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OBSERVATIONS
ON
CERTAIN PARTS
OF THE
ANIMAL OECONOMY.

By JOHN HUNTER.

SECOND EDITION.



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T O

SIR JOSEPH BANKS, BART.

PRESIDENT OF THE ROYAL SOCIETY,

&c. &c. &c.

DEAR SIR,

As the following Observations were made in the Course of those Pursuits in which you have so warmly interested yourself, and promoted with the most friendly Assistance, I should be wanting in Gratitude, were I not to address them to you, as a public Testimony of the Friendship and Esteem with which I am,

DEAR SIR,

YOUR OBLIGED, AND

VERY HUMBLE SERVANT,

LEICESTER-SQUARE,
NOVEMBER, 9, 1786.

JOHN HUNTER.

THE

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ASTON MEDICAL
MAR 15 1922

A DESCRIPTION OF THE SITUATION OF THE TESTIS IN THE FOETUS, WITH ITS DESCENT INTO THE SCROTUM.

A Discovery in any art, not only enriches that with which it is immediately connected, but elucidates all those to which it has any relation. The knowledge of the construction of a human body is essential to medicine, therefore every improvement in anatomy must throw additional light on that branch of science. These improvements strike more forcibly, if they are on subjects quite new or little understood; and this effect is well illustrated by the advantages which pathology has derived from the discovery of the lymphatics being the absorbent system; and likewise by that case of hernia, where the intestine lies in contact with the testicle; which has been perfectly explained by the discovery of the original seat of the testicle being in the abdomen.

Several years before Haller's *Opuscula Pathologica* were published, my brother informed me, that in examining the contents of the abdomen of a child, stillborn, about the seventh or eighth month, he found both the testicles lying in that cavity, and mentioned the observation with some degree of surprize. By this we are enabled to account for a circumstance that sometimes happens in the scrotal hernia, as depending on the discovery that, the testis is formed in the abdomen; and which we could never explain to our satisfaction, till the publication of the *Opuscula*, to which Dr. Hunter alludes, *Commentaries*, page 72, in the following words:

“ In the latter end of the year 1775, when I first had the pleasure of reading Baron Haller's *Observations on the Hernia Congenita**, it struck my imagination that, the state of the testis in the foetus, and its descent from the abdomen into the scrotum, would explain several things concerning ruptures and the hydrocele, particularly that observation which Mr.

* Alberti Halleri *Opuscul. Patholog.* Lausan. 1755, 8vo. page 53, &c.

Sharp had communicated to me, viz. That in ruptures the intestine is sometimes in contact with the testis. I communicated my ideas upon this subject to my brother, and desired that he would take every opportunity of learning exactly the state of the testis before and after birth, and the state of ruptures in children. We were both convinced that the examination of those facts would answer our expectation, and both recollected having seen appearances in children that agreed with our supposition; but saw now that we had neglected making the proper use of them.

“ In the course of the winter, my brother had several opportunities of dissecting fœtuses of different ages, and of making some drawings of the parts; and all his observations agreed with the ideas I had formed of the nature of ruptures, and of the origin of the tunica vaginalis propria in the fœtus. But till those observations were repeated to his satisfaction, and were sufficiently ascertained, he desired me not to mention the opinion in my lecture; and therefore, when treating of the coats of the testis, and of the situation of the hernial sac, &c. I only put in this temporary caution, that I was then speaking of those things as they are commonly in adult bodies, and not as they are in the fœtus: and at last, when I was concluding my lectures for that season, in the end of April 1756, with a course of the chirurgical operations, I gave a very general account of my brother's observations, and shewed both the drawing of Fig. II. which was then finished, and the subject from which it was made.”

The following observations on this subject were taken from my notes, and published by Dr. Hunter, in his Commentaries, to which I have added some practical remarks.

Until the approach of birth, the testes of the fœtus are lodged within the cavity of the abdomen, and may therefore be reckoned among the abdominal viscera. They are situated immediately below the kidneys, on the forepart of the psoæ muscles, and by the side of the rectum, where this intestine is passing down into the cavity of the pelvis: for in the fœtus, the rectum, which is much larger in proportion to the capacity of the pelvis than in the fullgrown subject, lies before the vertebræ lumborum as well as before the os sacrum. Indeed the case is pretty much the same with regard to all the contents of the pelvis; that is, their situation is much higher in

the foetus than in the adult. The sigmoide flexure of the colon, part of the rectum, the greatest part of the bladder, the fundus uteri, the Fallopian tubes, &c. being placed in the foetus above the hollow of the pelvis in the common or great abdominal cavity.

While the testis remains in the abdomen, its shape or figure is much the same as in the adult; and its position or attitude the same as when it is in the scrotum: that is, one end is placed upwards, the other downwards; one flat side is to the right, the other to the left; and one edge is turned backwards, the other forwards, and the vessels enter the posterior edge alike in both the foetus and adult. As the testis is not so immediately inclosed in the surrounding parts while it is in the loins, its position may be a little variable; and the most natural seems to be when the anterior edge is turned directly forwards; but as the least touch of any thing will throw that edge either to the right side, or to the left, then the flat side of the testis will be turned forwards. It is attached to the psoas muscle all along its posterior edge, except just at its upper extremity; and this attachment is formed by the peritonæum, which covers the testis and gives it a smooth surface, in the same manner as it envelopes the other loose abdominal viscera.

The epididymis lies along the outside of the posterior edge of the testis, as when in the scrotum, but is larger in proportion, and adheres backwards to the psoas. When the foetus is very young, the adhesion of the testis and epididymis to the psoas is very narrow; and then the testis is more loose, and more projecting: but as the foetus advances in months, the adhesion of the testis to the psoas becomes broader and tighter.

The vessels of the testis, like those of most parts of the body, commonly rise from the nearest larger trunks, viz. from the aorta and cava, or from the emulgents.

The artery generally rises from the forepart of the aorta, a little below the emulgent artery, and often from the emulgent itself, especially in the right side of the body, which may happen the rather, because the trunk of the aorta is more distant from the right testis than from the left. Sometimes, but much more rarely, the spermatic artery springs from the phrenic, or from that of the capsula renalis. Besides the artery which

rises from the aorta, or emulgent, &c. the testis receives one from the hypogastric artery, which is sometimes as large as the other. It runs upwards from its origin, passing close to the vas deferens in its way to the testis. The superior spermatic artery sometimes passes before the lower end of the kidney; and both these arteries run in a serpentine direction, making pretty large but gentle turnings. They are situated behind the peritonæum; and both run into the posterior edge of the testis, between the two reflected laminae of that membrane, much in the same manner as the vessels pass to the intestines between the two reflected laminae of the mesocolon or mesentery.

The veins of the testis are analogous to its arteries; but commonly change sides with the arteries respecting their origins from the emulgents. The superior spermatic vein, to begin with its trunk, rises commonly in the following manner: on the right side, from the trunk of the vena cava, a little below the emulgent, and on the left side from the left emulgent vein. The reason of this difference between the right and left spermatic vein, no doubt, is because the cava is not placed in the middle of the body; so that by the rule of ramification, which is observed in most parts of the body, the cava is the nearest large vein of the right side, and the emulgent is the nearest large vein of the left side. But the difference is inconsiderable; and accordingly we sometimes find the right spermatic vein coming from the right emulgent vein; and several other varieties are produced, which, so far as I can observe, follow no precise rule. There is likewise a spermatic vein, which rises from the internal iliac, and runs up to the testis with the inferior spermatic artery. Both the spermatic veins run behind the peritonæum with their corresponding arteries, and go into the posterior edge of the testis, where they are lost in small branches.

The nerves of the testis, like its blood-vessels, come from the nearest source; that is, from the abdominal plexuses of the intercostal; especially the inferior mesenteric plexus. They run to the testis, accompanying its blood-vessels, and are dispersed with them through its substance. The testis therefore, with respect to its nerves, may be reckoned an abdominal viscus; and this observation will hold good, when applied to the full-grown subject, as well as to the foetus; for those branches of the lumbar

nerves, which are commonly said to be sent to the testis, passing through the tendon of the external oblique muscle, in reality go not to the testis itself, but to its exterior coverings, and to the scrotum.

The testicle receiving its nerves from the plexuses of the intercostal, accounts for the stomach and intestines sympathizing so readily with it and its particular sensation, and for the effects arising in the constitution upon its being injured.

The epididymis begins at the outer and posterior part of the upper end of the testis, immediately above the entrance of the blood-vessels, where it is thick, round, and united to the testis. As it passes down, it becomes a little smaller and more flat, and is only attached backwards to the testis, or rather indeed to its vessels; for its anterior edge lies loose against the side of the testis forwards; and at its lower end it is again more firmly attached to the body of the testis, so that in the foetus there is a cavity or pouch formed between the middle part of the testis, and the middle part of the epididymis, more considerable than is commonly observed in fullgrown subjects. As the body grows, the epididymis adheres more closely to the side of the testis; and its greatest part is made up of one convoluted canal, which becomes larger in size and less convoluted towards the lower end, and at last is manifestly a single tube running a little serpentine. That change happens at the lower end of the testis, and there the canal takes the name of vas deferens.

The vas deferens is a little convoluted, or serpentine, in its whole course, but is less so as it comes nearer to the bladder; instead of running upwards from the lower end of the testis, as it does when the testicle is in the scrotum, while that remains in the abdomen, it runs downwards and inwards in its whole course, so that it goes on almost in the direction of the epididymis, of which it is a continuation. It turns inwards from the lower end of the epididymis, under the lower end of the testis, and behind the upper end of a ligament or gubernaculum testis, (which I shall presently describe) then it passes over the iliac vessels and over the inside of the psoas muscle, somewhat higher than in adult bodies; and at last goes between the ureter and bladder towards the basis of the prostate gland.

In those animals where the testicles change their situation, the cremaster muscle, which should be named *musculus testis*, has two very different positions in the fœtus, and in the adult: the first, being the same as in those animals whose testicles remain through life in the cavity of the abdomen; we must therefore conclude that, the same purposes are answered by this muscle in the fœtus, as in those animals.

The use of this muscle, when the testicle is in the scrotum, appears to be evidently that of a suspensory; for I find this muscle is strong in proportion to the size of the testicle, and pendulous situation in other animals. But what purpose it answers in the fœtus, or in animals whose testicles remain in the abdomen, is not easily imagined, there being no apparent reason why such a muscle should exist.

The cremaster, or *musculus testis*, appears to be composed of the lower fibres of the *obliquus internus* and *transversalis* muscles in the fœtus, turning upwards, and spreading upon the anterior surface of the gubernaculum, immediately under the peritonæum; it appears to be lost on the peritonæum, a little way from the testicle. This, although now inverted, is more evidently seen in adult subjects, who have had a hydrocele, or rupture; in such cases the muscle becomes stronger than usual, and its fibres can be traced spreading on the *tunica vaginalis*, and seem at last to be lost upon it, near to the lower end of the body of the testicle.

The nerves which supply this muscle are, probably, branches from the nerves of the *obliquus internus* and *transversalis* muscles; for the same cause which throws the abdominal muscles into action, produces a similar effect on the *musculus testis*; which circumstance appears to be most remarkable in the young subject. When we cough or act with the abdominal muscles, we find the testicles to be drawn up; the *musculus testis* and abdominal muscles taking on the same action from the same cause.

At this time of life the testis is connected in a very particular manner with the parietes of the abdomen, at that place where in adult bodies the spermatic vessels pass out, and likewise with the scrotum. This connection is by means of a substance which runs down from the lower end of the testis to the scrotum, and which at present I shall call the ligament, or *gubernaculum testis*, because it connects the testis with the scrotum,

and seems to direct its course through the rings of the abdominal muscles. It is of a pyramidal form ; its large bulbous head is upwards, and fixed to the lower end of the testis and epididymis, and its lower and slender extremity is lost in the cellular membrane of the scrotum. The upper part of this ligament is within the abdomen, before the psoas, reaching from the testis to the groin, or to where the testicle is to pass out of the abdomen ; whence the ligament runs down into the scrotum, precisely in the same manner as the spermatic vessels pass down in adult bodies, and is there lost. The lower part of the round ligament of the uterus, in a foetus, very much resembles this ligament of the testis, and may be plainly traced down into the labium, where it is imperceptibly lost. That part of the ligamentum testis, which is within the abdomen, is covered by the peritonæum all round, except at its posterior part, which is contiguous to the psoas, and connected with it by the reflected peritonæum, and by the cellular membrane. It is hard to say what is the structure or composition of this ligament : it is certainly vascular and fibrous, and the fibres run in the direction of the ligament itself, which is covered by the fibres of the cremaster or musculus testis, placed immediately behind the peritonæum. This circumstance is not easily ascertained in the human subject ; but is very evident in other animals, more especially in those whose testicles remain in the cavity of the abdomen after the animal is fullgrown.

In the hedgehog, the testes continue through life to be lodged within the abdomen, in the same situation as in the human foetus ; and they are fastened by the same kind of ligament to the inside of the parietes of the abdomen at the groin. Now, in that animal, I find that the lowermost fibres of the internal oblique muscle, which constitute the cremaster, are turned inwards at the place where the spermatic vessels come out in other animals, making a smooth edge, or lip, by their inversion ; and that then they mount up on the ligament to the lower end of the testis. Sometimes in the human body, and in many other animals, and very often in sheep, the testes do not descend from the cavity of the abdomen till late in life, or never at all. In the ram, when the testis is come down into the scrotum, the cremaster is a very strong muscle ; and, though it be placed more inwards at its beginning, it passes down pretty much as it does in

the human body, and is lost on the outside of the tunica vaginalis: but in the ram, whose testis still remains suspended in the abdominal cavity, I find that the cremaster still exists, though it is a weaker muscle; and instead of passing downwards, as in the former case, it turns inwards and upwards, and is lost in the peritonæum that covers the ligament which attaches the testis to the parietes of the abdomen; which, in this state of the animal is about an inch and a half in length. In the human fœtus, while the testis is retained in the cavity of the abdomen, the cremaster is so slender that I cannot trace it to my own satisfaction, either turning up towards the testis, or turning down towards the scrotum. Yet from analogy we may conclude that it passes up to the testicle; since, in the adult, we find it inserted or lost on the lower part of the tunica vaginalis, in the same manner as in the adult quadruped.

The peritonæum, which covers the testis and its ligament or gubernaculum, is firmly united to the surfaces of these two bodies; but all around, to wit, on the kidney, the psoas, the iliacus internus, and the lower part of the abdominal muscles, that membrane adheres very loosely to all the surfaces which it covers. Where the peritonæum is continued or reflected from the abdominal muscles to the ligament of the testis, it passes first downwards a little way, as if going out of the abdomen; and then upwards, so as to cover more of the ligament than is within the cavity of the abdomen. At this place the peritonæum is very loose, thin in its substance, and of a tender gelatinous texture; but all around the passage of that ligament, the peritonæum is considerably tighter, thicker, and of a more firm texture. When the abdominal muscles are pulled up so as to tighten and stretch the peritonæum, this membrane remains loose at the passage of the ligament, while it is braced or tight all around; and in that case the tight part forms a kind of border or edge around the loose double part of the peritonæum, where the testis is afterwards to pass. This loose part of the peritonæum, like the intro-suscepted gut, may, by drawing the testis upwards, be pulled up into the abdomen, and made tight; and then there is no appearance of an aperture or passage down towards the scrotum; but when the scrotum and ligament are drawn downwards, the loose doubled part of the peritonæum descends with the

ligament, and then there is an aperture from the cavity of the abdomen all around the forepart of the ligament, which seems ready to receive the testis. This aperture becomes larger when the testis descends lower, as if the pyramidal or wedge-like ligament was first drawn down, in order not only to direct, but to make room for the testis which must follow it. In some foetuses I have found the aperture so large, that I could push the testis into it, as far as the tendon of the external oblique muscle.

From this original situation within the abdomen, the testis afterwards descends to its destined station in the scrotum; but it becomes difficult to ascertain the precise time of this descent, as we hardly ever know the exact age of our subject. According to the observations which I have made, it seems to happen sooner in some instances than in others; but generally about the eighth month. In the seventh month I have commonly found the testis in the abdomen; and in the ninth I have as commonly found it in the upper part of the scrotum. The descent being thus early, and the passage being almost immediately closed, are the principal means of preventing the hernia congenita.

At the beforementioned period, the testis moves downwards till its lower extremity comes into contact with the lower part of the abdominal parietes: when the upper part of the ligament, which hitherto was within the abdomen, has sunk downwards, it lies in the passage from the abdomen to the scrotum, and in that which is afterwards to receive the testis. As the testicle passes out, it in some degree inverts the situation of the ligament passing down beyond it; what was the anterior surface of the ligament, while in the abdomen, now becoming posterior and composing the lower and anterior part of the tunica vaginalis, on which the musculus testis is lost. This is more evident in those animals whose testicles can readily be made to pass up from the scrotum to the abdomen. The place where the ligament is most confined, and where the testis meets with most obstruction in its descent, is the ring in the tendon of the external oblique muscle; and accordingly I think we see more men with one testis, or both, lodged immediately within the tendon of that muscle, than who have one or both still included in the cavity of the abdomen, which I shall take notice of hereafter.

After the testis has got quite through the tendon of the external oblique muscle, it may be considered as now in a way easily to acquire its determined station; though it commonly remains for some time by the side of the penis, and only by degrees descends to the bottom of the scrotum: and when the testis has descended entirely into the scrotum, its ligament is still connected with it, and lies immediately under it, but is shortened and compressed.

Having now given an account of the original situation of the testes, of the time of their descent from the abdomen, and of the route which they take in their passage to the scrotum, I shall in the next place describe the manner in which they carry down the peritonæum with them, and then explain how that membrane forms the tunica vaginalis propria in common, and the sac of the hernia congenita in some bodies.

While the testis is descending, and even when it has passed into the scrotum, it is still covered by the peritonæum, exactly in the same manner as when within the abdomen; the spermatic vessels running down behind the peritonæum there, as they did when the testis lay before the psoas muscle: that lamella of the peritonæum is united behind with the testis, the epididymis, and the spermatic vessels, as it was in the loins, and likewise with the vas ferens; but the testis is fixed posteriorly to the parts against which it rests, being unconnected and loose forwards, as while it remained in the abdomen. In coming down, the testis brings the peritonæum with it; and the elongation of that membrane; though in some circumstances it be like a common hernial sac, yet in others is very different. If we can imagine a common hernial sac reaching to the bottom of the scrotum, covered by the cremaster muscle; and that the posterior half of the sac covers, and is united with, the testis, epididymis, spermatic vessels, and vas deferens; and that the anterior half of the sac lies loose before all those parts, it will give a perfect idea of the state of the peritonæum, and of the testis when it comes first down into the scrotum. The testis, therefore, in its descent, does not fall loose, like the intestine or epiploon, into the elongation of the peritonæum; but slides down from the loins, carrying the peritonæum with it; and both that and the peritonæum continue to adhere, by the cellular membrane,

to the parts behind them, as they did when in the loins. This is a circumstance which I think may be easily understood; and yet that does not appear to be the case; for I find students very generally puzzled with it; imagining that, when the testis comes first down, it should be loose all round, like a piece of the gut or epiploon in a common hernia. The ductility of the peritonæum, and its very loose connection by a slight cellular membrane to the psoas muscle, and all the other parts around the testis, are circumstances which favour its elongation and descent into the scrotum with the testis. This peculiarity of descent often takes place in some of the intestines; but can only happen in those which have adhesions to the loins. This I suspect is only to be met with in old ruptures; never happening at the first formation of the hernial sac, in which the intestine lies; and, I should suppose, could only form very gradually. The cæcum has sometimes been found to have descended into the scrotum, and to have brought along with it the adhesions through its whole course. The same thing has happened to the sigmoide flexure of the colon; and I have found the whole of it in the left side of the scrotum, with its adhesions brought down from the loins. Such herniæ cannot be reduced; and in case of strangulation, which may be brought on by a fresh portion of intestine coming down, are not to be treated in the common way; the sac should not be opened, but the stricture divided, and the newly protruded part reduced.

It is plain, from this description, that the cavity of the bag, or of the elongation of the peritonæum, which contains the testis in the scrotum, must at first communicate with the general cavity of the abdomen, by an aperture at the inside of the groin. That aperture has exactly the appearance of a common hernial sac; the spermatic vessels and vas deferens lie immediately behind it, and a probe passes readily through it from the general cavity of the abdomen down to the bottom of the scrotum. And if this process of the peritonæum be laid open through its whole length on the forepart, it will be plainly seen to be a continuation of the peritonæum: the testis and epididymis will appear at the lower part of it; and the spermatic vessels and the vas deferens will be found covered by the posterior part of the bag, in their whole course from the groin to the testis.

Thus it is in the human body, when the testis is recently come down; and thus it is, and continues to be through life, in every quadruped which I have examined, where the testis is in the scrotum; but in the human body, the communication between the sac and the cavity of the abdomen is soon cut off. Indeed I believe that the upper part of the sac naturally begins to contract as soon as the testis has passed through the muscles; which opinion is grounded on the following observation: In an instance where from the age of the fœtus, and from every other mark, it was probable that the testis was very recently come down, and yet the upper part of the sac was very narrow I, pushed the testis upwards, in order to see if it could be returned: the attachments of the testis easily admitted of its ascent, and so did the aperture in the tendon of the external oblique muscle; but the orifice and upper end of the sac would not, by any means, admit of the testis being passed quite up into the abdomen. However this may be, the upper end of the sac certainly contracts and unites first, and is quite closed in a very short space of time; for it is seldom that any aperture remains in a child born at its full time; and this contraction and union is continued downwards till it comes near the testicle, where this disposition does not exist, leaving the lower part of the sac open or loose, through life, even in the human subject, and forming the tunica testis vaginalis propria, the common seat of an hydrocele. Many cases of hydrocele, in children, seem to prove that the progress of this contraction and union is downwards; for in them the water commonly extends higher up the chord than in the adult, except in those of a considerable size: yet in some children this union seems not to take place regularly, being interrupted in the middle, and producing an hydrocele of the chord, which neither communicates with the abdomen nor tunica vaginalis testis. The contraction and obliteration of the passage appears to be a peculiar operation of Nature, depending upon steady and uniform principles, and not the consequence of inflammation, nor of any thing that is accidental; and, therefore, if it is not accomplished at the proper time, the difficulty of bringing about an union of the parts is much greater, as is seen in children who have had the sac kept open by a turn of the intestine falling down into the scrotum immediately after the testis.

This looks as if Nature, from being balked when she was in the humour to do her work, would not, or could not so easily do it afterwards. I shall readily grant that what has been advanced here as a proof of the doctrine, may be explained upon other principles: but this, at least, is certain, that the closing of the mouth, and of the neck of the sac, is peculiar to the human species; and we must suppose the final intention to be, the prevention of ruptures, to which men are so much more liable than beasts, from their erect state of body. In some cases the aperture of the sac is not entirely closed, allowing a fluid to pass down and form an hydrocele; which fluid, upon pressure, can be squeezed back into the belly: and instances of this kind sometimes giving the idea of a gut being protruded, make it difficult to determine the exact nature of the case.

What is the immediate cause of the descent of the testis from the loins to the scrotum? It is evident, that it cannot be the compressive force of respiration; because, the testis is commonly in the scrotum before the child has breathed; that is, the effect has been produced before the supposed cause has existed. Is the testis pulled down by the cremaster muscle? I can hardly suppose that it is; because, if that was the case, I see no reason why it should not take place in the hedgehog, as well as in other quadrupeds; and if the musculus testis had this power, it could not bring it lower than the ring of the muscle.

Why do the testes take their blood-vessels from such distant trunks? Those physiologists, who have puzzled themselves about the solution of this question, have not considered, that in the first formation of the body, the testes are situated, not in the scrotum, but immediately below the kidneys; and that, therefore, it was very natural that their blood-vessels should rise nearly in the same manner as those of the kidneys, but a little lower. The great length of the spermatic vessels in the adult body will no doubt occasion a more languid circulation, which, we may suppose, was the intention of Nature.

The situation of the testis in the foetus may likewise account for the contrary directions of the epididymis and of the vas deferens in adult bodies, though these two in reality make only one excretory canal. In the foetus the epididymis begins at the upper end of the testis; and it is

natural, considering it as an excretory tube, that it should run downwards. And it is as natural that the rest of the tube, which is called vas deferens, should turn inwards at the lower end of the testis, because that is its most direct course to the neck of the bladder. Thus we see that in the foetus the excretory duct is always passing downwards. But the testis is directed in its descent by the gubernaculum, which is firmly fixed to the lower parts of the testis and epididymis, and to the beginning of the vas deferens, and thence must keep those parts invariably in their situation with respect to one another: and therefore in proportion as the testis descends, the vas deferens must ascend from the lower end of the testis; and it must, from the passage through the abdominal muscles, down to the testis, run parallel with the spermatic vessels.

The testis, its coats, and the spermatic chord, are so often concerned in some of the most important diseases and operations of surgery, particularly in the bubonocoele and hydrocoele, that their structure has been examined and described by the surgeons, as well as by the anatomists, of every age. Yet the descriptions of the clearest and best writers upon the subject differ so much from one another, and many of them differ so much from what is obvious and demonstrable by dissection, as to render it difficult to account for such a variety of opinions. The very different state of the parts in the quadruped, and in the human body, no doubt, must have occasioned error and confusion among the writers of more ancient times, when the parts of the human body were described from dissections and observations made principally upon brutes: and the structure of parts, which are peculiar to the foetus, having been imperfectly understood, we may suppose, has likewise contributed to cause perplexity and contradiction among authors.

Baron Haller, in his *Opuscula Pathologica*, has observed, that in infants the intestine sometimes falls down into the scrotum after the testis, or along with it, and occasions what he calls, the hernia congenita. In such a case, the hernial sac is formed before the intestine falls down, as that ingenious anatomist has observed. There are, besides, two circumstances peculiar to a rupture of this kind, the intestine being always in immediate contact

with the testis, and there being no tunica vaginalis propria testis. The structure of the parts, in a foetus, explains, in the most satisfactory manner, both these circumstances, however extraordinary they must appear to a man who has only been accustomed to view the parts in subjects of a more advanced age; and, indeed, it is so clear that it needs no illustration. It should be observed, however, that the hernia congenita may happen, not only by the intestine falling down to the testis before the aperture of the sac be shut up, but perhaps afterwards: for when the sac has been but recently closed, it seems possible enough that violence may open it again.

It must likewise be obvious to every anatomist, who examines the state of the testis in children of different ages, that the mouth and neck only of the sac close up, and that the lower part of the sac remains loose around the testis, and makes the tunica vaginalis propria. Whence it is plain, that this tunic was originally a part of the elongated peritonæum; and, as it is undoubtedly the seat of the true hydrocele, it is also plain that the hernia congenita and the true hydrocele cannot exist together in the same side of the scrotum. For when there is a hernia congenita, there is no other cavity than that of the hernial sac; and that cavity communicates with the general cavity of the abdomen.

The observations, contained in the two last paragraphs, occurred to my brother upon reading Baron Haller's *Opuscula Pathologica*, and gave rise to my inquiries upon this subject.

Having explained the situation of the testicles in the foetus, and their descent, with the circumstances attending it, I shall next consider the cases in which the change takes place, in one or both testicles, later than the usual or natural time. And having remarked the consequences of this descent, at so late a period, I shall take notice of those instances in which the testicles never pass out of the abdomen.

I have said, that the early descent of the testicles, and closing of the mouth of the sac, by usually happening before birth, prevent likewise the descent of any part of the abdominal viscera; but when the testicles remain in their first situation beyond this period, these advantages are lost; a part of the intestines or epiploon being, under these circumstances, liable to descend along with them.

The first or natural process, in some instances, not having been begun, or having been interrupted before birth, it becomes afterwards very uncertain when the descent will be completed: yet I think the completion most frequently happens between the years of two and ten, while the person is young and growing, being seldom delayed beyond the age of puberty.

It is not easy to ascertain the cause of this failure in the descent of the testicle; but I am inclined to suspect that the fault originates in the testicles themselves: this however is certain, that the testicle which has completed its descent, is the largest, which is more evident in the quadruped than in the human subject; as in these we can have an opportunity of examining the parts when we please, and can determine how small, in comparison with the other, that testicle is which has exceeded the usual time of coming down: it never descends so low as the other.

The descent of that testicle is very slow, which is not completed before birth, often requiring years for that purpose; and it sometimes never reaches the scrotum, especially the lower part of it. There is oftner I believe an inequality in the situation of the two testicles, than is commonly imagined; being seldom equally low in the scrotum: and I am of opinion that the lowest is the most vigorous, having taken the lead readily, and come to its place at once. The part where it meets with the greatest difficulty in its descent is, in the division of the tendon of the external oblique muscle called, the ring.

How far an erect position of body, the action of the abdominal muscles, and the effect produced upon the contents of the abdomen in breathing, may contribute mechanically to the descent of the testicles when the natural operations of the animal œconomy have failed, I will not pretend to decide; but when we see these combined actions producing an unnatural descent of a portion of intestine, we may conceive that they are likewise capable of contributing to the descent of the testicle.

When the testicle has remained in the cavity of the abdomen beyond the usual time, it is impossible to say whether the disposition for closing up the passage, after it has passed out, is in any degree lost or not; but when it comes down after birth, we can easily suppose a portion of intestine or epiploon is more likely to descend and prevent the closing of

the mouth of the sac, than before the child was born when certain actions had not taken place. We should therefore watch this descent of the testicle, and endeavour, by art, to procure that union which the natural powers are either not disposed to perform, or are prevented from completing by the descent of other parts: but art should not be used too soon, nor till the testicle has got a little way below the ring. As this progress is very slow, especially when the testicle is creeping through the ring, a doubt often arises, whether it is better entirely to prevent its passage, or to assist it by exercise or other means; and it would certainly be the best practice to assist it, if that could be done effectually and safely. When it has got upon the outside of the tendon it can in general be easily pushed up again into the abdomen; and in these two situations it will sometimes play backwards and forwards, for several years, without ever coming low enough to allow of the use of artificial means to hinder its descent, or to prevent a rupture. In this case it becomes difficult to determine what should be done; but, from what I have seen, I should be inclined to wait the descent, giving it every assistance in my power. Indeed, in all cases, I would advise waiting with patience; for in most of those which I have seen, years have elapsed from the first appearance of the testicle under the ring of the abdominal muscle before it has reached that situation in which we may safely apply a truss. I never have perceived that any inconvenience has arisen from waiting; and the danger, if there is any, may be in some degree avoided. I have always recommended moderate, not violent exercise.

When the testicle has got some way below the ring, then the case is to be treated as an inguinal hernia, and a truss applied upon the ring; taking care that the testicle is not injured by it: but as this generally happens at too early a period for the patients themselves to be capable of attending to it, the surgeon who is employed should be very attentive, and those in whose immediate care they are, particularly watchful, that no inconvenience is produced by the truss. I have, however, known a rupture happen in a man thirty years old, where the testicle had not even got into the ring. In such a case, I think a truss should be immediately ap-

plied. For, if it is thought advisable to prevent the testicle from coming down, a truss is equally adapted for that purpose, as for hindering the descent of an intestine where there is an hernial sac.

It sometimes happens, that one of the testicles remains in the cavity of the abdomen through life, never acquiring the disposition to change its situation; therefore the person naturally concludes that he has only one testicle; and it can only be known that he had two by an examination of these parts after death: it is, however, possible that in some instances one may be wanting; but, if we are to reason from analogy, we must suppose this to be a very rare case; for it is a very common circumstance, that many quadrupeds have only one testicle in the scrotum; and in such as are killed for food, and from that circumstance come more particularly under observation, if this peculiarity has been noticed, we in general find the other testicle in the cavity of the abdomen; though in some instances they are both found lying in that cavity.

When one or both testicles remain through life in the belly, I believe that they are exceedingly imperfect, and, probably, incapable of performing their natural functions; and that this imperfection prevents the disposition for descent from taking place. That they are more defective than even those which are late in passing to the scrotum, is to be inferred from what is very evident in quadrupeds; the testicle, that has reached the scrotum, being in them considerably larger than the one which remains in the abdomen. It is probable, that this peculiarity is a step towards the hermaphrodite; the testicle being seldom well formed. I have only seen one case, in the human, where both testicles continued in the abdomen; this proved an exception to the above observation, since we are led to conclude, that they were perfectly formed, as the person had all the powers and passions of a man. In such cases nothing is to be done by art; as it is not possible to give the testicles the stimulus of perfection, which I believe is necessary to make them assume the disposition requisite for their descent; and the ring of the external oblique muscle is perhaps less liable, in such instances, to allow a portion of intestine to push down, than where the testicles have passed through it; and such persons may, probably, be more secure from accidents of this kind than if they had been more perfectly formed.

The testicle in changing its situation, does not always preserve a proper course towards the scrotum; there being instances of its taking another direction, and descending into the perinæum. How this is brought about, is difficult to say; it may possibly be occasioned by something unusual in the construction of the scrotum; or, more probably, by a peculiarity in that of the perinæum itself. For it is not easy to imagine how the testicle could make its way to the parts about the perinæum, if these were in a perfectly natural state.

The first instance of this kind, that occurred to me, was the child of a shopkeeper in Oxford Street, which I visited in company with Dr. Garthshore, about the year 1775; but what became of the patient afterwards, I do not know. I have lately been consulted, in a similar case, by Mr. Hunt, a surgeon, at Burford in Oxfordshire, whose apprehensions of what may be the consequences of a testicle remaining in the perinæum, appear to be well founded. The most effectual method of obviating these will, probably, be to support the testicle in a situation near the groin, by the application of a bandage that may hinder its descent into the perinæum; by which the parts may be in time so consolidated, as to retain it by the side of the scrotum.

“ Dear Sir,

I take the liberty of writing to you, in consequence of having met with a *lufus naturæ* of a peculiar kind, in the son of a man in this neighbourhood.

The boy is about twelve months old; his right testicle is situated about an inch below the termination of the scrotum, and half an inch on the right side of the centre of the *rapha perinæi*, where a kind of pouch is formed of the common integuments, without the least rugous or scrotal appearance on its surface. It is perfectly detached from the scrotum; nor can the testis or spermatic process be at any time felt in any part of the scrotum, though I can readily make the testis pass from its situation quite up into the groin; but immediately upon removing my hand, the testis falls down into its pouch; and I can trace the spermatic

chord from the body of the testes up to the ring, running about a fourth of an inch on the right side of the scrotum. The scrotum, on each side, appears perfectly formed; and the left testis is in situ naturali. Now, Sir, as I conceive this peculiar conformation may be attended with great inconvenience to the child when he comes to ride on horse-back, and on many other occasions, I beg leave to request your opinion upon it, with respect to what ought to be done to prevent accidents which must, if left in its present situation, often occur.

Burford,
Oxfordshire.

(Signed) Thomas Hunt."

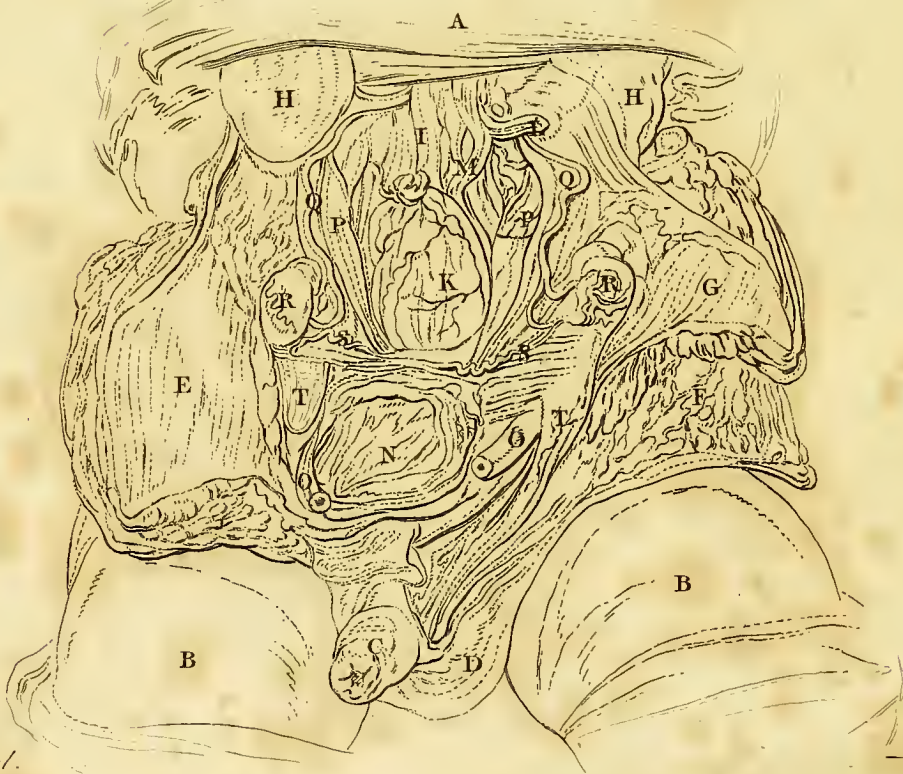
To illustrate the descriptions which I have given, I have annexed three figures that were carefully taken from Nature.



Fig. I.



Out-lines
of
Fig. I.



J. A. Nicolsky del.

P. C. Janet sculp.

P L A T E I.

THE first figure represents the testes within the abdomen, in an abortive fœtus of about six months. All the intestines, except the rectum, are removed; and the peritonæum, in most places is left upon the surfaces which it covers, so that the parts have not that sharpness and distinct appearance which might have been given to them by dissection. The outline is placed below, on which the letters of reference are marked; and the eye should be carried from the description to the letters of reference, and from them immediately to the corresponding part in the figure.

- A° The upper part of the object, covered with a cloth.
- BB The thighs.
- C The penis.
- D The scrotum.
- E The flap of the integuments, abdominal muscles, and peritonæum, of the right side, turned back over the os ilium, to bring the testis into view.
- F The flap of the skin, and cellular membrane of the left side, disposed in the same manner.
- G The flap of the abdominal muscles, and of the peritonæum of the left side, turned back over the spine of the os ilium. The lower part of this flap is cut away, in order to show the ligament of the testis passing down through the ring into the scrotum.
- HH The lower part of each kidney.
- I The projection formed by the lower vertebræ lumborum, and by the bifurcation of the aorta and vena cava.
- K The rectum filled with meconium, and tied at its upper part where the colon was cut away.
- L That branch of the inferior mesenteric artery which was going to the colon.

- M The lower branch of the same artery, which went down into the pelvis behind the rectum.
- N The posterior surface of the cavity of the bladder; the anterior part, which is higher than the ossa pubis in so young a fœtus, being cut away.
- OO The hypogastric, or umbilical arteries, cut through, where they were turning up by the sides of the bladder in their way to the navel.
- PP The ureter of each side passing down before the psoas muscle and iliac vessels, in its course to the lower part of the bladder.
- QQ The spermatic arteries running a little serpentine.
- RR The testes situated before the psoæ muscles, a little higher than the inguina. In this figure the interior edge of the testis is turned a little outwards, to show the spermatic vessels coming forwards to the posterior edge of the testis, in the duplicature of the peritonæum; which duplicature connects the testis, incloses its vessels, and gives it an external smooth coat, much after the same manner as the duplicature of the mesentery connects the intestine, conveys it vessels, and gives it a polished covering.
- The beginning of the epididymis is seen at the upper end of the testis, from which it runs down on the outside (and therefore in this view behind the body) of the testis.
- SS The vas deferens of each side, passing across, in a serpentine course, from the extremity of the epididymis at the outside of the lower end of the testis, and then before the lower part of the ureter, in its way to the vesicula feminalis.
- TT What I have called the gubernacula, or ligaments of the testes in a fœtus. On the left side this ligament is entire, and exposed in its whole length, the rings, skin of the groin, and scrotum, being removed, so that it is seen going down from the lower end of the testis into the scrotum; but on the right side, its upper and forepart is cut away, that the continuity of the epididymis and vas deferens may be seen; and no more of the ligament is exhibited than what is situated within the cavity of the abdomen.

