Professor Allen Thomson

with the Authors sincere regards

ANATOMICAL

AND

PHYSIOLOGICAL OBSERVATIONS.

ANATOMICAL

AND

PHYSIOLOGICAL OBSERVATIONS.

BY

JOHN STRUTHERS, M.D.

FELLOW OF THE ROYAL COLLEGE OF SURGEONS OF EDINBURGH,

LECTURER ON ANATOMY.

PART I.

EDINBURGH:

SUTHERLAND AND KNOX, GEORGE STREET;

SIMPKIN, MARSHALL, & CO., LONDON.

1854.

MURRAY AND GIBB, PRINTERS, EDINBURGH.

PREFACE.

This Volume was prepared for the students attending my lectures last winter Session. It consists chiefly of Papers which have already appeared at different times in the Medical Journals, to which Notes and several Papers are now added. For the shortness of these additions I can only offer the excuse, that they were made, and the Volume prepared, during the latter weeks of a very busy Session, during which I was occupied in lecturing twice a-day, and superintending the dissecting room studies of a class of four hundred students; a laborious system which custom has entailed upon the teacher of anatomy here, and which, though beneficial to the student, and although it may be held that our time cannot be more usefully occupied than in teaching,—leaves much less leisure than I would wish for the no less pleasing occupations of research and authorship.

J. S.

Edinburgh, June 12, 1854.

ERRATUM.

Page 89, line 8—for discus *read* viscus.

CONTENTS.

		Page
I.	ON THE SUPRA-CONDYLOID PROCESS, . .	3
II.	ON THE OBLIQUE MUSCLES OF THE EYE IN MAN AND VERTEBRATE ANIMALS, . . .	7
III.	ON DOUBLE STOMACH IN MAN, . . .	19
IV.	ON THE NERVES OF THE ORBIT, . . .	27
V.	ON OPEN FORAMEN OVALE AND THE FŒTAL HEART, ...	63
VI.	CASE IN WHICH THE PULMONARY ARTERY GAVE ORIGIN TO THE DESCENDING AORTA AND LEFT SUBCLAVIAN, ...	75
VII.	ANATOMICAL INQUIRY INTO THE MODE OF ACTION OF LOCAL BLOOD-LETTING, . .	79

Page

VIII. DISSECTION IN A CASE OF PARALYSIS OF THE COMMON MOTOR OCULI NERVE, . . 95

IX. DESCRIPTION OF AN ESQUIMAUX FEMALE PELVIS, 105

X. ON THE SEMILUNAR VALVES OF THE HEART, . 109

XI. ON RUDIMENTARY RIBS, . . . 119

XII. ON BRANCHES FROM THE FIFTH PAIR OF NERVES TO THE MUSCLES OF THE EYE, . 133

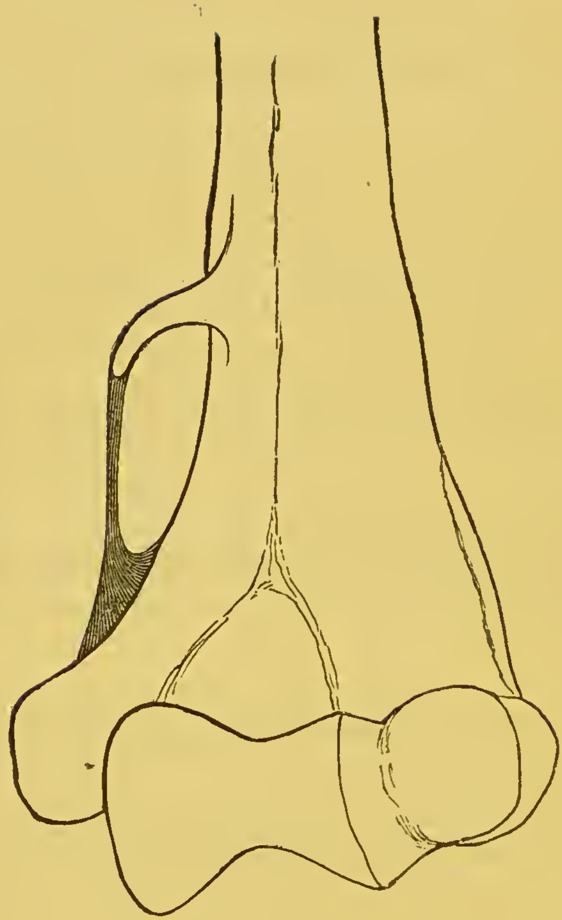
XIII. ON DIVERTICULA FROM THE SMALL INTESTINE, ... 137

XIV. ON VARIOUS POINTS IN ANATOMY, CONSIDERED IN A REVIEW, ... 167

XV. ON THE ABNORMAL ANATOMY OF THE ARM, . 193

XVI. ON THE FASCIA OF SCARPA, . . . 231

Part II. will contain Observations On the Hip Joint—The Fœtal Circulation—The Surgical Anatomy of Club Foot—on the Cystic Duct—On Some Points in the Anatomy and Physiology of the Brain and Spinal Cord—and On the Structure and Adaptation of the Muscles of the Inferior Extremity.



I.

ON THE SUPRA-CONDYLOID PROCESS.

FROM THE MONTHLY JOURNAL OF MEDICAL SCIENCE, OCTOBER 1848.

A FEW instances have been recorded in which the humeral artery and median nerve continued downwards in a direction towards the internal condyle, above which, in order to gain their usual position in front of the elbow, they passed around a peculiar process, there developed from the humerus, as if for the purpose of affording them protection.

This singular arrangement has occurred comparatively so often of late in the subjects dissected in my rooms, as to induce me to call more particular attention to it, since it is one of some importance to the surgeon, and is interesting also from the analogy it presents to the usual arrangement of these parts in certain animals.[1]

The process, and the ligament by which the protective arch is completed, are represented by Tiedemann;[2] but, in this instance, the artery which passed through was not the main trunk, but a large interosseous, which had arisen high in the arm. The first description which I find of this peculiarity is by Dr Knox,[3] who —after reasonably doubting the correctness of a statement, which

[1] In many animals—amongst the Quadrumana, Rodentia, Edentata, Marsupialia, and more especially the Carnivora—the median nerve and humeral artery pass through an opening in the humerus, which is formed by an arched process crossing obliquely the inner surface of the bone, close above the internal condyle.

[2] Tabulæ Arteriarum, Pl. xv. fig. 3. 1822.

[3] Ed. Med. and Surg. Journal, 1841, p. 125.

appears to have been made by Magendie, that there existed a "supra-condyloid" process and foramen in the humerus of the yellow races of Africa—states that approaches to the same structure might be found in many European arms; and records an instance in which the arrangement had just occurred to him in "the left arm of a stout muscular middle-aged man," with the deviation of the artery and nerve in order that they might pass around the process. Another instance of this arrangement is recorded by Mr R. Quain in his recent work on the arteries, and is represented in Plate xxxvi. fig. 3.

I first saw this variety in the instance already quoted from Dr Knox, the preparation—showing the artery and nerve passing underneath the arch—being now in my possession; and I have since met with it on seven occasions in the dissecting room. The first of these I met with four years ago, on the left arm of a female subject. The second and third occurred the following year, in both arms of another female subject; and the remaining four during last winter session, being in both arms of two male subjects.

In all of them the protective arch was well formed, and transmitted the median nerve and undivided humeral artery, except on the right side of the last subject, on which the artery held its usual course, the nerve passing through the arch, along with a small muscular artery, which, in size and origin, resembled the inferior profunda. The osseous portion of the arch arose at a distance from the internal condyle, varying from an inch and a quarter to two inches and a half, and nearer to the anterior than the internal border of the humerus. It then curved downwards and inwards, and—in none of the above instances having exceeded three-fourths of an inch in length—gave attachment to the ligament, which, stretching in the same direction, joined the line close above the condyle, and thus completed the arch. In four of the above instances, the process, as now seen in the macerated bones, is from half to three-fourths of an inch in length. In the other three it is shorter; more especially in that one in which the arch transmitted the nerve but not the artery, whilst on the other humerus, from the same subject, the process is the longest. The latter is represented in the drawing of the lower third of the bone. The process and ligament are seen on their external and inferior aspect, so as to show the size of the opening as it now appears in the preparation.

In observing the development of the arch in the cat, I find that, at different stages of its growth, it forms a process resembling very much the conditions which it has presented in man. It is developed as part of the shaft of the bone, the ossification proceeding from above downwards; and very soon after birth it becomes united below to the lower end of the shaft, not to the epiphysis, of the bone. This account of the formation of the opening in animals, is opposed to the view of those who believe "that the passage of the artery through a foramen of the humerus, results simply from the extensive development of the inferior extremity of that bone,"[1] by which the vessel becomes at length encircled, in order, merely, that it may not be thrown out of its original and direct course; and tends to confirm the more common view, that the arrangement is one specially for the purpose of affording protection to the parts which the opening transmits; although, on the other hand, a consideration of the habits of the animals in which the arrangement occurs, and the dissection of the parts, do not render it evident that they, more than some others, require such a special means of protection for the nerve and artery.

The above instances of the occurrence of this singular variety in man, having led me to think that it might have been present more frequently than had been noticed or recorded by anatomists, I lately examined the preparations of these parts in the museum of the Royal College of Surgeons of this city; and amongst those which had belonged to Dr Barclay, I found four instances of it, although he has not alluded to the variety in his work on the arteries. Two of these occur in separate arms, both of the left side. One that of a very young child, where the process is long; the other that of an adult, where the ligament also is preserved. The other two are seen in the preparation of

[1] Introduction to the Natural History of Mammiferous Animals, etc. P. 89. By W. C. Linnæus Martin.

a child. The process is equally developed on both sides, but the ligament has been removed. On the right side, the undivided artery deviates and turns round the process. On the left, as in the other two instances, the radial artery arises above the middle of the arm, and continues downwards along the inner margin of the biceps, and the larger trunk (ulnar-interosseous) is removed to the inner side, and passes around the process.

The number of the above instances is sufficiently great to render the variety one of interest to the surgeon. It will be observed that in one-third of them there was a high division of the humeral artery, and that, whilst one of the trunks occupied the usual position of the vessel, the other was removed considerably to its inner side; and in the remaining two-thirds, that the undivided trunk was so situated, that it could scarcely have been reached by an incision made in the usual course of the artery. Further, at the bend of the elbow—as the pronator teres (or an additional muscular slip, which below joins the pronator) arises, in such a case, as high as from the ligamentous part of the arch—the artery is so overlapped or covered, that before it could be exposed here, some muscular fibres would require to be displaced or divided; although, it may be remarked, the circumstances usually requiring an operation upon this part of the vessel could scarcely occur, owing to its deep situation.

It may be added, that the bony prominence was very readily felt before the dissection of the parts was begun, in the subjects in which it occurred in my rooms, even in the instance in which it was shortest; and the detection of this—in the preliminary examination, in a case where the operation was about to be performed—would, together with the situation of the pulsation, and the effect of pressure on the artery or arteries, at once indicate the existence of the variety in question to one who was aware of its occasional occurrence.[1]

[1] For farther observations on the supra-condyloid process, see Article XV.

II.

ON THE OBLIQUE MUSCLES OF THE EYE,

IN MAN AND VERTEBRATE ANIMALS.

FROM THE MONTHLY JOURNAL OF MEDICAL SCIENCE, OCTOBER 1849.

NEARLY ninety years ago it was remarked by Porterfield, that "though the action of these two oblique muscles seems pretty evident, yet there is scarce any part of the human body about which anatomists have differed more than in assigning them their proper offices:" and the observation is still appropriate, although the subject has since engaged the attention of various anatomists.

The question does not admit of being decided by experiments on the lower animals, chiefly because mammals are provided with an additional muscle, or series of muscles, for retracting or suspending the eye, which are beyond the reach of trustworthy experiment, and to which motions of the eyeball might be owing after the straight muscles had been divided. Accordingly, the opinions of most authors on this matter are founded upon a consideration of the manner in which it appeared to them that these muscles, from their direction and attachments, would influence the ball of the eye, so as to change the direction of its axis; and very different conclusions have been arrived at by the most eminent anatomists.

A necessary preliminary to such an inquiry, is a correct understanding as to the manner in which the eyeball actually moves. It appears to me that all the movements of the human eyeball are purely rotatory in their nature, as if it moved within a closefitting socket, or, as it were, on an universal pivot at its centre. The axis on which the rotatory motions occur will always be at right angles to the moving power—transverse when the upper and lower recti act, vertical during the action of the lateral recti, and intermediate in the diagonal movements, which are performed by the conjoint action of two neighbouring recti muscles. Again, in the motion of circumduction, as in turning the eyes round a room, which is performed by the alternate action of the four straight muscles, the centre, as it were, still remains unmoved, one end of the axis being depressed as the other is raised, and so on. In accordance with this we find the straight muscles sweeping over the globe of the eye, to become inserted considerably anterior to its middle, so as to fit them for producing these various motions; by all of which, it will be observed, the direction of its axis is changed. But it is held by some, that, independent of any change in the direction of its axis, the eye, under certain circumstances, undergoes a movement quite different in its nature—that of rotation on its antero-posterior axis, or what is usually called the axis of the eye,—and that it is the function of the oblique muscles to produce this movement: which I shall call *lateral rotation.*

It appeared to me that light might be thrown on this disputed subject by the evidence which comparative anatomy would no doubt afford; for, although the existence of corresponding muscles has been noticed in passing, by various writers on comparative anatomy, the arrangement of them with reference to their action on the eye does not appear to have been inquired into; and, having made careful dissections of these parts, in a great variety of instances, I am enabled to furnish, from my notes, the following summary of the arrangement of the oblique muscles in the different classes of vertebrate animals,—avoiding notice of the comparative anatomy of the other muscles of the eye, as in no way affecting the present inquiry.

COMPARATIVE ANATOMY.

In Mammalia, the *superior obliqiie* has the same general arrangement as in man. The differences may be stated thus:—1. That it is usually fleshy for some distance beyond the pulley, and therefore longer, at the same time that it is relatively larger. 2. It is usually not, as in man, directed backwards to the axis of the eye, but meets it nearly at right angles; and its course from the pulley to the eye is more horizontal than in man: and 3, That its point of insertion is not so far towards the outer and back part of the eye as in man, but is usually only a little external to the vertical axis, and not more behind than in front of it. The insertion varies slightly in different animals, but in the greater number the insertion of the tendon is chiefly concealed by the upper rectus, the posterior border only appearing beyond it; whereas, in man, the tendon passes some way beyond, and also behind the vertical axis of the eye. The *inferior oblique*, also arising as in man, is not, as in him, directed backwards to the axis, but either forwards or at right angles to it. But the most striking difference is in the insertion. This in man is towards the posterior part of the eye, but in quadrupeds it is much farther forwards, as well as lower down. Usually it is attached between the lower and outer recti—its posterior border often reaching a little beneath the latter—and to the anterior third of the sclerotica, in some coming quite close to the margin of the cornea.

In Birds, Reptiles, and Fishes, the *superior* oblique arises from the fore part of the orbit, and is precisely the counterpart of the *inferior*, which is disposed much as in the mammalia; and there is so little difference in their arrangement in these different classes of vertebrate animals that they scarcely require separate description in each. The two muscles arise together from the fore part of the inner, or anterior, wall of the orbit. Passing outwards, they soon separate, the superior to be inserted near to the tendon of the upper rectus, the inferior to a corresponding point below; their direction to the axis of the eye being at right angles, or more usually somewhat forwards. In *birds*, the superior oblique is much the largest muscle of the eye, being two or three times the size of the inferior, which is not much larger than one of the recti. It is broadest at the insertion, which is partly beneath the tendon of the superior rectus, reaching near, and sometimes up to, the outer border of that tendon; but the greater part is attached, on its inner side, at the upper part of the space between it and the internal rectus. The *inferior* oblique is inserted partly in front of the tendon of the lower rectus, and partly to its inner side, and is nearer to the cornea, or osseous zone, than the superior. The space between the eye and the two oblique muscles is occupied by a large branch of the fifth nerve and part of the Harderian gland, which supports the eye on its lower and inner aspects. In the *frog* they seem not to pass so far outwards. The superior is attached to the inside of the tendon of the upper rectus, and is not larger than the inferior; and the two muscles meet the axis of the eye at right angles. But in *fishes* they pass further outwards—the superior in front of and over the tendon of the upper rectus, to be inserted partly in front of it, and partly to its outer side; and the inferior has a corresponding position and attachment below. They are directed somewhat forwards to the axis of the eye.

In inferring the action of these muscles from their anatomy, it is essential to notice their direction with reference to the axis, and also whether they are attached before, behind, or at the middle of the eye. In man they are attached behind the middle, especially the inferior, and their direction is backwards to the axis. In most mammals the superior oblique is not nearer to the front than the posterior part of the eye, but the inferior is rather nearer to the former, and both the muscles are of great breadth. In man the axes of the eyes are directed almost straightforward, but as we descend the scale of vertebrate animals, they become more and more turned outwards. Among mammals the direction of the axes varies considerably. When they look much forwards—as, for instance, in the cat—the line of action of the oblique muscles is directed backwards, as in man. When the axes are turned quite outwards—as in the rabbit and hare, in which the axes of both eyes cannot possibly be directed to the same object—the oblique muscles are then directed forwards to the axis; but in most quadrupeds,—in which the eyes look forwards and outwards (so that, if both eyes are used on one object, the visual axis cannot correspond to the axis of the eye),—they may be said to meet the axis at a right angle, as well as to embrace the middle of the eye. In birds, most reptiles, and in fishes, the eyes are placed so that the rays from one object cannot enter both pupils, and from this outward direction of the eyes the oblique muscles are directed somewhat forwards to the axis, although often nearly at right angles. Also they are usually, in these classes, attached rather in front of the middle of the eye, although from their great breadth they embrace a considerable part of it. Whilst I have described their direction in these classes, and in some mammals, as being rather forwards *to the axes of the eyes*, their actual direction *in the orbits* is backwards when the opposite sides are considered together; but in most mammals their course is nearly horizontal, and in some even a little forwards in the orbits, at the same time that they are nearly at right angles to the axes of the eyes; so that one of their uses cannot be to advance the eyes, as has been supposed.

As to their relative size. In the two lower classes of the vertebrata, the two oblique muscles are of nearly equal size, the inferior is, at least, not smaller than the superior, and they are larger than the straight muscles. In birds, while the inferior continues somewhat larger than one of the recti, the superior has undergone a great increase, being, as already stated, two or three times larger or broader than the inferior. Lastly, in mammals, the superior oblique undergoes a further development, at least in length, by its reflection over a pulley, after arising backwards from near the apex of the orbit. Notwithstanding this, it does not seem to be the more powerful of the two oblique muscles, as the inferior appears to contain more muscular substance, from its being broader and fleshy nearer to its insertion, and is besides situated so as to be capable of effecting a more extensive motion, at least of a lateral rotatory kind, than the superior. This is more the case in man than most quadrupeds, as in most of the latter the superior has the advantage of being fleshy beyond the pulley, which is then large and loose, so as not to confine the muscular fibres. The oblique muscles appear, also, to be more developed in most quadrupeds than in man, when compared with the straight muscles and with the size of the eye; but, on the other hand, in man the oblique muscles pass more round the eye, from which they would appear to gain an advantage, as regards turning the eye on its axis. Farther, the lesser development of the oblique muscles, compared with the recti, in mammals, than in the inferior classes, may perhaps be more correctly regarded as due to the increase of the recti in the former, in accordance with the greater frequency and extent of the movements of the eye, more especially in man, and also in other mammalia.

USE OF THE OBLIQUE MUSCLES.

These observations may be regarded as affording just grounds from which to draw conclusions as to the use of the oblique muscles. Whatever minor differences they may present, they are evidently the same muscles, and provided for the same purpose in all vertebrate animals, and no hypothesis can be entertained if it is incompatible with, or not supported by, comparative anatomy.

It has been a common opinion that the oblique muscles advance the eye, and act as antagonists to the recti, so as also to steady the eye during the action of the latter. To this it has been objected, that they exist in animals where the eye cannot be retracted, and that they are not proportionally increased where a special retractor or suspensory muscle exists. To this it may be added, that they exist in man in whom retraction of the eye does not naturally occur as a voluntary movement (as is easily ascertained by experiment, the strongest voluntary effort to do so being fruitless), although, by the conjoint action of all the straight muscles, or of two opposite ones, such a movement would be produced; and in most mammals the course of the oblique muscles in the orbit is not sufficiently backwards to permit of their advancing the eye, which is probably accomplished by the resilience of the soft parts, against which it has been drawn by the action of the suspensory muscle.

It has also been held, that one use, at least of the oblique muscles, is to compress the eye laterally, so as—by rendering the cornea more convex, and by increasing the distance of the lens from the retina—to fit the eye for the vision of near objects. But, as this could be only a secondary use of these muscles, it may be observed, that, although both eyes usually converge equally when near objects are viewed, the effect would be liable to be produced unequally, and at times when it was not required.

Again, it has been supposed that the oblique muscles might act so as to turn the cornea inwards. It is not easy to see how, singly or combined, they could perform a rotatory movement of this nature, as this would require them to be attached considerably before the centre of the eye, and the internal rectus does not appear to stand more in need of assistance than any of its fellows. This opinion, however, has been brought by some to explain why the power of turning the eye inwards may remain after the internal rectus is divided, or believed to be so, in the operation for convergent strabismus; whilst others explain the same circumstance by supposing that the inner fibres of the upper and lower recti can act so as to invert the eye. Even those who believe that the power of turning the eye inwards may not be destroyed by division of the internal rectus, will admit that such is at least not usually the case, from which it may be inferred, that there is no muscle, or combination of muscles, by which inversion of the eye can be naturally performed after the internal rectus is divided; otherwise there is no reason why the power of inversion should not always remain. This I have never yet observed in any case where I felt satisfied that the muscle was wholly divided. When any power of inversion remains, I regard it as indicating the necessity of searching for some part of the muscle which has escaped division, and I have not yet found it necessary to divide any other muscle, in whole or in part, in addition to the internal rectus, for the complete removal of convergent strabismus. The power of inversion returns, though to a lesser extent, after the muscle or tendon has re-adhered to the sclerotica.

The view most commonly entertained is, that each of the oblique muscles performs, or assists in performing, one or more of the diagonal movements of the eye, whereby the direction of its axis is changed. Acting singly, the superior has been variously held to turn the cornea downwards and inwards, and, again, downwards and outwards; and the inferior upwards and outwards, and, again, upwards and inwards. The names of the most eminent of anatomists and physiologists are attached to these opposite views, although the more strongly supported view is, that each performs the latter of the two movements above assigned to it. Assuming the latter view to be correct,—namely, that by the superior the cornea is directed downwards and outwards, and upwards and inwards by the inferior,—Valentin has founded on it an ingenious theory, chiefly intended to give an explanation of the singular arrangement of the motor nerves of the orbit. But, irrespective of a variety of objections of another nature, which might be taken to this theory, it is enough to remark here, that it essentially rests on the ground that the oblique muscles are capable of performing the movements above attributed to them. This, indeed, they might possibly be believed to do in man, but it is at once evident, from a consideration of their comparative anatomy, that they could not perform these movements in animals; in which, notwithstanding, there is the same peculiar arrangement of the motor nerves. Indeed, there is no good reason to believe that any of the muscles of the eye, or their nerves, are more involuntary than the others, all being alike under that influence which maintains the harmony of the ocular movements; which are alike consensual whether the muscles employed be corresponding, or non-corresponding ones, on the opposite sides.

The view that the oblique muscles are provided for the purpose of turning the eye on its antero-posterior axis, is not a new one. It was advocated, in 1759, by Porterfield, and seems, from the remarks in his treatise on the eye, to have been the subject of discussion previous to that time, some having denied the existence of such a movement, as they could neither perceive its utility nor observe its occurrence. It was brought forward in 1786 by John Hunter, in a short paper on the use of the oblique muscles, among his observations on some parts of the animal œconomy, in which he has endeavoured to explain the circumstances under which such a movement is necessary; and the same view is attributed to Dr Jacob, of Dublin, in the last edition of Quain's Anatomy.[1] The view of Hunter is well advocated by Dr G. Johnson, in the article "Orbit" in the Cyclopædia of Anatomy and Physiology; and he adduces in support of it the result of experiments performed by him on a newly killed dog, from which it appeared that the contraction of the oblique muscles caused the eye to roll on its axis in opposite directions, without causing, in the slightest degree, any of the movements by which the direction of the axis is changed.

The physiological uses of such a movement are not at first sight very apparent. In those lateral motions of the head, which take place between atlas and the axis, the eyes, in being kept fixed on an object previously viewed, are moved laterally by the recti muscles; but, as Hunter explains, when it is moved laterally, as from shoulder to shoulder, rotatory motion of the eyes, on their antero-posterior axes, is also required, to prevent the picture from changing its place *circularly* on the retina. Thus, when the head is bent down towards the right shoulder, the right superior and the left inferior oblique will be in action. It is evident, under such circumstances, when the head is twisted round on an axis more or less corresponding to that of the eye or eyes—that the latter must be rotated more or

[1] Since the above appeared in the Journal, I have been favoured with a copy of Dr Jacob's very instructive paper "On Paralytic, Neuralgic, and other Nervous Diseases of the Eye," from the *Dublin Evening Press,* for January 6, 1841, on which I had been unable to lay my hands in Edinburgh. In support of his opinion that the function of the oblique muscles is to turn the eye on its antero-posterior axis, Dr Jacob relates cases in which the action of the superior oblique was rendered apparent, from the muscles supplied by the third and sixth nerves being paralysed. It became evident when the patients were directed to look downwards, the motion being a delicate rotatory one, the eye being distinctly twisted on its axis. Dr Jacob also takes the same view as to the purely rotatory nature of the various movements of the eye.

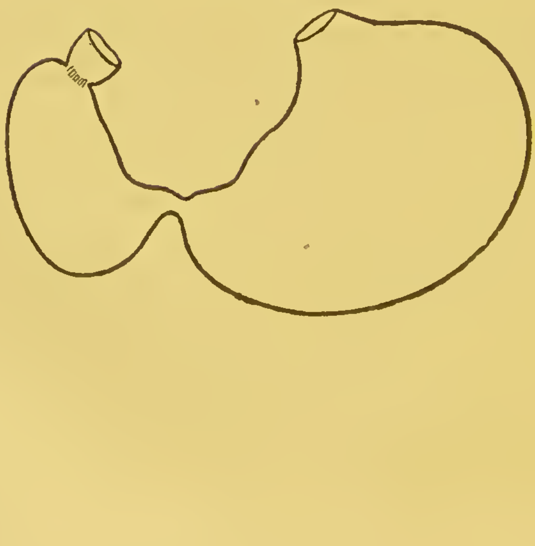
less directly on their axes, and in a contrary direction, otherwise the picture of the object would change its place circularly on the retina. This motion will be more or less combined with those performed by the straight muscles, according as the axis of motion of the head corresponds more or less to that of the eyes; and it may be that the oblique muscles, from their not exactly embracing the middle of the eye transversely, are intended also, and at the same time, to turn the axis of the eye, so as to change, to a certain extent, its direction. It would appear, therefore,—as it is essential to the correct viewing of an object when the head is being moved, that the picture fall in all respects on the same part of the retina,—that a more or less direct movement of rotation on the antero-posterior axis is necessary; and it may be added, that the nature of this movement—as it causes little or no change in the direction of the eye—together with the circumstances under which it takes place, are such as not to render its occurrence evident to an observer. The circumstances under which this movement would be required in those animals in which the eyes are placed in the side of the head, and look outwards, are different; as, in the simple movement of raising or depressing the head, the latter moves more on an axis corresponding to that of the eyes, so as to render a more direct lateral rotatory motion necessary, in order to prevent the image from changing its place circularly on the retina. In these animals, it is doubtful if the eyes, which cannot be directed together to the same object, are moved simultaneously; but if so, the corresponding oblique muscles will be employed at the same time on opposite sides, whereas, in the circumstances explained by Hunter, the superior oblique of the one side would be in action along with the inferior of the other.

These, however, are so far only theoretical considerations, and do not affect the conclusion to which comparative anatomy points, that the oblique muscles—whatever minor differences may obtain as to their arrangement in different vertebrate animals—are, in all, not only capable of performing this movement in opposite directions, but are evidently intended for doing so.

It would thus appear, that, while comparative anatomy is irreconcilable with all the other theories above alluded to, it affords strong confirmation of the view that *the oblique muscles are specially provided for turning the eye, more or less directly, on its antero-posterior axis.*

Fig. 1.

Fig. 2.



III.

TWO CASES OF DOUBLE STOMACH IN MAN;

WITH OBSERVATIONS ON THAT CONDITION.

FROM THE MONTHLY JOURNAL OF MEDICAL SCIENCE, FEBRUARY 1851.

On opening the abdominal cavity of a middle-aged female subject, at lecture during the winter session 1847–48, my attention was attracted by an unusual appearance of the stomach. It seemed contracted in the middle, and on distension it became evident that the stomach consisted of two sacs, connected together by a narrow communication. The woodcut, Fig. 1, will convey a better idea of its form than could be done by detailed description.

The communicating passage is just large enough to allow the forefinger to pass with ease, and the length of the constricted portion is about half an inch. On turning the stomach inside out, the mucous membrane is seen to be smoothly continuous from one cavity to the other; and, whether in the collapsed or distended state, there is no appearance of a valvular fold or thickening of the coats in the contracted part, or where it opens into either sac. The left division is fully one-third more capacious than the other, and forms a large cul-de-sac projecting in front of the œsophagus, which enters towards the upper and back part. In the sketch, which presents an anterior view, the left division is represented as twisted forwards, so as to show the place of entrance of the œsophagus, the lower part of which is naturally concealed by the prominence there appearing to lie to its left side; and this rounded prominence seems naturally to have been received into the concavity of the lesser curvature of the pyloric sac. The right or pyloric division corresponds very much to the narrow elongated form of stomach which we sometimes see, and is not much less than some adult entire stomachs of that form. The two sacs are together much more capacious than any large single stomach that I have seen. In the sketch, a small part of the duodenum is represented beyond the pylorus, the valve of which is fully formed. About three inches to the cardiac side of the pylorus will be observed a slight constriction, especially marked on the greater curvature. This corresponds to the slight constriction which usually, or at least frequently, exists at this part of the stomach.

Instead of agreeing with me in regarding this case as one of true or congenital bilocular stomach, some may suppose that this state of parts may possibly have resulted from morbid action. On the one hand, that the second sac was merely a dilated portion of the duodenum, due to a partial constriction beyond, whilst the constricted part between the two sacs corresponded to or was the original pylorus. Or again, that the constriction was of the nature of a morbid contraction or stricture on an originally single sac. These suppositions, however, are incompatible with the facts of the existence at the right extremity of the second sac of neither more nor less than the natural pyloric thickening and projection, and the entire absence of thickening or puckering at or near the narrow part between the two sacs, as well as the absence of the least trace of old or recent disease on any part or surface of the preparation.

Death in this case had resulted from typhus fever, and I was unable to obtain any previous history.

In my museum there is a dried preparation showing a similar peculiarity in the form of the human stomach, of which Fig. 2 is a sketch:—

This preparation originally belonged to Dr Knox. For the only information I can furnish regarding it I am indebted to Dr Lonsdale of Carlisle, formerly assistant and successor to Dr

Knox, who writes to me that it was simply a dissecting-room preparation, without, so far as he is aware, any previous history.

Here, but for the constricted portion, there is less departure from the usual form of the stomach than in the former case, and in capacity the two sacs do not exceed a good sized simple stomach. The pyloric division is in this case proportionally smaller than that of the one first described, and terminates more abruptly at the pylorus, which is seen as a well-formed nearly circular projection in the interior. On cutting into the sacs on either side of this constriction, there is no appearance of thickening or valvular projection. The orifices are smooth, and now scarcely of sufficient size to receive the little finger. On the posterior surface of the cardiac division, between two and three inches to the left of the constricted part, there is an irregular patch of thickening larger than a shilling, as if the wall had been thickened and adherent at this part. Although this thickening appears to be of pathological origin, it has no connection with the constricted part, which appears to be quite natural or healthy, as in the case just recorded.

The occurrence of a contraction about the middle of the stomach has been noticed by various anatomists. Morgagni,[1] in relating the post-mortem examination of a lady who had for many years been troubled with frequent vomiting, states that "the stomach was contracted, and near the antrum pylori the contraction was so much greater than in other parts, that the viscus appeared as if it had been divided into two cavities." After ascribing the symptoms in this case to other conditions, he goes on to say, "I might have supposed that the contracted state of the stomach had some influence in producing this affection had I not observed the same structure in other women, who had not been subject to vomiting. Similar contractions, too, have been discovered by other anatomists in persons of both sexes; and sometimes the aperture of communication between the two portions of the stomach was exceedingly small. Doubtless in the preceding case the contraction was congenital."

[1] The Seats and Causes of Diseases investigated by Anatomy, 1779. Edition by W. Cooke. London, 1822.

Morgagni again refers in the same work to this condition, as having been noticed in several of his dissections, and observes that "the aperture of communication between the two portions of the stomach was scarcely larger than the natural extent of the pylorus." On this the editor remarks, in a note, "central contraction of the stomach is often met with; but, in general, it appears to be a temporary affection. In all the instances which have occurred to me, it was instantly removed by inflation. Sometimes, however, it is permanent; and the aperture between the two portions has been found by others as small as Morgagni has represented it above. Sir E. Home and Dr Monro have described this morbid appearance. In the Medico-Chirurgical Journal for January 1816, Dr Palmer has related a case in which the contraction was as perfect as if a tight ligature had been applied, and great difficulty was found in passing the little finger through the narrow ring which separated the cardiac and pyloric portions. The inner coat seemed healthy, except at the strictured part where the coats were thickened; and adjacent to it there were some spots resembling minute granulations."

Dr Monro[1] states that a contraction in the middle of the stomach has been described by Blasius, Lorry, Walter, Hufeland, and Van der Kolk, and he also notices the cases described by Morgagni. The constrictions here referred to by Dr Monro appear to have been chiefly spasmodic, and to have disappeared soon after death or on distension, since he refers, as a contrast, to one case of permanent contraction in a preparation before him, but the constriction was not greater than to give the appearance as if a cord had been "tied very slightly around the stomach."

Sir E. Home has paid particular attention to this subject, having written several papers on the structure and form of the stomach in various animals and in man. In one of these[2] he expresses the opinion that the human stomach is divided by a muscular contraction into a cardiac and pyloric portion; that

[1] Morbid Anatomy of the Human Gullet, Stomach, and Intestines. 1811.
[2] Philosophical Transactions of the Royal Society of London, 1807, pp. 170, 171.

this contraction is the result of the natural action of the organ in digestion, and disappears soon after death; so that unless the body be examined within twenty-four hours the contraction will not be seen. In connection with this he gives a representation (Plate XI.) of the figure of the stomach in this condition, in which the contraction is moderate and gradual, leaving between it and the pylorus about a fifth part of the length of the organ. In a subsequent paper[1] the same author relates a case in which, by a morbid contraction near its middle, the stomach was as completely divided as in the two cases I have related. In the drawing illustrating this case, the constricted portion has not a diseased appearance, but the contraction is described as morbid, and it is supposed that the morbid action had ensued in the part at which the physiological contractions had formerly taken place.

In describing the form and curvatures of the stomach, E. Huschke observes:[2] "Home admits, as in the stomach of the rodentia, a contraction towards the pylorus; this constriction does not always exist, and when it is present, distension with air sometimes removes and sometimes leaves it. In the first case, it arises from spasm of the muscular coat which existed during life; in the second, it is due to a contraction of all the coats of the viscus."

M. Cruveilhier remarks on this subject:[3]—"The examples of double or triple stomachs in the human subject are merely cases of single stomachs having one or more circular constrictions. The essential character of a double stomach is not an accidental or even a congenital contraction, but a difference in structure. Bilocular stomachs, indeed, are very common; but this form (resembling that of some kind of calabash-gourds), though sometimes extremely well-marked when the stomach is empty, disappears almost when it is much distended by inflation."

From the consideration of these various statements and opinions regarding constricted or double stomach, it appears that the condition chiefly referred to is that of a moderate constriction, which

[1] Philosophical Transactions of the Royal Society of London, 1817, pp. 350, 351.
[2] Encyclopédie Anatomique, 1845, tome v., Splanchnologie, p. 46.
[3] Descriptive Anatomy, 1841, vol. i. p. 467.

disappears soon after death or on distension. In a few of the cases, however, the constriction has evidently been permanent, though the want of precision in the descriptions leaves it doubtful whether the constriction was moderate or deep. In Sir E. Home's case the constriction was deep, but was regarded by him as morbid. In Dr Palmer's case, the communicating passage was very narrow, but there seems to have been thickening of the coats, and ulceration of the lining membrane at this part. It may therefore be concluded that there have been very few if indeed any instances placed on record in which the stomach was so deeply and permanently divided, independent of disease, as in the two cases I have had the opportunity of recording.

An analogy may be drawn between this condition of the human stomach, and that more especially of some rodent animals; but only in so far that there are two cavities separated by a constriction, varying in position and extent in different genera; in other respects, as regards form, position of orifices, and structure of the lining membrane, there is no correspondence. In regard to the latter point, I speak only from the appearances presented to the naked eye, the specimen in the case first related having been too old for miscroscopic examination; but the lining membrane of both cavities presents the same appearance, and the thickness of the walls was the same in both and at the constriction. M. Cruveilhier would regard a difference in structure as essential to constitute the case one of "double stomach." But,—whilst in animals in which the stomach consists of more than one cavity, there is a difference in structure in accordance with their habits and the stages of their digestive process,—I should rather expect, in the case of a division of the human stomach into two parts, not to find any difference in their lining membrane, greater at least than that which normally obtains in the microscopic structure of the cardiac and pyloric portions.

Independent of the condition of true or permanent constriction, the preceding notice of the history of this subject brings out prominently the circumstance of the occasional or frequent existence of a *temporary constriction*, which is worthy of notice in connection with the physiology of the stomach.

Sir E. Home is of opinion that this is the natural condition of the organ during digestion, the constriction serving the purpose of retaining the food in the cardiac division until it be prepared for farther change in the pyloric. He admits, however, that the circular muscular fibres are in no way more developed at any particular part, which we would rather expect to find were this a regularly recurring condition. In support of this view he states that he has seen a similar temporary constriction on the stomachs of some animals in which the organ is naturally simple, when the examination was made soon after death. In connection with this may be mentioned the observation made in regard to contraction of the stomach in the well-known case of St Martin. It is described as contracted about four inches from the pylorus, so that a tube introduced by the fistulous aperture was first resisted at this part, and, having passed the contraction, was then grasped and drawn on by the action of the parietes beyond. From the description given, however, it does not appear whether there was really an hour-glass contraction, as supposed, at this part, or whether the whole of the pyloric portion was not also in a contracted condition.

The question as to whether the stomach is naturally and periodically constricted so as partially to divide it into two cavities during digestion, is worthy of being inquired into. In doing so, it must not be forgotten that the constriction will not be apparent under all circumstances;—that it will be noticed especially when death has taken place during or soon after digestion;—that too rapid or forcible distension may at once destroy it;—and that it cannot be noticed unless the body be examined soon after death.

[Note.—*March* 1854.—Since the above paper was written, I have met with a considerable number of instances of *temporary constriction*, and I add this note chiefly to correct the natural supposition that it will disappear soon after death. In all the cases I have seen, the contraction was to the right or pyloric side of the middle of the stomach, usually about four or five inches from the pylorus. I have made notes of a few of the cases. In one, the stomach was examined the day after death. The contraction was considerable, and resisted moderate distension with air, although tried several times for several days after death, when on being forcibly distended with a large blow-pipe it yielded, and there is now no mark of constriction on this stomach.

In other two, the abdomen was not opened till nearly a fortnight after death, and the constrictions were well marked; in one, so much, that it at first resembled the pylorus, the portion of stomach beyond being intestiniform, and moderate distension did not remove the contraction. In another instance, which occurred during the present session, there was a considerable contraction in the same situation, which still remained strongly marked although forcible distension was made, and this appeared to be the true form of the organ.

But, in most of the cases I have seen, the contraction entirely disappeared when the stomach was distended with the blow-pipe. The contraction is seen on the empty stomach, still better when it is moderately and gently filled with air, and then yields when forcible distension is made. In all the cases it was not a sharp constriction, but a gradual contraction. Although, at the same time, especially in some of the instances, deeply marked.

Although it may be correct to say, that this temporary and partial division of the stomach is frequently met with, I have no hesitation in saying that, whether in bodies examined early or late after death, it is the exception and not the rule. It appears reasonable to suppose that, the contraction being originally muscular, its existence will be influenced by the length of time between the last digestion and death. The occurrence of death during or soon after digestion, would account for the occasional presence of the contraction, death having overtaken the stomach in this condition; whilst the more frequent opposite circumstances of death would correspond to its usual absence. It is possible that this may be the natural condition of the stomach during digestion, but facts are yet wanting to entitle us to draw the inference that it is very probably so, and we also recur to the fact, that there is no special development of the circular fibres at this or any part, until we come to the pylorus.]

IV.

ON THE ANATOMY AND PHYSIOLOGY OF THE NERVES OF THE ORBIT.

FROM THE MONTHLY JOURNAL OF MEDICAL SCIENCE, 1852.

ALTHOUGH it may be said that we are now pretty intimately acquainted with the functions of these nerves, there are, notwithstanding, points about each class which are either not fully understood, or not generally agreed upon. Such are,—the relation between the motor nerves at their origin, and the meaning of their singular distribution among the muscles of the orbit as connected with the movements of the eyeballs,—the relation which the fifth pair bears to the motor nerves and the muscles of the eye, and the influence which it exerts over the parts subservient to the function of the proper nerve of vision,—the nature of certain supposed varieties in the connections of the ciliary ganglion,—and also the remarkable influence which the sympathetic in the neck appears to exert on the eyeball and on the actions of the iris. In now proceeding with this inquiry, I shall bring to bear upon it the results of a large number of dissections I have made with a special view to the elucidation of the above points; which, together with the careful consideration I have given to what has been written on the subject, may enable me to speak with some confidence when considering the views of others.

PART I.—ON THE MOTOR NERVES.

The questions I propose to consider concerning the motor nerves are—1. Their cerebral connection; 2. Their relative distribution, as connected with the movements of the eyeballs; and, 3. Their connections, in the cavernous sinus and orbit, with the fifth or common sensory nerve, and with the sympathetic.

ORIGINS OF THE MOTOR NERVES.

Third, Fourth, and Sixth Nerves.—In looking to the origins of these three motor oculi nerves, we are struck, in the first place, with the fact, that they are connected with very different parts of the brain. But some anatomists hold this to be only apparent, the true origins being deeper, and such as to establish a relation between them. I shall now examine briefly into the opinions of different anatomists on this point.

The FOURTH nerve is usually said to arise from the upper part of the valve of Vieussens. Valentin[1] describes it as arising partly from the testis, and partly from the valve of Vieussens. Cruveilhier[2] says,—by one or several roots below the corpora quadrigemina, on each side of the valve of Vieussens; and, after alluding to the supposition that some of the fibres come from the cerebellum, and that others arise much deeper than the apparent origin, he adds, "all that can be seen is, that these nerves arise from the valve of Vieussens; and he farther states, that, "the nerves of the two sides are often united by some white streaks which form a transverse commissure." This nerve is believed by some to be derived from a tract of fibres which may be traced up from the olivary body of the medulla oblongata, to below the corpora quadrigemina. Dr John Reid[3] describes this tract very clearly as a motor one, and assigns this as the

[1] Encyclopédie Anatomique. Névrologie. Tome iv. p. 257. G. Valentin, 1843.
[2] Descriptive Anatomy. 1841. Vol. ii. p. 1099.
[3] Physiological, Anatomical, and Pathological Researches. 1841. P. 305.

source of the fourth nerve. Tracing this tract upwards, he finds it to be connected with the anterior or motor roots of the upper cervical nerves, with the ninth, the sixth, the portio dura of the seventh, and the motor portion of the fifth, and, lastly, "the trochleator nerve is attached to the internal margin of the same band of fibres when it has ascended the processus a cerebello ad testes, and is about to enter the optic lobes." The same view as to the source of the fourth nerve is taken by Longet.[1] He traces part of the antero-lateral column of the cord up as a motor tract, to beneath the corpora quadrigemina, and assigns this as the source of the fourth nerve just before the tract enters beneath the testes; and he adds, "this nerve then, like the other motor oculi nerves, has an origin corresponding to its uses." Mr Solly, in his last edition,[2] admits the correctness of Dr Reid's description of the olivary tract, and the apparent connection of the fourth nerve with it. He adds (p. 294), "The fourth pair of nerves at their origin are connected together by a distinct commissure, more evident in some brains than in others." As to this supposed source of the fourth nerve, I shall only observe at present, that however clearly the olivary tract may have been traced up to the neighbourhood of its origin, yet no direct continuity has, apparently, been observed between the tract and the roots of the nerve.

Owing to the difficulty of clearing away all the pia mater without tearing off the nerve, the examination of the origin of the fourth nerve is a matter of some delicacy. It presents various appearances. Sometimes it seems in part to come out as if deeply from the testis or posterior quadrigeminal body. Sometimes it appears more to plunge down through either the middle or internal part of the processus a cerebello, a little way behind the testis; and generally, if not always more or less, it appears to pass across the upper part of the valve of Vieussens, and, joining with its fellow of the opposite side, gives rise to the appearance of a commissure. This apparent commissure has not been generally described in anatomical works, but has been noticed, in the

1 Anatomie et Physiologie du Système Nerveux. 1842. Tome ii. p. 392.
2 The Human Brain. 1847.

terms already quoted, by M. Cruveilhier and Mr Solly. It is likewise mentioned in Quain's Anatomy[1] that "the roots of the nerves of opposite sides are connected together across the middle line, in the form of a white band or commissure in the substance of the velum." This is a point in the anatomy of the fourth nerve which, I believe, deserves more notice than it has hitherto received. My attention was drawn to it first in 1844 from the distinctness with which I found it in some animals. It is seen usually as a distinct white band between the fourth nerves, crossing the valve of Vieussens close behind the testes, but still distinct from them. This connection seems to have given rise to the common method of assigning the valve as the origin of the fourth nerve, which seems to me, strictly speaking, not to be correct. But it is necessary first to understand the nature of this so-called *valve of Vieussens*. This thin velum is formed primarily and below of the lining membrane of the fourth ventricle, the fore part of the roof of which it forms. In some animals it is seen to consist of nothing else until we come to the commissure of the fourth nerves, but in the human brain there is, as it were, spread on the serous membrane, a thin layer of white nervous matter, overlapped behind by the grey matter of some of the small median laminæ of the cerebellum. The fibres are seen to be arranged longitudinally, and it tears in this direction when it has been hardened in alcohol. These fibres, then, have the same direction as those of the superior peduncles of the cerebellum, the space between which they fill up. Now, over and in front of these are added transverse fibres, gathered into a white flattened bundle, from which the fourth nerves appear more or less to arise, and forming what we shall call *the commissure of the fourth pair of nerves*. I have notes of the appearance of this commissure, from examinations made at the time above referred to, in the sheep, calf, and horse, and have seen it, I think I am correct in saying, in every brain I have since examined, whether human or among the larger of the lower animals. In the sheep, the testes, which are apart about a quarter of an inch, are joined by a white commissure, and behind this, and separated from it by a clear space,

[1] Fifth Edition. 1846. P. 747.

is the commissure, a line in breadth and two or three lines across, into which the fourth nerve runs on each side; and behind this the valve is again only clear serous membrane. Notwithstanding, the fourth nerve appears to arise also in part as if from the testis, and part of this commissure seems to be merely a crossing from one peduncle to the other, the whole seeming too large to pass entirely into the fourth nerve, although this may be due to its being flattened out upon the valve. On examining these parts recently in the cat, the commissure was seen to be very distinct. The fourth nerves adhere intimately to the lower and back part of the testes, and then run across into the commissure, which may be compared to the optic commissure in form, only the nerves here are more divergent in front. Two bands run into the centre of it behind, one from each peduncle; there then appears to be a union and crossing, and from this, at least the greater part of the fourth nerves passes off on each side. I have just examined these parts in the human brain in two recent specimens, hardened and prepared on purpose. In one, the fourth nerve comes by one filament from the upper part of the superior cerebellar peduncle; another comes out of the base of the testis, running backwards and outwards to join the others; and a third, the smallest of the three, evidently comes out of the well-marked commissure. The commissure is a line in breadth, the same distance behind the testes, and three lines across. It connects the upper end of the peduncles, the internal fibres of which seem to run into it, and it seems to contain more fibres than run out from it into the fourth nerves. The longitudinal arrangement of the medullary fibres of the valve is very distinct. They appear not to run into the fourth nerves, or into the commissure, but pass underneath the latter, and, collected together, mount up along the groove between the testes, and spread out on these and on the nates. On turning round the preparation, the commissure is scarcely visible, owing to the fibres of the valve passing underneath it as above noticed. In the other preparation, the longitudinal fibres of the valve partly pass beneath the commissure, but chiefly turn to each side behind the commissure, cross over the peduncle, just behind the fourth nerve, as a distinct band,

and disappear beneath the great crus cerebelli, so as to enter deeply the back part of the pons. In this preparation the largest root of the fourth nerve comes out of the commissure. Now, in neither of these do the fibres of the valve appear to run into the fourth nerves or their commissure, but the latter seems to be derived entirely from the upper part of the peduncle.

