us no reason to doubt, that, except in the case in which the finger and thumb, placed immediately beyond, and almost in contact with, the first ligature, felt the direct impulse, from the heart, of the column of blood through the tied part, all the other sensations of pulse were merely faint perceptions of the longitudinal movement of the artery, from the systole of the ventricle already described, more or less continued along the coats of the vessel through the ligatures, and, of course, just as strong when the blood between the two first, or the second and third, ligatures, had been evacuated, as when it remained in the vessel.

In order to prove, that there exists in the coats of arteries, a power of dilatation and contraction, independent of the blood which they contain, GALEN relates his having made the following experiment. In the artery of a living animal, he made a slit, into which he introduced a tube, so as to fill the vessel, and prevent the evacuation of the blood through the slit. A pulse was now perceptible to the eye, in the

* Experiment XX.

vessel beyond the tube. But when a ligature was firmly made round the vessel upon the tube, the pulse beyond the tube was no longer to be seen.*

This experiment we tried to repeat; but without success. It failed, in the manner related in Experiment XXVII.

I am, therefore, altogether unable to decide as to the truth of GALEN's relation, or as to the cause of the circumstances, if they exist as he states them.

It is, however, difficult to see how these results, if admitted, go any way towards establishing the inference which he draws from them; since, if the pulse depended simply on the spontaneous dilatation of an artery, there seems to be no reason why an attachment of its coats to an included tube in one spot should prevent this spontaneous dilatation of the same artery in another spot, at some distance beyond the former.

Many of the authors, whom I have quoted, speak of the locomotion of arteries from the systole of the heart. BICHÂT especially characterizes this affection by the term *redressement*;

* GALENI, An in Arteriis Natura Sanguis contineatur, liber. Edit. Charterii, tom. iii. p. 163.

a term, which, in the connection in which he uses it, I cannot understand, unless it means a disposition to straitening of vessels previously tortuous, or interrupted by angles. Such a tendency certainly takes place in the animal frame in one remarkable instance; which is that, in which a large column of blood, driven into the aorta at each systole of the ventricle, tends to bring those parts towards a strait line, and thus throws the apex of the heart upwards, against the ribs.

Conformably to this principle, we find, that if a leather tube, already stretched by a contained fluid, is placed in a curved direction, and a new quantity of fluid is forcibly impelled into one end, which is fixed, while the other parts of the tube remain free, it tends to assume a rectilinear form.

It is not, however, easy to apply this principle to vessels in the living body, which are chiefly attached to surrounding parts. I have tried to examine what really occurs in tortuous vessels, laid bare, in living animals; but have hitherto been unable to discover any, which would be fit subjects for this species of observation. The spermatic artery in Rams is sufficiently tortuous; but is so involved in vascular substance,

that, in one attempt of this kind, our examination was defeated by the great hæmorrhage, which followed the dissection down to the artery.

In the experiments above related, it appears, that, exclusively of the longitudinal motion from the systole of the heart, there was occasionally, from the same cause, a slight lateral and even rotatory motion. When, however, the carotid, by bending the neck, was relaxed, and assumed somewhat of a curvilinear direction, so far was it from being straitened by the impulse of the blood, that the curvature was sensibly increased.

We are told by HALLER, that, in arteries which formed a bend, he often saw the systole of the ventricle cause the portion nearest the heart to approach to that which was farthest, so as to render the angle more acute.* In a patient, who had suffered hemiplegia, and in whom the right temporal artery was very prominent and tortuous, no perceptible movement existed in the artery, until, imitating the effect of a ligature, I with my thumb compressed a certain part of the vessel; in consequence of which, the part before the pressure suffered a visible motion, which, on close examination,

* Memoires sur le Mouvement du Sang, p. 34.

appeared to be an approach of the first curved line, containing the angle, to the next beyond it, as in the case above described by HALLER.

That this movement was mere locomotion, and not dilatation and contraction, is, I think, confirmed by its having occurred only when the artery was obstructed; since it has been shewn, with regard to denuded arteries, that, when obstructed by ligatures, they suffer an increase, and sometimes an entire production, of locomotion, but never a dilatation and contraction.

It is highly probable, that the pulse, which is sometimes visible in the arteries of living animals, when it does not arise from the effort of the blood to remove a compressing substance, is owing to this locomotion of the vessel; which, if the artery is considerably relaxed, will take place in that direction, consistently with the longitudinal tendency, in which it meets with least resistance. Thus, in my own left wrist, if I straiten the radial artery, by forcing the hand strongly backwards, I can perceive no movement of the vessel. But if I bend the wrist, so as to relax the artery, I then, at each systole of the heart, perceive in it a motion, which is partly forwards towards the eye, and partly diagonal between the longitudinal axis of

the vessel and the ulna. Many other instances of visible pulsations of arteries, exactly corresponding with the systoles of the heart, and probably produced by a similar locomotion, are observable in human beings, as in the lower part of the carotids of nervous women, &c.

If the locomotion of an artery, concurring with the common cause of the pulse, can in any degree contribute to the occasional modification of that phænomenon; we may conceive, that, under certain circumstances, it may, of itself, exhibit, both to the eye and finger, somewhat of a similar affection. I have already exemplified one modification of this effect, in arteries obstructed by ligatures.

To this head may, I think, be referred a case, which I have often witnessed, and which is that of a double stroke in the radial artery. From among the different examples of this kind may be selected the following, which not long ago occurred. In a thin youth, aged 17, recovering from a slight typhus fever, the systoles of the ventricles, and the pulse of both carotids and of the left radial artery, corresponded, and were exactly 90 in a minute, while the pulse of the right radial was precisely 180; the alternate strokes differing in strength. The true stroke

was sharp and hard; and the secondary one was perceptible, only when a very faint pressure was made on the artery by the finger, but immediately and entirely vanished, either when the pressure was, in a certain degree, increased, or when the artery was confined in one place, below the part felt, by being driven sideways towards the outer edge of the radius. Twenty days after, this second stroke had ceased; the pulse being 96 in a minute, and softer and fuller than before. The young man was, himself, well acquainted with this peculiarity of the pulse in his right wrist; which, he said, he could always produce by running, or other violent exercise. Accordingly, several days having elapsed, when I felt it after he had used a good deal of exertion, it was 108 in a minute; and the second stroke could again be perceived, though not so distinctly as before.