N. B. The lower part of the ligament, as it is seen in the right side of this figure, lies so loose in the passage through the muscles, and is there so loosely covered by the peritonæum, that, when the testis is pulled up, more of the ligament is seen within the cavity of the abdomen, and then the peritonæum is made tight and smooth at that place; but, on the contrary, when the scrotum is pulled downwards, the lower part of the ligament is dragged some way down through the passage in the muscles, and the loose peritonæum is carried along with it; so that then there is a small elongation of that membrane, with an orifice from the cavity of the belly, like the mouth of a small hernial sac, on the forepart of the ligament.

Cut-lines of Fig. II.



Fig. II.



J. V. Riemsdyk del.

P. C. Lanot sculp.

P L A T E II.

THE second figure represents nearly the same parts in a fœtus, somewhat older, in order to show the state of the testes when they have recently descended from the abdomen into the scrotum. The small intestines are removed, and the large intestines are left in their natural situation, not now obstructing the view of the testes. On the left side the integuments only are removed, which shows the chord passing out through the ring, with the testicle in the vaginal coat. On the right, the ring is cut through, and the whole vaginal coat is slit open, exposing the testicle and chord.

AA The liver, in outlines.

BB The thighs, unfinished.

C The penis.

D The middle part of the scrotum ; on each side of which the forepart of the scrotum is cut away, that the testes may be seen.

EE The two flaps of the skin, and of the cellular membrane, dissected off from the lower part of the abdomen, and turned down upon the thighs.

F The intestinum cœcum.

GG The appendicula cœci vermiformis.

H The arch of the colon.

I The turn of the colon under the spleen.

K The colon passing down on the outside of the left kidney.

L The last turn of the colon, commonly called its sigmoide flexure, which in adults is seated quite in the cavity of the pelvis.

M The beginning of the rectum.

N Part of the abdominal muscles of the right side, with the smooth investing peritonæum, turned out over the spine of the os ilium.

OO The lower part of the obliquus externus muscle of the left side.

- P The lower part of the rectus muscle of the right side, turned outwards, and towards the left side, so that the epigastric artery is seen going to the inside of that muscle.
- Q The forepart of the bladder.
- R The urachus, (as it is called.)
- S The crural vessels coming into the thigh from behind the ligamentum Fallopii.
- T The external appearance of the spermatic rope of the left side.
- U The external appearance of the testis, when its tunica vaginalis, or process of the peritonæum, is a little distended with air or water poured into it from the cavity of the abdomen.
- V The right testis, brought fully into view by laying open the process of the peritonæum in its whole length.
- W The head of the epididymis, of the same side.
- XX The spermatic vessels.
- Y The vas deferens.
- Z The ureter.
- & The remains of the gubernaculum, or ligament, which bound and conducted the testis to the scrotum.

N. B. It is evident, that part of the peritonæum, which in this figure is carried down in the form of an hernial sac to a little below the testis, lies before the testis, epididymis, spermatic vessels, and vas deferens, and that it covers those parts in the same manner as it covers the abdominal viscera, viz. The posterior part of the sac (supposing the sac to be cut lengthways into two halves) is united with them, and gives them a smooth surface; while the anterior half of the sac lies loose before them, and may be removed to some distance from them, as when the sac is distended with water.

Fig. III.



Out-lines of Fig. III.



P L A T E III.

THE third figure represents the testes, &c. in the same subject; all the parts above the ossa iliûm being removed, and the abdominal muscles of the left side turned down, to show the opening of the sac into, or from the abdomen; the bladder being likewise turned downwards, to show the vasa deferentia winding round behind it.

- AA The thighs, unfinished.
- B The penis.
- C The middle part of the scrotum, its lateral parts being removed, to show the testes.
- DD The skin and cellular membrane of the abdomen turned down over the thighs.
- EE Part of the abdominal muscles and peritonæum turned down at each groin.
- FF The peritonæum covering the iliacus internus muscle of each side.
- G The intestinum rectum filled with meconium.
- H The bladder with the umbilical artery on each side of it, turned a little forwards over the symphysis of the pubes.
- II The ureters passing over the iliac vessels to the pelvis.
- K The right testis exposed, as in Fig. 2. V. W. XX. Y.
- L The left testis inclosed in the process of the peritonæum. See Fig. 2. U.
- M The spermatic vessels of the left side, seen through the peritonæum which covers them, in their descent through the abdominal muscles at the groin.
- N The left vas deferens seen through the peritonæum, in its passage from the mouth of the sac to the posterior part of the bladder.
- O The mouth, or aperture of the process of the peritonæum, whereby its mouth or cavity communicates with the general cavity of the

belly. This aperture closes up, and the membrane becomes smooth at this place, when the foetus grows a little older; unless when the gut falls down after the testis, and keeps it open: in that case it makes the mouth of the hernial sac.

- P The left epigastric artery branching upon the inside of the rectus muscle, which is here turned downwards and outwards. This artery is always situated, as in this figure, on the inside of the mouth of the hernial sac, or passage of the spermatic vessels.



P L A T E IV.

A side view of the pelvis of a young ram, to show the right testicle remaining in the cavity of the abdomen, after the left had come down, but which is removed with that half of the pelvis.

The testicle which lies in the loins is flatter than common, and is only attached by one edge, which is principally by the epididymis; there is also a ligament passing from the upper part of the common attachment which binds the testicle to the posterior part of the abdominal muscles; this is analogous to the ligament that attaches the ovarium to the same part in the female quadruped.

The epididymis passes along the outer or posterior edge; and at the lower part becomes larger and pendulous, making a little twist upon itself where it becomes vas deferens.

The vas deferens is a little contorted, and passes down obliquely over the psoas muscle to the bladder.

From the lower end of the testicle there is a ridge continued along the psoas muscle through the abdominal ring, going on to the scrotum, which is most probably the gubernaculum; but it was so much covered by a hard suety fat, that I could not exactly ascertain its structure: at the lower part of this ridge, about an inch and a half from the ring, I found the termination of the cremaster, which was a tolerably large muscle; part of its fibres seemed to arise in common with the internal oblique; while the rest appeared to come from the psoas and iliacus internus behind it; the outer portion passed inwards and downwards, and spread upon the forepart of the ridge, or gubernaculum, where the greatest part of its fibres were lost, and the rest of them were continued into the backpart of it. The posterior portion got upon the inside of the ridge and was lost in the same manner as the former.

A The inside of the thigh, only having the outline drawn.

BB The inside of the abdominal muscles spread out.

- C The symphysis of the os pubis.
- D The muscles of the thigh cut through at their origin where they arise from a middle tendon.
- E The lower end of the right kidney.
- FG The iliac vessels exposed to show their situation.
- H The remains of the umbilical artery.
- I The urinary bladder.
- K The body of the right testicle, with the ramifications of the veins upon the surface.
- L The epididymis.
- M The vas deferens.
- N The vesiculæ, commonly called seminales.

OBSERVATIONS ON THE GLANDS SITUATED
BETWEEN THE RECTUM AND BLADDER,
CALLED VESICULÆ SEMINALES.

THOSE bags, in the male of some animals, which are situated between the bladder and rectum, and commonly called vesiculæ feminales, have been considered as reservoirs of the semen secreted by the testicles in the same manner as the gall-bladder is supposed to be a reservoir of the bile. Physiologists must have been led to form this opinion, from observing, that in the human subject their ducts communicate with the vasa deferentia before their termination in the urethra. This communication was supposed to allow the semen, when not immediately wanted, to pass into the bags from the vasa deferentia by a species of regurgitation. But more accurate observations respecting their structure and contents in the human subject, and on corresponding parts in other animals supposed to answer a similar purpose, joined to the circumstance of their not being found in every class, induced me to conclude that this opinion was erroneous. To throw as much light upon this subject as possible, I made a number of experiments, and availed myself of every opportunity which offered, of examining whatever could in any way elucidate the point; and, from what I have been able to collect, I think it will appear, that they cannot be considered as reservoirs of the semen.

To proceed regularly with my investigation, I shall begin by comparing the contents of these vesiculæ with the semen as it is emitted from the penis of a living man; from which comparison it will appear that, the two secretions are very different in their sensible properties of colour and smell; and although the semen which constitutes the first part of the emission is evidently different from the last, yet every part of it is unlike the mucus found in these vesiculæ.

The semen, first discharged from the living body, is of a bluish white colour, in consistence like cream, and similar to what is found in the

vafa deferentia after death; while that which follows, is somewhat like the common mucus of the nose, but less viscid. The semen becomes more fluid upon exposure to the air, particularly that first thrown out; which is the very reverse of what happens to secretions in general. The smell of the semen is maukish and unpleasant, exactly resembling that of the farina of the Spanish chestnut; and, to the taste, though at first insipid, it has so much pungency, as, after some little time, to stimulate and excite a degree of heat in the mouth. But the fluid contained in these vesiculæ, in a dead body, is of a brownish colour, and often varies in consistence in different parts of the bag, as if not well mixed. Its smell does not resemble that of the semen; neither does it become more fluid by being exposed to the air.

It may, however, be answered to this, that the contents of the vesiculæ are generally found in a putrid state, and have, by that means, undergone a change in their sensible properties. But the objection is readily obviated by comparing this fluid with that in the vafa deferentia as it comes from the testicles of the same dead body, between which there appears to be no resemblance. To be still more certain of the nature of what these vesiculæ contain, than was possible from the examination of bodies which had been dead some time, I took an opportunity of opening a man immediately after his death, who had been killed by a cannon-ball. The fluid in the vesiculæ was of a lighter colour than has usually been found in men who have been dead a considerable time; but it was not, by any means, like the semen, either in colour or smell. In another man who died instantaneously, in consequence of falling from a considerable height, whose body I inspected soon after the accident, the contents of the vesiculæ were of a lightish whey colour, having nothing of the smell of semen; and in so fluid a state as to run out on cutting into them.

I have likewise examined, with attention, a mucus which some men discharge upon straining hard while at stool, or after throwing out the last drops of urine, an action which requires a considerable exertion of the parts. This discharge is generally called a feminal weakness, and is I believe commonly supposed to be the semen*; but in all the cases of

* Vide Treatise on the Venereal Disease, Edit. 1st and 2nd. page 197.

this kind in which I have been consulted, it nearly resembled the contents of the vesiculæ in the dead body, though perhaps not quite of so deep a colour. I endeavoured, in vain, to persuade a gentleman who had this complaint, that the discharge was not seminal; till, by examining his own semen, and comparing it with that mucus, he was convinced of the difference. This gentleman had the power of emitting the semen in the same quantity as usual, immediately after the mucus had been discharged; which is a further proof that this fluid is not semen*.

In this country, eunuchs seldom come under our examination; but we have sometimes opportunities of opening the bodies of those who have, in consequence of disease or accident, lost one or both testicles: and several subjects of this kind I have inspected after death. Persons who have had one testicle taken away, will better illustrate the point in dispute, than those who have been deprived of both. For, it is to be presumed, that such men have afterwards had connection with women, and consequently had the action of emission; which must have emptied the vesiculæ of the castrated side, if these had contained semen; and, as they could not be replenished, they should have been found empty after death. We have also, in such cases, an opportunity of making comparative observations between the vesiculæ of the perfect and those of the imperfect side. In the eunuch, such emissions never can happen; for the testicles being gone, the natural and leading stimulus is lost; therefore, if in them the vesiculæ were found full after death, it might be supposed to be the semen which they had received from the testicles before castration, that had remained there from the time of the operation: but castration being, in such cases, usually performed on children, this circumstance should rather be considered as a proof that they secrete their own mucus. Yet it is probable, the vesiculæ will neither be so large nor so full in eunuchs as in the perfect man; for I am of opinion, that they are connected with generation, and that if the constitution is deprived of that power, these

* The discharge was truly supposed to be the contents of the vesiculæ; and it being imagined that these contained semen, according to this reasoning, the discharge must be seminal.

bags will not grow to the full size. But where only one testicle is removed, its loss does not in the least affect generation, therefore does not produce any change in the vesicula of that side from which the testicle is taken; because the vesicula does not depend upon the testicle for its secretion, but upon the constitution, and on the person being capable of the action of generation: therefore, as one testicle is sufficient to preserve manhood, it is of course capable of keeping up the action of both those glands.

A man who had been under my care, in St. George's Hospital, for a venereal complaint, died there, and was discovered to have lost his right testicle. From the cicatrix being hardly observable, it must have been removed some considerable time before his death; and the complaint for which he was received into the hospital is a convincing proof that he had connection with women after that period.

I inspected the body in the presence of Mr. Hodges, the house-surgeon, and several of the pupils of the hospital. Upon dissecting out and examining the contents of the pelvis, with the penis and scrotum, I found that the vas deferens of the right side was smaller and firmer in its texture than the other, especially at that end next to the abdominal rings, near to the part which had been cut through in the operation. The cellular membrane surrounding the duct, on the right side, was not so loose as on the left; neither were the vessels which ramified on the right vesiculæ so full of blood. But upon opening the vesiculæ, both appeared to be filled with the same kind of mucus, and similar to that which is found in other dead bodies; the vesicula of the right side being rather larger than that on the left. Whatever, therefore, may be the real use of these vesicula, we have a proof from this dissection, that in the human subject they do not contain the semen.

In a man, who died in St. George's Hospital, with a very large bubo-nocle, the testicle of the diseased side was discovered to have almost lost its natural texture from the pressure of the hernial sac; and upon examining the testicle with attention, there was no appearance of vas deferens till we came near the bladder, where it was almost as large as usual.

The vesicula of that side was found to be as full as the other, and to contain the same kind of mucus.

I extirpated the left testicle of a Frenchman, who was a married man, and died about a year afterwards, having been extremely ill for several months before his death. On examining the body, the vesiculæ were both found nearly full; more especially that on the left side; which might be accidental: but the vas deferens of the left side, where it lies along this bag, and where it has a similar structure with the vesiculæ, was likewise filled with the same kind of mucus; which I believe is always the case, whether the testicle has been removed or not.

A young man, a coachman, with his left testicle much diseased, had it removed, at St. George's Hospital, by Mr. Walker, in August 1785; and in February 1786, he returned again to the hospital, on account of uncommon pains all over him: for these he requested to be put into the warm bath; but as he was going from the ward, for that purpose, he dropped down and died almost immediately. The body was inspected, with a view to discover the cause of his death; and, upon examination of the vesiculæ, the bag of the left side was as full as that on the right; and the contents in both were exactly similar. In the winter 1788, another case occurred nearly resembling the above.

In dissecting a small subject, in the year 1755, for a side view of the contents of the pelvis, I found a bag on the left side, lying contiguous to the peritonæum, just on the side of the pelvis where the internal iliac vessels divide above the angle of reflection of the peritonæum at the union of the bladder and rectum. The left vas deferens was seen passing on to this bag; and what is very singular, that of the right, or opposite side, crossed the bladder, near its union with the rectum, to join it. I traced the left vas deferens down to the testicle; but on following the right through the ring of the external oblique muscle, I discovered that it terminated at once, about an inch from its passage out of the abdomen, in a blunt point, which was impervious. On examining the spermatic chord from this point to the testicle, I could not find any vas deferens; but by beginning at the testicle, and tracing the epididymis from its

origin, about half way along where it lies upon the body of the testicle, I perceived that it at first became straight, and soon after seemed to terminate in a point. The canal at this part was so large, as to allow of being filled with quicksilver; which, however, did not pass far, so that a portion of the epididymis was wanting; and likewise the vas deferens for nearly the whole length of the spermatic chord of the right side*. On the left side the vas deferens began where the epididymis commonly terminates; and there was a deficiency of nearly an inch of the extremity of the epididymis†. I then dissected the bag abovementioned, which proved to be the two vesiculæ; for, by blowing air from one vas deferens, I could only inflate half of it; and from the other vas deferens, the other half. They contained the mucus commonly found in these bags; but upon the most accurate examination I could neither discover any duct leading from them to the prostate gland, nor the remains of one‡.

It was evident, in this subject, that there was no communication between the vas deferens and epididymis; nor between these bags and the urethra. The caput gallinaginis had the common appearance; but there were no orifices to be found. The testicles were very sound, and the ducts from them to the epididymis were very manifest and contained semen**.

* Vide plate V, fig. 1.

† Vide plate V, fig. 2.

‡ Vide plate VI.

** As the semen, in consequence of this preternatural formation of parts, could not be conveyed to the urethra in the usual way, I conceived it possible that there might be another unnatural construction to make up for the deficiency in the vas deferens, and therefore examined it very carefully to see if there was no supernumerary vasa deferentia. I was led to do this more particularly, from often finding parts resembling them, where they could answer no kind of purpose. By a supernumerary vas deferens, I mean a small duct which sometimes arises from the epididymis, and passes up the spermatic chord along with the vas deferens, commonly terminating in a blind end, near to which it is sometimes a little enlarged. I never found this duct go on to the urethra; but, in some instances, have seen it accompany the vas deferens as far as the brim of the pelvis. There is no absolute proof that this is a supernumerary vas deferens; but as we find the ducts of glands in general very subject to singularities, and that there are frequently supernumerary ducts; that there are often two ureters to one kidney, sometimes distinct from beginning to end; at other times both arising from one pelvis. These ducts, arising from the epididymis, I am inclined from analogy, to believe

From these circumstances we have a presumptive proof, that the semen can be absorbed in the body of the testicle, and in the epididymis ; and that the vesiculæ secrete a mucus which they are capable of absorbing when it cannot be made use of. We may likewise infer from what has been said, that the semen is not retained in reservoirs after it is secreted, and kept there till it is used ; but that it is secreted at the time in consequence of certain affections of the mind stimulating the testicles to this action : for we find, that if lascivious ideas are excited in the mind, and the paroxysm is afterwards prevented from coming on, the testicles become painful and swelled from, we may suppose, the quantity of semen secreted, and the increased action of the vessels ; which pain and swelling is removed immediately upon the paroxysm being brought on and the semen evacuated ; but if that does not take place, the action of the vessels will still be kept up, and the pain in the testicles in general continue till the paroxysm and evacuation of the semen is brought on, to render the act complete ; without which a stop cannot be so quickly put to the action of the vessels that produce the secretion ; nor the parts be allowed so easily to resume their natural state. There is at this time no sensation of any kind felt in the seat of the vesiculæ seminales ; which shows that the action is in the testicles, and in them alone. The pain in the testicles, in consequence of being filled with semen, and of the action being incomplete, is sometimes so considerable as to make it necessary to produce an evacuation of the semen to relieve the patient.

It may be observed, in support of this opinion, that these bags are as full of mucus in bodies much emaciated, where the person has died from a lingering disease, as in those of the strong and robust, whose death

are of a nature similar to the double ureters. They resemble the vas deferens, as being continuations of some of the tubes of the epididymis ; are convoluted where they come off from it ; afterwards become a straight canal, and passing along with it for some way, they are then most commonly obliterated.

The idea of their being for the purpose of returning the superfluous semen to the circulation, must certainly be erroneous, from their being so seldom met with, and so very seldom continued further than the brim of the pelvis.

has been occasioned by violence or acute diseases ; and they are nearly as full in the old as in the young ; which, most probably, would not be the case if they contained semen. These facts, taken from the human subject, are, I think, sufficient to establish the opinion which I have laid down ; but, for the satisfaction of others, I shall give such facts and observations as have occurred in my dissection of different animals, as tend to clear up the point in question.

These vesiculæ are not similar either in shape or contents in any two genera of animals which I have dissected ; and they differ more in size, according to the bulk of the animal, than any other parts whose uses in different animals are supposed to correspond ; while the semen in most of those which I have examined, may be said to be similar.

The resemblance which obtains between these bags and the gall-bladder, in the human subject, by no means holds equally good when applied to other animals. In the horse they are like two small urinary bladders, almost loose and pendulous, with a partial coat from the peritonæum, under which there are two layers of muscular fibres ; they are thicker in their coats at the fundus than any other part ; and appear there to be glandular. Their openings into the urethra are very large ; and although they open close to the vasa deferentia, do not communicate with them. The septum between the two ducts is not continued on quite to the urethra, so that they cannot, in strict language, be said to enter that passage separately ; but there is not length of common duct sufficient to admit of regurgitation from the vasa deferentia into these bags. They are not of the same size in the gelding and in the stone-horse, being large in the last. Their contents in both are exactly similar, and nearly equal in quantity ; but in no way resembling the semen emitted by the stone-horse in the coitus, or what is found in the vas deferens after death.

In the boar, these bags are extremely large, and divided into cells of a considerable size ; or they may more properly be said to form ramifications closely connected with one another, and having a large canal or duct common to the whole. The ducts contain a whitish fluid, very unlike

what is found in the vasa deferentia of the same animal, with which they have not the least communication.

In the rat, the bags are large and flat, with serrated edges, and lie some way within the abdomen, containing a thick ash-coloured mucus, nearly of the consistence of soft cheese; very different from what is found in the vasa deferentia of the same animal, with which they do not communicate.

In the beaver, the bags are convoluted; their ducts have no communication with the vasa deferentia; but both the one and the other open on the veru montanum.

In the Guinea-pig, they are composed of long cylindrical tubes, and lie in the cavity of the belly; are smooth on their external surface, and do not communicate with the vasa deferentia. They contain a thick bluish transparent substance, which is softest near the fundus, and becomes firmer towards the openings into the urethra, where it is as solid as common cheese. From this circumstance, and what is observed in the horse, the fundus appears to be the part that secretes this substance, which is very different in colour and consistence from the contents of the vasa deferentia, and is often found in broken pieces in the urethra.

To be more certain that the substance contained in these bags was not the secretion of the testicle, I extracted one of the testicles of a Guinea-pig; and six months afterwards gave it the female. As soon as the action of copulation was over, (in which all the parts containing semen should naturally have emptied themselves) I killed the animal, and upon examination found the vesicula of the perfect side, and that of the side from which the testicle had been removed, both filled with a substance in every respect similar. It will scarcely be alledged that this substance had been contained in the bag before the extirpation of the testicle; nor could it be semen, which must have been all thrown out in the previous connection with the female.

To ascertain that the contents of the vesiculæ are not discharged into the vagina of the female, with the semen, in the act of emission, I killed a female Guinea-pig as soon as the male had left her, and examined with

attention what was contained in the vagina and uterus ; in neither could I find any of the mucus of the vesiculæ ; which, from its firmness, must have been easily detected.

In the hedge-hog, these bags are very large, being more than twice the size of the vesiculæ in the human subject.

Many animals have no such bags ; and I believe they are wanting in the greater part of that class which live chiefly upon animal food : they are, however, to be found in some of them ; and the hedge-hog is an example. There is no apparent difference in the testicles, vasa deferentia, or semen of the animals which have vesiculæ and of those which have none ; and the mode of copulation, as far as these bags can be concerned, is very similar in both.

In birds, as far as I have yet observed, there is nothing analogous to these bags ; and yet there appears to be no difference between the mode of copulation of the drake and the bull, or ram. It is very natural to suppose, that if the vesiculæ were reservoirs of semen, they would be more necessary in birds ; who have the power of repeating the act of copulation in an infinitely greater degree than quadrupeds : and indeed we find that in birds there are reservoirs, which may account for this power ; the vasa deferentia being enlarged just before they open into the rectum, probably to answer that intention. As birds have no urethra, some having merely a groove, as the drake and gander ; and many being even without a groove, as the common fowl, it was absolutely necessary there should be such a reservoir somewhere ; and the necessity of this will appear more evidently by and by.

What I have observed of the reservoir of birds, is equally applicable to amphibious animals, and to that order of fish called rays.

From the above observations I think we may fairly conclude, that these vesiculæ are not for the purpose of containing semen ; the single circumstance of their ducts being united to those of the testicles in the human subject not appearing sufficient to set aside the many facts which are contradictory to such an opinion.

Having endeavoured to show that the function of these vesiculæ has hitherto been misunderstood, the following observations will tend to prove that they are subservient to generation, though their particular use is not yet discovered; and, for the better understanding this part of the subject, I shall premise the following facts.

Animals have their natural feelings raised or increased, according to the perfection of the parts connected with such feelings. And the disposition for action is also in proportion to the state of the parts and the excitement of such feelings. But that these feelings may be duly excited, it is necessary that the animal and the parts should be healthy, in good condition, and in a certain degree of warmth suitable to that class to which the animal belongs. In the greatest part of the globe there is a difference in the warmth of the same district at different periods, constituting the seasons; and the cold in some of them is so considerable, as to prevent those feelings or dispositions in animals from taking place, and to render them, for the time, unfit for the purposes of generation*. This is owing to the testicles becoming at this season small, and being therefore unfit to give such dispositions, as is the case in very young animals. This fact is very obvious in birds, of which the sparrow may be produced as a proof. For if a cock-sparrow is killed in the winter, before the days have begun to lengthen, the testicle will be found very small†; but if that organ is examined at different times in other sparrows, as the warmth of the weather increases, and if this examination is continued to the breeding season, the difference in the size of the testicle will be very striking‡. This circumstance is not peculiar to birds, but is common, as far as I yet know, to all animals which have their seasons of copulation. In the buck we find the testicles are reduced to a very small size in the winter; and in the land-mouse, mole, &c. this diminution is still more remarkable.

* It is not required that the season for the copulation of different animals should be equally warm; for the frog copulates in very cold weather; while the snake and lizard, which are also cold, sleeping animals, do not copulate till the season is warm.

† Vide plate VII, fig. 1.

‡ Vide plate VII, fig. 5.

Animals, on the contrary, who are not in a state of Nature, have no such change take place in their testicles ; and not being much affected by seasons, are consequently always in good condition, or in a state to which other animals that are left to themselves can only attain in the warmer season. Therefore in man, who is in the state we have last described, the testicles are nearly of the same size in winter as in summer : and nearly, though not exactly, the same thing may be observed in the horse, ram, &c. these animals having their seasons in a certain degree.

The variation above-taken notice of, is not confined to the testicles ; but also extends to the parts which are connected with them. For, in those animals that have their seasons for propagation, the most distinctly marked, as the land-mouse, mole, &c. the vesiculæ are hardly discernable in the winter ; but in the spring are very large, varying in size in a manner similar to the testicle. It may, however, be alledged, that the change in these bags might naturally be supposed to take place, even admitting them to be seminal reservoirs : but what happens to the prostate gland, which has never been supposed to contain semen, will take off the force of this objection ; since in all the animals which have such a gland (and which have their season for propagation) it undergoes a similar change. In the mole, the prostate gland in winter is hardly discernable, but in the spring becomes very large and is filled with mucus.

From these observations it is reasonable to infer, that the use of the vesiculæ in the animal œconomy must, in common with many other parts, be dependent upon the testicles. For the penis, urethra, and all the parts connected with them, are so far subservient to the testicles, that I am persuaded few of them would have existed if there had been no testicles in the original construction of the body ; and these would have been so formed as merely to assist in the expulsion of the urine. To illustrate this opinion, let us observe what is the difference between these parts in the perfect male, and in a male that has been deprived of the testicles when very young, at an age in which they have had no such influence upon the animal œconomy as to affect the growth of the other

parts. In the perfect male the penis is large; the corpora cavernosa* being capable of dilatation. The corpus spongiosum is very vascular; that part of the canal which is called the bulb is considerably enlarged, forming a cavity; and the muscoli acceleratores urinæ, as they are termed, are strong and healthy. In many animals which have a long penis, the muscular fibres are continued forwards to the end of it; and in others, though not extended so far, they are very large.

On the contrary, in the castrated animal the penis is small, and not capable of much dilatation; the corpus spongiosum is less vascular; the cavity at the bulb is little larger than the canal of the urethra; and the muscles are white, small, and have a ligamentous appearance. The same observations are true, if applied to the erectores penis.

The penis of the perfect male is of a sufficient length, when erected, to reach to the further end of the vagina of the female. In the castrated animal it is much shorter; and erections having then become unnecessary, the parts which should project, often adhere to the inside of the prepuce. The erectores muscles in the perfect male are strong enough to squeeze at once the blood out of the crura into the body of the penis, so as to straiten and contract the urethra instantaneously, and the acceleratores urinæ‡ have sufficient power to throw out the semen that is gradually accumulated at the bulb for ejection.

* The cells of the corpora cavernosa are muscular, although no such appearance is to be observed in men: for the penis in erection is not at all times equally distended. The penis in a cold day is not so large in erection as in a warm one; which, probably, arises from a kind of spasm that could not act upon it if it were not muscular.

In the horse, the parts composing the cells of the penis appear evidently muscular to the eye; and, in a horse just killed, they contract upon being stimulated.

† It may not be improper to observe, that the corpus spongiosum urethæ, and glans penis, are not spongy or cellular, but made up of a plexus of veins. This structure is discernable in the human subject; but much more distinctly seen in many animals, as the horse, &c.

‡ I shall call these muscles, expulsores feminis, as I apprehend their real use to be for the expulsion of that secretion: these muscles likewise throw out those drops of urine which are collected in the bulb from the last contractions of the bladder; and they have been, from this circumstance, named acceleratores urinæ; but if a receptacle had not been necessary for the semen, those muscles had probably never existed; and the last drops of urine would have been

The prostate gland*, Cowper's glands, and the glands along the urethra, (of which the lacunæ are the excretory ducts) are in the perfect male large and pulpy, secreting a considerable quantity of slimy mucus, which is salt to the taste; it is most probably for the purpose of lubricating those parts, and is only thrown out when in vigour for copulation: while in the castrated animal these are small, flabby, tough and ligamentous, and have little secretion. From this account there appears to be an essential difference between the parts connected with generation of the perfect male, and those which remain in one that has been castrated; more especially if that operation had been performed while the animal was young.

If it is objected, that the same changes did not take place in the men from whom one testicle had been removed; it may be answered, that the operation was performed late in life: and one testicle being left, that was sufficient to carry on the necessary actions, and consequently to preserve the powers; therefore whatever parts had a connection with these powers would still have the stimulus of perfection given to them.

The different appearance of the bulb, and the muscles, would seem to point out, in the perfect male, the enlargement of the bulb to be for the purpose of a receptacle for the semen; for although I have denied the vesiculæ to be reservoirs, yet as it was necessary that the semen should be accumulated somewhere before ejection, I shall endeavour to prove, from the mode of copulation in the animals we are best acquainted with,

thrown out by the action of the bladder and urethra, as in some measure is the case in the castrated animal. That the urethra has the power of contraction, is evident upon the application of any stimulus: for I have seen the urethra refuse to allow an injection to pass on: and in that part where the injection stopped, a fulness was felt which terminated at once: this contraction is most probably in the internal membrane; it also will often refuse the passage of a bougie.

* The prostate gland is not common to all animals. It is wanting in the bull, buck, and most probably, I believe, in all ruminating animals. In this class the coats of the vesiculæ are much thicker, and more glandular, than in those who have prostate glands; it is therefore natural to suppose, that the vesiculæ answer nearly the same purposes as the prostate gland.

The prostate gland, and Cowper's glands, as well as the vesiculæ, are wanting in birds, in the amphibious animals, and in those fish which have testicles, as all of the ray kind.

that the bulb is intended for that purpose. Let us, therefore, give a short account of the different parts concerned in coition; and by observing the dependence which they have upon one another, see how this proof will come out.

The erection of the penis is produced by a stop being put to the returning blood; and this stoppage is so complete, that no mechanical pressure applied to the body of the penis, can force the blood on into the veins. This erection answers two purposes; it gives size and strength to the penis, and it renders the canal of the urethra smaller. The corpus spongiosum of the urethra, and the glans, which is only a continuation of it, are filled with blood from the same cause, but not so completely as the body of the penis; since from them it can be forced out into the veins by pressure*. This accumulation of blood in the corpus spongiosum diminishes the canal of the urethra so much, that any pressure upon one part of it will have a considerable effect upon the other; not only by lessening its capacity at the part pressed; but by forcing the blood forward, the parts beyond will be still more distended, and consequently the canal of the urethra be in that proportion diminished. The semen, in the time of copulation, in such animals as remain long in that act, is gradually squeezed along the vasa deferentia (as it is secreted) into the bulb; and when the testicles cease to secrete, the paroxysm, which is to finish the whole operation, comes on. The semen acting as a stimulus to the cavity of the bulb of the urethra, the muscles of that part of the canal are thrown into action; the fibres nearest the bladder, probably, act first, and those more forward in quick succession; the semen is projected with some force; the blood in the bulb of the urethra is by the same

* In April 1760, in the presence of Mr. Blount, I laid bare the penis of a dog, almost through its whole length; traced the two veins that came from the glans, (which, in this animal makes the largest part of the penis) and separated them from the arteries by dissection, that I might be able to compress them at pleasure without affecting the arteries. I then compressed the two veins, and found that the glans and large bulb became full and extended; but when I irritated the veins, in order to see if there was any power of contraction in them which might occasionally stop the return of the blood, no such appearance could be observed.

action squeezed forward ; but requiring a greater impulse to propel it, is rather later than the semen, on which it presses from behind ; the corpus spongiosum being full of blood, acts almost as quick as undulation, in which it is assisted by the corresponding constriction of the urethra, and the semen is hurried along with a considerable velocity.

From the facts which I have stated respecting the organs of generation, the observations which I have made, and the series of actions which I have considered as taking place in the copulation of animals, I think the following inferences may be fairly drawn.

That the bags, called vesiculæ seminales, are not seminal reservoirs, but glands secreting a peculiar mucus ; and that the bulb of the urethra, is, properly speaking, the receptacle in which the semen is accumulated previous to ejection.

Although it seems to have been proved that the vesiculæ do not contain the semen, I have not been able to ascertain their particular use ; we may, however, be allowed upon the whole to conclude that they are, together with other parts, subservient to the purposes of generation.

Fig. I.



Fig. II.



P L A T E V.

SHOWS two testicles, with the spermatic chords dissected; in the one the vas deferens, in the other a portion of the epididymis, is wanting.

Figure 1. The right testicle and spermatic chord.

AA The body of the testicle.

BB The spermatic chord, in which there is no appearance of vas deferens.

C The epididymis, where it takes its origin from the body of the testicle.

D The abrupt termination of the epididymis, it not being continued to the lower end of the testicle.

Figure 2. The left testicle.

AA The body of the testicle.

B The blood-vessels of the testicle separated from the vas deferens.

C The origin of the epididymis.

D The termination of the epididymis; to show which, the tunica vaginalis is removed.

E The origin of the vas deferens.

F The vas deferens, as it passes up towards the ring of the abdominal muscles.





P L A T E VI.

A side view of the pelvis, taken from the same subject as plate V, in which the vasa deferentia did not communicate with the vesiculæ, and the vesiculæ did not communicate with the urethra.

- A The body of the penis.
- B The symphysis of the pubis.
- C The bladder.
- D The left ureter.
- E E The rectum.
- F The anus.
- G The sphincter muscle of the anus, turned aside.
- H The levator muscle of the anus, turned down.
- I The prostate gland.
- K The Cowper's gland of the left side.
- L The peritonæum, which lined the left side of the pelvis.
- M The sacrum, where it is articulated with the os ilium.
- N The left vas deferens.
- O The vesiculæ.

H





Plate VII

 1 *January*

 2 *Middle of February*

 3 *Beginning of March*

 4 *Latter end of March*

 5 *Middle of April*

MAR 15 1922

[51]

P L A T E VII.

TO show the gradual increase, in size, of the testes of the sparrow, from the middle of winter to the beginning of the breeding season, I examined those glands in January, February, March and April; and the appearances they put on at these different periods are faithfully represented in the plate, with the date of their examination annexed to each.

If we compare their size in January, with what it is in April, it hardly appears possible that such a wonderful change could have taken place during so short a period.

ELEVEN of the following papers have been read at the Royal Society, and published in the Philosophical Transactions; but in a work of so general a nature, and of which physiological inquiries make so small a part, the few facts and observations which I have given upon such subjects may, probably, be overlooked by those who are not members of that society. That they may be more easily procured by students in medicine, and other readers, I have, by an application to the President and Council of the Royal Society, obtained leave to reprint them, in this work, as being connected with the principles and actions of the animal œconomy; and I have added such observations and remarks as have occurred to me since the time they were read before the Royal Society.

ACCOUNT OF THE FREE MARTIN.

GENERATION, from a seed, requires the concurrence of two causes to give it perfection ; the one to form the seed, the other to give it the principle of action*.

The cause, forming the seed, is called the female ; the other, the male : but those two causes, in general, make only a part of a whole animal, or are rather parts superadded to an animal. Probably these characteristics were first observed in such animals as had the female parts complete in one, and the male in the other ; therefore the terms, female and male, have been applied to the whole animal, dividing them into two distinct sexes ; and the parts, which formed the one sex or the other, were called the female, or the male parts of generation ; but, upon a more accurate knowledge of animals, and of their parts of generation, these were found in many of the inferior tribes to be united in the same animal, which, from possessing both, has got the name of hermaphrodite.

As the distinction of male or female parts is natural to most animals, as the union of them in the same animal is also natural to many, and as the separation of them is only a circumstance making no essential dif-

* It may be necessary for some of my readers to have explained to them what I mean by a seed. I do suppose that, the word seed was first applied to grain, or that which is always called seed in the vegetable ; which seed is the part of such vegetables in which the matter of the young vegetable exists, or is formed. The principle of arrangement in the farina, or male part, fitting the seed for action, being at first not known, a false analogy between the vegetable and animal was established, and the matter secreted by the testes was called the seed : but, from the knowledge of the distinct sexes in the vegetable, it is well known that, the seed is the female production in them, and that the principle of arrangement for action is from the male. The same operation and principles take place in many orders of animals, the female producing a seed in which is the matter fitted for the first arrangement of the organs of the animal, and which receives the principle of arrangement fitting them for action from the male.

ference in the structure of the parts themselves, it becomes no great effort or uncommon play in Nature, sometimes to unite them in those animals in which they are commonly separated; a circumstance we really find takes place in many animals of those order in which such an union is unnatural. From this state of the case, hermaphrodites may be divided into two kinds, the natural and unnatural.

The natural hermaphrodite belongs to the inferior and more simple genera of animals, of which there is a much greater number than of the more perfect; and as animals become more complicated, have more parts, and each part is more confined to its particular use, a separation of the two necessary powers for generation seems also to take place.

The unnatural hermaphrodite, I believe, now and then occurs in every tribe of animals having distinct sexes; but is more common in some than in others*; and is to be met with, in all its gradations, from the distinct sex to the most exact combination of male and female organs. This, I fancy, happens most rarely in the human species, never having seen an instance. I can say the same of dogs and cats, with which last, however, I am less acquainted; but in the horse, ass, sheep, and black cattle, it is very frequent.

There is one part common to both the male and female organs of generation in all animals which have the sexes distinct; in the one sex it is called the penis; in the other, the clitoris; its specific use, in both, is to continue, by its sensibility, the action excited in coition till the paroxysm alters the sensation. In the female it probably answers no other purpose; but in the male it is more complicated, to adapt it for the purpose of conducting and expelling the semen that has been secreted in consequence of the actions so excited.