These observations appear sufficient to show that the fourth nerve is in part derived from the opposite side, and this nerve may, therefore, be regarded as composed of a decussated and a non-decussated portion. I have no theory to connect with this commissure, but dwell upon it merely as a point in anatomy which does not seem previously to have attracted much notice, or to have been understood in the light of a distinct decussation; and this, it may be observed, still leaves open the question as to the real origin or source of the fibres of the fourth pair of nerves.

The THIRD nerve appears at the inner and back part of the crus cerebri, at about a quarter of an inch in front of the deeper part of the pons. Very various accounts are given of its deeper connections. Longet[1] describes the fibres as diverging in the crus, to enter the locus niger, and be continuous with its lower fibres continued on from the antero-lateral column of the cord. He disputes the view of Zinn, that some of fibres may be traced to the anterior commissure, and also the older view of Varolius, Vicq-d'Azyr, and Vieussens, that the third pair of nerves become united together, a supposed union to which they attributed the simultaneous action of corresponding branches of this nerve. Cruveilhier[2] traces the fibres through the inner part of the crus, diverging as far as to the pons, not farther; but he has not found any of it to come from the locus niger of the crus. Valentin[3] give a very minute account of the origin of this nerve. He traces the fibres deeply, radiating to the nates, the superior peduncles of the cerebellum a little below the valve of Vieussens, the deep and superficial longitudinal fibres of the pons, the crus cerebri, and especially the locus niger of the latter. In one

[1] Op. cit. Tome ii. p. 378. [2] Op. cit. P. 1098. [3] Op. cit. P. 257.

instance the fibres towards the cerebellar peduncles could be traced, he states, into the interior of the cerebellum.

Farther, Valentin, with characteristic minuteness, describes (p. 278) an arrangement of the fasciculi, as if this nerve consisted of two distinct roots or parts where it issues from the crus,—an internal portion ("pars interna"), formed by anterior as well as posterior fasciculi, and an external portion ("pars externa"), from the inferior and internal parts of the crus, which converges on the outside of the other. The latter description appears to be correct enough, but still without authorising the inference that there are two roots physiologically distinct. The external portion arises only a very little higher up, as the brain lies for dissection, and, as it were, naturally so, from the manner in which the nerve comes out, and then abruptly winds round the inside of the crus. On carefully separating the fasciculi of each portion, I found, in one brain, the outer portion to consist of eight or nine fasciculi, and the internal portion of nineteen. In another, the outer portion contained fifteen fasciculi, and the inner portion eighteen or nineteen. The smaller part lies at first between the greater portion and the crus, but the fasciculi of the two portions very soon unite and interlace. In fact there is no arrangement such as would have suggested to me that there were to this nerve two distinct roots, worthy of being so named and described; and I have noticed it only as Valentin has connected it with his view, founded on the result of his experiments on rabbits, that the third nerve is sensory as well as motor, from its origin.

Of the deeper connections of the third nerve, Swan[1] observes, that "its origin may be traced in the direction towards that of the superior oblique oculo-muscular nerve." Mr Solly[2] again states, that "it will be found not merely to be connected with the surface of the crus cerebri, but dipping beneath it, and there dividing into two portions;—one of these ascends through the pons Varolii to be connected with the motor tract in its passage through that commissure; the other passes through the locus

[1] A Demonstration of the Nerves of the Human Body. 1830. P. 19.
[2] Op. cit., p. 293.

niger, and splits in five or six white threads, which, separated by the grey neurine, present a beautiful appearance in a fresh brain." He then traces these filaments through the grey matter to be connected (so far as I can understand, after comparing pages 234, 264, and 294) with some fibres, which pass downwards and forwards to the crus cerebri, from the deeper part of the superior peduncle of the cerebellum. Mr Solly seems to regard the third nerve as intimately related to the grey matter of the crus, as, although he also here speaks of the distant radiation of its fibres through and beyond it, he remarks (p. 234), in speaking of this grey matter,—"This has been called the locus niger; I would rather designate it the ganglion of the third pair of nerves."

The origin of the SIXTH nerve is, as usually given, from the upper end of the anterior pyramid of the medulla oblongata, or from the groove or the pons immediately in front of this, or from both; usually by a couple of roots. The more modern anatomists have failed to trace this nerve to a deeper origin. It is usually regarded as satisfactory that it is seen to emerge from a connection with what is regarded as a motor tract.

The preceding references and observations will, I think, make it evident that we really know very little about the deep connections or true origins of the three motor nerves of the eye; and it may likewise be doubted whether there is really anything either of truth or value in the view that these nerves are connected or related to each other by their deeper attachments. It has perhaps been too customary to regard tracts or columns, from which certain nerves issue, as necessarily having the same function as these nerves, and as in reality sending out the fibres which compose them; but now that the important physiological distinction between the grey and the white nervous matters is better understood, it may appear less bold to question the correctness of these inferences. The various tracts of white matter which compose the bulk of the medulla oblongata may be regarded as in great part simply passing through it, from the grey matter of various parts of the cerebrum and cerebellum above, to various parts of the grey matter of the cord below, or *vice versa*—commissural in their nature, and not necessarily contributing any of the fibres which compose the various nerves, cerebral or spinal. And again, the various nerves which seem to arise from these tracts in their course may be regarded as merely coming out through these tracts from certain points of grey matter. We cannot be fully satisfied with tracing a nerve to, or into, a tract—the tract itself must have an origin; but, although the association carries a certain weight with it, we must be able to follow the nerve down or into its grey matter, until we can say, physiologically speaking, that we have done with it and got it to its real origin. Thus, we are not contented with tracing the vagus nerve to the tract which occupies the side of the medulla, between the olivary and restiform bodies, but we follow it down through this to the grey matter on the back of the medulla, with which it is known to be connected.

Now, even granting that the anterior pyramidal and olivary columns together form the great motor tract of the medulla, and that the motor nerves of the eye can be traced towards the upward continuations of this column, it does not therefore follow that we have ascertained the true origins of these nerves. Nor, even supposing the view just questioned to be correct, does it follow that the tracing of the three motor oculi nerves towards these tracts thereby establishes a special relation between them, or entitles us to conclude that, however distant their apparent origin, their true origins are the same; for the same line of argument would apply also to the other motor cerebral nerves. If the third is connected with the same column as the sixth, so also is the ninth or motor linguæ; and if the fourth comes out from the olivary tract, so also do the portio-dura of the seventh pair, and the motor portion of the fifth,—the motor nerves of expression and mastication.

Of the origins of the fourth and sixth nerves, then, we know nothing except the places of their apparent origin; and of the fourth also, that part of its fibres are derived from the opposite side. Beyond this, we can only suggest the probability that their fibres are connected with the grey matter in the neighbourhood of their apparent origin. The third nerve can be traced a little deeper than its apparent origin; but anatomists are not agreed as to the extent or exact direction of the deeper radiating fibres, and we cannot say where these radiating fibres really begin. It may be doubted that they can be traced farther backwards than the crus cerebri; and when they are spoken of as going nearly as far as the quadrigeminal bodies, it must be recollected that these bodies lie on the upper and back part of the crus itself, the nates being directly above the origin of the third nerve, and at a distance only of from half to three quarters of an inch. Of the third nerve, then, we can only say, that it arises from the crus cerebri, the fibres being probably derived partly from the grey matter collected in the locus niger, and partly from some more deeply-seated grey matter in the crus.

The endeavour to trace the motor nerves of the eye to the same cerebral connection, must be therefore held to have failed; and I cannot therefore agree on this point with Mr Solly, who seems to hold that especially the third and sixth nerves have some intimate relation to each other. Of the latter,[1] he observes, —"This nerve, let it be remembered, in a physiological point of view, is merely a portion of the third, its separation from which by the pons Varolii is perfectly analogous to the separation of the roots of the spinal nerves by a blood-vessel running between them, and of no greater physiological importance." And again (p. 285), he suggests that, physiologically, it would be more correct "to consider the third pair and the sixth as merely separate roots of the same nerve, and to describe the two together by the name of the common oculo-muscular: for the circumstance of the commissure of the cerebellum separating their roots is merely accidental to their arrangement, in a physiological point of view; but the fact of their being described as if they were distinct nerves, has frequently led the student to believe that they must be endowed with distinct offices, and wonder why the abductor muscle of the eye should be supplied by a peculiar nerve, while the other muscles, with the exception of the superior oblique, receive their supply from the same source."

Should it be deemed necessary farther to show the incorrect-

[1] Op. cit., p. 296.

ness of the inference from such methods of description, it may be observed that, in the lower animals, these nerves are always distinct, and have the same peculiar and distant connections with the brain. In the mammalia, in which the pons Varolii is much smaller than in man, the sixth and third nerves still keep their distance from each other, not by any means coming nearer and nearer as the pons is less and less developed; and in birds, in which there is no pons, these nerves are still quite distinct, and do not arise proportionally at less distances from each other than they do in the human brain.

We therefore retain the observation that it is very remarkable that the motor nerves of the eye should be connected with so widely different parts of the brain; and further, that while the common motor oculi nerve arises as near as possible to its distribution, the other two nerves, which supply each a separate muscle, come from very different and distant parts.

DISTRIBUTION OF THE MOTOR NERVES, AND MOVEMENTS OF THE EYEBALLS.

That there is a meaning in the peculiar distribution of these nerves among the muscles of the orbit, cannot be doubted. It is singular and constant, and is found to be the same in all vertebrate animals. This is stated by writers on comparative anatomy, and I have always found it so. The fourth nerve is never distributed to any muscle but the superior oblique, and to this muscle it always goes, although the muscle has a very different position in the three lower classes of vertebrate animals. But the sixth nerve in most mammals, and in birds, supplies additional muscles; in the latter, the muscles which move the third eyelid, and in the former, the retractor or suspensory muscle of the eye. When this muscle is single, a single division of the sixth nerve enters it; when it consists of four separate slips, intermediate to the recti, two or three delicate filaments leave the sixth to supply them. It is worthy of remark, that the part of the nerve which goes to the retractor muscle is less than a third of the whole trunk, whilst the bulk of the muscle is usually much greater than that of the external rectus.[1]

In inquiring into the function of these nerves in connection with the motions of the eyeballs, it is necessary in the first place that we determine upon the action of the muscles which they supply.

About the actions of the recti muscles there can be no doubt or obscurity, providing it be granted, first, that each turns the eye in its own direction, and, secondly, what is equally evident, that two neighbouring recti turn the eye diagonally between them; and that thus all the diagonal as well as the direct movements are performed; a view which would never have been doubted but for the apparent necessity of discovering some use for the oblique muscles. And as to the oblique muscles, I think, from what is stated in my paper on this subject, it is impossible to avoid the conclusion,—in accordance with a view which is older even than the time of John Hunter, who specially supported it, notwithstanding which it had again fallen into disrepute,—that the use of these two muscles is to perform *lateral rotation, i. e.*, to roll the eye on its antero-posterior axis. This becomes still more evident when we descend below the mammalia, when the superior oblique is seen to be no longer reflected over a pulley, but lies at the fore part of the orbit, simply as the counterpart of the inferior; and it is at once evident, not only that these two transverse muscles pull the eye round about on its axis, but also that they cannot act so as to change the direction of the axis, so as to assist or partly supplant the recti in the manner in which they are commonly supposed to do so in man. It is likewise, I think, made evident that such a motion of the eye, as Hunter supposed, is necessary, during the side inclination of the head, to prevent the impression of the object changing its place *circularly* on the retina. It is possible that, from the slight variations they present, in their point of attachment and in their direction to the axis of the eye, in different animals, they may associate their rotatory movement with a slight change in the direction of the axis. As to this I cannot speak positively, but I

[1] See note, p. 60.

hold that, whatever minor actions they may or may not have, the action of these muscles is to rotate the eye on its axis; that, in short, this is the purpose for which they are provided. I arrived at this conclusion chiefly from the consideration of their comparative anatomy, and since then, I find additional evidence of the correctness of this view from the writings of Jacob and Longet, without reference to comparative anatomy, which are deserving of notice here also from their bearing on the subject of paralysis of the motor nerves of the eye. Dr Jacob, in a very instructive paper on the muscles and nerves of the eye,[1] observes, "The two oblique muscles, not running from the inside of the orbit directly outward, but inclining backward, until they become attached to the posterior part of the eye, not only cause the sphere to revolve on its longitudinal axis, but probably change the direction of that axis; the inferior directing the cornea perhaps a little upward and inward, and the superior a little downward and outward. I have, however, great doubts as to the actual production of this effect. That the inferior oblique causes the eye-ball to revolve round its longitudinal axis, may fairly be inferred from its attachments, but that it alters the direction of that axis in any considerable degree, cannot be so easily admitted. We cannot observe the action of this muscle distinct from that of the others, but we can that of the superior oblique. In cases of paralysis of the three straight muscles, the inferior oblique, and the elevator of the upper lid, from disease affecting the third pair of nerves, the action of the abductor supplied by the sixth nerve, and of the superior oblique supplied by the fourth, remains unimpaired. The patient, unable to raise, depress, or turn in the eye, or elevate the upper lid, turns it out as effectually as ever, when directed to do so; and when directed to look downward, the action of the superior oblique is clearly distinguished. It is a delicate rotatory motion, with perhaps a very slight inclination downward and outward. Of this I have now no doubt, having repeatedly within the last two years observed it, and pointed it out to the students." And, again, in connection with a

[1] On Paralytic, Neuralgic, and other Nervous Diseases of the Eye. By Arthur Jacob, M.D. From the Dublin Medical Press. January 6, 1841. P. 8.

case of complete paralysis of the muscles supplied by the third nerve, he observes,—" On directing her to look down, the eye is distinctly twisted from the nose toward the temple without any visible direction of the pupil downwards. It is a mere rotatory motion." Longet[1] refers to several writers as having held this view regarding the action of the oblique muscles:—MM. J. Guerin, 1840; Huech, 1841; Helie, 1841; and more especially to M. Szokalski, who has written a memoir on the subject.[2] The nature of the motion seems to be very clearly apprehended by them, and they appear to see the necessity for this motion when the head is inclined from one shoulder to the other, the eyes being kept fixed on some object. Longet also refers to M. Berard's view that, in this case, the superior oblique of one side will act at the same time as the inferior of the other side; and adds that " it does not seem to me to be possible to perform voluntarily the rotatory motion of the eye, of which we spoke above, when the head is fixed."

In connection with the point now under consideration, I shall next refer to the subject of *paralysis of the fourth nerve*, or superior oblique muscle. For doing so here I need offer no apology, as the subject is one regarding which very little is known. Those who look for the symptoms of paralysis of this nerve to accord with Sir Charles Bell's now abandoned theory that the muscle is concerned in an involuntary upward motion of the eye, must necessarily be mistaken ; but more generally paralysis of the fourth nerve is passed over as a subject of which nothing is known, whilst the symptoms of paralysis of the third and sixth nerves are simple and well understood. The only writings bearing directly on the subject of paralysis of the fourth nerve, with which I have met, are those of Dr Jacob and M. Szokalski. Dr Jacob's remarks scarcely admit of condensation. He observes[3] :—

" Paralysis of the superior oblique muscle has not been noticed, because it is not easily detected. I have already said that I believe the action of this

[1] Op. cit., p. 396.
[2] De l'Influence des Muscles Obliques de l'œil sur la Vision, et de leur Paralysie. Paris, 1840.
[3] Loc. cit., p. 19.

muscle to be very delicate, and confined to communicating a slight rotatory motion to the eye. The grounds upon which I have arrived at this conclusion are, repeated observations of cases of paralysis of the other muscles, from diseases of the third. It must, I think, be admitted that when the levator, depressor, adductor, and inferior oblique muscles are paralysed, the eye must remain fixed, unless moved by the abductor, or superior oblique. That this is the case, an attentive examination of an eye affected with what is commonly called *ptosis* proves. The eye is turned out by the action of the abductor, supplied by the sixth nerve, as perfectly as ever, and when the patient is directed to look downward, or toward his shoulder, the cornea is seen distinctly to revolve, with little if any depression of the pupil. In other words, the eye-ball is turned round its antero-posterior, or longitudinal axis. In this I think I cannot be mistaken, as I have repeatedly called those about me to observe the fact, in order to bear testimony to it. I have alluded above to a case in the city of Dublin Hospital, in which the four straight muscles of the eye, with the elevator of the upper lid, and probably the inferior oblique, were paralysed from disease of the third and sixth nerves within the skull, and in which this delicate rotatory motion was obvious. If the action of the superior oblique be to turn the eye downward and outward in a considerable degree, there could be no difficulty in demonstrating that action, in the case of the other muscles to which I allude; yet this cannot be done, but on the contrary the delicate rotatory action to which I allude is the result of the effort. The next question, however, is, whether paralysis, either sudden or slow, of the superior oblique, takes place or not; and this question is not, I admit, so easily resolved by demonstration. One thing must, however, be admitted, and that is, that it is most improbable that this superior oblique muscle, and the fourth nerve which supplies it, should be exempted from disease or its consequences, while the other nerves and muscles so frequently suffer. On the contrary, looking at the remote origin of the nerve, the length of its course, and its small dimensions, we should rather expect to find it more frequently engaged than any of the rest. If the action of the muscle be what I say it is, it is no wonder that paralysis of it should be difficult of detection, while all the other muscles are in a state of activity. Its loss is not perceived in the multiplicity of other motions. There is, however, I think, good grounds for supposing that the consequences of such paralyses are occasionally evident. There are certain cases of anomalous and unintelligible defects of vision, which can scarcely be accounted for in any other way than by attributing them to this cause. I allude particularly to double vision with great confusion of sight, and with little or no squint."

Dr Jacob then refers to several such cases. One patient had double vision. When he looked downwards, he lost sight of objects, while he saw well enough on looking upwards. He saw quite well with either eye singly. In three cases the patients saw near objects correctly, but those at some distance appeared double. Another where vision was good with one eye closed, but imperfect when both were open; and it is not stated that there was any squint. Dr Jacob farther states that he has met with many similar cases; but not taking this view of them at the time, did not pay that attention to them he now would. Although I have thought it right to quote the points of interests in these cases, which the author merely suggests might possibly have been of this nature, yet they do not strike me as exactly the symptoms which we would expect to arise from want of action of the superior oblique muscle.

According to M. Szokalski,[1] paralysis of the fourth nerve is rare as a separate condition; but he has seen it several times along with paralysis either of the third or sixth pair. He relates two interesting cases, which I shall translate entire:—

Case I.—" The eyes of the patient have no trace of lesion, their mobility is in no way constrained, and they can be directed easily to all sides, which proves that there is no anomaly in the straight muscles of the two eyes. The two pupils are of equal size, and equally dilatable. Nevertheless, the centre of the *left* cornea manifests a tendency to be placed a little lower down than the right; there is double vision, and constantly *one image of the object is placed above the other*. If the right eye is shut, the upper image disappears; if the left eye, the lower image ceases to be visible. The two images resemble each other always so exactly, that the patient learns only by judgment and habit, that the inferior is false. When the head is inclined to the left, the images separate more and more; but if it is inclined to the right only one image is seen. Since I have pointed this out to him, adds M. Szokalski, he goes with the head inclined to the right; formerly he managed by keeping one eye shut."

Case II.—" An attentive examination showed to M. Sichel and myself, that the two eyes are perfectly moveable in every sense. The patient states that every object seems double to him, in such a manner that he perceives two images, *the one above the other;* these two images separate in proportion as he retires from the object; the lower image appears less distinct. Some vessels were found injected in the external corner of each eye; we looked carefully whilst we moved the patient's head alternately to the left and to the

[1] See Longet's work already quoted. Vol. ii. p. 399.

right, holding it by the temples, and we remarked then that *the left eye remained attached to the wall of the orbit, which followed the movements of the head,* whilst the right eye underwent a rotatory movement in the orbit. The patient was astonished to see single when he inclined the head to the right."

"It appears impossible," says M. Szokalski, "to explain these phenomena otherwise than by paralysis of the left superior oblique muscle." And M. Longet concludes—"According to these observations of M. Szokalski, the symptoms of paralysis of the fourth nerve are the following:—1. The impossibility of the rotation of the eye in the orbit. We recognise that impossibility when the patient is directed to bend his head alternately from side to side, while he keeps his eyes fixed on some object; we see then that the affected eye remains fixed, and that it does not follow the rotations of its fellow. 2. There is constantly double vision, and the two images are placed *one above the other;* the affected eye furnishes the lower image. 3. The double vision disappears when the head is inclined to the side away from the affected eye."

Now, if the report of these cases is faithful, and they seem very circumstantially related, they form a very interesting contribution to the subject of paralysis of the fourth nerve. Since my attention was drawn to this subject, I have not had an opportunity of verifying or testing these observations; but, looking theoretically to the matter, I should think that the passive state of the eye, whilst the head was being bent to the side, would require to be carefully looked for, to be noticed. According to our view of the action of the oblique muscles, when the head is bent over to the right, the right superior oblique and the left inferior are in action, so that the upper part of the right eye is turned round towards the inner canthus, and the upper part of the left is turned round towards the outer canthus, so as to counteract the chiefly circular displacement which the picture would otherwise undergo on the retina. Now, were the movement a purely rotatory one, without the least depression or elevation of the axis, we should expect to find, not double vision with one object above the other, but confused vision, one image of the object being across the other; that, for instance, a pencil, held up, should appear more or less like a cross. But as a purely rotatory motion, without any change in the direction of the axis, would be required only if the head was being bent round on an axis exactly corresponding to the axis of the eye, which can never be exactly the case, at least with both eyes at the same time, we can readily understand why the oblique muscles, whilst they roll the eye round, may be employed also slightly to alter the direction of its axis, and why therefore paralysis of one of these muscles should cause double vision with separated images, unless when its antagonist on the same side is in action; that is, for instance, why paralysis of the right superior oblique should cause a second image to appear, belonging to the right eye, when the head was bent over to the same side, or unless when it was turned to the opposite side. Just in the same way as, in paralysis of the right sixth nerve, the patient has double vision when he looks to the right, but sees single when he looks to the left side. I cannot, however, see, still merely reasoning on the matter, why in paralysis of the superior oblique, the false image should necessarily be situated *below* the other, because if the superior oblique has any little power in changing slightly the direction of the axis whilst it is rolling the upper part of the eye round towards the inner canthus, it will certainly be downward; and, when the muscle is paralysed, there would be a want of this slight downward motion, and the false image should therefore be, if not above, at least not below, the other. This I infer from the experiment of causing double vision by pressing one eye in different directions. If the right eye be pressed inwards with the finger, the false image moves to the left. If the eye be pushed up, by pressing, for instance with the end of a small key, deeply between the lower lid and the orbit, the false image moves up. And if, in a similar way, by pressing in deeply between the orbit and the upper lid, the axis be *depressed*, the false image will move down, and appear *below* the one from the untouched eye, somewhat as in M. Szokalski's cases of presumed paralysis of the fourth nerve.

My object in presenting these theoretical considerations, however, is not to endeavour to prove the superior oblique not to have been affected in these cases, but to show on the one hand that this relative position of the two images, in diplopia, is not to be regarded as essential to establish the case as one of paralysis of the superior oblique, a condition which might give rise to various positions of the false image; and on the other hand, that such a relative position of two images does not necessarily, of itself at least, point to the superior oblique as the muscle affected, but might be due to a slight inequality in the action of the upper or lower recti muscles.

Before leaving the subject of our view of the action of the oblique muscles, I may illustrate it by reference to the affection called "oscillation of the eyeball." This oscillatory movement is rotatory round the antero-posterior axis, and is not to be confused with the oscillatory movement from side to side by the lateral recti, which is not uncommon, and has received a different name. In his practical treatise on diseases of the eye, Dr Mackenzie observes, "in oscillation the eyeball is affected with an almost perpetual rotatory motion round its antero-posterior axis. The patient is not conscious of this motion, from any particular feeling he has in the eyes, nor can he restrain it. It goes on even when the lids are closed, but it ceases during sleep. The motion varies in extent, from a scarcely perceptible degree, to perhaps nearly a quadrant. In some cases the motion seems to be rather from side to side, but often so small in degree and so rapid, that it is difficult to say what is exactly its direction. In general, it is pretty distinctly rotatory, and seems to be produced by the antagonising action of the obliqui, the recti having lost, in a great measure, their control over the eye."

Before proceeding to inquire into the relation between the nerves and muscles, let us now understand clearly the actions of the different muscles of the eye in its various motions. Here we first separate the straight muscles from the oblique. The former perform all those movements by which we look in various directions, and in which the direction of the axis is changed. The four direct movements are performed each by the corresponding rectus acting alone. The four diagonal movements are effected each by the conjoint action of the two neighbouring recti. The oblique muscles are not necessarily in action during the performance of any of the above movements, nor are they concerned in their accomplishment. They turn the eye round on its axis. The superior turns the top of the eye round towards the inner canthus; the inferior rolls it back by an opposite movement, by which the lower part of the eye is turned towards the inner canthus. This is the action of each class on each eye; let us now see what relation the muscles have to those of the opposite eye in the various movements. Here we observe that the eyes are bound to each other in their motions, in order that the convergence of the axis to a greater or less degree shall never be destroyed; so that when the eyes are moved sideways a non-corresponding muscle must be employed. The superior rectus always acts with its fellow of the opposite side; so likewise the inferior. But were the external rectus to act with its fellow farther than is sufficient to bring the axes from convergence near to parallelism, divergence of the axes would result. Therefore, beyond this at least, the external rectus does not act with its fellow; but as, in looking sideways, one eye moves in when the other moves out, the action of the external rectus is attended by that of the internal of the opposite eye. The internal rectus, lastly, is or is not attended by the action of its fellow, according as we direct both eyes inwards, or look sideways; so that this muscle acts sometimes with its fellow and sometimes without it. It will thus be observed that the upper and lower recti always act with their fellows, that the external rectus never does so, and that the internal does or does not according as the movement is a corresponding or non-corresponding one. The various movements and muscles concerned in each, therefore, are:—*Corresponding*, or symmetrical. 1. Both eyes upwards; both upper recti. 2. Both downwards; both lower recti. 3. Both inwards; both internal recti. 4. Both downward and inward; both inner and both lower recti. 5. Both upward and inward; both inner and upper recti. *Non-corresponding*, or a-symmetrical. 6. Right eye out, the left in; right external and left internal rectus. 7. Right eye downwards and outwards, the left downwards and inwards; right external and left internal, and both inferior recti. 8. Right eye upwards and outwards, left upwards and inwards; right external and left internal, and both superior recti.

Now as to the relation between the oblique muscles of opposite sides. As is stated by those who have written on the subject, the superior oblique of one side will be in action with the inferior oblique on the other. When the head is inclined over to the right side, the action of the right superior and left inferior oblique is required. Consequently each oblique muscle acts with the opposite oblique muscle of the other eye; the action of either of them with its fellow of the other eye would rotate one of the eyes altogether in the wrong direction. This, however, holds good only of man and those animals in which the eyes look well forwards, and can be directed at the same time to the same object. As I have remarked in considering the anatomy and physiology of these muscles, it must be different in those animals in which the eyes are directed so much outwards that they cannot make the axes of both eyeballs converge, nor in any way use both eyes on the same object at the same time. Such are, among mammals, the hare and rabbit, and the three inferior classes of vertebrate animals generally. Now as both eyes here—in a bird, for instance, where the eyes are set quite in the side of the head, and look quite outwards—cannot possibly be used on the same object, it becomes a matter of less interest to inquire into the relation between corresponding muscles of opposite sides, as we do not know whether the animal is ever moving or attending to the sensations of both eyes at the same time. But supposing both eyes to be so employed, then, under the circumstances requiring the action of the oblique muscles, the corresponding oblique muscles will act together, the inferior with the inferior, and the superior with the superior. This action of the oblique muscles would appear to be still more required when the eyes are set in the side of the head, as in the simple movement of raising or depressing the head while an object is being viewed; as then the head moves as if on an axis approaching in direction to that of the eyes themselves. The superior oblique will be required to act when the head is raised, and the inferior when it is depressed. Whilst, then, we can see the necessity of the oblique muscles *to each eye* in these animals, we can say nothing as to any relative action of the oblique muscles on opposite sides, except that if both eyes are being used at the same time, though not on the same object, then each oblique muscle must act with its fellow of the opposite side; but if not, there can then be no relative action between the oblique muscles of opposite sides; nor, indeed, could any advantage, so far as we can see, accrue to animals in which the eyes are so placed, were even the straight muscles associated in their action, as they are in man and all animals in which both eyes can be used forwards on the same object at the same time. In regard to the relation between the oblique muscles of opposite sides, we may therefore conclude that, in man and in animals, which use both eyes on one object, and in which the eyes are known to move consensually, the superior oblique of one side acts constantly with the inferior, not with its fellow, of the opposite side, and *vice versa*.

We are now then, in a position to inquire into the meaning of the relative distribution of the motor nerves. The question of interest is, why the external rectus and the superior oblique receive each a separate nerve, and whether this, together with the distribution of the third nerve to all the other muscles, can be connected in any way with the regulation of the ocular movements? I shall first notice a few of the observations which physiologists have offered on this subject, although I do so more on account of the authority with which they come than for any value which they appear to me to possess.

Valentin,[1] more especially, has written on this subject, and has, with much argument and illustration, presented a theory. It is, that some of the muscles and nerves are voluntary, and the rest involuntary or automatic; and that the action of the voluntary nerves of the one side is attended by that of the automatic nerves on the other, for the non-corresponding movements. His voluntary nerves are the fourth and sixth, and the upper division of the third; the lower division of the third, supplying one-half of the muscles, being automatic or self-acting. Thus, he observes, the upward and outward movements are voluntary, the downward and inward involuntary or automatic. "All regular motions which are harmonious and a-symmetrical are so performed that on one side a voluntary muscle acts,—either the superior or external rectus or superior oblique; whilst, on the other, there is the harmonious action of a muscle more or less

[1] De Functionibus Nervorum. 1839.

automatic."[1] That is, in the non-corresponding movements the voluntary muscles lead off on the side to which we look, and the automatic, or self-acting, muscles make the other eye follow. He finds no difficulty in bringing two of the so-called voluntary nerves to act together in raising both eyes, or two of the involuntary ones in depressing both eyes; but here the nerves are corresponding. Valentin, it may be observed, believes that the inferior oblique muscle turns the eye upward and inward, and we must infer, from the preceding remarks, which give the substance of his theory, that he believes the superior oblique to be concerned in some outward movement. I have stated this as his view in my paper on the muscles of the eye, following the account given of Valentin's views in another work, but in the original he nowhere makes this statement, so far as I can find. In enumerating the muscles concerned in the various movements, the only mention made in any of them of the superior oblique is in the following passage (p. 30):—"4. Uterque oculus primo initio ad interna et inferiora movetur. Quæ contractio aut m. m. rectis interno et inferiori aut m. m. recto interno et obliquo superiori simul agentibus evenit." His view as to the actions of the recti muscles is the same as that which I have given above, but the oblique muscles he brings in to assist in the inward diagonal movements of both eyes. When both eyes turn upwards and inwards—a movement which he gives as involuntary—he employs the internal recti and the inferior obliques; when both look downwards and inwards, the superior obliques are brought in, according to the passage above quoted, to assist also the internal recti. Still it must be inferred, from his remarks on the nerves, that he believes the chief action of the superior oblique to be, to assist in an outward movement. But the superior oblique seems rather to stand in the way of this theory, as indeed of all others in which the oblique muscles are supposed to assist some of the recti. But, although we put aside that part of Valentin's theory which relates to the oblique muscles, it may be said that the same explanation may be shaped to our view of the action of these muscles. With due deference, however, to the high

[1] Valentin, op. cit., p. 31, s. 69.

authority by whom it is proposed, it must be observed that the theory or explanation really does not explain what we wish to understand, or in any way render the matter more simple. It assumes that some of the muscles and nerves are involuntary, for which there does not appear to be any good ground; for if we take the ordinary ocular movements as we find them, without being influenced by any preconceived theory, no one appears to be more voluntary than another; and to say that one nerve is automatic, and necessarily acts with a voluntary one of the other side, is merely another way of expressing the fact that the one eye moves with the other, whilst what we are seeking for is some explanation of the fact itself.

There is next Müller's explanation.[1] The substance of his remarks, in endeavouring to explain the arrangement of the nerves and the harmony of the movements, may be given thus: —1. That the corresponding branches of the third nerve have an innate tendency to act together on opposite sides, evinced from the time of birth, and therefore due to some peculiarity of structure at the origins of the two nerves. The corresponding muscles supplied by them, therefore, always act together on opposite sides; and to explain why such is not the case with the internal rectus when we look sideways, he supposes that the internal rectus of the everted eye really acted, but was overcome by the stronger action of the external. 2. That the sixth nerve wants this tendency to act with its fellow, and that the strong action of the one is incompatible with that of the other. Also, that this enables us to understand why in all vertebrata the external rectus receives a separate nerve. 3. That the reason why the fourth also is a separate nerve is, that its muscle moves the eye downwards and outwards, and that therefore the opposite muscle is not to be employed.

Before commenting on this hypothesis, I may notice, lastly, the remarks of M. Ph. Berard, as quoted by Longet,[2] with the view of explaining why the external rectus and superior oblique receive separate nerves. He observes, that when we look up and

[1] Elements of Physiology. Trans. by Baly. 1835. Pp. 928-931.
[2] Op. cit., tom. ii., pp. 396, 397, 405.

down we no doubt use branches of the third nerve in opposition to each other; but this opposition is on the same side. But whenever we look out sideways, there is a double antagonistic action; for the external rectus is not merely the antagonist of the internal of the same side, but of the external rectus of the other side, in so far that they never act together. It was necessary, then, that one of the lateral recti should receive a special nerve; and he thinks the reason why it is given to the external rather than to the internal rectus is, that a more extended motion is required outwardly than inwardly. Also of the oblique muscles, in accordance with our view of their action, which he holds, as each acts not with the corresponding but with the non-corresponding oblique of the opposite side, therefore one of them—the superior as it happens—has received a separate nerve; and, therefore, he adds, as "the same pair of nerves could not produce so complicated a movement, it was necessary that the superior or inferior oblique should receive a special nerve."

Now these theories, when examined, whilst they have an appearance of plausibility, in reality, so far as they are correct as to fact, amount to nothing beyond a mere statement of the fact. Passing over the part of Müller's theory which relates to the action of the oblique muscles, the fallacy of it in other respects is evident, as has been well pointed out by Dr G. Johnson,[1] in the first place by showing, that there really is no such tendency in all the corresponding branches of the third pair; and, secondly, by the observation, that "we must not suppose we are explaining the necessity for this arrangement by asserting, to use the words of Müller, that if, in place of the sixth nerve, the external recti muscles had received each a branch of the third nerve, it would have been impossible to make one of these muscles act without the other." M. Berard's remarks are likewise objectionable, from his assuming the necessity of there being separate nerves for opposite movements on the two sides; but they evidently, embracing as they do the correct view of the use of the oblique muscles, point to the only explanation of which the matter admits,—viz., that separate nerves *are* employed when, and only

[1] Cyc. of Anatomy and Physiology. Article, Orbit. 1844. Pp. 791, 792.

when, non-corresponding muscles are in action on opposite sides, whilst corresponding nerves, as is natural, are employed when the muscles are corresponding. Or, as Dr Johnson expresses it, "assuming that the use of the oblique muscles is such as we have mentioned, it is certainly curious to observe that when corresponding muscles of the two eyes are intended to act together, as the superior rectus of one eye with the superior rectus of the other, and the same with the inferior recti, both muscles are supplied by the third nerve; but the external rectus, which acts consentaneously with the internal rectus of the opposite eye, has a separate nerve, the sixth; and the superior oblique, which acts with the inferior of the opposite eye, has the fourth nerve entirely devoted to it."

Let us now, therefore, see what we really know as to the motor functions of these nerves. Of the third nerve, we have observed that it supplies muscles which act in the corresponding movements on the two sides; but the various branches act under different circumstances. The upper division supplies the levator muscle of the upper lid as well as the superior rectus. Now, the action of these two is frequently conjoined on the same side, the action of the superior rectus being usually accompanied by that of the levator palpebræ for an evident purpose; but not necessarily so, as we can, with very little effort, keep the eyelids closed, and at the same time move the eyes upwards; and again, the levator palpebræ acts often enough without the superior rectus, as when we are looking in any direction except upwards, the upper eyelids being raised sufficiently to expose the eye. There is, therefore, no necessary association in action on the same side of the two muscles supplied by the upper division of the third nerve. On opposite sides, the physiological relation of these two muscles is quite different. The levator palpebræ usually acts with its fellow, but not necessarily, so that, after a very little practice, one eyelid may be raised without the other, although at first this is effected, until the tendency be overcome, by the orbicular muscle keeping the lid down. Indeed the tendency to simultaneous action in the levator palpebræ muscles is not greater than that between the orbicular muscles of the two sides, supplied by the portio dura of the seventh pair. But no amount of effort or practice will enable us to use one superior rectus without the other. Of the lower division of the third nerve, the branch to the inferior rectus is exactly in the same position as that to the superior. The lower and upper recti are the direct opponents of each other, but they cannot act without their fellows of the opposite side. The branch to the internal rectus again differs from these. It acts in the direct or diagonal inversion of both eyes with its fellow, like those to the upper and lower recti, but, unlike them, not necessarily exactly in the same degree, as when the eyes converge unequally; and again, without its fellow and with the sixth nerve, as when we look sideways, in this case resembling the sixth nerve itself. And, lastly, the branch to the inferior oblique, according to our view of the use of the oblique muscles, will never act with its fellow, but, at the same time, as the fourth nerve of the opposite side, in this respect exactly resembling the fourth.

Farther, the muscles supplied by the third nerve do not seem to have, on the same side, any greater tendency to act with each other than with the muscle supplied by the sixth nerve. Although we use the upper and lower recti more frequently along with the internal than with the external rectus, still we do not feel any more difficulty one side than on the other when one eye is directed upwards and outwards, and the other upwards and inwards. Thus the upper division of the third acts as readily with the separate sixth nerve as with the nerve to the internal rectus, which comes from the same trunk as the former.

There is another branch of the third nerve—that to the iris, through the ciliary ganglion—the action of which is always, like that of the nerves to the upper and lower recti, attended by the action of its fellow of the opposite side. It is maintained by Müller,[1] that there is a necessary association in action between the iris and the internal rectus, and still more so with the inferior oblique, This falls more naturally to be noticed in considering the functions of the ciliary nerves; but I may observe here that this association between the iris and the internal rectus

[1] Op. cit., p. 773.

does not appear to be a necessary one, or due, as Müller supposes, to any contiguity or connection of their nerves. We observe, it is no doubt true, the consensual contraction of the pupil to occur always when the eyes converge on a near object; but this is not due merely to the internal recti being in action, but occurs simply because it is required when a near object is being viewed. The two occur together, because both are necessary to the viewing of a near object.

But the consentaneous contraction of one pupil may occur independent of the action of the internal rectus, as when we look out sideways at a near object. Here both pupils contract, whilst on one side the external, not the internal, rectus is in action. And again, in looking sideways at a distant object, one of the internal recti is in action, but both pupils are enlarged. Therefore the consensual contraction or dilatation of the pupils occurs simply according as we view a near or distant object, whatever may be the muscles employed. These remarks relate only to the consensual movements of the irides, the influence causing which must originate in the brain; otherwise the pupils are moved by reflex action, through the impressions on the retina, conveyed backwards along the optic nerve, and reflected outwards along the third nerve. Here the tendency to association is so remarkable, that an impression on one retina only, causes an equal contraction of both pupils. When the pupils act consensually,—*i.e.*, along with one or other of the muscles of the eye, the fact of their keeping exactly of the same size is only what we would expect to meet the purpose to be served; but when the action of the pupils is reflex,—*i.e.*, from the quantity of light, independent of any motion of the eyes, the fact of impression on one eye causing an action of both pupils may seem to favour the view taken by Müller, that there is a peculiarity or identity at the roots of that part of the third pair of nerves which goes to the irides,—that is to say, that it is impossible for the nervous centre to act on the nervous fibrils which go to one iris without also acting on those which go to the other iris. But this hypothesis seems to be unnecessary. Symmetrical reflex action, when the irritation is applied to one side only, is at any rate not confined in its occurence to the iris; but the fact, as affecting the latter, seems to me to be sufficiently explained by the decussation of the optic nerves. As each optic nerve divides, a part going into each tract, both sides of the nervous centre are necessarily and simultaneously affected by the stimulus. Thus the equal reflex action of both pupils is secured, independent of any inequality of the light falling on the two eyes, which, had there been no decussation, or had each nerve entirely crossed over to the other side, so that each retina regulated the reflex actions of one pupil only, would have caused an unequal state of the pupils; and thus we understand why an impression on one retina should cause a reflex action of the opposite pupil, just as readily and as completely as it does of the pupil of the same eye.

Next, as to the sixth and fourth nerves, we observe that they do not act along with their fellows, but with the antagonist nerve of the opposite side, the fourth with the nerve to the inferior oblique muscle, and the sixth with that, also from the lower division of the third, which goes to the internal rectus,—and that these nerves go to one of each of the two pairs of muscles which are employed in non-corresponding movements. As regards the sixth nerve, it is worthy of remark that, in animals which possess a retractor muscle, which is supplied by the sixth nerve, a part of this nerve acts differently. It is probably the case that the retractor muscles act at least generally together; and here we have a single nerve, part of which is generally used with its fellow, whilst the other part is not so, but with its antagonist of the opposite side.

Now, if we endeavour to explain all these differences in the different nerves, or different parts of the same nerve, by saying that each nerve has a certain endowment whereby it always, or sometimes, or never, acts with its fellow of the other side, we must recollect that we are merely expressing the fact that we observe such and such to be the case, and take care not to put it so that our way of stating the fact should lead us to suppose that we have in any way explained it. The object of these differences in the mode of employment of all these nerves and muscles is, that both eyes may be used at the same time for single vision, and to accomplish this the eyes must move so that the axes shall always converge more or less. For this purpose, then, we have, first, suitable muscles; secondly, nerves going to each; and, thirdly, these nerves are subjected to a central influence, by which these motions, otherwise voluntary, are regulated so as to establish and preserve the harmony of the ocular movements. However that influence may originate, or whatever its nature, whether we speak of it as an instinctive action or original endowment, or an irresistible influence, or as partly the result of the guiding sensations of the retinæ, there is such a central influence, which exerts itself, alike as a guide and check, on all the movements of the eyes, giving rise to a class of movements which stand by themselves, and can be compared to, or illustrated by, no others in the body. The nerves we should regard not as themselves possessed of special endowments, but merely as channels by which the nervous centres, with their special endowments, act down upon the muscles. The mere fact of the third nerve supplying the superior recti by corresponding branches, does not explain why these two muscles should always act together, because the neighbouring fibres to the levatores palpebrarum muscles are not so bound together. Again, the mere fact of the sixth nerve being a separate one, does not explain why the external rectus cannot be made to act with its fellow, from any endowment of the nerve, for that part of it which goes to the retractor muscle is not so tied up. But the reason why the nerves of the upper recti always act together, and why those of the external recti never do so, is simply, that they obey the central regulating influence; and the reason why the nerves which supply the levators of the upper eyelids may act separately, and that those from the sixth nerves to the retractor muscles in animals are not tied up from acting together, is simply, that such may be the case without interfering with the harmony of the ocular movements, by which the parallelism or convergence of the axes is preserved. Further, this regulating influence is exerted no less in the parallel than in the non-corresponding movements. We have just as little power of moving one eye up and the other down as of making both look outwards, the harmony is the same, and the central influence guides and controls alike in both cases. We are not, therefore, to conclude that it is only when the sixth and fourth nerves, and non-corresponding muscles, are employed, that this influence is needed or exerted. Still, when we look to those movements where non-corresponding muscles are employed, we observe that on one side a separate nerve is provided for the muscle. But we see no reason why this should be so, beyond the inference we draw from noticing the fact that it is so; nor are we to suppose that this explains why one eye follows the other in an opposite movement; we merely observe the facts together, and we therefore conclude that the design contained in the provision of separate nerves, the sixth and fourth, for the external rectus and superior oblique muscles, is explained, so far as it can be, by the circumstance, that one of these nerves is used on one side, when the movements are non-corresponding or in opposite directions; and it is, in connection with this, also remarkable, as before stated, that while the third or common nerve arises as near to its distribution as possible, the origins of these two single nerves are so remote and separate.

As regards the nature of the movements of the eyeballs, by which the direction of the axes is changed, we observe that they are all perfectly voluntary,—that is, we can, when we wish, look with both eyes in any direction we choose, only the will is so subjected to the central regulating influence, that the two eyes are obliged to move conjointly, so as to preserve the convergence of the axes. But it is a very common belief, that there is likewise an *involuntary* movement of the eyes in the upward direction. This was maintained more especially by Sir Charles Bell, who describes the eyeballs as being turned involuntarily upwards in the act of winking, during forcible expiratory efforts, in sleep, fainting, and the insensibility preceding death. His theory as to the use of this motion in winking was, that the cornea is turned up in order that the mucus and impurities brought down by the lid might not be left across the cornea after being collected in the little triangular gutter, which was formerly supposed to remain between the margins of the closed lids and the cornea. The motion he attributed, on very theoretical grounds, to the action or influence of the superior oblique muscle and fourth nerve; but later writers attribute it to the involuntary action of the inferior oblique muscles. But it does not appear to have been very carefully inquired into, whether such movements actually occur under these circumstances. If the eye be forcibly exposed by holding up the eyelid, as in searching for a foreign body in the fossa of the conjunctiva, the eye may be seen to roll up; but this is a voluntary action, performed in order that the front of the eye may be placed out of danger deeply under the upper eyelid. But is it the case that the eye rolls up every time the eyelids meet, either in the ordinary winking motions, or that which may occur during the acts of sneezing and coughing? Is there any satisfactory proof that such a motion actually and regularly occurs? The statement that the eyeballs are turned upwards during sleep, is certainly incorrect. I do not say that they may not occasionally be so found, but they are not generally so; on the contrary, as Mr Mayo[1] remarks, in raising the lid of a person in sound sleep, "the eye is found directed straight forwards; but in some instances the eye is directed upwards and outwards." That the eyes are sometimes rolled upwards in fainting and on the approach of death must have been observed by every one, both when the eyelids are wide apart and when they are nearly closed. But it does not always occur under these circumstances, and the direction is not constantly the same. The more common opinion seems to be, that they turn upwards and inwards, owing perhaps to the striking appearance which this motion gives; but it has seemed to me that the motion is more frequently directly upwards, and occasionally both eyes are turned a little upwards and outwards. Looking to their anatomy in man only, it was perhaps natural enough to ascribe this upward rolling motion to the inferior oblique, especially when no other use could be found for that muscle; but it is not necessary for me to go back on this matter now to show that the inferior oblique muscle has quite a different purpose to serve, and is not, in all, so placed as to be capable of turning the cornea up-

[1] Anat. and Phys. Commentaries. 1822. Part ii., p. 6.

wards. We must attribute these actions chiefly to the superior recti, acting sometimes with the internal and sometimes with the external recti, although we cannot explain why they should occur. That the upward direction is not always the same, shows that it is not an action of one muscle provided for this purpose; and that its occurrence is not invariable, shows that the motion is not an essential one.

But, according to the view maintained as to the use of the oblique muscles, their action cannot be held to be voluntary, in the full sense at least, as that of the recti muscles is so; for, as Longet has remarked, we do not seem to have the power of voluntarily turning the eye round its axis when the head remains fixed. Still this is not equivalent to regarding the oblique muscles as involuntary. They are subjected to the central influence, which prevents any motion from which double or confused vision would result; and we should have no power of using these muscles voluntarily, for the same reason that we cannot turn one eye up and the other down, or both eyes out. Still it may be said, as the regulating influence does not restrain the actual motion, but only regulates the motions of the two eyes, that if we use the superior of one side with the inferior oblique of the other, we should be able to twist the eyes round about. But this power would be of no use to us under any circumstances; and indeed such a movement of the eyes would be injurious to correct vision at all times, except when the motion is actually required. If we keep the eyes fixed on an object straight before us, and move the head round to the right side, by a motion at the joint between the two first vertebræ, we use, to keep the eyes fixed, the left external and the right internal recti; but if we bend the head down towards the right shoulder, we use the right superior and the left inferior oblique; we are alike unconscious of the effort in either case, both the motions are equally voluntary or involuntary, being performed under the direction of the guiding sensation.