In this case, it seems probable, that, during the systole, the impulse of blood produced some locomotion in the artery, which returned to its place during the diastole of the ventricle; giving to the finger a secondary impulse, similar to a weak pulse. On this principle, it is easy to see, how the prior change of place might be occasioned by the strong or rapid impulse of the

column of blood from fever, or violent exercise; while no such change would occur during the more undulating impulse produced by the slower and gentler contraction of the systole during rest and health.

If this explanation of the phænomenon of the double pulse be just, it will suggest the mode, in which, it is probable, that the elasticity of arteries may have led certain physiologists to attribute slight occasional anomalies in the pulse to an independent action of the vessel. Since, however, there are various other occasions, on which there is a difference, in point of frequency, between the systole of the heart, and the pulse in an artery; and also between one or both of these, and the pulse in another artery; it is necessary to enter on a more extensive investigation of these varieties.

I may here begin with remarking, that, in making such observations respecting the difference of pulses, during the same time, a little more of caution seems necessary, than appears to have been sometimes employed.

In the first place, the comparison must always be made between arteries, the beats of which are sufficiently strong to admit of being distinctly felt; for, agreeably to a former observation, the

systole of the ventricle does not always give the power of producing a pulse to every artery within its influence. So, also, an examination of the pulse in two arteries in succession will by no means answer the purpose. If the tact, and power of attention, of the examiner, be not nice, these arteries should be felt, at the same time, by two accurate observers; of whom one should count aloud, while the other silently observes whether the beats in the second artery correspond with those in the other. On most occasions, however, a nice observer will, without the embarrassment of arithmetical reckoning, easily, at the same time, compare two pulses, if they are distinct; and if any confidence is to be placed in frequently repeated observation, I may venture to assert, that if the author, who has drawn his example of a difference of pulsation from a branch of the pudic artery, comparatively with the radial, had only felt that branch with his left hand, while he felt the radial artery in his left wrist with his right hand, he would have found the pulsations in these two arteries exactly synchronous.*

* There may be an exception to this rule with regard to the pudic artery, under certain circumstances, in which the action of the accelerator muscle, whether automatic or vo-

Those who would give the arteries a pulsation independent of the heart, must found their arguments on those cases only, in which that of the former exceeds in number the systoles of the latter; because various circumstances, some of which have already been particularized, and others of which are easily assignable, may cause the number of pulsations in arteries, to fall short of that of the sensible strokes of the heart.

Were we, however, unable to point out all these causes, that would be a curious kind of argument, by which we should infer, that, because a pulse was sometimes wanting in an artery, therefore the pulse, when existing, was produced by the artery. Just as legitimately might we conclude, that the tube of a syringe

luntary, gives a pulse to that artery, independently of the systole of the heart. This, however, is so far from proving an action of the artery itself, that it actually confirms the principle of the passiveness of arteries in common circulation; since the pulse, in this case, is produced by the mechanical effect of a sudden and rapid propulsion of the blood in the artery, by the action of a power external to the vessel, just as in common circulation, it arises from the mechanical contraction of the left ventricle of the heart. In a nearly similar way, a pulse may any where be produced in a long subcutaneous vein, by a strong stroke with the finger, when the vein is compressed between the struck part and the heart.

propelled the fluid within it, because, though the piston continued to act, the fluid would no longer pass. In both cases, a certain condition of the conduit may be necessary, although in both the conduit is equally passive.

Let us, however, inquire into the particular instances of this defect. In the first place, from causes which it is, perhaps, impossible to assign, some arteries are more readily filled than others. Thus, the carotids lose their pulse last; and, on the return of suspended life, recover it first. Twenty instances have occurred to me, of the total loss of pulse in the radial arteries, from various maladies of the alimentary canal, whether a considerable time before death, or under circumstances that admitted of recovery; while, at the same time, the pulse in the carotids was full and strong. In one instance of general dropsy, the patient had no pulse in either wrist for seventeen days; yet was restored to perfect health. Sometimes, before death, the pulse has been wanting in one radial only.

I have seen a total loss of pulse in one arm, with coldness, but complete power of motion in that part; while the other arm was warm, and possessed a perfectly good pulse, but had lost all power of voluntary motion. These symp-

toms commenced suddenly, two or three days after parturition. The patient soon died, but a dissection was not obtained.

In another case, a young man, labouring under pulmonary hectic, was found to have lost the pulse in one wrist, immediately after coming out of a warm bath. Several months afterwards, it had returned, though in an almost imperceptible degree.

Another patient, a female of middle age, the mother of several children, affected with severe cough, was apparently in a state of convalescence, and walking about her house; when it was discovered that the pulse in one arm was wholly wanting. A few days afterwards she died suddenly. The whole course of the artery, to the aorta, was carefully examined; but no deviation from the healthy state could be perceived in it.

If, conformably to various of the experiments above detailed, the approach to death produces a great degree of contraction in arteries; and if, during life itself, certain parts of arteries are capable of contracting themselves, even to a degree equal to that produced by death; we may reasonably conclude, that such a contraction may have taken place in the examples just

mentioned, so as to prevent that momentum of blood in the several arteries affected, which should give the sensation of pulse beyond the stricture. This defect would, however, still be compatible with a considerable flow of blood through the affected arteries.

In one of our experiments, the carotid of a Ewe, after having been some time laid bare, was found to have spontaneously shrunk in circumference from $\frac{210}{400}$ of an inch to $\frac{188}{400}$; and, on being examined by the touch, gave a very weak and soft pulse; while a portion of the same artery, situated immediately before this part, and which, having been more recently exposed, was $\frac{254}{400}$ of an inch in circumference, had a very strong and full pulsation.*

In order, however, to carry this examination still farther, the same carotid artery was reduced, by a ligature, to somewhat less than that circumference, which we supposed it would probably reach by death. The pulse being now felt, a little beyond the ligature, was found to be very faint and weak, while it continued very strong before the ligature. It appeared to us, that this reduction of diameter had, in both cases, so weakened the pulse in the

* Experiment XXVI.

more remote portions, even of this artery, as to have made it incapable of being felt through very thin integuments.