Though the unnatural hermaphrodite be a mixture of both sexes, and may possess the parts peculiar to each in perfection, yet it cannot possess in perfection that part which is common to both. For as this common

* Quere: Is there ever, in the genera of animals that are natural hermaphrodites, a separation of the two parts forming distinct sexes? If there is, it may account for the distinction of sexes ever having happened.

part is different in one sex from what it is in the other, and it is impossible for one animal to have both a penis and clitoris, the common part must of course partake of both sexes, and consequently render the hermaphrodite so far incomplete; but these parts, peculiar to each sex, may be perfectly joined in the same animal, which will convey an idea of the truest hermaphrodite. Although it may not be necessary to constitute an hermaphrodite, that the parts peculiar to the one sex should be blended with those of the other, in the same way that the penis is with the clitoris; yet this sometimes takes place in parts whose use in the distinct sexes is somewhat similar, the testicle and ovarium sometimes forming one body, without the properties of either. This compounded part in those animals that have the testicle and ovarium differently situated, is generally found in the place allotted for the ovarium; but in such animals as have the testicle and ovarium in the same situation as the bird tribe, the compound of the two, when it occurs, will also be found in that common situation.

The parts of the female, appropriated for the purpose of supplying the young with nourishment, are variously placed in different animals. In the horse, black cattle, sheep, and other granivorous animals, their situation is between the hind legs; and this being also the place allotted for the testicles of the male of this tribe, and probably of all those in which the testicles come out of the cavity of the belly; in the hermaphrodite, therefore, which has both these parts, the testicles must, to a certain degree, descend into the udder, though that cannot receive them so readily as the scrotum.

The hermaphrodites, which I have seen, have always appeared externally, and, at first view, to be females, from the penis being the part principally deficient, and there being an opening behind like the bearing in the female; and as the testicles in such hermaphrodites seldom come down, the udder is left to occupy its proper place. In animals the female of which is preserved for breeding only, as sheep, goats, pigs, &c. these are generally kept, from their being supposed to be females.

Among horses such hermaphrodites are very frequent: I have seen several, but never dissected any. The most complete was one in which

the testicles had come down out of the abdomen into the place where the udder should have been, (viz. more forward than the scrotum) and though not so pendulous as the scrotum in the perfect male of such animals, had all the appearance of an udder. There were also two distinct nipples, which, although they exist in the male, have no perfect form, being blended with the sheath or prepuce, of which there was none here. The external female parts were exactly similar to those of the perfect female; but instead of a common sized clitoris, there was one about five or six inches long, which, when erect, pointed almost directly backwards.

I procured a foal-afs, very similar in external appearance to the horse, abovementioned, and killed it to examine the parts. It had two nipples, but the testicles were not come down as in the above; owing, perhaps, to the animal's being yet too young.

There was no penis passing round the pubis to the belly, as in the perfect male afs.

The external female parts were similar to those of the she-afs. Within the entrance of the vagina was placed the clitoris; but much longer than that of a true female, it measuring about five inches. The vagina was pervious a little beyond the opening of the urethra into it, and from thence up to the fundus of the uterus there was no canal. The uterus was hollow at the fundus, or had a cavity in it, and then divided into two horns, which were also pervious. Beyond the termination of the two horns were placed the ovaria as in the true female; but I could not find the Fallopian tubes. From the broad ligaments, to the edges of which the horns of the uterus and ovaria are attached, there passed towards each groin a part similar to the round ligaments in the female, which were continued into the rings of the abdominal muscles; but with this difference, that there accompanied them a process or theca of the peritonæum, similar to the tunica vaginalis communis in the male afs; and in these thecæ were found the testicles; but I could not observe any vasa deferentia passing from them.

Here then were found, in the same animal, the parts peculiar to each sex, (although very imperfect) and that part which is common to both, but different in each, was a kind of medium of that difference.

Something similar to the above I have seen in sheep, goats, &c. but I shall not at present trouble the reader with a description of hermaphrodites in general, as it is a very extensive subject, admitting of great variety, which would make them appear a production of chance; whereas the intention of this account is to point out a circumstance which takes place in the production of hermaphrodites in black cattle, that appears to be almost an established principle in their propagation; and is, perhaps, peculiar to that species of animals.

It is a fact known, and I believe almost universally understood, that when a cow brings forth two calves, and one of them a bull-calf, and the other to appearance a cow, that the cow-calf is unfit for propagation; but the bull-calf grows up into a very proper bull. Such a cow-calf is called, in this country, a FREE MARTIN; and is commonly as well known among the farmers as either cow or bull. Although it will appear, from the description of this animal, that it is an hermaphrodite, (being in no respect different from other hermaphrodites) yet I shall retain the term, free martin, to distinguish the hermaphrodite produced in this way, from those which resemble the hermaphrodite of other animals: for I know that in black cattle, such a deviation may be produced without the circumstance of twins; and even when there are twins, the one a male the other a female, they may both have the organs of generation perfectly formed. But when I speak of those which are not twins, I shall call them hermaphrodites; the only circumstance worth our notice being a singularity in the mode of production of the free martin, and its being, as far as I yet know, peculiar to black cattle.

This calf has all the external marks of a cow-calf, similar to what was mentioned in the unnatural hermaphrodite, viz. the teats and the external female parts, called by farmers, the bearing; and when they are preserved, by those who know the above fact, it is not for propagation, but for all the purposes of an ox or spayed heifer, viz. to yoke with the oxen, and to fatten for the table*.

* I need hardly observe here, that if a cow has twins, and they are both bull-calves, that they are in every respect perfect bulls; or if they are both cow-calves, they are perfect cows.

It is known, that they do not breed; they do not show the least inclination for the bull; nor does the bull ever take the least notice of them*. They very much resemble, in form, the ox or spayed heifer, being considerably larger than either the bull or the cow, having the horns very similar to the horns of an ox.

The bellow of the free martin is similar to that of an ox, having more resemblance to that of the cow than of the bull. Free martins are very much disposed to grow fat with good food. The flesh, like that of the ox or spayed heifer, is generally much finer in the fibre than either the bull or cow; is supposed to exceed that of the ox and heifer in delicacy of flavour; and bears a higher price at market.

However, it seems that this is not universal; for I was lately informed by Charles Palmer, Esq. of Luckley in Berkshire, that a free martin having been killed in his neighbourhood, from the general idea of its being better meat than common, every neighbour bespoke a piece, which turned out nearly as bad as bull-beef; worse at least than that of a cow. It is probable that circumstance might arise from this animal having more the properties of a bull than the cow; as we shall see hereafter, that they are sometimes more the one than the other†.

Although what I have advanced with respect to the production of free martins be in general true, yet by the assistance of Benjamin Way, Esq. of Denham, near Uxbridge, who knew my anxiety to ascertain this point, I was lately furnished with an instance which proves that it does not invariably hold good.

One of his cows having produced twins, which were to appearance male and female; upon a supposition that the cow-calf was a free martin,

* Vide Leslie on Husbandry, p. 98, 99.

† The Romans called the bull, *taurus*; they, however, talked of *tauræ* in the feminine gender. And Stephen observes, that it was thought the Romans meant by *tauræ*, barren cows, and called them by this name because they did not conceive. He also quotes a passage from Columella, lib. vi. cap. 22. "and like the *tauræ*, which occupy the place of fertile cows, should be rejected, or sent away." He likewise quotes Varro, *De re Rustica*, lib. ii. cap. 5. "The cow which is barren, is called *taura*." From which we may reasonably conjecture that the Romans had not the idea of the circumstances of their production.

he obligingly offered either to give it me, or to keep it till it grew up, that we might determine the fact : as I conceived it to be a free martin, and was to have the liberty of examining it after death, I desired that he would keep it ; but, unfortunately, it died at about a month old. Upon examining the organs of generation, they appeared to be those of the female, and perfectly formed ; but to make this more certain, I procured those of a common cow-calf, and comparing them together, found them exactly alike. This made us regret that the animal had not lived to an age that might have determined if it was capable of breeding ; for the construction of the parts being to appearance perfect, is not sufficient of itself to stamp it a true or perfect female ; as I can suppose that, the parts being perfectly formed, but without the power of propagation, may constitute the most simple kind of hermaphrodite. It is, however, most probable, that this was a perfect female, which is an exception to the common rule ; and I have been informed, there are instances of such twins breeding. If there are such deviations, as of twins being perfect male and female, why should there not be, on the other hand, an hermaphrodite produced singly, as in other animals ? I had the examination of one which seemed, upon the strictest inquiry, to have been a single calf ; and I am the more inclined to think this true, from having found a number of hermaphrodites among black cattle, without the circumstances of their birth being ascertained.

Hermaphrodites are to be met with in sheep ; but, from the account given of them, I should suppose that they are not free martins. I have seen several which were supposed to be hermaphrodites, but which were imperfect males, having the penis terminating in the perinaeum ; the orifice of which appeared like the bearing in the female. Such are not naturally stimulated to put themselves in the position of the female when they void their urine, so that when it passes, the surrounding parts are wetted by it, and being covered with wool, and retaining the urine, keeps them continually moist, and gives the animal a strong smell. They are mentioned as both male and female.

I believe it had never been even conjectured, notwithstanding all these peculiarities, what was the true nature of the free martin ; and from

the singularity of the animal, and the account of its production, I was almost tempted to suppose the whole a vulgar error. Yet by the universality of the testimony in its favour, it appearing to have some foundation, I eagerly sought for an opportunity to see and examine them. I have succeeded in this inquiry, and have seen several; the first of which was one belonging to John Arbuthnot, Esq. of Mitcham, and was calved in his own farm. He was so obliging as to allow me to satisfy myself, first by permitting a drawing to be made of the animal while alive, which was executed by Mr. Gilpin; and afterwards to examine the parts when the animal died. At the time the drawing was made of Mr. Arbuthnot's free martin, John Wells, Esq. of Bickley Farm, near Bromley in Kent, was present, and informed us, that a cow of his had calved two calves, one of which was a bull-calf and the other a cow-calf. I desired Mr. Arbuthnot to request Mr. Wells to keep them, or let me buy them of him; but, from his great desire of natural knowledge, he very readily consented to preserve both till the bull shewed all the signs of a good bull; and when the free martin was killed he allowed me to inspect the parts.

Of all the specimens which I have dissected, I shall only give the descriptions of the three which point out most distinctly the complete free martin, with the gradations towards the male and female.

THE DESCRIPTION OF THE THREE FREE MARTINS.

MR. WRIGHT'S FREE MARTIN, FIVE YEARS OLD.

This animal had more the appearance and general character of the ox, or spayed heifer, than of either the bull or cow. The vagina terminated in a blind end, a little way beyond the opening of the urethra, from which the vagina and uterus were impervious. The uterus, at its extreme part, divided into two horns. At the termination of the horns were placed the testicles instead of the ovaria, as is the case in the female. The reasons why I call these bodies, testicles, are the following. First,

they were above twenty times larger than the ovaria of the cow, and nearly the size of the testicles of the bull; or rather of those of the ridgil, the bull whose testicles never come down. Secondly, the spermatic arteries were similar to those of the bull, especially of the ridgil. Thirdly, the cremaster muscle passed up from the rings of the abdominal muscles to the testicles, as it does in the ridgil*.

There were the two bags placed behind, between the bladder and the uterus. Their ducts opened into the vagina, a very little way beyond the opening of the urethra; but there was nothing similar to the vasa deferentia.

As the external parts had more of the cow than the bull, the clitoris, which may be reckoned an external part, was also similar to that of the cow; not at all in a middle state between the penis of the bull and the clitoris of the cow, as I have described in the hermaphrodite horse. There were four teats; the glandular part of the udder was but small.

This animal cannot be said to have been a mixture of all the parts of both sexes, for the clitoris had nothing similar to the penis in the male, and it was deficient in the female parts, by having nothing similar to the ovaria; neither had the uterus a cavity.

MR. ARBUTHNOT'S FREE MARTIN†.

The external parts were rather smaller than in the cow. The vagina passed on, as in the cow, to the opening of the urethra, and then it began to contract into a small canal, which passed on to the division of

* Although I call these bodies testicles, for the reason given, yet they were only imitations of them; for when cut into, they had nothing of the structure of the testicle: not being similar to any thing in Nature, they had more the appearance of disease. From the seeming imperfection of the animal itself, it was not to be supposed that they should be testicles, for then the animal should have partaken of the bull, which it certainly did not.

† This animal was seven years old; had been often yoked with the oxen; at other times went with the cows and bull; but never shewed any desires for either the one or the other.

the uterus into the two horns, each horn passing along the edge of the broad ligament laterally towards the ovaria.

At the termination of these horns were placed both the ovaria and the testicles; they were nearly of the same size, and about as large as a small nutmeg.

To the ovaria I could not find any Fallopian tube.

To the testicles were vasa deferentia, but they were imperfect. The left one did not reach near to the testicle; the right only came close to it, but did not terminate in a body, called the epididymis. They were both pervious, and opened into the vagina near the opening of the urethra.

On the posterior surface of the bladder, or between the uterus and bladder, were the two bags, called vesiculæ seminales in the male, but much smaller than what they are in the bull; the ducts opened along with the vasa deferentia. This was more entitled to the name of hermaphrodite than the first or third; for it had a mixture of all the parts, though all were imperfect.

MR. WELLS'S FREE MARTIN.

This animal was between three and four years old when killed; and had never been observed to show any signs of desire for the male, although it went constantly with one; and looked more like an heifer than free martins usually do.

The teats and udder were small compared with those of an heifer; but rather larger than in either of the former examples; the beginning of the vagina was similar to that of the cow, but soon terminated a little beyond the opening of the urethra, as in the first described. The vagina and uterus, to external appearance, were continued, although not pervious; and the uterine part divided into two horns, at the end of which were the ovaria.

I could not observe in this animal any other body which I could suppose to be the testicle.

There was on the side of the uterus an interrupted vas deferens broken off in several places.

Behind the bladder, or between that and the vagina, were the bags called *vesiculæ feminales*; between which were the terminations of the two *vasa deferentia*.

The ducts of the bags, and the *vasa deferentia*, opened as in the last instance.

This could not be called an exact mixture of all the parts of both sexes, for here was no appearance of testicles.

The female parts were imperfect, and there was the addition of part of the *vasa deferentia*, and the bags called *vesiculæ feminales*.

This circumstance of having no testicles, perhaps, was the reason why it had more the external appearance of an heifer than what they commonly have, and more than either of the two former.



S. Goussier del.



W. H. Mason sculp.

P L A T E VIII.

THIS plate is a representation of Mr. Wright's free martin, taken from a drawing of the living animal, by Mr. Gilpin. It shows the external form of that animal, which is neither like the bull nor cow ; but resembling the ox or spayed heifer.





P L A T E IX.

THIS plate represents the organs of generation of Mr. Wright's free martin, which are more the parts of a bull than those of a cow; and the animal, while alive, had a good deal the character and look of an ox.

- A The peak of the labia.
 BB The labia.
 C The glans clitoridis.
 D D D D The inner surface of the common vagina.
 E E The orifices of the ducts of two glands.
 F Meatus urinarius.
 G G The inner surface of the true vagina, terminating in a blind end at H.
 H The termination of the vagina in a blind end.
 I I I I What may be called uterus, but impervious.
 K K What may be called horns of the uterus.
 L L The testicles.
 M M The spermatic vessels.
 N N The cremaster muscles.
 O O The vesiculæ feminales.
 P P The ducts of the vesiculæ feminales seen through the vagina.
 Q Points to the ducts of ditto, into which are introduced bristles.





P L A T E X.

THIS plate shows the organs of generation of Mr. Arbuthnot's free martin, which are almost a complete mixture of the male and female: with this structure of the parts, the external appearances and general character of the animal corresponded, it being neither that of the bull nor cow, but a mixt character.

- A The peak of the labia.
 B B The two labia.
 C The glans clitoridis.
 D D The inside of the common vagina.
 E E Orifices of the ducts of two glands.
 F The orifice of the meatus urinarius.
 G G The true vagina.
 H H Either the contracted vagina, or what may be called uterus.
 I I The horns of ditto, only pervious a little way.
 K The right ovarium deprived of its capsula.
 L The left ovarium inclosed in its capsula.
 M A bristle introduced through the orifice into the capsula.
 N The right testicle.
 O O O O The right vas deferens.
 P P The vesiculæ feminales.
 Q Q The ducts of vesiculæ feminales seen through the vagina.
 R Points to the openings of the vasa deferentia and vesiculæ feminales.



P L A T E XI.

THIS plate exhibits a front view of the organs of generation of Mr. Wells's free martin, which are more the parts of a cow than of a bull; and the animal itself resembled a young heifer very much in its appearance.

- A The clitoris.
 B B The crura clitoridis.
 C The urethra.
 D The bladder.
 E The body of the uterus beyond the bladder, which is impervious.
 F F The horns of ditto, which are also impervious.
 G The left ovarium deprived of its capsula.
 H The capsula inclosing its ovarium.
 I I I I Interrupted parts of the vasa deferentia.
 K K The spermatic vessels.
 L The gubernaculum of the right side.
 M The beginning of the tunica vaginalis communis, into which is introduced a bristle to show that it is hollow.
 N N Vessels going behind the bladder.
 O O The two ureters.
 P P The vesiculæ feminales.

AN ACCOUNT OF AN EXTRAORDINARY PHEASANT.

EVERY deviation from that original form and structure which gives the distinguishing character to the productions of Nature, may not improperly be called monstrous. According to this acceptation of the term, the variety of monsters will be almost infinite; and, as far as my knowledge has extended, there is not a species of animals, nay, there is not a single part of an animal body, which is not subject to an extraordinary formation. Neither does this appear to be a matter of mere chance; for it may be observed, that every species has a disposition to deviate from Nature in a manner peculiar to itself. It is likewise worthy of remark, that each species of animals is disposed to have nearly the same sort of defects, and to have certain supernumerary parts of the same kind: yet every part is not alike disposed to take on a great variety of forms; but each part of each species seems to have its monstrous form originally impressed upon it.

It is well known, that many orders of animals have the two parts designed for the purpose of generation different in individuals of the same species, by which they are distinguished into male and female; but this is not the only mark of distinction; in the greatest part, the male being distinguished from the female by various other marks. The varieties which are found in the parts of generation themselves, I shall call the first, or principal marks, being originally formed in them and belonging equally to both sexes; all others depending upon these I shall call secondary, as not taking place till the first are becoming of use, and being principally, although not entirely, in the male.

One of the most general marks is, the superior strength of make in the male; and another circumstance, perhaps equally so, is this strength being directed to one part more than another, which part is that most

immediately employed in fighting. This difference in external form is more particularly remarkable in the animals whose females are of a peaceable nature ; as are the greatest number of those which feed on vegetables, and the marks to discriminate the sexes are in them very numerous. The males of almost every class of animals are probably disposed to fight, being, as I have observed, stronger than the females ; and in many of these there are parts destined solely for that purpose, as the spurs in the cock, and the horns in the bull ; and on that account, the strength of the bull lies principally in his neck, that of the cock in his limbs.

In carnivorous animals, whose prey is often of a kind which requires strength to kill, we do not find such a difference in the form of the male and female ; very little being discernible in the dog and bitch ; in the he or she cat ; or in the cock and hen of the eagle. A difference, however, is often perceivable in the whole or in some part of their external covering ; the mane of the lion distinguishing him from the lioness : and the males of such animals as neither fight nor feed on flesh, being only distinguishable from the female by some peculiarity in the covering of their bodies, as the cock and hen in many birds. The male of the human species is distinguished from the female, both by his general strength and his covering, as also by a difference of voice.

In these orders of animals whose sexes are distinct, we may not only observe the genital organs to be subject to mal-conformation, as in any other part of the animal ; but that an attempt is sometimes made to unite the two organs in the same animal body, making what may be called, an unnatural hermaphrodite. In producing the unnatural hermaphrodite, the same laws seem to operate as in the mal-conformation of other parts of animals ; it being observable, that these deviations obtain through a whole species precisely in the same manner. I have already given an account of the free martin, which exhibits a mixture of the two parts of generation in the same animal.

It is my intention, at present, to extend my inquiry on this subject no further than what relates to the resemblance which one sex bears to another, in those distinguishing properties which I term secondary ; for we find

that there is often a change of the natural properties of the female sex into those of the secondary of the male; the female, in such cases, now and then assuming the secondary peculiarities of the male. It is to be observed, that some classes are more liable than others to this change; a singular example of which is to be the subject of the following pages.

To bring the foregoing observations into one point of view, I here beg leave to remark, that in animals just born, or very young, there are no peculiarities to distinguish one sex from the other, exclusive of what relates to the organs of generation, which can only be in those who have external parts; and that towards the age of maturity, the discriminating changes beforementioned begin to appear; the male then losing that resemblance he had to the female in various secondary properties*; but that in all animals which are not of any distinct sex, called hermaphrodites, there is no such alteration taking place in their form when they arrive at that age. It is evidently the male which at this time in such respects recedes from the female; every female being at the age of maturity more like the young of the same species than the male is observed to be: and if the male is deprived of his testes when young, he retains more of the original youthful form, and therefore more resembles the female.

From hence it might be supposed, that the female character contains more truly the specific properties of the animal than the male; but the character of every animal is that which is marked by the properties common to both sexes, which are found in a natural hermaphrodite, as in a snail, or in animals of neither sex, as the castrated male or spayed female.

But where the sexes are separate, and the animals have two characters, the one cannot more than the other be called the true; as the real distinguishing marks of each particular species, as has been mentioned above, are those common to both sexes; and which are likewise in the unnatural hermaphrodite. That these properties give the distinct character of such animals is evident, for the castrated male and the spayed

* This is not common to all animals of distinct sexes; for in fishes there is no great difference; nor in many insects: nor in dogs, as has been already observed; however, it is considerable in many quadrupeds, but appears to be most so in birds.

female have both the same common properties ; and when I treated of the free martin, which is a monstrous hermaphrodite, I observed that it was more like the ox than the cow or bull ; so that the marks characteristic of the species which are found in the animal of a double sex, are imitated by depriving the individual of certain sexual parts, in consequence of which it retains only the true properties of the species.

They are curious facts in the natural history of animals, that by depriving either sex of the true parts of generation, they shall seem to approach each other in appearances, and acquire a resemblance to the unnatural hermaphrodite.

In some species of animals that have the secondary properties we have mentioned, there is a deviation from the general rules, by the perfect female, with respect to the parts of generation, assuming more or less the secondary character of the male.

This change does not appear to arise from any action produced at the first formation of the animal ; and, in this respect, is similar to what takes place in the male ; neither does it grow up with the animal as it does to a certain degree in the male ; but seems to be one of those changes which happens at a particular period, similar to many common and natural phenomena ; like to what is observed of the horns of the stag, which differ at different ages ; or to the mane of the lion, which does not grow till after his fifth year, &c.

This change has been observed in some of the bird tribe, but principally in the common pheasant ; and it has been observed by those who are conversant with this bird, when wild, that there every now and then appears a hen pheasant with the feathers of a cock : all, however, that they have described on the subject is, that this animal does not breed ; and that its spurs do not grow. Some years ago, one of these was sent to the late Dr. William Hunter, which I examined and found it to have all the parts peculiar to the female of that bird. This specimen is still preserved in Dr. Hunter's museum.

Dr. Pitcairn having received a pheasant of this kind from Sir Thomas Harris, showed it as a curiosity to Sir Joseph Banks and Dr. Solander. I

happening to be then present, was desired to examine the bird ; and the following was the result of my examination.

I found the parts of generation to be truly female : they being as perfect as in any hen pheasant that is not in the least prepared for laying eggs ; and having both the ovaria and oviducts.

As the observations hitherto made have been principally upon birds found wild, little of their history can be known ; but from what took place in a hen pheasant, in the possession of a friend of Sir Joseph Banks, it appears probable that this change of character takes place at an advanced period of the animal's life, and does not grow up with it from the beginning. This lady, who had for some time bred pheasants, and paid particular attention to them, observed that one of the hens, after having produced several broods, moulted ; when the succeeding feathers were those of a cock ; and that this animal was never afterwards impregnated. Hence it is most probable, that all the hen pheasants, found wild, having the feathers of a cock, were formerly perfect hens, but have been changed by age, or perhaps by certain constitutional circumstances.

Having bought some pheasants from a dealer in birds, among which were several hens, I perceived, the year after, that one of the hens did not lay, and that she began to change her feathers. The year following she had nearly the plumage of the cock, but less brilliant, especially on the head ; and it is more than probable that this was an old hen, nearly under circumstances similar to those before described.

Lady Tynte had a favourite pyed pea-hen which had produced chickens eight several times ; having moulted when about eleven years old, the lady and family were astonished by her displaying the feathers peculiar to the other sex, and appeared like a pyed peacock. In this process the tail, which became like that of the cock, first made its appearance after moulting ; and in the following year, having moulted again, produced similar feathers. In the third year she did the same ; and, in addition, had spurs resembling those of a cock. She never bred after this change in her plumage, and died in the following winter during the hard frost,

in the year 1775-6. This bird is now preserved in the museum of the late Sir Ashton Lever*.

From what has been related of these three birds, we may conclude, that this change is merely the effect of age, and obtains to a certain degree in every class of animals. We find something similar taking place even in the human species: for that increase of hair observable on the faces of many women in advanced life, is an approach towards the beard, which is one of the most distinguishing secondary properties of man.

Thus we see the sexes which, at an early period, had little to distinguish them from each other, acquiring about the time of puberty secondary properties, which clearly characterise the male and female; the male at this time receding from the female, and assuming the secondary properties of his sex.

The female, at a much later time of life, when the powers of propagation cease, loses many of her peculiar properties; and may be said, except from mere structure of parts, to be of no sex; even receding from the original character of the animal, and approaching, in appearance, towards the male, or perhaps more properly towards the hermaphrodite.

* It might be supposed, that this bird was really a cock which had been substituted for the hen; but the following facts put this matter beyond a doubt. First, there was no other pyed pea-fowl in the county. Secondly, the hen had nobs on her toes, which were the same after her change. Thirdly, she was as small after the change as before, therefore too small for a cock. Fourthly, she was a favourite bird, and was generally fed by the lady, and used to come for her food, which she still continued to do after the change in the feathers.

AN ACCOUNT OF THE ORGAN OF HEARING IN FISHES.

NATURAL history having ever been considered as worthy the attention of the curious philosopher, has in all ages kept pace with the other branches of knowledge ; and as both arts and sciences have of late years been cultivated to a degree, perhaps, beyond what was ever known before, we find that natural history has not been neglected. All the nations of Europe appear solicitous to encourage the study ; and in this island it has been pursued with more philosophic ardour than was ever known in any country. It has become an object of pursuit to men possessed of affluent fortune ; which they have not only dedicated to the cultivation of this science, but have even risked their health and lives in exploring unknown regions to increase the sources of information, and in settling correspondences every where, so as to bring materials into this country that might render it the school of natural history. It is no wonder then, that a spirit of inquiry is diffused through almost all ranks of men ; and that those who cannot pursue it themselves, yet chusing at least to benefit by the industry of others, are eager to be informed of what is already known.

These reflections have induced me to publish this short account of the Organ of Hearing in Fishes ; for though the existence of such an organ is now known to many, it is still a subject of dispute with others, whether they possess the sense or not.

Some time before I quitted my anatomical pursuits, in the year 1760, and went with the army to Bellisle, I had discovered this organ in fishes, and had the parts exposed and preserved in spirits. In some the canals were filled with coloured injection, which showed them to great advantage ; and in others were so prepared, as to fit them to be kept as dried

preparations*. My researches, in that and in every other part of the animal œconomy, have been continued ever since that time. I am still inclined to consider whatever is uncommon in the structure of this organ in fishes, as only a link in the chain of varieties displayed in its formation in different animals, descending from the most perfect to the most imperfect, in a regular progression†.

As in this age of investigation, a hint that such an organ existed would be sufficient to excite a spirit of conjecture or inquiry, I was aware that there would not be wanting some men, who, whether they only imagined the fact might be true, or really found it to be so, would be very ready to assume all the merit of the discovery to themselves. My attention was more strongly called to this point by hearing, in conversation, that some anatomists in France, Germany, and Italy, had discovered the organ of hearing in fishes, and intended to publish on the subject. I therefore thought that it would be only justice to myself to deliver to the Royal Society, a short account of that organ, a discovery of which I had made more than twenty years before. This account I shall reprint here, without adding any thing to what I had before written; reserving a more complete examination of this subject for a larger work, on the structure of animals, which I one day hope to have it in my power to publish.

I do not intend to give a full account of this organ in any one fish, or of the varieties in different fishes, but only of the organ in general; those therefore who may wish to pursue this branch of the animal œconomy will think it deficient perhaps in the descriptive parts. If it was a difficult task to expose this organ in fishes, I should perhaps be led to be more full in my description of it; but in fact there is nothing more easy.

* I have injected these parts in other animals, both with wax and metals; which, the bone being afterwards corroded in spirit of sea-salt, make elegant casts of these canals.

† The preparations to illustrate these facts have been, ever since, shewn in my collection, to both the curious of this country and foreigners. In shewing whatever was new, or supposed to be new, the ears of fishes were always considered by me as one important article.

It may be proper just to observe here, that the class called sepia has the organ of hearing, though somewhat differently constructed from what it is in fishes.

The organ of hearing in fishes is placed on the sides of the skull, or cavity which contains the brain; but the skull makes no part of it, as it does in the quadruped and the bird; the organ being a distinct and detached part. In some fishes, as in those of the ray kind, the organ is wholly surrounded by the parts composing the cavity of the skull; in others it is in part within the skull, or cavity which contains the brain, as in the salmon, cod, &c. the skull projecting laterally, and forming a cavity.

The organ of hearing in fishes appears to increase in dimensions with the animal, and nearly in the same proportion; which is not the case with the quadruped, &c. the organs being in them nearly as large in the growing fœtus as in the adult. Neither is its structure, by any degree, so complicated in fishes as in all those orders of animals which may be reckoned superior, such as quadrupeds, birds, and amphibious animals; but there is a regular gradation from the first of these to fishes.

It varies in different genera of fishes; but in all, it consists of three curved tubes which unite one with another; this union forms in some only one canal, as in the cod, salmon, ling, &c. and in others a tolerably large cavity, as in the ray kind. In the jack there is an oblong bag, or blind process, which is an addition to these canals, and communicates with them at their union. In the cod, &c. this union of the three tubes stands upon an oval cavity; and in the jack there are two: the additional cavities in these fishes appearing to answer the same purpose with the cavity observed in the ray or cartilaginous fishes, which is at the union of the three canals.

The whole organ is composed of a kind of cartilaginous substance, very hard or firm in some parts, and in some fishes crufted over with a thin bony lamella, to prevent it from collapsing; for as the skull does not form any part of these canals or cavities, they must be composed of a substance capable of keeping its form.

Each tube describes more than a semicircle ; resembling, in some sort, what we find in most other animals, but differing in the parts being distinct from the skull*.

Two of the semicircular canals are similar to one another, may be called a pair, and are placed perpendicularly ; the third is not so long, and in some is placed horizontally, uniting as it were the other two at their ends or terminations. In the skate, this is somewhat different, being only united to one of the perpendicular canals. The two semicircular canals, whose position is perpendicular, are united, forming one canal ; at their other extremities they have no connection with each other, but join the horizontal one, near its entrance into the common cavity. Near the union of these canals they are swelled out into round bags, and become much larger.

In the ray kind all these canals terminate in one cavity ; and in the cod, in one canal, placed upon the additional cavity or cavities, in which there is a bone or bones. In some there are two bones ; and in the jack, which has two cavities, we find in one of them two bones, and in the other one ; in the ray there is only a chalky substance†.

In some fishes the external communication, or meatus, enters at the union of the two perpendicular canals ; which is the case with all the ray kind, the external orifice being small, and placed on the upper flat surface of the head ; but it is not every genus or species of fishes that have the external opening.

The nerves of the ear pass outwards from the brain, and appear to terminate at once on the external surface of the enlarged part of the semicircular tubes above described. They do not appear to pass through these tubes so as to get on the inside, as is supposed to be the case in quadrupeds ; I should therefore very much suspect, that the lining of the tubes in the quadruped is not nerve, but a kind of internal periosteum.

* The turtle and the crocodile have a structure somewhat similar to this ; and the intention is the same, for their skulls make no part of the organ.

† This chalky substance is also found in the ears of amphibious animals.

As it is evident that fishes possess the organ of hearing, it becomes unnecessary to make or relate any experiment made with living fishes, which only tends to prove this fact; but I will mention one experiment, to show that sounds affect them much, and is one of their guards, as it is in other animals. In the year 1762, when I was in Portugal, I observed in a nobleman's garden, near Lisbon, a small fish-pond full of different kinds of fish. The bottom was level with the ground, the pond having been made by forming a bank all round, and had a shrubbery close to it. Whilst I lay on the bank, observing the fish swimming about, I desired a gentleman, who was with me, to take a loaded gun and fire it from behind the shrubs. The reason for desiring him to go behind the shrubs was, that there might not be the least reflection of light. The moment the report was made, the fish seemed to be all of one mind, for they vanished instantaneously, raising a cloud of mud from the bottom. In about five minutes afterwards they began to appear, and were seen swimming about as before.

Monf. Geoffroi, who has written on this organ, considers the ray as in the class of reptiles; and with that idea has examined their organ of hearing. He is by no means clear in his description, so that it is almost impossible to follow him; yet it is but doing him justice to allow, that he has discovered what is analogous to the three semicircular canals in other animals, together with their union into one cavity. He mentions the chalky substance contained in that cavity, and also the nerves: but it is by no means clear, that he was acquainted with the external opening which leads to these canals. He says, "The entrance of the organ of hearing (by which one would suppose he means the meatus auditorius externus) is not easily discovered"; but that which he describes does not correspond with the real situation of the external communication; we may therefore reasonably conclude, that he is describing something else. He is not more clear in his mode of reasoning on the application of the parts to produce the sense of hearing. He observes, that the organ of hearing is very imperfect in this species of animals; but supposes this to be com-

penfated, by the medium in which they live, and by which found is conveyed to them, being more denfe than that of the air, by which found is communicated to animals living on the land; and of this idea he is certainly the author. Monf. Geoffroi cannot indeed be faid to have given a perfect account of the organ of hearing in fishes, yet on the whole he fhould be confidered as a discoverer: for though he only made his obfervations on the ray, as belonging to the clafs of reptiles, yet as it may be properly confidered of the fifh kind, he has a juft claim to that credit. Had I formerly been acquainted with this author's refearches and pretentions, I fhould not have claimed that to which I had not a prior right; nor fhould I have held the difcovery of the external communication alone, an object of confequence enough to induce me to difpute the honour with Monf. Geoffroi.

In looking over the works of the different authors who have treated of the organ of hearing in fishes, I find from a paffage in Willoughby,* who publifhed prior to Monf. Geoffroi, and indeed is quoted by him, that my claim, even to the difcovery of the external opening, is not fo ftrong as I believed it to be; as he mentions an external orifice in the skate, contiguous to what he fupposes the organ of hearing in that fifh. If what he alludes to is really the external opening of the ear, it gives him a prior claim to the difcovery of that part of the organ; although from his account, he does not feem to have been acquainted with the organ itfelf: for as in defcribing the external ear of the thornback, he has evidently miftaken the nofe of it, of which he gives a tolerable full account, it is very obvious that he was ignorant of the opening into the ear.

Although profeffor Camper publifhed an account of the organ of hearing in fishes fo late as 1774, he did not feem, at that time, to have been acquainted with the external opening of the ear in the ray. After giving a defcription of the organ of hearing in the pike, he makes fome general obfervations on the fimilarity of this organ in other fishes; but excepts

* Willoughbeii *Historia Piscium*, Oxonii 1686, lib. iii. cap. viii.

† Lib. iii. cap. xiv.

the shark and ray*. This exception we might suppose alluded to the auditory canal ; but further on he explains what is meant by this exception, and does not mention the external opening in the ray ; from which we may fairly conclude that he was not acquainted with it.

* “ Il est très-vraisemblable que toutes les autres espèces de poissons, tant *malacopterygie* qu’ *acanthopterygii*, aussi-bien que les *branchiostegi* & les *chondropterygii* d’Artedi, à l’exception des *squalis* & des raies, ont l’organe de l’ouïe construit à peu près de la même façon ; je n’excepte pas l’esturgeon, quoique M. Klein, *ibid.* ait donné la description du conduit auditif, page 19, figure A, Tab. 2. b ; ce poisson étant rare parmi nous, je n’ai eu occasion de l’examiner qu’une seule fois sans avoir trouvé ce conduit.” *Memoirs Etrangers de l’Academie des Sciences*, 1774, tom. 6, page 190.

† “ Au contraire, les chiens de mer, les *galeis* de Rondelet & les poissons qu’il a décrits, *lib. XII* ; les *squalis* d’Artedi & les raies, ont bien l’organe à peu près de la même composition, mais il est enfermé dans une caisse tout effusée ou cartilagineuse, ce qui ne fait pas une différence essentielle ; ils entendent donc comme les églefins, les morues, les baudroyes & les brochets, en un mot comme tous les autres poissons non amphibies : M. Geoffroi s’est trompé en comparant leurs organes avec celui de reptiles, tels que la vipère, les lézards, &c. qui entendent le son comme les quadrupèdes, les oiseaux & les amphibies aquatiques, savoir par le moyen de l’air & d’un tambour, comme j’ai dessein de le prouver dans une autre occasion.” *Memoirs Etrangers de l’Academie des Sciences*, 1774, tom. 6, page 190.

AN ACCOUNT OF CERTAIN RECEPTACLES OF
AIR IN BIRDS, WHICH COMMUNICATE WITH
THE LUNGS AND EUSTACHIAN TUBE.

SINCE the account of these receptacles was read before the Royal Society, in the year 1774, I have, by the dissection of a number of birds, been able to make some additional observations relative to the extent of the air-cells which communicate with the lungs in animals of this class. These latter observations were not, however, made in consequence of any regular design to investigate this subject further, as to have established the principle seemed all that was necessary; unless by general observations we could hope to throw more light on the final intention of this remarkable piece of mechanism.

Before the period I have mentioned, the communication subsisting in birds, between the air-cells of the lungs and other cavities of the body, had not been clearly explained, nor even much attended to by anatomists or natural historians. It is a singularity of structure peculiar to this tribe of animals; and an account of it, cannot, I imagine, be unacceptable to the public.

It is not my present intention to enter into minute descriptions of all the particular communications of this sort discoverable in birds by dissection, but only to mention such general facts as may serve to introduce the subject into natural history, and lead to an inquiry into the purposes which this structure was intended to answer. With this view I shall endeavour to give some idea of the construction of the lungs, and of the air-receptacles in birds; occasionally remarking the circumstances in which these principally differ from what is seen in other animals.

The mechanism of the lungs in birds, which renders them fit for conveying air to different parts of the body, consists principally in certain communications.

It has been asserted that birds have no diaphragm; but this opinion must have arisen either from a want of observation, or from too confined an idea of a diaphragm; for there is a moderately strong, but thin and transparent membrane, covering the lower surface of the lungs, and adhering to them, that affords insertion to several thin muscles which arise from the inner surfaces of the ribs. The use of this part seems to be that of lessening the concavity of the lungs towards the abdomen, at the time of inspiration, and thereby assisting to dilate the air-cells; for which reason it is to be considered as answering one main purpose of a diaphragm. Besides this attachment of the lungs to the diaphragm, they are also connected to the ribs, and to the sides of the vertebrae.

Such adhesions are peculiar to this tribe of animals, and are of singular use, nay in fact are absolutely necessary in lungs like those of birds; out of which it is intended the air should find a passage into other cavities. For if the lungs were loose in the cavity of the thorax, as is the case in many other animals, the cells of the lungs could not be expanded, either by the depression of the diaphragm, or the elevation of the ribs; since the air rushing in to fill up the vacuum produced in the cavity of the chest, by these actions, would take the straight road from the trachea through the passages, and of consequence would expand no part of the lungs which lay out of that line, whereby respiration would be totally prevented, and an effect produced exactly similar to what happens in other animals when the lungs are so much wounded as to allow a free exit to the air at that part.