There is just one other question concerning the oblique muscles which has not been considered. We have seen their relation to each other on the same side,—that they act alternately as direct antagonists; and also their relation to their fellows of the opposite side—that the superior of one side acts with the inferior of the other; and the remaining consideration is, what relation exists, if any, between the two classes of muscles, the straight and oblique, *on the same side*—whether either oblique muscle acts more with one rectus than another. First, we observe, as bearing on this, that the inferior oblique is seemingly grouped in nervous supply, by the inferior division of the third nerve, with the lower and internal recti; and this might lead us to suppose that it was associated in action with these muscles more especially. This inference, however, is met by the considerations, that the arrangement of the motor nerves has reference more to the relative action of the muscles of opposite eyes; and that, as has been already remarked, the nerve to the inferior rectus acts as readily with the sixth nerve, as with that to the internal rectus, with which it is associated in origin. Also, that only one of each of the two pairs of muscles which act non-correspondingly receives a separate nerve, whilst the common nerve goes to all the other muscles in the orbit; and that, from the position of the muscle, it is as natural for the inferior oblique to be supplied by the division of the third nerve, which goes to the lower and inner recti, as it is for the levator palpebræ to be supplied from the division which supplies the superior rectus. Apart, however, from these reflections, on considering carefully which of the straight muscles will be more or less in action with each of the oblique muscles, it does not appear that there is any constant relation. It will depend very much on how far and in what way the two kinds of motion of the head are blended. The superior oblique will be in action as often with the internal rectus, and the inferior oblique as often with the external, as with any of the other recti muscles; and it therefore does not appear that there is in the same orbit any special association between either of the oblique muscles and one or more of the recti.

[NOTE.—*March* 1854.—*On the Third Nerve and Ciliary Ganglion in the Horse.*—I am aware that Valentin has described the third nerve as supplying in part the retractor muscle in the horse, and on his authority the statement has been made by others that this is the arrangement "in some animals."

Valentin states,[1] that "in some mammalia, the horse for example, it (the third nerve) supplies also the retractor muscle of the eye; and in his work "De Functionibus Nervorum"[2] he states that, in the horse, the retractor muscle is supplied by the third as well as by the sixth nerve, the external part by the sixth, and the internal part by the third; and this suits his theory, according to which the muscles which invert and depress the eye are involuntary, and regulated by the lower division of the third nerve.

My own observation of the nerves in the horse does not agree with that of Valentin. I find the internal part of the retractor to be supplied by the sixth nerve equally with the external part. The two filaments for the retractor come off, one close after the other, from the inner side of the trunk of the sixth nerve, immediately after it has entered the orbit—one going to the outer and the other to the inner half of the muscle. I could find no filaments from the third nerve to the retractor muscle. There is, however, the appearance as if the lower fasciculus from the ophthalmic ganglion supplied the muscle, but I could not determine whether this filament supplied the muscle or only pierced it to reach the eye. It is, however, very delicate compared with the large supply equally to the inner half of the muscle from the sixth nerve.

The retractor muscle in the horse is pierced by several filaments from the nasal branch of the fifth nerve, both on its internal and superior aspects. The latter arise in the apex of the orbit, and may, in part at least, be easily traced on to the eye as long ciliary nerves. This is the common arrangement in animals with a complete retractor: the long ciliary nerves pierce it to reach the eye. But they may branch in the muscle and unite with the sixth nerve in it, and have at least the appearance of supplying it. These, however, are derived from the sensory or ganglionic portion of the sixth nerve, which, in some

[1] Op. cit., p. 289. [2] 1839, p. 21.

animals at least, regularly supplies filaments to the muscles of the eye.

The above remarks imply that I have found the ophthalmic ganglion in the horse. Some anatomists (Muck and Desmoulins) had denied its existence in that animal. Retzius, admitting its presence, says it is very small; and it has been well and fully described by Mr Alfred Poland,[1] in a paper which, I regret to say, was unknown to me until a few weeks ago. I had dissected this ganglion in the horse several years previously, and observe in my notes, made in 1844, that it is described as "very distinct;" and I have a drawing of it made from another dissection in 1852.]

[1] Guy's Hospital Reports, 1849.

V.

CASE OF OPEN FORAMEN OVALE,

AND CONTRACTED PULMONARY ORIFICE;

WITH OBSERVATIONS ON THE

DEVELOPMENT AND PHYSIOLOGY OF THE FŒTAL HEART.

[Read to the Edinburgh Physiological Society, 13th March 1852; and extracted from the Monthly Journal of Medical Science, July 1852.]

This heart is from a child aged fifteen months. It presents,—1. An open foramen ovale; 2. A large Eustachian valve; 3. Great contraction of the pulmonary orifice; and 4. Contraction and hypertrophy of the right ventricle, with narrowing of the tricuspid valve.

The *foramen ovale* admits the little finger,—*i.e.*, is equal in size to a circular opening of the diameter of half an inch. Its valve is imperfectly developed, thick, and muscular-looking. It does not reach to the upper margin of the foramen, although its horns on each side reach higher than the opening.

The *Eustachian valve* is very large and loose, much more so than at any period of fœtal life. It measures nearly an inch (7-8ths) from its attached to its free border; and when lifted upwards, it reaches for one-third of its depth above the upper margin of the foramen ovale. It is cribriform, especially at the right half of its attachment, and also above, near its free margin.

Right ventricle.—The walls are 3-16ths of an inch in thickness, whilst those of the left are from 3-16ths to 4-16ths. In addition to hypertrophy of the walls, there is likewise contraction of the cavity, which is apparently about one-half the size of the left.

Tricuspid orifice admits the little finger (4-8ths inch), whilst the *mitral orifice* admits the forefinger (5-8ths). *Vena cava superior* admits little finger with difficulty. *Vena cava inferior* admits forefinger easily. *Foramen ovale* admits little finger moderately, as already noticed. *Tricuspid valve* much thickened. Cordæ tendineæ shorter and thicker than on left side, and musculi papillares large. *Mitral valve* appears healthy, except that there are a few vegetations on both its flaps, where the cords chiefly join them. *Aortic valves and orifice* natural. The orifice measures 3-8ths of an inch in diameter.

Pulmonary orifice and valves much contracted and altered. The semilunar valves are united together into one solid mass, with a small aperture in the centre. Seen from above, there is the appearance of a nipple, smooth and rounded, and perforated in the centre by an aperture not larger than a crow-quill, the whole somewhat resembling a cervix and os uteri. Surrounding the base of this nipple-like projection, and separated by partitions which join the central papilla, are *four* semilunar recesses, corresponding to the sinuses of Valsalva, and to the cavities of formerly existing semilunar valves. One of these is nearly as large as the other three together; it is left and posterior; one of the smaller is anterior; the other two are on the right side. Seen from below, the orifice appears blocked up by four irregular tubercular projections, two anterior, and two posterior; one of the posterior is large, the others are of equal size. They are firm and irregular, but covered by a serous membrane; and in between them is the small aperture which leads up through the central papilla. On pushing the probe down into the sinuses of Valsalva, it pushes out the membrane at a considerable distance below the tubercles.

The *ductus arteriosus* admits a small wire with difficulty, and was therefore practically closed. Both *lungs* are studded with tubercles of the size of small peas or grains of corn, on their surfaces as well as throughout their substance.

History during life.—A month before death there was difficulty of breathing, the lungs appeared healthy, and the case much resembled one of disease of the mitral valve. Ten days before death, occasional blueness of the surface made its appearance, lasting only a short time, without any paroxysm of difficult breathing, and then passing off. Death occurred suddenly. The case belonged to the late Dr Campbell, for whom I conducted the post-mortem examination, and to whose kindness I was indebted for this use of the preparation.

REMARKS.

On the Causes of Open Foramen Ovale.—A more or less open foramen ovale is well known to be the most common imperfection of the heart, but the causes of this condition are perhaps not generally understood or agreed upon. We may refer it to two causes—1. An impediment to the free passage of the blood through the right ventricle, from contraction of one of its orifices; and 2. To an imperfect development of the valve by which the foramen is usually completely closed at, or soon after, birth.

1. The connection between contracted pulmonary orifice and open foramen ovale has been remarked by various observers. M. Louis regards it as the most common cause of the open foramen, and farther looks upon this pulmonary contraction as almost always a congenital condition. Out of 53 cases of imperfect heart quoted by Dr Joy in the "Library of Medicine," the foramen was open in 33, the pulmonary artery contracted in 22; and it is not unlikely that in many cases the open state of the foramen ovale has been recorded, whilst the condition of the pulmonary artery and its orifice had not been carefully examined. The case I have related illustrates this connection between open foramen ovale and contracted orifices of the right ventricle. Still the question occurs, why should there so frequently be valvular disease and contracted orifice on the *right* side in the fœtus and child, and not in the left, as in the adult. I do not think that this can be explained by the supposition that the right ventricle

has, relatively to the left, more labour to perform in the fœtus than in the adult. In the fœtus, it is usually remarked as interesting to observe that the walls of the right ventricle are as strong as those of the left, or nearly so; but this I think has been overstated by some. In the fœtus between the fourth and fifth month, now before the Society, it will be observed that the left is twice the thickness of the right; and in the other fœtus I now show—at the third month—the difference is already well marked. We can see no reason, then, why, during the latter two-thirds of fœtal life, and in early childhood, the *right* ventricle should be the seat of hypertrophy and valvular contraction, and *not the left.* The fact of the foramen ovale being open, seems to be sufficient proof of the correctness of the view of M. Louis, that the pulmonary contraction is congenital, otherwise in the meanwhile the foramen would have become closed. Still it is not apparent why in the fœtus the pulmonary orifice should be the seat of malformation or disease more than the corresponding aperture of the left and stronger heart. The view may occur, that this contracted state of the orifices of the right ventricle and pulmonary artery, is not the cause but rather the result of the condition of open foramen ovale. This view might accord with the state of *simple contraction* of the pulmonary artery or its orifice, as part of the blood which should pass through them has found another channel; but it certainly will not account for *diseased* or *malformed* conditions of the right ventricle and its orifices.

I may observe that in the case I have related, it was the orifice merely which was contracted, not the artery, as, a short distance above their orifices, the measurements of the pulmonary artery and aorta were the same. Some cases have been recorded of contracted or obliterated pulmonary artery, in which the ductus arteriosus remained open so as to allow of a recurrent circulation to the lungs, but in this case the ductus arteriosus was practically closed. From this it may be inferred that the severe contraction of the pulmonary orifice had not been of very long standing, possibly not before the symptoms became aggravated, and occasional cyanosis made its appearance, about ten days before death.

2. Passing over the possibly correct view of some that the fora-

men ovale is occasionally again opened, or burst open, by severe falls, prolonged fits of coughing, or severe straining,—the next cause of open foramen ovale is the imperfect development of its valve. In connection with this, it is necessary to understand clearly the natural means by which the closure of this aperture is effected after birth.

Formation of the Foramen Ovale and Development of the Valve by which it is closed.—In the well-developed fœtal heart, the inferior vena cava terminates so that it may be said to open into both auricles, and each opening is provided with a valve. The Eustachian valve partly guards, or lies over, and diminishes, its opening into the right auricle; and its opening into the left auricle, which is the foramen ovale, is guarded behind by the valve of that foramen—the valvula foraminis ovalis. It is still a disputed question, whether the blood of the lower cava mixes with that of the upper. Now there is nothing to prevent the blood of the lower cava from coming forwards into the general cavity of the right auricle, only it must turn forwards at right angles to its previous course, around the free margin of the Eustachian valves and that part of it does so, appears evident from the consideration that the inferior cava is larger than the foramen ovale, whilst the tricuspid orifice is as large as those of both cavæ together. This is seen in the fœtus, between the fourth and fifth month, now on the table. And the foramen ovale becomes smaller as fœtal life advances, for, although it attains, as usually described, its maximum size about the sixth month, still, relatively speaking, the communication between the two auricles gradually decreases from the first appearance of the auricular septum, about the ninth week. Whilst, then, that blood which does go by the foramen ovale is still entirely of the purer current, from the lower cava, it follows, as the foramen is of less size than the vein, that some of the blood of the latter, that which the foramen ovale cannot take in, will enter the right auricle and mix with the other current.

This appears to be one of four points where there must occur some mixture of the pure and the less pure currents of the fœtal blood—the second is in the left auricle, where the blood which has passed through the foramen ovale is mixed with that blood which comes, whatever its quantity may be, by the well-developed pulmonary veins; the third, where the arch of the aorta and ductus arteriosus join, where it does not appear how some of the blood of the former can avoid passing down to the thoracic aorta; still, however, this is not till the vessels of the head have been filled by the purer current; and the fourth, or rather the first and chief point of mixture, is where the blood of the hepatic veins and ductus venosus is mixed with that of the vena cava inferior.

A great part, therefore, of the blood of the lower cava passes through the foramen ovale. But to prevent this after birth its valve is provided. This valve is rather a provision for closing the aperture at birth, than for any part it has to perform before this time. Although, many years ago, it was the subject of much attention in France, it has perhaps been a little overlooked by some, partly, perhaps, for want of a distinctive name. It is the "valvula foraminis ovalis," and I may venture to suggest for it the term *obturator valve*, as its office is to shut up the foramen at and after birth. According to most authorities, this valve begins to be developed towards the end of the third month. It is not until the end of the second month that the septa of the ventricles and bulbus arteriosus are completed, and then an imperfect auricular septum is formed, leaving the foramen ovale at its lower part. The true or defined foramen ovale is itself, therefore, not formed or marked out until, at least, after the end of the second month. Some have found the valve, at the end of the second month (Senac and Portal, according to John Reid), and in the fœtus now before the Society, which I have ascertained to be near the end of the third month, this valve is developed so far that it rises up above the middle of the foramen, and is as high as the free edge of the large Eustachian valve, which lies in front of it. At the end of the fifth month, according to Cruveilhier, this valve is large enough to cover over completely the orifice of the foramen ovale; and this will be seen to be the case in the fœtus between the fourth and fifth month now before the Society. The valve in its lower two-thirds grows from the sides of the foramen ovale, but, above, its horns pass upwards and outwards from the opening as far as 1-16th of an inch above it; and, when the valve is lifted or floated up, it is more than sufficient to cover over the foramen ovale.

We see, then, that as early as the middle period of uterine life, this valve, or obturator membrane, is so fully formed as to be capable of shutting up the foramen completely. At birth it is floated up by the reversed current, and applied against the back of the foramen; and, becoming united or glued to it, the septum auricularum is completed.

We can understand, then, how an imperfect development of this valve will give rise to the condition of open foramen ovale, as there is no other means by which the communication between the auricles can be closed; and if in any case we find this valve so undeveloped that, on being raised, it cannot shut up the foramen ovale, we may justly consider its non-development as a sufficient reason for the foramen ovale being open.

This imperfect condition of the valve may be attributed either to its simple non-development, as the growth of this, as well as of various other parts, may be withheld without any apparent physical cause; or it may possibly be due to obstruction at the orifices of the right ventricle. Were the latter condition to exist, thus rendering impossible the closing of the foramen ovale at birth, although its valve had been well formed, analogy would lead us to expect that the valve should present a thin and reticular appearance, like an ordinary wasting Eustachian valve, rather than appear simply small and undeveloped; but were the pulmonary orifice to become malformed or contracted during the two first months of fœtal life, before the time for the development or the valve had arrived, this might possibly influence the non-development of the valve, besides causing all the other appearances which the heart presented in the case I have related to-day. However, whilst contracted pulmonary orifice is undoubtedly a frequent concomitant, and, it may be, precedent, of open foramen ovale, it is at the same time by no means invariably or necessarily so. In examining cases of open foramen ovale, I would suggest that care be taken to examine especially into these two points:—
1. Whether there is contraction of the pulmonary or tricuspid

orifices, as compared with the aortic or mitral; and 2. Whether, with or without this, there is deficiency of the obturator valve, which, lying on the left aspect of the foramen ovale, should be found in its fully developed state more than sufficient, when lifted up, to shut up the aperture.

These, then, may be laid down as the two causes of the condition of open foramen ovale. In the one case, its obturator membrane may have been fully formed, but it is kept open by the blood which cannot find a free passage through the right side of the heart; in the other case, the membrane, which should be ready to close it up, is too small, and it remains open for want of any provision to close it; and again, these two conditions may be found to co-exist, as in the case I have related to-day.

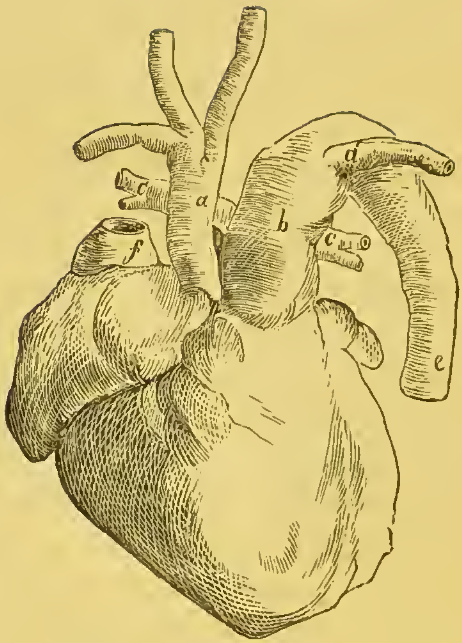
Use of the Foramen Ovale in the Fœtus, and result of its imperfect closure after Birth.—Another question in connection with open foramen ovale after birth is—Whether any mixture of the blood occurs, and if so, to what extent, and under what circumstances? It is well known that this foramen has often been found open to a considerable extent in those in whom such a condition was not suspected during life—I mean a much larger opening than the small oblique slit which is very often found at the upper part of the fossa ovalis. To understand this, let us first see how it is in the fœtus. I believe it is the common idea that the foramen ovale is merely a hole from one auricle to the other, and that the right auricle drives the blood through it to the left. But it cannot be so. There is no reason to believe that the auricles do not fill and contract together as in the adult, and so also the ventricles. Now the right auricle would require to contract first, were the left filled by it through the foramen ovale, and thus at the same time the right ventricle would be filled before the left auricle had time to contract and distend the left ventricle. We must conclude that the auricles and ventricles act synchronously as in the adult.

The fact is, that the lower cava fills the left auricle just as the upper cava fills the right, both auricles being filled during their conjoint diastole and repose; and it may be that, after all, this is the purpose designed by the mode of entrance of the lower cava and by the foramen ovale, as much as that the two currents should be kept separate, in order that the purer blood may go to the upper half of the body. Whilst either purpose separately would have required this arrangement, both are at the same time accomplished by it.

In the fœtus, then, the auricles being filled at the same time in this way, they contract; but no mixture can occur at this time, as the valve of the oval hole will not allow any blood to pass back from left to right. Previous to the third month this does not hold, as the valve is not developed; but until then it may be said that the two auricles simply form *one*, the lower cava opening to the left side, the upper to the right. But what I desire to demonstrate is, that when the parts are well formed, as during the two latter thirds of fœtal life, there is no exchange of blood between the two auricles from the mere existence of the foramen ovale; that the right auricle does not fill the left through the foramen ovale, nor does any regurgitate through it from left to right; but the left auricle is simply filled from the lower cava, as the right chiefly is from the superior cava.

Now, it appears to me that after birth it will be very much the same, when the foramen remains more or less open. Supposing that there is no contraction of the pulmonary orifice, which certainly would occasion the employment of the foramen ovale, still it appears to me that there cannot but be some amount of passage of venous blood into the left auricle, and this to a lesser or greater degree according as the valve is developed or not, as the lower cava still pours its stream in the direction of the aperture, through which it must partly pass, and encounter and mix with the blood still entering the auricle from the pulmonary veins. Still, the admixture occurs during the diastole and repose of the auricles, and is not from the cavity of the right auricle, but from the inferior cava; although this can make no difference on the symptoms or to the patient.

Even in those common cases where there is a small oblique slit only, I do not see but that there must be some amount of admixture. This occurs, it is well known, as often as in one out of every five or six subjects. There is usually (from the manner in which the valve shuts up the opening at birth) at the *upper* part of the fossa ovalis a recess, which occasionally presents a small unclosed passage admitting a probe or even a quill. Of this I have brought a preparation, now before the Society, where a common quill readily passes through a very oblique passage, and along with it is seen a corded condition in front of the oval fossa, and an extremely reticular condition of an old Eustachian valve; and there is in my museum a preparation, taken from a female aged sixty, in which the aperture is twice this size; also the Eustachian valve is large and strong. It is commonly remarked, that this condition can allow of no admixture on account of the obliquity of the passage. But, whilst I am aware that obliquity of perforation is productive of a very perfect valvular effect, as in the case of the termination of the ureters and bile duct, the circumstances are reversed here. The force which shuts the oblique passage—the passive flow of the pulmonary blood into the left auricle—is not stronger than, and is not so direct as, the force which tends to open it—viz., the current of the lower cava; and as the direction of this is exactly in the direction of the slit, it seems to me that there cannot but be, during each diastole, a small quantity of venous blood sent into the left auricle. This, however, can be only to a very trifling extent, and would not, perhaps, be worthy of notice, were it not for the sake of fully reasoning out the effect of the various conditions of open foramen ovale.



a, Ascending aorta, ending in right subclavian, and right and left common carotid arteries. *b*, Pulmonary artery. *c*, *c*, Branches to lungs. *d*, Left subclavian artery. *e*, Descending aorta. *f*, Vena cava superior.

VI.[1]

CASE OF MALFORMATION OF THE HEART AND BLOOD-VESSELS OF THE FŒTUS:

PULMONARY ARTERY GIVING OFF DESCENDING AORTA AND LEFT SUBCLAVIAN.

By DAVID GREIG, M.D.,

DEMONSTRATOR OF ANATOMY IN THE UNIVERSITY OF EDINBURGH.

FROM THE MONTHLY JOURNAL OF MEDICAL SCIENCE, JULY 1852.

During the summer of 1850, having procured a fœtus, which seemed to be about the ninth month, and which, on external examination, presented nothing peculiar with regard to form, colour, etc., I injected it from the umbilical vein. On examining the chest, I found the following arrangement of its blood-vessels:— The ascending *aorta* is small, passes straight up, and, after a course of about three-quarters of an inch, terminates in three branches of equal size, which are the right *subclavian*, and the right and left *common carotid* arteries. The *pulmonary artery* is very large, measuring about half an inch (5-12ths) in diameter, being nearly twice that of the ascending *aorta*, the diameter of which is one quarter of an inch. After a course of half an inch, the *pulmonary artery* gives off the branches which go to the

[1] The following case is by my demonstrator, Dr Greig. It is inserted here as it was published with the preceding paper, and describes a very rare and interesting malformation. The case originally formed part of the prize essay on the Fœtal Circulation, Session 1850-51, in my class, in which Mr Greig was a distinguished student. The preparation is preserved in my museum.

lungs, these arising from its back part and almost by a common trunk. A quarter of an inch further on, the *pulmonary artery* (or *ductus arteriosus*) gives off from its anterior and left side the *left subclavian.* The pulmonary artery, hitherto undiminished in size, now contracts rapidly to half its former size, and is continued on as the descending *aorta.* There is thus no communication between the ascending and descending aortæ. The right and left subclavian arteries appear of equal diameter. The venæ cavæ and pulmonary veins are normal.

Heart.—On opening the ventricles, they are seen to communicate by a large aperture, arising from a *deficiency of the septum ventriculorum* at its upper part. The right ventricle is twice the size of the left, though its walls are at least not thicker than those of the latter. The pulmonary artery arises normally from the upper and anterior part of the right ventricle. The aorta arises exactly above the communication between the two ventricles, its mouth being equally visible from either cavity. The aperture of communication is of a rounded form, measuring one-third of an inch in diameter, bounded below and on each side by a smooth thick edge, and above by the opening of the aorta. The semilunar valves of the aorta and pulmonary artery are well developed and normal. The tricuspid orifice is three times the size of the mitral orifice. The right auricle appears to be dilated, whilst the left is about one-third the size of the right, and a little larger than its corresponding ventricle. The *foramen ovale* is normal, and its valve is fully formed; but the passage remaining between the upper end of the foramen and the free border of the valve, appears to be considerably smaller than that which is usually seen in the full grown fœtus.

Remarks.—As far as I am aware, no case has yet been recorded in which the above remarkable abnormalities were combined. In the third volume of the "Library of Medicine," article "Malformations of the Heart," reference is made to two cases somewhat analogous. In the one, related by Sir A. Cooper, the pulmonary artery arose from both ventricles and furnished the descending aorta, the ascending aorta originating naturally In the second, related by M. Breschet, the left subclavian arose directly from the pulmonary artery. In the above case, however, both of these conditions are combined.

Had the malformation been confined to the blood-vessels alone, interesting inferences might have been drawn, with regard to the fœtal circulation. There was no communication between the ascending and descending aortæ; and this corresponds to the view commonly entertained, that the purer blood of the ascending aorta is distributed by the vessels which arise from the *arch*, whilst the descending aorta is filled with the less pure blood from the pulmonary artery. Again, it will be observed that the two superior extremities were supplied with blood from different sources, which, in the normal state of the heart, would have been of different qualities, whilst in no respect was there any difference in degree of development between the superior extremities. The additional complication, however, of such a large aperture between the ventricles, must have established complete admixture of the blood of the two sides of the heart, before it was sent into the aorta and pulmonary artery, so that, independent of subsequent abnormalities of the vessels, all parts of the body would be supplied with blood of the same quality.

Fig 1.

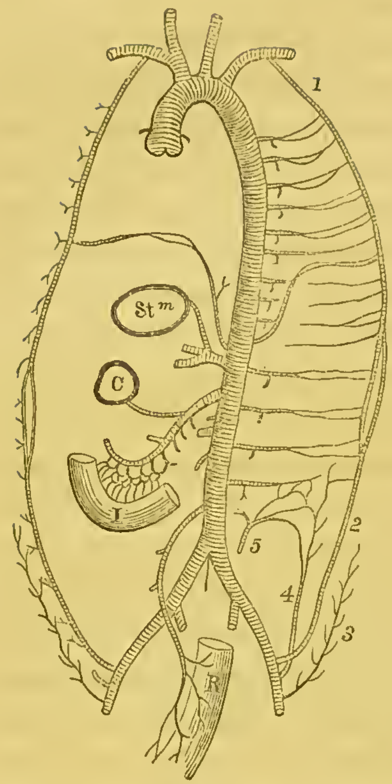


Fig. 2.

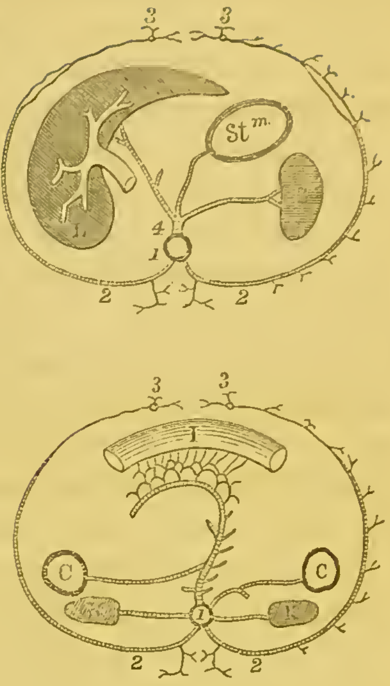


Fig 3

Fig. 1.—Longitudinal plan of the arteries of the trunk. The left side shows both the longitudinal and transverse anastomosing arteries of the wall; the right side only the longitudinal and diaphragmatic anastomoses belonging to the wall, and the various visceral arteries springing from the aorta.

Left Side.—1, Internal mammary; and 2, deep epigastric; connected behind to aorta by series of intercostal, lumbar, and diaphragmatic arteries. 3, Superficial epigastric. 4, Circumflex iliac. 5, Ilio-lumbar from internal iliac.

Right Side.—Branches of abdominal aorta, from above downwards; diaphragmatic, cœliac axis, superior mesenteric, right supra-renal and renal, right spermatic or ovarian, and inferior mesenteric.

Fig. 2.—Transverse plan of the arteries of the abdomen opposite to liver, spleen, and stomach. 1, Aorta, giving off from its back part the arteries (2, 2) for the wall, which unite in front with branches of the internal mammary arteries (3, 3). 4, Cœliac axis, coming from front of aorta, and supplying spleen, stomach, and liver, the latter also receiving the vena portæ.

Fig. 3. The same, lower down, showing portion of small intestine and sections of ascending and descending colon and of kidneys. 1, Aorta, giving off, behind, the lumbar arteries (2, 2), which join in front with branches of the deep epigastric (3, 3). In front, as if coming from the aorta at the same part, are shown the superior and inferior mesenteric, and at the sides the renal arteries. Fig. 1 shows the order in which these visceral arteries come off at different stages of the aorta, and their relative size.

The anatomist will notice that it is chiefly the anastomosing branches of the arteries of the wall which are shown, and also that these are represented proportionally larger than natural.

VII.

ANATOMICAL INQUIRY

INTO THE

MODE OF ACTION OF LOCAL BLOOD-LETTING IN AFFECTIONS OF THE INTERNAL VISCERA.

[Read before the Edinburgh Medico-Chirurgical Society, February 17, 1853; and extracted from the Monthly Journal of Medical Science, April 1853.]

My object in the following observations is, to inquire into the mode in which local blood-letting can be of service in affections of the internal viscera.

The practice generally followed in this country is, to apply leeches or to cup over the affected part, whether in the trunk or limbs; and it will be generally admitted that this is done with the view of affording special relief to the affected part, by the more or less direct withdrawal of blood from its distended blood-vessels,—that is to say, in following this practice it is generally believed that the blood letting is a *local*, and not a general, one.

Before proceeding with this inquiry, I shall assume two points as the groundwork of my argument. 1. That the relief in local blood-letting is through the blood-vessels or vascular system, and not through nervous agency; and 2. That, as blood can come from one part to another only by blood-vessels, there can be no special relief by local blood-letting, unless the blood-vessels of the one communicate or anastomose with the blood-vessels of the other.

It is evident, therefore, that this inquiry must be founded on a careful consideration of the arrangement of the blood-vessels of these parts.

In following out these principles in the endeavour to discover the *modus operandi* of local blood-letting, we notice that there is a wide difference between the local blood-lettings practised by the surgeon in affections of the limbs and external parts, and those by the physician in affections of the internal viscera. In the former parts, the blood-vessels communicate more or less directly from the surface inwards to the affected part, and we can therefore easily understand how leeches, applied, for instance, over the knee or ankle, can afford direct relief to the distended vessels in or about these joints. But in the latter parts there are no such communications. The internal viscera of the chest and abdomen on the one hand, and the walls which enclose them on the other, are supplied with blood from entirely different sources, and the two sets of blood-vessels do not communicate with each other except through the general circulation, or, what is the same thing, through the parent trunk; and it would therefore appear that we cannot specially relieve these internal viscera, or any one of them, by local blood-letting.

I shall now demonstrate this by referring briefly to the sources of vascular supply to the viscera and walls of the abdominal and thoracic and cranial cavities.

1. The Abdomen.—Here the *viscera* are supplied from behind, and solely by large arterial trunks, which spring from the front and sides of the aorta. The spleen, liver, pancreas, stomach, and duodenum, are supplied by the cœliac axis, the rest of the small intestines, and first half of the great, by the superior mesenteric artery, and the remainder of the great bowel by the inferior mesenteric, down near to the anus. The kidneys receive their blood entirely by the corresponding renal artery, direct from the aorta, and return it direct to the vena cava by the renal vein, whilst the blood returned from the other organs above named is again distributed to the liver by the vena portæ before it is returned to the vena cava.

Thus, to generalise, all the viscera of the abdomen receive their blood from behind direct from the aorta, and return it, directly or indirectly, to the vena cava,—an abdominal circulation by itself from the aorta to the vena cava, and that without any communication with the wall of the cavity, except indirectly through the parent trunks, the aorta and vena cava.

To this one exception must be noticed,—viz., the anastomosis along the rectum and down to the anus, by which the vessels of the perineum communicate freely with branches of the superior hemorrhoidal artery and vein; the artery communicating freely with the arteries along the intestine in the abdomen, whilst the vein communicates with the other pelvic veins, and, passing up, joins the splenic vein, whence its blood is conveyed to the liver. This arrangement has led to the French practice of applying leeches to the perineum to relieve the abdominal viscera, and, so far as anatomy is concerned, there are good grounds for its adoption, as indeed the only means by which a local blood-letting for the alimentary part of the abdominal viscera can be practised; but the further discussion of this point does not form part of the present inquiry.

The *wall* of the abdomen is supplied from altogether different sources, from above, from below, and from behind. Behind, from the aorta, come the lumbar, and lower intercostal, and diaphragmatic arteries. Below, from the internal iliac comes the ilio-lumbar branch, from the external iliac close to the groin, the circumflex iliac, and deep epigastric; and from the common femoral the superficial epigastric, from which the skin of the front of the abdomen is chiefly supplied. Lastly, from above, there is the end of the internal mammary branch of the subclavian.

All of these arteries are accompanied by veins which return the blood to the venous trunks, corresponding to the arterial trunks, from which the branches are supplied. Thus the abdominal wall is supplied with blood from the subclavian and iliac arteries, and from the aortic trunk itself; but these branches from the aortic trunk do not communicate with the visceral branches from the same trunk. It is evident, therefore, that any increased flow of blood along these parietal arteries cannot act on one visceral artery more than on another, nor indeed on any of these visceral branches, except simply by a diminution of the general current within the aorta.

The kidney is perhaps the best example with which to illustrate this argument. Lying in its bed of fat and cellular tissue, it has no vascular connections but by its artery from the aorta and its vein to the vena cava. It can, therefore, be reached and acted on through the renal artery and vein only, and these only through the aorta or vena cava circulating the general current, and any diminution of this general current could not tell on the current to or from the kidney more than on that to the stomach or the lower extremity.

There cannot, therefore, be a local blood-letting for the kidney; it can be influenced only through the aorta or vena cava—that is, through the general circulation.

The same remark applies to the other abdominal viscera already named, and we are, therefore, shut up to the conclusion, that, to these, blood-letting on the abdominal wall acts as a general, and not as a local blood-letting, as it must not only fail to act on any particular organ, but it does not appear how it can even relieve the circulation within the abdomen more than that of the hand or foot.

2. The Chest.—The same line of argument is applicable to the chest. The wall and viscera are supplied from different sources, and the blood-vessels of the two do not communicate in their distribution.

The *heart* is supplied solely by the two coronary arteries, which arise from the aorta immediately above its valves. We can, therefore, have no special command over the supply of blood to the substance of the heart.

The *lungs* are entirely separated from the wall by the pleural cavities, being attached only by their root, through which the blood-vessels pass. The lung has two sets of blood-vessels, the large pulmonary and the small bronchial arteries and veins. The former we assume to be concerned chiefly in the conditions referred to as congestion of the lung and pneumonia, whilst the distribution of the bronchial arteries renders the inference a fair one, that they are the vessels concerned in affections of the bronchial membrane,—that is, the one set of vessels appear to be chiefly concerned in pneumonia, and the other in bronchitis.

Now, it is evident that blood taken from the wall of the chest cannot drain or derivate from the pulmonary system of vessels, but can influence them only by acting through the general circulation, and that by lessening the quantity of blood to be returned to the right side of the heart and thence sent to the lungs. But it may be said, that the pulmonary vessels may be relieved by means of their anastomosis with the bronchial vessels. As to the nature or even existence of such communications between the two sets of vessels of the lung, anatomists are not quite agreed, although the result of minute injections has led most to believe that the deeper set of the bronchial vessels, ramifying on the smaller bronchial tubes, do communicate with the pulmonary capillaries or veins. Very little weight can be attached to an explanation depending on this obscure anatomical point; but supposing, for the sake of argument, that the pulmonary veins could be drained to a slight extent by drawing blood from the bronchial veins, this again could be of no service unless these veins themselves can be locally acted on from without. Is it, then, in our power to relieve the bronchial vessels by local blood-letting? The arteries are usually three in number,—two on the left side, which spring from the thoracic aorta, and usually one for the right side, which may arise either from the aorta, or in common with or from the first aortic intercostal artery of this side. In such an argument it is scarcely worth while to mention the occasional varieties of these arteries in their origin, nor probably would physicians be disposed to explain any supposed benefit from local blood-letting by referring to the occasional or frequent origin of the right bronchial artery from one of the arteries going to the wall. The bronchial arteries, then, must be regarded as deriving their blood direct from the aorta, so that they could not be relieved either by draining or derivation in local blood-letting to the wall, this serving merely to draw off so much more blood from the general current of the aorta, exactly as in the case of the abdominal aorta in leeching the abdominal wall. Nor could local blood-letting to the side or back relieve the bronchial veins. These end on the right side in the vena azygos, on the left in the left superior intercostal vein, on its way to terminate in the left innominate or in the lesser azygos vein. Now these same azygos and superior intercostal veins receive the blood returned from the side and back of the chest by the ordinary intercostal veins. But, in order to relieve the bronchial veins through the intercostals, the current in the latter would require to be reversed, which local blood-letting would scarcely produce, even although the intercostal veins were not, as they are, provided with valves.

The *wall* of the chest is supplied from various sources, chiefly by the aortic intercostal arteries, which spring separately from the back part of the aorta. The other arteries come chiefly from the subclavian. The superior intercostal supplies the first intercostal space; the internal mammary the anterior part of the thoracic wall, coursing down behind the cartilages of the ribs; the posterior scapular partly supplies the surface of the upper part of the back, where, however, the chief vessels come from the posterior divisions of the aortic intercostals; and lastly, the axillary aspect of the chest and the pectoral region are supplied from the outside by the thoracic branches of the axillary artery; and all of these arteries are accompanied by corresponding veins.

When, therefore, blood is drawn from any part of the thoracic wall, it comes either from the subclavian or axillary arteries, on the one hand, or, on the other, the thoracic intercostal arteries, the latter springing from the back part of the aorta, so that any increased flow by them would simply diminish the general volume and current within that vessel.

We see, therefore, that the vessels of the lungs can be relieved through the general circulation only, either, on the one hand, by a little derivation from the general current in the aorta, but which will not relieve the lungs more than the lower extremities; or, on the other, by lessening the quantity of blood returned by the veins to the right side of the heart.

I think, then, it has now been sufficiently shown that local blood-letting on the chest cannot act on the heart and lungs except as a general blood-letting, and that there is, therefore, no such thing, in the proper sense of the term, as local blood-letting for these organs.

Reflections of the Serous Membranes from the Walls to the Viscera.—To render complete the demonstration of the absence of communication between the blood-vessels of the walls and those of the viscera of the chest and abdomen, it is necessary that I should notice the connection between the viscera and the wall by means of the reflections of the serous membranes, as it might be supposed that by these a means of affording local vascular relief is established.

In the abdomen the peritoneum is reflected from the wall on to the viscera at various places. On each side it is reflected on to the ascending and descending colon, so as to leave their posterior third or fourth uncovered and in contact with the posterior wall. But it is not the conductor of vessels between the wall and the colon. It possesses its own nutrient vessels only, and the vessels of the colon are derived from the colic branches of the mesenteric arteries which do not anastomose with the vessels of the wall, as they run outwards behind the peritoneum to reach the intestine.

The liver is connected to the diaphragm by means of its suspensory and posterior ligaments, each consisting of a double reflection of peritoneum; and these, as described by Harrison, contain minute blood-vessels, derived from the internal mammary and diaphragmatic arteries. But these blood-vessels are minute, and their distribution is very limited, supplying at most but a small region of the coats of the liver where it is joined by its serous ligaments; and I am not aware that any connection has ever been demonstrated by injection between these minute vessels and the proper internal blood-vessels of the viscus. Besides, these vessels come from the diaphragm only, the vessels of which again are but very remotely connected with those of the skin of the trunk; and, even supposing that these minute vessels did join with those of the liver, physicians will probably not be disposed to look to them as affording an explanation of the relief from local blood-letting, any more than to the additional fact, that often one, or occasionally both, of the diaphragmatic arteries may arise from the cœliac axis, instead of separately from the aorta; or to attribute the benefit derived from leeching the epigastrium, in a supposed affection of the stomach, to any minute anastomosis that might obtain between the blood-vessels of the diaphragm and œsophagus, as the latter is passing through its aperture in the former, an inch previous to its termination in the stomach.

In the chest the pleuræ are reflected from the wall to the viscera in the formation of the mediastina. On the lung the pleura is supplied as part of the lung, and on the wall as part of the wall; and it might be supposed that relief might be conducted from one to the other along these reflections. But I am not aware that any part of the pleuræ is occupied by conducting vessels, or supplied with more than it requires for its own nourishment, these being derived from the vessels of the part on which the membrane is lying. The reflection forming the posterior mediastinum is the shorter of the two. It is supplied with nutrient arteries from the thoracic aorta. The pleura forming the anterior mediastinum is supplied from the mammary arteries. It then passes on to the pericardium, and behind this it joins the pulmonic pleura by a narrow union only, as it envelops the root of the lung. But there are here no conducting vessels that I know of; none beyond those which enter the membrane to nourish it; and even if taking blood from the front of the chest did, by derivation, cause a determination along the pleura of the anterior mediastinum, the relief would be to the pericardium.

The same view applies to the reflection of the serous pericardium on to the great vessels near the heart; and again to the reflections of the arachnoid membrane, near the free margins of the falx cerebri and tentorium cerebelli, around the cerebral veins as they pass across to the longitudinal sinus, and around the nerves at the base.

The reflections, therefore, of the serous membranes cannot be regarded as establishing a medium through which the blood-vessels of the viscera can be relieved.

3. The Head.—The question here is, can the vessels of the brain be specially relieved by local blood-letting on the head?

We observe that here also there is a serous cavity between the wall and the contained viscus. The parts internal to this cavity are supplied by the internal carotid and vertebral arteries; whilst those outside, viz., the dura mater, the skull, and the external parts, are supplied by the external carotid. Now the arteries of the brain and pia mater do not communicate with those outside the arachnoid cavity, but it is different with the veins. I do not here refer to any minute anastomosis between the periosteum on the outside, and the dura mater, or internal periosteum, on the inside, whilst the latter at the same time lodges the returning veins from the brain; nor to the venous canals in the diploe, for these chiefly open internally; but I refer to the direct communications, by means of what we term the emissary veins, which occupy the parietal and mastoid foramina. By these veins the longitudinal and lateral sinuses may directly empty themselves into the veins of the scalp. This is more especially the case with the mastoid communication, which may be looked upon as a kind of safety opening by which the lateral sinus may be relieved; and this sinus, it will be recollected, receives the entire blood from the brain, and also from the orbit.

There is another communication between the veins of the scalp and the cranial sinuses, by means of the ophthalmic vein, which externally communicates freely with the veins of the eyebrow and temple, and internally joins the cavernous sinus; but to afford internal relief by this communication, the natural direction of the current in the ophthalmic vein would require to be reversed, which probably could not be effected by anything less than a free opening in the angular vein.

It would appear, then, from anatomy, that, by leeching the scalp, especially behind the ear, the sinuses of the dura mater and veins of the brain may be relieved, and that as directly, though not so rapidly, as by the method of opening the external jugular vein at the root of the neck.

The common practice of cupping on the back of the neck, with the view of specially relieving the brain or head, is no doubt generally, or in great part, efficacious, simply as a slow or mild general blood-letting; but is at the same time not without anatomical recommendation as a local blood-letting. The mastoid communication from the lateral sinuses is to the occipital veins, and these communicate freely with the veins of the back of the neck; and also the arterial supply to the back of the neck, derived from the deep cervical and occipital arteries, has free communications with the posterior muscular twigs which the vertebral arteries give off during their ascent to the brain.

I do not, therefore, propose to include the brain amongst the internal viscera which cannot be specially relieved by local blood-letting, although at the same time it is not necessarily so from the mere fact of its lying inside opposite to where the treatment is applied externally. Besides, we must recollect that here also there is the uncertainty of diagnosis in many cases, so that the affection we are treating may be situated either partially or entirely outside the arachnoid cavity; and even did no communication whatever exist between the blood-vessels of the internal and external parts here, so that no relief could take place by direct draining, it by no means follows that the practice of applying leeches to the scalp may not afford special relief to the brain, on the principle of derivation, or lessening the arterial currents to the organ within.

Before leaving this part of the subject, it is well to remark, that in these considerations I have not overlooked the question as to the non-variation of the quantity of blood, or at least of fluid, within the cranium. It would be out of place for me to enter on this question here, and I shall, therefore, merely remark, that, whilst I am one of those who cannot understand how the *quantity* of fluid within the cranium can vary, until it can be proved that the brain can be compressed, *i.e.*, diminished in bulk, by any force that the heart is capable of exerting, I at the same time hold, that we ought not to allow this question to influence our practice. Whichever view be correct, we have still the same principles to contend with in practice; we have, in both, increased or diminished *pressure*, and, in both, increased or diminished *rapidity* of the circulation; and it therefore appears to me that those are in error who suppose that this question should influence our practice or our reasoning thereon.

It appears, then, that local blood-letting cannot affect the viscera in the abdomen or chest, except through the general circulation; and that, therefore, these blood-lettings are general and not local, according to the ordinary sense in which the term local blood-letting is employed.

Modes in which Local Blood-letting may act.—But, although it be granted that leeching the wall cannot draw blood directly from the affected discus, some may be disposed to hold that it may still afford to it special relief by what is called revulsion or *derivation*, this being the principle, or supposed principle, which guides those continental practitioners who apply the leeches not over or close to any affected part, but at a distance from it. By these terms they mean to express the view, that the result of local abstraction is to draw more blood to the bleeding part, and thus diminish the supply elsewhere.

Without meaning to dispute the correctness of this view within certain limits, it may be remarked, on the *modus operandi* of all local blood-lettings, that they may act in two ways. 1. By the direct abstraction of blood through the anastomosing vessels between the affected and the bleeding part, and it matters not whether this be by the veins or the communicating arteries: this we may call *draining*. And, 2. By lessening the quantity *going to* the affected part, by creating an increased flow or determination elsewhere. This is the revulsion or derivation of authors, and we may call it *counter-draining*.

Now it is evident that to be of local and special benefit, this practice must not be applied too far from the affected part; the farther it is, the more it will lose its effect, and degenerate, so to speak, into a general blood-letting. It is probable that in most true local blood-lettings there is relief in both of these ways; but can it be so in the case of the abdominal and thoracic viscera?

That there can be no draining I have already shown; and it appears to me, that neither can there be any derivation or counter-draining from these viscera. The blood taken from the abdominal and thoracic walls comes from the iliac and femoral, or subclavian and axillary arteries, or from the back part of the aorta. Now this cannot lessen the circulation into the visceral arteries more than into those of the leg or arm. Nor can the effect which it has on the general current within the aorta bear more upon one viscus than another.

It must therefore be held, that the abstraction of blood from the wall of the abdomen or chest cannot in any way, or according to any principle, be regarded, as far as specially influencing the contained viscera or any one of them is concerned, in any other light than as a general blood-letting.

Conclusions.—I next proceed to inquire whether these considerations should lead us to alter the common practice of blood-letting in affections of the internal viscera?

Now, notwithstanding what has been said, it is nevertheless not difficult to show, that the prudent and proper practice still is to apply the treatment over the affected part. This is because we cannot be sure of the exact situation and limits of the disease. There is pain as if in the liver. Is the affection one of the liver itself, or of one of the serous layers of the diaphragm, or of the parietal peritoneum? There is pain as if in the kidney. Is it in the kidney, or in some part of the wall, or in both? We cannot be sure, and therefore we must treat the affection as a regional one—as an affection of, or in, the right hypochondriac region in the one case, and of the lumbar region in the other. If the viscus alone be affected, the blood drawn from over it will at least relieve it *as much* as a similar quantity taken from some other part would do; and, if the wall also should be affected, or be the entire seat of the disease, the patient, without running the risk of missing a better practice, has had the benefit of the doubt.

The same remarks apply to the chest. Local blood-letting must have the same direct effect in pleuritis that it has in peritonitis, the membrane in either case forming part of the wall; and can the practitioner be sure that the parietal pleura is not involved as well as the lung? The pericardium, again, receives part of its vascular supply from the internal mammary arteries, and it is therefore in our power to derivate from this part by leeching the front of the chest.

We must therefore come to the conclusion, that, in treating affections of the viscera of the abdomen and chest, it is best to take the blood from over the painful or affected part, grounding, however, the localization of our practice, not on the vague belief that we can thereby specially relieve the viscus, but on the uncertainty of diagnosis, which leaves us unable to say whether the wall also is affected or not.

But whilst this may be laid down as the general rule, which may be safely followed by all in all cases, it is at the same time to be remarked, that cases may occur in which it is the object of the practitioner simply to lessen the quantity of blood in a congested or over-active viscus; and after he has duly exercised his judgment in the matter, I think enough has been said to prove that *then* he may cup or leech on any part he chooses, or instead, take an equal quantity from the arm, or from a vein in some other situation, resting assured that it is only on the principle of a general blood-letting that these internal viscera can be acted upon.