It is highly probable, that such a reduction of calibre had, in several of these patients, taken place in the subclavian artery, or some of its branches; although, on the principles already ascertained, the complete death of the artery, and the consequent restoration of its usual calibre by elasticity, which must have existed at the period of dissection, would wholly frustrate the attempt to discover, in that mode, the cause of the malady.

In reality, the larger arteries in the human body soon lose their contraction by tonicity; for in a man aged 64, who, in consequence of pressure on the brain, from astonishing fullness of its whole vascular system, died suddenly, late at night, on the 22d of October, 1815, and who was opened the next day at four in the afternoon, a ring of the right carotid artery, which measured transversely $\frac{353}{400}$ of an inch, after having been stretched out with great force, returned precisely to its former length.

Another cause of the failure of stroke in the pulse is the state of the heart itself. We know that this occurs from various causes impairing

the propulsive power of the left ventricle. Thus, any obstruction of the aortic orifice may so impede the efflux of blood, as to occasion more or less frequent losses of pulsation, in a remote branch like the radial artery, while sometimes the due pulsation is perceivable in the carotid. In this case, therefore, the pulse in the radial and carotid does not accord; and that in the former disagrees with the stroke of the ventricle.

An additional instance of this disproportion sometimes occurs; the action of the heart, as perceived by the hand applied to the thorax, being much more frequent than the pulse in any artery. This especially happens, where there is constriction or induration in the left auriculo-ventricular orifice, or annulus venosus; in consequence of which, the passage of the blood from the auricle into the ventricle being obstructed, or a regurgitation into the auricle by the systole of the ventricle being permitted, the pulsation of the auricle can be felt by the hand, on the outside of the thorax, alternately with that of the ventricle. This double pulsation occurs more especially after exercise.

I have reason to believe, that a similar phænomenon can take place without any organic disease whatever.

In both these cases, it is obvious, that, if the systole of the auricle is uniformly felt, and the left ventricle duly acts, the arterial pulse must be exactly one half as quick as the sensible beating of the heart. On the other hand, if the force, or expulsory power, of the ventricle be considerably unequal, so that a stroke in the artery is sometimes lost, the arterial pulse will be proportionably less frequent. The contrary proportion will take place, if, every thing else being natural, all the beats of the auricle are not distinguishable.

These are the chief occasions on which there is a diversity of quickness in the pulsation of different arteries, relatively to each other and the heart; but by what connection any of them prove that the dilatation and contraction of arteries are the cause of the arterial pulse, I am wholly at a loss to discover.

There is, however, another series of phænomena, at first view similar to the double pulse above described, but probably arising from a very different cause. A man, much advanced in years, had long suffered under what appeared to be organic disease of the heart, or large vessels, and of the liver, followed by hydropic effusions in the thorax, abdomen, and cellular mem-

brane of the lower extremities. These effects had been frequently removed by blood-letting and diuretics, when, at length, he was attacked with an increase of difficulty of breathing, and a copious spitting of blood.

During this state, as he lay on his back in bed, there appeared on the left side of the neck, from just above the clavicle, upwards to the spot where the large cervical vessels are crossed by the sterno-mastoid muscle, a strong pulsation, clearly perceptible both to the eye and finger. No such pulsation was visible on the right side, nor did that on the left extend above the anterior edge of the sterno-mastoid muscle. When the pulsating part was gently pressed, a reduplication of stroke occurred, but ceased on strong pressure. The visible dilatation was alternate with the tangible pulse of the left carotid artery, above the anterior edge of the sterno-mastoid muscle; and the latter was synchronous with that of the temporal artery on the same side, the right carotid, and both radial arteries.

From these circumstances, it is probable, that the visible dilatation, in this case, occurred in the left internal jugular vein, in consequence of the difficult evacuation of the blood from the

right side of the heart, and therefore the regurgitation of that fluid into the vena cava superior, and subclavian and jugular veins, at each systole of the right auricle.

This phænomenon must, however, be distinguished from the usual dilatation of the jugulars by the act of exspiration; since, in the example before us, the visible pulsations were eighty-four in a minute, while the exspirations, during the same period, were only thirty-two.

Why, in this case, the pulsation was confined to the left side, I know of nothing in the structure of the parts which can explain, and I was not permitted by dissection to inquire.

Besides the varieties of pulse which have been mentioned, there are others, which respect the length of the intervals, the velocity or sharpness, the magnitude or fulness, and the strength, of the stroke.

Under the length of the intervals are comprehended the states well known by the name of quickness or slowness; and also that kind of irregularity, which we often find, not only from organic, but less important maladies of the heart. Both modifications are immediately dependent on the action of the heart itself.

By the terms velocity or sharpness, I understand the length of time occupied by the systole of the ventricle, which may give to the finger, applied to the artery, the sensation of smartness or sharpness on one hand, or of deliberateness or leisure on the other. It is this sharp pulse, with a comparatively long interval, which seems to characterize the three states of articular inflammation, dropsy, and hæmorrhage ; so as to produce more or less of the sensation of metallic balls passing through a tube, as I have already described in a former work. Some of the experiments, which I have related, afford ground for the suspicion, that the longitudinal projection of the artery, described in them, may have some share in the production of this peculiar pulse.

The magnitude, or fulness, of a pulse is, evidently, when the artery is either large in itself, or has its parietes considerably stretched by its contained blood ; as, on the other hand, a small pulse arises from the small dimensions of the artery. We may conceive this latter state to occur, when the artery is either primarily slender, or is contracted, from the want of a due quantity of blood, or other causes.