The cells in the bodies of birds which receive air from the lungs, are to be found both in the soft parts, and in the bones; and have no communication with the cavity of the common cellular membrane. Some of these air-bags are placed in the larger cavities, as in the abdomen; and others are so lodged in the interstices of muscles, blood, vessels, and nerves, about the breast, axilla, &c. as at first to give the appearance of the common connecting membrane. Some communicate immediately with one another; and all may be said to have a communication by means of the

lungs. They are of very different sizes, as may best suit the particular circumstances of the parts in which they are placed.

The bones which receive air are of two kinds; some, as the sternum, ribs, and vertebræ, having their internal substance divided into innumerable cells; whilst others, as the os humeri and os femoris, are hollowed out into one large canal, with sometimes a few bony columns running across at its extremities. Bones of this kind may be distinguished from those that do not receive air, by several marks: first, By their less specific gravity: secondly, By being less vascular than the others, and therefore whiter: thirdly, By their containing little or no oil, and consequently being more easily cleaned; and when cleaned, appearing much whiter than common bones: fourthly, By having no marrow, or even any bloody pulpy substance in their cells: fifthly, By not being in general so hard and firm as other bones*; and sixthly, By the passage that allows the air to enter the bones, which can be easily perceived. In the recent bone we may readily discover holes, or openings, not filled with any soft substance as blood-vessels or nerves; several of these holes are placed together, near that end of the bone which is next to the trunk of the bird; and are distinguishable by having their external edges rounded off, which is not the case with the holes through which either nerves or blood-vessels pass into the substance of the bone. When birds break any of the bones which contain air, the surrounding parts often become emphysematous.

There are openings in the lungs, by which air is transmitted to the other parts; and the membrane or diaphragm, abovementioned, is perforated in several places with holes of a considerable size, which admit of a free communication between the cells of the lungs and the abdomen, a circumstance which has been frequently noticed. To each of these perforations is joined a distinct membranous bag, extremely thin and transparent; which bags being afterwards continued through the whole of the abdomen and attached to the back and sides of that cavity, are kept firm

* The bones of some birds are so soft that they can be squeezed together with the finger and thumb; the bones of the extremities, however, have very solid sides.

in their proper situations ; each receiving the air from their respective openings. There is no occasion to describe here all the bags, or their attachments, it being sufficient to have said, that they extend over the whole abdomen.

The lungs at the anterior part, contiguous to the sternum, have openings into certain membranous cells which lie upon the sides of the pericardium, and communicate with the cells of the sternum. At the superior part the lungs have a communication with the large cells of a loose net-work, through which the trachea, œsophagus, and great vessels pass as they are going to and from the heart. When these cells are distended with air, the size of that part where they lie is very considerably increased, and this enlargement is in general a mark of either the passion of anger or love. It is plainly seen in the Turkey-cock, the pouting pigeon, &c. and is very visible in the breast of a goose when she cackles. These cells communicate with others in the axilla, under the large pectoral muscle ; and in some birds are still further extended. In the pelican, for instance, the skin of the whole body, even to the tip of the wing, is united to the part underneath, by means of these cells, which are equally formed ; and when the skin is removed, the two separated surfaces appear as if honey-combed. When the cells are distended, the skin is removed to a considerable distance, by which means the volume is proportionally increased. In most birds, I believe in all that fly, these axillary cells communicate with the cavity of the os humeri, by means of small openings in the hollow surface near the head of that bone ; in some they are continued down the wing, communicating with the ulna and radius ; in others they reach even as far as the pinions. The ostrich, however, is an exception.

The posterior edge of the lungs (which lies on the sides of the spine and projects backwards between the ribs) communicates with the cells of the bodies of the vertebræ, with those of the ribs, the canal of the medulla spinalis, and the cells of the sacrum, and other bones of the pelvis ; from which parts the air finds a passage into the cavity of the thigh-bone. This takes place in the greatest number of birds ; but in some the air is even continued part of the way down the thighs. This account

agrees with what we generally find, though some birds have more, and some fewer of these communications; for, in the ostrich, no air gets into the os humeri, yet it enters into every other part, as before described, and in very large quantities. In the common fowl no air appears to enter any bone except the os humeri. The wood-cock has no air-cells, either in the first bones of the wing, or in the thigh bones. On the other hand, in the pelican the air passes on to the ulna and radius, and into those bones which answer to the carpus and metacarpus of quadrupeds.

Thus the cells of the abdomen, those surrounding the pericardium, those situated at the lower and forepart of the neck, and in the axilla, those in the cellular membrane under the pectoral muscles, as well as in that which unites the skin to the body, all communicate with the lungs, and are capable of being filled with air; and again from them, the cells of the sternum, ribs, vertebræ of the back and loins, bones of the pelvis, the humeri, the ulna and radius, with the pinions and thigh bones, can in many birds be furnished with air.

It is not by the lungs alone, that air is conveyed into the bones of birds; for the cells of the diploe between the two plates of the skull, in some birds, receive a considerable quantity of air by the Eustachian tube.* Of this the owl is a remarkable instance. The lower jaw of some kinds, is likewise supplied with air, and often by the same canal.† Some authors have considered the diploe in the cranium of a bird as a continuation

* The only thing, in other animals, similar to this communication in birds, of the cells of bones with the external air, is that which takes place in the internal ear of quadrupeds, by means of the Eustachian tube.

† When I wrote this account to send it to the Royal Society, I did not then know by what means this was done; for in that I said, "but by what means I do not know"; that is, I did not know whether it was conveyed by the trachea, where it passes along the neck; or the Eustachian tube. Professor Camper, when he did me the honour to call upon me, was so obliging as to take some pains to show me, in the lower jaw of the hawk, the hole where the air entered; which makes me suspect he did not understand what I had written. For after having given the marks by which such openings were particularly distinguished, it will hardly be supposed I could say that I did not know the hole where the air entered.

of the mamillary process; and looked upon it as a circumstance peculiar to singing birds; which is not really true.

These facts, which had been formerly observed, led me in the year 1758 to make several experiments upon the breathing of birds, that might prove the free communication between the lungs and the before mentioned parts.

First, I made an opening into the belly of a cock, and having introduced a silver cannula, tied up the trachea; I found that the animal breathed by this opening, and might have lived; but by an inflammation in the bowels coming on, adhesions were produced, and the communication was cut off.

I next cut the wing through the os humeri, in another fowl, and tying up the trachea, as in the cock, found that the air passed to and from the lungs by the canal in this bone. The same experiment was made with the os femoris of a young hawk, and was attended with a similar result. But the passage of air through the divided parts, in both these experiments, especially in the last, was attended with more difficulty than in the former one; it was indeed so great, as to render it impossible for the animal to live longer than evidently to prove that it breathed through the cut bone.

I have made several preparations of these cells, by throwing into the trachea an injection, commonly called the corroding injection, which first filled the air-cells of the lungs, then all the others, such as the cells in the abdomen, anterior and superior part of the chest, axilla, os humeri, cells of the back-bone and thigh; and the whole being afterwards put into spirit of sea-salt, and corroded, the cast of injection came out entire.

The singularity of these communications in birds, put me upon considering what might be their final intention. At first I supposed it might be intended to assist the act of flying, that being the circumstance which appears the most peculiar to birds; and it might be of service in that respect, I thought, by increasing the volume and strength with the same quantity of matter, without adding to the weight of the whole, which indeed would rather be diminished by the difference of specific gravity be-

tween the external and internal air. This opinion was strengthened, by discovering that the feathers of birds contained also a considerable quantity of air, in the very part which requires the greatest strength; and by the analogy which is observed between this mechanism in birds, and what is discoverable in most kinds of fishes. For these last have air contained within their bodies, which I believe is commonly supposed to lessen their specific gravity, although this does not appear so necessary in fishes, who move in a much heavier element than birds.* But when I found that

* When we consider that the elevating and suspending apparatus is much smaller in fishes than in birds, we may reasonably conceive the air in them was intended as a kind of equilibrium between the fish and water; and that progressive motion was the only thing wanted in the actions of fishes. Were we to reason upon general principles alone, we should suppose that those fishes who have the largest air-bags should have their muscles of a greater specific gravity; and those fishes that have none, should have the lightest flesh; therefore that the flesh of the salmon and cod, which have an air-bag, should be heavier than that of the shark, which has none: but to know how far this, which appeared to be reasonable, was a fact, I made the following experiments:

Experiment 1. I took a portion of muscle of the shark, cod, and salmon, of the same weight in air; and first examined how far they occupied the same space, by immersing them in water, and observing the rise or fall of the water upon each of them being separately immersed in it.

The shark occupied the smallest space, the salmon a little more, and the cod the largest.

Experiment 2. I then suspended the same three portions, upon a level, in a glass vessel filled with water about two feet high, and let them all go at the same instant, to see which would fall through the water in the shortest space of time. The shark got to the bottom first, the salmon next, and the cod last.

It is necessary to observe that, in both these experiments, the difference in bulk, and in the times of their falling was very little; but, however, sufficient to ascertain the fact for which the experiments were instituted.

To see how far the muscular flesh of birds was specifically lighter than that of a quadruped, I repeated the above experiments upon a portion of a hind, of a pigeon, and of a sheep, but could discover no visible difference in their weight.

It may be observed, there are in common two situations of oil in fishes; in one it is diffused through the fish, as if the body had been steeped in it, as in the salmon, herring, &c. In the other it is found in the liver, as in all of the ray kind, cod, &c. and in general those that have it in one part have none in the other; however, there are some, although I believe but few,
who

the ostrich (which is not intended to fly) was amply provided with these cells; and that the common fowl, and many others of that class, which are endowed with the faculty of flying, were less liberally supplied: when I saw that even the wood-cock, which flies, and is supposed a bird of passage, was inferior in this respect to the ostrich; and that the bat differed not in structure from animals that do not fly, I was compelled, by so many contradictions to theory, to suppose that this singular mechanism might be intended for some other purpose.

The next conjecture that offered, was, that these cells were to be considered as an appendage to the lungs; and to this I was led by the analogy observable between birds and amphibious animals. For although both in the bird and amphibious tribe, as the snake, viper, and many others, the lungs are continued down through the whole belly, in form of two bags, and therefore appear to be larger than the lungs in any other animal, yet in all of them the quantity of surface exposed to the air is much less than in the quadruped; for the cells of the lungs in the bird are larger, and in the snake, &c. the upper part only can perform the office of respiration with any degree of effect; the lower having comparatively but few air-vessels. The air must pass through this upper part before it gets to the lower in inspiration, and must also repass in expiration, so that the respiratory surface has more air applied to it than what the lungs of themselves could contain. It is not however to be supposed, that the air can be made to pass to and fro in bones as in parts which admit of contraction and dilatation; the purpose answered by these bony cells must therefore be different; and perhaps they should be considered as reservoirs of air.* There is in fact a

who have their oil in form of what may be called fat, viz. in flecks in the interstice of parts, as the sturgeon. The liver, in those of the ray kind, is large and extended through the belly; therefore it might be supposed to lighten the body, from oil being lighter than water or the flesh; but we have oil in the liver of the cod; and in the salmon and herring, the oil is diffused through the whole; therefore I am afraid we are not yet acquainted with the full effect of the air-bladder in fishes.

* It is not to be supposed that, the air in the cells in birds will be changed while flying; only accumulated and retained; not in the least influenced by either inspiration or expiration.

great similitude between birds and that class of animals called amphibious ; and although a bird and a snake are not the same in the construction of the respiratory organs, yet the circumstance of the air passing in both beyond the lungs, into the cavity of the abdomen, naturally leads us to suppose, that a structure so similar is designed in each to answer a similar purpose. This analogy is still further supported by the lungs in both consisting of large cells. Now in amphibious animals, the use of such a conformation of the lungs is evident ; as it is in consequence of this structure that they require to breathe less frequently than others : and in this respect it may, in birds, have some connection with flying ; as that motion might easily be imagined to render frequency of respiration inconvenient, and a reservoir of air would therefore become singularly useful. Although we are not to consider this structure in birds to be an extension of lungs, yet I can easily conceive this accumulation of air to be of great use in respiration. For, it was observed before, respecting the amphibii, that the air in its passage to and from these cells, must certainly have a considerable effect upon the blood in the lungs, by allowing a much greater quantity of air to pass in a given time than if there was no such construction of parts ;* and this opinion will not appear to be ill founded, if we consider that both in the bird and the viper, the surface of the lungs is small in comparison to what it is in many other animals which have not

piration. It might be asked, Where is the stricture upon the air when flying, so as to keep the parts distended ; is it upon the outlets from the lungs, or is it at the glottis, as in the quadruped ? For we may observe, that when an animal is using considerable exercise, it never either expands the lungs, nor makes a full expiration, giving the ribs and diaphragm as little extent of motion as possible, so that the body may be kept firm, which obliges it to breathe oftener ; and as this quantity of air is not sufficient for the accelerated motion of the blood, the animal gets what is called, out of breath ; which is no more than the two not being proportioned ; and when it rests, it breathes as quick, and takes as long strokes as possible, to make up the loss. So that in exercise we probably breathe less air.

* It may, perhaps, occur to some that, the whole of these communicating cells are to be considered as extended lungs ; but I can hardly think that any air which gets beyond the vesiculated lungs themselves is capable of affecting the blood of the animal, as the other cavities into which it enters, whether of the soft parts or of the bones, appear to be very little vascular.

this extension of cavity. It is also a corroborating circumstance, that in the fowl the air might have passed by a much readier way than through the lungs, into all the cells about the breast, neck, axilla, wings, &c. as these could have been filled from the lower end of the trachea, upon which many of them lie. But the air must now take a roundabout passage both in its way in and its way out, those openings being upon the exterior surface of the lungs. We must not however give up the idea of such structure being of use in flying; for I believe we may set it down as a general rule, that in the birds of longest and highest flight, as eagles, this diffusion of air is extended further than in the others. This opinion is strengthened by comparing the structure above described with the respiratory organs in the flying insects, which are composed of cells diffused through the whole body: these are extended even into the head and down the extremities, while there is no such appearance in the insects that do not fly, as the spider: but why the pelican should be so amply provided, I cannot say, not knowing the natural history of that bird sufficiently to be able to judge of this point. Do they carry weights in the large fauces so great as to require such an increase of substance without increase of weight?

How far this construction of the respiratory organs may assist birds in singing, deserves investigation; as the vast continuance of song, between the breathings, in a Canary-bird, would appear to arise from it. This is a subject however which I shall not at present enter upon.

EXPERIMENTS AND OBSERVATIONS ON ANIMALS, WITH RESPECT TO THE POWER OF PRODUCING HEAT.

SOME late ingenious experiments and observations, published in the Philosophical Transactions, upon a power which animals seem to possess of generating cold, induced me to look over my notes, containing some which I had made in the year 1766, indicating an opposite power in animals, whereby they are capable of resisting any external cold while alive, by generating within themselves a degree of heat sufficient to counteract it. Those experiments were not originally instituted with any expectation of the event which resulted from them, but for the purpose of satisfying myself, whether an animal could retain life after being frozen, as has been confidently asserted both of fishes and snakes. For that these, after being frozen, still retain so much of life, as when thawed to resume their vital actions, is a fact so well attested, that we are bound to believe it; and had my experiment succeeded, it was my intention to have tried the effects of freezing on living animals to a much greater degree than can ever happen accidentally.

I mention these circumstances, to account for what might otherwise be attributed to negligence and inattention; namely, the little nicety that was observed in measuring the precise degree of cold applied in the experiments. Accuracy in this particular was not aimed at, being of no consequence in the inquiry more immediately before me. The cold was first produced by means of ice and snow with sal ammoniac or sea-salt, to about the 10° of Fahrenheit's thermometer: ice was then mixed with spirit of nitre; but what degree of cold was thus produced I did not examine. This cold mixture was made in a tub surrounded with woollen cloths, and covered with the same, to prevent the effects of the heat of the atmosphere upon the mixture itself, and to preserve as much as possible a

cold atmosphere within the vessel. Animal juices, as the blood, freeze at 25° ; so that a piece of dead flesh could be frozen in an atmosphere, cooled to that point.

EXPERIMENTS.

Experiment I. was made on two carp. These were put into a glass vessel with common river water, which was placed in the freezing mixture. The water not freezing fast enough to hasten that effect, as much cooled snow was added as to render the whole thick. The snow round the carp melting, we put in more fresh snow, which melting also, was repeated several times, till we grew tired, and at last left them covered up in the yard to freeze by the joint operation of the surrounding mixture and the natural cold of the atmosphere. They were frozen at last, after having exhausted the whole powers of life in the production of heat. That this was really the case, could not be known till I had completed that part of the experiment for which the whole was begun, viz. the thawing of the animals. It was done very gradually; but the animals did not, with flexibility, recover life; and while in this cold, shewed signs of great uneasiness by their violent motions. *N.B.* In some of these experiments, where air was made the conductor of the cold and heat, that the heat might be more readily carried off from the animal, a leaden vessel was used. It was small for the same reason; and as it was necessary for the animal's respiration that the mouth of the vessel should communicate with the open air, it was made deep, that the cold of the atmosphere round the animal might not be diminished too quickly by the warmth of the open air, which would have spoiled it as a conductor.

Experiment II. was upon a dormouse; the vessel in which it was confined being sunk in the cold mixture almost to its edge. The atmosphere round the animal soon cooled; its breath froze as it came from the mouth; an hoar-frost gathered on its whiskers, and on all the inside of the vessel; and the external points of the hair became covered with the same. While this was going on, the animal shewed signs of great uneasiness: sometimes

it would coil itself into a round form, to preserve its extremities, and confine its heat; and finding that ineffectual, would then endeavour to make its escape*. Its motions became less violent by the sinking of the vital powers; its feet were at last frozen; but we were not able to keep up the cold a sufficient time to freeze the whole animal, the hair being so bad a conductor of heat, that the consumption was not more than the animal powers were capable of supporting.†

Experiment III. was made with another dormouse; and taught by the failure of the last experiment, I took care that the hair should not a second time be an obstruction to our success. Having therefore first made the animal wet all over, that its heat might be more instantaneously carried off, it was put into a leaden vessel, and the whole placed in the cold mixture as before. The animal soon gave signs of feeling the cold, by repeated attempts to make its escape; and the breath and water evaporating from its body being soon frozen, appeared like a hoar-frost on the sides of the vessel, and on its whiskers; but while the vigour of life lasted, it defied the approach of the cold. However, from the hair being wet, and thereby rendered a good conductor, there was a much greater consumption of heat than in the former experiment; which hastened on a diminution of the power of producing it. The animal dying, soon became stiff; and upon being thawed, was found quite dead.

Experiment IV. A toad being put into a vessel with water, at such a depth as not to cover its mouth, was placed in the mixture cooled to between 10° and 15° . The water froze so near to the body of the animal, as quite to inclose it, but without destroying life; yet though not frozen, it hardly ever recovered the use of its limbs.

* This shows, that cold carried to a great degree rather rouses than depresses the animal action; but it appears from many circumstances and observations, that a certain degree of cold produces inactivity both in the living and sensitive principle, which will be further illustrated hereafter.

† These experiments were made in presence of Dr. George Fordyce and Dr. Erwin, teacher of Chymistry at Glasgow; the latter of whom came in accidentally in the middle of our operations.

Experiment V. was with a snail, which froze very soon, in a cold between 10° and 13° . These two last experiments were made in the winter, when the living powers of the animals selected for the trial are very weak; they might have resisted the cold more strongly in the summer. Why the animals mentioned in the above experiments died before they were frozen, while those which are exposed to the atmosphere in very cold climates do not, is a point I shall not pretend to determine; not knowing the difference between the effects of a natural and an artificial cold. It may be accounted for, by supposing that the natural cold in climates in which animals are found frozen, is so intense as to produce congelation immediately, before the powers of life are exhausted; at least whether it is so or not is worthy of inquiry.

It appears from the above experiments; first, That most probably the animals were deprived of life before they were frozen: secondly, That there was an exertion or expence of animal power in resisting the effects of cold, proportioned to the necessity: thirdly, That this exertion was in proportion to the perfection of the animal, and the natural heat proper to each species, and to each age. This exertion might also perhaps depend in some degree on other circumstances not hitherto observed: for from experiment II. and III. upon dormice, I find that in the animals, which are of a constitution to retain nearly the same heat in all temperatures of the air, it required the greatest cold I could produce to overcome this resisting power; while by experiment IV. and V. on the toad and snail, whose natural heat is not always the same, but is altered very materially according to the external heat or cold, this power was exhausted in a degree of cold not exceeding 10° or 15° : and the snail being the most imperfect of the two, its powers of generating heat appeared to be much the weakest.

That the imperfect animals will allow of a considerable variation in their temperature of heat and cold, is proved by the following experiments. The thermometer being at 45° , the ball was introduced by the mouth into the stomach of a frog, which had been exposed to the same cold. It rose to 49° . I then placed the frog in an atmosphere made warm by heated water, where I allowed it to stay twenty minutes; and

upon introducing the thermometer into the stomach, it raised the quicksilver to 64° . To what degree the more imperfect animals are capable of being rendered hotter and colder at one time than another, I have not yet ascertained; but the torpidity of these animals in our winter is probably owing to the great change wrought in their temperature by the external heat and cold. The cold in their bodies is to such a degree, as in a great measure to put a stop, while it lasts, to the vital functions; while in warmer climates no such effect is produced. This variety not only takes place in animals of different orders; but in some degree in the same animal at different ages, even according to the different age of the parts in the same animal: a young animal requires more warmth than one full grown; and although an animal is equally old in all its original parts, yet there are often new ones formed in consequence of diseases; and we find, that these new or young parts in animals are not so able to support life as the old, at least for some time: but as animals are of different ages, and the same animal is always growing older, and of course more and more perfect, they then become more capable of generating heat than when they were younger. This, however, has its limitations; for after a certain period they again lose this power, and therefore require a less strongly conducting medium, or warm atmosphere.

This power of generating heat, seems to be a property in an animal while alive. In the more perfect animals it is to preserve a standard heat; and as they are most commonly in an atmosphere colder than themselves, they have most commonly occasion to exert it; and it is therefore a power only of opposition and resistance; for it is not found to exert itself spontaneously and unprovoked; but must always be excited by the energy of some external frigorific agent, or disease; yet it is natural to such animals that this power should be called forth; as will be observed by and by. It does not depend on the motion of the blood, as some have supposed, because it likewise belongs to animals who have no circulation: and the nose of a dog, which is always nearly of the same heat in all temperatures of the air, is well supplied with blood; although we must allow where this power is greatest, the circulation is the quickest: neither can it be

said to depend upon the nervous system ; for it is found in animals that have no brain or nerves. However, it must be allowed, that all that class who possess this power in the highest degree, have the largest brain, although this power is not in the least in proportion to the quantity of brain in that class. It is most probable that it arises from some other principle ; a principle so connected with life, that it can, and does, act independently of circulation, sensation, and volition ; and is that power which preserves and regulates the internal machine. This power of generating heat, is in the highest perfection when the body is in health ; and in many deviations from that state, we find that its action is extremely uncertain and irregular ; sometimes rising higher than the standard, and at other times falling much below it. Instances of this we have in different diseases, and even in the same disease, within very short intervals of time. A very remarkable one fell under my own observation, in a gentleman who was seized with an apoplectic fit ; and while he lay insensible in bed, covered with blankets, I found that his whole body would, in an instant, become extremely cold in every part, continuing so for some time ; and, as suddenly, would become extremely hot. While this was going on alternately, there was no sensible alteration in his pulse for several hours.

Being satisfied of the foregoing fact, that animals had a power of generating heat, I pursued the subject still further ; not so much with a view to account for animal heat, as to observe the different phenomena, with the variations or difference in the heat in different animals. In the course of my experiments having found variations in the degree of heat and cold in the same experiment, for which I could not account, I suspected that this might arise from some imperfection in the construction of the thermometer. I mentioned to Mr. Ramsden my objections to the common construction of that instrument, and my ideas of one more perfect in its nature, and better adapted to the experiments in which I was engaged. He accordingly made me some very small thermometers, six or seven inches long, not above two-twelfths of an inch thick in the stem ; having the external diameter of the ball very little larger than that of the stem, on which was marked the freezing point. The stem was embraced by

a small ivory scale so as to slide upon it easily, and retain any position. Upon the hollow surface of this scale were marked the degrees which were seen through the stem. By these means the size of the thermometer was very much reduced, and it could be applied to soft bodies with much more ease and certainty, and in many cases in which the former ones could not be conveniently used; I therefore repeated with it such of my former experiments as had not at first proved satisfactory, and found the degrees of heat very different, not only from what I had expected, but also from what I had found by my former experiments with thermometers of the common construction.

I have observed above, and find it supported by every experiment I have made on the heat and cold of animals, that the more perfect have the greater power of retaining a certain degree of heat, which may be called their standard heat, and allow of much less variation than the more imperfect animals: however, it will appear from the three experiments which I am now going to relate, that many, if not all of the more perfect, are still incapable of keeping constantly to one degree; but may be altered from their standard heat, either by external applications, or disease. These variations are much greater below that standard than above it; the perfect animals having a greater power of resisting heat than cold, so that they are commonly near their ultimate heat. Indeed we do not want any other proof of a variation than our own feelings; being all sensible of heat and of cold, which sensations could not be produced without an alteration really taking place in the parts affected; and that alteration could not take place if they did not become actually warmer or colder. I have often cooled my hands to such a degree, that I could warm them by immersing them in water just pumped; therefore my hands were really colder than the pump-water.

An increase of absolute heat must alter the texture or position of the parts, so as to produce the sensation which we call heat: and as that heat is diminished, the texture or position of the parts is altered in a contrary way; and, when carried to a certain degree, becomes the cause of the sensation of cold. Now these effects could not take place in either case without an increase or decrease of absolute heat in the part; heat there-

fore in some one of its different degrees must be present. I shall not in this place attempt to settle whether heat is a body or matter, or only a property of matter; which appears to me to be merely a difference in terms; for a property must belong to something. When heat is applied to the surface of the body, the skin becomes in some degree heated according to the application; which may be carried so far as actually to burn the living parts: on the contrary, in a cold atmosphere, a man's hand may become so cold as to lose that sensation altogether, and change it for pain. Absolute heat and cold may be carried so far as even to alter the structure of the parts upon which the actions of life depend.

As animals being subject to variations in the degrees of heat and cold from external applications, are of course, in this respect, affected in some measure like inanimate matter: and therefore, as parts are elongated or recede from the common mass, these effects more readily take place: for instance, all projecting parts and extremities, more especially toes, fingers, noses, ears, combs of fowls, particularly of the cock, are more readily cooled, and are therefore most subject to be affected by cold. Animals are not only subject to an increase and decrease of heat, similar to inanimate matter; but the transition from one to the other (as far as they admit of it) is nearly as quick. I shall not however confine myself to sensation alone, as that is in some degree regulated by habit: for a habit of uniformity in the application of heat and cold to an animal body, renders it more sensible of the smallest variation in either; while by the habit of variety it will become, in a proportional degree, less susceptible of all such sensations. This is proved every day, in cold weather, by people who are accustomed to clothe themselves warm. In them the least exposure to cold air, although the effect produced in the skin is perhaps not the hundredth part of a degree, immediately gives the sensation of cold, even through the thickest covering: those, on the contrary, who have been used to go thinly clothed, can bear the variation of some degrees without being sensible of it: of this the hands and feet afford an instance in point: exciting the sensation of cold when applied to another part of the body, without having before given to the mind an impression of cold existing in these parts themselves. The projecting

parts and the extremities are those which admit of the greatest change in their degrees of heat and cold, without materially affecting the animal, or even its sensations. I find that by heat or cold externally applied to such parts, the thermometer may be made to rise or fall; but not in an equal proportion as when applied to inanimate matter. Nor are the living parts cooled or heated in the same proportion, as appears from the application of the thermometer to the skin; for the cuticle is to be considered as a dead covering, capable of receiving greater degrees of heat and cold than the living parts underneath; and as it might be suspected that the whole of the variation was in this covering, to remove any such doubt I made the following experiments.

Experiment I. I placed the ball of the thermometer under my tongue, where it was perfectly covered by all the surrounding parts; and having kept it there for some minutes, I found that it rose to 97° ; yet it rose no higher by being continued there. I then took several pieces of ice, about the size of walnuts, and put them in the same situation, allowing them only to melt in part, that the application of cold might be better kept up, occasionally spitting out the water arising from the solution. Having continued this for ten minutes, I found, on introducing my thermometer, that it fell to 77° ; so that the mouth at this part had lost 20° of heat. The thermometer gradually rose to 97° again; but did not in this experiment sink so low as it would have done in the hand, if a piece of ice had been held in it for the same length of time. Perhaps the surface under the tongue being surrounded with warm parts, renders it next to an impossibility to cool it below that degree: but I rather suspect that such parts as the hand will allow of greater latitude in this respect, from having insensibly acquired the habit of varying the degree of cold, and becoming of course less susceptible of its impressions, and therefore less easily excited.

As a further proof, that the more perfect animals are capable of varying their heat, in some measure, according to the external heat applied, I shall adduce the following experiments made on the human subject.

The mouth being a part so frequently in contact with the external

atmosphere in the action of breathing, whatever is put into it may be supposed to be influenced by that atmosphere; this will always render an experiment made in that part, relative to heat and cold, somewhat uncertain. I imagined that the urethra would answer better, because being an internal cavity it can only be influenced by heat and cold applied to the external skin of the parts. I imagined also, that whatever effects the application of heat and cold might have, they would sooner take place in the urethra, as being a projecting part, than in any other part of the body; and therefore if living animal substance was in any degree subject to the common laws of matter in this respect, the urethra would be readily affected: to determine this I procured a person who allowed me to make such experiments as I thought necessary.

Experiment II. I introduced the ball of my thermometer into the urethra about an inch; which having remained there about a minute, the quicksilver rose to 92° : at two inches it rose to 93° ; at four inches to 94° ; and when the ball had got as far as the bulb of the urethra, where it was surrounded by warm parts, the quicksilver rose to 97° .

Experiment III. These parts being immersed for one minute in water, heated only to 65° , and the thermometer introduced about an inch and a half into the urethra, the quicksilver rose to 79° ; which was repeated several times with the same result. To discover if there were any difference in the quickness of the transition of heat and cold in living and dead parts, and to determine if the extent to which each would go, were likewise different, I procured a dead penis for the purpose of making the comparative experiments that follow; being clearly of opinion that all such trials should be as similar as possible, except in those points where the difference (if there is any) makes the essential part of the experiment.

Experiment IV. The heat of the penis of a living person, an inch and a half within the urethra, being found exactly 92° ; and the dead one being heated to the same degree, I had both immersed in the same vessel, with the water at 50° , when by introducing the thermometers several different times, I was able to note the comparative quickness with which they cooled from 92° ; and observed that the dead cooled sooner

by two or three minutes; the living sunk the quicksilver to 58° , and the dead to 50° : the thermometer, although continued there some time longer, fell no lower. I repeated this experiment several times, with the same result; although at one time there was a small difference in the degrees of heat of the penis and also of the water; but the difference in the result was nearly proportional in all the three different trials, therefore the same conclusions may be drawn from them. In these last experiments very little difference was observed between the cooling of a dead and of a living part; a circumstance which we cannot suppose to take place uniformly through the whole body; as in that case, living animals would always be of the same degree of heat with the atmosphere in which they live. The subject of these experiments not choosing to have the part cooled lower than 53° or 54° , prevented my observing if the powers of generating heat were exerted to a greater degree when the heat was brought so low as to threaten destruction; but by some experiments on mice, which will be related hereafter, it will appear that the animal powers are roused to exert themselves in this respect when necessary.

Having found, from the above experiments, that parts of an animal were capable of being cooled below the common or natural heat; I proceeded to make others, with a view to ascertain if the same parts were capable of being made much hotter than the standard heat of animals. The experiments were made in the same manner as the former, only the water was now hotter than the natural heat of the animal.

Experiment V. The natural heat of the parts being 92° , they were immersed for two minutes in water heated to 113° , and the thermometer being introduced as before, the quicksilver rose to 100° and a half. This experiment I also repeated several times, but could not raise the heat of the penis beyond 100° and a half: this was probably owing to the person not being able, at the time to bear the application of water warmer than 113° . By way of comparison, I made

Experiment VI. The living and dead parts being both immersed in water, gradually made warmer and warmer from 100° to 118° , and continued in that heat for some minutes, the dead part raised the thermo-

meter to 114° , while the living raised it no higher than 102° and a quarter. It was observed, by the person on whom the experiment was made, that after the parts had been in the water about a minute, the water did not feel hot; but on its being agitated it felt so hot that he could hardly bear it. Upon applying the thermometer to the sides of the living glans, the quicksilver immediately fell from 118° to about 104° , while it did not fall more than a degree when put close to the dead; so that the living glans cooled the surrounding water to a certain distance.*

Experiment VII. The heat of the rectum in the same man was 98° and a half exactly.

In the second, third, fourth, fifth, and sixth experiments, an internal cavity, which is both very vascular and sensible, was evidently influenced by external heat and cold, though only applied to the skin of the part; while, in the seventh experiment, another part of the same body, where external heat and cold could make little or no impression, was of the standard heat. Although it will appear from experiment, that the rectum is not the warmest part of an animal; yet, in order to determine how far the heat could be increased by stimulating the constitution to a degree sufficient to quicken the pulse, I repeated the seventh experiment after the man had eaten a hearty supper and drank a bottle of wine, which increased the pulse from 73° to 87° , and yet the thermometer only rose to 98° and a half.

Having formerly made experiments upon dormice during the sleeping season, with a view to see if there were any alteration in the animal œconomy at that time, I found among my notes an account of some which appear to our present purpose: but to be more certain of the accuracy of the former experiments, I repeated them with my new thermometer.

* This might furnish an useful hint respecting bathing in water, whether colder or warmer than the heat of the body: for if intended to be either colder or hotter, it will soon be of the same temperature with that of the body; therefore in a large bath, the patient should move from place to place; and in a small one, there should be a constant succession of water of the intended heat.

Experiment VIII. In a room, in which the temperature of the air was between 50° and 60° , a small opening was made in the belly of a dormouse, of a sufficient size to admit the ball of my thermometer, which being introduced into the belly at about the middle of that cavity, rose to 80° , and no higher.

Experiment IX. The mouse was put into a cold atmosphere of 15° above 0, and left there for fifteen minutes; after which the thermometer being introduced a second time, it rose to 85° .

Experiment X. The mouse was again put into a cold atmosphere for fifteen minutes; and the thermometer being introduced, the quicksilver at first rose to 72° only, but gradually came up to 83° , 84° , and 85° .

Experiment XI. It was put a third time into the cold atmosphere, and allowed to stay there for thirty minutes; the lower part of the mouse at the bottom of the dish, was almost frozen; the whole of the animal was numbed, and a good deal weakened. The thermometer being introduced, the heat was found to vary in different parts of the belly; in the pelvis, near the parts most exposed to the cold, it was as low as 62° ; in the middle, among the intestines, about 70° ; but near the diaphragm it rose to 80° , 82° , 84° , and 85° ; so that in the middle of the body the heat had decreased 10° . Finding a variation in different parts of the same cavity in the same animal, I repeated the same experiments upon another dormouse.

Experiment XII. Having brought a healthy dormouse, which had been asleep from the coldness of the atmosphere, into a room in which there was a fire (the atmosphere at 64°); I introduced the thermometer into its belly, nearly at the middle, between the thorax and pubis, and the quicksilver rose to 74° or 75° ; turning the ball towards the diaphragm, it rose to 80° ; and when I applied it to the liver, it rose to 81° and a half.

Experiment XIII. The mouse being placed in an atmosphere at 20° , and left there half an hour; when taken out was very lively, even much more so than when put in. Introducing the thermometer into the lower

part of the belly, the quicksilver rose to 91° ; and upon turning it up to the liver, to 93° .

Experiment XIV. The animal being replaced in the cold atmosphere at 30° , for an hour, the thermometer was again introduced into the belly; at the liver it rose to 93° ; in the pelvis to 92° ; the mouse continuing very lively.

Experiment XV. It was again put back into an atmosphere, cooled to 19° , and left there an hour; the thermometer at the diaphragm was 87° ; in the pelvis 83° ; but the animal was now less lively.

Experiment XVI. Having been put into its cage, the thermometer being placed at the diaphragm, in two hours afterwards, was at 93° .

As I was unable to procure hedge-hogs in the torpid state, to ascertain their heat during that period, I got my friend, Mr. Jenner, surgeon, at Berkley, to make the same experiments on that animal, that I might compare them with those in the dormouse; and his account is as follow:

“ Experiment I. In the winter the atmosphere at 44° , the heat of a torpid hedge-hog, in the pelvis, was 45° , and at the diaphragm 48° and a half.

Experiment II. The atmosphere 26° , the heat of a torpid hedge-hog, in the cavity of the abdomen, was reduced so low as 30° .

Experiment III. The same hedge-hog was exposed to the cold atmosphere of 26° for two days; and the heat of the rectum was found to be 93° ; the wound in the abdomen being now so small that it would not admit the thermometer.

A comparative experiment was made with a puppy, the atmosphere at 50° ; the heat in the pelvis, as also at the diaphragm, was 102° .

In summer, the atmosphere at 78° , the heat of the hedge-hog, in an active state, in the cavity of the abdomen, towards the pelvis, was 95° ; at the diaphragm 97° .”

We find from these experiments, that the heat of an animal is increased under the circumstances of cold, whenever there are actions to be carried on for which heat is necessary.

In the experiments on the first dormouse, the heat of the animal was

80°; which is below the standard heat of the actions of that animal; and after being put into the cold mixture, its heat was raised to 85°. In the second dormouse the heat was raised, by repeated experiments, from 75° to 93°. This question naturally occurs here; Was the increase of heat in the animals generated to resist the artificial cold, produced by placing them in a cold atmosphere? Or was it owing to a wound having been made into the cavity of the abdomen, and an exertion of the animal powers being required to repair the injury; which exertion could not take place without the increased degree of heat? That it was in consequence of the wound, appears evident from the experiment made upon the second hedge-hog; for in an atmosphere of 26° of heat, it was in a very torpid state, and did not raise the thermometer higher than 30°; but after being wounded and put back into the cold, and kept there for two days, its heat in the rectum was 93°; and so far from being torpid, it was lively, and the bed in which it lay felt warm*.

Why the heat of the dormouse should be so low as 80°, in an atmosphere of between 50° and 60°, is not easily accounted for, except as the effect of sleep. But I should very much suspect, that sleep, simply considered, is out of the question; it being an effect that takes place in all degrees of heat and cold. In animals whose voluntary actions are suspended by cold, that appears to produce its effect by acting in a certain degree as a sedative, in consequence of which the animal faculties are proportionably weakened, though they still retain, even under such circumstances, the power of carrying on all the functions of life. Beyond this point cold seems to act as a stimulant, and rouses the animal powers to action for self-preservation. It is more than probable, that most animals are in this predicament; and that there is a degree of cold, corresponding with every particular order of animals, by which, when applied, the voluntary actions must be suspended.

* It is found from experiments, that the heat of an inflamed part is nearly the greatest or standard heat of the animal; it appearing to be a part of the process of inflammation to raise the heat up to the standard.

When a man is asleep, he is colder than when awake; and I find, in general, that the difference is about one degree and a half, sometimes less. But this difference in the degree of cold between sleeping and waking, is not a cause of sleep, but an effect; for many diseases produce a much greater degree of cold in the animal, without giving the least tendency to sleep; therefore the inactivity of animals from cold, must be different from sleep. Besides, all the operations of perfect life, as digestion, sensation, &c. are going on in the time of natural sleep, at least in the perfect animals; but none of these operations are performed in the torpid state of animals.