It may be urged that experiments of this kind are not warrantable;—first, Because, on my own showing, the blood-letting from over the painful part will prove at least equally beneficial with one from any other part, whilst the rule is simple as a general guide to the practitioner, besides being more likely to ensure the confidence of the patient; and, second, Because experience has led to the adoption of the local practice. To this it may be answered, that experience has by no means proved the superiority of the local over the general blood-letting in affections of the internal *viscera*. We can readily understand how experience may have established the superiority of the local blood-letting in affections of the chest and abdomen generally, because we know that in a large number of them the pleura and peritoneum are involved to our certain knowledge; whilst in those cases where we with equal certainty know the viscera to be affected, we do not know but that the affection may also in part involve the wall. But experience has not yet declared that two or four ounces of blood taken from over the kidney, or liver, or lung, will afford more relief to these viscera than two or four ounces taken from the hand or foot, or by venesection from the bend of the arm. All that experience can be held to have declared is, that a certain quantity of blood taken there is productive of a certain amount of benefit; but experience has yet to declare, whether similar benefit may not be derived by drawing the same quantity of blood from some other part.

The advantages that would attend this alteration in practice in such cases would be the substitution of a more simple and less expensive procedure, as by taking the blood from a small opening in some vein, as for instance, on the back of the hand or foot; or, if leeching or cupping be still followed, it could be done without the lengthened exposure of the chest and abdomen, during which the patient is exposed to the influence of cold.

I say, therefore, with reference to the abdominal and thoracic viscera, in a case in which the practitioner feels assured that the affection is *limited to the organ*, it would appear that it matters not where or how he bleeds, according to circumstances he may take his choice, both of the part and the mode, feeling assured, that from whatever part he takes the blood, whether by the leech, the cupping-glass, or the lancet, he will reach the affected organ only by having withdrawn so much blood from the general circulation.

The same line of argument, it is evident, may be pursued with regard to the other parts of local treatment in affections of the chest and abdomen, more especially to counter-irritation, only, however, in as far as this and other remedial means have their action through the blood-vessels and the circulation. But the farther discussion of this matter would lead me beyond my purpose, which was simply to demonstrate from anatomy the conclusions which I have already drawn.

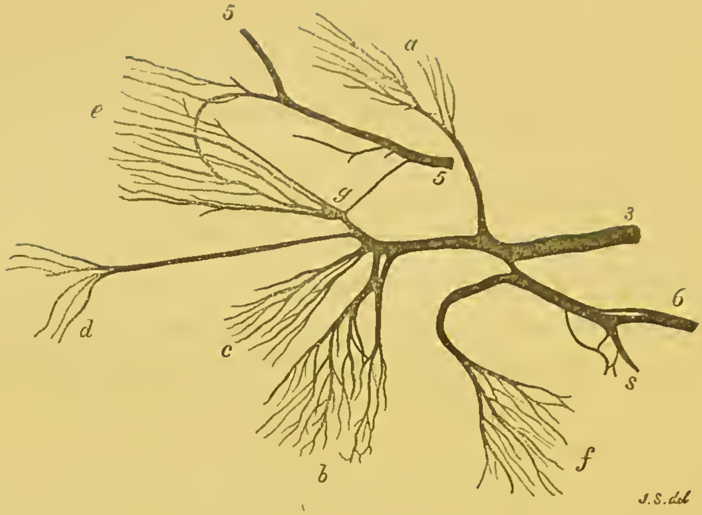
[NOTE.—*March* 1854.—The satisfaction which I have derived from the favourable reception of the above paper at the Medico-Chirurgical Society, and since its publication, has been lessened by finding some to be under the impression that I had written to prove that local blood-letting was of no use in affections of the abdomen and chest, and, consequently, that the practice of the present day was wrong. But a perusal of the paper will show that I have come to the exactly opposite conclusion,—that "*the prudent and proper practice still is, to apply the treatment over the affected part.*"

The conclusions I have drawn on anatomical grounds, which are not matters of opinion, but matters of fact, may be shortly stated thus:—

1. Local blood-lettings on the chest or abdomen are truly so, as far as the *wall* is concerned, but can act upon the *viscera* only on the principle of general blood-letting.

2. But that it is proper to continue the local practice, because the wall is often affected along with the viscus, to our certain knowledge, and we are often unable to decide whether the wall is not in whole or in part the seat of the disease.

3. And that, therefore, the physician is not at liberty to depart from the local practice, unless he could be certain that the affection was limited to the viscus.]



I have preserved the nerves from both orbits, as dried preparations, and the above is an exact copy of those of the left side, the individual filaments being also represented.

6. Sixth nerve, split at its union to (*s*) the sympathetic, and ending in filaments (*f*) to the external rectus muscle.

3. Third nerve, joined to the sixth by the communicating branch which existed on this side.

(*a*) Upper division of third nerve ending in filaments to superior rectus and levator palpebræ.

(*b*) Branches of third nerve to internal rectus.

(*c*) Ditto to inferior rectus.

(*d*) Ditto to inferior oblique.

5. Nasal branch of fifth nerve, giving long root of ganglion, and long ciliary nerves.

(*g*) Ophthalmic or ciliary ganglion; receiving short root from the branch of the third to the inferior oblique, and long root from nasal branch of fifth; and giving short ciliary nerves.

(*e*) Short and long ciliary nerves, with an anastomotic loop between two of them. On this side, seventeen ciliary nerves were dissected where they pierce the eye.

The third nerve is small relatively to the sixth and nasal, as this was the paralysed side. Also the ciliary ganglion is now shrunk a little from the original size, and appears smaller than that body usually is when recent and moist.

Dissections of the nerves of the orbit, and especially of the ganglion and its connexions, may be easily preserved as dried preparations; and an instructive series may be thus conveniently put up. After being carefully removed, they are to be laid out wet and placed correctly with needles, or the point of the scalpel, on paper; and when dry, they are easily raised stiff, and gummed down on board. I have many such preparations, amongst others, one of all the nerves of the orbit, with portions of the muscles, from the calf, laid down in this way, showing also the fifth nerve supplying filaments to the muscles of the eye.

VIII.

DISSECTION OF THE ORBITS

IN A

CASE OF PARALYSIS OF THE COMMON MOTOR OCULI NERVE; WITH REMARKS ON CERTAIN VARIETIES OF THE MOTOR NERVES.

[Read before the Edinburgh Medico-Chirurgical Society; and extracted from the Monthly Journal of Medical Science, July 1853.]

I have thought it worth while to give an account of this case, as I had the opportunity of observing it carefully in its various stages, and of making a complete disseetion of all the parts concerned.

The patient was a man aged 73. When I saw him first, on September 16, 1852, the following symptoms presented themselves in connection with the left eye:—The upper eyelid was completely fallen, and without any motion. When the lid was held up, the eye was seen to be a little prominent and a little everted. The eye could not be moved upwards or downwards or inwards, but it could be moved freely in the outward direction. The rotatory motion described by Dr Jacob as visible in such cases, was distinctly seen when the patient was directed to look downwards. It is a delicate rotatory motion, in a direction to bring the top of the eye round towards the inside, and perhaps also a little forwards, the whole being like a vibration of the eye in that direction.

The pupil was enlarged to about twice the size of the other, and also fixed, even under the strongest light, whilst the other moved readily. The vision of the eye was good, and apparently unaffected by the attack, and its common sensibility was unimpaired.

The falling of the eyelid was noticed a fortnight before, one morning, after he had been sitting in the open air the previous evening, in his usual health.

The diagnosis was evident. The unaltered vision and unimpaired common sensibility of the eye and forehead, showed that the optic nerve and the fifth pair were unaffected. The power to move the eye outwards, and the somewhat everted position, showed that the sixth or abducens nerve was not involved. The loss of power was in the parts supplied by the third or common motor oculi nerve,—the levator muscle of the lid, the superior, inferior, and internal recti muscles, and the fibres of the iris by which contraction of the pupil is effected.

We have no very decided indication of the result of paralysis of the inferior oblique, the remaining muscle supplied by the third nerve. The rotatory motion already referred to may, however, be looked upon in this light. According to the view, which I have brought evidence to establish, in my papers on the muscles and nerves of the eye, that the use of the oblique muscles is to turn the eye more or less directly on its antero-posterior axis, we can explain this rotatory motion on the supposition that it is occasioned by the action of the superior oblique, unopposed by the action of the inferior oblique, which must be paralysed along with the other muscles supplied by the third pair.

The treatment in this case consisted in the application of blisters to the nape, and a mild mercurial course, with the use of occasional laxatives, under which a gradual improvement of the symptoms connected with the paralysis took place. Towards the end of October (two months after the commencement of the attack) the eyelid could be raised about a third, and there was evident power of inversion and depression, but the pupil still remained dilated and fixed.

Soon after this the iris also began to regain its power of contracting the pupil. By the middle of November the pupils were equal, and equally contractile, the left having returned to the original size of the right. The eyelid could be raised so as to show half of the cornea. The inferior rectus had regained nearly all its power, the internal rectus acted moderately, and the superior rectus least. The rotatory motion was not now perceptible.

The improvement on the motions followed the mild action of the mercury on the gums. An indolent ulcer formed on the cornea of the other eye, and there was considerable chronic ophthalmia, especially of the right eye, with flaky discharge, and also some ophthalmia tarsi; all of which improved under the use of the nitrate of silver and the red oxide of mercury ointment.

It will be observed, that the ulceration and chief inflammation affected the right eye. Had these occurred on the left or paralysed side, the erroneous inference might have been drawn, that the paralysis had injured the nutrition of the eye, as is known to occur when the fifth pair of nerves is paralysed.

The eye symptoms continued to improve up to December 5th, when my brother was suddenly summoned to see the patient on account of an apoplectic seizure. He was not completely paralytic or comatose, but gradually became so, and died next day, about thirty hours after the seizure.

The post-mortem examination had an additional interest from the previous history of the patient. About ten years before, he had an attack of hemiplegia of the right side, from which he recovered in a few weeks, so as to be able to walk, but it was longer before he regained the use of the hand for writing. For several years back, although he has had no actual paralytic seizure, there were several threatenings of them; indeed, on one occasion he was found insensible, but recovered in a few hours after free venesection. Formerly, he was a man, well known in his day, of strong and active intellect, though wanting in the higher qualities of mind. For several years back he has, however, been an invalid, and latterly partially imbecile. Long under the care of my brother in Leith, he was also much indebted to the kind attentions of Dr Alison.

Post-mortem examination 48 hours after death. Present—Dr James Struthers, Mr Doig, and myself.

Brain.—About four ounces of serous fluid escaped when the brain was being removed. Convolutions slightly flattened. Lateral ventricles distended by clear serum, the right to three times, the left to twice the natural size. The foramen of Monro nearly admitted the point of the little finger. Pacchionian bodies large, and pia mater generally slightly congested. The whole of the cerebral hemispheres and cerebellum were carefully sliced, but no trace of former or recent disease could be seen. After the removal of the arteries at the base, and the escape of all the fluid, the brain weighed 51¾ ounces.

We next examined the *nerves at the base.* Pia mater between crura cerebri a little thickened and opaque, as if from infiltration of serum, but there was no lymph. Trunks of internal carotid arteries slightly atheromatous. At its origin, it is known that the third nerve lies close between the posterior cerebral and superior cerebellar arteries, so close that one of the old anatomists supposed the heaviness of the eyes in sleep to be due to the distended arteries pressing on the nerve. Now, the third nerve on the right side seemed so closely embraced by these two arteries, that had this been the paralysed side, it might have been supposed to have been the cause of the paralysis; but on the paralysed side the arteries lay at some distance from the nerve. All the nerves, including the other two motor oculi nerves, were healthy and equal on the two sides, except the third or common motor oculi nerve. On the left side it was fully one-third smaller than on the right, and was of a brownish-yellow colour, all the others being of a pure white. The origin of the third nerves was carefully examined at and below the surface, but no difference could be detected, nor was any trace of disease apparent in the crura cerebri, pons, or quadrigeminal bodies. Under the microscope, the nerve tubes from the left third nerve were about half the size of those from the right, also less distinct, and proportionally more mixed up with filamentous tissue.

Dissection of the Orbits.—The straight muscles supplied by the paralysed nerve were evidently somewhat wasted, and on weighing their fleshy portions, were found to be lighter than those on the right side; the levator palpebrae one grain; rectus superior, and rectus internus, each half a grain; rectus inferior two and a half grains, less than on the right side. Each oblique muscle seemed to be of equal weight with its fellow on the other side; and the left external rectus weighed one grain more than the right.

I made a careful dissection of all the nerves of both orbits, but will confine myself here to a general notice of these. The trochleator and abducens nerves were healthy and of equal size on the two sides. The common motor oculi nerve generally was smaller on the left than on the right side, both on the cavernous sinus and orbit. The upper was about one-half, and the lower division about a fourth or fifth smaller than on the right side.

The ophthalmic ganglion and ciliary nerves were examined. The ciliary nerves appeared equally large on the paralysed side. On the left side seventeen, and on the right fifteen separate ciliary nerves were dissected entering the eye, eleven of these on each side being short ciliary, or derived from the ganglion. The ganglion on the left side was of the usual size and form, but on the right side it appeared to be smaller. This was perhaps due to its irregular shape. It was elongated more upwards than forwards, and the lower anterior angle was prolonged some distance before it divided into nerves. Whilst the long root resembled that of the other side, the motor root on the right side was shorter than usual. On the whole, it had at least the appearance of being smaller than the other, and smaller than the ganglion usually is, and had this been on the left side, we might have concluded that the ganglion had, as we should expect, wasted under the paralysis of the third nerve.

The result of the dissection may be summed up briefly, that death was occasioned by what is usually termed serous apoplexy. That the third nerve generally, and the muscles supplied by it, were somewhat wasted, and that no special cause could be found, the serum effused at the base not lying more in the way of the left nerve than the right. The motions having returned to a considerable extent, we could hardly expect to find anything more, and the case may be regarded as one of those usually designated *rheumatic*, and probably arising from exposure to cold, like the more common cases of paralysis of the face, and as in another case of paralysis of the eye, which I saw lately. The attack in this case also came on suddenly after exposure to cold. The whole of the common motor oculi nerve is paralysed, especially the upper division, there being only slight power in the parts supplied by the lower division of the nerve. The sixth nerve also is completely paralysed, and probably also the fourth, as the eye remains straight and motionless. Vision is good, at least when the patient is made to look through a small hole in a card, to make up for the enlarged and paralysed condition of the pupil. This patient, however, has no other complaint, and is likely to do well, as may be expected of most of those cases in which the paralysis is local, and has come on suddenly after exposure to cold.

Remarks on an occasional Connection between the Third and Sixth Nerves, and supposed Influence of the latter on the Iris.—In the course of the dissection, I met with an interesting variety in the anatomy of the motor nerves on the paralysed side. The third and sixth nerves were joined to each other in the cavernous sinus by a large communicating branch, and the third nerve close behind and at the ciliary ganglion supplied a distinct white nervous filament to the external rectus muscle.

As this case might be added to the list of cases in which it has been said [1] that the sixth nerve gave a branch to the third, and thus was enabled to move the iris after the third nerve was paralysed, I may take the opportunity of examining into this view.

A connection, more or less direct, between the sixth nerve and the ciliary ganglion has been noticed by various anatomists. Otto [2] found the nasal nerve, which furnishes the long root of the ganglion, coming from the sixth. Petit [3] says that the sixth nerve may join the third or the nasal of the fifth, and thus send filaments to the ciliary ganglion. Longet has once seen a very distinct branch from the sixth nerve to the ciliary ganglion; and states that Mr Grant of New York has informed him he has seen

[1] Longet. Anat. and Phys. du Systeme Norveux, ii. p. 388. Also Kirkes' Hand-Book of Physiology. 1851. P. 439.

[2] Valentin de Functionibus Nervorum, p. 114.

[3] Longet Op. Cit, p. 380.

it several times, having been led to the research by observing a case of paralysis of the third nerve in which the motions of the pupil remained, and, on dissection, a branch was found proceeding from the sixth nerve to the ophthalmic ganglion.

Again, a direct communication between the sixth and third nerves has been noticed by Munnicks,[1] and Cruveilhier[2] states that it appears to him there is a communication between these two nerves in the cavernous sinus.

The case now under consideration furnishes the third instance in which I have met with this variety. The first two occurred in 1844. In both it existed on one side only. The preparation of one of these is still preserved, and shows a distinct filament from the sixth nerve entering the commencement of the lower division of the third nerve. In the present case the communication was very distinct on the left or paralysed side. As seen in the woodcut, p. 94, it was by a short flattened band about a line in length, and about half the size of a healthy upper division of the third nerve. It passed nearly at right angles between the nerves, joining the trunk of the third nerve one-tenth of an inch before its division. It had much the appearance of a sympathetic nerve. Rather more than half an inch behind this, the sixth nerve was joined to the sympathetic by one large and two smaller twigs. The third nerve on this side was not joined to the sympathetic by any separate branch; but on the right side it was joined by three good sized sympathetic filaments, just at the place where, on the left side, it was joined by the band from the sixth. On the right side, farther, there were six smaller twigs and one large one between the sixth and sympathetic; and this double union of the latter to the right sixth and third nerves, gave the false appearance of a communication between these two motor nerves themselves.

I am disposed to look upon this occasional apparent connection between the third and sixth nerves, as chiefly, if not entirely, composed of sympathetic, taking this mode of reaching the third nerve, along or through the sixth, which constantly forms a large

[1] Longet, Op. Cit., p. 380.

[2] Descriptive Anat., p. 1106.

communication with the sympathetic. The connection in the present case between the sixth and third nerves, further explains the apparent anomaly of the third nerve supplying, close behind and at the ciliary ganglion, a twig to the external rectus; and that this apparent union between the third and sixth nerves did not affect the relative distributive influence of either, is evident from the fact that the parts supplied by the third nerve, not excepting the pupil, were entirely paralysed, whilst the external rectus continued to act. Had the motions of the pupil continued in this case, the erroneous inference might have been drawn, that the iris received its innervation from the sixth nerve, instead of simply concluding that the part of the third nerve which goes to the iris had not been involved in the disease, or that the nerve could still act reflexly in obedience to the stimulus of light, after there was no longer any power to use it under the influence of the will.

It has been suggested that the existence of the ciliary ganglion might explain the occasional continuance of the motions of the iris in palsy of the third nerve;[1] but were this the reason, the motions of the pupil should always or generally continue, instead of rarely so. We have no evidence to show that the ciliary ganglion is a centre for reflex motion on the iris, or that it in any way governs the motions by which the pupil is either contracted or dilated, although we know that, in some animals at least, the sympathetic in the neck appears to have such an influence. We are, indeed, as yet quite ignorant of the uses of the ciliary ganglion, knowing only, as facts, that it is present in all animals which possess a movable pupil, and that we have not the same voluntary power over the iris as over the other parts supplied by the third pair of nerves; from which, however, it does not necessarily follow, as has been supposed, that the use of the ganglion is to intercept the transmission of voluntary influence.

As to the apparent varieties which anatomists have from time to time observed among the nerves of the orbit, it may be remarked that no such observation is of any physiological value

[1] Dr Brinton, Cycloped. Anat. and Phys., vol. iv. p. 622.

unless a complete and careful dissection is made of the nerves in the whole of their course. A motor filament may appear to come from the fifth pair, or a sensory filament from one of the motor nerves, but on tracing them back through the sinus, it will be found that the filament had left the parent trunk to accompany and then leave the other. This I have often found. Hence the explanation of the common description of the lachrymal nerve arising in whole or in part from the fourth nerve, hence also the supposed cases of union between the third and sixth nerves, and the anomalous origins of ciliary nerves and roots for the ciliary ganglion. It would be a greater physiological deviation to find the sixth nerve really supplying the ciliary ganglion and moving the iris, or the third nerve supplying the external rectus and abducting the eye, than to find the portio dura of the seventh pair supplying the masseter muscle, or the motor portion of the fifth pair the orbicularis palpebrarum. These varieties may be illustrated by one we occasionally meet with in the neck, in the origin of the descendens noni nerve from the vagus, or in the descendens noni furnishing a branch to the heart. Physiologically these appear strange deviations, until we recollect that the vagus and hypoglossal nerves lie close together after they leave the cranium, and frequently there present an intimate union with each other.

These remarks are not intended to question the occasional existence of irregular twigs crossing between the nerves of the orbit in the anatomical sense, but to throw doubt on the natural inference that they establish physiological varieties also. Were it so, nature would merely have succeeded in defeating her own scheme in the peculiar distribution of these nerves, and I am not satisfied that any case has yet been recorded in which there was proof that such physiological variety existed, especially that it has not yet been shown that the sixth nerve had anything to do with the motions of the iris.[1]

[1] The statement of some, that, "in several animals the sixth nerve sends filaments to the iris,"—grounded apparently on the supposition of the existence of such filaments in the rabbit,—appears to me to be erroneous. I have never seen such filaments, and do not believe in their existence.

Action of Belladonna on the Iris in Paralysis of the Third Nerve.—I may also mention an interesting physiological observation which I made in the course of the treatment of this case, viz., that the paralysis of the third nerve, involving the iris, does not interfere with the usual action of belladonna in causing dilatation of the pupil. On two occasions the pupil, on the paralysed side, was well dilated before half an hour after the application of the atropine, and after two or three days began to return to its former size. On a third occasion both pupils dilated equally, and returned nearly equally to the former size. I have since repeated this observation on another case. Within a quarter of an hour of my applying the atropine, the pupil was undergoing its usual active dilatation. This adds additional evidence to the view that the third nerve regulates only the contraction of the pupil, whilst its dilatation is accomplished through another nervous agency which we possess the power of calling into activity by the influence of the belladonna.

IX.

DESCRIPTION

OF AN

ESQUIMAUX FEMALE PELVIS.

I GIVE the following notice of this pelvis with the twofold view of making its peculiarities known to those interested in the subject of the various forms and dimensions of the pelvis in the different races of man, or in different individuals; and of suggesting to those of my pupils who may visit these regions, the endeavour to obtain the pelves of the Esquimaux as well as the skulls, of which latter so many have been brought to this country that we are now well acquainted with their usual characters.

This pelvis was brought to me by my former pupil, Dr Pirie of Liff. It was found by him "on the coast of Davis Straits during the summer of 1851. It was part of a female body which I discovered lying upon the land near Cape Hooper, in lat. N. 68° 6′, lon. W. 64° 36′, and overlaid by a cairn of stones, which is the ordinary form of burial there."

The true pelvis belongs most to the so-called square form of pelvis, but is remarkable chiefly from its large dimensions.

The following are the measurements in inches, which I ascertained carefully along with Dr Pirie :—

	Conjugate.	Transverse.	Oblique.
Brim, . .	$4\frac{5}{8}$	6	$5\frac{6}{8}$
Cavity, . .	$5\frac{5}{8}$	$5\frac{2}{8}$	$5\frac{4}{8}$
Outlet, . .	$5\frac{2}{8}$	$5\frac{1}{8}$	.

Circumference of brim, . .	$16\frac{1}{2}$ inches.
Depth of pelvis, . . .	4
Depth of symphysis pubis, . .	$1\frac{3}{4}$
Between iliac crests, . .	11
Between antr. supr. spinous processes,	10

Sacrum, with medium and gradual curvature. Pubic arch, very wide. The bones are light and delicate throughout, like other well developed female pelves. There is a peculiar rough, deep groove $1\frac{1}{2}$ inch long and $\frac{3}{4}$ broad, on both ilia at the lower and outer edge of the sacro iliac synchondrosis, for which I am at a loss to assign a probable use; and on the horizontal ramus of the pubis there is a strongly marked spine on either side, where the psoas parvus tendon usually is fixed.

The upper pelvis does not, it will be observed, exceed the usual size, but the true pelvis, in all its measurements, much exceeds the average size usually stated by writers, and is considerably larger than any pelvis I have seen. The character of the female true pelvis, its development in the transverse direction, is here carried unusually far, the transverse diameter of the brim and outlet being from an inch to half an inch more than the average corresponding measurements of the pelves met with in this country.

Whether this be a typical specimen of the true Esquimaux pelvis, either of the usual dimensions, or of these somewhat larger than usual, or only an Esquimaux pelvis accidentally large, as we occasionally see in this country, though to a less degree, must remain for future observers to determine. The pelvis is now in the possession of Professor Retzius of Stockholm, to whom I presented it during his visit to Edinburgh last summer.

If this be a typical specimen of the Esquimaux pelvis, it will correspond to the statements made by writers regarding the rapidity of the process of parturition in that race, although one reason at least for this occurrence in uncivilised, compared with civilised countries, is, as Professor Simpson has shown, the smaller size of the head in the former, from the lesser development of the brain.

Dr Pirie writes me, of his inquiries regarding the Esquimaux :

—"Till within a few hours of labour, the woman goes about her usual duties, and shortly before it is expected, she retires alone to a skin hut built for the purpose, which is so small as to confine her almost entirely to the recumbent posture. While there, no one is allowed to enter the hut, and should she require any food it is placed at the door by her friends. No one is present while labour is going on, and this is usually finished in about half an hour."

"In one case which I have heard from authentic sources, the female was seized with labour pains while on board a whaling vessel. She immediately left the ship, and retired behind a hillock of ice, returning again with her child on her back in less than half an hour."

X.

ON MALFORMATION

OF THE

SEMILUNAR VALVES OF THE HEART,

WITH

REMARKS ON THE VARIETIES OF THESE VALVES, AND ON THEIR NATURAL ACTION.

[Read before the Edinburgh Physiological Society, June 11, 1853.]

CASE OF MALFORMATION OF THE AORTIC VALVES.

There are two large valves, and one much smaller. The large valves are lateral, right and left, and separated posteriorly by the small one. The right measures $1\frac{3}{4}$ inch from horn to horn, the left $\frac{1}{8}$ inch more. Depth of right, one inch, of left $\frac{7}{8}$. Where they meet by their anterior horns, the left is attached half an inch below the right, the appearances indicating that this part of the valve had been lost. Also, this end of the left valve has a large smooth hole in it, which nearly admits the point of the little finger, and through which the blood must have regurgitated freely into the ventricle. Posteriorly the large valves are attached a little above the horns of the small one, and in each there is a perforation, in the left the size of a crow quill, and in the right of a common quill. Through these the blood would regurgitate only into the cavity of the lesser valve. *The small or supplementary valve* measures five-eighths of an inch from horn to horn, and half an inch in depth. By its left horn it is attached a little below the top of the contiguous horn of the left valve, and by its right it joins the right valve one-fifth of an inch from the top of the posterior horn of the latter. Being attached a little lower than this end of the large valves, it would receive the blood which regurgitated through the perforations in them, and its cavity has a somewhat oblique inclination to the right side where the larger perforation looks down into it. It has a thickening like a corpus Arantii in its centre. The corpus Arantii of the right valve is situated so much behind the middle, that, were this valve and the small one joined into one, it would be exactly in the centre; and a thick band of fibres passed from this corpus Arantii on each side. The left valve is uniformly thick and opaque. The coronary arteries arise, one from above the middle of the left valve, the other above the right, half way between its middle and the anterior horn.

The aorta near the valves is very irregular, covered with soft tubercular elevations and a few calcareous deposits. The mitral, tricuspid, and pulmonary semilunar valves, are healthy. The left ventricle is much dilated and thickened, the right ventricle normal. The whole heart weighed about a pound and a half. The patient was an emaciated man, æt. 33, broken down by dissipation. The history, known only for a few days before death, states, that the symptoms were—dyspnœa, cough, hemoptysis, sputa tinged with blood—pulse full, strong, and about 90—considerable cardiac dulness. First sound obscured by a moderate bruit; second sound very indistinct.

Remarks on Varieties of the Semilunar Valves.—However frequent it may be to meet with cases of variety in the number of the semilunar valves, in which only two, or again more than three, were found, there is want of evidence to show that such a condition has been known to occur as an original anatomical variety. It is interesting to observe that the variety of diminution of the number usually occurs at the aortic orifice, whilst the cases in which the usual number was exceeded have chiefly occurred at the pulmonary orifice.[1]

This corresponds to the probable view, that the condition of two valves arises from disease in the adult, by which two of the valves become joined together, and then thrown down as one large one; and that the condition of more than three valves arises from disease in utero, one or more of the original valves being divided into two, by an adhesion to the wall of the artery, the condition being found almost always at the pulmonary orifice, and in young subjects. The greater frequency of disease at the pulmonary orifice in young subjects, whilst in the adult it is so much more frequent at the aortic orifice, cannot, however,

[1] See an interesting collection of facts on this subject by Dr Peacock of St Thomas' Hospital.—*Medical Times* and *Gazette*, November 15, 1851; and September 11, 1852. And also, in the former, remarks on this subject by Dr W. T. Gairdner, lecturer on practice of medicine in this city.

be explained by the supposed greater activity of the right side of the heart before birth, as the fact is that, at least during the latter two-thirds of fœtal life, the walls of the left ventricle are already considerably thicker than those of the right.

We must distinguish, in using the term "malformation," between malformation owing to disease in utero, and to original deviation. The latter only can be regarded by the anatomist as cases of variety in the number of the valves, and there is little evidence to show that such a condition has occurred. If the two, or four, valves were of equal size, healthy, and in all respects corresponding to each other, then only could we conclude the case to be one of original variety.

The suitable number of valves for orifices of this size and activity appears to be *three.* Where valves occur in the trunks of the larger veins, two is the usual number—three being rarely found in man ; but, in the larger animals, it is stated that three occur not unfrequently. When the vessel becomes large, and especially here, where so much strength is required, it seems that three valves become necessary. Two large valves, each occupying a half of the large orifice, would be less easily or completely pushed aside by the blood than the three ; and, when in action, the strain would have told more upon them than it does upon the three, which are so doubled against each other, when closed, that each fits into an angle between the other two, and exerts its pressure against the walls of both of the others in this angular recess. On the other hand, a greater number than three would give complexity, without additional openness to the passage, or security against regurgitation ; as, whatever the number, each would still require to be at least projecting enough to meet the others in the centre of the orifice, besides an additional portion to be doubled up against the others.

At the same time, that two valves might be sufficient, is evident from the cases in which, although more or less diseased, they were found when death had been due to other causes ; and in these cases their diseased or irregular condition might be regarded either as affording evidence, on the one hand, that the condition of two or four valves had its origin in disease ; or, on the other, if the two valves were of equal size, that they had been two in number originally, and that their consequent less perfect action had brought about the disease.

Cases such as the one recorded cannot be explained farther than by the supposition of an arrest of development—that, in early life, one of the valves had ceased to grow. It is possible that a small valve had been a portion of a large diseased valve, united by adhesion to the wall of the artery, as in the case of the mode in which it is supposed the additional valve is formed when more than three are present; but, in a similar case noticed by Dr Peacock, the small third valve had, still more than in this one, the appearance of being an originally separate valve, which had ceased to grow in early life.

On the Natural Action of the Semilunar Valves.—When the blood in the ventricles is compressed by their systole, it meets with the convexities of the closed semilunar valves, and pushes them aside. This of itself must require considerable force, when we consider the strain backwards, especially upon the aortic valves, from the elastic reaction of the arteries, which tends to force the blood backwards upon the valves, as well as forwards. This forcible throwing aside of the semilunar valves, and driving of the greater part of the blood out of their cavities behind, must contribute to the production of the first sound of the heart fully more than the part of that sound supposed to be contributed by the mere rushing of the blood through the narrow orifices of the aorta and pulmonary artery. When we consider the matter closely, it seems as if a small quantity of blood would regurgitate into the ventricles before the valves had time to close, as the valves have been washed up, and all the blood expelled from behind them, except so much as there is room for in the sinus of Valsalva, or the recess in the wall behind each valve. When the ventricles have thrown the blood forcibly up into the arteries, these are distended, and, reacting, tend to force the blood back, but the reversed current or weight of the column pressing back, meets the partially open mouths of the valves, a condition of them which is ensured by the sinus behind and the blood in it, which prevent the valves being washed up altogether flat against the wall of the artery; the cavities of the valves are thus quickly and inevitably filled, and the valves are quickly and forcibly thrown down towards the ventricle against each other, completely closing the orifice, and producing the second sound of the heart. It may be, that during life, a few drops of blood get back into the ventricles before the complete closure of the valves can be effected, but we observe in the dead body, when the valves have not been diseased or altered from age, that the action of the aortic valves is complete, as shown by the injection of the arteries. In injecting our subjects from the femoral artery, the injection passes up the aorta and down the arch, and is arrested at the orifice of the aorta by the semilunar valves. If it is a diseased or old subject these give way, or, without actually tearing, allow the injection to enter the left ventricle; but if the subject is young or a healthy adult, the arrestment is complete, and not a drop of the injection is seen in the ventricle. We would expect that a few drops would pass through the centre before the valves could be filled out and thrown down, but again we must recollect that the valves in the dead body lie loosely against or towards each other, in the absence of any force to keep them up, and are therefore perhaps more in the way of being immediately closed, than in the living body, in which they have to return from the position into which they had been washed by the blood rushing out of the ventricles.

The action of these valves is perfectly mechanical, and contrasts with that of the auriculo-ventricular valves, to the perfect action of which vital contractility is necessary, in the employment of the musculi papillares. The principle upon which these papillary muscles are used, is, it seems to me, simply that of shortening of their tendons—the cordæ tendineæ—so as to accommodate these to the size of the cavity of the ventricle, the contraction of which during the systole would otherwise relax the cords too much and allow the flaps of the valves to yield too far. I do not, however, deny that the musculi papillares may act first of all, pulling the flaps towards each other so as to make sure of the blood getting behind them, thus serving the purpose of the sinuses of Valsalva in relation to the semilunar valves, only it seems to me that such a preliminary action on the flaps of the valve is not required. Except, however, in so far as just expressed, the action of the auriculo-ventricular valves is mechanical also, the blood getting in behind the flaps of the valve, and pushing them together, whilst the cords prevent them being pushed through the orifice into the auricle; but that their action is not mechanically perfect, but depends for its perfection on the vital act of the shortening of the musculi papillares, is shown in the dead body by the result of injection, which, if it gets into the ventricle is not arrested by the mitral valve, but is certain to pass through and fill the left auricle, and from it the pulmonary veins.

It is perhaps commonly supposed that the use of the corpus Arantii in the centre of the free margin of the semilunar valves was to ensure the filling up of a possible or imaginary chink left in the centre by the meeting of three convex surfaces; but a close consideration will show that the relation of the valves to each other is different from this. Whatever the number of valves, it was necessary that, being equal, each should be at least large enough to reach to the centre. But each is large enough to reach farther than this. Allowing an inch as the diameter of the aortic orifice, each valve will be found to measure from each of its horns to the corpus Arantii from $\frac{5}{8}$ to $\frac{6}{8}$ of an inch, *i.e.*, from an eighth to a sixth, or even a fourth, of an inch more than is sufficient to reach the centre or axis of the aorta. The upper part of the valve is therefore doubled up, and rests against a corresponding part of the other two, pressing back to back against each other, whilst the lower part of the valve forms a convex bulging towards the ventricle. This corresponds to the natural difference of the semilunar valves at their upper and lower parts, the former being thin and the latter thick and strong. The middle fibrous bands of the valve seem to correspond to the points where the valves ceased to support each other. These median fibrous bands passing from each horn into the valve and then curving upwards towards the corpus Arantii, appear to me not properly to pass up to the corpus, but to meet some distance, $\frac{1}{8}$ inch more or less, below it, in a triangular thickening (corpus Arantii inferius), from the apex of which a narrow median elevation or thickening passes up to join the true or superior corpus Arantii. This is sometimes very distinct, and especially when they are thickened by disease, the inferior body will often be felt to be the thickest and hardest of the two. When the valves are closed, the thin part above the median band is a perfectly flat surface, each half supported against the corresponding half of the other two valves. Seen from above, there is a triple line, or three straight lines meeting in the centre; and although each valve is large enough to pass beyond the centre, their upper margins seem to be stretched, by passing not horizontally inwards, but inwards and downwards.

When viewed from below they are also seen to meet in a triple line, and this appears to correspond to the median fibrous band, and the inferior corpus Arantii meets its fellows in the centre, where the three straight lines meet. Below this the valves are seen to be convex, and look to the cavity of the ventricle. When, therefore, the valves are closed, the upper thin portions are flattened against and mutually supported by each other, and the corpora Arantii, are some distance up in the interior of the artery. Part of the force of the elastic reaction of the arteries is thus sustained; and were this part of the valve to be perforated, as we sometimes see in the upper thin portion of the pulmonary valves, the blood would not regurgitate into the ventricle, but merely pass from the cavity of one valve into that of the other. The lower and thicker portion of the valves begins where they are about to loose this mutual support, and is thus fitted for resisting the strain backwards towards the ventricle, into the cavity of which the blood would regurgitate in the event of a perforation occurring in this part of the valve.

The sinuses of Valsalva, as is well understood, are no doubt essential to the perfect action of the semilunar valves. The falling back of the valves is ensured in three ways—first, by their own weight, as soon as the blood which pushed them aside has passed by; secondly, by the weight of a certain amount of blood which has not been expelled from the sinus; and, thirdly, by the sinus beginning above the valve, so as to expose the mouth of the latter to the returning flow of the blood; all contributing to the rapid and complete closure of the valves.

But this arrangement of sinuses behind the valves is not confined to the aortic and pulmonary valves; it seems to exist equally with all the valves of the vascular system; in the lymphatics as well as in the veins. The great importance of the valves in the veins, in the extremities especially,—to the veins of which, indeed, the valves are all but confined,—in rendering the muscles accessary to the onward motion of the blood in the veins, cannot be too much insisted on. Without them the muscles would tend to retard as much as to accelerate the venous flow, but the valves, not of themselves of direct use during the onward flow, prevent this, and render the action of the muscles equivalent to a second heart acting on the deep veins. In this way it is, that sudden and severe exercise makes us out of breath, by the rapidity with which the blood is thrown forwards to the heart, more rapidly than the lungs can at first transmit it; and this force, it must be recollected, is more or less constantly in action in the limbs, unless during sleep, or when the body is otherwise at complete rest, and then, the position being horizontal, the heart has only to circulate the blood through a set of level tubes, at least it is so with the large returning veins.

Now, the action of these valves is rendered certain by sinuses behind them, which natural sinuses or pouches, not any unnatural dilation of the vein by the injection meeting the resisting valves, is the cause of the swellings seen outside, in injected veins and lymphatic vessels; and a *valvular sinus* is therefore an essential part in the construction of a mechanically acting valve.

Some physiological writers seem to object to the view that the forcing action of the heart is the one great motive power of the circulation, under the impression that it is too mechanical a view of the matter; and have inferred on insufficient grounds, the existence of another motive power, or *vis a fronte*, from the phenomena presented in the capillaries, in the exercise of the vital endowments of the blood and the tissues, necessarily existing to serve the purposes of nutrition, respiration, etc., altogether irrespective of motive power. But as experiments show the power of the heart to be fully sufficient, and as this great central force, modified by the influence of the nervous system in causing dilatation or contraction of the larger or smaller arteries, or of the capillaries themselves, is sufficient to explain all the phenomena of the circulation, including local determinations, it is perhaps unnecessary to invent other and hypothetical powers; and so far from a mechanical explanation being objectionable, it may be remarked that such a mode of accomplishing an object in the living body, is indeed not a lower but a higher manifestation of power and design, being a more simple means of attaining the end in view; and mechanical laws, be it recollected, come from the same Author as the vital endowments. In the whole of animal physiology there is perhaps nothing more perfect and beautiful, and at the same time more simple and mechanical, than the action of the semilunar valves; and they are kept working by the vital endowment of the living heart, and enabled to resist friction and wear by the vital endowments of the blood-vessels which nourish them. The valves open and close with force at every action of the heart. Allowing the heart to act 75 times in a minute, this will give us 4500 actions of these valves in an hour; and although not thicker than very thin writing paper, these valves, thus acting incessantly through every hour of a long life, may be found as good at the end of it as at the beginning.

Fig. A

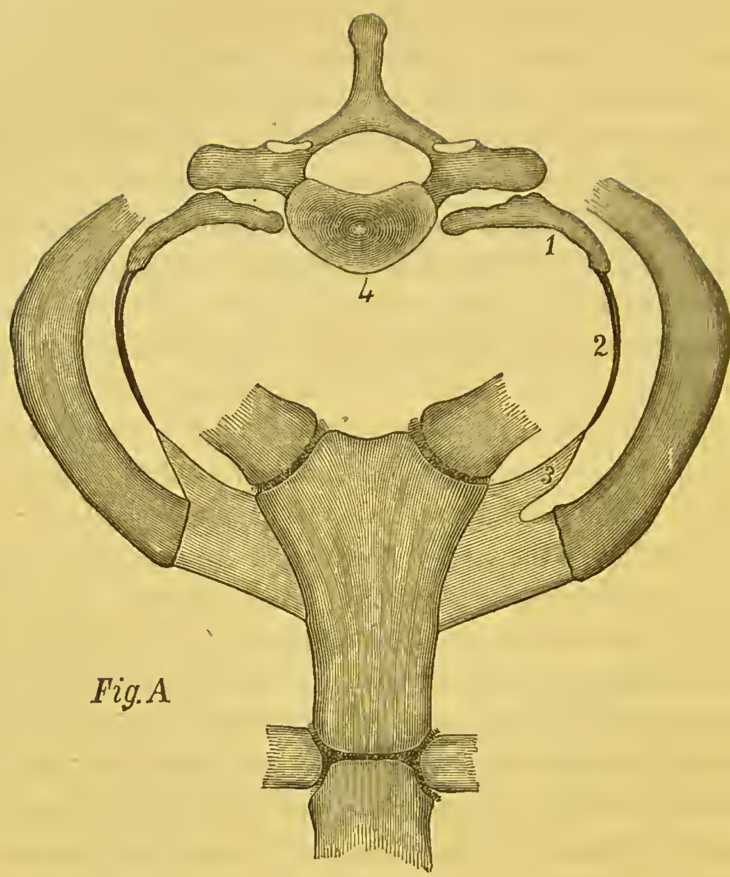


Fig. B

Fig. C

Fig. A.—1. Rudimentary first rib. 2. Ligament representing part of first rib. 3. Small first cartilage becoming blended with cartilage of second rib. 4. First dorsal vertebra.

Fig. B.—Rudimentary first rib of left side. 1. Head. 2. Groove for subclavian artery.

Fig. C.—Rudimentary twelfth rib of left side. 1. Head.

XI.

ON

RUDIMENTARY RIBS,

AND ON

SOME POINTS IN THE ANATOMY OF THE VERTEBRÆ.

FROM THE MONTHLY JOURNAL OF MEDICAL SCIENCE, OCTOBER 1853.

I SHALL first describe the parts in two instances which have occurred in my anatomical rooms, in which the highest and lowest ribs were in an imperfectly formed condition; and then offer a few considerations as to the nature of the various irregularities of the ribs, and their relation to the spine.

I.—*Imperfect condition of the First Pair of Ribs, and their relation to surrounding parts.*

This variety occurred during summer session 1852, in a male æt. 24, in all other respects well formed. There are twelve ribs on each side. The first or highest rib on both sides is short, a large part being represented by a ligament. The right is two inches in length, the left two inches and a quarter. Each joins by its head the upper part of the body of the first dorsal vertebra by one entire facet, the lower part of the body of this vertebra receiving also, as usual, part of the head of the second rib. Both articulate by their tubercles, an inch outward from the head, with the transverse process of the first dorsal vertebra. They are broadest at the tubercle, where each measures nearly half an inch, and before and after this they are about half this breadth. The right tapers to a point, the left is rather expanded at its anterior end, and distinctly grooved here where the subclavian artery lay. This groove is much less marked on the right side. A ligament, narrow but strong, passes forward from the tip of each rib, and joins in front a short and pointed first cartilage, which thus connects it to the sternum. The ligament is two inches in length. The cartilage is an inch in length and a fourth of an inch in breadth, and pointed at its outer end where it receives the ligament. Where it joins the sternum, it is close up to the clavicular notch, and by the inner half of its lower border is quite continuous with the broader cartilage of the second rib, with which at the sternum it therefore forms one piece. The mannbrium sterni is proportionally long, measuring three inches, while the body measures only a quarter of an inch more; and at the cartilaginous union between the mannbrium and body the cartilage of the third rib is fixed, this being the place where the cartilage of the second rib is normally attached. The handle of the sternum is thus joined by the cartilages of the first two ribs and half of the third, although it is to be observed that the first two cartilages are fused into one before they join the sternum. The cartilage of the eighth rib comes within a quarter of an inch of the lower end of the sternum, joining the appendix ensiformis, which is ossifying above, and presents a well bifurcated cartilage below.

The superior aperture or inlet of the thorax is thus bounded; behind, by the first dorsal vertebra, and on each side of this by the rudimentary rib, the tips of which do not project farther forwards than on a line with the front of the body of the vertebra; on each side by the ligamentous cord, and in front by the short cartilages and the sternum. The transverse diameter of the inlet measures four and a half inches, the antero-posterior two and a half, and the plane of the opening slopes a very little downwards and forwards. The second ribs are well formed, fully six inches in length, and the transverse diameter of the space between them is six inches. The space between the first rib and ligament above and the second rib below is rather more than half an inch in breadth, and is occupied in the usual way by the intercostal muscles and membrane.

The scalenus anticus muscle is attached on each side chiefly to the point of the rudimentary rib and to the ligament close to it, and a large tendinous slip passes down inside the internal intercostal muscle, to be fixed to the upper margin of the second rib. The subclavian vessels were normal in their relative anatomy. The artery lay immediately behind the scalenus anticus, and was supported by the groove in the tip of each rib. The position of the artery in the neck must, however, have been farther back than usual, as the tip of each rib is only on a level with the front of the body of the vertebra, and an inch behind the transverse axis of the inlet of the thorax.

Looking merely to the ribs themselves, and to the circumstance that the cartilage fixed to the sternum where the handle and body join was that of the third rib, it might be supposed that these were instances of supernumerary cervical ribs connected with the vertebra prominens; but though I had not the opportunity of examining the vertebræ above, I carefully ascertained that there were only eleven ribs below them on each side, and had there been a small rib below, as in the next case, it would have been found in the after dissection of the abdomen. Besides the vertebra on which they are supported being in all respects like an ordinary first dorsal, the neighbouring anatomy is normal, except that the rib stops short and is represented by a ligament.

II.—*Rudimentary condition of Twelfth Ribs.*

This variety occurred during last winter session, 1852-3, in a female æt. 19. From external examination the subject was at first supposed to present an example of the occurrence of eleven ribs only, but on subsequent examination on the inside, I detected a pair of very small twelfth ribs, lying hidden among the muscles, but so small that they still were imperceptible from the outside. There is very little difference between them. The right measures an inch in length and an eighth of an inch in breadth; the left seven-eighths in length and one-sixth in breadth. They are flattened from before backwards, a little expanded at the tip, and by an inner rounded end, or head, joined, by a freely movable articulation, to the pedicle of the twelfth dorsal vertebra, at a point corresponding to where the eleventh rib joins its vertebra, at the upper part of the root of the pedicle. It is the twelfth rib from above downwards, and there are five lumbar vertebræ between its vertebra and the sacrum. The eleventh rib is six inches in length. The eleventh dorsal vertebra has here the character by which the twelfth is usually distinguished, its inferior articulating processes looking outwards instead of forwards. This, together with the small size of the rib, would without doubt suggest to one merely seeing the specimen, that the case was one of supernumerary lumbar rib, but I ascertained carefully that it was only the twelfth rib from above downwards, and there were five lumbar vertebræ below.

In this subject there was also a peculiarity of the cartilage of the left fourth rib. The rib is very broad where it joins the cartilage, and the latter is bifurcated at its costal end, leaving an aperture which received the point of the little finger.

The drawings illustrate the descriptions. In Fig. A the parts are reduced two-thirds from the natural size, retaining the proportions, but in the other two the full size is represented.

III.—*Remarks on Variation of the number of the Ribs in Man, and on some points in the Anatomy of the Vertebræ.*

It is well known that the ribs may vary in number, from the addition of a thirteenth rib on the seventh cervical or first lumbar vertebra, or from the absence of one at the upper or lower end of the range, although on this point writers are less precise. But, although the occurrence is noticed by almost every anatomical author, comparatively few appear to have actually met with instances of it. A case of supernumerary cervical rib has been recorded by Dr A. Dymock, in an interesting paper,[1] which con-

[1] Edin. Med. and Surg. Journal, vol. xl. 1833.

tains references to the history of this point, and also some able remarks on its nature. In the case recorded by him, the rib of the right side is an inch and a half in length, and that of the left three inches. Both are articulated to the upper part of the side of the body of the seventh cervical vertebra. They ended abruptly without any cartilaginous prolongation. That of the left side was joined by a strong fasciculus of tendinous fibres to the next rib, but no farther account of the relation to the other soft parts could be given, as putrefaction was far advanced before the peculiarity was noticed.

Mr R. Quain has recorded two instances of cervical rib.[1] Regarding one of these no details could be given, as the preparation was destroyed, and he has represented the other in Plate 25, fig. 9, being from a preparation in the University of Cambridge. They are placed on the seventh cervical vertebra. That of the right side reaches forwards, and joins a tubercle near the middle of the first ordinary rib. That of the left side is less than an inch in length, and both are movable on the vertebra. The first ordinary rib is drawn as articulated entirely on the body of the first dorsal vertebra.