The hardness of a pulse depends on the degree of resistance which the artery makes to the pressure of the finger; and we can easily conceive, that this hardness may be chiefly attributable to the momentum of blood in the vessel from the quick and strong contraction of the left ventricle; in consequence of which, it is difficult to press through the current.* So, also, as, in proportion to this difficulty, the finger will be apt to remain farther from the centre of the vessel, and thus to reach a smaller diameter of the area, there will be, in this case, a feeling of that wiriness, or contraction, of the pulse, on which authors so much dilate, as indicative of inflammatory action.

The softness of the pulse, or its easy compressibility, may arise from an opposite state of the circulation in an artery, whether it be large or small.

Strength and weakness of pulse seem to be terms expressive of states little different from the two immediately preceding.

* This effect of velocity, in producing a resistance like solidity, may be illustrated by the current of air driven from the cylinders through a small aperture into the apex of the conical smelting furnace at Carron, by which, if I rightly remember, the impulse of a walking stick was resisted as by a solid body.

Although, however, the causes assigned for these two latter states of the pulse, may have a considerable share in their production, certain circumstances in Experiments XXVI. and XXVII., to which I have already alluded, suggest other causes as contributing to like effects. In these experiments, a portion of an artery, spontaneously contracted below its usual diameter by exposure, gave the perception not only of a smaller pulse, but of one which was preternaturally weak and soft. These properties were augmented in Experiment XXVI., when the diameter of the artery was still farther reduced by a slack ligature. Now since it cannot be doubted, that, in this case, the velocity of the blood in the contracted portion was at least as great as in that immediately before it, and that the artery itself, however reduced in size, was perfectly full, we seem obliged to admit, that the softness and weakness of pulse depended on a greater laxity and sponginess of the cellular coat of the vessel, in consequence of the diminished tension; so that the progression of the blood through the artery, at each systole of the ventricle, was less strongly felt by the compressing finger. On the contrary, when, from the greater distention of the artery, the coats

became more firm and dense, the impulse of the blood would be more perceptible, and the sensation of strength or hardness proportionably greater. This would more especially happen, if, conformably to the observation of HALLER respecting minute arteries, the several coats of the larger ones approximate more to each other, when the vessel is more fully dilated.

If also, as is asserted by HALLER, with regard to sanguiferous vessels, arteries are sometimes not full of blood, it is evident that vessels in such a state, easily yielding to the compression of the finger, would give the pulse a character of peculiar softness and weakness.

How far the capacity of the coats to transmit to the finger the perception of the impulse of blood in the artery, may depend on the state of the vasa vasorum, I am altogether unable to suggest.

Mr. HUNTER supposes, that when, by exercise, the blood is more quickly returned to the heart, the left ventricle is more distended, and consequently, throwing out more blood at each systole, stretches the arterial system in a greater degree, and therefore produces a fuller and stronger pulse.* This conclusion is, however,

* On the Blood, &c. p. 145, 146.

by no means justified by what occurred in the foregoing experiments; for though, in them, we found the pulse temporarily quickened by the violent struggles of the animal, the diameter of the carotid, during that increased action of the heart, remained, at each systole, just as before.

If, indeed, the ventricle, as on this physiologist's supposition, should thus become much increased in capacity, and the aorta should not readily yield, in proportion to the increased quantity of blood expelled at each systole; either the velocity of that blood must be suddenly and enormously augmented, or else there would be danger of fatal accumulations in the cavities of the heart.

On the other hand, we find, in fact, that whatsoever urges the blood more quickly into the heart, increases the number and sharpness of its systoles; a provision, which should seem to be superfluous, were this supposed dilatation of the ventricle to take place.

If, then, the heart, continuing to throw out, at each systole, precisely the same quantity of blood as before, only repeats those systoles with a frequency and velocity proportioned to the quantity of blood which it receives; no increase

of capaciousness in the ventricle would be required, and all the phænomena, preceding, accompanying, and following the operation, would seem to admit of an easy explanation.

The strength with which, under such circumstances, the apex of the heart strikes the side, is doubtless proportioned to the momentum, with which the blood is expelled at each systole into the aorta. It does not, however, from thence follow, that the quantity is increased. The increase of velocity alone would produce an increase of momentum, although the quantity remained the same.

For these reasons, it appears to me improbable, that any such change, as Mr. HUNTER suggests, occurs in the heart; and therefore the hard, strong, or even full, pulse of an artery is not attributable to that cause.

The different states, which have been described, include the more simple variations observable in the pulse of arteries; and it is easy to see different modes, in which they are capable of being simultaneously combined, so as to produce other varieties.

How far the theory of their causes may be complete or correct, it remains for the experience of others to determine.

A FARTHER POWER OF ARTERIES.

IN order that the reader may be enabled to form an accurate judgment of a power of the larger arteries, which is about to be offered to his attention, it is proper that he should be acquainted with the general disposition of the arteries which supply the head, neck, and fore-extremities, in sheep.

From the arch of the aorta arises, nearly at right angles, one trunk only, or innominata;* from which, after a short distance, springs, on each side, either a single vessel, dividing into various branches, or a congeries of vessels, more or less from a common root.† These vessels

* Plate II. Fig. I. A. † Ibid. 3. 3.

vessels consist of the axillaries, the mammaries, and some others, which go chiefly to the outside of the neck, and fore-extremities. From some one of these branches respectively, but, so far as I have seen, not the same on both sides, arise the vertebrals, which are usually the smallest of the whole. The innominata afterwards, continuing its course in the same vertical direction, divides, like a fork, into the two carotids,* which pass onwards, with little variation as to diameter, throwing out only a few minute branches, till, just behind the angle of the lower jaw, they divide, on each side, into external and internal.† Of each of these common carotids, the area, or light, is greatly larger than that of either of the vertebrals.

The effects of ligatures, on the arteries of living animals, have long excited great interest among physiologists; but although most of the phænomena, respecting the coalescence of these vessels, were formerly ascertained by PETIT,

* Plate II. Fig. 1. B. C.

† It is extremely difficult to account for the assertion of Dr. HALES, that, in the sheep, "both carotids rose separate "immediately from the aorta;" unless the wether, which he examined, exhibited a most extraordinary deviation from the usual arterial structure in those animals. Hæmastatics, p. 27.