To see if the result of these experiments upon dormice was peculiar to that species, I wished to repeat the same experiments upon common mice; for which purpose, in

Experiment XVII, I made use of one strong and vigorous; and the atmosphere being at 60° , I introduced the thermometer into the abdomen; the ball being at the diaphragm, the quicksilver was raised to 99° ; but at the pelvis only to 96° and three quarters.

Here there was a real difference of about 9° between the dormouse and the common, the dormouse only raising it to 80° , in two animals of the same size, in some degree of the same genus, and at the same season of the year, and the atmosphere of nearly the same temperature.

Experiment XVIII. The same mouse was put into a cold atmosphere of 13° , for an hour, and then the thermometer was introduced as before; but the animal had lost heat, for the quicksilver at the diaphragm was raised only to 83° , in the pelvis to 78° .

Here the real heat of the animal was diminished 16° at the diaphragm, and 18° in the pelvis, while in the dormouse it gained five degrees, but lost upon a repetition.

Experiment XIX. In order to determine whether an animal that is weakened has the same powers, with respect to preserving heat and cold, as one that is vigorous and strong, I weakened a mouse by fasting, and then introduced the ball of the thermometer into its belly; the ball being at the diaphragm, the quicksilver rose to 97° ; in the pelvis to 95° , being

two degrees colder than the strong mouse: the mouse being put into an atmosphere as cold as the other, and the thermometer again introduced, the quicksilver stood at 79° at the diaphragm, and at 74° in the pelvis.

In this experiment the heat at the diaphragm was diminished 18° , in the pelvis 21° .

This greater diminution of heat in the second, than in the first, we may suppose proportional to the decreased power of the animal, arising from want of food.

To determine how far different parts of other animals than those already mentioned, were of different degrees of heat, I made the following experiments upon a healthy dog.

Experiment XX. The ball of the thermometer being introduced two inches within the rectum, the quicksilver rose to 100° and a half. The chest of the dog was then opened, and a wound made into the right ventricle of the heart; and immediately on the ball being introduced, the quicksilver rose to 101° exactly. A wound was next made some way into the substance of the liver; and the ball being introduced, the quicksilver rose to 100° and three quarters. It was next introduced into the cavity of the stomach, where it stood exactly at 101° . All these experiments were made within a few minutes.

Experiment XXI. The thermometer was introduced into the rectum of an ox, and the quicksilver rose exactly to 99° and a half.

Experiment XXII. This was also repeated upon a rabbit, and the quicksilver rose to 99° and a half.

From experiments on mice, and upon the dog, it plainly appears, that every part of an animal is not of the same degree of heat; and hence we may reasonably infer, that the heat of the vital parts of man is greater than either the mouth, rectum, or the urethra.

To determine how far my idea was just, that the heat of animals varied in proportion to their degree of perfection, I made the following experiments upon fowls, which I considered as one remove below what are commonly called quadrupeds.

Experiment XXIII. I introduced the ball of the thermometer suc-

cessively into the intestinum rectum of several hens, and found that the quicksilver rose as high as 103° , 103° and a half; and in one of them to 104° .

Experiment XXIV. I made the same experiments on several cocks, and the result was the same.

Experiment XXV. To determine if the heat of the hen was increased when she was prepared for incubation, I repeated the twenty-third experiment upon several sitting or clucking hens; in one the quicksilver rose to 104° ; in the other to 103° and a half; 103° , as in the twenty-third experiment.

Experiment XXVI. I placed the ball of the thermometer under the same hen, in whose rectum the quicksilver was raised to 104° , and found the heat as great as in the rectum.

Experiment XXVII. Having taken some of the eggs from under the same hen, where the chick was about three parts formed, I broke a hole in the shell, and introducing the ball of the thermometer, found that the quicksilver rose to 99° and a half. In some that were addled, I found the heat not so high by two degrees; so that the life in the living egg assisted in some degree to support its own heat.

Is the increase of three or four degrees of heat, which is the difference found between the fowl and the quadruped, for the purpose of incubation? The heat in the eggs, which was caused and supported by that of the fowls, was not above the standard of the quadrupeds; and it would probably have been less, if the heat of the hen had not been so great.

Finding from the above experiments, that fowls were some degrees warmer than that class commonly called quadrupeds (although certainly less perfect animals) I chose to continue the experiments upon the same principle; and made the following upon those of a still inferior order. The next remove from the fowl being what is commonly called the amphibious.

Experiment XXVIII. I introduced the thermometer into the stomach, and afterwards into the anus of a healthy viper, and the quicksilver rose from 58° (the heat of the atmosphere in which it was) to 68° ; so that it was ten degrees warmer than the common atmosphere.

Experiment XXIX. Having ascertained the heat of the water in a pond, in which there were carp, to be 65° and a half, I took a carp out of this water, and having introduced the thermometer into its stomach, the quicksilver rose to 69° ; so that the difference between the water and the fish was only three degrees and a half.

Experiment XXX. The heat of the atmosphere at 56° , some earth-worms were put into a glass vessel, and a thermometer being immersed among them, the quicksilver stood at 58° and a half.

This experiment was repeated, the atmosphere at 55° , and the worms were found to be 57° .

Experiment XXXI. The atmosphere at 54° , four black slugs were put into a small vessel, and a thermometer immersed among them, stood at 55° and a quarter.

Experiment XXXII. The atmosphere at 56° , three leaches were put into a small glass vessel, and a thermometer immersed among them, stood at 57° .

This experiment was repeated, the atmosphere at 54° , when the thermometer stood at 55° and a half.

To see how far the colder animals had a power of preserving their standard heat, when exposed to severe cold, I made the following experiments.

Experiment XXXIII. A viper, whose heat was 68° , was put into a pan, and the pan into a cold mixture of about 10° ; after remaining there about ten minutes, had its heat reduced to 37° . Being allowed to stay ten minutes longer, the mixture at 13° , its heat was reduced to 35° . It was continued ten minutes more in the mixture at 20° , and its heat was reduced to 31° , nor did it sink lower; its tail beginning to freeze; and the animal now becoming very weak. It may be remarked, that it cooled much slower than many of the animals mentioned in the following experiments.

The frog being, in its structure, more similar to the viper than to either the fowl or fish, I made the following experiments on that animal.

Experiment XXXIV. I introduced the ball of the thermometer into

its stomach, and the quicksilver stood at 44° . I then put the frog into a cold mixture, and the quicksilver sunk to 31° ; the animal appeared almost dead, but recovered very soon: beyond this point it was not possible to lessen the heat, without destroying the animal. But its decrease of heat was quicker than in the viper, although the mixture was nearly the same.

The next experiments were made on fishes.

Experiment XXXV. In an eel, the heat in the stomach, which at first was at 37° , sunk, after it had been some time in the cold mixture, to 31° . The animal at that time appeared dead, but was found to be alive the next day.

Experiment XXXVI. In a snail, whose heat was at 44° , it sunk, after it had been put into the cold mixture, to 31° , and then the animal froze.

Experiment XXXVII. Several leaches having been put into a bottle, and the bottle immersed in the cold mixture, the ball of the thermometer being placed in the middle of them, the quicksilver sunk to 31° ; and by continuing the immersion for a sufficient time to destroy life, the quicksilver rose to 32° , and then the leaches froze. In all these experiments, the animals when thawed were found dead.

Finding that animals of the imperfect classes will, without life being totally extinguished, admit of their heat being reduced to that point at which the dead solids and fluids freeze; but if sunk much below that, death must be the consequence; I wished therefore to be able to determine to what degree the heat of the animal could be raised.

Experiment XXXVIII. A healthy viper was placed in an atmosphere heated to 108° , and allowed to stay seven minutes; when the heat of the animal, in the stomach and anus, was found to be 92° and a half; beyond which it could not be raised in the above state of the atmosphere. The same experiment was made upon frogs, with nearly the same result.

Experiment XXXIX. An eel, very weak, its heat at 44° , which was nearly that of the atmosphere, was put into water heated to 65° , for fifteen minutes; and, upon examination, it was found of the same degree of heat with the water.

Experiment XL. A tench, whose heat was 41° , was put into water at 65° ; and left there ten minutes; the ball of the thermometer being introduced both into the stomach and rectum, the quicksilver rose to 55° . These experiments were repeated with nearly the same result.

To determine whether life had any power of resisting heat and cold in inferior classes of animals, I made comparative trials between living and dead ones.

Experiment XLI. I took a living and a dead tench, and a living and a dead eel, and put them into warm water; they all received heat equally fast; and when they were exposed to cold, both the living and the dead admitted the cold likewise with equal quickness.

I had long suspected, that the principle of life was not wholly confined to animals, or animal substance endowed with visible organization and spontaneous motion; but supposed, that the same principle might exist in animal substances, devoid of apparent organization and motion, when the power of preservation was simply required.

I was led to this opinion about twenty years ago, when busied in making drawings of the growth of the chick in the process of incubation. I then observed, that whenever an egg was hatched, the yolk (which is not diminished in the time of incubation) was always perfectly sweet to the very last; and that the part of the albumen, which has not been expended on the growth of the animal, some days before hatching, was also perfectly sweet, although both were kept in a heat of 103° in the hen's egg for three weeks, and in the duck's for four; but I observed, that if an egg was not hatched, that egg became putrid in nearly the same time with any other dead animal matter.

To determine from other tests how far eggs possessed a living principle, I made the following experiments.

Experiment XLII. After having placed an egg in a cold about 0, till it froze, I allowed it to thaw; by which process it was to be supposed the preserving powers of the egg must be destroyed. I then put this egg into the cold mixture, and with it one newly laid, and found the difference in

freezing was seven minutes and a half, the fresh one, so much longer time resisting the powers of cold.

Experiment XLIII. A new laid egg being put into a cold atmosphere, fluctuating between 17° and 15° , took above half an hour to freeze; but when thawed and put into an atmosphere at 25° , it froze in half the time. This experiment was repeated frequently with nearly the same result.

To ascertain the comparative degree of heat between a living and a dead egg, and also to determine whether a living egg be subject to the same laws with the more imperfect animals, I made the following experiments.

Experiment XLIV. A fresh egg, and one which had been frozen and thawed, were put into the cold mixture at 15° ; the thawed one soon came to 32° , and began to swell and congeal; the fresh one sunk to 29° and a half, and in twenty-five minutes later than the dead one, it rose to 32 , and began to swell and freeze.

In this experiment, the effect on the fresh egg was similar to that produced on the frog, eel, snail, &c. where life allowing the heat to be diminished two or three degrees below the freezing point, afterwards resisted all further decrease; but the powers of life being expended by this exertion, the parts froze like any other dead animal matter.

From these experiments it appears, that a fresh egg has the power of resisting heat, cold, and putrefaction, in a degree equal to many of the more imperfect animals; and it is more than probable, this power arises from the same principle in both.

From the circumstance of those imperfect animals (upon which I made my experiments) varying their heat so readily, we may conclude, that heat is not so very essential to life in them as in the more perfect; although it be essential to many of the operations, or what may be called the secondary actions of life, such as digesting food*, and propagating the species, both which, especially the last, requiring the greatest powers

* How far this idea holds good with fishes, I am not certain.

an animal can exert. The animals which we call imperfect being chiefly employed in the act of digestion, we may suppose their degree of heat to be only what that action requires; it not being essentially necessary for the life of the animal that heat should ever rise so high in them as to call forth the powers necessary for the propagation of the species.* Whenever therefore these imperfect animals are exposed to a cold so great as to weaken their powers, and disable them from performing the first of these secondary actions, they in some measure cease to be voluntary agents, and remain in a torpid state during that extreme degree of cold which always occurs during some part of the winter in the countries they inhabit; and the food of such animals not being in general produced in the cold season, is a reason why this torpidity becomes in some measure necessary.

From the heat of such animals sinking to the freezing point, or even lower, and then becoming stationary; and the animal not being able to support life in a much greater degree of cold for any length of time, we see a reason why they should always endeavour to procure places of abode in the winter where the cold seldom sinks to that point. We find toads burrowing, frogs living under large stones, snails seeking shelter under stones and in holes, and fishes having recourse to deep water; the heat of all those places being generally above the freezing point even in our hard-

* The hedge-hog may be called a truly torpid animal; and we find that its actual heat is diminished when the actions are not vigorous. From a general review of this whole subject it would appear, that a certain degree of heat in the animal is necessary for its various œconomical operations, among which is digestion; and that necessary heat will be according to the nature of the animal, and, probably, the nature of the operations to be performed. A frog will digest food when its heat is at 60°, but not when at 35° or 40°; and it is very probable that, when the heat of the bear, hedge-hog, dormouse, bat, &c. is reduced to 70°, 75°, or 80°, they lose their power of digestion; or rather that the body, in such a degree of cold, has no call upon the stomach. That animals, in a certain degree of heat, must always have food, is further illustrated by the instance of bees. The construction of a bee is very similar to a fly, a wasp, &c. A fly and a wasp can allow their heat to diminish, as in the fish, snake, &c. without losing life, but a bee cannot; therefore a bee is obliged to keep up its heat as high as what we call its digestive heat, but not its propagating; for which purpose they provide against such cold as would deprive them even of their digestive heat, if they had not food to preserve it.

est frosts ; which are however sometimes so severe, as to kill many whose habitations are not well chosen.

When the frost is more intense and of longer standing than common, or in countries where the winters are always severe, there is generally snow on the ground, and the water freezes : the advantage arising from these two circumstances is great ; the snow serving as a blanket to the earth, and the ice to the water*.

As all the experiments I ever made upon the freezing of animals, (with a view to see if it were possible to restore the actions of life when thawed) were tried upon whole ones ; as I never saw life return by thawing[†], and wished to see how far parts were similar to the whole in this respect ; it being asserted, and with some authority, that parts of a man may be frozen, and afterwards recover, I made, for this purpose, the following experiments upon an animal of the same order as ourselves.

In January 1777, I mixed salt and ice till the cold was about 0 ; in the side of the vessel was a hole, through which I introduced the ear of

* Snow and ice are perhaps the worst conductors of heat of any substance yet known. In the first place, they never allow their own heat to rise above the freezing point, so that no heat can pass through ice or snow when at 32°, by which means they become an absolute barrier to all heat that is at or above that degree ; hence the heat of the earth, or whatever substance they cover, is retained : but they are conductors of heat below 32°. Perhaps that power decreases in proportion as the heat decreases under that point.

In the winter 1776, a frost coming on, the surface of the ground was frozen ; but a considerable fall of snow fell, and continued several weeks : the heat of the atmosphere, during the time, was often at 15° ; but so little did the frost affect the ground underneath, that the surface of the ground thawed, and the earth retained the heat of 34°, in which beans and peas will grow.

The same thing took place in a pond where the water was frozen on the surface to a considerable thickness ; a large quantity of snow having fallen, and covered the ice, the heat of the water was preserved ; the ice thawed, and the snow, at its under surface, was found mixed with the water.

The heat of the water under the snow was at 35°, in which fishes lived very well.

It would be an attempt worthy the attention of the philosopher, to investigate the cause of the heat of the earth, upon what principle it is preserved, &c.

† Vide Phil. Trans. for the year 1775, vol. LXV, part II, p. 446.

a rabbit; and to carry off the heat as fast as possible, it was held between two flat pieces of iron that went further into the mixture. That part of the ear projecting into the vessel became stiff, and when cut did not bleed; the part divided by the scissars flying from between the blades like a hard chip.

The ear having remained in the mixture nearly an hour, soon thawed when taken out, began to bleed, and became so very flaccid, as to double upon itself, from losing its natural elasticity. When out of the mixture nearly an hour, it became warm, and this warmth increasing to a considerable degree, it also began to thicken, in consequence of inflammation, while the other ear continued in its usual degree of cold. The day following the frozen ear was still warm; and even two days after retained its heat and thickness, which continued for many days after.

About a week after this, the mixture being the same as in the former experiment, I introduced the ears of the same rabbit through the hole, and froze them both: the sound one however froze first, probably from its being considerably colder at the beginning. When withdrawn they soon thawed, both soon became warm, and the fresh ear thickened as the other had done before.

Such a change in the parts does not always take place so quickly; for on repeating the experiment on the ear of another rabbit till it became as hard as a board, it was found to be longer in thawing than in the former experiment, and much longer before it became warm; in two hours however, it had acquired some degree of warmth; and on the day following was hot and thickened.

In the spring, 1776, I perceived that my cocks in the country had their combs smooth, with an even edge, and not so broad as formerly, appearing as if near one half of them had been cut off. Having inquired into the cause of this, my servant told me, that it had happened in that winter during the hard frost. He having then observed, that the combs had in part dropped off: also, that the comb of one cock had entirely separated; but this I did not see, as by accident he was burnt to death. I naturally imputed this effect to the combs having been frozen to so

great a degree during the severe weather, as to have the life of the part destroyed: to determine, therefore, by experiment, the solidity of this reasoning, I made the following experiment.

I selected, for the purpose, a very large young cock, having a comb of considerable breadth, with deep serrated edges, the processes of which were full half an inch long. My attempts to freeze the substance of the comb did not succeed; for that being thick and warm, resisted the effects of the cold, and only the serrated edges were frozen. The frozen parts became white and hard; and, when I cut off a little bit, did not bleed, nor did the animal show any signs of pain. I next immersed in the cold mixture one of his wattles, which was very broad and thin; it froze very readily, and upon thawing both the comb and wattle, they became warm, but of a purple colour, having lost that transparency which remained in the other parts of the comb and in the other wattle. The wound in the comb now bled freely.

Both comb and wattle recovered perfectly in about a month. The natural colour returned first nearest to the sound parts, increasing gradually till the whole had acquired a healthy appearance.

There was a very material difference in the effect between those fowls, the serrated edges of whose combs I suspected to have been frozen in the winter of 1765-6, for they must have dropped off. The only way in which I can account for this difference is, that in those fowls the parts were kept so long frozen, that the unfrozen or active parts had time to inflame, and had brought about a separation of the frozen parts, treating them exactly as dead, similar to a mortified part; and that before they thawed, the separation was so far completed as to deprive them of further support.

As it is confidently asserted, that fishes are often frozen, and again return to motion; and as I had never succeeded in any of my trials of the kind upon whole fishes, I made some experiments upon particular parts, to which I was led by having found a material difference in the result of experiments made upon the whole, and on parts of the more perfect animals:

I froze the tail of a tench, as high as the anus, which became as hard as a board; when thawed, that part was whiter than common; and when it moved, the whole tail moved as one piece, and the termination of the frozen part appeared like the joint on which it moved.

On the same day I froze the tails of two gold fishes till they became as solid as a piece of wood. They were put into cold water to thaw, and appeared for some days to be very well; but that part of the tails which had been frozen had not the natural colour, and the fins of the tails became ragged. About three weeks after, a fur came all over the frozen parts; their tails became lighter, so that the fishes were suspended in the water perpendicularly; they had almost lost the power of motion; and at last died. The water in which they were kept was New River water, shifted every day, and in quantity about ten gallons.

I made similar experiments upon an order of animals still inferior, viz. common earth-worms.

I first froze the whole of an earth-worm as a standard; when thawed it was perfectly dead.

I then froze the anterior half of another earth-worm; but the whole died.

I next froze the posterior half of an earth-worm; the anterior half continued alive, and separated itself from the dead part.

From some of these experiments it appears, that the more imperfect animals are capable of having their heat and cold varied very considerably, but not according to the degree of heat or cold of the surrounding medium in which they can support life; for they can live in a cold considerably below the freezing point, and yet the living powers of the animal will not allow their heat to be diminished much beyond 32° . Whenever the surrounding cold brings them so low, the power of generating heat takes place; and if the cold is continued, the animals exert this power till life is destroyed; after which they freeze, and are immediately capable of admitting any degree of cold.



Plate XII.

Fig. I.



Fig. II.



P L A T E XII.

TWO views of a thermometer which has the scale so constructed as to admit of its being introduced into any cavity that can receive the ball. The scale is moveable ; but the freezing point is marked on the stem or glass.

FIGURE I. A front view, exposing the glass stem of the thermometer, through which the divisions marked upon the concave surface of the sliding ivory scale which embraces it, are very distinctly seen.

a The freezing point, which is marked upon the stem by a scratch on the glass.

FIGURE II. A side view, showing the degrees marked near the edge of the convex side of the ivory scale.

The thermometer is to be adjusted for measuring high or low degrees of heat, by bringing any number marked upon the scale opposite the freezing point, and counting either upwards or downwards.

PROPOSALS FOR THE RECOVERY OF PERSONS APPARENTLY DROWNED.

HAVING been requested by a principal member of the society established for the recovery of persons apparently drowned, to commit my thoughts on that subject to paper, I readily complied, hoping, that although I have had no opportunities of making actual experiments upon drowned persons, it might be in my power to throw some lights on a subject so closely connected with the inquiries which, for many years, have been my business and favourite amusement: I therefore collected together my observations and experiments relative to the loss and recovery of the actions of life, which I now offer to the public. The endeavour to recover persons apparently drowned, is a new practice, and has furnished, as yet, few important and clear facts: our knowledge of the animal œconomy is so imperfect, that I am afraid our reasoning from that alone, must not be relied on, in a question so interesting to the cause of humanity. But let us reason, as well as we can, from the few data we have; and let every man bring forward, freely, the observations he has made, that the subject thus fairly before the public, may in time, by its united efforts, be more perfectly understood.

I shall consider an animal, apparently drowned, as not dead; but that only a suspension of the actions of life has taken place. The difference between a suspension of the actions of life, and absolute death, is well illustrated by the common snail when drowning. If a snail is immersed in water and kept there, certain voluntary and instinctive actions take place; but after remaining a certain time covered by the water, all these actions cease. Hence the animal being relaxed, naturally comes out of the shell in that state; its stomach is filled with water, and the body appears larger than natural; but without motion. These actions continue

thus suspended, till either the cause of suspension be removed, or some other stimulus shall bring the parts into action : but under such circumstances life cannot be preserved for any considerable length of time ; and when the stimulus which precedes death takes place, the whole animal is thrown into action, and in that contracted state, possibly, absolute death is produced. A state of relaxation should therefore (where an universal violence has not been committed) be considered as the criterion of life ; and even in such cases, should be for some time admitted as a probable reason for supposing life still to exist.

If an animal appears so far dead as to have lost all the actions characteristic of life, yet a certain degree of action in all the parts will be produced when absolute death is taking place ; and that animal, being still susceptible of stimulus, is recoverable if the proper stimulus could be applied.

It is asserted that men have recovered the actions of life, even after the contraction, in consequence of the stimulus which precedes death, has taken place. If this be true, which I very much doubt, the stimulus must first produce relaxation, which is an action dependent on life.

This is probably the case in the first appearances of death from all violent accidents, except those caused by lightning, electricity, an universal shock, a blow on the stomach, a violent affection of the mind, or some other modes by which absolute death may be instantaneously produced ; which all appear to act in the same way, producing absolute and instant death. For in cases which have fallen under my observation, the concomitant circumstances have resembled those which attend death caused by lightning or electricity ; such as a total and instantaneous privation of sense and motion without convulsions ; consequently, no rigor of muscles having been produced, and the blood remaining uncoagulated ; differing entirely in these respects from what appears in persons deprived of sense and life by any injury done the brain. It seems only possible to account for this effect of a blow on the stomach, from the connection subsisting between that viscus and every part of the body, at least with vital parts ; the blow, most probably, causing instant death in that organ, of which

the death of the whole animal is the consequence*. When death takes place from violent affections of the mind, it must be referred to the universal influence which the mind has over the body.

To ascertain when a body is deprived of life, it is first necessary to know in what manner apparent death took place; whether in the common way, or from the vital actions being too long suspended. In either case, stiffness of the muscles is probably the most certain and most evident proof of absolute death; since that arises from the stimulus immediately preceding death having taken place. But if the privation of life is produced by any of the modes abovementioned, which kill instantaneously and universally, the stimulus which produces stiffness is not allowed time to act, and the muscles are all left in a relaxed state. Yet this state of relaxation must not, on that account be always considered as a proof of life still remaining.

A degree of flaccidity in the eye-balls, which produces glassiness, is a certain mark of death; but is, however, only a secondary mode of ascertaining it in those instances where the body becomes stiff; but may be the first mode where absolute death takes place instantaneously; and putrefaction will be the second; while in the other cases putrefaction will be the third.

That I may more fully explain my ideas upon this subject, it will be necessary to state some propositions.

First; that so long as the animal retains the susceptibility of impression, though deprived of the action of life, it will, most probably, retain the power of action when impressed; therefore the action may frequently be suspended, and yet recoverable; but, when the susceptibility of impres-

* I should consider the situation of a person drowned to be similar to that of a person in a trance. In both, the action of life is suspended, without the power being destroyed: but I am inclined to believe, that a greater proportion of persons recover from trances than from drowning; because a trance is the natural effect of a disposition in the person to have the action of life suspended for a time; but drowning being produced by violence, the suspension will more frequently last for ever, unless the power of life is roused to action by some applications of art.

sion is destroyed, the action ceases to be recoverable. Secondly; it is necessary to mention, that I consider part of the living principle as inherent in the blood*. Thirdly; that the stomach sympathizes with every part of an animal, and that every part sympathizes with the stomach; therefore, whatever acts upon the stomach as a cordial, or rouses its natural and healthy actions, and whatever affects it so as to produce debility, has an immediate effect upon every part of the body. The last proposition I have to make is, that every part of the body sympathizes with the mind; for whatever affects the mind, the body is affected in proportion. These sympathies are strongest with the vital parts; but besides these universal sympathies between the stomach, the mind, and all parts of the body, there are peculiar sympathies; of which the heart sympathizing immediately with the lungs, is an instance. If any thing is received into the lungs, which is a poison to animal life, such as inflammable air, volatile vitriolic acid, and many other well known substances, the motion of the heart immediately ceases, even much sooner than if the trachea had been tied; and, from experiments, it appears, that any thing salutary to life, applied to the lungs, will restore the heart's action after it has been at rest some time.

I shall divide violent deaths into three kinds: First, where a stop is only put to the action of life in the animal, but without any irreparable injury to a vital part; which action, if not restored in a certain time, will be irrecoverably lost. The length of that time is subject to considerable variation, depending on circumstances with which we are at present unacquainted. The second is, where an injury is done to a vital part; as by taking away blood till the powers of action are lost; or by a wound or pressure being made on the brain or spinal marrow, while life remains

* That the living principle is inherent in the blood, is a doctrine which the nature of this account will not allow me to discuss: thus much, however, it may be proper to say, that it is founded on the result of many observations and experiments. But it may be thought necessary I should here give a definition of what I call the living principle: so far then as I have used the term, I mean to express that principle which preserves the body from dissolution with or without action, and is the cause of all its actions.

in the solids sufficient for the preservation of the animal, if action could be restored to the vital parts. The third is, where absolute death instantly takes place in every part, as is often the case in strokes of lightning; in the common method of killing eels, by throwing them on some hard substance, in such manner as that the whole length of the animal shall receive the shock at the same instant; by a blow on the stomach; by violent affections of the mind, and by many diseases, in all which cases the muscles remain flexible*.

How far that may be strictly considered as a violent death, which is caused by affections of the mind, I will not pretend to say; but if it is to have a place in that class, it must be ranked with those which happen from lightning, and a blow on the stomach: and in most cases of persons drowned, I can easily conceive the mind to be so much affected, prior to the immersion, and in the moment immediately succeeding it, as to make a material difference in the power of recovery. In many sudden deaths, arising from violence, and even from disease, death shall take place so immediately, that the muscles neither contract, nor does the blood coagulate.

The present consideration is, under which of the kinds of violent death drowning can be classed or arranged? I am of opinion it will most commonly come under the first; and upon that ground I shall principally consider the subject, always supposing the body to remain flaccid.

The loss of motion in drowning seems to arise from the loss of respiration, and the immediate effects which that has upon the other vital motions of the animal; except what may have arisen from the affections of the mind. The privation of breathing appears, however, to be the first cause; and the heart's motion ceasing, to be the second or consequent; therefore most probably the restoration of breathing is all that is necessary to restore the heart's motion: for if sufficient life still exists to produce

* On the other hand, when an eel is killed by chopping it into a number of pieces, the powers of life are by those means roused into action; and, as every part dies in that active state, every part is found stiff after death. This explains the custom of cutting fish into pieces while yet alive, in order to make them hard, usually known by the name of crimping.

that effect, we may suppose every part equally ready to move the very instant in which the action of the heart takes place, their actions depending so much upon it. What makes it very probable, that in recovering persons drowned, the principal effect depends upon air being thrown into the lungs, is, what happens at the birth of children, when too much time has intervened between the interruption of that life which is peculiar to the fœtus, and that which depends on breathing; they then lose altogether the disposition for this new life; and in such cases, there being a total suspension of the actions of life, the child remains to all appearance dead; and would certainly die, if air were not thrown into its lungs, and by such means the first principle of action restored. To put this in a still clearer light, I will give the result of some experiments which I made in the year 1755 upon a dog.

A pair of double bellows were provided, constructed in such a manner as by one action to throw fresh air into the lungs, and by another to suck out again the air which had been thrown in by the former, without mixing them together. The muzzle of these bellows was fixed into the trachea of a dog, and by working them he was kept perfectly alive. While this artificial breathing was going on, I took off the sternum of the dog, and exposed the lungs and heart; the heart continued to act as before, only the frequency of its action was considerably increased. When I stopped the motion of the bellows, the heart became gradually weaker and less frequent in its contractions, till it entirely ceased to move. By renewing the action of the bellows, the heart again began to move, at first very faintly, and with long intermissions; but by continuing the artificial breathing, its motion became as frequent and as strong as at first. This process I repeated upon the same dog ten times, sometimes stopping for five, eight, or ten minutes, and observed that every time I left off working the bellows, the heart became extremely turgid with blood, the blood in the left side becoming as dark as that in the right; which was not the case when the bellows were working. These situations of the animal appeared to me exactly similar to drowning.

Death in persons drowned has been accounted for, by supposing that

the blood, rendered unfit for the purposes of life, by being deprived of the action of the air in respiration, is sent in a vitiated state to the brain and other vital parts; by which means the nerves lose their effect upon the heart, and the heart in consequence its motion. This, however, I am fully convinced is false; first, from the experiments on the dog, in which a large column of blood so vitiated (consisting of what had been propelled from the heart, after respiration stopped, and might be supposed the cause of the heart ceasing to act, together with all that remained in the heart and pulmonary veins) was again pushed forward without any ill effect having been produced: and next, from the return to life of persons drowned and children still-born, which, were such a supposition true, could never happen; unless we imagine a change of the blood to take place in the brain, prior to the restoration of the heart's motion. This restoration must therefore depend immediately on the application of air to the lungs, and not on the effects which air has upon the blood, or that blood upon the vital parts.

If the affections of the mind have had any share in the cessation of action in the heart, its motion will not be so easily restored as in other cases. In our attempts to recover those who have been drowned, it might therefore be proper to inquire, if there had been time sufficient for the person to form any idea of his situation, previous to his being plunged into the water; as it is not unlikely, that the agitated state of mind might assist in killing him; and in such case I should very much doubt the probability of restoring him to life. In the history of those who have, and who have not been recovered, could the difference be ascribed to any such cause, it might lead to something useful; as in those who have had an intention to destroy themselves, a great difference in the chance of recovery may arise, from the mind having been previously very much affected.

It frequently happens, in the case of drowning, that assistance cannot be procured till a considerable time after the accident; every moment of this delay renders recovery more precarious, the chances of which are not only diminished in the parts where the first powers of action principally reside, but also in every other part of the body.

In offering my sentiments on the method of treating persons who are apparently drowned, I shall say first, what I would recommend to have done ; secondly, what I would wish might be avoided.

When assistance is called in, soon after the immersion, perhaps blowing air into the lungs may be sufficient to effect a recovery* ; but if a considerable time, as an hour, has been lost, it will seldom be sufficient ; the heart, in all probability, having by that time lost its intimate connection with the lungs. It will in these cases, therefore, be proper to apply, mixed with the air, such stimulating medicines as the vapour of volatile alkali ; which may easily be done, by holding spirits of hartshorn in a cup under the receiver of the bellows. I would advise the air and volatile alkali to be thrown in by the nose, rather than the mouth, as the last mode of administering, by producing sickness, is more likely to depress than rouse the living principle. It will be still better if it can be done by both nostrils, as applications of this kind to the olfactory nerves certainly rouse the living principle and put the muscles of respiration into action, and therefore are the more likely to excite the action of the heart : besides, that affections of these nerves are known to act more immediately on the living principle ; since while a strong smell of very sweet flowers, as orange flowers, will in many cause fainting, the application of vinegar will as immediately restore the powers to action again. All perfumes in which there is some acid, rather rouse than depress, as the sweet-brier, essence of lemon, &c. If during the operation of the bellows, the larynx be gently pressed against the œsophagus and spine, it will prevent the stomach and intestines being too much distended by the air, and leave room for the application of more effectual stimuli to those parts. This pressure, however, must be conducted with judgment and caution, so that the trachea and the aperture into the larynx may both be left perfectly free. While this business is going on, an assistant should prepare bed-cloaths, carefully brought to the proper degree of heat. I consider

* Perhaps the dephlogisticated air, described by Dr. Priestley, may prove more efficacious than common air. It is easily procured, and may be preserved in bottles or bladders for that purpose.

heat as congenial with the living principle ; increasing the necessity of action it increases action : cold, on the other hand, lessens the necessity, and of course the action is diminished : to a due proportion of heat, therefore, the living principle owes its vigour ; and, from observations and experiments, it appears to be a law of Nature, in animal bodies, that the degree of external heat should bear a proportion to the quantity of life ; when it is weakened, this proportion requires great accuracy in the adjustment ; while greater powers of life allow a greater latitude*.

I was led to make these observations, by attending to persons who are frost-bitten ; the effect of cold in such cases being that of lessening the living principle. The powers of action remain as perfect as ever, but weakened ; and heat is the only thing wanting to put these powers into action : yet that heat must at first be gradually applied, and proportioned to the quantity of the living principle ; which increasing, the degree of heat may likewise be increased. If this method is not observed, and too great a degree of heat is at first applied, the person or part loses entirely the living principle, and mortification ensues. Such a process invariably takes place with regard to men ; and the same thing, I am convinced, happens to other animals. For if an eel is exposed to a degree of cold sufficiently intense to benumb it till the remains of life are scarcely perceptible, and still retained in a cold of about 40° , this small proportion of living principle will continue for a considerable time without diminution or increase ; but if the animal is afterwards placed in a heat about 60° , after showing strong signs of returning life, it will die in a few minutes. Nor is this circumstance peculiar to the diminution of life by cold. The same phenomena take place in animals who have been very much reduced by hunger.

If a lizard, or snake, when it goes to its autumnal hiding place, is not sufficiently fat, the living powers are, before the season permits it to come out, very considerably weakened ; perhaps so much as not to admit of

* It is upon these principles that cold air is found of so much service to people who are reduced by disease, as the confluent smallpox, and fevers, by diminishing heat in proportion to the diminution of life ; or lessening the necessity of the body's producing its own cold.

the animal being again restored. If animals, in a torpid state, are exposed to the sun's rays, or placed in any situation which by its warmth would give vigour to those of the same kind, possessed of a larger share of life, they will immediately show signs of increased life; but quickly sink under the experiment and die; while others, reduced to the same degree of weakness, as far as appearances can discover, will live for many weeks, if kept in a degree of cold proportioned to the quantity of life they possess.

I observed, many years ago, in some of the colder parts of this island, that when intense cold had forced blackbirds or thrushes to take shelter in out-houses, such of them as had been caught, and were, from an ill-judged compassion, exposed to a considerable degree of warmth, died very soon. The reason of this I did not then understand; but I am now satisfied that it was owing, as in other instances, to the degree of heat being increased too suddenly for the proportion of life remaining in the animal.

From these facts it appears, that warmth causes a greater exertion of the living powers than cold; and that an animal in a weakly state may be obliged by it to exert a quantity of the action of life sufficient to destroy the very powers themselves*. The same effects probably take place even in perfect health; it appearing, from experiments made in a heated room, that a person in health, exposed to a great degree of heat, found the actions of life accelerated so much, as to produce at last faintness and debility†.

If bed-cloaths are put over the drowned person, so as scarcely to touch him, steam of volatile alkali, or of warm balsams and essential oils, may be so conveyed as to come in contact with many parts of his body; and it will certainly prove advantageous, if the same kind of steams can be conveyed into the stomach, as that seat of universal sympathy will be roused by such means. This may be done by a hollow bougie and a syringe; but the operation should be performed with all possible expedition; because the instrument, by continuing in the mouth may produce sickness,

* It is upon this principle that parts mortify in consequence of inflammation.

† Vide Phil. Trans. for the year 1775, vol. 65. p. 111.

an effect I should chuse to avoid, unless it is intended to produce the action of vomiting. Some of the stimulating substances, which are of a warm nature, and have an immediate effect, as spirits of hartshorn, peppermint-water, juice of horse-raddish, and many others which produce a more lasting stimulus in a fluid state, and are found to quicken the pulse of a man in health, as balsams and turpentine, may be thrown into the stomach; but the quantity must be small, as they have a tendency to produce sickness; for it may be imagined, that what would produce debility, or lessen action when in health, would, in opposite circumstances prevent actions from taking place. The application of steams, and other substances, should also be thrown up by the anus; and the process recommended under the first head of treatment should still be continued, while that recommended under the second is putting in practice, the last being only an auxiliary to the first. The first, in many cases, may succeed alone; but the second without the first, must, I think, always fail where the powers of life are considerably weakened. Motion may possibly be of service; it may at least be tried; but, as it has less effect than any other of the usually prescribed stimuli, it should be the last applied*. I would recommend to the operator the same care in regulating the application of every one of these methods, as I did before in that of heat; as each may have the same property of entirely destroying the feeble action which they have excited, if administered in too great a proportion: instead therefore of increasing and hastening the operations, on the first signs of returning life being observed, as is usually done, I should wish them to be applied more gently and gradually, that their increase afterwards may be directed, as nearly as possible, in a degree proportioned to the powers as they arise. As the heart is commonly the last part that

* Electricity has been known to be of service, and should be tried when other methods have failed. It is probable, the only method we have of immediately stimulating the heart; all other methods being more by sympathy. I have not mentioned injecting stimulating substances directly into the veins, though it might be supposed a proper expedient; because, in looking over my experiments on that subject, I found none where animal life received increase by that method.

ceases to act, it is probably the first part that takes on the action of recovery. When it begins to move, I would advise lessening the application of air to the lungs, and enjoin those employed to observe with great attention when the muscles of respiration begin to act, that our endeavours may not interfere with their natural exertions; yet that we may be still ready to assist. I would by all means discourage blood-letting; which I think weakens the animal principle, and life itself; consequently lessens both the powers and dispositions to action; and I would advise being careful not to call forth any disposition that might depress, by introducing things into the stomach, which ordinarily create nausea; as that also will have a similar effect, except it can be carried so far as to excite the action of vomiting, by which the stomach could relieve itself. It will be prudent, likewise, to avoid administering by the anus, any thing that may be likely to produce an evacuation that way; every such evacuation tending to lessen the animal powers. I have purposely avoided speaking of the fumes of tobacco, which always produce sickness or purging, according as they are applied.

Whoever is appointed for the purpose of recovering drowned persons, should have an assistant, well acquainted with the methods intended to be made use of; that while the one is going on with the first and most simple methods, the other may be preparing what else may be proper, so that no time may be lost between the operations; and this is the more necessary, as the first means recommended, will, in all cases, assist the second; and both together may often be attended with success, though each separately might have failed.