The examination into this point, in connection with the two cases which occurred to me, has led me to put the question, whether some of the cases recorded as instances of cervical and lumbar ribs were not of the same nature,—instances rather of the ordinary first and twelfth ribs in an imperfectly formed condition. This latter condition has attracted much less notice, and may be rare. It is alluded to, however, by several anatomists. Bourgery[2] states, that the absence of the first rib is often only apparent, the rib being well developed behind. Mr Ward, in his excellent work on the bones,[3] says, "more rarely the first rib on each side is deficient in front, losing itself, like the floating ribs, in the substance of the muscles." Sœmmerring[4] alludes to

[1] Anatomy of the Arteries, 1844. Pp. 149-187.

[2] Traité Complet de l'Anatomie de l'Homme. Bourgery et Jacob. Paris, 1831. Vol. i., p. 59.

[3] Outlines of Human Osteology. By F. O. Ward. London. 1837.

[4] Traité d'Osteologie et de Syndesmologie. S. T. Sœmmerring. Paris, 1843.

a case in which there were only eleven ribs, these eleven, however, appearing larger than usual. Of high supernumerary ribs, he says, "it resembles more or less the first, and is united by a cartilage, or only by a ligament, with the upper piece of the sternum. The first rib proper exists, and is attached to the superior piece of the sternum, is longer and narrower than ordinary, owing to which the space between it, the sternum, and the body of the vertebra, is sensibly larger." But he does not refer to any ease, nor mention such an occurrence as an imperfect first rib, liable to be mistaken for a eervieal rib.

These remarks are not, of course, intended to question the occurrence of cervical or lumbar ribs, but merely to suggest eaution in admitting any case to be of such a nature, unless after due examination of other parts of the spine and chest. Were the preparations in the two eases I have described now placed before any anatomist, without any history or information, they would, without doubt, be fixed on as instances of supernumerary cervical and lumbar ribs, although they are merely instanees of the first and twelfth ribs in an imperfectly developed condition.

My second ease also shows how readily a mistake might be committed, in supposing the number of the ribs to be reduced to eleven, as the twelfth rib may be so small that its presenee can be ascertained only by dissection of the museles elose to it.

These cases, however, illustrate, perhaps not less than the actual oceurrence of supernumerary ribs, the analogy that obtains between the transverse proeesses of the lumbar vertebræ, the anterior part of the same processes of the vertebræ in the neck, and the ribs. As the case of supernumerary rib shows the transverse process expanded into a rib, these cases show a rib, as it were, shrinking into a transverse process. The now well known faet of the development of the anterior arch of the transverse process of the seventh cervical vertebra in man, from a separate centre, enables us not only to understand the analogy more clearly, but likewise readily to account for the occasional existence of a small rib on this vertebra; and the analogy may reasonably be extended to the other cervical vertebræ, although the anterior arch or costiform portion of their transverse processes, is not, with perhaps rare and partial exceptions, developed in man from a separate centre, but of "exogenous" formation, as it is termed by some.

The position of the little twelfth rib, in my second case, illustrates very clearly the analogy between the ribs and the transverse processes of the lumbar vertebræ. It shows an intermediate link between a twelfth rib and a transverse process, or rather, it is like a lumbar transverse process, a little longer and narrower than usual, articulated but not ossified to the vertebra, and much like what we should expect in the case of the transverse process of the first lumbar vertebra not becoming ossified to the pedicle, when it had been, as it is known occasionally to be, developed from a separate centre. Also, the analogy between the tubercle on the root of the superior articulating processes of the lumbar vertebræ and the transverse processes of the dorsal vertebræ is very apparent in this specimen. In no case almost has the twelfth dorsal vertebra a distinctly projecting transverse process, but here there is only a tubercle, or little elevation, deeper vertically, but scarcely more projecting than the tubercle on the first lumbar vertebra; whilst the latter or analogical lumbar transverse process, holds the same relation to the transverse process usually so called, as the transverse process of a dorsal vertebra does to the commencement of a rib.

In considering these analogies, we must not forget, as some writers on transcendental anatomy seem to have done, that the parts which may be so regarded, do not the less serve a specific purpose, and to serve which they are provided. The lumbar transverse processes and tubercles are required to give attachment to certain muscles in the loins, and the transverse processes of the cervical vertebræ are constructed with a view to several purposes; chiefly to form a foramen for the lodgment of the vertebral artery, which implies an anterior and posterior arch during growth, and, in the four common vertebræ, a couple of tubercles, one for the anterior and one for the posterior group of muscles, and a groove between for the safe passage of the nerve; all of which, it is not too much to say, would have been present whether any part of the process was analogous to a rib or not; that is to say, the anterior part of the cervical transverse process does not exist from necessity, merely, so to speak, from the force of any general law or type of formation, as the "costal appendage" of the vertebra, but exists there because it is required or implied in the formation of a hole, which aperture again is formed here, like apertures elsewhere, because required for a purpose. It is, however, at the same time, most interesting to observe that this purpose is effected by the working out of a general plan; and here, as frequently elsewhere in anatomy, we see several principles combined or ends served by one and the same means. It would indeed be wonderful if the skeletons of the various vertebrata were not all laid down on the same general plan; but would it not be more so, if this plan could not be varied or departed from when it was necessary or better that it should be so,—the variation being the specific law, while the other is the general law, and both equally original and fixed?[1]

These remarks apply to those "analogies" of the human skeleton which are constant, and where the parts at the same time serve specific purposes, which we must distinguish from those occurring occasionally only, and to which the term "anomaly" is applied,—a term to which there can be no objection, if we understand the sense in which it is used. Of these we have examples in the occurrence of true supernumerary ribs, or still better, in the hooked process that occasionally springs from the humerus above the internal condyle, and is the analogue of the arch of bone completing the foramen in many of the mammalia, through which the median nerve and brachial artery pass. These anomalies serve no specific purpose, nor do they interfere with the specific uses of any other parts, and of such deviations we can give no explanation beyond observing the fact and tracing the analogy. To speak of such anomalies as the result of a general law and type of formation, or a "law of deformation," is at best an assumption, or a use of terms without any intelligible

[1] I have here, in the common language of descriptive anatomy, used the terms—Rib—Transverse Process—Body, etc. Those desirous of knowing the terms proposed as substitutes for these, in accordance with the views of Owen and others, may consult Mr Holmes Coote's short Treatise on "The Homologies of the Human Skeleton."

meaning. The fact is, that they are not the result of any law, but the exceptions to a law, the *specific law*, by which the human skeleton received and transmits unaltered its peculiarities; exceptions the rarity of which only tends to render the law the more striking, and of which exceptions we know nothing beyond observing the fact and noticing the analogy.

The constant existence of a hole in the transverse process of the seventh cervical vertebra, whilst at the same time the artery does not pass through it, might be looked upon as indicating some other principle in regard to the formation of the aperture in the cervical transverse processes, irrespective of their occupation by the artery. However, although the mere circumstance of one of the seven holes not being used by no means leads to such a conclusion, the fact regarding the seventh vertebra appears to be easily explained by looking to the special anatomy of the parts. First, it is part of the design in the construction of the cervical transverse processes to provide a hole for the vertebral artery; and secondly, from the place where the vertebral artery usually arises, it would require to deviate from its direct course to enter the foramen in the lowest vertebra, and accordingly, so to express it, the artery finds it more convenient to pass over and enter usually the sixth. In accordance with this view I may mention that I have once seen the left vertebral artery pass through the foramen of the seventh vertebra, but it arose about an inch farther down than usual from the subclavian trunk, whilst the normal artery of the other side entered the sixth. This, however, may not be the whole explanation. It is common enough, for instance, for the vertebral artery of the left side to arise separately from the arch of the aorta, but then it does not enter the foramen in the seventh vertebra. Indeed, Mr Quain, with his large observation of varieties of the arteries, appears to have seen it enter only once, and, as in my case just referred to, it was the artery of the left side. It is possible that the explanation may lie in the development or growth of the transverse processes, as we occasionally find the artery pass over even three or four of the lower vertebræ before it enters the foramina. The common statement that the foramen in the seventh cervical transverse process transmits, not the artery, but the vertebral vein, is certainly erroneous. The vein normally comes out where the artery goes in, at the foramen of the sixth vertebra, and gains the subclavian vein, rarely by passing behind the subclavian artery, a position which I have seen it occupy only twice, but in front of the artery, constituting one of its anterior relations internal to the scalenus muscle.

I have observed particularly the interesting fact mentioned by several authors,[1] that in many of the mammalia, it appears to me the majority, the foramen is wanting in the transverse process of the seventh cervical vertebra, although present in those above it. This cannot be owing to a separate anterior arch being lost during maceration, as it is in the adult skeleton, where the bone is finished without any trace of an anterior transverse process. This absence of the foramen in the transverse process of the seventh cervical vertebra in most of the mammalia is an interesting fact when contrasted with its constant presence in man although no use is made of it, but rather harmonises with the fact of the formation in him of the anterior arch from a separate centre.[2]

The case of supernumerary cervical rib in man has been advanced[3] to prove the inaccuracy of the common statement that the mammalian cervix consists always of seven vertebræ; and the author alluded to has given an ideal drawing of two pairs of supernumerary cervical ribs in a human figure, a condition of which I doubt only the occurrence, not the theoretical possibility. The occurrence, however, in man or any mammal of a supernumerary rib on the seventh cervical vertebra, as a rare anomaly, is not a reasonable objection to the rule in question. Although a sixth or supernumerary finger or toe has been met with, we are not the less entitled to hold that, as a general rule, the number of the human digits is five. Whether there is any regular and true exception to this otherwise general rule, will depend on the name

1 Martin's Introduction to the Natural History of Mammiferous Animals. London, 1841. Professor Owen on the Marsupialia. Cycloped. Anat. and Phys., vol. iii.

2 Dr Knox states, in his recent Manual of Anatomy, that the anterior arch of the seventh cervical vertebra is formed from two distinct parts, the anterior of which is the true analogue of the rib, but he does not refer to any facts or observations in support of this view.

3 Cycloped. Anat. and Phys., vol. iv.—Skeleton, by Mr Joseph Maclise.

we give to the eighth and ninth vertebræ of the three-toed sloth; the little ribs appended to their transverse processes indicating them as dorsal vertebræ,[1] while the foramina,[2] said to exist in addition, in their transverse processes would assimilate them to the cervical vertebræ. We know of no reason why the mammalian cervix should be limited to seven vertebræ beyond observing the fact, and even if the above be reckoned an exception, it is a remarkable fact that, in so vast a field, only one exception, and that a doubtful one, can be found to the general rule, that in all mammalia, however much the actual and relative number of the vertebræ in the other regions of the spine may vary, in the neck, be it short or long, the number is uniformly seven.[3]

In conclusion, it is to be observed that no certain opinion can be formed as to the nature of any specimen of irregular rib, unless a careful examination has been made of the other ribs and vertebræ. If it is at the upper end of the thorax, the cervical vertebræ must be counted, as it cannot be determined by an examination of the vertebra supporting the rib or of that above it. If it is a true cervical rib we do not then expect to find the foramen in the transverse process behind it, and the vertebra above may be passed over by the artery, and want its anterior tubercle under such circumstances, that is, the seventh cervical vertebra may resemble the first dorsal, and the sixth vertebra may then resemble the seventh. Carefully counting the ribs may also determine the question but not absolutely, as the conditions of additional or imperfect rib above might be accompanied by those of the absence or addition of a rib below, a rule which if rigorously applied would render it a matter of opinion only, not of absolute certainty, that the rib in my first case was not a supernumerary cervical rib. The question whether a small lowest rib is an imperfect twelfth dorsal, or a true lumbar rib, will be

[1] Cycloped. Anat. and Phys., vol. ii. Edentata. By Professor Bell.
[2] Martin, Op. Cit.
[3] As already mentioned, the foramen is wanting in the transverse process of the seventh cervical vertebra in many of the mammalia, and in many also, the first rib may be seen to rest partly on the body of the seventh cervical vertebra. These two circumstances may be seen frequently to be combined, as in the horse, in which all the ribs rest on the bodies of two vertebræ; but this does not render the lowest vertebra in the neck the less a "cervical" one.

settled by counting the number of the free lumbar vertebræ, whilst the number of the ribs is carefully ascertained.

There is another source of fallacy in determining the nature of a rudimentary rib—there may be eleven or thirteen ribs, and still the cervical vertebræ be seven, and the lumbar five in number. Mr Quain has seen an instance of the former with deficiency of a dorsal vertebræ, and there is an instance of the latter in the Barclay Collection in the Edinburgh College of Surgeons' Museum. The skeleton is marked "Skeleton of a European male used by Dr Barclay in his lecture room, as having thirteen dorsal vertebræ, and thirteen ribs on the left side." There are seven well formed cervical, five well formed lumbar, and thirteen dorsal vertebræ. The first dorsal vertebra receives the first and part of the next rib as usual. The seven upper ribs are true, joining the sternum in the usual manner. The lowest three ribs are floating. The twelfth is six inches in length. The thirteenth is two inches in length, and one-third of an inch in breadth, and is movably articulated on a projection on the upper part of the side of the body of the thirteenth dorsal vertebra. There appears to have been a corresponding one on the right side. The lower articulating processes of the twelfth dorsal vertebra are not turned outwards, those of the thirteenth are the first to be so. The thirteenth dorsal vertebra has a thick short transverse process, intermediate in position between the transverse process of the twelfth above it, and the tubercle of the first lumbar vertebra below it.[1]

Variation in the number of the vertebræ is perhaps a more

[1] Since this paper appeared in the Journal, Mr Holmes Coote of St Bartholomew's Hospital, has published (Medical Times and Gazette, January 21, 1854), an interesting account of the skeleton of a Chinese, in which there is the peculiarity, that the thorax is placed upon the spine, a vertebra higher up than usual. The twelve ribs have been attached to the twelve middle vertebræ, leaving six cervical vertebræ above and six lumbar below. So far as I am aware, no similar case has been recorded. Mr Coote mentions that the Chinese had died in the Sidney Hospital, from which a pupil had brought the skeleton; and suggests the inquiry whether this be the normal condition of the Chinese skeleton.

It so happens that one of my pupils, Mr Cox, who has recently come from the Sidney Hospital, has also brought a Chinese skeleton, which he has afforded me an opportunity of examining carefully. I am also indebted to Mr Cox for much interesting information, regarding the various races to be met with there, for procuring specimen skeletons of which, it appears the Sidney Hospital is favourably situated. A remarkable circumstanee than that of variation of the ribs; but when there is a vertebra more or less, the aceompanying addition or deficiency of a rib is perhaps what might be expeeted. This is to be distinguished from the ease of variation of the ribs only, in which the number of free cervical or lumbar vertebræ is affected. In reckoning the number of the lumbar vertebræ, we will be careful not to reckon only four, when the fifth is partially ossified to the sacrum, or again six, when the highest portion of the sacrum remains separate, as it normally does till between the twenty-fifth and thirtieth year.

I may further observe, that we cannot judge of the individuality of a rib by notieing its exaet relation to the vertebræ. Usually the first dorsal vertebra receives the entire head of the first rib and part of the seeond, but sometimes the first rib reaehes in part upon the body of the seventh eervical vertebra. Nor can we absolutely reeognise any partieular dorsal vertebra below by attending to the usual deseription of their speeifie characters. The twelfth dorsal vertebra is not always the first to have its lower articulating processes turned outwards. In the second of my cases above recorded, the eleventh vertebra possessed this character. I have seen another instanee in which the lower processes of the eleventh looked more outwards than forwards, and another eleventh in whieh the lower proeesses looked in different direetions, the right outwards and the left forwards. I have a preparation of several vertebræ, where apparently the eleventh dorsal has a facet on its transverse process, and only part of a faeet on its body, or if this is a tenth then the lower articulating proeesses of the eleventh are directed outwards. And it appears to me that most of our text books on anatomy are wrong in describing the tenth dorsal vertebra as possessing a

considerable number of Chinese are there as servants, all being males, and Mr Cox is quite certain that both his skeleton and that brought to Mr Coote by his fellow-student, were from true Chinese. The skeleton I have examined presents no peculiarity similar to that described by Mr Coote; the spine of the axis has an average bifurcation, and that of the sixth cervical vertebra is not bifid; the seventh cervical vertebra is in all respects normal. The first dorsal vertebra has an entire facet for the head of the first rib, and a half one below for its part of the second rib. The three lowest dorsal vertebræ have the usual facets, and the lower articulating processes of the twelfth, as usual, look outwards.

facet for the entire head of the corresponding rib. Were it so' the ninth vertebra would be equally entitled to be described as a peculiar one, in the absence of the half facet below, as indeed several anatomists describe. In twelve sets of articulated vertebræ in my collection, the tenth rib has articulated also with the ninth vertebra, freely in four, doubtfully in one, in three on the right side, but apparently not on the left, whilst in the remaining four it appears to have been wholly received by the tenth. In one of the latter there was no facet on the transverse process of the tenth vertebra; and in the specimens where there is no part facet on the lower edge of the ninth, there is a small bony projection, which before the bones were macerated, may have been tipped with cartilage, besides giving attachment to the upper band of the stellate ligament. It seems therefore, that it is at least as frequent to find the tenth rib articulating also with the ninth vertebra. The tenth vertebra can be distinguished from the two below it by the facet on its transverse process, but not clearly from the ninth, unless it has an entire and well finished facet on its body. If the ninth vertebra has been touched by the tenth rib, it cannot be known from the eighth, if it has not, then it cannot be distinguished from a tenth.

It is evident, therefore, although we can generally identify any of the lower dorsal vertebræ, that still no anatomist could be absolutely certain about these vertebræ if separated from the rest of the set; nor could we determine the nature of a lumbar rib, by observing its relation to its vertebra, or the relation of those above it to the bodies and transverse processes of their vertebræ.

These various considerations will suffice to show how careful we should be, before forming an opinion as to the nature of any case of rudimentary rib, and the evidence we expect before any case can be received as one of true supernumerary rib.

XII.

ON BRANCHES FROM THE FIFTH NERVE TO THE MUSCLES OF THE EYE.

In some animals I have found the muscles of the eye to be regularly supplied with distinct filaments from the sensory or ganglionic portion of the fifth pair of nerves, in addition to their ordinary supply from the motor nerves. These filaments may readily be found in a careful dissection of the orbit of the sheep, according to the following description, which shortly embraces the result of much labour in making minute dissections of these parts.

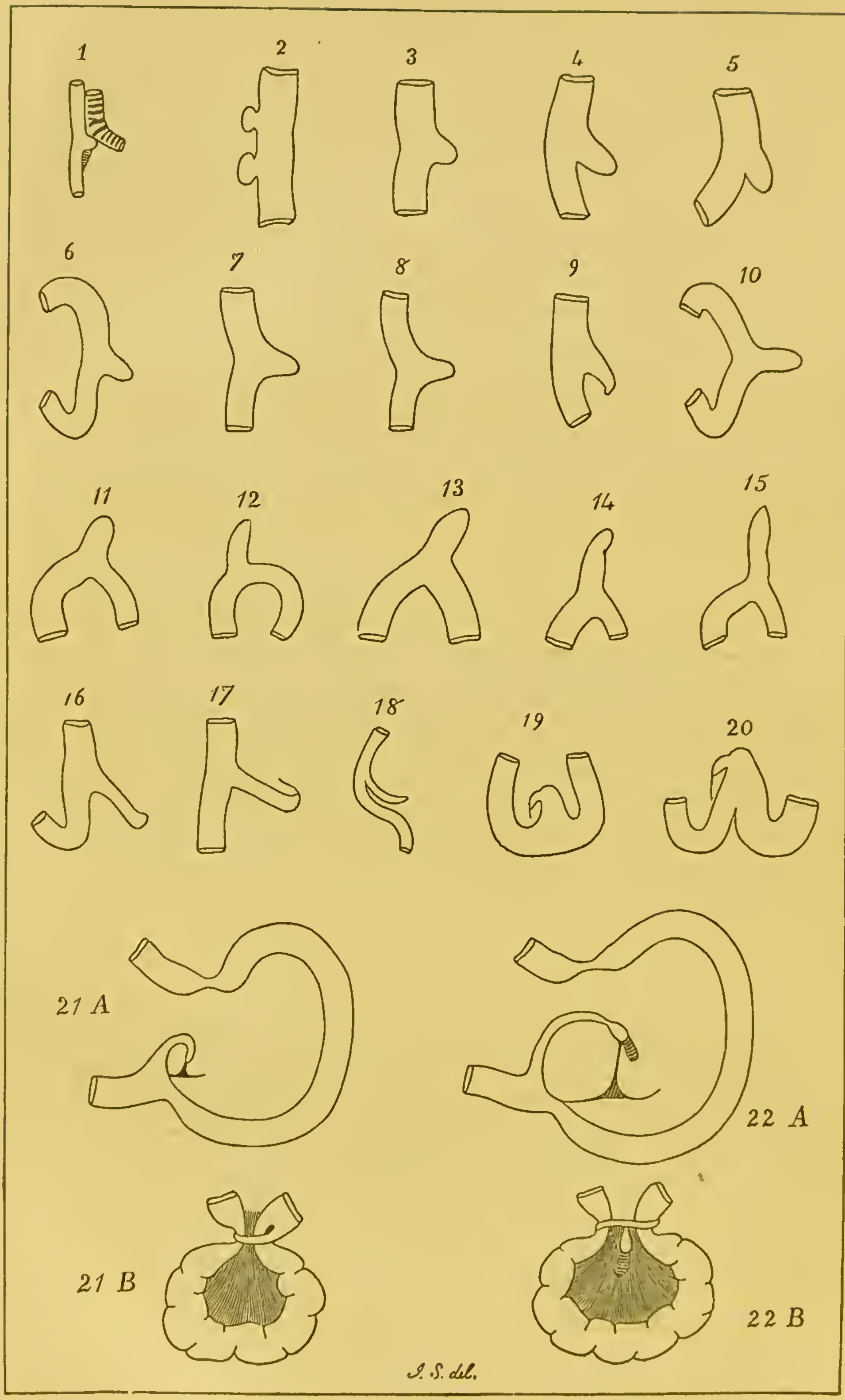
The *superior rectus* usually receives two separate filaments, one on its orbital surface from the supra-orbital nerve, and another on its ocular surface, entering the muscle some distance in front of the point of entrance of its motor filaments from the upper division of the third nerve. But both of these may not be found in the same dissection. The *internal rectus* receives two filaments from the nasal branch of the fifth nerve, which enter on its orbital surface. The *external rectus* receives one or two small filaments from the lachrymal nerve, also entering on the orbital aspect of the muscle. The *retractor* muscle is pierced by a filament or filaments from the fifth nerve, arising in the sphenoidal fissure, and associated with the long ciliary nerves. These filaments to be seen in various animals piercing the retractor muscle, may be in part traced on to the eye as long ciliary nerves, but, at the same time, they branch, and at least partly end in the muscle.

Filaments from the fifth nerve do not pass directly to the oblique muscles, but they join the motor nerves near the muscles. The *superior oblique* may be supplied directly by a filament from the branch of the fifth which lies to the inner side of the roof of the orbit, but this filament usually joins the fourth nerve as the latter is about to enter the muscle, or it may partly join the fourth nerve and partly enter directly into the muscle. The *inferior oblique* is not entered directly by a branch from the fifth nerve, but the latter constantly sends a twig to join the branch from the third nerve to the inferior oblique, as it passes forwards along the inferior rectus to reach the inferior oblique. This twig comes from below arising in the apex of the orbit from the second division of the fifth nerve, just after the latter has entered the orbit in connection with and close below, the first division. This twig runs obliquely upwards and forwards and terminates by three portions; one or two twigs enter the *inferior rectus* about or behind its middle; another passes obliquely forwards to join the motor nerve of the inferior oblique, or this one may come up as a long separate filament; and the third passes to the ciliary ganglion as its long root.

I was often disappointed in my search for the long or sensory root of the ciliary ganglion in the sheep, looking for it to come from the nasal branch of the fifth, but finding it either wanting, or present as a very delicate filament, lying with the long ciliary nerves,—until I found that the true long or sensory root came from below, from the second division of the fifth nerve, at the back part of the floor of the orbit. This inferior long root crosses the nerve of the inferior oblique muscle, partly joining it, and passes to the ganglion, which lies close above the nerve of the inferior oblique.

I am not mistaken in the nature of these filaments to the muscles of the eye. They are easily seen in a careful dissection, and easily recognised as nerves, traceable backwards to the ganglionic or sensory portion of the fifth nerve. Although an anatomist can readily recognise the nature of even a small nervous filament when it is quite fresh, I have, to prevent doubt on the part of others, often placed each of the filaments in question under the microscope, after its connections were made out, and always found what I had before concluded, that they were white nerves.

I have numerous preparations of these filaments from the sheep and calf, which I made ten years ago, and have since repeatedly dissected them and shown them to my pupils and others; but I have not yet been able to find a corresponding arrangement in my dissections of the human orbit.


1
2
3
4
5
6
7
8
9
10
11
12
13
14
15
16
17
18
19
20
21 A
22 A
21 B
22 B
I. S. del.

XIII.

ON DIVERTICULA FROM THE SMALL INTESTINE,

ANATOMICALLY AND PATHOLOGICALLY CONSIDERED.

[From the Edinburgh Medical and Surgical Journal, April 1854.]

Having met with a considerable number of instances of the diverticulum from the small intestine, and with several in which it was concerned in producing death, I have thought it might interest others were I to record the facts with which my investigation of this subject has made me acquainted.

It is well known to anatomists that, as a rare variety, a diverticulum or cæcal appendage may be found projecting from the small intestine; and various opinions have been expressed regarding the origin or nature of this process.

My object in this paper is chiefly to state anatomical and pathological facts in connection with the cases I have met with, but it is well that I should first briefly glance at the different views, so as to elicit the bearing of the details to be furnished.

Monro[1] has observed, "There are two kinds of diverticula, original malformations, and those formed consequent upon the protrusion of the intestines. The preternatural appendices occasionally found within the sac of a hernia are generally formed as stated by my grandfather, viz., by the gradual elongation of the

[1] The Morbid Anatomy of the Gullet, Stomach, and Intestines. By Alexander Monro. Edinburgh, 1830, p. 93.

side of the intestine, and probably also by the addition of new matter." His view seems to be that, by an adhesion forming, a portion of bowel is tied down and pulled upon, and thus becomes "gradually elongated into a cul-de-sac of some length." And, although this remark is applied only to their production in a hernial sac, the view may be extended to the intestine generally. Monro farther remarks—"There is also a species of appendix different from any of the preceding; it has a much thinner covering, is of a globular form, and is shorter and wider. It seems to be formed by a protrusion of the mucous membrane, through the other coats of the intestines."

This view is strengthened by the comparison to the sacculi, or herniæ, which are known occasionally to occur from other parts, as the gullet, urinary bladder, and gall-bladder, and by the consideration that more than one may be found on the intestine of the same subject.

The other view is, admitting the occasional occurrence of acquired protrusions on the small intestine, as on other hollow viscera, that the diverticulum is a congenital or original formation, to which is farther to be added Meckel's explanation, that it results from the early connection between the umbilical vesicle and the intestine of the embryo, remaining partially unclosed and growing as the intestine grows.

Some, admitting the original or congenital nature of the true diverticulum, objected to Meckel's view on the ground that he could not demonstrate an actual communication from the umbilical vesicle into the intestine in the human embryo; and there was the hypothesis of Oken, admitting the communication, but that it took place not with the ileum but the cæcum, and that the appendix vermiformis was the remains of this early communication.

The latter view, however, was a hypothesis, not an inference, and is set aside by the demonstrable fact that the cæcum is seen forming or formed at some distance from the point where the vitelline duct, or pedicle, is seen to be connected with the small intestine.

For an able discussion of the whole question, with a statement of the facts and arguments on either side, I may refer to Meckel's excellent work on anatomy,[1] and shall here only state briefly the chief arguments advanced by Meckel as the foundation of his view, although he laboured at that time under the disadvantage of being unable actually to demonstrate a continuity of canal between the intestine and the umbilical vesicle in the human embryo. His chief arguments are—That their characters indicate them to be primitive formations, having the same structure as, and continuity with, the intestinal coats; that they occur almost always at the same part of the intestine, and that the connection between the umbilical vesicle and the bowel occurs at this same part—viz., on the ileum near the cæcum; that "we sometimes perceive in very young fœtuses a canal which goes across the umbilical sheath, from the vesicle to the abdomen, and by which we can at pleasure empty the vesicle of this fluid, and fill it again;" that "it is not unfrequent, proportionally speaking, to find in the full-grown fœtus a canal which extends from the intestine to the umbilicus, which opens in this latter place, and is always attended by the omphalo-mesenteric vessels." And lastly, he alludes to "the analogy with birds, reptiles, and cartilaginous fishes, in which it is proved that the communication in dispute exists at all periods of fœtal existence." He admits of course that protrusions or herniæ may occur here as elsewhere, and thus account for the occasional existence of a diverticulum or pouch on the jejunum, and for the existence occasionally of more than one.

It is necessary therefore to distinguish diverticula into two kinds, true and false. On this M. Cruveilhier[2] remarks:—"These diverticula, into the composition of which all the tunics of the intestine enter, are very different from the hernia of the mucous membrane through the muscular coat, a hernia of which I have seen an example in the duodenum, and several in the rest of the small intestine. In a subject recently submitted to my observation, the small intestine presented fifty spheroidal

[1] Manual of Descriptive and Pathological Anatomy. By J. F. Meckel, Professor of Anatomy at Halle. London edition, 1838, pp. 375-386.

[2] Traité d'Anatomie Descriptive. Paris, 1852, p. 351.

tumours, of unequal volume, all situated along the mesenteric border of the intestine, and formed by the hernia of the mucous membrane, through the muscular fibres."

Having now drawn attention to the points of interest in connection with these diverticula, I shall next give an account of the various cases I have met with and examined, considering them first anatomically and then pathologically. These are twenty in number, of the true diverticulum ilei, and two of the false diverticulum, one on the jejunum, the other on the œsophagus, which will serve to contrast with the former or true diverticula.

In the woodcut is given an outline of each. These sketches I made with care from the preparations, showing in each a sufficient portion of intestine, and representing accurately the proportion and the various points in the anatomy of each diverticulum. They are arranged in a gradual series; and as the numbers of the figures correspond to the same numbers in the descriptive notes, each may be referred to from the notes.

I have deemed it best, at the risk of being thought tedious, to give all the facts in connection with each case, and, for brevity's sake, in the form of notes, which were made with the preparations before me. Of the twenty-two preparations, eight (Nos. 1, 3, 6, 9, 16, 17, 19, 21) are in my own collection; for the others I am indebted to the Edinburgh Museums; eight (Nos. 2, 4, 7, 10, 12, 13, 20, 22) are in the University Museum; and the remaining six (Nos. 5, 8, 11, 14, 15, 18) in the Museum of the College of Surgeons.

Descriptive Notes of Cases of Diverticula, etc.

1. Pouch on œsophagus. Dried preparation, without history. Projects forwards from œsophagus at bifurcation of trachea, and occupies the recess between and behind the bronchi. It is a blunt knob-like projection, measuring in depth, length, and thickness $\frac{5}{8}$ inch. It begins gradually and without any con striction, occupying the entire breadth of the œsophagus, the

measurement of which, and of the base of the pouch is $\frac{7}{8}$ inch. The œsophagus at this point is slightly enlarged laterally, and is if anything larger below the pouch than above it. Pouch terminates in a smooth rounded extremity. The muscular fibres which run longitudinally on the gullet, are seen distinctly to be continued half-way along the pouch, splitting above and below to encase it. Circular fibres are visible on the gullet, but not on the pouch. The pouch is slightly more transparent than the gullet.

2. Dried preparation marked "portion of jejunum, with two small diverticula." The smaller pouch projects directly outwards like a nut. It measures one inch across and three-fourths of an inch vertically. The commencement is abrupt, and marked by a sharp constriction on all sides. The larger pouch is situated three-fourths of an inch from the other, measures $1\frac{1}{2}$ inch from above down, and $1\frac{1}{4}$ across. It also begins by a sharp circular constriction, and on one side it is considerably bulged or prolonged. The portion of intestine is plainly marked with valvulæ conniventes; but as it is stuffed, it is impossible to say whether the walls of these pouches differ in thickness from those of the bowel.

3. Dried preparation; portion of small intestine, with diverticulum. Diverticulum one inch in length. Has funnel-shaped origin from whole breadth of bowel. Passes somewhat obliquely from bowel, which is considerably larger on the side of the obtuse angle. Forms a blunt cone, terminating in a smooth rounded extremity, without any appearance of ligament or mesentery. It proceeds from the bowel nearer to the mesentery on one side than the other. No valvulæ conniventes, but a small crescentic fold at commencement of diverticulum on side of acute angle. There is an evident continuity of tissue between the walls of the bowel and diverticulum.

4. Dried preparation, without history. Diverticulum two inches in length and $1\frac{1}{4}$ in diameter. Arises from whole breadth of bowel abruptly, and marked by a slight constriction, except on one side where they are gradually continuous. Direction, oblique from the bowel, which is considerably smaller on the

side next the acute angle. The body is straight and very little tapering, and terminates in a smooth and rounded extremity, without any appearance of a ligament or mesentery. It is situated on the bowel much nearer the mesentery on one side than the other. The intestine presents no valvulæ conniventes, but there is a small crescentic fold at the acute angle. Both the longitudinal and circular muscular fibres are distinctly visible on the diverticulum, and continuous with those of the intestine.

5. Wet preparation; "small intestine having a cul-de-sac, from the body of a person in which no other morbid appearance was observed." Diverticulum $1\frac{3}{4}$ inch in length. Begins gradually from entire breadth of convexity of bowel. Directed obliquely, and bowel considerably smaller on the side next the acute angle. Terminates in a rounded, slightly conical extremity, without appearance of a ligament. It is somewhat bent on the side of the acute angle, and along this edge of it lies a narrow mesentery.

6. Dried preparation, without history. Diverticulum two inches long. Arises gradually from whole breadth of bowel. Is directed obliquely from bowel, which is larger on the side of the obtuse angle than on the other. It is conical, and terminates in a smooth rounded apex, without any appearance of a ligament. It proceeds from one side of the bowel close to the mesentery, and, continued from the latter, rising up at right angles from it, is a small mesenteric fold, which reaches $\frac{1}{3}$ of the length of the diverticulum. On the concavity of this curved piece of intestine, crescentic folds, like small valvulæ conniventes, are seen, and one more distinct at the acute angle where the diverticulum begins, but there are no folds on the convexity of the intestine.

7. Dried preparation. Diverticulum two inches in length. Begins by funnel-shaped origin on both sides, from whole breadth of bowel, and from that part of it which is directly opposite from the apparent former attachment of the mesentery. Is directed with very little obliquity, and the intestine is considerably larger on the side of the more obtuse angle, being in diameter $1\frac{1}{2}$ inch on one side, and one inch on the other. In form it is conical, the depth considerably exceeding the thickness or

breadth, and terminates by a smooth rounded end, without any appearance of ligament or mesentery. The intestine presents no valvulæ conniventes.

8. Dried preparation. Diverticulum two inches in length, and conical in form. Has a funnel-shaped origin from the whole breadth of the bowel, opposite from the traces of the former mesenteric attachment. Has a slightly oblique direction, and the bowel is largest on the side of the obtuse angle. Extremity smooth and rounded, and no appearance of ligament or mesentery. No valvulæ conniventes on the intestine.

9. Dried preparation. Diverticulum two inches in length, conical and curved. Has a gradual origin from about half the breadth of the bowel. It is directed obliquely, and the bowel is much larger on the side of the obtuse angle than on the other. Termination like point of fore-finger, with a little pea-shaped prolongation towards the concave side. It is connected with the bowel on one side, close to the mesentery, which sends a small mesenteric fold along the concavity of the diverticulum. No valvulæ conniventes, but there is a crescentic fold at the acute angle, a little below the entrance into the diverticulum.

10. Dried preparation. Diverticulum two inches in length. Origin funnel-shaped from whole breadth of bowel, and nearer the mesentery on one side than the other. Direction horizontal, and bowel apparently of equal size on the two sides. Form somewhat conical, the last half-inch tapering to a blunt apex, which is smooth and rounded without any appearance of a ligament. There is a small mesentery for ⅔ds of its length, prolonged from the mesentery of the bowel, on that side which is nearest the mesentery. No valvulæ conniventes on intestine. The longitudinal and circular muscular fibres are distinct on the diverticulum, and are seen to be continuous with those on the bowel.

11. Dried preparation. Diverticulum two inches in length, and conical. Origin funnel-shaped, from entire breadth of bowel on side opposite from mesentery, but nearer to it on one side than the other. Direction somewhat oblique, and bowel considerably larger on the side of the obtuse angle. Termination

smooth and rounded, without appearance of ligament or mesentery. Intestine without valvulæ conniventes.

12. Dried preparation. Diverticulum two inches in length. Origin gradual, or without constriction, from convexity of bowel, nearly equi-distant on both sides from mesentery. Direction nearly horizontal, but is, as it were, continued from one end of the bowel, and this is a little larger than the other. Termination smooth and rounded, without ligament or mesentery. Both sets of muscular fibres are plainly visible on the diverticulum, though less so than on the bowel, and they are scarcely distinguishable towards the termination of the diverticulum. Intestine has no valvulæ conniventes.

13. Dried preparation. Diverticulum 2½ inches in length. Origin, from whole breadth of bowel, funnel-shaped on one side, and somewhat constricted on the other; and a little nearer the mesentery on one side than the other. It is more in the direction of the bowel on one side than the other, and the end from which it seems more continued is here the smaller of the two, being that next the obtuse angle. Termination smooth and rounded, without appearance of ligament or mesentery. Intestine has no valvulæ conniventes.

14. Dried preparation. Diverticulum 3½ inches in length. Origin gradual, from whole breadth of convexity of bowel, but nearer mesentery on one side. It is shaped somewhat like the forefinger, but twice as large, and is slightly bent to one side; and that end of the bowel which opens more directly into it is considerably larger than the other end. Termination smooth and rounded; the blood-vessels are injected, and are seen to cross over the bowel on both sides, and run along both the convexity and concavity of the diverticulum. On the concavity, an inch from the blind extremity, is a depression and puckering, and convergence of blood-vessels, as if a cord had passed off here. Small valvulæ conniventes are seen in the interior of the bowel, and a more distinct crescentic fold at the angle in the centre of the concavity of the portion of bowel.

15. Dried preparation. Diverticulum four inches in length. Origin somewhat abruptly from nearly whole breadth of bowel,

and nearer to mesentery on one side. Bowel much larger at one end than the other. Body of diverticulum wider than the commencement, increasing to within 1½ inch from the extremity, towards which it now tapers to a distinct apex, without appearance of ligament. Vessels are seen to run along that surface which lies nearest the mesentery of the intestine. Intestine destitute of valvulæ conniventes.

16. Dried preparation. Diverticulum three inches in length. Origin gradual, from about half the breadth of the intestine, and nearer the mesentery on one side. Is directed obliquely from the bowel, which is largest on the side next the obtuse angle, and somewhat bulged or uniformly dilated opposite the commencement of the diverticulum. Body of diverticulum is curved, with convexity on side nearest the mesentery, and has uniform diameter of ¾ inch, dilating in its last half-inch to a termination abruptly bulged on one side, giving it a transverse diameter of 1¼ inch. No appearance of mesentery or ligament. Intestine presents no appearance of valvulæ conniventes.

17. This specimen I examined in the recent state. Diverticulum three inches in length. Origin, gradual or somewhat funnel-shaped, from about ¾ of the breadth of the intestine, the diameter of the latter being 1¼ inch, and that of the diverticulum one inch. Direction oblique from the intestine, which is somewhat larger on the side of the obtuse angle. Body of diverticulum is round, and increases somewhat in diameter towards the end, where it is bulged or enlarged to one side. The termination is smooth and rounded, except to the side where it is bulged. Here there is a pointed extremity giving off a ligamentous cord, which was two inches in length, and blended with the mesentery.

I regret that this was not more exactly noted at the time, but from my notes made then, and the appearance of the dried preparation now, I believe I am correct in stating that there was a narrow mesentery posteriorly in which this terminal ligament was contained. The diverticulum is attached to the bowel nearer the mesentery on one side, that on which the mesentery of the diverticulum has been situated. The diverticulum was at once seen hanging free down in the abdomen, not tied up upon the mesentery of the bowels. The line indicating the attachment of its former mesenteric fold, is on one surface near that border of the diverticulum which is continued from the obtuse angle, a little posterior to where the ligament is seen in the illustration, fig. 17.

The intestine is entirely destitute of valvulæ conniventes, and the diverticulum was situated on it about three feet from the cæcum. The longitudinal and circular fibres are obscurely visible on it, but as much so as on the bowel, the coats being evidently continued from one to the other. The ligament at the extremity appears to arise or be continued gradually from the walls or tissue of the pointed portion of the diverticulum which runs towards it.

18. Dried preparation of the small intestine of a child, with diverticulum $1\frac{1}{2}$ inch in length. Arises somewhat abruptly from nearly entire breadth of intestine, and near mesentery on one side. Body of diverticulum is oblique and curved, and enlarges as it passes outwards, at middle having a diameter very little less than that of bowel, which is half an inch. Towards the extremity it again contracts, and from the apex is continued a narrow pointed portion, doubled upon the rest, in the preparation. There is no appearance of a mesentery or ligament.

Intestine destitute of valvulæ conniventes.

19. I removed this specimen at a *post-mortem* examination of a male aged 71, who died much emaciated, but without any apparent disease. Diverticulum $1\frac{1}{2}$ inch in length. Origin gradual from whole breadth of bowel, which is somewhat enlarged from before backwards opposite the diverticulum. It arose from the concavity of the intestine close to the mesentery, and passed up resting on and attached to the mesentery. It is somewhat conical, passing up to a rounded extremity, the diameter of the body being $1\frac{1}{4}$ inch, and that of the extremity 1 inch. Proceeding from its posterior aspect near the extremity, is a *secondary diverticulum*, forming a curved conical process, $\frac{3}{4}$ inch in length. This secondary diverticulum arises abruptly from the parent diverticulum at its back part near the extremity, a circular constriction passing round, across which some ligamentous fibres pass. It now curves downwards and backwards

into the mesentery. It is at first somewhat dilated, being here the size of the little finger, half an inch in diameter, and then tapers to a small rounded apex, from which a ligament passed. From the upper and fore part, near the apex, of the parent diverticulum, there projects a small pouch, like a tubercle or split pea, which has much the appearance of a hernia of part of the coats. It begins gradually at one side, and two-thirds round its base there is a slight constriction.

The parent diverticulum is evidently continuous with the structure of the intestine, both longitudinal and circular fibres being plainly continued from the one to the other. The circular fibres form a uniform layer, and at the base run obliquely across the bowel as its circular fibres, whilst the longitudinal fibres of the bowel turn and run up longitudinally, especially on one surface. The walls of the diverticulum and of the intestine have the same thickness. The secondary diverticulum is thicker and less transparent, but the circular fibres are obscurely visible. The little secondary hernia-like pouch is more transparent, and no fibres can be distinguished on its walls, as if it had passed through the muscular coat. In the dried preparation the apex of the secondary diverticulum is smooth and rounded, as if no ligament had been attached to it; this, along with the mesentery, having been removed before drying the preparation.

The intestine is wholly destitute of valvulæ conniventes. The diverticulum was situated on the bowel a distance of eighteen inches from the cæcum. In the illustration, fig. 19, to show the position and form of the secondary diverticulum, I have drawn it as if passing from the side; naturally it passes backwards, and is not seen in a front view.

20. Dried preparation. Diverticulum three inches in length and very wide, with secondary diverticulum an inch in length. Origin gradual, from concavity of intestine, close to mesentery. Diverticulum, at its base or origin, measures two inches in breadth, and $1\frac{1}{2}$ in thickness, being considerably larger than the largest side of the intestine. This on one side is twice as large as on the other. Diverticulum forms a wide blunt cone, the extremity smooth and rounded, but from its posterior aspect, near

to the extremity, passes off a secondary diverticulum, resembling, but larger than that in the last preparation. This secondary pouch is a curved cone, bent backwards upon the parent diverticulum. It begins by a constriction, is one inch in length, and at the middle $\frac{3}{4}$ inch in diameter. From its apex passes a ligament or band, forming the thick free edge of a narrow mesentery, $1\frac{1}{2}$ in length, which lies along the corresponding surface of the parent diverticulum, and is continued from the mesentery of the bowel. The circular fibres are plainly visible on the diverticulum, becoming gradually thinner towards the extremity, but they are more distinct on the intestine, the longitudinal fibres of which are likewise visible.

The intestine is destitute of valvulæ conniventes.

21. This specimen I examined in the recent condition, and it is now preserved as a wet preparation.

The diverticulum is $3\frac{1}{2}$ inches in length, intestiniform, curved, and bound down at the extremity, by the attachment of a terminal ligament. Origin funnel-shaped, from the convexity of the intestine, but nearer the mesentery on one side. The diameter of the intestine here is $1\frac{1}{4}$ inch. The diverticulum, at its commencement, has a diameter of one inch. The body of the diverticulum may be divided into three stages. The first inch is funnel-shaped, contracting at the end to a diameter of half an inch. The second inch continues to have this diameter, and the last $1\frac{1}{4}$ or $1\frac{1}{2}$ inch is contracted to a diameter of $\frac{1}{4}$ inch, the entire length of the tube being from $3\frac{1}{4}$ to $3\frac{1}{2}$ inches. From the narrow but rounded apex proceeds a ligament $\frac{3}{4}$ inch in length, which is inserted into or continuous with the mesentery, half an inch from the mesenteric margin of the bowel, at the part where the diverticulum arises. This ligament is formed, as it were, by the serous coat of the diverticulum continued from its extremity. It is at first a narrow flattened band, but expands triangularly as it joins the mesentery, with the serous surface of which it is continuous upwards and downwards, and it consists of two layers of serous membrane, containing some fat and cellular tissue. There is no mesentery along the diverticulum. There is no appearance of effused lymph or morbid adhesion.

The extremity of the diverticulum being thus bound down to the mesentery, a ring or oval space is formed, bounded chiefly by the diverticulum, partly by the ligament, by half an inch of the surface of the mesentery, and by the side of the bowel from this to the origin of the diverticulum. This ring has a diameter of about an inch, and admits the points of three fingers. Through this, as I shall afterwards relate, a loop of intestine had passed, and, becoming strangulated, was the occasion of death. As I had only a mass of intestines sent me for examination, I am unable to say how far from the cæcum the diverticulum was situated, but, as there are valvulæ conniventes moderately marked near the place, I suppose it to be about the beginning of the last third of the small intestine.

22. Wet preparation. Diverticulum six inches in length, vermiform, and at the extremity tied down to the mesentery by a ligament.

The ring thus formed was likewise the cause of death, and the constriction near the extremity, and the dilatation beyond, seem to have been acquired or caused by the peculiar position of the diverticulum in the strangulated mass, as I shall subsequently describe.

It arises by a funnel-shaped commencement from the bowel, which has here a diameter of one inch, whilst that of the diverticulum is about half an inch. Its diameter is pretty uniform until near the extremity, $1\frac{1}{2}$ inch after its origin there is a slight twist upon it, and at this part it is rather narrower than before and after. The last portion forms an abrupt oval expansion, one inch in length, and $\frac{3}{4}$ inch in diameter at the middle. This is separated from the rest by a narrow constriction, $\frac{1}{4}$ inch in length and diameter, and feels thick as if from the gathering together of the coats. On rubbing the coats between the finger, those of the diverticulum feel thicker than those of the bowel, and the dilated extremity of the diverticulum feels thicker still, though not so much so as the constricted portion. This constriction corresponds to where the diverticulum itself was embraced in the strangulating ring.

Attached to the extremity of the diverticulum, there is a peculiar pendulous body, soft, solid, and dark eoloured. It is covered by serous membrane, and, on division, is seen to be eomposed apparently of fat and cellular tissue, very like a soft substance lying in eonsiderable quantity between the two layers of the mesentery of the preparation, and is therefore apparently analogous to one of the appendices epiploieæ on the great intestine of fat subjects.

Passing from the point where this pendulous body joins the divertieulum, is a ligamentous eord. It is round, strong, and fibrous-looking; is two inches in length, and then joins the mesentery by a triangular expansion $\frac{3}{4}$ ineh in length, the serous surfaee of the mesentery running up each of its sides. The point where it becomes continuous with the mesentery is two inches up from the mesenteric border of the bowel. There is a narrow mesentery, $\frac{1}{8}$ inch in breadth, running along the concavity of the diverticulum, as far as the eonstricted point, within an ineh of the extremity, and arising below from the bowel, not from the mesentery of the bowel. The divertieulum is placed on the bowel, one-half nearer the mesentery on one side than the other. The bowel at this part presents no valvulæ conniventes, but there is a distinet traee of them about eighteen inehes higher up the canal.

In this case, then, it will be observed, there is formed a large ring, bounded for six inches by the diverticulum, for two inches by its terminal ligament, and for other two inches by the surface of the mesentery. Its diameter is about three inches, and it admits the whole hand.

Remarks on the preceding Cases.