HALLER, and some subsequent writers, they were never systematically arranged, so as to shew their true relations to each other, till the appearance of the very philosophical work of the late Dr. JONES.*

It is now ascertained, that when the serous and fibrous coats of an artery are by any cause, as especially a ligature, broken through all around, the ruptured vasa arteriarum pour out fibrine; in consequence of which, the coats, if sufficiently approximating to each other, coalesce at the wounded part, and thus firmly shut up the area of the artery.

Much assistance to this process is usually contributed by the formation of a coagulum of blood, within the artery, near the ligature. This may take place on both sides; but, in our experiments, was always found between the ligature and the heart, as if it were there most wanted, as a barrier against the disuniting effects of the blood propelled by the systole of the ventricle.† This coagulum varies in length from a quarter of an inch to an inch, or more; taking the form of the vessel in which it is included, but with which it does not appear to unite, although

* On Hæmorrhage, &c.

† Experiments XVII. XXII. XXIII.

it slightly adheres to the fibrine at the point of coalescence of the artery.

These processes have been observed to occur in a very short time after the formation of the ligature. In the experiment above, No. XVII., they were completed in seven days: in Experiment XXIII., in less than forty hours. Dr. JONES relates an instance, in which the humeral artery of a dog had cohered after an interval of only twenty-four hours.*

The quantity of fibrine, which is adequate to the production of this cohesion, is very small. In our experiments, it could not be perceived in the artery, till the attached parts had been forcibly disunited.

Soon after this period, the contraction of the tied artery, which had begun near the ligature, usually increases to entire obliteration; which reaches to a greater or less extent on each side, according to the power of substitution of circulation, afforded by branches arising from the same trunk. It has been supposed, that part of this contraction may depend on an inflammatory process, which takes place between the coats of the arteries themselves; but the chief agent is, probably, the tonicity of the fibrous arterial coat.

* P. 139.

From the experiments of Dr. JONES, we also learn, that the wounded cellular membrane, surrounding the artery, usually pours out more or less of fibrine, which gives additional strength to the artery in the neighbourhood of the tied part.

Amidst these processes, another takes place in the cellular coat of the artery, which, in consequence of the compression by the ligature, undergoes suppuration; and it is evident that this suppuration must extend to the complete division of the artery, in all those cases, in which, as not uncommonly happens, the ligature comes away round and unbroken.

The ultimate reduction of volume, in the obliterated part of the artery, is said to be, in many cases, so great, that nothing remains but a mere filament, scarcely, if at all, distinguishable from the cellular substance, with which it is blended.

When, in a living animal, an artery leading to any part has been thus obliterated, it is well known, that a quantity of blood, sufficient for the performance of the functions of the part, is usually conveyed by collateral trunks, or small branches given off nearer to the heart than the point of obliteration. These are

dilated at first, according to the necessity of the case; but, in process of time, a few only, become permanently distended, superseding the rest, which collapse in different degrees, when no longer required for the purposes of circulation. Many facts of this kind, are collected in the late valuable work on the Diseases of Arteries and Veins, by Mr. HODGSON.

Neither in that work, however, nor in any which has preceded it, can I find the least mention of another benevolent provision, for the same purpose, by the wise and all-powerful Author of nature.

On the 27th of September, 1815, Mr. G. NORMAN tied both carotids of a Ram, three years old, each with a single ligature. No inconvenience appeared to have been sustained by the animal, except what might reasonably be supposed to result from a painful operation.

On the 17th of October, the ligatures had come away, and the wounds were completely healed.*

On the 7th of August, 1815, this ram was killed, by opening the aorta from above. The weather being very hot, he was almost imme-

* See above, p. 1, 3, 5.

diately injected; and the parts were, as soon as possible, dissected by Mr. COOMBS.

Notwithstanding the shortness of the time which had elapsed subsequently to the death of the animal, and which probably occasioned the injection on the right side to fail, that on the left had run extremely well, and exhibited the extraordinary construction, which is very accurately represented in Plate II. engraved from drawings made from the object, on the 8th of August, by Dr. CHARLES PARRY.*

In this Plate, Fig. I.

1. Refers to the Trachea.

2. 2. 2. 2. Adjacent muscles.

A. The common arterial trunk, or Innominata, arising from the arch of the aorta.

3. 3. The roots of the various arteries of the neck, &c. before-mentioned.

B. The right carotid.

C. The left carotid.

D. The spot where, probably, the ligature on the left carotid was made.

E. to F. The extent to which, it is probable, that the old portion of the artery was

* The drawing is rather less than nature, and the object is represented as somewhat fore-shortened.

obliterated; its place being supplied by five new ramifications, uniting, in different points, the extremity of the inferior portion of the old artery, with the superior portion.

Since some of the points of union above are considerably distant from what looks like the abrupt termination of the upper portion of the artery, it is not improbable, that as much of that part of the artery as extends nearly from D. to F. and is equal in size to other parts of the vessel, is itself a regeneration of the entire artery; which, if it had completely met the inferior portion, might have wholly superseded the small new branches, and have caused them to be obliterated, as no longer useful. This, however, I offer as a mere conjecture, to be confirmed or annulled by future and more long continued trials.

Plate II. Fig. II.

This is merely an enlarged. and somewhat different outline view of the newly generated vessels, with similar referential letters.

To this conclusion, as to the new creation of vessels in the case before us, objections have been raised, with an ardour, which has certainly

reached the utmost limits of philosophical candour. Thus it has been argued,

First, that these communicating branches must have previously existed, although they were not noticed.

This is an objection, which could be made only by those, who are unacquainted with the structure and relations of the carotid arteries in sheep. I have assisted at a careful examination of thirty-four of these. They may, with facility, be dissected for several inches from all their connections, whether of nerves or cellular membrane. Several fingers may be placed under them: they may be drawn out, so as to project an inch or two from their natural seat, and may be examined all around, as easily and as distinctly as dead cords. Under these circumstances, it would be absurd to suppose, that a plexus of vessels could ever be mistaken for a single trunk; or that such vessels, projecting as they do all around from the main line of the artery, could have been separated from the surrounding parts, without being wounded by the knife.