A proper apparatus is also essentially necessary to the institution; a description of which I here annex. First, a pair of bellows, so contrived, with two separate cavities, that by expanding them, when applied to the nostrils or mouth of a patient, one cavity may be filled with the common air, and the other with air sucked out from the lungs; and by shutting them again, the common air may be thrown into the lungs, and that which is sucked out of the lungs be discharged into the room. The pipe of these should be flexible, in length a foot or a foot and a half,

and at least three-eighths of an inch in width : as the artificial breathing may be continued by such means, while the other operations, except the application of the stimuli to the stomach, are going on ; which cannot conveniently be done if the nozzle of the bellows be introduced into the nose. The end next the nose should be double, and applied to both nostrils. Secondly, a syringe, with a hollow bougie, or flexible catheter, of sufficient length to go into the stomach, and convey any stimulating matter into it, without affecting the lungs. Thirdly, a small pair of bellows, such as are commonly used in throwing fumes of tobacco up the anus, by which stimulating fluids, or even fumes may be thrown in.

I shall conclude this account by proposing, that all who are employed in this practice be particularly required to keep an accurate journal of the means used, and the degree of success attending them ; whence we may be furnished with facts sufficient to enable us to draw conclusions, on which a certain practice may hereafter be established.

OBSERVATIONS TENDING TO SHEW THAT THE
WOLF, JACKAL, AND DOG, ARE ALL OF THE
SAME SPECIES.

THE true distinction between different species of animals must ultimately, as appears to me, be gathered from their incapacity of propagating with each other an offspring capable again of continuing itself by subsequent propagations : thus the horse and ass beget a mule capable of copulation, but incapable of begetting or producing offspring. If it be true, that the mule has been known to breed, which must be allowed to be an extraordinary fact, it will by no means be sufficient to determine the horse and ass to be of the same species ; indeed, from the copulation of mules being very frequent, and the circumstance of their breeding very rare, I should rather attribute it to a degree of monstrosity in the organs of the mule which conceived, as not being a mixture of two different species, but merely those of either the mare or female ass. This is not so far-fetched an idea, when we consider that some true species produce monsters, which are a mixture of both sexes, and that many animals of distinct sex are incapable of breeding at all. If then we find Nature in its most perfect state deviating from general principles, why may not it happen likewise in the production of mules ; so that sometimes a mule shall breed from the circumstance of its being a monster respecting mules ?

The time of uterine gestation being the same in all the varieties of every species of animals, it becomes a necessary circumstance towards determining a species.

The affinity between the fox, wolf, jackal, and several varieties of the dog, in their external form and several of their properties, is so striking, that they appear to be only varieties of the same species. The fox would seem to be farther removed from the dog than either the jackal or wolf,

at least in disposition, being naturally a solitary animal, and neither so sociable respecting its own species or man : from which I should infer that it is only allied to the dog by being of the same genus. It is confidently asserted by many, that the fox breeds with the dog ; but this has not been accurately ascertained : if it had, the enquiry would probably have been carried further ; and once breeding, according to what we have said, does not constitute a species ; this, however, is a part I mean to investigate. I do not know if, in a wild state, there ever is in the same country a variety in any species of animal ; but am inclined to think there never is : if so, as both wolves and foxes inhabited this country, they cannot then be of the same species.

Wolves, as also jackals, are found in herds ; and the jackal is so little afraid of the human species, that, like a dog, it comes into houses in search of food, more like a variety of the dog, the consequence of cultivation rather than of chance. It would appear to be much the most familiar of the two ; for we shall find, that in its readiness to copulate with the dog, and its familiarity with the dog afterwards, it is somewhat different from the wolf ; however, this may depend on accident. The wolf being an animal well known in Europe, the part of the world where natural history is particularly cultivated, some pains have been taken to ascertain, whether or not it was of the same species with the dog ; but, I believe, it has been hitherto considered as only belonging to the same genus.

Accident often does as much for natural history as premeditated plans, especially when Nature is left to itself. The first instance of the dog and wolf breeding in this country seems to have been about the year 1766. A Pomeranian bitch of Mr. Brookes's, in the New Road, was lined only once by a wolf, and brought forth a litter of nine healthy puppies. The veracity of Mr. Brookes is not to be doubted, respecting the bitch having been lined by a wolf ; yet, as it was possible she might have been lined by some common dog without his knowledge, the fact was not, in that, clearly made out ; but it has been since ascertained, that the dog and wolf will breed. One of the above-mentioned litter was presented to me by

Mr. Brookes, who likewise informed me that others had been purchased by different noblemen and gentlemen, and named Lord Clanbraffil as having bought a bitch-puppy. I reserved mine for the purpose of experiment; and from observation, it appeared that its actions were not truly those of a dog; having more quickness of attention to what passed; being more easily startled, as if particularly apprehensive of danger; quicker in transitions from one action to another; being not so ready to the call; and less docile. From these peculiarities it lost its life, having been stoned to death in the streets for a mad dog.

Hearing that Lord Clanbraffil's bitch had bred, Sir Joseph Banks was so obliging as, at my request, to write to his Lordship, who sent the following account.

“ SIR,

“ About seventeen or eighteen years ago, the late Lord Monthermer and I happened to see a dog-wolf at Mr. Brookes's, who deals in animals, and lives in the New Road. The animal was remarkably tame; and it struck us, for that reason, that a breed might be procured between him and a bitch.

“ We promised Mr. Brookes a good price for puppies, if he succeeded. In about a year a bitch produced nine, and Lord Monthermer bought one; and I had another, which was a bitch. Lord Monthermer's died of fits in about two years: mine lived longer, and had puppies only once. One I gave to Lord Pembroke; but what became of it I do not remember. It was grand-daughter of the wolf by the dam, and got by a large pointer of mine.

“ It might be considered, that Mr. Brookes's word was not sufficient proof that the puppies were really got by the wolf; but the appearance of the animals, so totally different from all others of the canine species, did not leave a doubt upon our minds; and I remember Hans Stanley, who had adopted Buffon's opinion, was thoroughly convinced upon seeing mine. The animals had the shape of the wolf refined; the fur long, but almost as fine as that of the black fox.

“ I am afraid I have trespassed too much upon your time, and will only beg you will be assured nothing can give me more pleasure than any opportunity of assuring you how truly

I am, Sir, &c.

Jan. 7, 1787.

CLANBRASSIL.”

Upon the supposition that Mr. Brookes's bitch was not lined by a dog, but by the wolf, which I think we have no reason to doubt, the species of the wolf is ascertained; but choosing to trace this matter still further, and hearing that Lord Pembroke's bitch had likewise bred, I was desirous to know the truth of it; and as his lordship was in France, I took the liberty of writing to Lord Herbert, and received the following answer.

“ S I R,

WILTON-HOUSE, DEC. 20, 1786.

“ The half-bred wolf-bitch you allude to was given, as I always understood, to Lord Pembroke by Lord Clanbrassil. She might, perhaps, have been bought at Brookes's by him. She had four litters, one of ten puppies, by a dog between a mastiff and a bull-dog. One of these was given to Dr. Eyre, at Wells in Somersetshire, and one to Mr. Buckett, at Stockbridge. The second litter was of nine puppies, some of which were sent to Ireland, but to whom I know not. This litter was by a different dog, but of the same breed as the first. The third litter was of eight puppies, by a large mastiff. Two of these were, I believe, sent to the present Duke of Queensberry. The fourth litter consisted of seven puppies; two of which were sent to M. Cerjat, a gentleman who now resides at Lausanne in Switzerland, and is famous for breaking dogs remarkably well. These two puppies were, however, naturally so wild and unruly, that he found it impossible to break them. She died four years ago, and the following inscription was put over the place where she is buried in this garden, by Lord Pembroke's orders.

Here lies Lupa,
 whose grandmother was a wolf,
 whose father and grand-father were dogs, and whose
 mother was half wolf and half dog. She died
 on the 16th of October, 1782,
 aged 12 years.

“ I am sorry it is not in my power to give you any better account ; but if you think proper to write to Lord Pembroke, who is at Paris, I am convinced he will be very happy to give you any further information.

I am, &c.

HERBERT.”

Buffon, whose remarks in natural history are well known, made experiments to ascertain how far the wolf and dog were of the same species, but without success. He says, “ A she-wolf, which I kept three years, altho’ shut up very young, and along with a greyhound of the same age, in a spacious yard, could not be brought to agree with it, nor endure it, even when she was in heat. She was the weakest, yet the most mischievous ; provoking, attacking, and biting the dog, which at first only defended itself, but at last killed her.” And in another part of his work, he makes the following observation : “ The dog, the wolf, the fox, and the jackal, form a genus, of which the different species are really so nearly allied to each other, and of which the individuals resemble each other so much, particularly by the internal structure and parts of generation, that it is difficult to conceive why they do not breed together*.”

* In the Supplement to his Works, he gives the following account which had been sent to him. “ A very young she-wolf, brought up at the Marquis of Spontin’s, at Namur, had a dog, of nearly the same age, kept with it as a companion. For two years they were at liberty, coming and going about the apartments, the kitchen, the stables, &c. lying under the table, and upon the feet of those who sat round it. They lived in the greatest familiarity.

“ The dog was a strong greyhound. The wolf was fed on milk for six months ; after that, raw meat was given her, which she preferred to that which was dressed. When she ate

This part of natural history lay dormant till Mr. Gough, who sells birds and has a collection of animals on Holborn-Hill, repeated the experiment on a wolf-bitch, which was very tame, and had all the actions of a dog under confinement. A dog is the most proper subject for comparison, as we have opportunities of being acquainted with its disposition and mode of expressing its sensations, which are most distinguishable in the motion of the ears and tail; such as pricking up the ears when anxious, wishing, or in expectation; depressing them when supplicant, or in fear; raising the tail in anger or love, depressing it in fear, and moving it laterally in friendship; and likewise in raising the hair on the back from many affections of the mind. This animal became in heat in the month of December 1785; and Mr. Gough having an idea of obtaining a breed from wild animals, as monkies, leopards, &c. he was desirous to have the wolf lined by some dog; but she would not allow any dog to come near her; probably from being always chained, and not accustomed to be with dogs. She was held, however, while a greyhound dog lined her, and they fastened together exactly like the dog and bitch. While in conjunction she remained pretty quiet; but when at liberty, endeavoured to fly at the dog; yet in this way was twice lined. She conceived, and brought forth four young ones; and though the time she went with young was not exactly known, it was believed to be the same as in the bitch. Two

no one durst approach her; but at other times people might do as they pleased, provided they did not use her ill. At first she made much of all the dogs which were brought to her; but afterwards she gave the preference to her old companion, and from that time she became very fierce if any strange dog approached her. She was lined for the first time on the 25th of March; this was frequently repeated while her heat continued, which was sixteen days; and she littered the 6th of June, at eight o'clock in the morning; the period of gestation was therefore seventy-three days at the most*. She brought forth four young ones of a blackish colour, some of whose feet, and a part of the breast, were white; in this respect taking after the dog, who was black and white. From the time she littered she became furlly, and set up her back at those who came near her; did not know her masters, and would even have killed the dog, if it had been in her power."

* This is a longer period than in the bitch by at least ten days; but as the account was made from the first time of her being lined, and she was in heat for a fortnight, and lined in that time, it is very probable, if the time was known when she conceived, that it would prove to be the same period as in the dog.

of these puppies were like the dog in colour, who had large black spots on a white ground; another was of a black colour; the fourth of a kind of dun, and would probably have been like the mother. She took great care of them, yet did not seem very anxious when one was taken from her by the keeper; nor did she seem afraid when strangers came into the room. Unfortunately these experiments were carried no further; one of the puppies being sold to a gentleman, who carried it to the East-Indies; and the other three, one of which I was to have had, were killed by a leopard. The same wolf was in heat in December 1786, and was lined several times by a dog. She pupped on the 24th of February 1787, and had six puppies, one of which, a bitch, I had, and kept it till it was in heat; but missed the opportunity of having her lined; that loss, however, was made up by a wolf-bitch belonging to James Symmons, Esq. of Grosvenor-House, Milbank; the history of which is as follows.

This female-wolf had been in his possession some time, had been lined by a dog, and brought forth several puppies, which I saw in company with Sir Joseph Banks, soon after Mr. Gough's wolf, the subject of my former Paper, had produced her litter; so that these puppies were nearly of the same age with mine. Mr. Symmons reared them all; but one only was a female, which more resembled the mother or wolf-kind, than any of the others. I communicated to Mr. Symmons my wish, that we should endeavour to prove the fact of the wolf and dog being of the same species, by having either his female or mine lined by a dog. This he very readily acceded to; and his bitch received the dog on the 16th, 17th, and 18th of December 1788; and the 18th of February following, she brought forth eight puppies, all of which she reared.

If we reckon from the 16th of December, she went sixty-four days; but if we reckon from the 17th, the mean time, then it is sixty-three days, the usual time for a bitch to go with pup. These puppies are the second remove from the wolf and dog, and similar to that given by my Lord Clanbrassil to the Earl of Pembroke, which likewise bred again. (See Philosophical Transactions, vol. LXXVII. p. 255.) It would have equally proved the same fact if she had been lined either by a wolf, a dog, or one of the males of her own litter.

It is remarkable, that there seems to be only one time in the year in which impregnation is natural to the wolf, which is the month of December: for Mr. Gough's wolf has always been in heat in that month; so was that of Mr. Symmons. The time of heat in his of the half-breed (which is nearly of the same age with mine) corresponded likewise with that of the mother, and of those bred from Mr. Gough's wolf.

OF THE JACKAL.

THIS animal being so nearly allied to the dog, and only found wild like the wolf, I became desirous of ascertaining of what particular species it was; and while pursuing the subject, I was informed that Captain Mears, of the Royal Bishop East-India-man, had brought home a bitch-jackal with young, which brought forth soon after his arrival; and that he had given the bitch-jackal and one puppy to Mr. Bailey, bird-merchant, in Piccadilly. I went to see them, and purchased the puppy, the subject of the following experiment, which we found to have dispositions very similar to those of the half-bred wolf, beforementioned, which I had from Mr. Brookes.

To have a true history of this animal, I took the liberty of writing to Mr. Mears, who politely called upon me, and, at my request, sent me the particulars in a letter, of which the following is a copy.

“ SIR,

“ I had the honour of yours the 15th instant; and with regard to the female-jackal, I can assure you, that she took a small spaniel dog of mine on board my ship, the Royal Bishop. I had her, when a cub, at Bombay; and a very short time before I arrived in England she got to heat, and enticed this small dog into the long-boat, where I saw them repeatedly fast together. I brought her to my house in the country, where she pupped six puppies, one of which you have seen. Mr. Plaw, at No. 90, Tottenham-Court-Road, has a dog-puppy, which will be at your service

at any time you chuse to fend for him, to make further experiments: I called on Mr. Plaw, and got his promise to let you have the dog.

I have the honour to be, Sir, &c.

WM. MEARS.

N^o 107, Hatton-street,
16th Jan. 1786.

P. S. I had the bitch on board fourteen months."

Having taken this puppy into the country, and chained it up near a mastiff-dog, they became very familiar, and seemingly fond of each other. When the bitch became first in heat, I could not get a proper dog; but about the latter end of September, she being again in the same state, several dogs were procured, and left with her. They appeared indifferent about her, probably from being in a strange place; nor did she seem inclined to be familiar with them. One was a large dog, which might not perhaps be able to line her; but she was twice tied by a terrier, on the third of October. In a few weeks she was evidently bigger; and on the 30th of November, in all fifty-nine days, brought forth five puppies. A few days before this period, she dug a hole in the ground, by the side of her kennel, in which she littered; and it was some time before she would allow the puppies to stay in the kennel when put there. Some of these began to open their eyelids in about eight, others of them in nine days.

Here then being an absolute proof of the jackal being a dog; and the wolf being equally made out to be of the same species; it now therefore becomes a question, whether the wolf is from the jackal, or the jackal from the wolf, (supposing them but one origin)? From the supposition, that varieties become more tame in their nature than the originals, we should be led to believe the wolf to be the original, and that the jackal was a step towards civilisation in that species of animal, and that therefore the jackal should be considered as a variety of the wolf. There are wolves of various kinds, each country having a kind peculiar to itself; but the jackals that I have seen have been more uniform in resemblance

to each other ; probably because only to be found in one country, the East-Indies. I am informed, however, that they vary in size. Whether the wolves of different countries are of one species, or some of them only of the same genus, I do not know ; but I should rather suppose them to be all of one species. An argument with me in favour of this supposition is, that, if there were wolves of distinct species, we should have had by this time a great variety of every species of wolf, with the various dispositions arising from variation in other respects ; and those varieties would now have been turned to very useful purposes, as in the case of the dog ; for all the wolves we are yet acquainted with, should have naturally the principle of cultivation in them, (as much probably as any animal) as much at least as those wolves we now know by the name of dogs. The not having a civilised species with all the characteristics of the wolf is, indeed, with me a proof that they are all of the same species with the dog. If they are all of the same species with the dog, then the first variety that took place would be still in the character of a wolf, differing only in colour, or some trivial circumstance, which could only arise from a difference in climate. The wolf is naturally, I believe, the inhabitant of cold climates, and little variety could take place while it remained in such a situation : but if the jackal was originally a wolf, which had strayed by accident more to the southward, a greater variation from the genuine character might be produced, the difference of climate, and perhaps of food, becoming causes of variety. By continuing to inhabit a warm climate, this circumstance would in time lose part of its influence on the animal, and the jackal would admit of little more variety. This, however, is a point not now to be determined, it being difficult (perhaps impossible) to say, where the wolf became jackal, or (what we call) dog ; and, as dogs differ much from one another, what particular dog may be considered as the first remove ; or whether the jackal is the intermediate link connecting the wolf and dog. In any case, we may reckon three great varieties in this species, wolf, jackal, and dog ; which again branch into their respective less obvious varieties. If the dog proves to be the wolf tamed, the jackal may probably be the dog returned to its wild state ; which leads

to another curious question : whether, as animals vary from climate, cultivation, or what may be called differences in mode of living, they would return to their genuine character if allowed to go wild again in the original country ?

To ascertain the original animal of a species, all the varieties of that species should be examined, to see how far they have the character of the genus, and what resemblance they bear to the other species of the genus ; for it is natural to suppose, that the original animal, or that which is nearest to it, will have more of the true character of the genus, and a stronger resemblance to the species nearest allied to it, than any of the other varieties of its own species.

If we apply this to the dog, and consider the fox as a distinct species, which there is great reason to believe it is, that variety which has the greatest resemblance to the fox, is to be looked upon as the original of all the others ; which will prove to be the wolf.

Another mode of considering this subject, which is however secondary to the above, is by supposing that all animals were at first wild ; and, therefore, that those animals which remain wild, are the original stock ; and that when we find animals far removed from their originals in appearance, the variation takes place in consequence of cultivation ; yet so that we can still trace the gradation. What gives some force to this idea is, that where the dogs have been least cultivated, there they still retain most of their original character, or similarity to the wolf or the jackal, both in shape and disposition. The shepherd's dog, all over the world, has strongly the character of the wolf or jackal ; so that but little difference is to be observed, except in size and hair. That of size may perhaps take place under a variety of circumstances ; but difference in hair is, in general, although not always, influenced by climate. Thus the wolf has longer and softer hair than the jackal, because a more northern animal ; while the jackal of the East, and shepherd's dog in Portugal and Spain, have shorter and stronger hair than those of Germany or Kamschatka, from inhabiting warmer climates. But when we consider their general shape, the character of countenance, the quick manner with the pricked and erect

ears, we must suppose them varieties of the same species. The smelling at the tale has been mentioned as characteristic of the dog ; but, I believe, it is common to most animals, and only marks the male ; for it is the most certain way the male has of knowing the female ; and by another scent discovering whether the female is disposed to receive the male, which is perhaps the final intention.

The Esquimaux dog, and that found among the Indians as far south as the Cherokees ; the shepherd's dog in Germany, called Pomeranian ; the shepherd's dog in Portugal and Spain ; have all a strong similarity to the wolf and jackal.

Buffon, on the origin of dogs, seems to have had nearly the same idea ; for he says the shepherd's dog is the original stock from which the different kinds of dog have sprung.

As the wolf turns out to be a dog, it seems astonishing, that there was no account of dogs being found in America. But this I consider as a defect in the first history of that country, as there are wolves ; and I must think, in spite of all that has been said to the contrary, that the Esquimaux and Indian dog is only a variety from a wolf of that country, which had been tamed. Mr. Cameron, of Titchfield-street, who was many years among the Cherokees, and considerably to the westward of that country, observes, that the dog found there much resembles the wolf ; and that the natives consider it to be a species of tame wolf ; but as we come more among the Europeans who have settled there, the dogs are more of a mixed breed : Why the Cherokees should have had only this kind of dog transported among them, while every other part of America has the varieties of Europe, is not easily solved.

The voice of animals is commonly characteristic of the species ; but I should suppose it to be only characteristic of the original species, and not always of the variety, and the supposition holds good in the dog species. Dogs may be said to have a natural voice, and a variation, arising either from a variety having taken place in the species, or a kind of imitation. It would appear, that the voice of the wolf and the jackal is very similar, being the natural voice, and is principally conveyed through the nose, and

exactly resembling that noise in dogs, which is a mark of longing or melancholy, and also of fondness; but having no resemblance to the bark of the dog, which they do not perform. However, in catching a jackal, when the animal found it could not escape, it yelped like the dog, which is a kind of barking, and which is, probably, the natural sound. Barking is peculiar to certain varieties of the dog kind, and even of those that bark, some do it less than others. The dogs in the South-sea islands do not bark: our greyhound barks but little; while the mastiff, and many of the smaller tribe, as spaniels, are particularly noisy in this way. There is reason to believe, that the frequency of this noise may arise from imitation; for the dogs in the South-sea islands learn to bark; the half-bred jackal barked; and so did the half-bred wolf, although but little; and others, as the hound, have a peculiar howl, by huntsmen, called the tongue; which noise, and the barking also, are both made by opening the mouth. A variety in the voice, or some parts of the voice, in varieties of the same species, is not peculiar to the dog.

It is a curious circumstance that variety not only takes place in colour and form, from the change of habits in the parents; but that the dispositions are also changed; and that the dispositions are most commonly changed in such a way as appears best adapted to the form of the animal. The change in the habits of the parent-animal, arise principally from its connection with the human kind, which has now succeeded in training dogs, so as to fit them, both in body and mind, for almost every purpose of human œconomy; as if man himself had formed them expressly with such intention; while, at the same time, he can only be considered as an occasional cause. For we may observe, that all the males of the wolf kind are nearly the same, and so are likewise those of the jackal; having little or no variety in their dispositions. Those of the half-breed, and even those that are three removes, although tame, yet they have not the docility of dogs, nor are they so immediately at the command of the human will; neither are they perfectly satisfied with an artificial life, having, when left to themselves, a propensity to fall back into their original instinctive principles.

The following account from Mr. Jenner of Berkley, to whom I gave a second remove, viz. three parts dog, is very descriptive of this propensity.

“ The little jackal-bitch you gave me is grown a fine handsome animal ; but she certainly does not possess the understanding of common dogs. She is easily lost when I take her out, and is quite inattentive to a whistle. She is more shy than a dog, and starts frequently when a quick motion is made before her. Of her inches she is uncommonly fleet, much more so than any dog I ever saw. She can turn a rabbit in the field ; she is fond of stealing away and lying about the adjacent meadows, where her favourite amusement is hunting the field-mouse, which she catches in a particular manner.”

As animals are known to produce young which are different from themselves in colour, form, and dispositions, arising from what may be called, the unnatural mode of life, it shews this curious power of accommodation in the animal œconomy, that although education can produce no change in the colour, form, or disposition of the animal ; yet it is capable of producing a principle which becomes so natural to the animal, that it shall beget young different in colour and form ; and so altered in disposition, as to be more easily trained up to the offices in which they have been usually employed ; and having these dispositions suitable to such change of form.

It also becomes a question, whether they would not go back again to their original state, if put into the situation of the original from whence they sprang ; or acquire a form resembling the original of that country where they are placed ? I do not conceive that they must necessarily go back through the same changes ; but I have some reason to suppose they would gradually return to a resemblance of that original. And it would be difficult to prove, whether in many of the gradations they are progressive or retrograde. But this is a subject that requires particular attention and investigation, and upon which I hope, some time or other, to be able to throw more light.

AN EXPERIMENT TO DETERMINE THE EFFECT OF EXTIRPATING ONE OVARIUM UPON THE NUMBER OF YOUNG PRODUCED.

IN all animals of distinct sex, the females, those of the bird kind excepted, have, I believe, two ovaria, and of course the oviducts are in pairs.

By distinct sex I mean when the parts destined to the purposes of generation are of two kinds, each kind appropriated to an individual of each species, distinguished by the appellation of male and female, and equally necessary to the propagation of the animal; the testicles, with their appendages, constitute the male; the ovaria, and their appendages, the female sex.

As the ovaria are the organs which, on the part of the female, furnish what is necessary towards the production of the third, or young animal; and as females appear to have a limited portion of the middle stage of life allotted for that purpose; it becomes a question, whether those organs are worn out by repeated acts of propagation; or whether there is not a natural and constitutional period to that power on their part, even if such power has never been exerted? If we consider this subject in every view, taking the human species as an example, we shall discover that circumstances, either local or constitutional, may be capable of extinguishing in the female the faculty of propagation. Thus we may observe when a woman begins to breed at an early period, as at fifteen, and has her children fast, that she seldom breeds longer than the age of thirty or thirty-five; therefore we may suppose, either that the parts are then worn out, or that the breeding constitution is over. If a woman begins later, as at twenty or twenty-five, she may continue to breed to the age of forty or more; and there are, now and then, instances of women, who, not having

conceived before, have had children as late in life as at fifty years or upwards. After that period few women breed, even though they should not have bred before; therefore, there must be a natural period to the power of conception. A similar stop to propagation may likewise take place in other classes of animals, probably in the female of every class, the period varying according to circumstances; but still we are not enabled to determine, how far it depends on any particular property of the constitution, or of the ovarium alone.

As the female of most classes of animals has two ovaria, I imagined, that by removing one it might be possible to determine how far their actions were reciprocally influenced by each other; from the changes which by comparison might be observed to take place, either by the breeding period being shortened, or perhaps, in those animals whose nature it is to bring forth more than one at a time, by the number produced at each birth being diminished.

There are two views in which this subject may be considered. The first, that the ovaria, when properly employed, may be bodies determined and unalterable respecting the number of young to be produced. In this case we can readily imagine, that, when one ovarium is removed, the other may be capable of producing its determined number in two different ways: one, when the remaining ovarium not influenced by the loss of the other, will produce its allotted number, and in the same time: the other, when affected by the loss, yet the constitution demanding the same number of young each time of breeding, as if there were still two ovaria; it must furnish double the number it would have been required to supply, had both been allowed to remain; but must consequently cease from the performance of its function in half the time. The second view of the subject is by supposing, that there is not originally any fixed number which the ovarium must produce, but that the number is increased or diminished according to circumstances; that it is rather the constitution at large that determines the number; and that, if one ovarium is removed, the other will be called upon by the constitution to perform the operations of both, by which means the animal should produce, with one ova-

rium, the same number of young as would have been produced if both had remained.

With an intention to ascertain those points, as far as I could, I was led to make the following experiment; and for that purpose gave pigs a preference to any other animal, as being easily managed, producing several at a litter, and breeding perfectly well under the confinement necessary for experiments. I selected two females of the same colour and size, and likewise a boar-pig, all of the same farrow; and having removed an ovarium from one of the females, I cut a slit in one ear, to distinguish it from the other: they were well fed and kept warm, that there might be no impediment to their breeding; and whenever they farrowed, their pigs were taken away exactly at the same age.

About the beginning of the year 1779, they both took the boar; the one which had been spayed, earlier than the perfect female. The distance of time, however, was not great, and they continued breeding at nearly the same times. The spayed animal continued to breed till September 1783, when she was six years old, which was a space of more than four years. In that time she had eight farrows; but did not take the boar afterwards, and had in all seventy-six pigs. The perfect one continued breeding till December 1785, when she was about eight years old, a period of almost six years, in which time she had thirteen farrows, and had in all one hundred and sixty-two pigs; after this time she did not breed: I kept her till November 1786.

I have here annexed a table of the different times of each farrow, with the number of pigs produced.

SPAYED SOW.

Farrows.	Number of young.	Time.
1	6	Dec. 1779.
2	8	July 1780.
3	6	Jan. 1781.
4	10	Aug. 1781.
5	10	Mar. 1782.
6	9	Sept. 1782.
7	14	May 1783.
8	13	Sept. 1783.
	<hr/>	
	76	

November following she was put to the boar, but brought no pigs. April 1784, she was again put to the boar, without effect, and never was observed to take the boar afterwards, although often with him. November 1784, she was killed.

PERFECT SOW.

Farrows.	Number of young.	Time.
1	9	
2	6	
3	8	
4	13	Dec. 1781.
5	10	June 1782.
6	16	Dec. 1782.
7	13	June 1783.
8	12	Oct. 1783.
	<hr/>	
	87	

Eleven pigs more than were produced by the spayed sow in her eight farrows.

Farrows.	Number of young.	Time.
9	12	Feb. 1784.
10	16	June 1784.
11	12	Dec. 1784.
12	16	May 1785.
13	19	Dec. 1785.
	<hr/>	
	75	

After which she bred no more.

The first eight farrows were	87
The last five farrows were	75
		<hr/>
Total	162
The number from the spayed one	76
		<hr/>
More than farrowed by the imperfect animal	86

It is observable, that both fows rather increased in their number each time as they grew older, although not uniformly; the difference between the first and last in both animals being considerable.

From the above table we find, that the fow with only one ovarium bred till she was six years old, from the latter end of 1779 till September 1783, about four years, and in that time brought forth seventy-six pigs. The perfect animal bred till she was eight years of age; and if conception depended on the ovaria, we might have expected, that she would bring forth double the number at each birth; or, if not, that she would continue breeding for double the time. We indeed find her producing ten more than double the number of the imperfect animal, although she had not double the number of farrows; but this may, perhaps be explained by observing that the number of young increased as the female grew older, and the perfect fow continued to breed much longer than the other.

From a circumstance mentioned in the course of this experiment it appears, that the desire for the male continues after the power of breeding is exhausted in the female ; and therefore does not altogether depend on the powers of the ovaria to propagate ; although it may probably be influenced by the existence of such parts.

If these observations should be considered as depending on a single experiment, from which alone it is not justifiable to draw conclusions, I have only to add, that the difference in the number of pigs produced by each was greater than can be justly imputed to accident, and is a circumstance certainly in favour of the universality of the principle I wished to ascertain*.

From this experiment it seems most probable, that the ovaria are from the beginning destined to produce a fixed number, beyond which they cannot go, although circumstances may tend to diminish that number ; but that the constitution at large has no power of giving to one ovarium the power of propagating equal to both ; for in the present experiment, the animal with one ovarium produced ten pigs less than half the number brought forth by the sow with both ovaria. But that the constitution has so far a power of influencing one ovarium, as to make it produce its number in a less time than would probably have been the case if both ovaria had been preserved, is to be inferred from the above recited experiment.

* It may be thought by some, that I should have repeated this experiment ; but an annual expence of twenty pounds for ten years, and the necessary attention to make the experiment complete, will be a sufficient reason for my not having done it.

ON THE STRUCTURE OF THE PLACENTA.

THE connection between the mother and foetus in the human subject, has in every age, in which science has been cultivated, called forth the attention of the anatomist, the physiologist, and even the philosopher; but both that connection and the structure of the parts which form the connection, were unknown till about the year 1754. The subject is certainly most interesting, and the discovery important; and it is my intention, in the following pages, to give such an account of it as I hope may be acceptable to the public*; while, at the same time, I establish my own claim to the discovery. But that I may not seem to arrogate to myself more merit than I am entitled to, let me, in justice to another person, relate what follows.

The late indefatigable Dr. Mc. Kenzie, about the month of May 1754, when assistant to Dr. Smellie, having procured the body of a pregnant woman, who died undelivered at the full term, had injected both the veins and arteries with particular success; the veins being filled with yellow, the arteries with red†.

Having opened the abdomen, and exposed the uterus, he made an incision into the fore part, quite through its substance, and came to what seemed to be an irregular mass of injected matter. The appearance being new, he proceeded no further, and greatly obliged me, by desiring my attendance to examine parts, in which the appearances were so uncommon. The examination was made in his presence, and in the presence of several

* This Paper was read at the Royal Society; but as the facts had, before that time, been given to the public, it was not published in the Philosophical Transactions.

† Dr. Mc. Kenzie being then an assistant to the late Dr. Smellie, the procuring and dissecting this woman, without Dr. Smellie's knowledge, was the cause of a separation between them: for the leading steps to such a discovery could not be kept a secret. The winter following, Dr. Mc. Kenzie began to teach midwifery in the Borough of Southwark.

other gentlemen, whose names I have now forgotten ; but I have reason to believe that some are settled in this country, who I hope will have an opportunity of perusing this publication*.

I first raised, with great care, a part of the uterus from the irregular mass, and in doing this, observed regular pieces of wax passing obliquely between it and the uterus, which broke off, leaving part attached to that mass ; and on attentively examining the portions towards the uterus, they plainly appeared to be a continuation of the veins passing from it to this substance, which proved to be placenta.

I likewise observed other vessels, about the size of a crow-quill, passing in the same manner, although not so obliquely ; these also broke upon separating the placenta and uterus, leaving a small portion on the surface of the placenta ; and on examination they were discovered to be continuations of the arteries of the uterus. My next step was to trace these vessels into the substance of what appeared placenta, which was first attempted in a vein ; but that soon lost the regularity of a vessel, by terminating at once upon the surface of the placenta in a very fine spongy substance ; the interstices of which were filled with the yellow injected matter. This termination being new, I repeated the same kind of examination on other veins, which always led me to the same terminations, never entering the substance of the placenta in the form of a vessel. I then examined the

* If I should be so fortunate as to have this publication fall into any of those gentlemen's hands, I hope they will favour me with their opinion of my state of the facts, which led to the discovery.

It may be suspected by some, (but none I hope to whom I have the pleasure of being known) that I am not doing Dr. Mc. Kenzie justice, and am perhaps suppressing some part of that share of the discovery to which he is entitled. This idea, (if ever it should arise) I may, probably not be able to remove ; but I hope it will also be seen, that I myself have given rise to it ; believing, if I had been so inclined, that I might have suppressed Dr. Mc. Kenzie's name altogether, without ever running the hazard of being detected. I was indeed so tenacious of my claim to the discovery, that I wrote this account in Dr. Mc. Kenzie's life-time, with a design to publish it ; and often communicated my intentions to Dr. George Fordyce, who I knew was very intimate with the Doctor, in consequence of both teaching in the same place, and making many experiments together ; therefore he is a kind of collateral witness, that what I now publish is the same account which I gave in Dr. Mc. Kenzie's life-time.

arteries, tracing them in the same manner towards the placenta, and found that, having made a twist, or close spiral turn upon themselves, they were lost on its surface. On a more attentive view, I perceived that they terminated in the same way as the veins; for opposite to the mouth of the artery, the spongy substance of the placenta was readily distinguished with the red injection intermixed.

Upon cutting into the placenta I discovered, in many places of its substance, yellow injection, in others red, and in many others these two colours mixed. The substance of the placenta, now filled with injection, had nothing of the vascular appearance, nor that of extravasation, but had a regularity in its form which showed it to be naturally of a cellular structure, fitted to be a reservoir for blood.

I perceived likewise, that the red injection of the arteries, (which had been first injected) had passed out of the substance of the placenta into some of the veins leading from the placenta to the uterus, mixing itself with the yellow injection; and that the spongy chorion, called the decidua, by Dr. Hunter, was very vascular, its vessels going to and from the uterus, being filled with the different coloured injections.

After having considered these appearances, it was not difficult for me to determine the real structure of the placenta and course of the blood in these parts: but the company, prejudiced in favour of former theories, combated my opinion; and it was even disputed, whether or not these curling arteries could carry red blood. After having dissected the uterus, with the placenta and membranes, and made the whole into preparations, tending to show the above facts, I returned home in the evening, and communicated what I had discovered to my brother, Dr. Hunter, who at first treated it and me with good humoured raillery; but on going with me to Dr. Mc. Kenzie's he was soon convinced of the fact. Some of the parts were given to him, which he afterwards showed at his lectures, and probably they still remain in his collection.

Soon after this time, Dr. Hunter and I procured several placenta, to discover if, after delivery, the termination of the veins, and the curling arteries, could be observed: they were discernible almost in every one; and

by pushing a pipe into the placenta, we could fill not only its whole substance, but also the vessels on that surface which was attached to the uterus, with injection.

The facts being now ascertained, and universally acknowledged, I consider myself as having a just claim to the discovery of the structure of the placenta, and its communication with the uterus; together with the use arising from such structure and communication, and of having first demonstrated the vascularity of the spongy chorion.

It is not necessary at present to enter into the various opinions which have been formed on this subject; because, whatever they were, they could not be just, the structure of the parts not being known: neither shall I endeavour to give a complete description of all the parts immediately connected with uterine gestation, but content myself with describing the structure of the placenta, as far as it has any relation to the uterus and child; and with explaining the connection between the two; leaving the reader to examine what has been said upon this subject by others, especially by Dr. Hunter, in that very accurate and elaborate work which he has published on the Gravid Uterus, in which he has minutely described, and accurately delineated the parts, without mentioning the mode of discovery.

The necessary connection subsisting in all animals between the mother and foetus, for the nourishment of the latter, as far as I know, takes place in two ways. In some it is continued, and subsists through the whole term of gestation; in others the union is soon dissolved; but an apparatus is provided, which at once furnishes what is sufficient for the support of the animal till it comes forth.

The first of these are the viviparous, the second the oviparous animals, both of which admit of great variety in the mode by which the same effect is produced*. In the first division is included the human species,

* It may be remarked here, that the oviparous admit of being distinguished into two classes, one where the egg is hatched in the belly, as in the viper, which has been commonly called viviparous; the others, where the eggs have been first laid and then hatched, which is the class commonly called oviparous, such as all the bird tribe; and many others, as snakes, lizards, &c.

which alone will engage our present attention. But before I describe this connection, it may be necessary that the reader should understand my idea of generation: I shall therefore refer him to what I have said upon that subject in my account of the free martin*.

In the human species, the anatomical structure of the mother and embryo, relative to foetation, being well known, it will only be necessary fully to describe the nature of the connection between them, which is formed by the intermediate substance, called placenta. For this purpose we must first consider the placenta as a common part; next, the uterus as belonging to the mother, yet having an immediate connection with the placenta, from which the nourishment of the foetus is to be derived; which will lead us lastly to a consideration of those peculiarities of structure, by means of which the foetus is to receive its nourishment, and which likewise constitutes its immediate communication with the placenta. It is the structure of this intermediate substance, and its connection with the child and the uterus of the mother, which have hitherto been so little understood; and without an accurate knowledge of which, it was impossible any just idea could be formed of its functions.

The placenta is a mass lying nearly in contact with the uterus; indeed it may in some degree be said to be in continuity with a part of its internal surface. On the side applied to the uterus the placenta is lobulated, having deep irregular fissures. It is probable, from this structure of the placenta, that the uterus has an intestine motion while in the time of uterine gestation; not an expulsive one, which those lobes of the placenta allows of; but all these lobes are united into one uniform surface on that surface next to the child, where its umbilical vessels ramify. When we cut into the placenta, its whole substance appears to be little else than a network, or spongy mass, through which the blood-vessels of the foetus ramify, and indeed seems to be principally formed by the ramifications of those vessels; it exhibits hardly any appearance of connecting membrane;

* Vide page 55.

but we cannot readily suppose it to be without such a membrane, as there is so much regularity in its texture. The cells, or interstices of each lobe communicate with one another, even much more freely than those of the cellular membrane in any other part of the body; so that whatever fluid will pass in at one part, readily diffuses itself through the whole mass of lobe; and all the cells of each lobe have a communication at the common base.