Reviewing these notes of eases of diverticulum from the intestine, it is evident that they must be eonsidered as *original or congenital formations*, and not of the nature of herniæ or yieldings of one or all of the intestinal eoats. Figure 2, representing two false divertieula from the jejunum, presents a striking contrast in respect to the form of the projeetion, beginning by a constriction, and then bulging outwards, aneurism-like, in the mode in which a poueh formed by a dilating force from within usually enlarges. Here we find two pouches, and notice that they occur on the jejunum; but no instance has been recorded of the occurrence of two processes having the form or structure of the true diverticulum ilei. As long as the pouch was short or small, I would not regard a constriction at the commencement as a necessary character, as illustrated by the pouch on the œsophagus, represented in fig. 1; which, though almost as natural looking as the true diverticulum ilei represented in fig. 3, must still be regarded as a pouch formed by partial rupture or sudden yielding of the œsophageal walls into the recess between the bronchi, as we do not know of any other explanation, from development or otherwise, which could be given for the existence of such pouches.

Dilatations or pouches formed by rupture or yielding of one or all of the œsophageal coats occur, according to Meckel, only at the commencement of the œsophagus, as the pharynx contracts somewhat rapidly at this point, behind the cricoid cartilage, to the proper commencement of the œsophagus. A well-marked case in point, taken from William Hunter's collection, is related by Baillie,[1] in which a large pendulous pouch projected downwards and backwards from the commencement of the gullet. A cherry stone had rested there for a few days and formed a recess, which, during five years, became gradually dilated by the entrance of the food, and at last occasioned death by preventing the food passing to the stomach.

According to Rokitanski these cylindrical or conical dilatations or pouches on the œsophagus may form at all parts, but are most common near the bifurcation of the trachea. In fig. 1 it will be seen that the pouch exists precisely in this situation. Above this point the œsophagus is supported by the contact of the soft but still resisting membranous back of the trachea; but at the bifurcation of the latter it suddenly loses this support, and, although in all its course the œsophagus is as little supported on each side as it suddenly here becomes in front, it is still interesting to notice this occasional position of the pouch in the recess between the

[1] Morbid Anat. of the Human Body, London, 1799; Part 3, Plate I., fig. 2.

bronchi. This recess is naturally filled up by a mass of bronchial glands, usually of a dark colour; and in connection with this circumstance may be mentioned the case referred to by Rokitanski, "in which the mucous membrane of the œsophagus was dragged out in consequence of the shrivelling of an adherent tracheal gland."

The diverticula represented from fig. 3 to fig. 22, inclusive, are, however, evidently original pouches or processes, continuous with the tissue of the intestine. They appear all to have been situated near, or towards, the lower or colic extremity of the ileum. Of the two of which I had the opportunity of ascertaining the exact situation on the length of the canal of the small intestine, one, fig. 19, was eighteen inches, and the other, fig. 17, twice that distance upwards from the cæcum.[1] As to the situation of the diverticulum in the other cases, I can judge only by the valvulæ conniventes, as I had only separate portions of the intestine. We can, however, form a very accurate opinion usually as to the situation of a portion of bowel whether it was from the upper or lower end, or middle, of the small intestine, by the size or absence of these valvulæ. Very large and close together from about the entrance of the bile duct, downwards through the upper third, they gradually diminish in size and number in the middle third, and in the lower third are at first few and slight, and then altogether wanting. A portion of jejunum has thick coats, and when held up to the light, presents dark rings, and when everted or laid open the valvulæ are seen. A portion of ileum at the lower end again, has thin coats, is more transparent, and no dark rings or valvulæ are visible. The same distinction is easily made although the preparation is dried. When held up to the light the jejunum presents numerous dark characteristic rings, and the lower portion of the ileum is thin, transparent, and destitute of rings. If, then, a portion of small intestine is presented to us, either wet or dried, in which there are no valvulæ or dark rings, we can with certainty pronounce it to be from the lower end of the ileum. The statements of anatomists differ as to how much

[1] In another case since met with, the diverticulum was thirty inches from the cæcum.

of the ileum is wanting in valvulæ conniventes. It may vary in different cases, but M. Cruveilhier's statement, that "sometimes they are altogether wanting in the last two or three feet of the bowel," is, I think, incorrect, as representing them to exist too low down. My own observation would lead me to say that generally they are wanting in the lower third of the small intestine, that is for about six feet upwards from the cæcum, allowing twenty feet as the average length of the small intestines. In one instance in which the small intestine was twenty-four feet in length, the lower half was entirely destitute of valvulæ conniventes. At the commencement of the lower third, six feet from the cæcum, they are very small and scattered, or can scarcely be said to exist, and soon altogether disappear, so that (while I do not deny that sometimes they may exist nearer the cæcum) at least in the last four or five feet of ileum, there are usually no valvulæ conniventes; and therefore a portion of bowel bearing a diverticulum, and free from these valvulæ, or dark rings, may be safely pronounced to have been taken from within this distance from the cæcum.

Now this conclusion embraces all the specimens of true diverticulum, twenty in number, above described and figured. In one or two there were traces of valvulæ, where the diverticulum occurred, indicating that the situation was not more than five or six feet above the cæcum, whilst in all the others the intestine presented no valvulæ.

For this remarkable fact, that the true diverticulum occurs constantly near the cæcum (whilst the false diverticula occur indiscriminately at any part of the alimentary canal, and may be one or more in number), taken in connection with its evident original nature and continuity with the intestine, there must be a reason; and the view of Meckel that this explanation is afforded by the early connection between the vitelline duct and the intestine, appears a very satisfactory one, when it is further considered that the vitelline duct joins the canal at the same part where the diverticulum occurs, viz., at the lower part of the small intestine; and seeing that it is not a point capable of proof from actual demonstration, I am not aware of any objection that can be advanced against the conclusion, that Meckel's explanation is a very probable one, and a sound deduction from the consideration of all the facts of the case.

The occurrence of a cord or ligament passing from the extremity of the diverticulum is a point of considerable interest. This cord, which may be called the *terminal ligament*, may be looked upon as the remains either of the early vitelline canal, or of the more persistent pedicle of the vitelline sac, and of the omphalo-mesenteric vessels which passed along it. Its presence was distinctly noted in only five out of the twenty cases of true diverticulum above recorded. But I may observe that it may have been present in the cases of the dried preparations, and removed in the dissection without leaving any trace. This has been done in one of the cases in which I dissected it myself when recent, and now the dried preparation presents no trace of a terminal ligament, but a smooth apex. In all the cases in which the specimens were examined in the wet condition—Nos. 17, 19, 21, 22,—there was a cord or terminal ligament. In case 21, as already observed, this ligament was short and might pass for a serous adhesion, but this view of its nature was improbable, and in case No. 22, this ligament is a true cord, fibrous, rounded and smoothly covered by serous membrane.

On several of the specimens there are what I have called *secondary diverticula*. In Nos. 19 and 20 these aie very distinct. Also in No. 18 there is a narrower portion from the end; and in Nos. 16 and 17 there is a projecting corner or portion to one side, from which in No. 17 the terminal ligament passed off. These appear to be portions of the original diverticulum which had not undergone the same growth or enlargement as the rest. The form, as seen in figs. 19 and 20, seems to render it likely that the original diverticulum had been curved down, and that the part at the turn had become dilated, or that beyond it had ceased to grow.

The existence of a mesentery to the diverticulum does not appear to be constant. In the above dried preparations it may have existed, although no traces are now visible. In several there was a narrow mesentery running along one side. A mesentery is not necessary for such a process as it is for the intestine, where it is necessary both for the retention of the bowel and the conveyance of the vessels and nerves. In one of the cases, No 15, the vessels were seen to run along one side, that nearest to the mesentery of the bowel; and in another No. 14, they are seen to cross directly over the bowel on both aspects, and run along the diverticulum both on its concavity and convexity. That the diverticulum does not necessarily require a mesentery, as far as a conveyance for the blood-vessels is concerned, is also seen in a case figured by Cloquet.[1] "The appendix was three inches long. Its serous membrane was perfectly smooth. The mesenteric artery furnished a principal branch, which descended upon one surface of the appendix, giving off branches to the right and left; the other side was only supplied by some fine, irregular, vascular ramifications.[2]

With regard to their *relation to the intestine and mesentery*, it will be noticed that one or two came off at the mesenteric attachment, and ascended, adhering to the mesentery,—that one or two came off from the middle or nearly the middle of the bowel,—that is, equidistant from the mesenteric border; but that the great majority were placed upon the bowel nearer the mesentery on one side than on the other, having usually about two-thirds of the circumference of the bowel on one side, and one-third on the other. Whether this situation was anterior or posterior could not at any rate be determined from the examination merely of a portion of intestine; and in reality, when we consider how the convolutions of the intestines naturally lie, the terms anterior and posterior become inapplicable. We com-

[1] Surgical Pathology; by Jules Cloquet. With Plates. London edition, 1832. Plate XVI., fig. 1.

[2] Since the above appeared in the Journal, I have met with another specimen of true diverticulum ilei, in the dissecting room. As mentioned on page 152, it is situated 30 inches from the cæcum, and there are no valvulæ conniventes near it. It is 3½ inches in length, begins funnel-shaped, is cylindrical in form, of the same diameter as the bowel, and terminates by a smooth blunt extremity, without any terminal ligament. There is no mesentery to the diverticulum, but the blood-vessels are seen to run along both sides, especially that next the cæcum. The lining mucous membrane is natural, and is covered with villi. Here, then, there is neither mesentery nor ligament, but the diverticulum is free on all sides, with its smooth serous investment.

monly speak of the intestine presenting two borders and two surfaces—a mesenteric or attached border, and a free or convex border (although this is not a *border* in the proper sense of that term, in, at least, the distended bowel), commonly designated by the obscure expression of the part "opposite the mesentery," meaning thereby farthest from the mesentery. But the surfaces have no constant position; what is anterior at one time may become posterior at another, according as the convolutions change; what is the front of a convolution passing from left to right, may, by the turning over of that convolution, become posterior, and then the bowel is passing from right to left; as, it is clear, in the arrangement or retention of the bowels, there must be a turn to the left side for every one that exists to the right, except the last to join the cæcum. Nor has the mesentery itself, except at and towards its root, any fixed anterior or posterior surface. It rather lies laterally, like the ribs of a partially opened fan, that surface which is anterior joined to a convolution passing from left to right, becoming posterior on the next convolution, passing from right to left; and these, together with the convolutions, change about in regard to convexity and concavity, so that, of a sigmoid portion of bowel and mesentery, what was convex may be changed to concave. It is therefore deceptive to speak of the movable intestine having an anterior or posterior surface, and in expressing any relation or attachment, as that of a diverticulum, we can only say that it was placed on the bowel nearer the mesentery on one side than on the other.

Lastly, it will be observed of these diverticula, that the great majority have a more or less oblique *direction* from the bowel, and that the bowel on one side is larger, usually considerably larger, than on the other. Whether the larger side was the colic or gastric side in the above cases, I am unable to say with certainty, but am inclined to the view that the larger was the gastric end, which would give the conclusion that the diverticulum was usually prolonged from the bowel obliquely downwards, *i.e.*, in the direction in which the chyle or intestinal contents were moving, and it is conceivable that the obliquity had been determined in this way, although I do not propose this as more than a suggestion. There is usually no valve or other means to prevent a free entrance to the cavity of the diverticulum. When the acute angle is well marked, there is a small crescentic fold at this part, and again, in Figs. 11, 13, 14, 15, and 20, there is a small crescentic fold at the angle where the two sides of the doubled-in piece of intestine meet over against the mouth of the diverticulum, like the septum of the bronchi facing the trachea; but such a crescentic fold will always be produced by bending up any portion of intestine at the sharp point of the bend, even although the interior was previously quite smooth; and any such small crescentic fold seen on one side or near the mouth of the diverticulum, is not to be regarded as a valvular obstruction, or in any way involving this principle. The mouth of the diverticulum is usually funnel-shaped and smoothly open, so that the intestinal fluids must freely enter the recess, and again leave it by its own contraction.

Diverticula from the small intestine pathologically considered.

These diverticula may be considered pathologically under four heads. 1. As a cause of strangulation of the bowels. 2. As part, or the whole, of the contents of a hernial sac. 3. As predisposing to the lodgement and accumulation of foreign matters; and 4. As a part liable to disease.

1. Under the first of these heads are included two of the preceding list of cases, being the two at the end, Nos. 21 and 22, and one or two others have been recorded by authors.

Case I.—This is No. 21 in the notes and figures. The case occurred in the practice of Dr Archibald of St Andrews, and the intestines were put into my hands for examination by my friend, Dr Keiller. The patient, a young woman, was seized with symptoms of strangulation, and died after a short illness. The mass of intestines which I received was much gorged with blood, and there was a peculiar loop, embracing and strangulating a portion of the intestine, which was in a gangrenous condition, and easily gave way in the centre during the examination. On careful examination, in the empty state, and also by inflation of the bowels, I made out the arrangement already described in the notes, No. 21, and seen in fig. 21 A, and also the following particulars regarding the strangulation, as represented in fig. 21 B.

The diverticulum was, as I have described, bent over and fixed down to the mesentery by a short terminal ligament. Whether this ligament existed originally, or is of the nature of an elongated adhesion, I cannot positively say, only there is no appearance of adhesion or old inflammation elsewhere, and it has all the appearance of a natural serous ligament, or double fold of peritoneum connecting a viscus. A loop or *ring* was thus formed. When the bowel and diverticulum are inflated, it has the appearance I have drawn, fig. 21 A. The ring is oval, and, when rounded, an inch or little more in diameter. It is chiefly formed by the diverticulum, the rest being completed by the ligament, and a small extent of the mesentery and side of the bowel. Through this fatal ring, lying loose and open among the convolutions of the intestine, a convolution had passed. It is now easily drawn out of the ring, and the part embraced is marked off by two constrictions; the first is immediately above the origin of the diverticulum, and is but slightly marked; the second is fifteen to eighteen inches (according as it is measured along the concavity or the convexity), farther up the canal, and is well marked, the bowel being, for an inch in length, contracted to one-half its diameter. The narrower constriction appears to be higher up the canal, *i.e.*, on the gastric side of the other. These constrictions continue, notwithstanding the distension of the intestine with air. When the strangulated portion of bowel is replaced, and the intestine distended, forcible strangulation is then apparent. This will be readily understood when it is recollected that the size of the ring is not greater than the ordinary diameter of a moderately distended single piece of small intestine, whereas, besides two portions, there was, of course, also through it, the mesentery passing to the strangulated portion.

Case II.—This is No. 22 in the notes and figures. It is a wet preparation in the Monro collection, and in the printed catalogue is designated, "Process from the ilium which formed a knot round a fold of the intestine, causing strangulation of the included portion of the bowel; the process near its extremity is attached to a ligamentous band."

On careful examination of the preparation, I made out the anatomy described in No. 22, and the following additional points connected with the strangulation. We have seen that there was formed a large ring; first, by the diverticulum, six inches in length; secondly, by its strong terminal ligament, two inches in length; and thirdly, by two inches of the surface of the mesentery, besides a part of the side of the bowel; the ring being three inches in diameter, and sufficient to allow the whole hand to pass within it. Through this ring a portion of small intestine, eighteen inches in length, had passed; and, on withdrawing it from the ring, which is easily done, the strangulated portion is still plainly marked off by constrictions. One of these, moderately marked, is immediately above the origin of the diverticulum, where the bowel had turned round and passed through the ring close below it; the other is situated eighteen inches higher up the intestine, and is well marked, being, for an inch in length, as narrow as the little finger, whilst at the lower constriction the bowel is contracted for a third only of its diameter, for the distance of an inch and a half.

Eighteen inches then of bowel, with its mesentery, which is thick and loaded with fat, had passed through the ring; but this as yet had not caused strangulation, as, after distending the bowel on both sides, there is ample room for it, in the three-inch diameter ring, to move freely in and out, without obstruction or bending round the edge of the ring. The loose end of the diverticulum itself has turned round and passed through between the two pieces of intestine, and has thus tightened itself round one side of the bowel. Through the fatal ring is seen to have passed, as shown in the diagram, fig. 22 B—first, the two sides of the knuckle of intestine, with the accompanying mesentery included between them; and, secondly, between these and in front of their mesentery, the pendulous body, the last inch of the diverticulum, and the contiguous portion of the terminal ligament.

But for the end of the diverticulum also passing through the ring, there would have been no strangulation with this amount of intestine and mesentery, as the ring is wide and loose, but, by the end of the diverticulum passing through, its first porti n became tightened and the ring narrowed. There is first that side of the bowel where the divertieulum arises, strangulated by the diverticulum drawn across its front, the mesentery compressing it behind; then the other side of the strangulated bowel is erossed in front by the diverticulum, which then turns round behind it, and then forwards through the ring, thns going completely round this end of the bowel. The farther the diverticulum made its way through the ring, the tighter it would draw the ring and constriet especially that end of the bowel whieh it embraced on all sides. On the latter, accordingly, the stricture is now best marked. The narrow eonstriction on the diverticulum itself within an inch of its end, is now seen to be the part where it is eompressed by the ring, and the dilated last inch is the strangulated part beyond the ring. The ligament is not eoncerned in producing the strangulation, although primarily it formed part of the large ring. It has, as seen in the sketeh, where it is represented by a dotted line, also passed through the ring, but it not tight, and would aet rather in preventing the diverticulum from going further through, if that were possible.

The great resemblanee between these two eases will be rendered more apparent by referenee to the figures 21 and 22. These, it will be understood, are merely outline sketches. They were made with the preparations before me, and convey a more accurate idea of the anatomy and arrangement, than could have otherwise been done. Fig. 21 A represents the diverticulum and the fatal ring formed by it, and its terminal ligament, and the surface of the mesentery between the ligament and the bowel; the moderate eonstriction on the bowel elose above the divertieulum, and the second eonstriction which was eighteen inches higher up. Fig. 21 B shows the manner in which the ring embraced the portion of intestine and mesentery. Figs. 22 A and B are eorresponding views of the preparation in the second case, showing the parts as already described. This preparation is figured by Monro. Of this I was not aware until after the above description and my two sketches of it were in the hands of the printer and engraver. Monro's figure (Plate III., p. 179, *op. cit.*) shows the portion of bowel strangulated by the diverticulum encircling it, and the latter itself through the loop. No farther information is furnished, either as to the anatomy of the diverticulum, or the history of the case, than that above given in the extract from the printed catalogue.

A case remarkably similar to the two preceding, in its principal features, has been recorded by Professor Pirrie of Aberdeen.[1]

A lad seventeen years of age, and in good health, was suddenly seized with symptoms of strangulation, and died in sixty hours from the commencement of the attack. On dissection, Dr Pirrie found a portion of intestine, about twelve inches in length, strangulated through a loop formed by a diverticulum adhering to the mesentery by a ligament, the included twelve inches of bowel being greatly distended and in a state approaching gangrene. There was no adhesion between the loop and the included bowel, as the latter could be drawn out of the loop.

Two constrictions are seen on the drawing given by Dr Pirrie, one close above the origin of the diverticulum, and moderately marked, the other twelve inches farther up, deeper and more abrupt, contracting the bowel to about half its diameter. The diverticulum was $1\frac{1}{2}$ inch in length, and the terminal ligament being figured as of the same length, and as attached to the mesentery two or three inches from the edge of the bowel, the ring must have had a diameter of about two inches.

"The diverticulum," says Dr Pirrie, "at its entrance into the intestine, was furnished with a semilunar valve, formed by a doubling of the mucous membrane; but the diverticulum communicated with the intestine, and contained intestinal matters. It was $1\frac{1}{2}$ inch in length, and terminated in a slightly dilated cul-de-sac; from the extremity of which a membranous band was sent off, one extremity of which was evidently continuous with the serous coat of the diverticulum, and the other as evidently not merely attached to, but becoming continuous with,

[1] Monthly Journal of Medical Science. July 1849. "A remarkable case of strangulation caused by a diverticulum."

the anterior lamella of the mesentery." In the drawing, it is seen to leave the intestine near the mesentery on one side by a funnel-shaped origin. It is directed with very little obliquity, and has an average diameter of $\frac{1}{2}$ to $\frac{1}{3}$ inch, enlarges somewhat towards the extremity, and presents two slight constrictions, which, however, appear to be accounted for in the drawings by their position as they present themselves at different parts of the strangulating loop. It is not said how far from the cæcum the diverticulum was placed on the intestine; but from the statement, in the description of the pathological appearances, that "the lower third of the *ileum*" was collapsed, I may infer that it was about four feet above the cæcum.

I am also indebted to Dr Pirrie's paper for a reference to the catalogues of the Museums of St Bartholomew's Hospital and the College of Surgeons of Ireland.

The catalogue of the collection at Bartholomew's contains the following reference to a preparation which appears very closely to resemble the three preceding ones. "Portion of small intestine, from which a diverticulum is continued. The extremity of the diverticulum is adherent to the contiguous part of the mesentery, so as to form a circular aperture or ring. Through this aperture a portion of intestine, twelve inches long, passed and became strangulated. The patient, a lad subject to constipation, died four days after signs of strangulation of the intestine.

Referring probably to this case among others, Mr Lawrence[1] observes regarding internal strangulation, "Membranous cords forming adhesions frequently cause strangulation. They may be attached to any part of the cavity, or of its contents. The appendix vermiformis, the Fallopian tube, the omentum, and diverticula of the small intestine, when fixed at their loose extremities to some neighbouring part by such adhesions, may soon cause death in this way. I have seen several examples of such occurrences." Also Rokitanski mentions in his Manual of Pathological Anatomy, among the causes of internal strangulation, "an intestinal deverticulum (*verum*), which is directly or indirectly,

[1] Treatise on Ruptures. London, 1838.

by means of an obsolete vascular cord, attached to a certain portion of the peritoneum;" but he does not refer to any case.

A more dangerous condition than such a ring lying loose and open among the coils of the small intestine could scarcely occur; yet, unless we adopt the improbable view that the ligamentous cords passing from the extremity of the diverticula were of the nature of elongated morbid adhesions, and therefore comparatively recent, we see the patients had attained nearly the adult state before the accident of the fatal passage of the intestine though the opening had actually occurred.

The only natural ring or opening in the abdominal cavity is the foramen or canal of Winslow, and it has happened that a coil of intestine has passed through this opening, and become strangulated. Mr Lawrence refers to M. Jobert as an authority for this occurrence, and Rokitanski mentions that he "once found a large portion of small intestine strangulated in the fissure of Winslow."

We may say it is well that the foramen of Winslow exists above and not below the transverse meso-colon. Before the small intestines can reach the entrance of this passage between the greater and lesser sacs, they would require to be displaced over the transverse colon. It is interesting to observe how the movable or loose intestines are kept together in the lower region of the abdomen. They are encircled by the great bowel, and kept down by the transverse meso-colon and colon, from the latter of which the great omentum hangs down in front of them; the transverse meso-colon lies horizontally above them, and forms what I am in the habit of calling the great floating partition of the abdomen, which separates the stomach spleen and liver above, from the coils of small intestine below, and moves up or down according as the hollow viscera below or above are distended or empty. It is evident the foramen of Winslow would be a dangerous aperture indeed if the floating coils of small intestine were permitted to ascend to its situation.

2. A diverticulum may pass down and form part or the whole of the contents of a hernia. The only writer who refers to this, so far as I am aware, is Monro, whose attention appears to have

been drawn to this point. He observes,[1] "The symptoms occasioned by such diverticula included within the hernial sac are in some instances somewhat different from those occasioned by stricture made upon a protruded turn of intestine. My grandfather has observed—'that, instead of an entire piece of intestine being thrust out, one side of a gut has been stretched out into an appendix cæca. When this happens, the ingesta will not be stopped in their passage towards the anus, and the patient will go to stool, even though a strangulation of the hernia should come on.' But when the diverticulum only is contained within a hernial sac, it sometimes happens that a very violent inflammation spreads from the diverticulum to the contiguous intestine, and proves fatal. The unnatural process does not constitute an essential part of the intestinal canal, and has been cut off when in the sac of a strangulated hernia. Dr Wardrop did so in a case of this description, and after the operation the patient went to stool, and had a complete recovery." No reference is made to cases as a foundation for the above general remarks.

3. When we consider the position of these diverticula, we would expect them to be very liable to receive foreign bodies, and be the seat of the accumulation of intestinal matters. A glance at the sketches in the woodcut will show that the intestinal contents must pass into them, almost as readily as past them into the canal below. When intestinal fluids are making their way actively down the canal, they must fill these diverticula, and they will remain full until, the intestine above having ceased to fill them, they are enabled by their own muscular power to empty themselves again. We would expect, then, that any foreign matters descending in the fluids would be very liable to pass into a diverticulum. On the other hand, it may be observed that they in this respect only resemble the normal cæcum, which is, in this sense, a diverticulum from the onward direction of the alimentary canal; and into it most foreign bodies and matters must go for a time, as soon as they pass the ileo-colic aperture, yet the lodgment and accumulation of such matters

[1] Op. cit., p. 94.

here is but a rare occurrence, even although, in the erect and sitting postures, the caput cæcum coli is a depending recess.

However, a case of such accumulation within a diverticulum ilei is referred to in the catalogue of the Museum of the College of Surgeons of Ireland. The preparation is thus described, vol. i. p. 38, "A diverticulum on the human intestine. This preparation was taken from a young woman who died of fever; it forms a circumscribed bag about the size of a turkey's egg, and opens into the ileum at a point about three feet from the cæcum, by a passage sufficiently large to admit the end of the little finger; it is filled with hard brittle matter, apparently the solid residue of fæces stained with bile. The omentum and intestine in the neighbourhood were closely joined to the tumour by adhesions, the result of a former inflammatory attack; this woman had complained for many years before death of occasional very severe pains in the abdomen."

4. In conclusion, I have to add one case in which a diverticulum verum ilei was the seat of disease, without any apparent cause. This is the same case as No. 17 in the notes, and fig. 17. I found it in an adult male subject, and, on inquiry, learned that the patient, previously in good health, died after a few hours' illness, with symptoms of ileus or gangrene, but that the bowels had continued to move. The diverticulum was in a state of grangrene or approaching gangrene, healthy at its origin, as was also the intestine itself, but the rest of the diverticulum was gangrenous-looking. There was no strangulation or constriction at any part, nor was any other morbid appearance found. The diverticulum was carefully examined also by eversion, but inside there was no appearance of ulceration or irregularity, and it contained air alone when first examined. The diverticulum was, without any ascertainable cause, simply in a state of gangrene.

XIV.[1]

ON VARIOUS POINTS IN ANATOMY;

CONSIDERED IN A REVIEW OF A MANUAL OF ANATOMY.[2]

[From the Edinburgh Medical and Surgical Journal, April 1854.]

Since the first edition of this treatise was published, in 1838, several text-books on anatomy have appeared, and become the favourites with the student. As systematic works, we have had the translation of M. Cruveilhier's anatomy, and the new edition of Quain's anatomy, by Mr R. Quain and Dr Sharpey, both of them complete and authoritative works; and, as less comprehensive works, the Manual of Anatomy, by Dr Knox, besides our author's well-known systematic manual, entitled "the Anatomist's Vade Mecum." As dissecting-room guides, treating anatomy regionally, and describing the parts as they lie in relation to each other, we have had the work of Mr Ellis, and also that of Mr Holden, both excellent guides in the dissecting-room; and, besides, we have the new and enlarged edition of the long famous Dublin Dissector, in which there is, to some extent, a combination of these two methods, by the introduction of dissections along with the muscles.

[1] I have reproduced this Review here, with the permission of the Editor of the Journal in which it appeared, as it contains my views and opinions on several points in anatomy. Although the work reviewed deserved, perhaps, a more severe condemnation at the hands of a teacher of anatomy, I would not have retained the critical remarks here if they could have been separated from the others.

[2] The Dissector's Manual of Practical and Surgical Anatomy. By Erasmus Wilson, F.R.S. Second edition, 8vo, pp. 626. London, 1853.

We feel somewhat at a loss to which of these classes to refer the work now before us. In the preface we are told the work is devoted to display by demonstration, "the component structures in their relation to each other, and their complicity in the injuries and surgical operations to which the body may be subjected," whilst the systematic anatomy is "treated of in my other anatomical work, the Anatomist's Vade Mecum." In accordance with this, the work is divided into chapters, embracing different regions of the body,—as Inferior Extremity—Superior Extremity—Thorax—Back—Head and Neck, etc.; and we hoped, on seeing this arrangement, that the work was intended truly to supply the deficiencies of the author's Vade Mecum as a trustworthy guide to the student of anatomy; but have been much disappointed to find that this work does not treat essentially of regional, or at least of relative, anatomy, or "of the component structures in their relation to each other," but is arranged in a manner peculiar to itself, and not so as to be incompatible with the greater past being a reprint from the author's Vade Mecum,—the 1845 edition of which we happened to compare with it.

The description of each region commences with some general remarks on the parts to be found in it, and on their surgical interest; but from this fair beginning, the description soon breaks down into systematic anatomy; first of the muscles, and then of the vessels and nerves, each of these being usually described not only separate from the others, but without special reference to them. There may be exceptions to this general statement, but we are correct in saying of the extremities and head and neck, where relative anatomy should be more especially studied, that the above remark regarding the arrangement is correct. For instance, in the region of the palm of the hand, we find, after the fascia, first the muscles systematically described, down even to the interossei, then the arteries, and then the nerves, whilst no description is given here of the long flexor tendons, either in the palm or within their fibro-synovial canals along the fingers.

Of all parts of the body the neck is that which separate systematic descriptions fail to convey a correct knowledge of, and where the relative anatomist is most at home, with his various triangles and their contents, one following the other. But we find our author, after describing the platysma and fascia, giving first a separate description of the muscles down even to those of the tongue, without regard to the nerves and arteries, which follow, also systematically, in separate sections, whilst the relative anatomy in each triangle is given, if at all, in such a way that the parts are rather mentioned as if to be committed to memory than described so as to add to the student's knowledge.

This we need hardly say is not relative anatomy. We admit that, in describing a region, there must or should be more or less systematic notice of the muscles, vessels, and nerves in it; but this is very different from giving an altogether separate and systematic description of each, and is compatible with a full account of the relative anatomy of each region, and a particular account of the relations of each muscle, vessel, and nerve, to other muscles, vessels, and nerves,—such as may be found in the works of Ellis and Holden, already referred to.

We have said that the greater part of the work before us appears to be a reprint of the author's Vade Mecum. The same paragraphs are used, sometimes in the same, and sometimes in a different order. The description of the muscles especially is the same, only, singularly enough, omitting here the paragraphs on the relations. The arteries and nerves are occasionally altered, and more fully described. In the description of the viscera of the thorax and abdomen, the brain and spinal cord, the organs of the senses, and the ligaments, there is very little alteration. It is right, however, to mention, that it is the first edition of this treatise, which appears to contain the original descriptions, which have been subsequently transferred to the author's systematic treatise, and this may explain why the three books so closely resemble each other.

Perhaps our author may reckon it an advantage that the same descriptions should be given in both of his text-books, as the same muscles, vessels, and nerves form the theme of both; but, being so, one of two conclusions are evident,—either that the descriptions which are appropriate in a short systematic manual, cannot be so in a manual professing relative anatomy, or, if they are so, that the appearance of the second book is altogether uncalled for.

But, as it may not be fair to condemn a book merely because the title is inappropriate, or the plan which the author has set for himself may not meet our approval, we shall now proceed to notice how the author has acquitted himself in the execution of his plan. On this score there should be little room for comment. In a student's manual, treating of matters of fact, which have long been familiarly known, and compiled by one who, even before writing the first edition, has had the "experience of seven years, engaged in the active duties of the practical teacher in the dissecting-room," the descriptions could scarcely fail to be correct, and suited to meet the difficulties which the student feels in his progress. Taken by themselves, many of the descriptions and hints for assisting the memory are good enough; and most of the illustrations are useful, well designed, and certainly almost all of them well executed by the artist; so that, indeed, the book might be recommended to the student as a good and useful one, were it not that we already possess several which are much superior to it, both in plan and execution.

But, while we think the descriptions well executed according to the plan of the work, we at the same time can by no means give to them our unqualified approval. There are not a few errors in point of anatomical fact; there is frequently a want of precision, and not a few omissions, unpardonable in any text-book, but more especially in one professing to treat of surgical anatomy.

We shall now notice a few of these, in the order in which we have happened to meet with them, and make such comments as may serve to furnish some information, as well as to point out the deficiencies of this treatise.

Inferior Extremity.—On page 8 there is a diagram of the thigh, of which we observe first, that the *saphenous opening* is placed too far inwards, as, when it is dissected, the outer edge of the opening corresponds usually to either the middle or the inner edge of the femoral artery; and secondly, that the apex of

Scarpa's triangle is placed on the inside of the thigh nearly half way down to the patella, instead of at four inches below Poupart's ligament, and just over the femoral artery. It is the upper, not the lower, border of the adductor longus that we have always been accustomed to consider as forming the internal boundary of the femoral triangle proper, just as we take the upper and not the lower border of the sartorius for the outer boundary.

Fascia of Scarpa.—The true or deep layer of the superficial fascia of the groin is not correctly described. It is described as a deep portion of the superficial fascia, separated from the other at the groin by the glands and superficial vessels, adhering to Poupart's ligament, passing down and becoming lost on the fascia lata, and also covering in the saphenous opening. Now, this fascia (which we shall call Scarpa's) is an important one, simple in its arrangement, and easily displayed if intelligently dissected.[1] * * * *

Important muscles and tendons are described without any reference to their surgical anatomy. Thus, there is no mention of the mode of effecting complete relaxation of the rectus femoris, as in the treatment of fractured patella,—that the hip must be somewhat flexed, in addition to extending the knee;—the anatomy of the tendo Achillis is meagerly described, and there is no mention of the operation of division of the tendon, or of the necessity of flexing the knee as well as extending the ankle, in order to relax the gastrocnemius completely, as in the treatment of the accident of rupture of the tendon; nor is any allusion made to operations on any of the tendons at the ankle; all points of importance according to our understanding of "surgical anatomy."

Superior Extremity.—In connection with the axillary and brachial arteries, we find no allusion to their varieties, or even to the fact that there is often a high origin of one of the arteries of the forearm, although this occurs as often as in one in every five subjects. Nor can we appreciate the author's objections to the

[1] To save repetition, I here omit the farther remarks on this subject, and refer to my paper on the Fascia of Scarpa, No. XVI.

operation of tying the third portion of the axillary artery, which may be done with ease and safety, where it lies in front of the conjoined tendon.

Another blameable omission is, the absence, in the description of the hand, of all allusion to the *thecæ*, or fibro-osseous canals, with their synovial linings, *for the flexor tendons.* The tendons are briefly noticed with the muscles of the forearm, but without any allusion to the thecæ or to the synovial lining and nutrient vinculæ within the canals; and with the ligaments of the hand, there is but a short notice of them, without any reference to their surgical anatomy. It is surely within the province of surgical anatomy to point out that these canals shut in the tendons along the fingers; that the incision for the relief of tension, or evacuation of matter, in whitlow, must be deep enough to lay this canal more or less open, without which the incision will be of no use; that an incision made longitudinally, and in the centre, will effect this purpose, and avoid the blood-vessels and nerves, whilst one made across, or obliquely, would divide the vessels and nerves, and, if it went deep enough, the tendons also, besides insufficiently opening the canal. If anatomists do not point out the reason for it, the student is not unlikely afterwards to forget the surgical direction as to incision here, for want of the free practice of which so many fingers are lost, or remain stiff after the loss of the flexor tendons.

Errors are made in the description of the passage of the tendons beneath the *posterior annular ligament* of the wrist. As our Edinburgh students know very well, this ligament is like a bridge with six unequal arches, each, without any variation, transmitting its tendon or tendons, with their synovial linings; and they could tell our author, as well as we can, that the tendons of the extensor longus digitorum and extensor indicis lie in the same groove and canal, not in separate ones; and that the extensor minimi digiti lies, not in a groove on the ulna with the extensor carpi ulnaris, but in a canal of its own, without contact with either bone.

In the description of the *thorax*, we happened to observe that no notice is taken of the dilatation of the last part of the great coronary vein into a muscular sinus, as was particularly described more than twelve years ago by John Reid. At the beginning of this sinus, some distance down the vein, we have found one or two valves, as described by Reid, as well as by several of the older authors; notwithstanding which, the remark has crept into several text-books, that the veins of the heart are destitute of valves.

Valves in the Internal Jugular and Subclavian Veins.—We may here also remark that we have always found a pair of valves, apparently unknown to most anatomists, at or near the lower orifice of the internal jugular vein, and also in the subclavian vein. This does not accord with the common statement that there are no valves in the veins of the head and neck, except in the external jugular vein; or again, that "the subclavian vein is the continuation of the axillary, but unlike it, has no valves." —(Quain's Anatomy, new edition). But our author does not mention that they exist even in the external jugular. The valves of the internal jugular vein, we have always found to occur close above its termination in the innominate, or from half an inch to an inch above its lower orifice. They are two in number, either transverse or oblique, and one of them is apt to be divided in slitting open the vein to search for them. We have found valves also in the trunk of the subclavian vein, near to its termination in the innominate, and are not sure but that, in the instances in which we met with one only, the other had been destroyed in slitting the vein. We have long been in the habit of pointing out these valves at the root of the neck, and find that they were known to former anatomists, as to Sir Charles Bell, who has indicated their existence by the swellings he has figured on the veins in this situation. The point comes to be one of practical interest. Can the distension of the right side of the heart and neighbouring great veins in asphyxia be relieved backwards by an opening in the external jugular vein? This point had attracted the notice of Dr John Reid, who has shown in his paper, "On the effects of venesection in renewing and increasing the heart's action under certain circumstances," that the heart's action may be renewed in asphyxiated animals, by an opening made in the external jugular vein. That there are valves in these veins there is no doubt, but they do not appear usually to resist injections, and would, therefore, probably allow of a return of the blood, especially in the then distended state of all these veins. The question is one deserving of further investigation on the human subject, and it would be interesting to institute a series of experiments to ascertain whether water thrown up, for instance, by the abdominal veins, would freely escape by a wound in the external jugular.

Neck.—Here we find a passage indicating a startling boldness in practice, upon the fascia of the neck, p. 231. "When, however, tumours form beneath it, as bronchocele, enlargements of the lymphatic glands, aneurism, etc., the pressure which it then exerts may be fatal to the patient, from compression of the trachea, larynx, and nerves, *unless* (the italics are our own) *the tension be relieved by incision.*"

The list of primary *branches of the external carotid* artery is made up to *ten*, by the addition of sterno-mastoid and parotidean branches. *Eight* we hold to be quite enough for the learner to master, besides being the correct number of regular primary branches. The sterno-mastoid occasionally no doubt receives a separate branch from the external carotid, but is usually supplied chiefly by two twigs, one from the superior thyroid, which crosses the carotid sheath to reach it, and a larger one from the occipital artery. The parotid gland, again, is supplied by several twigs, coming both from the external carotid and its temporal branch.

We observe the omission of all reference to varieties of the subclavian artery, or its branches, except in the statement that "when a fifth artery exists, it arises from the third portion, and is the supra-scapular." Now, all anatomists who have observed these arteries in the dissecting-room, or those who have studied Mr R. Quain's work on the arteries (*London*, 1844) know that it is very common to find a large branch coming from the third portion of the subclavian artery, as often probably as once in every three subjects, very near where a ligature would have been placed on the artery; and that this branch is seldom the supra-scapular, but is an artery which thereafter dips backwards, usually between the two trunks of the plexus, to reach the upper angle of the scapula under cover of its levator muscle, and is therefore, not the supra-scapular, but the posterior-scapular artery.

Brain.—The *corpus callosum* is stated to be four inches in length. It may be so in a brain soft and flattened out on a table; but the brain it will be granted is not longer than the cavity in which it lies, and at most the seven inches thus given are thus distributed;—corpus callosum, three inches; from corpus callosum to front of brain, an inch and a half; and from corpus callosum to posterior end of brain, two inches and a half. The position of the corpus callosum bears an interesting relation to its relative thickness at the middle, and at the anterior and posterior extremities. Such points are easily and naturally settled by the application of a foot-rule, which, in a small form, the anatomist will find a very useful instrument.

The *foramen of Monro* is described as "transmitting" the plexus choroides, and the veins of the corpus striatum, pp. 317 and 321. Now, the foramen of Monro does not transmit any vessels, or anything at all but serum, being merely an open passage between the lateral and third ventricles, and formed simply by perforation or continuity of the lining serous membrane. It is not sufficiently expressed in descriptions of the brain, that the foramen of Monro, is not, as the communication between the third and fourth ventricles is, a hole or passage in the nervous substance, but in the lining serous membrane only. Were it not for the reflection or continuation of the internal arachnoid, from the upper surface of the fornix outwards across the rest of the floor of the lateral ventricle, there would be a foramen or fissure of Monro all the way back beneath the edge of the fornix, and the lateral and third ventricles would form one cavity, by continuity both above and below the choroid plexus and velum interpositum; but the lining serous membrane prevents this, and only where the crus of the fornix bends round the shoulder of the thalamus is it perforated, so as to be continuous with the lining membrane of the third ventricle below.

It was, we presume, the want of appreciation of these points regarding the nature of this hole, formerly so interesting to the

Edinburgh student, which for some time after it was brought into notice by Monro *secundus*, led to a discussion among anatomists as to whether the hole really existed; the point lying in this, that it is not a hole in the nervous substance or brain proper, but only in the lining serous membrane, as is still better seen in cases of chronic collection of serum in the ventricles, when we have seen it large enough to admit the point of the finger. As the foramen of Winslow does not transmit the vena portæ, but is a serous passage between the lesser and greater sacs, and is separated from the vein by a serous layer, so is the foramen of Monro an opening between the serous cavities of the third and lateral ventricles, and the plexus choroides does not pass through it, but bounds it behind, with a delicate serous layer between.

It may be here observed that, although this foramen is named after Monro, it appears to have been well known to previous anatomists. Indeed, it is not easy to understand how it could have been overlooked. In the dissection of the base of the brain, when the grey matter in the floor of the third ventricle has given way so as to expose the ventricle, the two openings are so plainly visible behind the pillars of the fornix, that they could not have escaped the notice of the many accurate anatomists who described the brain before the time of Monro. The claims of that anatomist to the discovery, now just a century ago, of these communicating apertures, may be found in his work on the Brain, Eye, and Ear (1797); and the facts showing their existence to have been well known to previous anatomists, may be found in Sir Charles Bell's separate work on the anatomy of the brain, with engravings (1802), in which it is remarked—"the only difference is that Winslow says it is a passage betwixt the third and the two lateral ventricles, while Dr Monro says it is a passage betwixt the two lateral ventricles and the third."

The division of the middle portion or lobe of the *cerebellum* into several parts, each with a distinct name, is adopted; the superior into three parts, or at least three names, and the inferior into four, the names of which we abstain from repeating as a step towards their suppression. We cannot see any good purpose to be served by this subdivision, the greater part of which, besides, is unnatural, or at least not very apparent. We wish anatomists would agree to abandon the use of these terms, as serving only to dishearten the student in his efforts to master the already overburdened nomenclature in connection with the brain.

The *decussation of the anterior pyramids* of the medulla oblongata is mentioned merely as a free communication of the pyramids across the fissure by a decussation of their fibres. No mention is made of the interesting fact, as fully described by John Reid twelve years ago, that each pyramid, in chiefly decussating, passes, not merely across, but also backwards, so as to be continued into the lateral column of the opposite half of the cord.

There is a diagram, fig. 48, of a transverse section of the *spinal marrow*, in which the grey matter is very erroneously figured like two new moons joined across by a very broad grey commissure. The commissure should be narrow, and the anterior end of the crescent large and rounded, compared with the posterior. However correct the text may be, the student will still rather take his impression from the diagram.

Perineum.—The fascia covering the levator ani is not noticed in the description of the *ischio-rectal fossa.* Now this fascia is of much importance in connection with the fossa. By its union with the obturator fascia it closes the fossa above, and, by lining the levator ani, it effectually prevents, in abscess in the fossa, the passage of matter into the rectum, at least earlier than close above the sphincter, an inch or little more above the anus, where the internal opening in complete fistula may be constantly found.

When in the neighbourhood of the *obturator internus*, we are reminded to remark, that if our author, in the next dissection he sees of the deep gluteal region, will divide the tendon of that muscle and lift it inwards, he will see behind it not only the grooved cartilage and synovial membrane covering the lesser notch, but an appearance so striking that we are sure he will give it a prominent notice in future editions.

Of *lithotomy* we read, that "the proportional distance between the tuberosity of the ischium and anus is the Scylla and Charybdis of the operation; for approaching nearer the former would endanger the internal pudic artery; and the latter the rectum," etc. This remark regarding the former danger may be a common one, but still we think the pudic artery so safe, in an operation with an ordinary knife making its way towards the bladder, that the incision might come close to either the tuberosity or ramus of the ischium, or even shave off a slice of the bone, without touching the artery. Even when the ischio-rectal space and the anterior part of the perineum are deeply dissected, there is considerable difficulty in bringing the trunk of the artery into view: it lies bound down by the obturator fascia, and concealed above or within the edge of the tuberosity and ramus; so that to succeed in wounding it in the operation would require a considerable amount of design and dexterity with any ordinary lithotomy instrument.

There is no allusion to the supposed danger of dividing the *reflection of the pelvic fascia* on the side of the prostate, so as to endanger infiltration of urine below the peritoneum; and indeed the relations of the prostate and the formation of its sheath are but meagerly described.

We have heard a great deal said about the danger of dividing this reflection of the fascia, but to us it appears the danger is an imaginary one, if the lateral operation be performed according to the common directions. The line of the reflection crosses the prostate obliquely, downwards and backwards, leaving in front about three-fourths, and behind two-thirds of the prostate, below this line. If, when the section of the prostate was being made, the knife was directed upwards and outwards, and carried at the same time deeply back through and beyond the neck of the bladder, the reflection of the fascia might be divided; but if the knife cuts even horizontally, or, as is usually directed, downwards and outwards, the reflection of the fascia, though, like the pudic artery, not very far away, is still perfectly safe.

Anatomy of Fœtus.—In the description of the fœtal vessels and heart, there is no direct statement of the fact that the point where the *ductus arteriosus* joins the arch of the aorta is invariably beyond the origin of the left subclavian, a fundamental point in connection with the fœtal circulation; nor is it mentioned with the anatomy of the thorax, that the left recurrent nerve hooks round the arch to the *left* side of the remains of the ductus arteriosus, an important point in connection with the early development of the arteries and nerves. Nor is the *valve of the foramen ovale* sufficiently described, as lying to the left side of the aperture, serving to prevent the blood from passing from left to right during fœtal life, as well as to close up the aperture after birth, by becoming glued to the edges of the hole on its left or posterior aspect.

Of the fœtal *liver* we read, "the left lobe is as large as the right." This is a common remark, but an erroneous one, at least as applied to the fœtus near or at the full time. The left lobe is considerably, apparently nearly a half, smaller than the right; though of course proportionally to the right it is larger than in the adult, which we presume is the truth intended to be conveyed by the erroneous statements such as the above. Nor is the *distribution of the umbilical vein* correctly given by our author. It is said to give branches—"1, two or three which are distributed to the left lobe; 2, a single branch, which communicates with the portal vein in the tranverse fissure, and supplies the right lobe; and, 3, a large branch, the *ductus venosus*, which passes directly backwards, and joins the inferior cava"—p. 557. Now we have always found the arrangement to be this. It gives three or four branches on each side, those to the left lobe being larger than those to the right; some of these, or others, may be concealed, coming off between the vein and the fissure; and, having reached the transverse fissure, it breaks up into three branches, one to the left lobe first; one twice as large to the right lobe, which, after a course of half an inch, is joined by the vena portæ; and a third, in the middle, the *ductus venosus*, not larger than the branch to the left lobe, which, after a course of three-fourths of an inch, enters, not the vena cava, but the left hepatic vein, about a quarter of an inch before the latter ends in the vena cava.

The *ductus venosus* is not a "large branch," but a small one; it cannot carry above a sixth or an eighth part of the blood of the umbilical vein straight on. Far too much importance is usually given to it in descriptions of the fœtal circulation, as if it were of equal importance with the ductus arteriosus and foramen ovale.

In the fœtal horse it is even altogether absent.[1] In man it forms an escape or direct passage to near the heart for but a small part of the umbilical blood; the great part must first traverse the liver where some important change, probably of the nature of purification, is no doubt effected on it by this organ, so large in the fœtus although it is not yet engaged in the secretion of bile.

Of the *membrana pupillaris* of the fœtal eye, our author says "sometimes it remains permanently, and produces blindness." In the next edition this passage may stand thus—"It has been supposed that the membrana pupillaris sometimes remains permanent, and produces blindness; but this is an error, as no authentic cases are on record, to prove that it has occurred as yet."

Orbit and Eye.—On pp. 219 and 370 we have the old and erroneous description of the *tendons of the recti muscles* forming an expansion at their insertion; thus—"The insertion of the four recti muscles into the globe of the eye forms a tendinous expanion, which is continued as far as the margin of the cornea, and is called the *tunica albuginea*."