But, it is said, they were too small to discharge blood; and only became enlarged, when

their enlargement was rendered necessary by a ligature.

This argument, then, implies, that a main trunk existed, from which these several ramifications extended, uniting the different parts of the artery by additional communications, which became apparent only when the trunk was destroyed.

There is, evidently, only one view of the case, to which this argument can possibly apply ; and that is, the supposition that the ligature was made on the artery within the extent of these ramifications. In what mode, however, a ligature could be applied to a round large vessel, included within such a plexus of collateral arteries, without that plexus being either cut or seen, it is for those to explain or conceive, who make the objection.

If, however, we for a moment admit, that some of these vessels, though cut, did not, on account of their minuteness, pour out blood; they certainly ought to have done so, when, on account of the ligature on the corresponding trunk, an immediate necessity was produced for the transmission, through them, of a greater quantity of blood, and for their consequent enlargement.

Notwithstanding, however, in the various experiments on the carotids of living sheep, which I have witnessed, twenty-four ligatures have been at different times employed, I have never seen any such plexus of vessels as that which this hypothesis suggests. The arterial branches given off by the carotids are, in reality, very few; but where they have existed, they have been easily enough seen, and have been readily secured by ligatures.

While, therefore, I contend, that no such plexus of vessels naturally occurs in the carotids of sheep, I would ask, whether any instance of a similar arterial structure is to be met with, on any other occasion, in the system of warm-blooded animals? In the anastomoses of arteries, the minute branches of one artery unite with those of another, or may even, occasionally, form circles among themselves; but I am not aware of any example, in which small branches, leaving a trunk, have, at the distance of one or two inches, returned again into the same trunk. If, therefore, such a structure is absolutely new, this argument is of that kind, which would solve one difficulty by a greater; or which, according to the language of the schools, would explain *ignotum per ignotius.*

If, nevertheless, it be contended, that this plexus originally existed without any collateral trunk; then I would ask, why it was not rendered impervious, and destroyed, by the ligature, as happens to other vessels, which have undergone a similar operation.

The same answer will apply to those who argue, that these vessels existed in the coats of the artery themselves, and, when the portion of the vessel beyond them was tied, served to carry the blood from the cavity of the artery before the ligature, to that beyond it.

This theory presumes, that the arteries of the coats, or vasa vasorum, take their rise from the cavity of the trunk itself; which, indeed, PORTAL asserts to be the case, with regard to some of the arteriæ arteriarum of the aorta. Mr. HUNTER, however, expressly denies this structure to exist in the carotids, as he was never able to make the finest injection into those arteries enter the vessels of their coats.

The argument, however, is, in every view, altogether nugatory; since a ligature on the artery, before such a set of vessels, would either not influence, or else destroy, them; and a ligature on, or beyond them, must cut off all communication between the vessels of the coats, just as effectually

as it intercepted the flow of blood in the main artery itself: so that this very theory must assume the production of new vessels, for the purpose of restoring the lost communication.

But it has been said, that the ligature was applied in some other part, and that this plexus was a lusus naturæ, altogether unconnected with the effect of any operation.

This term, lusus naturæ, is an extremely convenient mode of cutting a knot, which we have not the ingenuity or the patience to untie. Since, however, it is a mere gratuitous assertion, incapable of proof, and unproductive of any explanation of the phænomena; we can hardly suppose that it demands a serious answer, when the phænomena themselves are capable of being somewhat analogically, and therefore reasonably, explained.

That a ligature should have been carefully made on an artery, like the carotid of a sheep, without producing some effect on the organization of the vessel, will hardly be asserted by any experienced anatomist. It is even well known, that a moderate degree of pressure, by a fine ligature, will often cut through the serous and fibrous coats; in which case, as complete a coalescence of the parietes of the artery will

sometimes ensue, as if the ligature had been retained in its place, and produced the usual chain of effects. Although, however, this ligature was, in the case before us, actually applied, and was observed to have been retained to at least the thirteenth day; yet, throughout the whole course of the artery in question, no spot could, after the most careful examination, be found, on which a ligature could have acted, except the individual part to which I have referred its effects. Every other portion of the vessel was smooth, equal, and completely pervious.

In this process of reproduction, there is a considerable degree of analogy with certain well-known phænomena of the animal frame. If a muscular part be wounded, and the parts properly applied to each other, coagulable lymph, now better known under the name of fibrine, is poured out, and serves as a matrix, through which new vessels shoot, and unite the separated portions. So, also, if a loss of substance occur in any one of various textures, so that the defect prevents the apposition of the extremities of the old part, we usually see this loss, in a greater or less degree, supplied by the application of new substance, of which vessels form a due proportion, and are so disposed

as to re-unite those previously existing. This effect is the real result of the process, whatever be its mode; for it matters not whether new vessels begin, by a sort of crystallization, in the centre of the intervening fibrine, and find their way on each side into the old vessels; or the latter vessels shoot out, so as to meet in the center; or, lastly, a combination of these two processes occurs, so as to complete the march of the circulation. In either case, the fact, with regard to the intermediate part, is precisely the same; and carry the objection ever so far back, even, if you please, to the aorta at one termination, and the capillaries at the other, there must either be some point at which the new vessels communicate, at each end, with the old, or else you are reduced to the still farther absurdity of supposing the creation of an entirely new sanguiferous system, throughout the animal, every time he accidentally suffers a loss of substance of a few lines in diameter.*

* It is curious, that, on this subject, the zeal of scepticism has even denied the existence of newly-formed arteries in granulations. It would be hardly less unreasonable to deny that they contain blood; because, by whatever channels this blood is carried to that substance, and continues to nourish it, such channels, by whatsoever name we may choose to call them, so far perform the functions attributed to arteries.