This structure of the placenta, and its reciprocal communication with the two bodies with which it is immediately connected, form the union between the mother and fœtus for the support of the latter. Prior to the time I have mentioned above, anatomists seem to have been wholly unacquainted with the true structure of placenta. By notes taken from Dr. Hunter's lectures, in the winter 1755-6, it appears that he expressed himself in the following manner*. "The substance of the placenta is a fleshy mass, which seems to be formed entirely of the vessels of the umbilical rope." In another part, mentioning the appearances when injected, he says; "and upon a slight putrefaction coming on, you will find the whole appearing like a mass of vessels": then says, "there is always a white uninjected substance between the vessels; but whether lymphatics or what I cannot tell." This uninjected substance, mentioned by Dr. Hunter, is what forms the cellular structure.

The placenta seems to be principally composed of the ramifications of the vessels of the embryo, and may have been originally formed in consequence of those next to the uterus laying hold by a species of animal attraction of the coagulable lymph which lines the uterus. It might take place in a manner resembling what happens when the root of a plant spreads on the surface of moist bodies; with this difference, that in the

* These quotations were taken from Mr. Galhie's MS. of Dr. Hunter's lectures, who is one of the gentlemen that favoured Dr. Hunter, upon a former occasion, with the use of his notes. Vide Dr. Hunter's Commentaries.

present instance the vessels form the substance through which they ramify, as in the case of granulations.

At the time, or perhaps before the female seed enters the uterus, coagulable lymph, from the blood of the mother, is thrown out every where on its inner surface, either from the stimulus of impregnation taking place in the ovarium, or in consequence of the seed being expelled from it. But I think the first the most probable supposition; for we find in extra-uterine cases, that the decidua is formed in the uterus, although the ovum never enters it; which is a proof that it is produced by the stimulus of impregnation in the ovarium; and that it is prior to the entrance of the ovum into the uterus. When it has entered the uterus, it attaches itself to that coagulable lymph, by which, being covered and immediately surrounded*, there is formed a soft pulpy membrane, the decidua, which, I believe, is peculiar to the human species, and to monkeys, I never having found it in any other animal. That part which covers the seed or fœtus, where it is not immediately attached to the uterus, and likewise forms a membrane, was discovered by Dr. Hunter, and is by him called decidua reflexa†. The whole of this coagulable lymph continues to be a living part for the time; the vessels of the uterus ramify upon it; and where the vessels of the fœtus form the placenta, there the vessels of the uterus, after passing through the decidua, open

* This is somewhat similar to another operation in the animal œconomy. If an extraneous living part is introduced into any cavity, it will be immediately enclosed with coagulable lymph. Thus we find worms enclosed, and hydatids, that have been detached, afterwards enclosed; but in those cases this is a consequence of the pressure of the extraneous body; whereas in the uterus it is preparatory.

† The placenta is certainly a foetal part, and is formed on the inside of the spongy chorion, or decidua. How far the decidua reflexa is a uterine part, I do not yet know; if it is, then the ovum must be placed in a doubling of the coagulum, which forms the decidua; but if the ovum is attached to the inside of the decidua, then the decidua reflexa is belonging to the fœtus.

into the cellular substance of the placenta, as before described. As this membrane lines the uterus and covers the feed, it is stretched out, and becomes thinner and thinner, as the uterus is distended by the fœtus growing larger, especially that part of it, called decidua reflexa, which covers the fœtus; as there it cannot possibly acquire any new matter, except we could suppose that the fœtus assisted in the formation of it. This membrane is most distinct where it covers the chorion; for where it covers the placenta it is blended with coagula in the great veins that pass obliquely through it, more especially all round the edge, where innumerable large veins come out; but the chorion and decidua can be easily distinguished from one another, the decidua being less elastic.

From the description now given, I think we are justified in supposing the placenta to be formed entirely by the fœtus, which is farther confirmed by extra-uterine cases, and by the formation of the membrane in the egg; there being no living organic part to furnish them; and the decidua we must suppose to be a production of the mother; of both which, the circumstance of the decidua passing between the placenta and uterus, may be considered as an additional proof. For if the vessels of the fœtus branched into a part of the decidua, we might conceive the whole placenta to be formed from that exudation; the portion of it, where the vessels had ramified, like the roots of a plant, becoming thicker than the rest, and forming the placenta. If that were the case, this membrana decidua, when traced from the parts distinct, and at a distance from the placenta, should be plainly seen passing into its substance all round at the edges, as a continuation of it. But the fact is quite otherways; for the decidua can be distinctly traced between the placenta and uterus, hardly ever passing between the lobuli: the vessels of the fœtus never entering into it, and of course none of them ever coming in absolute contact with the uterus. But what may be considered as still a stronger proof that the decidua is furnished by the uterus, is, that in cases of extra-uterine conception, where the fœtus is wholly in the ovarium or Fallopian tube, we find the uterus lined with the decidua,

having taken on the uterine action; but no placenta, that being formed by the fœtus, and therefore in the part which contained it.

The vessels of the fœtus adhering, by the intervention of the decidua, to a certain portion of the uterus when both are yet small, as the uterus increases in every part of its surface during the time of uterine gestation, we must suppose that this surface of adhesion increases also; and that by the elongation of those vessels of the fœtus in every direction, this substance should likewise be increased in every direction: this is in some degree the case, yet the placenta does not occupy so much of the enlarged surface of the uterus as one at first would expect.

The vessels of the uterus in the time of the gestation, are increased in size nearly in a proportion equal to the increased circumference of the uterus, and consequently in a proportion much greater than the real increase of its substance. But when we reflect that the uterus ought not to be considered as hollow, but as a body nearly solid, on account of its contents, which derive support from this source, and that a much greater quantity of blood must necessarily pass than what is required for the support of the viscus itself, we cannot be at a loss to account for the greatly increased size of its vessels.

The arteries which are not immediately employed in conveying nourishment to the uterus, go on towards the placenta, and proceeding obliquely between it and the uterus, pass through the decidua without ramifying; just before they enter the placenta, after making two or three close spiral turns upon themselves, they open at once into its spongy substance without any diminution of size, and without passing beyond the surface, as above described. The intention of these spiral turns would appear to be that of diminishing the force of the circulation in the vessels as they approach the spongy substance of the placenta, and is a mechanism calculated to lessen the quick motion of the blood in a part where a quick motion was not required. These curling arteries at this termination are in general about half the size of a crow's quill, and sometimes larger.

The veins of the uterus appropriated to bring back the blood from the placenta, commence from this spongy substance by such wide beginnings, as are more than equal to the size of the veins themselves. These veins pass obliquely through the decidua to the uterus, enter its substance obliquely, and immediately communicate with the proper veins of the uterus. The area of these veins bear no proportion to their circumference, the veins being very much flattened.

This structure of parts points out at once the nature of the blood's motion in the placenta; but as this is a fact but lately ascertained, a just idea may perhaps be conveyed by saying, that it is similar, as far as we yet know, to the blood's motion through the cavernous substance of the penis.

The blood, detached from the common circulation of the mother, moves through the placenta of the fœtus; and is then returned back into the course of the circulation of the mother to pass on to the heart.

This structure of the placenta, and its communication with the uterus, leads us a step further in our knowledge of the connection between the mother and fœtus; the blood of the mother must pass freely into the substance of the placenta, and the placenta most probably will be constantly filled; the turgidity of which will assist to squeeze the blood into the mouths of the veins of the uterus, that it may again pass into the common circulation of the mother: and as the interstices of the placenta are of much greater extent than the arteries which convey the blood, the motion of the blood in that part must be so much diminished as almost to approach to stagnation; so far and no further does the mother appear to be concerned in this connection.

The fœtus has a communication with the placenta of another kind. The arteries from the fœtus pass out to a considerable length, under the name of the umbilical arteries, and when they arrive at the placenta, ramify upon its surface, sending into its substance branches which pass through it, and divide into smaller and smaller, till at last they terminate in veins;

these uniting, become larger and larger, and end in one, which at last communicates with the proper circulation of the fœtus.

This course of vessels, and the blood's motion in them, is similar to the course of the vessels and the motion of the blood in other parts of the body.

In addition to what I have said about the connection between the mother and child, in natural cases, it is necessary to observe, that though the uterus is appropriated for the support of the fœtus, as best fitted for that purpose, yet it is not essential to its growth; as any other part in which the child may be situated, is capable of receiving the same provisory stimulus for supplying it with nourishment as the uterus; and this, I believe, is peculiar to generation. This prompts me to make the following observations upon the different situations of the fœtus in extra-uterine cases, which are extraordinary, happen seldom, and when they do occur, are often attended with so many hinderances to critical investigation, as hardly to allow of thorough or satisfactory information.

Such cases are readily distinguished from natural ones, by the uterus being found entire and empty; and they may be divided into three different kinds, according to the situation of the fœtus in the ovarium, Falopian tube, or in the cavity of the abdomen.

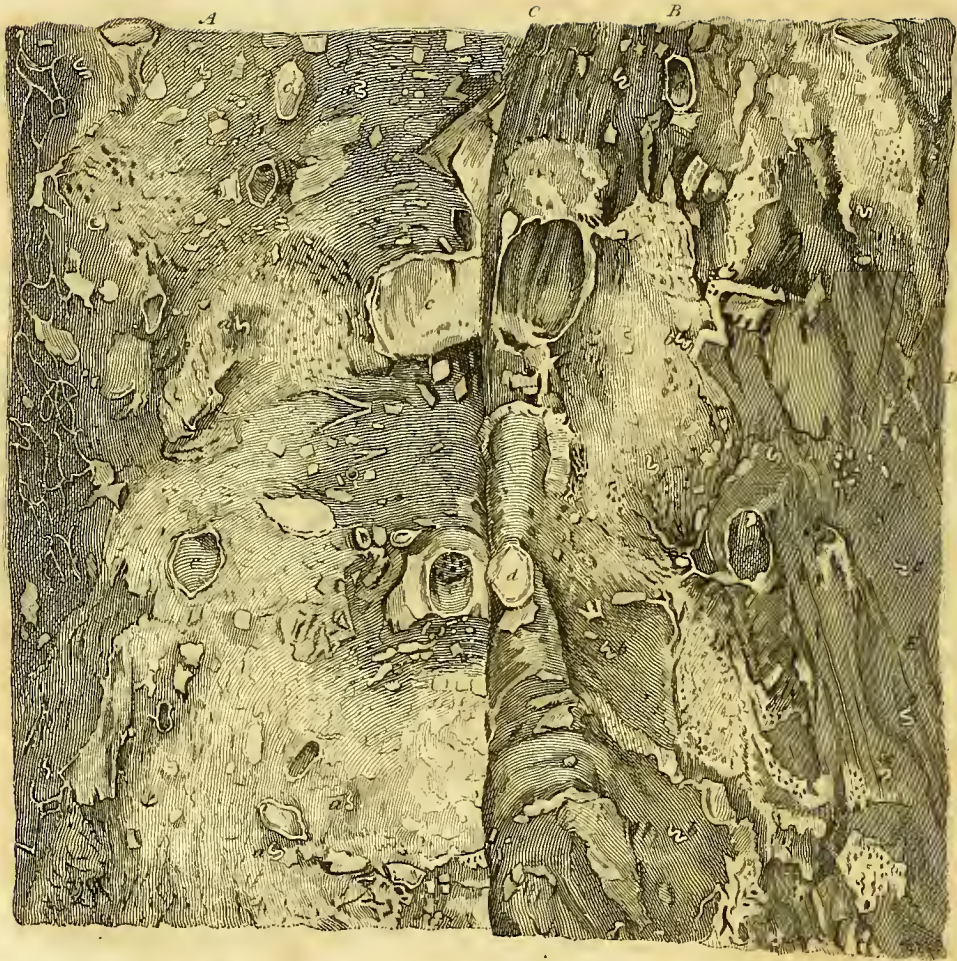
From a want of the appearances which usually attend the natural process, the investigation of extra-uterine cases is attended with considerable difficulty. For where uncommon actions have taken place, as well as in cases of disease, the natural texture of the parts is very much altered, and appears to be lost; not only by the parts themselves being enlarged, but from having a great deal of new matter superadded to them, by which they lose their natural distinctness, and become less fitted for examination than those which only have a relation to them, and which preserve their natural actions peculiar to that state.

From these difficulties, and a want of accuracy in those who made the examination, it is not at present clear, with respect to many of the extra-uterine cases upon record, whether they were ovarian cases, Falopian tube cases, or abdominal cases; when, if they had been acquainted with the

principle in which they differ, nothing could have been more easy than to distinguish them. It is not difficult, perhaps, at the very first view, to distinguish an abdominal case from either of the two first: for if the ovaria and Falopian tube are entire, natural, and can be well distinguished to be as those parts are when the circumstances are natural, then we may be sure it is an abdominal case. Appearances, however, may not in all cases be distinct; but the parts may adhere, or be otherwise rendered so obscure, that an abdominal case might be confounded with either of the two first; therefore it is essential to have a characteristic difference established between the two first, and the third.

The invariable difference between the two first, and the abdominal cases, will be in the vessels by which the child is nourished; for the arteries and veins belonging to the part in which the child is contained must be enlarged; which, being the increase of a natural part, will be readily ascertained, and the nature of the case as readily determined. We may lay it down as a principle, that when the spermatic artery, and veins of either side, is enlarged in an extra-uterine case, that the fœtus is in the ovarium or Falopian tube; since there are no other blood-vessels which supply these parts; and if any other system of vessels, as the mesenteric, are increased in size, while the spermatic are in a natural state, we may, with equal certainty, conclude the fœtus to be contained in the general cavity of the belly. As this becomes the great criterion, and as the situation and time will not always allow very nice investigation on the spot, where the person employed has an opportunity of taking away the parts concerned, I would advise his taking along with them the aorta and vena cava, cut through above the origins of the spermatic vessels.





P L A T E XIII.

A part of the uterus at the ninth month of utero-gestation, with a portion of the placenta, to show the mode in which the blood-vessels of the mother communicate with it.

- A The substance of the uterus, separated from the placenta, and turned back.
- B The surface of the placenta by which it is attached to the uterus, covered by the decidua.
- C The angle of reflection, at which the uterus is turned back upon itself.
- D The edge of the placenta.
- E The decidua covering the chorion.

Upon the surface of the uterus are to be seen the veins or sinuses, running in an oblique direction, filled with wax, and broken off where they pass through the decidua.

- a a a a The arteries injected and broken off as they pass from the uterus to the placenta.
- b b b b The continuation of these arteries, which make several spiral turns as they dip into the decidua, and afterwards terminate on the surface of the placenta.
- c c c c The veins injected and broken off where they pass into the substance of the uterus.
- d d d d The corresponding portions of the same veins, where they pass from the placenta through the decidua.
- e e e e The blood-vessels, ramifying upon the decidua, broken off from the uterus.

OBSERVATIONS ON THE PLACENTA OF THE MONKEY.

MONKEYS always copulate backwards ; this is performed sometimes when the female is standing on all-fours ; and at other times the male brings her between his thighs when he is sitting, holding her with his fore paws.

The female has her regular periods for the male, but she has commonly too much complaisance ever to refuse him. They carry this still further, for they receive the male when with young, even when pretty far gone ; at least this was the case with one of which I am going to give an account.

A female monkey, belonging to Mr. Enderfbay, in the summer 1782, had frequently taken the male. The keeper observed, that after the 21st of June she became less lively than usual, although it was not suspected that she had conceived ; but some time after appearing to be bigger in the belly, it created a suspicion of her being with young. Great attention was paid to her, and great care was taken of her. She went on gradually increasing in size ; and at last something was observed to move in her belly at particular times, and the motion could even be felt through the abdominal muscles. She became indolent, and did not like to leap or perform her usual feats of activity. Towards the latter part of the time, they perceived the breast and nipple to have become rather fuller ; and that a kind of water could be squeezed out at the nipple. Some time before she brought forth, she became red about the hips and posteriors ; which redness extended to the inside of the thighs, it being now certain that she was with young. I desired that she might be particularly attended to when there were signs of approaching delivery, both on her own account and that of the young one, and requested the afterbirth might be carefully preserved, as that part would assist to ascertain the mode of uterine gestation. These directions were attentively followed ; and when

in labour it was observed, that she had regular pains; that when the young one was in part come into the world, she assisted herself with her fore paws; and that it came with the hind parts first. This happened on the 15th of December 1782, in all about six months after conception; and when she brought forth her young one, it showed signs of life, but died immediately, owing, probably, to the unfavourable mode of its being brought into the world. When delivered, she took the young one up, and although it was dead, clasped it to her breast.

The afterbirth was preserved entire, and was perfectly fit for examination. It consisted of placenta, with the membranes and navel-string, which all very much resembled the corresponding parts in the human subject, as will now be described.

The placenta had the appearance of being divided into two oblong bodies, united by their edges, each terminating in an obtuse point at the other end, which were of course at some little distance from one another.

It is probable, that these two points were placed towards the openings of the Fallopian tubes, where the uterus assumes a form resembling two obtuse horns.

The two lobes abovementioned, were made up of smaller ones, united closely at their edges, which were more apparent and distinct at some parts than at others. Some of these lobes were divided by fissures which seem to be derived from one centre; while there were others near the edges, passing in a different direction; in which fissures are placed veins or sinuses that receive the blood laterally from the lobes. The substance of the placenta seems to be cellular, as in the human subject; this structure allows a communication to be kept up between different parts of each lobe, and the sinuses allowing of a communication between the different lobes of which the placenta is composed, the blood passes into the fissures before it enters the veins; in which respect it differs from the human placenta.

The arteries from the uterus, on the surface of the placenta, were visible, but too small to be injected; I cannot therefore say how they terminated in the placenta.

The principal veins arose in general from the fissures beginning from the surface, as in the human placenta ; but besides these, there were other small ones ; all which, we may suppose, pass through the decidua and enter the substance of the uterus, most probably in the same way as in the human.

The membranes are the amnios, the chorion, and the membrana decidua. These appear to be much the same as in the human, except that the decidua is considerably thicker, especially where it passes between the uterus and the placenta.

The navel-string in the monkey is not proportionally so long as in the human ; and is very much, and very regularly twisted.

There is no urachus, and of course no allantois, not even the small ligament that appears to be a drawing in of the bladder at its attachment to the navel, the bladder here being rounded.

OBSERVATIONS ON THE GILLAROO-TROUT,
COMMONLY CALLED, IN IRELAND, THE
GIZZARD-TROUT.

ONE of the digestive organs of the gillaroo-trout being so very remarkable as to have given name to the fish, and to have been considered as its distinguishing characteristic ; it is my intention to inquire whether its resemblance to a gizzard be sufficiently strong to render the term of gizzard-trout a proper appellation, and what place its stomach ought to hold among the corresponding organs of other animals. For this purpose it will be necessary to state certain facts connected with the subject ; and take a general view of the varieties which occur in the digestive organs in different animals.

The food of animals may be divided into two kinds, what does, and what does not, require mastication to facilitate digestion. The flesh of animals is of the latter kind ; but grain, and many other substances which serve for aliment, require a previous grinding or trituration ; and therefore animals living on this kind of food are furnished with organs for that purpose. Granivorous quadrupeds have the two powers, for mastication and digestion, separate or distinct from one another ; the first being executed by teeth, which serve as so many grindstones for reducing their food to smaller parts, before it is conveyed into the stomach for digestion ; but the form of these teeth varies very considerably in different animals, although the food be the same. This grinding also fits it for deglutition : for neither grain nor herbs could be swallowed without having first been masticated. When so prepared, it is, with regard to the digestive power, rendered similar to animal food : therefore in many of the granivorous, the stomach resembles that of the carnivorous animals ; and whenever the stomach in the granivorous quadruped departs from this general rule,

there is a peculiarity in the operations of digestion. Birds that live upon substances, for the digestion of which trituration is indispensably necessary, have the powers of mastication and digestion united in one part, the gizzard, which is particularly constructed for that purpose; but is more uniform in its construction than the teeth, varying only by being stronger or weaker in its powers; therefore the genus of birds exhibits less variety, respecting the organs relating to digestion, than the quadruped. In granivorous birds, therefore, one single organ answers both to the teeth and stomach of granivorous quadrupeds, and consequently the gizzard alone of birds will as clearly point out the food of the species as both teeth and stomach together, in those animals in which the two offices of mastication and digestion are not performed together in the same part.

As it appears to be the difference of stomachs only, that fits birds for their different kinds of food, as there is little difference in construction, excepting only in strength; and as the food of the different species is of every kind, from the hardest grain to the softest animal matter, we may conclude, that every gradation of the stomach is to be found among them, from the true gizzard, which is one extreme, to the mere membranous stomach, which is the other. In consequence of this, it must be as difficult to determine the exact limits of the two different modes of construction, to which the names of gizzard and stomach specifically belong, as, in any other case, to distinguish proximate steps in the slow and imperceptible gradations of Nature.

The two extremes of true gizzard, and membranous stomach, are easily defined; but they run so into each other, that the end of one and the beginning of the other is quite imperceptible. Similar gradations are observable in the food; the kinds suited to the two extremes mixing together in different proportions, adapted to the intermediate states of stomach.

A true gizzard is composed of two strong muscles placed opposite, and acting upon each other, like two broad grindstones. These muscles are joined together at their sides by a middle tendon, into which the muscular fibres are inserted, and which forms the narrow anterior and posterior sides

of the flat quadrangular cavity, in which the grinding is performed. The upper end of this cavity is occupied by the termination of the œsophagus, and the beginning of the intestine. The lower end consists of a thin muscular bag connecting the edges of the two muscles together.

By these two more soft and flexible substances being thus interposed between the two strong grinding muscles, a double advantage is gained; for whilst one gives an easy passage to the œsophagus and gut, when both act together they serve in some degree as a hinge, on which the two muscles may be said to move, by the middle tendon allowing of a free motion of the grinding surfaces on each other, which is necessary for the comminution of food.

The two flat lateral sides of the grinding cavity are lined with a thick horny substance similar to a hard and thick cuticle: the narrow anterior and posterior tendinous parts are also lined with a cuticle, but not so strong as the former: this horny substance is gradually lost at one end in a very thin cuticle, which lines the passages of the œsophagus and intestine for a little way; and at the other end is lost in the same manner in the membranous bag.

The two large muscles may be considered as a pair of jaws, whose teeth are occasionally supplied, being small rough stones or pebbles which the animal swallows; who, from the feeling of the tongue, can distinguish such as are proper, from those which are not; instantly dropping out of its mouth such as are smooth and otherwise unfit for the purpose.

Some birds, with gizzards, have also a craw or crop, which serves as a reservoir, and for softening the grain; but as all of them have not this organ, it is not to our present purpose.

There are other animals, besides this class of birds, which masticate their food in the stomach, but teeth are placed there by Nature: of this kind are crabs and lobsters.

The gradation from gizzard to stomach is made by the muscular sides becoming weaker and weaker, and the food keeps pace with this change, varying gradually from vegetable to animal. In one point of view, therefore, food may be considered as a first principle, with respect to which the

digestive organs, with their appendages, act but as secondary parts; being adapted to and determined by the food, as the primary object.

We find then that in all granivorous animals, there is an apparatus for the mastication of the food, although often differing in construction and situation. But in true carnivorous animals, of whatever tribe, mastication not being so necessary, they have no apparatus for that purpose. The teeth of such quadrupeds, as are carnivorous, serve chiefly to procure food and prepare it for deglutition. The same thing is performed in the true carnivorous birds, by their beak and talons, whose office it is to procure the aliment and fit it for deglutition, corresponding in this respect with the teeth of the others. Applying this opinion to fish, it seems, at first sight, as if there were no occasion in them for that variety of structure in the digestive organs, as is found in the beforementioned quadrupeds and birds; the food of fish being principally of one sort, namely, animal; which, however, with regard to the digestive powers, is to be distinguished into two kinds, viz. common soft fish and shell-fish. Such fish as live on the first kind, have, like the carnivorous quadrupeds and birds, no apparatus for mastication; their teeth being intended merely for catching the food and fitting it to be swallowed. But the shells of the second kind of food render some degree of masticating power necessary, to fit it for its passage either into the stomach or through the intestines; and accordingly we find in certain fish a structure suited to the purpose.

Thus the mouth of the wolf-fish is almost paved with teeth, by means of which it can break shells to pieces, and fit them for the œsophagus of the fish; and so effectually disengage the food from them, that though it lives upon such hard food, the stomach does not differ from that of other fish: the organs of mastication and digestion, therefore, in this animal, exactly correspond to those of many granivorous quadrupeds.

Other fish, on the contrary, approach nearer to the structure of birds, by having their stomach furnished to a certain degree, with a masticating power, which in many is very imperfect, compared with the gizzards of fowls. Perhaps the difference is only what the difference of food will

properly allow; as in fish which have this power, the food being still animal, and in general but imperfectly covered with the shell, it probably requires only to be broken; perhaps hardly that, for the mere purposes of digestion, as food is digested when introduced into the stomach in silver balls with only a few small holes; but it may be necessary to fit the shells for passing along the intestines after the fish is digested. In the *bulla lignaria* of Linnæus, this apparatus is more perfect, consisting of two bones, which we must suppose capable of grinding hard shells; but the food of granivorous birds requires to be ground into a kind of meal.

Of all the fish I have seen, the mullet is the most complete instance of this structure; its strong muscular stomach being evidently adapted, like the gizzard of birds, to the two offices of mastication and digestion. The stomach of the fish now before us holds the second place.

But still neither of these stomachs can be justly ranked as gizzards, since they want some of the most essential characters, viz. a power and motion fitted for grinding, and the horny cuticle. The stomach of the Gillaroo-trout is, however, more globular than that of most fish, better adapted for small food, and endued with sufficient strength to break the shells of small shell-fish; which will probably be best done by having more than one in the stomach at a time, and also by taking pretty large and smooth stones into the stomach, which will answer the purpose of breaking; but not so well that of grinding; nor will they hurt the stomach as they are smooth, when swallowed; but this stomach can scarcely possess any power of grinding, as the whole cavity is lined with a fine villous coat, the internal surface of which appears every where to be digestive, and by no means fitted for mastication.

The stomach of the common stream-trout is exactly of the same structure with that of the gillaroo; but its coat not so thick by two-thirds.* How far this difference in thickness of stomach is sufficient to form a

* The common stream-trout swallows shell-fish, and also pretty large smooth stones, which serve as kind of shell-breakers.

distinct species, or barely a variety of the same, is only to be determined by experiment.*

The œsophagus in the trout is considerably longer and smaller than in many other classes of fish.

The intestines are similar to those of the salmon, herring, sprat, &c.

The pancreas is appendiculated.†

The teeth show them to be fish of prey.

So far as we are led to determine by analogy, we must not consider the stomach of this fish as a gizzard, but as a true stomach.

* Viz. Take some gillaroo-trout, male and female, and put them into water in which there are no trout, to see if they continue the same.

† I chuse to give this name to the pancreas from its appearance.

SOME OBSERVATIONS ON DIGESTION.

THE Paper which I formerly presented to the Royal Society, “ On the Stomach itself being digested after Death”, was published in 1772, in the 62nd volume of the Philosophical Transactions, and has attracted the attention of Spallanzani, and others. In the course of these my observations, I shall make some remarks upon the experiments and opinions of these gentlemen ; compare them with those of Reaumur, and having given some general facts of my own upon digestion, shall conclude by adding a copy of the abovementioned Paper, with the hope that others will take up the subject in a more enlarged point of view, and prosecute an inquiry which is of so much consequence in the investigation of the operations of the animal œconomy. I cannot, at present, spare sufficient time to give my opinions at large on this subject, with all the experiments and observations I have made upon it ; but as soon as I have leisure I shall lay them before the public.

To discover new parts, has been a principal object in the researches of the young or practical anatomist ; but the connection, arrangement, mode of action, and uses of the whole, or of particular organs, have more commonly been reserved for the consideration of those whose views were extended further ; and whose powers of reasoning had been enlarged by habits of observation and inquiry. Curious and speculative men have likewise made attempts in this way, but often without being sufficiently acquainted with the structure of the parts they were about to consider ; and consequently ill-informed respecting their relations and connections with one another. Not contented to reason from those which were most obvious, which might have led to useful knowledge, they have been directed by what best suited their fancy, and have principally attempted the most obscure and intricate. Generation, or the mode of continuing the

species, and digestion, or the means of preserving the individual, have been with them the great objects of inquiry; yet it does not appear that they have been very successful. Although digestion, as being one of the most important operations of the animal œconomy, and most obvious in its effects, supplies a number of facts to assist in ascertaining its powers, little has been hitherto made out towards investigating the various circumstances under which it is performed.

The mode of dividing the food, for the increase of its surface in some animals, suggested one method of explaining the process of digestion; and the secretion of a juice, which was supposed to have the power of converting vegetable and animal matter into a fluid proper for the purposes of nutrition, furnished another. Both these opinions have had their advocates; and while one party contended for a mechanical power, supposed to exist in the gizzard; the other had recourse to a chymical power, and considered fermentation as the great agent in digestion: they were, however, rather speculative philosophers, than practical anatomists, and have frequently been misled with respect to the very facts and observations whose result was to decide the truth of their opinions. What, for instance, does it explain in digestion, that the force of the gizzard of a turkey is found equal to four hundred and seventy-three pounds? Does it afford a better solution of our doubts, than we should derive from determining the force of the mill that grinds the wheat into flour? Or, on the other hand, will the most correct idea of fermentation enable us to account for the various phenomena in the operation of digestion? But we can have no very high idea of experiments made by men, who, for want of anatomical knowledge, have not been able to pursue their reasoning beyond the simple experiment itself.

The great object should have been, an endeavour to discover the universal agent in digestion: for the digestive organ is evidently constructed in a different manner in different animals. The mechanical power for the division of the food is not universal; and those gentlemen who consider this power in the gizzard as the immediate cause of digestion, forgot that the same effect was produced in other classes of animals with a different

structure of stomach, by means of the grinding teeth. Thus while the gizzard favoured the theory of the mechanical reasoner, that idea was again destroyed by the membranous structure of the stomach in many animals; which equally supplied the chymist with arguments in favour of the process of fermentation.

It is more difficult than those gentlemen imagine, to acquire on this subject information sufficiently accurate, to be able to explain a process so complicated as that of digestion. There are in Nature's operations always two obvious extremes; and the mind of man eagerly adopts that which accords with some principle to which he is attached, and with which he is best acquainted; the intermediate connections and gradations as being less striking, not so forcibly affecting a superficial inquirer.

It happens unfortunately, that those who from the nature of their education are best qualified to investigate the intricacies, and improve our knowledge of the animal œconomy, are compelled to get their living by the practice of a profession which is constant employment. The only educated men who have leisure, are those of the church, some of whom we frequently find commencing philosophers and physiologists; though they have not had that kind of education which would best direct their pursuits. Experiments, it is true, may be made by men of this description; but these must neither be much complicated, nor have any immediate relation to those branches of knowledge with which they have had few opportunities of being acquainted; at best, they will seldom go further than to explain a single fact. To look through a microscope and examine the red globules of blood; to view animalculæ, and give a candid account of what they see, are points on which such inquirers may be allowed to indulge themselves. But it is presumption in them to affect to reason of a science in which they can have but a very superficial knowledge; or to expect to throw light on subjects that they have not taken the previous steps to understand. It should be remembered, that nothing in Nature stands alone; but that every art and science has a relation to some other art or science; and that it requires a knowledge of those others,

as far as this connection takes place, to enable us to become perfect in that which engages our particular attention.

These strictures are applicable to all those who have made experiments to explain digestion. The effect of the mechanical powers being easily understood, those who considered digestion mechanically, have in general explained them justly, as far as they applied to the gizzard; but their reasoning went no further, and they supposed these effects to be digestion. Those again who took it up chymically being little acquainted with chymistry, and totally ignorant of the principles of the animal œconomy, have erroneously explained the operations of the animal machine as subject to the laws of chymistry.

The first inquirers into digestion, struck only by the extremes of structure, the gizzard, and membranous stomach, paid no regard to the gradations leading from the one to the other; which, if properly examined, would have materially assisted them to explain the functions of the stomach.

Vallisneri, considering the power of the gizzard, in one view only, imagined it would be as liable to be affected by the mechanical powers necessary for digestion, as the grain which was to be digested; therefore supposed the existence of a solvent. But though Vallisneri is intitled to no merit from this idea, as the premises are false; yet this opinion of his set Reaumur to work, and has been the means of bringing several curious facts to light. The experiments of Reaumur were first made with a view to confute that opinion; and therefore birds having gizzards, were adapted to his purpose. In this pursuit he only attended to such parts of the experiments as best accorded with his own opinion; yet carefully guarded against every possible accident that might affect their accuracy. Had trituration been the immediate cause of digestion, his experiments on the gizzards of birds were unnecessary; since it would have been sufficient to have examined the food after it had been masticated by the teeth of animals who have grinders; the teeth and gizzard answering one and the same purpose: but the circumstance of animals who masticate their food

in their mouth, having also a stomach, should have taught, that there was something more in digestion than trituration.

Reaumur's first experiments were made to ascertain the strength of the gizzard, with its effects; to prove that sharp cutting substances, when swallowed, in no way injured its internal coat; and that the common food of the bird was not dissolved when guarded against its action. Yet after all these proofs he seems to doubt, and says, "are we to conclude that grinding alone is sufficient to convert the grain and other aliment into a matter proper for the nutrition of the animal, without undergoing any other preparation? Several reasons seem to oppose this: trituration alone might reduce the grain into a flour; but flour alone is not chyle." "From the smell of the aliment (taken from the gizzards of birds) are we not led to conclude that it undergoes a fermentation? This smell may be said to arise from the liquor with which the aliment is mixed; but is it likely that juices do not dispose to fermentation substances in which it is so easily excited? Fruit and flour, made into a paste, require little more than heat to make them ferment." From these experiments, made with a view to prove that digestion is carried on by trituration, Reaumur was led to suppose a solvent. But as there are some birds whose stomachs do not seem sufficiently strong to have the power of trituration, he selected the buzzard, as being of that kind, and the fittest, for the subject of his experiments, from the circumstance of its throwing up whatever is solid and indigestible; therefore, without killing the bird, he could know the result, and repeat the experiment as often as he thought necessary.

From the stomach in the buzzard being incapable of trituration, he concluded that a solvent was necessary for digestion; but to preclude all mechanical effects of the stomach, in his experiments, he employed tin tubes filled with meat, which, after the tubes had remained twenty-four hours in the stomach of the buzzard, was reduced to three-fourths of its size, was like threads, and was neither putrid, sour, nor volatile, but insipid. On this effect he made his remarks, which are very pertinent. In another experiment which was still more accurate and conclusive, he was convinced of the action of a solvent. He then tried the soft bones of

young animals, and found they were digested; and that though the hard bones were not acted on so readily; yet by returning the same bones several times into the stomach, they were digested at last.

Reaumur was next anxious to know, if such birds as were intended by Nature to live upon meat, could also digest vegetables; but the result was not so satisfactory. He gave bread to his buzzard, which upon being returned had the appearance of having been chewed. He next tried a piece of ripe pear; which, after having been twenty-four hours in the stomach had lost some of its weight, and had the appearance of being boiled or baked; and thence he concludes that its powers are too weak to digest vegetables so as to nourish the animal.

To ascertain the nature of the liquor which had such powers, he tasted the jelly to which the meat and bone had been reduced, supposing that it must be well impregnated with this fluid; but he could only distinguish a bitter or a saltish taste. To have an opportunity of more certainly determining the nature of this solvent, he made his buzzard swallow small tubes filled with sponge, which imbibed fifty grains of this liquor, having the same taste as the jelly, and changing blue paper to a red. He tried the effects of this liquor on meat out of the body, with comparative experiments in water; and after twenty-four hours, the meat in the water was become putrid; but that in the liquor from the stomach was only softened, not dissolved. To see how far the analogy held good in membranous stomachs, he gave two bones to a dog, which being killed, after twenty-six hours, they were found lessened in size, and become as soft as horn. He found that the stomach of the dog did not alter the shape of any of his tubes.

He conveyed grass and hay, enclosed in tubes, into the stomachs of ruminating animals, which substances were not digested, but appeared as if macerated.

Let us enumerate the experiments and facts made out by Reaumur.

The gizzard was not hurt by acting upon glass, which it ground to a powder.

The stomach, or gizzard, had hardly any visible motion.

The force of the gizzard was ascertained.

The size of the stones found in the gizzard was in proportion to the size of the bird.

The stomach of a buzzard digested bone ; from which he concluded the gastric juice had a solvent power ; but it did not digest bread, altho' it acted in a slight degree on fruit.

He made experiments with the gastric juice.

The juice in the ruminating animals stomachs produced no effect on hay or grafs, when inclosed in tubes.

Reaumur's experiments, although not complete, paved the way for future investigation ; and Spallanzani proceeding on the same ground, has not only confirmed them by his own, but has established several points not completely made out by Reaumur ; for in some instances Reaumur gave up the point too soon, especially in the experiments respecting the buzzard's power of digesting vegetables. Reaumur not possessing general knowledge sufficient to direct him in his pursuits, was necessarily confined to what he was most master of, the mere making experiments. Being neither an anatomist, nor a physiologist, he has not been perfectly just in his description of parts ; having considered the crop and the œsophagus leading from it to the gizzard, as two distinct stomachs : but this however is only to be set down as a piece of anatomical ignorance, not affecting the subject in the least. Spallanzani is also deficient in his anatomical knowledge ; yet it must be owned, that his experiments, as far as they go, are in themselves conclusive ; but like all mere experiment-makers, he is not satisfied even with those which are clear and decisive, but multiplies them most unnecessarily, without varying them to elucidate other and essential parts of the same subject. I think we may set it down as an axiom, that experiments should not be often repeated, which tend merely to establish a principle already known and admitted ; but that the next step should be, the application of that principle to useful purposes. If Spallanzani had employed half his time in this way, and had considered digestion under all the various states of the body and stomach, with all the

varieties of food, both natural and artificial, he had employed his time much better than in making experiments without end.

The food of animals in general being composed either of vegetables, animals; or both, and a solvent admitted as an agent in digestion; it only remained to prove, that the effect of the process of digestion was to produce from these various substances an animal matter, similar in all animals who live on such substances. But the application of principles requires more than simply the knowledge of the principle itself; and therefore those who cannot reason from analogy, or draw general conclusions from a few convincing facts; and who require to have every relative conclusion or inference proved by an experiment, must be pleased with Spallanzani; but he must tire even those whom he informs, and much more those who read his works in expectation of something new.

To make comparative experiments upon the digestive power, the different animals, destined for that purpose, should be under similar circumstances as far as relates to digestion; they should be equal in age, for the growing eat more than the full-grown, and of course digest faster; which point therefore can be best ascertained by selecting those in each class of animals which have attained their full growth. They should be equal in fatness, for this makes a very material difference in the powers of digestion in the same animal; and they should be equal in health, a circumstance which of all others, probably makes the greatest difference in the powers of the stomach. In comparing animals of the same class, the atmosphere should likewise be of the same temperature; for the different classes of animals are variously affected by the same degree of heat. Experiments made upon snakes and lizards in the winter, will differ greatly from those made in the summer, while similar experiments made on dogs will have nearly the same result in both seasons. Nor will the powers of the stomach be found always equal in the same class. Sleeping animals, of the quadruped kind, as hedge-hogs, do not digest in the winter, but in the summer only; therefore the conclusions to be drawn from experiments made respecting the digestive powers in the one, are not at all applicable to those made in the other season.