How this mode of description has come to be so generally adopted in text books on anatomy we are at a loss to understand, as it is so easily seen to be erroneous and entirely without foundation. This supposed coat was the tunica tendinea, or tunica albuginea of the old anatomists, the discovery claimed by Columbus, but before "known" to Galen. As long ago shown by Albinus and Zinn, not only is there no such membrane, but absolutely no arrangement in the least approaching to the formation of such a membrane. The error may have arisen from the use of the term "tunica albuginea," by which some may mean simply the white appearance of the sclerotica, while others fancied the supposed tendinous expansion of the ancients to be referred to in the use of that term. The term "tunica albuginea" ought therefore, to be altogether abandoned. But this source of confusion is scarcely an apology for the error of some modern anatomists in describing a tendinous expansion to be formed by the

[1] I make this statement on the authority of Mr Barlow, the accomplished anatomist of the Edinburgh Veterinary School.

recti tendons, and "continued as far as the margin of the cornea." A very slight examination will satisfy any one that these tendons are inserted abruptly into the sclerotica, having become only a very little broader, but far from anything like approaching each other. Their edges are as neat and clean cut as those of the muscles which end in them; their abrupt termination in the sclerotica is a little curved with the convexity towards the cornea, and about three lines from it; and they dip in and are soon lost by becoming blended with the fibres of the sclerotica; but neither in it, nor over it, is there anything like a membrane formed by the expansion or union of the recti tendons. In propagating this error in modern times, our author, though by no means singular in this respect, must be held to have had a principal share. We have not only the description, but, on page 371, a diagram representing the tendons joining each other, with beautifully curved edges, and forming a sheath over the front of the sclerotica; and, contrasting this with the statement in the preface to the first edition, that the descriptions "have not been copied from the works of his predecessors, but have been penned from the great book of nature," we trust we have convinced our author of his fallibility, and that he shares in common with many of us the tendency to see things as others have described them, rather than as they really exist.

We have equal fault to find with our author's statements regarding the *action of the oblique muscles*, besides that he places the insertion of the superior too far back in saying that it is inserted near the entrance of the optic nerve. But, as the action of the oblique muscles may be regarded by many as debatable ground, we shall remark only, that, whatever their actions may be, our author can give no proof that they are such as he describes, and that he seems to be quite unacquainted with all that has of late years been written in Dublin and in Edinburgh on this subject, all tending to establish the view of John Hunter and others, that they are provided for the purpose of rotating the eye on its antero-posterior axis.

We observe a diagram of the *nerves of the orbit* (fig. 27), in which the roots of the fifth nerve are traced to before and behind the olivary body, as if this were an admitted point in anatomy,—in which the third nerve is the same size as the sixth, instead of being four times larger,—and in which the long root of the ciliary ganglion is an eighth of an inch, instead of half an inch, in length, and the little ganglion itself is actually as large as the first division of the fifth pair.

The *structure of the cornea* is more fully described than in the first edition, but we find no mention of the anterior elastic lamina, discovered and described by Mr Bowman several years ago. We are informed that "the opacity of the cornea, produced by pressure on the globe, results from infiltration of fluid into the cellular tissue connecting its layers. This appearance cannot be produced in a sound living eye, although a small quantity of serous fluid (liquor corneæ) is said to occupy the areolæ of the cellular tissue." Can our author furnish any proof of the correctness of either of these three statements?

In connection with the *choroid*, we have a diagram (fig. 52) of the venæ vorticosæ, unlike any venæ vorticosæ we have ever seen; and we read—"in animals, the pigmentum nigrum, on the posterior wall of the eye-ball, is replaced by a layer of considerable extent, and of metallic brilliancy, called the tapetum." On the contrary, it will be seen, if the choroid be seized with the forceps near the tapetum, and pulled inwards, that the pigment is not here replaced, or wanting, but exists plentifully behind the tapetum, which is merely a brilliant arrangement of fibres on its anterior or internal surface.

The *ciliary ligament* is figured like a belt drawn round the eye, with an abrupt posterior edge; and nothing is said about the deeper and posterior part of it being muscular, as several observers have of late years pointed out; although we may observe, in passing, that they are wrong in attributing the view of its muscularity to Porterfield, who scarcely mentions the ciliary ligament, but describes the *ciliary processes* or *body* under this name, and strongly advocates the view that this part is muscular in its nature.

Of *Jacob's membrane* we read, "Dr Jacob considers it to be a serous membrane." Now, this implies the idea of a double reflected serous membrane, as Mr Dalrymple believed it to be when, in 1834, he wrote his book on the anatomy of the eye; but Dr Jacob, although originally he may have used the term serous, is careful to explain, writing in 1839, that it is not a serous membrane, part of his words being—"I have above stated my reasons for considering it a single layer, and not a double serous membrane;" and as the nature or minute structure of the membrane is now well known, and bears out Dr Jacob's description, it is hardly fair to assign to him, a living man in 1853, the antiquated opinion of another, which he had written to correct fifteen years before.

The *retina* is described, after Treviranus, as presenting minute papillæ on its internal surface, and our author would therefore seem to be unacquainted with the microscopical researches of Hannover and Bowman.

The *capsule of the lens* is mentioned without any reference to the important fact that it is several times thicker in front than behind; and the old error, that it "contains a small quantity of fluid, called *liquor Morgagni*," is reproduced, although followed by the remark that "Dr Jacob is of opinion that the lens is connected to its capsule by means of cellular tissue, and that the liquor Morgagni is the result of cadaveric change," p. 378. Now Dr Jacob not only expressed his belief, but clearly demonstrated the fact, that there is a strong adhesion between the lens and its capsule, that *there is no fluid here in a recent eye*, and that it is incorrect to attribute to Morgagni the deliberate belief that there is fluid here in the living healthy eye. That the capsule of the lens should allow a fluid to pass in after death, but exclude it during life, is not more wonderful than that the same capsule should so easily become opaque during life, but resist all means to make it so after death; or that the cornea should not absorb and swell out with the aqueous humour during life, as with water after death; the difference lying in the possession or loss of vitality; but whatever the explanation may be of the formation of the fluid which may always be seen to escape on puncturing the capsule of an eye not recent, especially if it has lain a day in water, it does not alter the fact, which we had thought every anatomist now knew, that in the recent eye there is absolutely none; and the single layer of cells by which the union between the capsule and the soft exterior of the lens is effected our author will find described and figured in Mr Bowman's lectures on the eye, published four years ago.

In fig. 54 the *lens* is drawn like that of a very old person, the axis or thickness little more than a third of the diameter or breadth. It is very common to find the lens figured out of all proportion to the eye, either too large or too small, too convex or too flat; but the proportions are easily struck if these points be recollected—that the breadth or width of the lens is one-third of the axis or diameter of the eye, and measures one-third of an inch, whilst the depth or thickness of the adult lens in the middle is one-sixth of an inch, that is, half the breadth.

We read that the free border of the *ciliary processes* "rests against the circumference of the lens," but in the diagram they are made to overlap in front of the rim so far, that were the pupil fully dilated they would be clearly seen from without in the living eye. The exact relation of the tips of the ciliary processes to the lens is a point of much interest. The above description accords rather with the view of Galen than that of modern anatomists, who are aware that the tips of the processes are free, not involved behind in the plaitings of the hyaloid membrane, but floating loose in the posterior chamber; not touching the rim of the lens, but lying a little in front of it; but how far they actually overlap in front of the rim is not quite certain.

The fact that they are not visible when the pupil is fully dilated might lead to the supposition that they were retracted along with the iris, from their connection with it, or from the action also of the ciliary muscle; but that they are not so altered we have found, on examining the eyes in the horse within half an hour, and the cat within a few minutes, after death, one eye having been previously brought fully under the influence of atropine, when no difference could be detected between the two eyes in the position of the tips' ciliary processes. Besides, the pupil of the human eye dilates only sufficiently far to equal the size of an average lens.

Of the *chambers of the aqueous humour* we read—"the two chambers are lined by a thin layer, the secreting membrane of the aqueous humour," p. 378.

How long will this error be continued in descriptions of the eye? No one ever saw or traced such a membrane, and we submit that, in a question of anatomy such as this, what has never been seen, either with the naked eye or microscope, does not exist, or must be held not to exist. The membrane of Descemet or Demours, so well described by Dr Jacob as the elastic cornea, is not, as some suppose, what is meant by the membrane of the aqueous humour, as it is a layer of the cornea, not reaching farther than into the ciliary ligament and origin of the iris. The single layer of cells on the surface of the cornea, in contact with the aqueous humour, extends no farther; if it did, the cells would be seen elsewhere as well as on the cornea, but there are no cells on the front of the iris, or on the capsule of the lens, nor on the back of the iris distinct from those holding the pigment of the uvea. The foundation for the view that there is a lining membrane to these chambers, is the supposition or idea that there must be one to secrete and contain the fluid, a serous membrane being, on the ground of analogy, supposed to be required. Analogy, we may remark, is at best not a proof or ground for inference, but merely suggestive of research to determine the fact. But we may ask, in what does the supposed analogy consist? Are there any serous membranes in the body, with the cavities distended with or containing a fluid? The halitus or steam of the older anatomists is now met with in a condensed condition only, as a moisture on the opposing surfaces of the serous membranes, and more than this would render them less fitted for their purpose. The only serous cavity naturally holding any serous fluid, and that but in small quantity in health, is the ventricular space, divided into the several communicating ventricles of the brain. On the other hand, there is only one instance of a considerable collection of serous fluid naturally existing in health, namely that around the spinal marrow and certain parts at the base of the brain, and there it exists, not in a serous cavity, although there is one near enough to it, but loose in the sub-arachnoid spaces, secreted by the vessels of the pia mater, as the aqueous humour no doubt is by the vessels of the iris and ciliary processes. The analogy then is rather the other way, agreeing with the fact, that no membrane can be traced lining these chambers.

THE JOINTS.—The last chapter embraces the anatomy of the joints, and is all but a literal reprint from the author's systematic manual, and also corresponds closely to the text in the first edition. So far from disputing the propriety of introducing the joints into a manual for the dissecting room and of surgical anatomy, we are of opinion that there is no part of anatomy of greater importance to the surgeon than that of the larger joints, and perhaps there is none more neglected; but we do not think such descriptions as those with which we are here presented can serve any practical or surgical purpose. The author's views regarding the study of the joints may be gathered from the circumstance that the chapter is headed, not of the articulations, but "Of the *Ligaments;*" and so it is with many, who seem to think that, in connection with the joints, they have only to commit to memory the names and attachments of the ligaments. Now, the study of the joints should embrace, first, a careful consideration of the form and adaptation of the articular surfaces, then the ligaments, then the synovial membrane, with the extent and recesses of its cavity, and lastly, and of great importance, the natural motions of each joint; not merely the names of these motions, but the nature and direction and extent of each. In connection with the hip-joint, for instance, it is not enough to mention, as we find here, that *rotation* is one of its motions, but the kind of motion which rotation is, inwards and outwards, the effect it has in rolling the whole limb, and the method of moving the limb so as to perform it, as in the examination of the limb after an injury, are all points quite as worthy of notice as the fact that there is a capsular or a round ligament. Farther, we conceive the surgical anatomy of the joints to embrace the notice of the osseous prominences outside and near the joint, and the relative position of these in the different motions. With such a knowledge of the joints the surgeon can usually see at a glance, when there is no fracture or dislocation, the natural figure of the limb and motions of the joint remaining; and when there is, he readily detects the nature of the injury.

We pass this general condemnation on the whole of this chapter, on the ground of its deficiency in practical and surgical bearing, and besides, notice several unpardonable omissions. Thus, in connection with the *shoulder-joint*, we are left to infer only that the long tendon of the biceps lies above the head of the bone. There is no allusion to the fact that it passes over the ball of the humerus, like a belt, serving, when the biceps acts, to keep it from rising up against the acromion process; or that the tendon lies in a grooved recess in the capsular ligament, the anterior boundary of which is so prominent that some have considered it as analogous to the round ligament at the hip; which, however, it is not truly, even although a probe may sometimes be passed round it, free within the joint, close to its attachment to the lesser tuberosity, and just in front of the commencement of the bicipital groove. There is consequently no allusion to the occurrence of dislocation or rupture of this tendon in the joint or groove, of which so many cases have been brought together in Mr Callaway's treatise on the shoulder-joint (London, 1849), which we would recommend to our author's notice.

Of the *hip-joint* it is enough to say, that not only is there no mention of the strong periosteum, with the raised fibrous bands on the neck of the thigh-bone, which may retain the fractured ends in apposition, as we have seen, but there is not even any direct mention of the pregnant fact that the neck of the thigh-bone lies within the cavity of the joint.

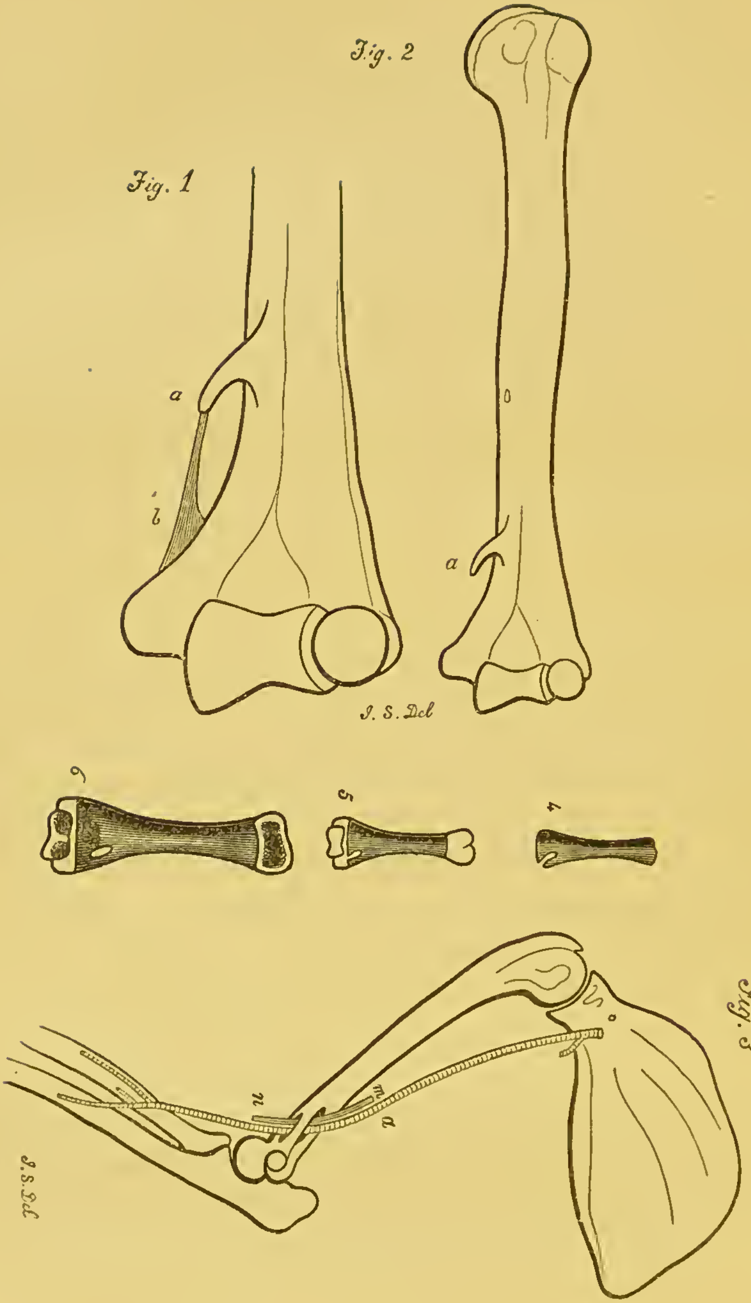
We would suggest for insertion in future editions, the general information as to the direction of flexion and extension of the great joints of the extremities, that, in the superior extremity, flexion is forwards and extension backwards at all the joints, whilst in the lower extremity, flexion and extension are *alternately* forwards and backwards, as at the hip, knee, ankle, and toes; from which it follows, that when a muscle crosses two joints in the upper extremity it either flexes or extends both, if in the lower extremity it flexes one and extends the other, except in the cases of the sartorius and gracilis muscles.

We would also venture to recommend to all anatomists the omission of such terms as flexors or extensors *of the thigh*, or flexors or extensors *of the leg*, and to substitute for them the expressions flexors and extensors *of the knee*, or *of the ankle*, as the flexors and extensors *of* the thigh are not the flexors and extensors *in* the thigh; the use of such terms leads to confusion, but no one can mistake what is meant when we speak of the flexors or extensors of any particular *joint*.

In connection with the *knee-joint*, the synovial pouches at the side of each condyle (into which a loose cartilage may be pushed and fixed by operation) are not mentioned; nor is there any mention of the prolongation of the synovial membrane carried out of the joint by the tendon of the popliteus muscle, the passing of which close to the back of the superior tibio-fibular joint, so readily explains how there can be, as there occasionally is, continuity of the cavity of this joint with that of the knee, a circumstance, which otherwise it would be difficult to account for.

The *bursa* is described as "placed between the anterior surface of the patella and the fascia lata. It is the latter which is inflamed in housemaid's knee." We have always found this bursa to be placed in the cellular tissue immediately under the skin, and to correspond to the lower part of the patella, that part of it on which we rest in kneeling, and not under the fascia lata. The facility with which swelling takes place in effusion into it, and with which, after suppuration, pus makes its way to the surface, corresponds to this superficial position of the bursa. We have, at the same time, often noticed a bursa, or apparent bursa, still deeper, and between the prolongation from the fascia lata and the fibrous covering of the patella; but never without a free communication with the regular sub-cutaneous bursa; and crossing the interior of this bursa, and also of that between the trochanter major and the gluteus maximus, may be often seen a series of soft fibrous bands, like cordæ tendineæ, giving a complicated appearance to the bursa.

In now concluding our notice of this manual, we trust that our readers will forgive us for the length of some of the digressions we have been tempted to make, on the ground of the information furnished, and the interest of the points discussed. We also trust that our author will be duly sensible of the obligations he is laid under to us for the corrections and hints we have ventured to bestow; and, for ourselves, we shall not grudge the trouble this has given us, as, besides, it has afforded us the opportunity of warning the student against being misled by a new and attractive title to an old inferior book.



Figs. 1 and 2. Illustrations of the supra-condyloid process in man. *a a*. Supra-condyloid process. *b*. Ligament completing the arch.

Fig. 3. Anterior extremity of cat, reduced one-half. *a*. Brachial artery. *m n*. median nerve.

Figs. 4 and 5. Humerus of kitten at birth, without and with the cartilaginous extremities.

Fig. 6. The same at fifth week after birth, also showing development of supra-condyloid foramen.

XV.

ON THE ABNORMAL ANATOMY OF THE ARM.

[From the British and Foreign Medico-Chirurgical Review, 1854.]

A considerable number of varieties in the anatomy of the brachial region have fallen under my notice in the dissecting room, and some of these are so rare and interesting that I have thought it worth while to draw up a systematic account of them, and especially to call the attention of anatomists to the occurrence of the supra-condyloid process on the humerus of man.

I shall arrange my cases and remarks under three heads—varieties of the muscles — the supra-condyloid process — and varieties of the arteries.

I.—Varieties of the Muscles in Relation to the Axillary and Brachial Arteries.

1. *The Axillary Artery Crossed by a Muscular Band.*

It is noticed by several anatomists that, occasionally, a slip of muscle is prolonged from the latissimus dorsi, across the axilla, to join the tendon of the pectoralis major, the coraco-brachialis, or the fascia over the biceps. This variety has occurred in eight out of 105 subjects dissected in my rooms since the first instance was noted. Of these eight, at ages between 3 and 70, three were males and five females. In seven, it was present on both sides, of equal size in five of these seven, and in two of them broader on the left than on the right side. In one it existed on the right side only, and a few scattered fibres were prolonged from the pectoralis major to join it. These fibres were present on the left side also, but not the slip.

This abnormal muscular band usually arises from the upper edge of the latissimus dorsi, about the middle of the posterior fold of the axilla. It is usually, I find, not a direct prolongation from the fibres of the latissimus, but, while the higher fibres may be so, the greater part or whole arises by a tendinous intersection between it and the fleshy fibres of the latissimus, or by a short intervening tendon, which in one instance was an inch in length. The muscular band varied from a quarter to three-quarters of an inch in breadth, in one case it was as thick as the pectoralis minor where this muscle crosses the axillary vessels; and its length varied from three to four inches. It disappears under the tendon of the pectoralis major, and in four of the cases where it was afterwards traced, it formed a tendinous expansion, and became blended with the tendon of the pectoralis major.[1]

The point of greatest interest in connexion with this variety is the position of the muscle in relation to the axillary artery, as

[1] This corresponds to the normal arrangement in some animals. It is well seen in the cat. As the latissimus dorsi approaches the axillary vessels it divides, the anterior part, being about a fourth of the entire muscle, passes across or in front of the axillary vessels and nerves, and ends in an expanded tendon, which above joins with the deep part of the pectoral muscle, and below expands as an aponeurosis binding down the flexor radii muscle, which corresponds to the biceps of man. The great part of the latissimus crosses behind the vessels and nerves, with the teres major, and rather higher up than the anterior portion. Both are fleshy where the vessels and nerves lie between them.

The whole mouth of the axilla is closed over by a thin muscular layer, continued upwards to the edge of the pectoral muscle from the edge of the anterior part of the latissimus, and also partly from behind from the panniculus muscle. The fleshy fibres forming the anterior part of the latissimus are continued onwards without any intervening tendinous intersection. In the cat this anterior part of the muscle as it crosses over the vessels and nerves is ½ inch broad. In the lion, it forms a tendon broader than the conjoined tendon of man, though not so thick, the lower edge continued into the aponeurotic expansion over the brachial region, the upper edge rounded and free; and the tendon, having crossed over the vessels and nerves, runs in underneath the pectoral muscle to be inserted into the same ridge on the humerus, having formed at the same time a large expansion over the flexor radii muscle, which it thus binds down.

it might mislead the surgeon in the operation of tying this vessel. It crossed always in front of the axillary vessels and nerves, and usually opposite the upper part of the conjoined tendon of the latissimus dorsi and teres major. It occupied this situation in all of the eight instances, except the one in which it was present on one side only, and, in this instance, it crossed as high up as opposite the tendon of the subscapularis muscle, and an inch higher than the edge of the conjoined tendon. It is usually fleshy where it crosses the vessels, but in one instance was tendinous on its posterior surface at this point.

As the artery in the upper part of the third stage of its course, opposite to, or at the lower edge of the subscapularis muscle, gives off large branches, the circumflex and subscapular,—the part to be chosen for operation is where it lies upon the conjoined tendon,[1] and therefore a little below where this muscular slip crosses over the vessels. It will, however, be exposed in the operation. In one of the cases, the operation was being practised by one of the pupils, and the ligature had been applied below the muscle on one side and above it on the other. In another of the cases I was showing the operation, and the muscle was exposed in the wound and recognised at a short distance above the point where the artery was tied. The fibres of this unusual muscle are transverse, whilst those of the coraco-brachialis, the only normal muscle which should be seen in the operation, are vertical.

2. *Muscle concealing the Brachial Artery in the Upper Third of the Arm.*

This variety occurred in the right arm of a male subject in 1851. The coraco-brachialis muscle is twice the usual size, and sends a muscular layer over the vessels and median nerve. This muscular covering begins at the lower edge of the conjoined tendon, and reaches down for three inches, near to the insertion

[1] I use the term "conjoined" tendon as it is a convenient one in describing the relative anatomy of the parts. Strictly speaking, there is but a very partial insertion of the fibres of the teres major into the tendon of the latissimus; and a little farther on they are altogether separated by a bursa, the teres ending in the internal bicipital ridge, while the latissimus glides over the ridge and bursa and is inserted in the floor of the bicipital groove.

of the coraco-brachialis. It is of considerable thickness, passes quite round to the inner side of the vessels, and joins the internal head of the triceps, or a tendinous septum between them. There is thus formed a kind of tunnel, which admits the little finger, and ends below by a distinct tendinous arch, the outer side of which is the ordinary tendon of insertion of the coraco-brachialis. This tunnel incloses the undivided brachial artery, its venæ comites, and the median nerve, in their usual relative position. There is no other variety, except that the posterior circumflex artery arises with the superior profunda, and, as usual, when it so arises, courses up behind the conjoined tendon, to gain its usual position at the neck of the humerus.

During the present winter I met with an example of an approach to this variety. The coraco-brachialis sent a thin muscular prolongation inwards over the vessels and nerve, as far as to conceal them partially, but on division of the aponeurosis continued from it, it passed outwards and left the sheath exposed.

3. *Brachial Artery concealed in the lower half of the Arm by a Broad Third Head to the Biceps Muscle.*

This variety occurred in 1848 in the right arm of a male subject. The brachial artery is concealed in the lower half of its course by a broad thick muscular layer, extending from the intermuscular septum to the biceps muscle. The artery, its venæ comites, and the median nerve, disappear through a tendinous arch, situated a little above midway between the internal condyle and the lower edge of the tendon of the teres major. This fibrous arch is about an inch in length, extending obliquely downwards and inwards, and is continuous above with the insertion of the coraco-brachialis, and below with the intermuscular septum.

The unusual muscle arises from this arch, and from the intermuscular septum, as far down as to within an inch of the internal condyle, being a length of four inches and a half, leaving only a narrow cellular space between it and the pronator teres. The fibres pass downwards and outwards, approaching and accompanying the inner part of the biceps. About one half of the muscle ends in the tendon of the biceps, just where that tendon receives the belly of its muscle, and the other half ends in the semilunar process from the biceps to the fascia of the fore arm. It forms an uninterrupted muscular layer of a thickness between an ordinary sartorius or gracilis muscle, concealing the brachial vessels and median nerve in more than the lower half of their course. As it approaches the biceps it becomes narrower, from its obliquity, so that where it joins the biceps it is only two inches in breadth. When the upper part of it is held inwards from the biceps, the upper part of the artery is exposed, but behind its lower half the artery could not be exposed without division of the muscle. The radial artery appears as usual between the tendon and semilunar process of the biceps.

On raising the biceps and looking under it from the outside, this large unusual third head is seen to be double, or split by the artery, a portion lying underneath the vessel, besides that already described. This deeper portion corresponds to the upper half of the superficial portion. At its origin from the intermuscular septum and internal margin of the humerus, it is two inches in breadth, but contracts below to three-fourths of an inch, and joins partly the tendon and partly the semilunar process of the biceps. The lower edge of this deep portion seemed to twist round or cross over the artery on its outside, to reach the semilunar process in front of it. This deep portion was thicker, though narrower, than the superficial. The fibrous arch protected the vessels and nerve from pressure as they entered on the deep part of their course, but it is not easy to see how below this the artery could escape compression, as it lay close between the two layers of the muscle.

We have here then an instance of a large third head to the biceps muscle, consisting of a superficial portion, forming a broad continuous muscle concealing the lower half of the brachial artery, and of a deeper portion, lying at first beneath the artery, and about half the breadth of the other.

On the left side of this subject, the biceps had a third head, corresponding to the deep portion of the third head on the right side, and like it, lying behind the artery. There was no other variety on the left side.

4. *Muscular or Tendinous Slip passing inwards across Brachial Artery to Lower Third of Arm.*

The three following instances have occurred during the present winter session :—

(1.) *Slip from Biceps across Brachial Artery.*—This fleshy slip is three inches in length, $\frac{1}{4}$ inch in breadth, and $\frac{1}{8}$ inch in thickness, and ends in a broad aponeurotic tendon two inches in length. The muscle separates from the inner edge of the biceps two inches below the lower margin of the teres major. It crosses obliquely over the brachial artery and conceals it for an inch and a half. The belly having just crossed to the inside of the artery, the tendon is placed to the inside, as it passes down to end in an aponeurosis over the pronator teres, close above and to the outer side of the internal condyle. The inner edge of the tendon is joined to the intermuscular septum. Between its outer margin and the tendon and semilunar process of the biceps, there is now a triangular space $\frac{3}{4}$ inch in breadth below, but they appear to have been joined across in front of the artery by a thin portion, although this cannot be determined now. This variety occurred on the left arm of an adult subject, and appears, from the remains of the tendon, to have been present on the other side also.

(2.) *Muscular and Tendinous Slip crossing over Brachial Artery.*—This variety occurred in the left arm of an adult female subject. The muscle arises from the external bicipital ridge by a long tendon, and again ends in a tendon, which is inserted into the aponeurosis over the pronator teres. The upper tendon arises from the upper part of the bicipital ridge of the humerus close to the great tuberosity, and crosses obliquely behind the long tendon of the biceps in the groove. After a course of $2\frac{1}{2}$ inches it ends in a fleshy belly, which appears on the inside of the arm between the biceps and coraco-brachialis muscles; passes down along the inner edge of the former and parallel to the outer side of the artery, which it now crosses obliquely, and, after a course of three inches, ends in a narrow flattened tendon, which is three inches in length before it spreads out over the pronator, $\frac{3}{4}$ inch below the condyle. The fleshy portion is $\frac{1}{6}$ inch, and the lower tendon $\frac{1}{12}$ inch in breadth, as they lie in front of the artery. They cross the artery very obliquely so as to lie in front of it for three inches, one inch of the fleshy and two inches of the tendinous portion. The artery lies in a groove in the brachialis anticus muscle, from the raised portion on the outer side of which is sent off, over the artery, an aponeurosis to join the tendon of this abnormal muscle, which is quite distinct from the intermuscular septun.

(3.) *Tendinous Slip from Pectoralis Major to Internal Condyle, crossing over Brachial Artery.*—This variety occurred on the right arm only, in an adult female subject. The pectoralis major gave off its usual expansion to the aponeurosis of the arm. The tendinous slip comes from below this, from the deeper part of the tendon of the pectoral, as a cord or tendon about the size of a common probe. It crosses over the brachial artery obliquely, at and below the insertion of the coraco-brachialis. It now lies behind the basilic vein and internal cutaneous nerve, passes to join the true intermuscular septum, and is continued with it to the condyle, an inch above which it also joins with the ligamentous cord behind the septum, from which, above, it is quite distinct. As this long ligamentous arch passes down the arm, its outer or anterior arch is free and rounded, and the posterior edge is continued backwards into the deep fascia of the arm. It lies obliquely over the artery for about an inch just at and a little below the tendon of the coraco-brachialis. In an operation, this slip might mislead from its resemblance to the intermuscular septum.

Remarks.—The two cases first related exceed any that I have read of in the extent to which the artery was concealed by a muscular covering. Mr Quain[1] has met with a case in which the artery was covered, for an inch and a half, by a thick muscular slip from the biceps to the intermuscular septum (Plate xxxvii. fig. 5). He also mentions another in which a third head to the biceps crossed over, instead of under, the artery; and refers to a preparation in Mr Allan Burns' museum, in which

[1] The Anatomy of the Arteries of the Human Body. London, 1844.

slip of muscle passed across the brachial artery, and is said to have impaired the activity of the limb during life. The surgeon should be prepared to meet with these varieties of the muscles. Certainly, the operation of tying the brachial artery in the two cases first related would have been a puzzling one, from the resemblance of the unusual muscle to the natural coraco-brachialis and brachialis anticus. But, seeing that the incisions were well placed, the nature of the difficulty might be recognised, and the pulsation of the covered artery, and the effect of pressure on it, would probably indicate the position in which it would be found on division of the muscle. If there happened to be a high division, and one of the arteries placed under such a muscle, it could scarcely but be overlooked in the operation.

5. *Brachial or Third Head to Biceps Muscle.*

The occasional occurrence of a third head to the biceps has long been well known to anatomists. Theile[1] states that it occurs as often as once in eight or nine subjects. In one case there were two additional heads from the humerus. I have noted this variety in four instances during the present session.

(1.) Third head arises as upper and inner part of brachialis anticus, with the fibres of which it is here quite continuous. It is half an inch broad and six inches long, and ends in the upper and inner part of a broader than usual semilunar process, and in the contiguous part of the tendon of the biceps. Left arm: no third head on right.

(2.) Right arm, none on left side. Arises as highest and most internal part of the brachialis anticus, immediately outside insertion of coraco-brachialis. Is half an inch in breadth and five inches in length. Inserted, fleshy, into deep surface and inner edge of tendon of biceps at its commencement, and a few fibres run into the semilunar process.

(3.) Both arms. Is small and narrow like a flattened lumbricalis muscle. Arises by a short tendon, as in the preceding cases, and inserted also as in them.

[1] Traité de Myologie et d'Angéiologie. F. G. Theile. Encyclopédie Anatomique. Paris, 1843.

(4.) Right arm. Left unascertained. Arises separate from the fibres of the brachialis anticus, for two inches in depth, immediately external to insertion of coraco-brachialis, but reaching an inch above this, and also higher up than the brachialis anticus. Is one inch in breadth and six inches in length. Insertion exactly as in preceding cases. As the third head in all of these cases passed, as usual, behind the brachial artery, this occurrence does not affect the surgical anatomy of that vessel.

6. *Brachial Artery overlapped or covered by a portion of the Brachialis Anticus Muscle.*

Mr Quain relates two cases in which he saw the principal of two arterial trunks covered by a thin layer of the brachialis anticus muscle (Plate 37, figs. 3 and 4), and mentions in connection, with this that the fibres of the brachialis not unfrequently project at the outer side of the artery, in some cases even overlapping it. This condition I have repeatedly noticed, so often indeed that I gave up noting the individual cases. It occurs more frequently perhaps in the case of two arteries, when the deeper one has this position, but often enough it is seen with the single and otherwise normal artery. It is seen in various degrees. The brachialis presents a raised portion on the outside of the artery, and an aponeurosis, deeper than the common aponeurosis of the arm, is sent inwards from it, over the artery, which is thus closely bound down upon the brachialis, and is as if sunk in a groove in the substance of the muscle. In a farther stage the fleshy fibres overlap the artery from the outside, although not so far but that, on division of the aponeurosis, these overlapping fibres slip outwards off the artery. In one of the cases noted this winter, the artery in both arms of an adult female subject was thus covered for three inches. But what is most commonly seen is a raised portion of the brachialis muscle, projecting on the outside of the artery, sending a deep aponeurosis inwards over it, and binding the artery down in a kind of groove in the muscle.

7. *High Origin to Pronator Teres Muscle.*

We not unfrequently meet with the pronator teres muscle arising a little higher than usual above the internal condyle, but without much altering the relative anatomy of the bend of the arm. In the two following instances, however, the muscle arose unusually high, and changed the relative position of the arteries at the bend.

1. On *left* side pronator teres arises $1\frac{1}{2}$ inch above the condyle. An aponeurosis reaches from it to join brachialis anticus, external to position of artery. There is thus formed a kind of arch or tunnel, under which the principal artery and the median nerve pass, so as to become concealed half an inch above the transverse level of the condyle. Radial artery rises above middle of arm, and passes over this aponeurosis, but lies under the aponeurosis of the arm and semi-lunar process of the biceps.

Right side. No high origin to pronator, but a high division of artery, and the two arteries are separated by a deep layer of aponeurosis at the bend of the arm.

2. Right side. All the anatomy on left side normal. Pronator teres arises two inches above condyle, from intermuscular septum. The high portion is almost a separate muscle, being at first separated from the usual pronator by a narrow cellular space. High pronator is soon joined, by aponeurotic fibres, to a raised portion of the brachialis anticus external to the position of the artery. Radial artery arises in axilla, and is covered by the usual aponeurosis of the arm and the semi-lunar process of the biceps, but the principal artery passes, along with the median nerve, underneath the above mentioned deep aponeurosis and high pronator, by which they are concealed for an inch and a half above the transverse level of the condyle.

II.—ON THE OCCURRENCE OF A SUPRA-CONDYLOID PROCESS IN MAN.

The part so named is a more or less hook-shaped process, which is occasionally developed on the inner surface of the humerus, two inches above the internal condyle. A ligament is continued from it to near the condyle, completing an arch, through which the median nerve and brachial artery pass, after deviating from their usual course; the whole forming an arrangement analogous to that which obtains in many animals, in the passage of the nerve and artery through an opening, the supra-condyloid foramen, in the humerus, in the same situation.

A few instances of this variety had been recorded previous to 1848, when I gave a short account[1] of several which I had met with in the dissecting-room, and I have now met with the variety so frequently, that I am desirous of noticing it more fully, and of calling the attention of anatomists to it, as a variety which, I believe, will be found to occur not unfrequently, to a more or less marked degree, if this part of the arm be carefully examined.

The sketches at the commencement of this paper are taken from two of the numerous specimens of this variety now in my collection. In fig. 2, the bone and process are represented one-fourth the natural size. In fig. 1, the lower end of the humerus is shown half the natural size. The supra-condyloid process is broader and stronger in this specimen. The bone is seen a little on its external and inferior aspect, to show the size of the oval space, completed by the ligament, through which the nerve and vessels passed. Fig. 2, is from case No. 4, and fig. 1, from case No. 8, in the following list.

List of Cases of Supra-Condyloid Process, with Notes of the Peculiarities of each.

1. Left side. Process $\frac{1}{3}$ of an inch in length. Adult female. Nothing known of right side.
2. Both sides. Process broad and long—nerve and artery passed round it on both sides. Adult female.
3. Both sides. Process of moderate length. Nerve and artery passed round on both sides. Adult male.
4. Both sides. Process $\frac{3}{4}$ of an inch long on left side, and very short on right. Nerve and artery passed round on left side; on right side, nerve only, accompanied by small muscular arterial twig.
5. On right side only. Process of moderate length, and well-

[1] Edin. Monthly Journal. Oct. 1848. See No. I. of this volume.

marked groove behind it. Nerve and artery pass round process. Left side—groove well marked though process entirely wanting. High division at axilla, both arteries run down inner edge of biceps, radial most external. A small third head to biceps, behind arteries. Female æt. 37.

6. Right side only. Process was of good length, but now partly broken off. Median nerve passed round process, but artery did not. Uncertain whether a small branch did not accompany nerve.

Left side. All anatomy normal. Female æt. 4.

7. Right side only. Process moderate. Median nerve passed round process, accompanied by small muscular artery from inferior profunda. Artery did not deviate—was crossed behind by median nerve—divides an inch above elbow, and ulnar passes superficially, close under aponeurosis of forearm.

Left side. No process, but a well-marked groove, and slight roughness just where process arises on other side. High division of artery at lower edge of conjoined tendon. Arteries usual in position, separated only by median nerve. Female æt. 50.

8. Left side. Nothing known of right. Process $\frac{3}{4}$ of an inch in length. Nerve and artery pass round it. This specimen is from the case related by Dr Knox,[1] and came into my possession with his Museum of Human Anatomy. Middle-aged male.

9. Left humerus in my collection, without any history. Rough ridge $\frac{1}{4}$ inch in length, where process arises in other specimens, and a distinct groove behind it.

The following are specimens in other museums or from cases described by others.

10. Both sides. Process well formed. Preparation of child, showing arteries, in Barclay collection in Museum of Edinburgh College of Surgeons.

Right side. Artery passes round process. *Left.* Radial arises above middle of arm and keeps by edge of biceps—

[1] Edin. Med. and Surg. Journal, 1841.

chief trunk deviates and passes round process. Nerves not preserved.

11. Left side. Nothing known of right. Young child.
12. Left side. Nothing known of right. Nearly full grown arm. This, and the last, also in Dr Barclay's collection. In both, process is well developed, and in both there is the same arrangement of arteries as on left side of No. 10.
13. Right side only. Skeleton, also in College Museum. Female, and probably æt. about 30. Process moderate on right humerus, with well marked groove behind it. Left has no process and only a shallow groove.
14. Case by Tiedemann.[1] Right side. Nothing said regarding left. Process distinct. Brachial artery usual in position. High inter-osseous passes round process. Nerve not represented. Process designated "an unusual excrescence from the humerus."
15. Case by Mr Quain.[2] Right side. No information regarding left. Adult. Nerve and artery pass round process.

The following generalizations may be made with regard to this process and its relation to the nerve and artery.

As regards situation, length, and form.—The situation is remarkably constant. In the adult bones it is two inches above the internal condyle, measuring from the upper edge of that prominence to the middle of the base of the process. In one it was $\frac{1}{6}$ inch nearer, and in one $\frac{1}{4}$ inch farther from the condyle. In case No. 2 only did it lie considerably nearer the condyle, being an inch and a quarter from it, and here the process was long and of unusual breadth.

The length varies from $\frac{1}{10}$ inch to $\frac{3}{4}$ of an inch. It is seen in all stages—as a short rough line, a pointed tubercle rising from a base elongated upwards and downwards, and a hook or spur like process of greater or less length. I have never seen it longer than $\frac{3}{4}$ of an inch. In the specimens figured it is of this length. It begins by a vertical ridge half an inch to an inch in length,

[1] Tabulæ Arteriarum, 1822. Plate xv., fig. 3.
[1] Op. Cit., p. 223. Plate xxxvi., fig. 3.

gradually rising to the commencement of the process proper. The process projects away from the bone, forwards, downwards and inwards, is flattened from before backwards, tapers to a blunt point; and, if prolonged for an inch, would form an arch of bone joining the ridge half an inch above the condyle. This is represented by the ligament, and the process and ligament together, enclose between them and the bone an elliptical space, an inch in length and one-third of an inch in breadth, through which the median nerve and the brachial artery, with its venæ comites, passed.

The origin of the process with regard to the borders of the humerus is also constant. It arises from the internal surface of the bone, midway between the internal and anterior borders or a little nearer to the latter, and behind it there is usually a well marked groove, to which I shall again allude as existing on many arm bones on which there is no process.

As regards symmetry.—In seven of the cases nothing was known of the other arm. Of these seven, six were on the left arm and one on the right.

In four of the cases it was present on both sides—In three of these it was of equal length on the two sides, in one pair long, in one pair short, and in the third of medium length; and in the fourth they were unequal, the left very long, the right very short.

In four cases in which both bones were examined, but in which the process was present on one side only, this side was the right in all; but in the case where it was present on both sides, and unequal, the left was much the longest.

As regards sex and age.—The sex was known in ten of the cases; three of these were in males, and seven in females. In two of the three males it was present on both sides, and was so in only one of the females.

The specimens are from subjects of all ages. Three of them were young children, and in all of the three the process is well formed.

As regards the arteries.—The state of the arteries was known in twelve of the subjects, and four of these presenting the process on both sides, we have sixteen instances in which to notice the relation of the arteries to the process.

Where the artery is affected by the existence of the process, it leaves the biceps near the insertion of the coraco-brachialis, and passes down, with the median nerve, along the internal inter-muscular septum to reach the concavity of the process, underneath which it then passes.

Out of the sixteen instances, in nine the undivided brachial artery deviated and passed round the process; in four, there was a high division, one of the arteries keeping normally along the edge of the biceps, the other deviating to pass round the process. The vessel which deviated in three of these four was the principal trunk, the high radial not deviating; in the remaining instance—that by Tiedemann—the deviating vessel was a high interosseous. In the remaining three, there was no deviation of the artery, although the process was present. In two of these the other arm was in all respects normal, in the third there was a long process and the artery deviated.

Relation to Median Nerve.—In all the instances in which the artery deviated, and in which the position of the nerve was known, it also deviated and passed around the process, lying internal to the artery as they entered the arch. In three of my cases the nerve deviated without the artery. In at least two of these it was accompanied by a small artery derived from the inferior profunda, which ran down with it, in the same way as the inferior profunda itself runs down with the ulnar nerve. This, however, was not a leading vessel, but only a muscular branch, not extending beyond the elbow, nor again joining with the brachial or either of its divisions. In every case where there was a process, whether short or long, and the nerve examined, it deviated to pass under the process, whilst in three of these instances the artery did not deviate with it. This points to the inference that the supra-condyloid foramen is provided not so much for the artery, as is commonly supposed, but principally for the nerve.

Relation to Muscles and Region.—The anatomy of the soft parts in the neighbourhood of a supra-condyloid process appears to be the same whether the nerve is accompanied by the artery or not. Proceeding from the tip of the process is a *ligament* or fibrous band, which arches downwards and inwards, and, blending with the intermuscular septum, is inserted into the ridge a short distance above the condyle.[1] The true intermuscular septum dips down to be attached to the internal condyloid ridge of the humerus, and between this on the inside and the brachialis anticus and supra-condyloid process externally, is a grooved space, in which the deviating nerve and artery are placed. This hollow is bounded behind by the humerus, on which there is a

[1] *Internal Intermuscular Septum.*—It appears to me that the normal anatomy of the part so named is not usually fully described. Reaching down on the inside of the arm, and connected with the aponeurosis, is a white cord like band, but this is not the intermuscular septum. The true intermuscular septum lies some distance in front of this, and is not so evident on the surface of the muscles. The true septum dips down as a strong fibrous membrane to be attached to the internal condyloid ridge, and is fully half an inch in depth, forming a fibrous partition, between the brachialis anticus in front, and the inner head of the triceps behind, to both of which it gives origin. Below it passes to the internal condyle, having before been joined by the posterior ligamentous band; and above it is continued along the internal border of the humerus, as far as the insertion of the teres major, having again been joined by the posterior ligamentous cord. The latter separates from the true septum at or above the insertion of the coraco-brachialis muscle, and passes down on the surface of the internal head of the triceps, lying posterior to the true septum, at a distance varying from three-fourths to a fourth of an inch, and again it joins the true septum, about an inch, more or less, above the condyle. It is connected in front to the true septum by an aponeurosis, through which the muscular fibres are visible, and behind it is continued into the aponeurosis of the posterior brachial region. Close behind it is the ulnar nerve and inferior profunda artery. I have seen the nerve lie some distance behind it and uncovered by it, but usually the nerve will be exposed by an incision along the posterior edge of this fibrous cord. It appears to have reference to the position of the nerve, and, forming a tolerably resisting band over or along the hollow on the inside of the humerus in the lower half of the arm, may serve to protect the ulnar nerve from any pressure made against the inside of the arm. It is united above to the true intermuscular septum, the two together forming a fibrous cord running up along, and attached to, the internal border of the humerus and passing behind the tendon of the coraco-brachialis. The latter is usually said to be intimately connected at its insertion with the intermuscular septum. It appears so, and on one or two occasions I have found some fibres of the tendon continued into the septum; but usually they may be easily separated down to the bone, and it is evident that the septum passes up behind the tendon, as far up as the teres major, or as high as the highest point of origin of the internal head of the triceps, of which it appears almost like a tendon of origin in its whole length. I have repeatedly examined these points with care, dissecting down to the bone, and the above will be found almost invariably to be the precise arrangement. I shall again refer to the deceptive feeling as if the true intermuscular septum was attached not to, but in front of, the internal condyloid line. We must therefore distinguish the true intermuscular septum, dipping down to the bone, the edge of which is not very apparent at first on the surface, from the very evident ligamentous cord behind it, which usually bears a close relation to the ulnar nerve, and might be called the *internal brachial ligament*, as a means of distinguishing it from the true septum.

more or less marked groove, the artery or nerve not lying actually in this groove, but in front of it. The inner boundary of the groove is the internal condyloid ridge, the outer gives origin to the most internal fibres of the brachialis anticus muscle. The nerve and artery begin to deviate at the tendon of the coraco-brachialis muscle, and pass down in this grooved space in front of the intermuscular septum, the nerve internal to the artery, and bound down by an aponeurosis from it to the brachialis anticus. They now lie between the intermuscular septum and the process, and passing underneath the concavity of the latter, are covered by it and protected from pressure. Their direction is now changed, and they pass obliquely outwards to gain their normal position at the elbow. From the point where they enter underneath the arch they may be covered by a high pronator teres, or by a strong aponeurosis passing inwards from a raised portion of the brachialis anticus. A high origin to the pronator teres, which may at first form a separate muscle, appears to be frequently if not generally present in cases of supra-condyloid process.

Relation of Supra-Condyloid Process to Humerus.—It has been already mentioned that the supra-condyloid process is situated, with very little variation, at the distance of two inches above the upper edge of the internal condyle. Its relation to the surface and borders of the humerus is also constant. It is always placed on the internal surface, either midway between the internal and anterior borders of the bone, or a little nearer to the latter; and the ligament which completes the arch has to pass downwards and inwards to join the intermuscular septum above the condyle. This constancy of the position of the process, both as regards the distance above the elbow and the surface of the bone, is remarkable and interesting; and, both in position and direction, the process will be found to correspond closely to the arch of bone by which the foramen is completed in the lower animals.

I must here remark upon a method of describing the shaft of the humerus to be found in several of our best text-books of anatomy, by which this bone is represented as presenting two surfaces only, an anterior and posterior. But the humerus, like the other long bones of the limbs, has distinctly three surfaces and three borders. The error as regards the humerus, has no doubt arisen from the circumstance that two of the surfaces may be seen on the anterior aspect, but these two are no less distinct from each other than they are from the posterior. It is only in the lower fourth of the shaft that it becomes flattened from before backwards, and from this part up to the surgical neck, the shaft is at least as thick on a side view as when seen in front; and transverse sections of the bone are at all parts of the shaft more or less triangular. The lateral borders are continued up from the condyloid lines, the outer ending at the back part of the greater tuberosity, whilst the inner is lost at the surgical neck, after passing up, faintly, a short distance behind the inner edge of the bicipital groove. The anterior border is the most distinct of the three, except in the lower fourth, where, however, it is still quite distinct, though not sharp or rough. It is formed in the upper third by the external bicipital ridge, and is continued down from it to within half an inch of the coronoid fossa, which it bifurcates to enclose, and separates the external from the internal surface, the former in the lower half being the broader of the two, owing to the prominence of the external condyloid ridge.