The process of formation, in the new ramifications before us, seems extremely similar to that just described. Dr. JONES, and other writers, speak of an effusion of lymph round the external part of an artery suffering a ligature. If this is an universal occurrence, during a favorable progress of recovery, it is not improbable, that such an effusion may form a proper nidus, in which new vessels may shoot, so as at both extremities to unite the old one in such a manner, as to perform the due functions of circulation. This reproduction of artery would thus be conformable to the benevolent and powerful agency of the Creator, by which bone is formed, in order to repair the loss of bone, muscle of muscle, nerve of nerve, &c.; these several new parts ultimately performing the functions of the old, though, perhaps, with somewhat less of perfection as to their structure and powers.

In the explication which I have given of the first figure of the second plate, I have suggested the probability of a formation from both ends of the obliterated artery. This has been denied with regard to the more distant portion, on the supposition, that no adequate arterial impulse exists in that part. Such a supposition is, however, contradicted by the fact; for it appears

from Experiment XXII., that when, by cutting through the artery between two ligatures, the more direct impulse of the heart on the blood was intercepted, there was a strong retrograde motion of the blood in the more distant portion of the artery, evidently following the round of circulation by the anastomoses, and according with each systole of the ventricle. This fact, which was too palpable to admit of any mistake, wholly obviates the theoretical objection last stated.

The disappearance of the old portion of the artery, and of the fibrine in which the new plexus may have been formed, is conformable to the order of facts already described, and which is now generally admitted by physiologists.

Although, however, this was the probable way, in which, in the example which we are investigating, the new generation of artery took place, I am by no means prepared to assert that it was the real one; the proper demonstration of this fact requiring, not only that a considerable number of animals should be subjected to the experiment, but that the parts should be carefully examined at various periods after the application of the ligatures.

The last objection, which it may be worth while to mention as having been made to the production of small vessels, for the purpose of supplying the place of an useless trunk, is, that the process is altogether new in the animal frame.

In order to give any weight to this objection, it is incumbent on those who make it, to shew, that, in other cases, in which the same previous circumstances have existed, the same effect has not followed.

Who, however, is there, that has had opportunity and patience to make a similar experiment? Who, after having fixed ligatures on both carotids of a sheep, has waited a year, in order to witness the result of the trial?

In the mean while, I have shewn a considerable degree of analogy between this case, and that of the common reproduction of various animal textures.

But were it even true, that no analogy existed, shall we, for that reason, deny the fact? On this principle, no discovery of any new phænomena in the material world could ever be made. When SIR HUMPHRY DAVY first exhibited Potassium, had any one even suspected that Potassa, an alkali, was a metallic substance combined with oxygen? Such a conclusion

was not only not conformable to experience, but directly contrary to all presumed analogy. And yet, when the fact was seen, it was without difficulty admitted.

In order farther to investigate this subject, let us consult the work of Mr. HODGSON, which is, certainly, a very copious and accurate magazine of facts. Among the several arteries, which are there adduced, as having been the subjects of ligature, or of accidents producing obliteration, in man or brutes, are the femoral, the popliteal, the iliac, the subclavian, the axillary, the brachial, the carotid, and the aorta. In all these cases, except the last but one, the circulation is said to have been carried on by various branches, either known or supposed to have previously existed; and which, anastomosing with other branches, restored the circulation in an adequate degree, usually even to the trunk of the vessel beyond the obliterated spot. This happened, in various instances, even to the aorta itself.

With regard, however, to the common carotid, we know that, both in man and brutes, it has few anastomosing branches, till it unites, within the cranium, with its corresponding carotid, and with the vertebrals. Among the few examples in man, in which this artery has

been tied, or otherwise obliterated, I can find none, in which the mode of the vicarious supply of blood has been traced by dissection. Mr. HODGSON, indeed, suggests, that, exclusively of the large anastomoses within the cranium itself, the upper part of the carotid may be supplied with blood from the anastomoses of the carotids with each other, and with the subclavian arteries; and he mentions an instance of a Dog, in which, several months after both carotids had been tied, "the vertebral arteries "appeared rather larger than natural, and "branches which arose from the superior por- "tion of the obliterated carotid arteries, anas- "tomosed freely with others that were given "off by the vertebral, cervical, and subclavian "arteries."*

Although, however, in this instance, after a ligature on both carotids, a quantity of blood, sufficient for the purposes of the brain, was, in the dog, conveyed to that organ by the enlargement of some arteries, and the anastomoses of others; it by no means follows, that a similar consequence must necessarily ensue from the obliteration of the same arteries in other animals.

* P. 244. Rather *axillary* arteries. A Dog has no clavicle.

In fact, all the authors who have treated this subject in Great Britain, seem to have considered the dog as an animal nearly exempt from all the evils which might *à priori* have been expected to flow from so important a change in the arterial circulation.

From the experiments which I have related, it appears, that in the sheep, the sudden destruction of a large part of the communication between the heart and the brain is by no means always a matter of such perfect indifference. In a full-grown Ram, in which, some years ago, Mr. GEORGE NORMAN, in my presence, made a ligature on both carotids, without including either of the nerves, the immediate effect of the operation was so strongly felt by the animal, that we judged it expedient to kill him shortly afterwards. In Experiment XVII., an accurate account has been given of the effect of this operation in a younger Ram, from the beginning to the death of the animal.

It seems, therefore, that, in the sheep, there is a necessity for the reproduction of carotid, which, perhaps, may not exist in other vessels, or, possibly, not to the same extent in the dog, or certain other animals.

Is it, however, absolutely certain, that, on various occasions, arteries, which have been considered as mere anastomosing branches, brought into specific notice by enlargement, may not have been actual new productions? That this may sometimes have been the case, there is considerable reason to suspect.

Dr. JONES* relates the case of a Dog, the left carotid of which had been divided between two ligatures. Forty-nine days after, a considerable extent of the artery was found obliterated, and the right carotid evidently enlarged. An artery also, arising from the angle at the origin of the two carotids, passed up on the

* Chap. iv. Sect. I. Experiment XIII. Pl. xv. In the description which this author gives, he is guilty of an anatomical error, when he states the carotids as arising from the arch of the aorta. In the dog, the aorta first gives off a large vessel, which may be called Innominata. This is curved to the right, and from its curvature spring together the two carotids. The innominata, continued on, forms the right axillary, from which spring the right vertebral, and various other arteries. The left axillary arises by itself, from the arch of the aorta, to the left of the innominata. The small subsidiary artery, mentioned by Dr. JONES, does not arise immediately from the aorta, but from the innominata, in the angle between the two carotids. It is this innominata, and not the aorta, which constitutes the large curved vessel in Dr. JONES's plate.

right side of the œsophagus, which it crossed, and anastomosed with the upper part of the left carotid, where it was still pervious; thus maintaining the communication between the heart and the artery above the ligature.