The internal surface is that which I desire more particularly to notice. It supports the supra-condyloid process in the situation already defined; and behind the process, or in this situation, there is a groove. This groove has been mentioned by Dr Knox in his papers already referred to, as existing in many arm bones. This part of the internal surface of the humerus is generally somewhat concave, though sometimes it is flat or even a little convex, and I certainly have found this groove present in the majority of a large number of arm bones that I have examined on purpose. In some this groove is altogether wanting, in a considerable proportion it is well marked, and in the majority it is present. It is seen in some young and adolescent bones, and appears to be at least equally distinct in those bones, the smoothness and straightness of which indicate them in all probability to have belonged to female skeletons.[1] This *supra-condyloid groove*

[1] There is no separate bone in which the difference between the male and female

is bounded posteriorly by the internal border or condyloid ridge, and in front by a special ridge, which marks off the posterior half or third of the internal surface as the groove. It reaches for about an inch down from the situation which a supra-condyloid process would occupy, and from one to two inches upwards from it, to near the usual situation of the principal nutritious foramen.

Taken along with the existence of a supra-condyloid process, this groove is naturally supposed to correspond to the position of the deviating nerve or artery; but in two of my specimens with this process, the groove is not so distinct as in the opposite bone, where there is no process and was no deviation of the nerve or artery, and in case No. 3, the process existing on both sides with deviation of both nerve and artery, the groove cannot be said to exist. Besides, the occurrence of the deviation of the nerve and artery, or of a supra-condyloid process proper, is comparatively rare—whilst I have said this groove exists in the majority of arm bones; and farther, since the artery or nerve do not properly occupy or lie in it, but only in front of it, even when the deviation exists, we must conclude, that the groove is not there for the purpose of lodging a vessel or a nerve. Being much puzzled to explain the meaning of this groove I have lately carefully examined in a great many arms the exact relation of the soft parts to the humerus at this part, and the following will be found to be almost invariably the exact arrangement.

The true intermuscular septum gives attachment by its posterior surface to fleshy fibres of the inner head of the triceps. On dissecting these fibres off down to the bone, the septum is felt to be attached to a sharp ridge, and a little distance behind this ridge a prominent but somewhat rounded edge of bone is felt, as if it was the internal border of the humerus. It appears as if the septum were not attached to the border, but to a ridge $\frac{1}{6}$ or $\frac{1}{5}$

(or at least the muscular and less muscular) is usually more striking than the humerus. In articulating four pairs of these humeri to each other, I noticed the curious fact, in three of them, that the right is longer than the left. In one pair, male, the difference is a quarter of an inch; in the other two, female, it is as much as half an inch. It would be interesting to ascertain whether the right humerus is generally thus longer than the left.

inch in front of it, but this, however plainly felt by the finger in the dissection, is deceptive. The bone is at this time partially rotated outwards, so as to turn its posterior surface a little forwards. On careful examination of the macerated bones, the condyloid ridge is seen to be the true internal border when the humerus is fairly looked at in front, but if turned a little outwards the part of the bone behind the condyloid line now projects. The true intermuscular septum, then, is really attached to the internal condyloid ridge or true internal border. On now dissecting on the outer or anterior aspect of the septum, it is seen to give origin to fleshy fibres of the brachialis anticus, down to the bone. The brachialis is now seen to arise directly from the surface or floor of the groove, also by fleshy fibres, and on separating these the outer edge of the groove is usually very distinctly felt by the finger, and is often especially rough at about two inches above the condyle. From this ridge, and especially at this rough part, the brachialis anticus arises usually by distinct tendinous bundles or fibres, whilst its origin is again fleshy to the outside of the ridge. The groove is usually very distinct to the touch when we have dissected down to it in this way, the distinct and sharp inner boundary giving attachment to the intermuscular septum, while the sometimes still more distinct outer boundary, at the part mentioned, is seen to give origin to the tendinous bundles for the brachialis anticus. This groove, then, generally existing on the humerus, has no relation to the position of a nerve or artery. It appears not to be constructed to serve any purpose, as a groove, but to result from the development of the two ridges, the internal giving attachment to the intermuscular septum, whilst the meaning of the outer and shorter one remains to be noticed. Considering its position on the internal surface of the humerus, considering that its roughest part is just two inches above the condyle, that a small point or ridge-like tubercle may exist on it here, and that this is precisely the point from which the supra-condyloid process in its various stages grows, it appears legitimate to draw the conclusion that this short rough ridge is a rudimentary condition of the supra-condyloid process, the base of which grows out from such a ridge running vertically on the bone at this part. This development into a process, however, but rarely occurs, and the *rudimentary ridge* is employed for the specific purpose of giving origin to tendinous fibres for the brachialis anticus. We have seen that, in every case in which a process existed, the median nerve, when its position was noted, deviated and passed round it, but we have other cases still to record in which the nerve and artery deviated but in which there was no process beyond the existence of the short rough ridge. The correct view to take perhaps is this—that the median nerve, accompanied or not by the artery, occasionally deviates, and that, when it does so, we occasionally or usually find a protective arch thrown over it, formed by a supra-condyloid process above and a ligament below, being an arrangement analogous to that which occurs in the most highly formed condition of this part of the mammalian humerus.

Comparative Anatomy.—Although there is no donbt that the foramen in the humerus affords protection to the parts which it transmits, it is not understood precisely in what manner it does so in the animals in which it exists, nor why they, more than some others, require such an arrangement for the protection of the vessel and nerve.

The arrangement is found among the Quadrumana, Rodentia, Edentata, Marsupialia, and more frequently among the Carnivora. The cat presents the most familiar example, and the following description is taken from my dissections of the part in that animal.

Bone.—The humerus is three inches and a quarter in length, and the situation of the upper end of the arch which completes the foramen, is half an inch above the internal condyle. The supra-condyloid process in man occupies the same proportional situation, being two inches above the condyle, whilst the humerus is on an average twelve inches in length; that is to say, the distance of the foramen in the cat, and the process in man, from the lower end, or internal condyle of the humerus, is about one-sixth of the length of that bone. The supra-condyloid foramen is a short oblique passage, directed downwards and forwards. The space is oval, measuring one-fifth of an inch in depth and one-twelfth of an inch in breadth. It is formed on the internal aspect, by an arch of bone leaving and again joining the shaft. This arch, however, does not project abruptly outwards, like the process in man, but only slightly bulges outwards the natural sweep from the condyle to the inner border of the humerus, and, in the adult bone, forms a lesser projection than that of the condyloid ridge on the outside. The foramen, therefore, is not, as some have supposed, merely a space left to hold and protect the artery in consequence of the great development of this part of the bone,[1] and by which the artery would otherwise have been thrown out of its course; and the development of the foramen farther shows that it is originally formed as a specific provision.

Development of the Foramen.—I find that the arch, by which the foramen is at length completed, grows, as a process, from above downwards. It is developed from the shaft, and again unites with the shaft below, and is completed altogether independent of the epiphyses of the lower end of the bone. This will be better understood by reference to the illustrations, figs. 4, 5, and 6, in which the bone is reduced one-third from the natural size. Figs. 4 and 5 are from the new-born kitten, the cartilaginous ends being preserved in the second. Besides a number of previous observations, I recently examined both limbs in three new-born kittens of the same birth. In some the lower end of the process is not yet united to the shaft; in others it is just about to unite with it, so as to complete the bony foramen.

Fig. 6 is from the kitten, five weeks after birth. The foramen is seen to be completed as part of the shaft, and independent of the epiphyses, which are now ossifying.

The arch by which the foramen is completed, thus resembles, during the early stage of its development, the supra-condyloid process on the human arm; and were the latter prolonged in the direction of the ligament, it would correspond exactly to the arrangement in the cat, with this difference, however, that in man it starts more abruptly away from the humerus, although, on the other hand, its direction is not greater in this respect than

[1] Introduction to the Natural History of Mammiferous Animals. By W. C. Linnæus Martin, p. 89.

is necessary to leave a sufficient space for the nerve and blood-vessels. Seeing this to be the mode of its development in animals, we are not surprised to find it, when it is present in man, to occur in the very young subject and female, as well as in the adult and muscular. It is not unlikely that the arch may be liable to variety in the extent of its development, leaving the foramen unfinished, in animals which normally have it completed. In one arm of an adult cat, I found the arch represented by a ligament only, which, both above and below, joined a short spiculum of bone. On the other side, the arch was bony and well formed, as seen in fig. 3, which is sketched from the preparation made from it, and now in my collection. In the adult bone, the arch is directed downwards and backwards towards the internal condyle, is flattened, measuring one-tenth to one-twelfth of an inch in breadth, is somewhat concave towards the foramen, and convex and smooth superficially. There is a distinct groove on the humerus behind the foramen, both above, where the nerve and artery lie, and below, where no nerve or vessel is near, but almost no groove, or a very short one, in front of the opening, which corresponds to the abrupt departure of the artery and nerve after they have traversed the foramen.

Nerve and Blood-vessels.—The brachial artery does not lie as in man, along the course of the humerus, but as it crosses the subscapularis muscle, is removed considerably to the inside of that bone. It now reaches obliquely downwards and outwards, supported behind by the internal head of the triceps, approaches and touches the humerus just before entering the foramen, through which it now passes along with the median nerve. It then leaves the humerus, lies at a considerable distance in front of the elbow, and, after a course of an inch and a quarter after leaving the foramen, it divides into the radial and ulnar arteries. The median nerve accompanies the brachial artery in the arm, through the foramen, and below it, always lying to its outside, so that the nerve lies highest as they traverse the opening. I have not noticed any veins passing through the opening with the artery, and in one dissection in which the veins were injected, they did not pass through, but those accompanying the arteries in the forearm, joined the superficial veins below the foramen, and, passing with them over the arch, reached up the arm superficial to the artery.

Muscles.—There is no deep muscle by which the nerve and artery would apparently be directly or unavoidably compressed against the bone. They lie along, and are somewhat overlapped by, the inner edge of a very broad one-headed muscle corresponding to the biceps of man. A few fibres of the triceps arise from the posterior edge of the bony arch forming a thin separate lamina partially covering the nerve and artery as they enter the arch, but this could not directly compress them, nor can this be the object of the provision of the foramen. The pronator teres arises very slightly above the condyle, and does not cover the nerve or artery.

But, superficial to all these parts is a muscular layer derived as an expansion from the latissimus dorsi and pectoral muscles, and this covers the whole internal brachial region, down to the level of the elbow, and over the situation of the foramen. This expansion is derived posteriorly from the anterior edge of a muscle, which extends from the latissimus dorsi to the olecranon process. This muscle arises by a short tendon, or tendinous intersection, at right angles from the lower margins of the latissimus dorsi and teres major; passes vertically down the posterior part of the internal brachial region, with a length and breadth much like one of the recti muscles of the human eye, and is inserted by a flat tendon into the inside of the olecranon and the fascia of the forearm. From its anterior border the thin muscular expansion passes forwards and joins with a similar one from the lower and superficial part of the pectoral muscle. This expansion covers the whole of the region of the brachial artery, and reaches down even below the level of the condyle, ending now as an aponeurosis in front of the forearm. Considering the size of the animal this muscular expansion is of considerable strength, being about $\frac{1}{20}$ of an inch in thickness.

I have lately, by the kindness of Professor Traill, had the opportunity of dissecting these parts in the lion and ichneumon, in both of which the supra-condyloid foramen is present. *In the*

Lion (the animal dissected was from Mr Wombell's collection, an adult lioness, which died after parturition) there was no muscular expansion over the region of the brachial artery. The muscle, or muscular expansion, from the latissimus dorsi, lay along the posterior part of the internal brachial region, and, coming as far forwards only as the position of the ulnar nerve, it ended in a strong aponeurosis, which passed downwards over the olecranon and internal condyle, and forwards over the brachial vessels and nerves, where it united with a similar aponeurotic expansion from the pectoralis major. The foramen, therefore, is not for the purpose of affording protection from a superficial muscular expansion, as the examination in the cat alone might lead us to suppose. In other respects the anatomy is the same as in the cat, on a much larger scale. At the lower edge of the tendon of the teres major the brachial artery lies an inch inwards from the humerus, and, for three inches downwards from this, is related behind to the spiral nerve and inner head of the triceps muscle. It is now within two inches of the foramen and lies upon the humerus. It now passes through the foramen, and for an inch below it, is still related behind to the bone, some fat and cellular tissue lying between; and 2¾ inches below the foramen it divides in the forearm into radial and ulnar. When the elbow is extended the course of the artery appears direct, but when the elbow is flexed the artery appears to dip backwards to pass through the foramen. The median nerve accompanies the artery, lying parallel to its outside both in the arm and through the foramen, on leaving which the nerve has begun to divide. It is about one-third of the size of the injected artery, is smaller than the ulnar, and half the size of the spiral nerve. The veins do not pass through the foramen, but cross over the arch, as two large veins, which again meet the artery above the foramen. There is only one muscle which could press upon the nerve or artery, being the one corresponding to the human biceps. This flexor radii muscle overlaps the nerve and artery from the teres major downwards, and as they come to cross obliquely in front of the humerus this large muscle is so placed that it might press them against the bone, at the part where the arch is thrown over them to form the foramen. This, however, is not strikingly evident, as the nerve and artery are so situated that it might be maintained that they would be pushed inwards by it, and not directly backwards against the bone; but it is at the same time plain enough that, during its action, this large muscle will press backwards upon the protective arch. The arch, as seen on the macerated bone, corresponds remarkably to the supra-condyloid process of man. The arch springs from the internal surface, nearer the anterior than the internal border, and passes downwards and inwards to join the latter close above the internal condyle. The arch does not project much outwards. Lengthways, it is slightly convex externally, and concave towards the foramen. Across, it measures $\frac{1}{3}$ inch, is convex externally, and flat or a little concave towards the foramen. The foramen is oval, $\frac{3}{4}$ inch long, and $\frac{1}{4}$ across at the middle. The upper end of it is $1\frac{1}{2}$ inch above the higher and most prominent division of the internal condyle, the entire bone being $11\frac{1}{2}$ inches in length; and there is a groove for an inch above and behind, and for half an inch below and before the foramen.

In the Ichneumon (a carnivorous animal larger than the cat—the *mangusta* of Cuvier) the supra-condyloid foramen transmits the median nerve only, the artery and veins passing over the arch on both sides. In other respects the anatomy resembled that of the cat, only, the thin muscular expansion over the brachial region seemed to have become aponeurotic before it reached the supra-condyloid region. The median nerve crossed behind the brachial artery, and at the junction of the upper and middle thirds of the arm, deviated from the artery and passed down along the front of the small intermuscular septum, and entered the foramen. A small muscular artery accompanied the nerve down the arm but not through the foramen. The artery and veins keep close along or underneath the inner edge of the flexor radii muscle, and again meet the nerve below the foramen. The course of the blood-vessels appears direct, whilst the nerve appears to bend downwards and inwards, out of the direct course, in order to pass through the foramen. The flexor radii is not, at least in this domesticated specimen of the ichneumon, so bulky by one half as in the cat, and when it acts, it appears as if the vessels would be pushed aside, whilst the nerve is protected by the arch of bone.

It may be inferred from this, that the foramen is primarily intended for the nerve, and this agrees with what we have seen from the cases of the variety in man, that, when the process exists, the nerve always deviates, whilst the artery may or may not. It may be compared in this respect to the supra-scapular notch in man, which always transmits the nerve, as the structure of greatest importance and least tolerant of pressure, while the artery passes over the ligament and only occasionally under it, with the nerve. It is not unlikely that the nerve only may pass through in many of the animals, the skeletons of which present the supra-condyloid foramen, and also it is not unlikely that a process may occasionally be found as a variety on the humerus of animals which, like man, do not possess it as a regular character.

With regard to the necessity for protection, we observe that the nerve and artery approach the bone obliquely, and have to cross in front of it above the elbow. Here any compressing force would have pressed them against the bone, and here accordingly an arch of bone is thrown over the nerve and generally over the artery also. They here approach the humerus much in the same way as the femoral vessels approach the shaft of the thigh bone in man, as they are about to become popliteal, and an analogy may be drawn between this provision in the humerus and the oblique tendinous canal by which the femoral artery is protected from muscular pressure against the bone.

But even although the above explanation were to account satisfactorily for the existence and use of the foramen in those animals which possess it, it does not seem to explain why these animals have or require it more than some others in the same orders, but in which it is wanting. After examining carefully the arm and elbow of the dog, for instance, I cannot see why the median nerve should not require protection in this animal as well as in the ichneumon above noticed. In the same way, however, it might be said, that we can see no precise reason, from the anatomy of the surrounding parts, why, in man, the supra-scapular nerve should be so specially protected in addition to the protection afforded by the general hollow between the upper angle of the scapula and the coracoid process. We can only infer, from the fact of the median nerve, with or without the artery, being sent through a foramen under an arch of bone, that it required this protection, and that, therefore, it has it.

III.—ON THE VARIETIES OF THE ARTERIES OF THE ARM.

The varieties affecting the arteries in the arm may be arranged in three classes :—1. Varieties in the muscular relations: 2. Deviation of the artery from its usual course: And, 3, An early division of the artery by which two or more arteries exist, instead of one.

1. The first of these classes of varieties I have above considered and furnished examples of, in the cases of the artery being covered by a slip from the latissimus dorsi, by an expansion from the coraco-brachialis, by a broad third head to the biceps, by an aponeurosis from the brachialis anticus, or by a thin portion of the muscle itself, and by a high origin of the pronator teres.

2. The second class is made up nearly altogether by the variety in connection with a supra-condyloid process. In this, *the condyloid deviation*, the position of the artery is changed in the lower two-thirds of the arm. Accompanied, as usual, by its venæ comites and the median nerve, it is covered only by the aponeurosis and a sheath of fascia, derived from the outer edge of the intermuscular septum, along which it runs. But below the process, the artery, besides being directed obliquely outwards, is generally more deeply covered than usual, either by an aponeurosis from the brachialis anticus, or a high origin of the pronator teres.

As the artery, at the process, lies three-fourths of an inch from the inner edge of the biceps, the ordinary incision along the edge of that muscle could scarcely enable the surgeon to reach the artery, but an examination of the arm before the operation was begun, would probably indicate the deviation of the artery by the unusual situation of the pulsation, and in the cases where the process is well formed, it is easily felt from the surface.

But the artery may deviate in this manner independent of the existence of a supra-condyloid process; as in four cases described and figured by Mr Quain. In these, the median nerve continued in the course of the intermuscular septum,—accompanied, in two of the cases by the undivided brachial artery,[1] and in the other two by one of two trunks into which the brachial had divided,[2] and crossed or perforated the intermuscular septum, two or three inches above the internal condyle, and then inclined outwards to the usual position, under cover of a high origin to the pronator teres muscle.

I have met this winter with a well marked case of this condyloid deviation, without the co-existence of a developed supra-condyloid process. The median nerve and undivided brachial artery deviate and pass down in front of the intermuscular septum to within $1\frac{3}{4}$ inch of the condyle, and now pass beneath a fibrous arch and a high pronator teres, and are directed downwards and outwards to the bend of the elbow. As the nerve passes down with the artery and its venæ comites, the nerve lying internal, they are sunk in a depression between the septum and the brachialis anticus. On dissecting aside the fibres of the brachialis anticus which lie behind and support the nerve and artery, a groove is distinctly felt in the humerus. The outer edge of the groove is a ridge, giving origin to fibres of the brachialis, and presenting a more elevated portion, like a rudimentary supra-condyloid process, exactly two inches above the condyle, and situated nearer the anterior than the internal border of the humerus. From this more prominent point arises the fibrous arch, which then stretches down to join the intermuscular septum. The high pronator has an origin an inch broad, beginning two inches above the condyle. It arises highest from the fibrous arch and farther down from the intermuscular septum, the vessels and nerve lying beneath the lower part of this high pronator, after they have entered the arch. But the high pro-

1 Op. Cit. p. 259.
2 Plate xxxvi. fig. 4, and Plate xxxvii. fig. 1.

nator arises here also behind the vessels and nerve, which therefore appear for half an inch to be embraced by the muscle. The high pronator ultimately joins the normal pronator teres, but there is at the bend of the elbow a cellular space of half an inch between them, in which the brachial artery is seen dividing opposite the level of the condyle into the ulnar and radial arteries, the latter then passing through between the two pronator muscles to occupy its usual position over the true or normal pronator teres. In this case, then, there is a true condyloid deviation, exactly as in the cases of a developed process, but here the process could scarcely be said to exist.

As above related, in thirteen of the cases in which an artery deviated, in connection with a supra-condyloid process, it was the entire brachial artery in nine, and one of two large vessels in the remaining four. In the latter case the surgeon would be very apt to overlook one of the arteries. I suspect it must have been the occurrence of some such variety, which has led to the common observation, that the inferior profunda branch is liable to be mistaken for the brachial artery in an operation. A case in which such a mistake was made by a late eminent surgeon, has been placed on record;[1] "the inferior profunda, which was unusually large, was exposed and tied on the supposition of its being the brachial artery." As the pulsation in the aneurismal tumour was not arrested, an after operation was performed and the brachial trunk successfully tied. As the normal inferior profunda branch is not larger than a crow-quill, I infer that in this case, and in similar ones, there was a high division of the brachial artery, and that the vessel first secured was one of the two, either lying near to, or deviating from, the course of the other, which occupied the usual position along the inner edge of the biceps muscle.

3. Regarding the high division of the brachial artery, the relative frequency of it, and the various nature of the early branches, little remains to be done after the careful and extended observations of Mr Quain, as recorded in his beautiful and valuable work on the anatomy of the arteries. From the observation of

[1] Cycloped. Anat. and Phys. vol. i. 1836. Brachial Artery.

481 arms, he has shown that the proportion of cases in which two arteries exist in the arm, is one in $5\frac{1}{9}$. The previous estimate of Harrison, given in his valuable treatise on the arteries,[1] from the observation of 82 cases, gave a proportion of about one in four. Adding together the observations of Harrison and Quain, the proportion will stand about, though rather less than, one in five. The early vessel is well known generally to be the radial, sometimes the ulnar, and occasionally a high interosseous; or, by a short union or cross branch below, a condition equivalent to a double brachial artery, with the two divisions either equal or with one of the vessels much smaller than the other. But, although these details are of much interest to the anatomist, they in reality are of little importance to the surgeon, who is concerned with this fact only, that frequently there are two arteries instead of one, and that it may be so in the case in which he is about to operate. The important practical question then comes to be, supposing a second artery to be present, how is its existence to be ascertained, and how can it be reached. Harrison and Quain have remarked, that the two arteries generally lie close together. The latter farther remarks[2] that the radial artery "often arises on the inner side, and, after lying close to the larger vessel, crosses over it at the bend of the elbow;" and that the ulnar at the lower part of the arm has a tendency to incline inwards from the usual position of the brachial trunk.

My attention has been directed to observing the exact *relative position of the two trunks* more than to ascertaining the relative frequency of the different varieties; and, although this opportunity is very apt to be lost in the ordinary dissections in the anatomical rooms, I have been able to note the exact position of the two vessels in twenty arms, eighteen of these presenting instances of the high radial, and the remaining two, of the high ulnar artery; and ten of these occurred in sixty subjects dissected during the earlier part of the present session. It would be tedious to give the details in each case from the notes now before me, and will suffice if I state the conclusions to which I have come. In nearly all of the cases the division occurred in

[1] The Surgical Anatomy of the Arteries. Dublin, 1839. [2] Loc. cit. p. 263.

the axilla or high in the arm, thus presenting two arteries throughout the whole or greater part of the brachial region. It is necessary to consider the upper and lower parts separately, taking the insertion of the coraco-brachialis, or where the artery normally crosses the lower end of that muscle, as the termination and commencement of the two portions.

In the first portion of their course, the two arteries lay, in all the cases, very near to each other, separated only by the median nerve and a layer of deep fascia or sheath. One of the trunks lies superficial to the other. It is the high radial or ulnar as the case may be, and is usually, more or less, the smaller of the two; the other and deeper trunk represents more the position of the brachial, the median nerve being related to it as it is normally to the single brachial artery. The high radial, I have said, is somewhat superficial in position to the other, but it may, at the same time, lie to either side, or directly over the deeper trunk. In most of the cases it lay external and superficial, but in some it arose on the inner or anterior aspects, and then crossed over the deeper trunk to get to its outside. This will be found to be the exact position of the two arteries in the *upper part* of the arm, one lies immediately below the aponeurosis, and superficial, or on a plane anterior to the other. Close either on its inner or, it may be, its outer side, and behind, or on a plane posterior to it, is the other trunk, the median nerve crossing very obliquely between the two, and a layer of sheath also intervening.

The deeper trunk, if the division is as high as the subscapularis muscle, furnishes the circumflex and subscapular, and then the profundæ branches, while the more superficial trunk, keeping down by the edge of the biceps furnishes the external muscular branches.

In the *lower half* of the arm there is considerable variety in the relative position of the two trunks, and they seldom lie so close together as above described in the upper half. It is necessary here to distinguish between high radial and high ulnar. As Mr Quain has remarked, the high ulnar has a tendency to pass to the inner or ulnar side away from the edge of the biceps. In both of my cases, the high ulnar was, below, removed half an inch from the inner margin of the biceps, the deeper trunk in one of the cases being external to it, in the other internal, having the position of the condyloid deviation. In both, the high ulnar passed over the muscles of the forearm, although not superficial to the aponeurosis. In the more numerous and common cases of high radial, I have observed, that the more superficial of the two, or high radial, courses down along the inner edge of the biceps, covered by the aponeurosis and, it may be, slightly overlapped by the edge of the biceps, whilst the other trunk occupies a position internal to it and deeper, to a varying extent. The median nerve is at first still crossing obliquely between the two arteries, but, in the lower third, has now gained the inside of the deeper trunk and descends parallel to its inside, in the situation where the two arteries are most separated from each other. The deeper trunk is generally, if not always, covered by a second aponeurosis. It is very frequently, if not generally, lodged or sunk in a groove in the brachialis anticus muscle, and from the raised or projecting fibres on the outside of the artery, an aponeurosis is sent inwards over the artery and nerve, binding them down upon the surface of the brachialis muscle, or into a groove in it. Frequently also, the fleshy fibres themselves pass across or partly across, and bind down or overlap the artery and nerve, as already described to be a position not uncommonly occupied by the undivided brachial artery.

The deeper trunk is thus so placed, that, to expose it, requires the division of a deeper layer of aponeurosis, or the division or displacement of some muscular fibres, but a more important point is the frequent removal of the deeper trunk more or less to the inner side, gradually approaching the intermuscular septum, until it constitutes a condyloid deviation of the deeper of the two trunks.

In some of the cases the deeper artery lies close to the inside of the high radial, but deeper, in some it was a quarter of an inch to the inside, in some half, and in several three-fourths, of an inch; in either case bound down by the deeper layer of aponeurosis. In five of the cases, this inward separation went as far as to constitute the condyloid deviation. Two of these five were on opposite arms of the same subject, and in one of the subjects the non-deviating artery was the high ulnar. In the two occurring in the same subject, the anatomy was the same on both sides. The deviating trunk was three times as large as the non-deviating high radial. The deviation commenced at the insertion of the coraco-brachialis. The larger artery, with the median nerve lying in this case still to its outer side, passed down in front of the septum, sunk into a depression between it and a portion of the brachialis anticus muscle, to within 2½ inches of the condyle, when they inclined outwards to pass below a fibrous arch and a high pronator teres. This fibrous arch begins at the septum, 2½ inches above the condyle, and reaches downwards to the aponeurosis of the forearm opposite and below the condyle, and the artery and nerve are nearly opposite the level of the condyle before they become concealed below the arch and the high pronator. The high pronator teres, as large as the little finger, arises from the outer edge of the fibrous arch, but also, tendinous from 1½ inch of the intermuscular septum, behind the artery and nerve, which thus appear to split the high pronator at its origin. Higher up the arm than the high pronator and fibrous arch, the deviating artery is bound down by some overlapping fibres of the brachialis anticus, and an aponeurosis is prolonged from these to join the intermuscular septum. Here the artery lies half an inch inwards from the high radial. At the bend of the elbow they are separated by the high pronator, over which, but underneath the semilunar tendon of the biceps, the high radial artery lies.

In the other three subjects, the anatomy of the deviating vessel was so exactly the same that one description will suffice. There was no high pronator teres, or fibrous arch proper. The artery left the high radial, or ulnar, at the insertion of the coraco-brachialis, and passed down in front of the intermuscular septum, sunk into a deep depression between it and part of the brachialis anticus, supported behind by the fibres of the brachialis which arise from the septum. The median nerve has crossed over the deeper artery at the middle of the arm, and now passes down parallel to its inner side. Arrived at within 1½ inch from the condyle, the nerve and artery now turn gradually and incline outwards and downwards to the elbow. Before the artery thus begins to incline outwards, it is placed $\frac{1}{2}$ to $\frac{3}{4}$ inch to the inside of the high radial, which lay along the edge of the biceps, but, in one of the three, the other artery, being the high ulnar, was nearer to it, having itself inclined inwards half an inch from the inner edge of the biceps. From the middle of the arm downward, in these, as in all cases of condyloid deviation, the artery and nerve, lying in the deep groove in front of the intermuscular septum, were bound down by a strong aponeurosis passing between the septum and raised fibres of the brachialis anticus, and, especially below, for two or three inches, also partially concealed by some of the fleshy fibres of that muscle.

The *practical conclusions* from these observations regarding the relative position of the two arteries, may be easily drawn. In a preliminary examination of the arm, the existence of two arteries may be ascertained by the pulsation, or it may be visible, as I have seen in an emaciated person. If no such preliminary information can be obtained, and if the artery first met with is not unusually small, and the ligature of it arrests the pulsation or bleeding below, there is no necessity or warrant for making a farther search. If it is otherwise, another vessel must be sought for. The artery first met with will, in all probability, be the more superficial. If it is in the upper half of the arm, the other artery will, if indeed the pulsation has not already indicated its position, be readily found a little deeper, separated only by the median nerve and a layer of fascia or sheath, either directly behind, or close to one side of, the one first found. The mere appearance of the artery being small, does not necessarily indicate that it is only one of two vessels, as, in some female subjects especially, the arteries look very small, and are, in all persons, considerably smaller than the distended arteries seen in the dissecting room and in museums; and, besides, it is not easy to judge of the true size of an artery in an operation, owing to the very limited extent to which it is exposed. It has been recommended, in the case of two arteries being met with, to secure that one only which is connected with the aneurism or bleeding vessel, but it is evident that there can be no greater danger to the vitality of the fingers from tying two arteries in a high division, than in tying the one artery when it is single, and various good reasons might be assigned for the rule to secure both. This rule may be departed from in the cases of false aneurism, and wound, when it is enough to place two ligatures on the artery concerned, one on each side of the aneurism or wound. These being the chief conditions requiring an operation in the lower part of the arm, it may not be necessary to search for the other artery, although the one first secured should evidently be only one of two. But if it is desired, the above anatomical details show that it will not usually be so easily found as in the upper half of the arm. It is often concealed by fleshy fibres from the brachialis anticus, and generally, either with or without this, bound down upon that muscle by a deeper aponeurosis. The median nerve is not now over it, but to its inner side, and the artery and nerve are often more or less removed towards the intermuscular septum, sometimes for three quarters of an inch; so that the artery will be found, not only deeper, beneath muscular or aponeurotic fibres, but in some part of the space between the inner edge of the biceps and the intermuscular septum.

The existence of two arteries at the bend of the arm is commonly referred to as increasing the danger in the operation of venesection. There are two chances instead of one, of puncturing an artery by a rash plunge of the lancet; and besides, and more especially, one of the arteries lies more superficial than the other, pressing forwards, as it were, against the back of the semilunar process of the biceps, and may therefore be more readily punctured by the lancet. But the belief as to the greater danger of wounding an artery, in the case of a high division, seems to have arisen from the error of describing the high artery as actually superficial or sub-cutaneous. Thus—"in general the anomalous artery is the radial, and is sub-cutaneous in its course"—sometimes both radial and ulnar are sub-cutaneous.[1] I have never seen an artery of large size lie over the aponeurosis of the limbs or occupy a truly sub-cutaneous position, but Mr Quain has seen

[1] Cycloped. Anat. and Phys., vol. i. p. 466.

the radial artery once,[1] and the ulnar on two occasions, in dissection, occupy this position, and several cases in which the latter artery was sub-cutaneous at the elbow, but became covered by the aponeurosis, after passing a short distance down the forearm; and states that he has also seen an instance of each of these in living persons, the condition being, according to him, easily recognised.

The two arteries are usually very differently covered at the bend of the arm. The deeper, as I have already described, is usually bound down by fleshy or aponeurotic fibres upon the brachialis anticus, or it may be overlapped or covered below by a higher origin than usual to the pronator teres muscle. The more superficial artery usually lies close against the posterior surface of the semilunar tendon of the biceps, in front of the deeper vessel, which may now be more directly behind it than higher up. The high radial artery I have seen lately, on two occasions, splitting the semilunar tendon, the thicker portion being still above it. Besides the two cases of high ulnar, already mentioned, in which the artery passed over the muscles but under the aponeurosis of the forearm, I lately examined carefully a case in which the ulnar arteries in both arms, arose half an inch below the level of the condyle, and passed, as the high ulnar usually does, to this superficial position. They lay over the semilunar process, and over the aponeurosis of the arm for three inches, when they perforated the aponeurosis and lay below it. But still they were not sub-cutaneous, in the sense in which we speak of the veins being so. A layer of aponeurosis passed over it, forming a kind of arch over it, and binding it down in a kind of tunnel, so that it did not lie in the superficial fascia, but really under a fibrous aponeurosis, though over the thicker and proper aponeurosis of the arm. The artery in this position had no venæ comites, these, from three inches above the wrist, left the artery and joined the superficial veins. The aponeurosis over the ulnar arteries in this case, was very readily removed, giving the artery afterwards the appearance of having been truly sub-cutaneous. The artery was seen and felt through the skin before the dissection was begun, as the subject

[1] Op. Cit., p. 318.

was emaciated, but it was not so prominent as the veins, which were also injected. This is probably what some would call a subcutaneous position, and it is worthy of future notice whether, in those rare cases in which the radial or ulnar arteries appear to lie over the aponeurosis, a layer of it, binding them down, has not been removed.

As regards the operation of venesection, a preliminary examination of the front of the elbow should always be made, and if there is an artery pulsating very distinctly, the other arm may be taken, or the vein opened at some little distance from where the artery is felt pulsating.

In the case of a high division, giving two arteries in the arm, I have always found each artery accompanied by venæ comites, giving, when the veins have been injected, a very complex appearance to the vessels. Some describe one vein only as accompanying the brachial artery, but I have always found two, with the usual cross communications. A few inches above the elbow, the basilic vein usually passes through an oblique opening in the aponeurosis, and joins, or is joined by, the internal of the two venæ comites, which thereafter is so large that the other is apt to be overlooked. As a general rule, venæ comites exist with all arteries in the upper extremity below the axillary, and in the inferior extremity below the popliteal.

XVI.

ON THE FASCIA OF SCARPA.

The fascia which I shall describe under this name, is that usually termed the deep layer of the superficial fascia, or the true superficial fascia, of the groin, and frequently also the fascia of Scarpa. Although the description of this fascia by Scarpa, as contained in the translation of his work on hernia, is very brief and imperfect, I shall retain the title fascia of Scarpa, as it is preferable to have a short specific name for a part to which frequent reference is to be made.

This fascia, I believe, is not commonly understood fully, in the simplicity of its anatomy and importance of its surgical bearings, and is, at least in some text books, incorrectly described.

It is described as a deeper layer of the superficial fascia of the abdomen, passing down over Poupart's ligament, adhering to it as it crosses, and then terminating on the fascia lata across the groin, or by some it is said to pass down the femoral region, as a deeper layer of the superficial fascia there, separated from the other by the superficial vessels and glands, and spreading over the saphenous opening so as to form the cribriform fascia.

In the common mode of conducting the dissection of the groin, the true origin of the fascia is divided; and unless the dissection is made with a view to the demonstration of this fascia, its nature and connections cannot be seen or understood.

Dissection to demonstrate the Fascia of Scarpa.—Having re flected the skin of the groin for some inches both above and below Poupart's ligament, divide the superficial fascia of the thigh two or three inches below Poupart's ligament, and, using the point and handle of the scalpel, turn it up, off the surface of the glands and superficial vessels, to about an inch above Poupart's ligament. Next, by an incision curving down from the anterior superior spinous process of the ilium to near the symphysis pubis, divide the fascia through its whole depth, down to the tendon of the external oblique; and now dissect the whole fascia down, close off the external oblique. The lax cellular tissue here, yields almost to the handle of the scalpel, but at, or immediately below Poupart's ligament, the handle of the scalpel is suddenly and firmly arrested. The dissection is now done. Now take the femoral end of the dissected superficial fascia in one hand, and the abdominal end in the other hand, lifting them up a little and stretching them up and down, and, on looking in below, a thin semi-transparent fibrous membrane is seen, passing between the superficial fascia and the fascia lata. This is the fascia of Scarpa, fully displayed; but if the glands and surrounding cellular tissue be now carefully picked away from its lower or femoral aspect, the fascia will be more clearly seen.

Description of the Fascia of Scarpa and its Relations.—The fascia of Scarpa is a thin semi-transparent membrane, of considerable strength, and not merely cellular, but fibrous in its nature. It arises from the fascia lata, across the groin, immediately below Poupart's ligament; passes up over the latter, and, after a course of an inch, as a separate membrane, joins and blends with the common superficial fascia of the abdomen. It is separated from the common superficial fascia opposite Poupart's ligament, and for an inch, or half an inch, above it, by the transverse set of lymphatic glands and the superficial vessels. Immediately above the position of these glands, it unites or blends with the common superficial fascia, and is not naturally separable from it farther, that is, not without artificial dissection, although the fibres of it are no doubt continued up and across the abdomen, as the deeper aspect of the common superficial fascia.

This fascia belongs altogether to the abdominal region of the groin; it does not exist in the femoral region; but begins immediately below Poupart's ligament, as a prolongation upwards from the fascia lata, as a thin aponeurotic membrane, as broad as the groin itself, and about an inch in depth as a separate membrane, from its attached margin, or origin, obliquely upwards to where it becomes united with the common superficial fascia.

The error of some, in describing this fascia as passing downwards into the femoral region, forming a deeper layer of superficial fascia there and also the cribriform fascia, has no doubt arisen from the circumstance that the true origin of the fascia, from the fascia lata, is divided with the knife, under the impression that it is only an adhesion of the fascia to Poupart's ligament which is being divided; but if this be divided, the fascia of Scarpa is destroyed. For some inches below Poupart's ligament, no doubt there is the appearance as if the common superficial fascia consisted of two layers, separated by the femoral glands and superficial vessels; a thicker and superficial, containing more or less fat; and, behind and between the glands, laminated cellular tissue as a deeper layer. But these are not separate fasciæ; they both belong to the common superficial fascia, are both cellular in their nature, and quite continuous with each other, except at the points where a gland or vein necessarily separates them in making a bed for itself. It is therefore unnecessary and erroneous, to describe the superficial fascia below Poupart's ligament as consisting of two separate layers or separate fasciæ.

It is also incorrect to describe the fascia of Scarpa as passing over the saphenous opening and there forming the cribriform fascia; first, because this fascia stops, or rather begins, close, as close as possible, below Poupart's ligament; and secondly, because the cribriform fascia is a different structure, the correct understanding of which is bound up with a correct understanding of the nature of the saphenous opening.

The *cribriform fascia* is simply a thin covering to the saphenous opening, and limited to this; prolonged from the outer edge of the opening, and losing itself by covering and uniting with the femoral sheath, which, on the inside, adheres to the pubic portion of the fascia lata on the pectineus muscle. The outer and upper parts of the edge of the saphenous opening do not naturally exist as defined margins, until this true prolongation from them is divided with the edge of the knife; and the cribriform fascia is, therefore, a thin prolongation of the fascia lata itself across the opening, and not a portion of the superficial fascia adhering to the opening as it crosses it.

Relation of the Fascia of Scarpa to the Fascia of the Perineum.—Besides those already described, the fascia of Scarpa has another important connection, in its continuation around the spermatic cord, towards the perineum, where it becomes continuous with the true superficial fascia of that region; a connection which is usually correctly described. The fascia in the perineum is variously called the deep or true superficial fascia, the deep layer of the superficial fascia, or the superficial fascia. Amongst these various definitions the student is often at a loss to know which structure is being described, and to avoid this source of confusion and derive the advantage from the use of a short specific name, I am in the habit of calling it the *fascia of Colles*, as the general connections of this membrane, and especially the course of infiltrated urine determined by the fascia, appear to have been first fully described in this country by the late Mr Colles of Dublin, in his excellent and practical treatise on surgical anatomy; and if anatomists would agree to call these two fasciæ the fascia of Scarpa, and the fascia of Colles, it would save much perplexity to the student.

The fact is, there is only one superficial fascia in the body,—the common superficial fascia,—equally extensive with the skin, composed fundamentally of cellular tissue, and containing more or less or no fat according to circumstances, but whether very thin, or very thick from fat, it is still the same cellular membrane, composed of areolar or filamentous, or what is after all much better to be called by the old name of cellular tissue; and although, especially when fat is laid down in it stratum after stratum, presenting sometimes an appearance of layers, it is still one fascia, the common superficial fascia, reaching from region to region as the skin does.

Then we come to the deep fasciæ or aponeuroses, not cellular, but fibrous membranes, thicker or thinner according to circumstances, and serving the altogether separate purpose of binding down the muscles; and not usually, like the skin and superficial fascia, common from region to region, but with its special anatomy in each region, connected with its bones and binding down its muscles. Now, the fasciæ of Scarpa and Colles are, in their nature, intermediate between the superficial fascia and the aponeurosis. They are deep and fibrous membranes, aponeurotic, at their origin from fibrous structures or aponeuroses, but become cellular at their other connection, as they blend with the superficial fascia; they pass between the deep and the superficial fasciæ and partake of the nature of each as they join each.

The fasciæ of Scarpa and Colles form one continuous membrane.—The latter joins the base of the triangular ligament, or true deep fascia, of the perineum, and is also there, like the latter, continuous with the thin fibrous membrane or fascia on the outer surface of the levator ani muscle; at the sides, it is fixed to the rami of the ischium and pubes, and upwards from this, it is fixed to the fascia lata, keeping some distance outwards from the pubes. Here it runs directly into the fascia of Scarpa; which now forms a continuation of it, as a barrier across the groin, immediately below Poupart's ligament. The origin, or fixed or deep line of attachment, of this united fascia, then, is,—the base of the triangular ligament, the rami of the ischium and pubes, the pubic portion of the fascia lata over near the origin of the gracilis and adductor longus muscles, and the fascia lata all the way across the groin immediately below Poupart's ligament. The posterior part—fascia of Colles—is continued forwards into the scrotum, blending with the common superficial fascia, and thus forms a pouch, closed behind in front of the anus, closed laterally at the sides of the perineum, and open forwards into scrotum and upwards around the cord. The anterior part, or fascia of Scarpa, continues the attachment of the sides of the pouch across the groin, so that the pouch, continued up from the scrotum along the cord, now widens out and opens laterally and upwards on the abdominal wall, between the continuation of the fascia of Scarpa and the surface of the tendon of the external oblique muscle, in which space there is a plentiful and lax cellular tissue.

Relation of the Fasciæ to Infiltration of Urine.—When the urine escapes from the urethra into the perineum, it passes forwards. It cannot pass backwards to the ischio-rectal fossa or anal region, on account of the reflection of the fascia of Colles to join the base of the triangular ligament; it cannot gravitate downwards along the inside of the thighs, owing to the lateral closure of the pouch; and it must therefore pass forwards, in the direction where no obstruction is presented by the lax cellular tissue. This may be readily illustrated by introducing a blow-pipe, or bellows, through an opening in the fascia of Colles, in front of the anus, when the air will readily force its way forwards and inflate the scrotum, but does not reach backwards or side ways.

The infiltrated urine, if not evacuated by incisions, made deep enough to reach it, continues to ascend over the cord, passing up to the hypo-gastric or even to the umbilical region, but it does not gravitate downwards to the front of the thigh. This is prevented by the fascia of Scarpa, which, forming a fibrous barrier across the whole groin, fixed to the fascia lata, effectually prevents the fluid from passing downwards. It therefore passes upwards, and infiltrates the plentiful and very lax cellular tissne which lies close on the outer surface of the tendon of the external oblique; the continuation upwards of the fascia of Scarpa, as the deeper aspect of the superficial fascia of the abdomen, still preventing the fluid from becoming subcutaneous and descending to the thigh.

Relations of the Fascia of Scarpa to Hernia.—The fascia of Scarpa forms one of the coverings of the cord, as it passes to be continuous with the fascia of Colles, turning round in below the cord, and forming a kind of tube or canal within which the cord descends to the testis in the scrotum. The fascia of Scarpa, then, assists in directing a complete inguinal hernia down into the scrotum, and forms one of its coverings. It does not form a covering to the femoral hernia, but a complete femoral hernia lies upon it; and its union, a little above Poupart's ligament, with the common superficial fascia, will tend to obstruct the farther upward passage of a femoral hernia. The femoral hernia, after it has passed down the crural canal (which is rather, as far as the hernia is concerned, a broad ring than a canal) becomes complete by turning round the lower margin of the falciform process, there being now no longer any dense structure to keep it from swelling forwards, and now passes up so as to lie upon the falciform process and on the inner part of Poupart's ligament. Here it lies on the fascia of Scarpa, and has insinuated itself into the space occupied by the glands, and, meeting with the angle of union between the superficial fascia and the fascia of Scarpa, it will be arrested in its farther progress upwards, although at the same time, irrespective of this, there is little natural tendency in the femoral hernia to continue to ascend, but rather simply to swell out in all directions, if it continues to enlarge.

The true relation of the fascia of Scarpa to the fascia lata was, as far as I am aware, first correctly described by Mr Liston[1] in a Memoir, the chief purpose of which was to show that the crural arch, or deep crural arch, is formed independent of Poupart's ligament, by the iliac portion of the fascia lata passing in behind Poupart's ligament, and joining with the fascia transversalis; the union of the two, arching inwards over the vessels and crural ring, and forming a tight arch, which remains so after Poupart's ligament has been altogether dissected out. He describes the true origin of the fascia which I have described as that of Scarpa, and corrects the notion that it passes down upon the thigh. He thus very correctly describes the terminations or connections of the fascia lata at the groin—"The iliac portion, forming the falciform process, is again divided into two layers; the one passing anteriorly to the abdominal muscles, and named the superficial abdominal fascia; the other, again, lining their inner surface, and passing betwixt the transversalis muscle and peritoneum, forming the fascia transversalis. The lower part of the abdominal muscles

[1] Memoir on the Formation and Connections of the Crural Arch. Edin. 1819.

and Poupart's ligament thus lie in the angle of splitting, or union of all these fasciæ, and the crural arch is formed by this junction."

He refers to the relation of this fascia to inguinal hernia, but not to its relation to the fascia in the perineum, or to the course of the infiltrated urine, nor does he give a more extended view of its connections than that stated or implied in the above quotation, which, however, shows that he was acquainted with its true origin from the fascia lata close below Poupart's ligament.

The description I have given, embraces what I believe to be the true anatomy and surgical bearings of the fascia of Scarpa. The reason why it is by some incorrectly described, and perhaps in none of our anatomical works described clearly as having the anatomy I have endeavoured to explain, is, I believe, that its origin, as a continuation upwards of the fascia lata is usually divided, either from above or from below, after which the fascia cannot be seen, that in fact this fascia is but seldom seen in dissections. But if the dissection is conducted according to the method above described, the demonstration of the fascia is very easy, and it only requires to be once seen in this way, to satisfy the dissector that this is its true anatomy.

The whole anatomy of this fascia is very simple, and, together with its surgical bearings, may be shortly described thus—

The *fascia of Scarpa* arises from the fascia lata close below Poupart's ligament, passes upwards for an inch, and blends with the common superficial fascia. It is separated from the lower part of the tendon of the external oblique, behind, by very loose cellular tissue, and, before, from the common superficial fascia by the superficial glands and vessels; and is continued inwards around the cord, becoming continuous with the fascia of Colles or true superficial fascia of the perineum. It is a thin aponeurotic or fibrous membrane, forming a barrier or septum across the groin, by passing between the fascia lata and the common superficial fascia. When the urine, infiltrated in the perineum, has been directed upwards along the cord by the fascia of Colles, the fascia of Scarpa prevents it from passing down the front of the thigh. In relation to hernia,—it assists in directing an inguinal hernia into the scrotum, and forms one of its coverings; it is covered by a femoral hernia, and tends to prevent the femoral hernia from passing upwards on the abdomen, by means of its union with the common superficial fascia.

END OF PART I.

MURRAY AND GIBB, PRINTERS, EDINBURGH.