Having many years ago investigated, with my excellent friend Dr. JENNER, the disposition of the carotids, &c. in a dog, of which I then made a drawing, and having found no such artery as this of Dr. JONES, I was desirous of making some farther experimental inquiries; and therefore requested Mr. COOMBS to inject, and afterwards dissect, two other dogs. This he was so obliging as to do; and in the first, which was a large puppy, found a small artery situated, conformably to the description of Dr. JONES, near the angle of the two carotids. This artery, however, so far from ascending by the side of the œsophagus, was reflected downwards into the thorax, where it was lost. In the second dog, which was considerably older, no vessel whatever was to be found in or near the spot described.

What then; shall we say, that this extraordinary structure was expressly provided, in order to repair an accident, which should happen to the left carotid only, and in this individual animal?

It is much more reasonable to conclude, that this accessory artery itself was either a new formation, or an old artery turned into a new direction, for a new and important purpose.

Without, however, laying any stress on this supposed analogy, let us see whether the opinion, which I have thus attempted to defend, cannot be supported by more direct and unquestionable evidence.

I have mentioned above, that, in the Ram, Experiment III., the right carotid artery was tied with a single ligature, on the 12th of October, 1814; and that soon afterwards, in fighting, this ram had the ligature torn out, and suffered a considerable loss of blood. As Mr. G. NORMAN and myself were present when this accident happened, the hæmorrhage was restrained in the manner above described.* This ram was killed on the 7th of September, 1815,† and an attempt was made to inject the carotids. It succeeded very well on the left side, but failed on the right. A subsequent examination, by dissection, shewed the right carotid changed into that state, which is exhibited, somewhat enlarged, in Plate II. Fig. 3. This figure was drawn by Mr. COOMBS. The

* See p. 5. † See Experiment XV.

original trunk of the artery, for something more than two inches in length, from E to F, had disappeared; and for it were substituted three small branches, uniting the two extremities of the old artery. One of these, G, immediately united these extremities. The two other branches were continued into a somewhat flattened oval fleshy body, at D, from whence they separately proceeded into the more distant extremities of the old trunk, by the side of the first branch. All these ramifications, as well as the oval body, were pervious, although they would not admit the common coarse injection. The old artery, both before and beyond, was gradually contracted towards the newly formed vessels, in a greater degree than is expressed in the drawing. Whether the branch H existed before the ligature was made, I cannot determine.

The Ram of Experiment II., the left carotid of which was tied on October 5th, 1814, so as probably to cut through the cellular and fibrous coats, and was again loosely tied with another ligature, was killed on the 11th of September, 1815, and almost immediately injected from the innominata. Here the injection ran well on the right side; but on the left it failed, the artery being found to be, throughout, much

contracted below its natural degree. For three quarters of an inch, where the ligature had apparently been applied, the trunk of the artery was entirely lost; and in its stead were two small cylinders, uniting the ends of the pristine artery. One of these was, in some degree, tortuous, and appeared to be hollow; having in it some globules of blood, and communicating with the cavity of the artery below, from which it could be inflated with a blow-pipe. Its cavity was, however, too small to receive the coarse injection, which had stopped at its commencement, but was found to have filled the old part of the artery above, by some other communication. The other cylinder, which was attached to the former by a film of thin cellular substance, was not found to be hollow.

These appearances in the two last sheep were equally unexpected with those in the first; since it seemed *à priori* probable, that, as in them the ligature was applied on one side only, the corresponding artery would have transmitted to the brain as much blood as was adequate to all its purposes.

If, however, in these two instances, the reproduction of artery was less perfect than in the first, we may reasonably attribute this dif-

ference to the less degree of necessity which arose out of the perviousness of the opposite carotid.

Since, then, this peculiarity of structure occurred in all the three cases of sheep, in which the artery was, for a sufficient length of time, exposed to the effects of the ligature, and was never found in any of the much greater number of examples, in which the carotids had not been so treated; I think we may, with a degree of probability amounting almost to certainty, infer, that these accessory vessels were, in each case, new formations, intended to compensate the loss of the old artery, the functions of which had been destroyed by the ligature.

It will, however, from the detail of our experiments, have been seen, that I have now under trial three other Rams, of which one has a ligature on one carotid, and the two others on both. Whatever results these experiments may afford me, I propose to detail in an appendix to this work.

In the mean while, it were greatly to be wished, that similar trials might be made, by others, on animals of various kinds; and that the gradations of change at different periods were carefully examined, and faithfully recorded.

The language of HALLER ought to be that of every physiologist. " Des experiences réiterées " donnent un nouveau degree de force à ce que " les autres nous ont appris; et je pouvois esperer " de découvrir quelques vérités, qui leurs " seroient échappées; esperance fondée sur " une bonté, que je connois à la nature. On " ne la consulte jamais en vain; et elle recom- " pense toujours les travaux de ceux, qui " l' étudient."

FINIS.

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Fig. II

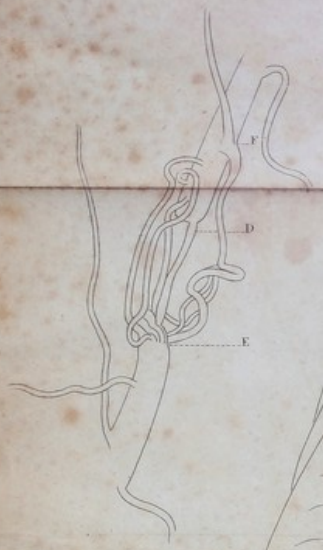

Fig. I

Fig. III

Fig: II.

Fig: I.

Fig: III.