Spallanzani observed that the snake digested food faster in June, when the heat was at 82° and 83° , than in April, when it was only 60° ; from whence he concludes, that heat assists digestion; but this heat is not the immediate, but the remote cause of the increased power; heat having produced in the animal greater necessity for nourishment, and of course greater powers, gastric juice was therefore secreted faster or in greater quantity.

As a proof that heat does not act as an immediate, but only as a remote cause in assisting digestion, I shall mention the effect it produced upon a hedge-hog, the subject of Mr. Jenner's third experiment on the heat of that animal, related in the former part of this work*.

“The hedge-hog, while the heat of the stomach was at 30° , had neither desire for food, nor power of digesting it; but when increased by inflammation in the abdomen to 93° , the animal seized a toad which happened to be in the room; and upon being offered some bread and milk, it immediately eat it. The heat roused up the actions of the animal œconomy; and the parts being unable to carry on these actions without being supplied with nourishment, the stomach was stimulated to digest, to afford them that supply.”

Spallanzani also mentions the slowness of digestion in serpents, and quotes Bomare, who gives an account of a serpent at Martinico, in whose stomach a chicken had remained for three months without being completely digested, the feathers still adhering to the skin†. The truth of this fact I should very much doubt, especially in so warm a climate as that of Martinico; where I must suppose the digestive powers to be constantly required, unless there is in Martinico, as in colder climates, a torpid season, where the act of digestion is not necessary; but in that case the serpent would not have swallowed the chicken. At Bellisle, in the beginning of the winter 1761-2, I conveyed worms, and pieces of meat, down the throats of lizards when they were going into winter quarters, keeping them afterwards in a cool place. On opening them at different

* Vide page 112.

† Bomare Dict. d' Histoire Nat.

periods, I always found the substances which I had introduced entire, and without any alteration: sometimes they were in the stomach; at other times they had passed into the intestine; and some of the lizards that were preserved alive, voided them towards the spring, with but very little alteration in their structure. So that digestion is regulated by the other actions of the body: warmth requires action suitable to that warmth; the body requires nourishment suitable to that action; and the stomach being called upon performs the office of digestion.

Nothing can show more clearly that the secretion of the gastric juice is increased in proportion to the call for nourishment in the body, than what happened to Admiral Byron, Captains Cheap and Hamilton, when shipwrecked on the West Coast of South-America; who, after suffering months of hunger and fatigue, were reduced to skin and bone; yet when they came to good living, Byron thus expresses himself*; “He ordered, (viz. the Governor) a table to be spread for us, with cold ham and fowls, which only we three sat down to, and in a short time dispatched more than ten men with common appetites would have done. It is amazing that our eating to that excess we had done, from the time we first got among these kind Indians, had not killed us; we were never satisfied, and used to take all opportunities, for some months after, of filling our pockets when we were not seen, that we might get up two or three times in the night to cram ourselves. Captain Cheap used to declare that he was quite ashamed of himself.”

Spallanzani has made several attempts to prove what few will subscribe to; that stones in the gizzards of birds are of no use towards the breaking or grinding down the grain; and that they are picked up without design. These stones have long been supposed to answer the purposes of trituration; and have been considered as affording assistance to the stomach, in the manner of teeth, and of course as being necessary to the act of digestion. Spallanzani combats this opinion; but as stones are universally found in gizzards, and it was necessary to account for the mode of there being conveyed there, he attributes it to chance. But we find that

the gizzards which have most occasion for them, and are most able to use them, are likewise best supplied with them: to corroborate which facts, may be added what we observed before, that in the larger gizzards are found the largest pebbles. In a turkey, two hundred were found; in a goose, a thousand; which could not depend entirely upon chance. In trying whether the stones were of service, Spallanzani introduced tubes, needles, and lancets into gizzards in which there were but very few stones, yet found them broken. In this experiment these substances had been forty-eight hours in the gizzards; whereas in the former experiments, with the same kind of tubes, thirty-six hours was the longest time; in another, eighteen hours; and in another, the breaking of them was begun in less than two hours; therefore the experiments were not perfectly fair, as the times were not equal. What he thinks the most conclusive is, that where he had taken care there should be no stones, the hard indigestible substances were acted upon much in the same way as when there were stones; but in this experiment he does not give the time, which is very accurately stated in most of the others.

He discovered that the inner surface of the stomach was not hurt by such substances; and indeed it is scarcely possible for the inner coat of the stomach of a fowl to be pierced by such as are even sharp pointed, the quantity of its motion being so inconsiderable, as hardly to make them pass through its inner coat. But the principal cause of their being harmless, arises from the motion being lateral, and not pressing perpendicularly to the axis, one surface sliding in a contrary direction to the other; and that not in a straight, but in a circular direction, as will be explained hereafter.

In considering the strength of the gizzard, and its probable effects when compared with the human stomach, it must appear that the gizzard is in itself very fit for trituration; we are not however to conclude that stones are entirely useless; for if we compare the strength of the muscles of the jaws of animals who masticate their food, with those of birds who do not, we shall say that the parts are well calculated for the purpose of mastication; yet we are not from thence to infer that the

teeth in such jaws are useless, even although we have proof that the gums do the business when the teeth are gone. If stones are of use, which we may reasonably conclude they are, birds have an advantage over animals having teeth, so far as stones are always to be found, while the teeth are not renewed. Spallanzani concludes, "That we have at length a decision of the famous question concerning the use of these pebbles, so long agitated by authors; it appearing that they are not at all necessary for the trituration of the firmest food, &c." but says, "He will, however, not deny that when put in motion by the gastric muscles, they are capable of producing some effects on the contents of the stomach." Now if we constantly find in an organ substances which can only be subservient to the functions of that organ, should we deny them to be of any use, because the part can to a certain degree do its office without them?

To account for pebbles being found in the gizzard, Spallanzani supposes the birds to have picked them up by chance, or not to have distinguished between their food and these stones. But it appears singular, that only those which have gizzards should be so stupid; and he owns, that Redi and himself found that birds died of hunger, yet without having picked up more stones than usual; which we might suppose they would have done if they had not had a choice, or could not have distinguished stones from the grain on which they feed.

The stones assist in breaking the grain, and by separating its parts in the beginning of the process, and afterwards by rubbing off the surface already digested, allow the gastric juice to come more completely in contact with the whole.

It has been said, that the motion of the gizzard is so small as hardly to be observable; and that it cannot be felt by the hand. But as its cavity is very small, and must be capable of adapting itself to the quantity it contains, (or it could not possibly grind) much motion is not necessary for the purposes of trituration: a swelling and collapsing, like the motion of the heart, would have no effect. The extent of motion in grindstones need not be the tenth of an inch, if their motion is alternate and in contrary directions. But although the motion of the gizzard is hardly visible,

yet we may be made very sensible of its action by putting the ear to the sides of a fowl while it is grinding its food, when the stones can be heard moving upon one another.

It may be remarked, that the motion of the whole intestinal canal, from the fauces to the anus, is naturally so slow, as not to be excited into quick actions. The food passes slowly along the œsophagus; and in a man, fluids which might be expected to act even by their own gravity, descend but slowly: yet I think we may be certain that the œsophagus has always a regular contraction; and that the lower parts must relax in progression, as it contracts above; so that no position of the body makes any difference in this action.

Upon exposing the stomach in living animals, it does not appear much agitated or affected, even by being handled or otherwise irritated. The same thing may be observed in the whole track of intestines: and we find that when the fæces are expelled by the action of the gut alone, that the expulsion is slow; the stomach and rectum, however, can be emptied at once; but that is done by the abdominal and other muscles. We know that the action of vomiting is performed entirely by the diaphragm and abdominal muscles; and we know that by the same action the contents of the rectum can be expelled. Neither is any other power required to empty the stomach in vomiting, these muscles being often capable of forcing the bowels themselves out of the abdomen, and of producing a rupture. It is not necessary the stomach itself should act violently to produce an evacuation of its contents; nor is it even necessary it should act at all: for the lungs themselves do not act in the least when any extraneous matter is to be thrown up; and coughing is to the lungs, what vomiting is to the stomach. The muscles of respiration are the active parts in emptying the lungs, and can act both naturally and preternaturally. The muscles of the thorax and abdomen do not act naturally on the contents of the abdomen, but often act preternaturally, producing an evacuation from its viscera.

There is this difference in the action of the parts in coughing and vomiting: the cough is performed by the proper muscles of respiration,

which are those of expansion, supported by the abdominal, while the diaphragm is passive.

Vomiting is performed by the abdominal muscles and diaphragm; while those of inspiration are supporting this action.

In coughing, the ribs are suddenly depressed, which diminish the capacity of the thorax; and that the diaphragm may not be allowed to sink down and increase the capacity of the thorax, which would counteract the depressors of the ribs; the abdominal muscles at the same time act, which supports the diaphragm in its place, and probably may by this action assist in bringing down the ribs. To give as much force to this action as possible, the glottis is shut till the action is begun, and then the glottis opens instantaneously, which obliges the depressors of the ribs to begin the effort with their full action.

The proper muscles of inspiration do not tire so soon in this action as the abdominal; for in violent coughing the muscles of the abdomen become sore.

In vomiting, these actions are reversed. The muscles of the cavity of the abdomen act, in which is to be included the diaphragm; so that the capacity of the abdomen is lessened, and the action of the diaphragm rather raises the ribs; and there is also an attempt to raise them by their proper muscles, to make a kind of vacuum in the thorax, that the œsophagus may be rather opened than shut, while the glottis is shut so as to let no air enter the lungs. The muscles of the throat and fauces act to dilate the fauces, which is easily felt by the hand, making there a vacuum, or what is commonly called a suction; so that when all these actions take place together, the stomach is immediately emptied.

In violent coughing, we find that a kind of mixed action takes place; for although the diaphragm has not acted, yet the stomach is so much squeezed as to discharge its contents; and it affects the diaphragm, which is often thrown into action, and brings on vomiting at the same time; therefore violent coughing palls the stomach.

There is reason to believe that the natural motion in all stomachs is regular; and I am more inclined to be of this opinion, from what takes

place in the stomach of animals who are covered with hair, and who lick their own bodies; and of such as feed on whole animals, who are likewise covered with hair. In the stomach of the calf, for instance, who licks his skin with his tongue, and swallows whatever is attached to the rough surface of that organ, balls of hair are often found; and on examining their surface, the hairs in each hemisphere seem to arise from a centre, and to take the same direction, which is circular, corresponding to what would appear to be the axis of this motion, and resembling what we see in different parts of the skin of animals whose hair takes different turns. This regularity in the direction of the hair, in such balls, could not be produced if there was not a regular motion in the stomach. This motion is also proved in the dog; for I have seen a ball of this kind, that had been thrown up from a dog's stomach, where the same regularity in the turns of the hair was very evident and complete. The same motion seems also to take place in the bird kind; and of this the cuckoo is an example, which, in certain seasons living on caterpillars, some of whom have hairs of a considerable length on their bodies, the ends of these are found sticking in the inner horny coat of the stomach or gizzard, while the hairs themselves are laid flat on its surface; not in every direction, which would be the case if there was no regular motion, but all one way, arising from a central point placed in the middle of the horny part; and the appearance on the surface of both sides of the gizzard evidently corresponding. These two facts prove, in my opinion, a regular circular motion taking place in the gizzard and membranous stomach; and therefore, most probably, something similar is carried on in stomachs of all the various kinds. Indeed this motion in the stomach is so considerable, that when there is no horny defence, we find the coats sometimes pierced by hard pointed substances. Thus the cows who feed on the grafs of bleaching-grounds have their stomachs, especially the second, stuck full of pins: and fish who prey upon, and swallow other fish entire, often have their stomachs pierced by the bones.

Spallanzani calls the inner coat, cartilagenous; whereas, in fact, it is a horny substance, forming an inner cuticle, but differing in some respects

from the common cuticle; this horny substance not only differs in structure from the common cuticle, but in its attachment, from cuticle, nails and hoofs. The cutis, where it is covered by such substances, has a vast number of villi on its surface, which pass into corresponding perforations in the cuticle; from this structure of parts, when the cuticle, nails, or hoofs are separated, their inner surface appears to be full of small perforations; and the cutis from which they have been removed is villous; and these villi are more numerous in some parts than in others, where the sense of touch is required to be delicate or acute. But the inner lining of the gizzard is just the reverse; that surface of the horny substance which is in contact with the gizzard being villous; and when separated, the inner surface of the gizzard appearing perforated. These villi are either the last formed parts of this horny substance, or are the fibres of which the horny coat is composed. It is probable, that this horny substance takes the form of villi that it may be more firmly connected with the gizzard, in which acute sensation is not required.

I may remark here, that the experiments made on the digestion of ruminating animals have been very deficient, arising from this process in them being more complicated than in the stomachs of other animals; and requiring attention to be paid to certain circumstances, which cannot take place in stomachs of only one cavity.

The circumstance mentioned by Spallanzani, of ruminating animals voiding the tubes by the anus, shows that the whole food is not necessarily returned into the mouth to be chewed a second time; for if it were, the tubes would certainly come up likewise, and would as certainly be thrown out of their mouths as improper to be chewed, a circumstance which often really happened. But it was hardly necessary to make experiments to ascertain whether ruminating animals digested meat, when we know that in some cold countries, the cattle are fed on dried fish; and most animals eat their own fecundines: indeed the circumstance of animals living upon both animal and vegetable food might have taught us, that the mode of digesting both (whatever it is) was the same; therefore all that was wanted must have been to discover that mode; except

we could absurdly conceive, that two different modes might take place in the same stomach at the same time.

Spallanzani gives the opinion of authors respecting digestion; and so anxious is he to combat the idea of its being fermentation, that he will hardly allow that fermentation ever takes place in the stomach. That fermentation can go on in the stomach, there is no doubt; but when that happens, it arises from the powers of digestion being defective. Milk, vegetables of all kinds, wine, and whatever has sugar in its composition, become much sooner sour in some stomachs, than they would, if left to undergo a spontaneous change out of the body: and even spirits, in particular stomachs, almost immediately degenerate into a very strong acid. I am inclined to suppose, that it is the sugar which is converted into spirit, and the spirit into acid; consequently, a glass of brandy, from being much stronger, because less diluted, most probably contains as much matter, likely to become acid, as half a pint of wine. In other substances, besides those mentioned above, the fermentative process (unless prevented by that of digestion) appears to begin sooner in the stomach than out of the body. All oily substances, particularly butter, very soon become rancid after being taken into the stomach; and this rancidity is the effect of the first process of the fermentation of oil. Mr. Sieffert has been able to restore rancid oils to their original sweetness, by adding to them their due quantity of fixed air*; the loss of which I consider as the first process in this fermentation, similar to what happens in the fermentation of animal and vegetable substances.

Animal food does not so readily ferment in the stomach, when combined with vegetables, as when it is not; for the vegetables running more quickly into fermentation, preserve the meat from putrefaction. Put a piece of meat and some sugar, or bread, into water, and let them stand in a warm place, the bread and sugar will begin to ferment, the water will become sour, and the meat be preserved; but the acid becoming weaker, as the fermentation advances towards the putrefactive, the meat at last

* Physical and chymical Essays, by Sir Tobern Bergman.

begins to acquire the same putrid disposition*. Yet this last part of the process cannot, I think, take place in the stomach, as a succession of acids will be formed, by which the meat will be preserved sweet till it is digested: the formation of this acid in the stomach, most probably, not preventing the digestion of those substances which are incapable of being rendered acid.

Bread allowed to remain in the stomach of a dog for eight hours, is so much changed, that it will not run into the vinous fermentation; but when taken out and kept in a warm place, becomes putrid: its putrefaction, however, is not so quick as a solution of meat that has been in the stomach for the same length of time. Similar effects are produced when milk and bread are the food administered; and perhaps the gastric juice, when in sufficient quantity, will always prevent the vinous fermentation.

Spallanzani's next trials were to determine, whether the gastric juice had the power of recovering meat already putrid: a fact which might have been proved by one experiment. For if very putrid meat is given to a dog, and the dog killed after some time, the meat will be found sweet, and all putrefaction at an end. Therefore his allowing fresh meat to continue a longer or shorter time in the stomach was immaterial, as it could not become putrid.

It appears from the above facts, that the stomach has not so much power in preventing the acetous fermentation in vegetables, as in correcting the putrefactive disposition in animal substances. For although this cannot be certainly known in those who eat both animal and vegetable food; yet it does not appear that the putrefaction of animal substances, where nothing else is eaten, takes place so quickly in the stomach, as the change which is produced in vegetables; the acetous disposition is therefore either stronger than the putrefactive, or it more readily takes place: and indeed the living body shews this sufficiently; for we very often find an acid thrown up, but seldom or ever any thing putrefactive.

It may be admitted as an axiom, that two processes cannot go on at the same time, in the same part, of any substance; therefore neither vegetable

* Of this Sir John Pringle was not aware in making his experiments on this subject.

nor animal substances can undergo their spontaneous changes while in the act of being digested; it being a process superior in power to that of fermentation. But if the digestive power is not perfect, then the vinous and acetous fermentation will take place in the vegetable; and the putrefactive in the food of those animals which live wholly on flesh; although in the last I imagine but very seldom. The gastric juice, therefore, preserves vegetables from running into fermentation, and animal substances from putrefaction; not from any antiseptic quality in the juice, but by making them go through another process, preventing the spontaneous change from taking place. In the greater number of stomachs there is an acid, even although the animal has lived upon meat for many weeks; but as this is not always the case, we must suppose it is only formed occasionally. Whether the stomach has a power of immediately secreting this acid, or first secretes a sugar which afterwards becomes acid, is not easily ascertained: but I should be inclined to suppose, from analogy, the last to be the case; animals in health seeming to have the power of secreting sugar; for we find it in the milk; and sometimes in the urine, in consequence of disease. Acid sometimes prevails in the stomach to so great a degree, as to become a disease, attended with very disagreeable symptoms; the stomach converting all substances which have a tendency to become acid, into that form. To ascertain whether there was an acid naturally in the stomach, the most satisfactory mode was to examine the contents before the birth, when the digestive organs are perfect, and when no acid could have been produced by disease, or any thing that had been swallowed: accordingly in the flink-calf, near the full time, there was no acid found in the stomach; although the contents had the same coagulating powers with those of animals who have suckled.

As we find stomachs possessed of a power of dissolving the whole substance of a bone, it is reasonable to suppose that its earth is destroyed by the acid in the stomach.

The stomach appears not only to be capable of generating an acid, but also to have the power of producing air; which last effect, I believe, arises from disease. It is not easy to account for the formation of this air; yet

as the stomach is a reservoir for substances disposed to ferment, it might reasonably be supposed to arise from the food going into that process. But this, in my opinion, will not account for the vast quantity of air frequently thrown up from the stomach, even where food has not been swallowed for a considerable time, and where digestion appeared to have been completed. For we must conclude this process to have been completed, if the food was not found to have disagreed with either stomach or bowels, and that the stools were good. When the gout falls on the stomach, the quantity of air thrown up is often immense; and the same thing may be observed in some cases commonly called nervous; yet the process of digestion will not account for this formation of air; as no air is to be found in healthy stomachs*; neither is it to be accounted for from a defect in digestion, as that would probably be productive of worse consequences.

I am inclined to believe that the stomach has a power of forming air, or letting it loose, from the blood, by a kind of secretion. We cannot, however, bring any absolute proof of this taking place in the stomach, as it may in all cases be referred to a defect in digestion: but we have instances of air being found in other cavities, where no secondary cause can be assigned. I have been informed of persons who have had air in the uterus or vagina, without having been sensible of it, but by its escaping from them without their being able to prevent it; and who, from this circumstance, have been kept in constant alarm lest it should make a noise in its passage, having no power to retard it, as when it is contained in the rectum. This fact being so extraordinary, made me somewhat incredulous; but rendered me more inquisitive, in the hope of being enabled to ascertain and account for it: and those of whom I have been led to inquire, have always made the natural distinction, between air passing from the vagina, and by the anus; that from the anus they feel and can retain; but that in the vagina they cannot; nor are they aware of it till it passes. A woman, whom I attended with the late Sir John Pringle,

* In all my experiments on digestion, in dogs, I have never been able to detect any air in the cavity of the stomach.

informed us of this fact; but mentioned it only as a disagreeable thing. I was anxious to determine, if there were any communication between the vagina and rectum, and was allowed to examine, but discovered nothing uncommon in the structure of these parts. She died some time after; and being permitted to open the body, I found no disease either in the vagina or uterus. Since that time I have had opportunities of inquiring of a number of women, concerning this circumstance, and by three or four have been informed of the same fact, with all the circumstances abovementioned: how far they are to be relied upon I will not pretend to determine. I have likewise found air in the cellular-membrane, in gunshot wounds, that had passed some way under the skin, without being able to account for its being there by any mechanical effect of the ball.

That air is either formed from the blood, or let loose by some action of the vessels, both naturally and from disease, is an undeniable fact. We find air formed in some fishes, to answer natural purposes; for in those whose air-bladders do not communicate externally (many of which there are) we must suppose it to have been formed there. We also find it in animals after death; and I have a piece of the intestine of a hog which has a number of air-bladders upon it*. Mr. Cavendish was so kind as to examine this air, and he found "it contained a little fixed air; and the remainder not at all inflammable, and almost completely phlogisticated." I have often seen such vesicles on the edges of the lungs; but these may be supposed to have been a kind of aneurismal air-cells filled from the trachea, and are circumscribed and impervious; so that in the state we find them, they have no communication with the external air. In one instance I have discovered air in an abscess, which could not have been received from the external air; nor could it have arisen from putrefaction. The case is as follow:

A lady, about forty years of age, had been afflicted with complaints in the bladder and parts connected with it. From the symptoms, her disease was supposed by some to be the stone, though upon examination no

* Vide Plate.

stone was found; and she had also an umbilical hernia, for which I had been consulted. She grew gradually worse; and from being lusty became a thin woman. A small tumor appeared in the groin, and the skin over it became red, similar to an abscess when the matter is beginning to point externally; but before her death this subsided. A few days before she died, I was desired to examine a swelling on the lower and right-side of the belly, extending nearly from the navel to the spine of the ilium on the right-side. It was tense, evidently contained air, and could be made to sound almost like a drum. It had come on within a few weeks, and I was puzzled to account for it; there being clearly no connection between that tumor and the umbilical hernia. I was inclined to suppose it to be a ventral hernia, containing the cæcum and part of the colon, filled with air; but as she had stools; as there were no symptoms of a strangulated gut, nor any uneasiness in the bowels; as I could not make the air recede, but felt it as if confined to that part, I own I could form no conjecture what the case really was. The woman dying in a few days, I was permitted to examine the body. That I might not interfere with the tumor, or umbilical hernia, I made an opening into the abdomen on the right-side of the linea alba; and on examining the cavity of the abdomen, found every thing natural, except a small portion of the epiploon adhering to the inside of the navel; the parietes of the abdomen corresponding with the tumor, being in a natural state. On pressing the tumor by the hand, air was heard to make its escape: whether by the vagina or anus was at first doubtful; but on examining with more attention, it was discovered to come from between the labia. I next opened the tumor externally, and let out the air, which was not in the least putrid, and was contained in a sac tolerably smooth on its inside, made up of compressed cellular-membrane; the abdominal muscles and tendons forming the posterior surface, which extended as low as the inferior edge of Poupart's ligament. The contents of the abdomen were tolerably sound; but when I inspected the viscera contained in the pelvis, they were found adhering to each other; the bladder to the body of the uterus; the broad ligaments and ovaria, to the uterus; and on examining these adhesions, I discovered

a cavity between the bladder, uterus, and vagina on the right-side, something like an abscess. From the right-side of this cavity there was a canal ascending to the brim of the pelvis, in the course of the round ligament, as far as to the going out of the iliac vessels, which it seemed to accompany; and this canal when it passed from behind Poupart's ligament, communicated with the tumour abovementioned. I then endeavoured to discover if there were any communication between the rectum and the abscess; but could find none, the gut appearing to be quite sound. Having removed the whole contents of the pelvis, with the canal leading to Poupart's ligament, and the ligament itself, with such of the abdominal muscles as composed part of the sac, I found both the rectum and vagina perfectly sound. The uterus had a polypus forming on its inside; neither the rectum nor uterus had any connection with the abscess; but there was a small communication between the abscess and the bladder; that portion of the bladder which made part of the abscess being very much diseased.

From this history of the appearances of the tumour before death, and the particular account I have given of the dissection, the reader may be able to draw his own conclusions relative to the origin of the air. It certainly appeared to have been formed in this bag; and it was only towards the latter end of her life that it could have made its escape into the cavity of the bladder; for it was not possible to squeeze the air out of the tumor, when I first saw her; but just before death it became more flaccid. It could not be formed or let loose in consequence of putrefaction, for the air itself was free from any smell; and although the cavity between the vagina and bladder had on its internal surface the irregular ulcerated appearance of an abscess, yet that on the abdomen had not, was tolerably smooth, and had rather the appearance of having been formed in consequence of some foreign matter accumulating there.

This circumstance, of an animal having the power of forming air, or separating it from the juices by a kind of secretion, appears at first view to be supported by the experiments of Dr. Ingen-houfz.*

* *Exp^{ts} upon Vegetables; proving their great Power of purifying the common Air, &c.*

The Dr. observed that, when we immerse our bodies “in a cold or warm bath”; or, “By plunging the hand and arm even in cold water,” that globules of air soon appear upon the skin: and to be certain of the air coming from the body, he took all the necessary precautions to prevent the external air being carried into the water along with the body, (which would certainly be a consequence, if the body or part were immersed quickly, or when dried.) But although his experiments seem to prove this opinion, yet I imagine there is a circumstance the Dr. did not attend to at the time, which renders them very fallacious; for he did not consider that water, for the most part contains a great deal of air; therefore the globules of air might as readily come from the water, as from the body; which makes it necessary to ascertain, by experiment, from whence the air comes which is attached to the body when immersed in water.

Water takes up air in proportion to its coldness, until it loses the property of water and becomes solid: upon this principle we may account for globules of air being found attached to the skin, when a part of the body is immersed in water colder than itself; for when we immerse the whole body we increase the heat of the water, especially that next to the skin; and if we immerse only a part, as an arm, it being commonly in a smaller quantity of water, the water immediately surrounding it is also warmed. As a proof that it is the air from the water, and not from the surface of the body* it matters not what the substance is that is immersed, if it is but warmer than the water: for a piece of iron heated to about 150°,

* “Count de Milly, in the Berlin Transactions for the year 1777, published experiments to show that there is an excretion of air; or, as it is termed, ‘an aerial transpiration,’ from the whole surface of the human body while it remains in warm water: but Dr. Pearson found, on repeating these experiments, that there was no appearance of aerial bubbles on the surface of the cuticle during bathing in warm water that had been previously boiled, so as to expel the air usually mixed and united to river, and spring-water. The human body when immersed in the bath at Buxton, and kept at rest in it for some time, was covered with air-like bubbles; but these bubbles appeared in the same manner, on any solid body whatever that was placed in it. It is therefore supposed that the attraction to the human body of the air, commonly suspended in water, especially when heated to the temperature of a warm-water bath, has been mistaken for an excretion of air from the cuticle.”

immersed in water about 70° , will warm the water in contact with it so as to make it part with its air. This effect of heat is further proved, by making another trial, with only this difference, that the iron be ten degrees colder than the water; in that case little or no air will be separated, and of course no bubbles observed. The bubbles of air do not appear to arise entirely from the degree of warmth of the water; but also in some measure from a solid body being immersed in it, that seems to have a power of attracting the air, whose affinity to the water is now weakened by heat; for simply heating the water to the same degree will not separate the air, as we find that no bubbles are then produced. The power of attracting the air appears therefore, in some sort, to depend upon the solidity of the body immersed; at least, bodies have a greater number of bubbles in proportion to their solidity: for upon making comparative experiments between iron, stone, wood, and cork, the air separated from the water upon the surface of the iron and stone is in considerable quantity; that upon the wood very small; and scarcely any at all upon the cork.

As these observations on the generation (or secretion) of air in cavities, seemed to have a connection with the present inquiry, I thought they might, properly enough, be introduced here; but I shall content myself with having mentioned the circumstance, and pursue the subject of digestion.

To determine with absolute certainty, in what particular portion of the canal this important process of digestion is performed, is perhaps impossible; but there is the greatest reason to believe that it is principally carried on in the stomach, with a little variation in different animals. We may venture to affirm that, it does not at all take place in the long and contracted œsophagus of the quadruped; the secretion of that part being a slimy mucus, possessed of no power similar to that of the gastric juice; but only intended to facilitate the passage of the food into the stomach.

Neither has the mucus secreted in certain parts of the œsophagus of birds, as in the crop of those who have one, any digestive power; while, on the contrary, we find the lower end, which is extremely glandular, to

be capable of secreting a juice with all the properties of the gastric ; and that passing into the cavity of the stomach, becomes a substitute in this class of animals for the deficiency of the secretion of the stomach itself ; which in some is lined with a horny substance, and in others with a cuticle. Even in birds, the seat of digestion is chiefly in the stomach ; the juice secreted in the lower part of the œsophagus passing into that cavity ; and the mucus secreted by the other parts of the œsophagus, as in the crop of those who have one, has no such power. But if any digestible substance should be retained in the œsophagus, as may happen in many of those who swallow whole animals, digestion may even go on in its inferior portion. In the gull and heron, which take down snakes and fish entire, the tails may remain in the œsophagus till the head is digested in the stomach ; and in such cases the tail itself may be acted upon in that situation.

As a further proof that digestion is carried on principally in the stomach, let us observe what happens to the yolk of an egg in the bird newly hatched. The yolk is not in the least consumed in the time of incubation ; but appears to be reserved for the nourishment of the chick, between the time of hatching, and its either being supplied with food by its parents, or being able to procure it for itself : for we find, that although the yolk passes into the gut at some distance from the stomach, yet it is carried up to the stomach to be digested ; and I have even seen it in the crop, being retained there till wanted.

In those animals whose stomach consists of several cavities, the precise place where digestion is carried on has not been ascertained. I think, however, that in the ruminating class, who have four cavities, it may be set down as a fact, that digestion goes on in the fourth ; which is best proved by feeding the animal with a substance that does not require any kind of preparation for digestion, such as milk. If a calf be killed about half an hour after it has sucked its mother, we shall find the whole milk in the fourth cavity firmly coagulated, and formed into a ball ; while the first, second, and third cavities, contain only such food as requires mastication, or what other preparation is necessary to fit it for digestion. Such

animals have the power of conveying the food from the œsophagus, either to the first or fourth cavity, according to the nature of the food; and for this purpose there is a groove leading directly from the œsophagus to the fourth stomach, which I suppose can be converted into a canal when wanted.

It is possible that digestion may likewise be carried on in the duodenum, especially in its upper part; if either the intestine secretes the same juice with the stomach; or that some of the gastric juice, and part of the food, has passed into the intestine before it has been completely turned into chyle.

Although the stomach is the seat of digestion, it is not solely appropriated to that purpose; and in many animals these organs are not to be considered as only a digesting bag or bags, but in part as a reservoir for food. This is most remarkable in the ruminating animals, where the first stomach or bag is merely a reservoir, and in this respect analogous to a crop. It is the same in the porpus, and, I believe, in most animals of this class: although it cannot be supposed that those return the food who have not the power to masticate. In some animals, who do not ruminate, there is not the same necessity for distinct pouches; the stomach consisting either of one bag, singly; or of stomach with appendages, as in the Pecari. But the whole organ is not endowed with the property of secreting the gastric juice; there being a part whose structure is very different from that appropriated to digestion, and covered by a cuticle, as in the first, second, and third stomach of the ruminating animals; and in the first stomach of the porpus. The pecari, the common hog, and the rat, are likewise instances of this; and the same circumstance takes place, in a smaller degree, in the horse. This increase in the cavity of the stomach, beyond what is necessary for digestion alone, is peculiar to the animals that take in more food than is immediately wanted, or whose food is of a nature which requires a certain degree of preparation prior to digestion. The crop of the eagle, and perhaps the stomach of the porpus, are of the first kind; the crop in the gallinaceous fowls, and the first stomach in ruminating animals, of the second. It is the disposition of such animals to

fill these cavities ; and the quantity which they are capable of containing, makes them seldom require to be filled : it is probable, likewise, that it is the sensation excited by this fulness which gives satisfaction to the animal, and takes off the further desire for food ; an effect similar to what is produced in other animals from filling the stomach itself ; and these having no such provision, are longer and oftener employed in pursuit of food.

I should be apt to consider the power of the gastric juice to coagulate milk, and some other animal mucilages*, as a test of the stomach being the seat of digestion : for although milk may be coagulated by other substances, yet when found in that state in the stomach, it is probably for the purpose of digestion ; milk, and many other natural substances, requiring to be coagulated before they can be digested. I have found this coagulating power in the stomach of every animal that I have examined for that purpose, from the most perfect down to reptiles : and in the appendages which I have considered as only reservoirs preparatory to digestion, (as the first stomach in the ruminating animal, and the crop in birds) I have discovered no such power. Yet it is not the digestive power which coagulates those substances ; complete coagulation taking place even where digestion does not at all go on. This is evident every day in children that suck, and who have diseased stomachs ; for we see them throw up the milk coagulated, and discharge it undigested by stool. A very remarkable instance occurred in a child that had lost entirely the power of digestion ; yet the milk taken down came away strongly coagulated ; some even as firm almost as cheese ; which seems to shew, that the coagulating power is seldom wanting, although the other may.

The gastric juice is a fluid somewhat transparent, and a little saltish or brackish to the taste ; but whether this is essential, or only accidental, is

* Milk is the substance commonly known to be coagulated by the gastric juice : but I find that it has also the same power over the white of an egg. Give to a dog some raw egg, and kill him half an hour after he has swallowed it, the egg will be found coagulated in his stomach, as if boiled ; the crystalline humour, in the stomachs of fishes, is likewise found coagulated.

not easily determined. Indeed, there are very few of our secretions which have not some salt in them; it being found in the tears, the saliva, the secretion of the glans penis, of the glands of the urethra, and in the first and the last milk secreted in the udders of animals.

I am not inclined to suppose that there is any acid in the gastric juice as a component or essential part of it; although an acid is very commonly discovered, even when no vegetable matter has been introduced into the stomach*. The acid may be increased in some diseases; and in others the disposition to form it may be destroyed; which may be the reason why, by a kind of instinctive principle, many girls are fond of eating sour fruit, and of drinking vinegar; while others, on the contrary, from a different cause, often eat chalk, lime, and other substances of that sort: but the acid not being always found, it is not yet determined on what occasions it is formed, or in what manner it is destroyed.

The process of digestion differs from every other natural operation, in the change it produces on different bodies; yet is by no means fermentation, though it may somewhat resemble it. For fermentation, a spontaneous process, is that natural succession of changes by which vegetable and animal matter is reduced to earth; therefore must be widely different from digestion, which converts both animal and vegetable substances into chyle; in the formation of which there cannot be a decomposition, similar to fermentation.

* The only trial to which I ever put the gastric juice, was with the syrup of violets, to ascertain if it was acid; and in many of the trials the colour of the mixture was changed to red: but it is necessary for the accuracy of the experiment, which is to determine this fact, that the animal should not be fed upon vegetables for some time before the trial is made, these being liable in some degree to become sour; therefore it is hardly fair to make the experiment on the contents of the stomach of animals who live upon vegetables. In many trials of this kind we may be deceived, and led to suppose an alkali. For certain animal secretions being of a yellow cast, when such are mixed with the syrup of violets, the mixture is changed to a green. The truth of the experiment may, however, be known by adding a little acid; for if the green has been produced merely by a mechanical mixture, it will become immediately a scarlet, by being then a mixture of red and yellow: but if the secretion is not only of a yellow colour, but of an alkaline nature, it will also continue green; and by adding a little more acid than what saturates the alkali, the colour will then become orange.

Digestion is likewise very different from chymical solution, which is only an union of bodies by elective attraction. But digestion is an assimilating process; and in this respect is somewhat similar in its action to that excited by morbid poisons. It is a species of generation, two substances making a third; but the curious circumstance is its converting both vegetable and animal matter into the same kind of substance or compound, which no chymical process can effect. The chyle is compounded of the gastric juice, and digestible substances when perfectly converted; and it is probable that the quantity of gastric juice may be nearly equal to that part of the food which is really changed into chyle; if so, it demonstrates the necessity of a very quick secretion, to supply a quantity so very considerable; but with this advantage, that it is not lost to the constitution.

The progress of the conversion of food into chyle, may be often seen in the stomach of animals at different times after feeding. Fishes are good subjects on which to make observations for this purpose, as they swallow their food whole; and as that food is commonly fish, and often too large to be completely admitted into the stomach. As they do not masticate their food, it is not adapted to the cavity of the stomach; and therefore part of it is often found lying in the œsophagus; a circumstance by which the comparative progress of digestion is rendered more obvious.

It may also be well observed in the stomach of a dog, in which the whole quantity taken has been swallowed at once. In the great end the food will be but little altered; towards the middle, more; and towards the pylorus, it will be similar to what is found in the duodenum.

From the structure of the stomach in ruminating animals, they are badly adapted to assist our inquiries on this subject: because metallic balls, or whatever is swallowed in so hard and solid a form, as to be unfit for digestion, requiring to be ruminated, will often be thrown out when returned into the mouth for that purpose; or it may lie a long time in the first stomach without being either thrown up or passed into the fourth, as I have frequently seen; therefore the chance of its getting into the fourth stomach in a proper time to fit it for the object of an experiment, being very uncertain, no great light can be derived from trials made on animals of this class.

Live or fresh vegetables, when taken into the stomach, are first killed, by which a flabbiness in their texture is produced, as if they had been boiled; and then they can be acted upon by the gastric juice.

Meat appears to undergo no change, as preparatory to digestion; but at once to submit to its union with the gastric juice: for after having been acted upon, it seems first to lose its texture; then becomes cineritious in colour; next gelatinous; and last, chyle. The first change made upon milk, and some other secretions, as the yolk and white of an egg, is coagulation; after which the gastric juice begins to acquire a power of uniting with them.

The first change which is produced on animal substances, out of the body, either by being exposed to heat, or by becoming spontaneously putrid, is similar to the second of the three changes which takes place in digestion; and is only preparatory to the complete change, whether that be digestion or putrefaction.

It appears from many experiments, that the digested or animalized part, when carried into the intestine, is attracted by the villous coat, or clings to it as if entangled among the villi; while the excrementitious part, such as bile, is found lying unconnected in the gut, as if separated from the other.

The food of animals in general consists of vegetable or animal substances; and vegetables seem intended to support one class, with a view to its being the food of another. Although there are classes of animals intended to subsist on each particular kind of food, yet they do not all invariably keep to the same kind in every stage of life; many being nourished by animal food when young, that afterwards live on vegetables: which circumstance will be more fully discussed when treating of the first food of pigeons.

All stomachs do not equally digest the same substance, although it be their natural food. The caterpillar digests the expressed juice, but not the substance; while other animals are capable of dissolving nearly the whole. Some animals, as the common cattle, can feed on a variety of vegetables, although they may have a preference; but there are others

that will hardly eat of more than one kind. Of this last sort are insects in general; and the silk-worm will scarcely touch any thing but mulberry leaves: but I believe those that live upon animal food are not so restricted in their choice.

It is probable that, all animal and vegetable substances are equally capable of being digested, if equally soft in their texture; but some being much firmer in that respect, and others also united with indigestible matter, as the earth in bones, more strongly resist the powers of the gastric juice; therefore mastication, and trituration, become necessary to bring them to a similar consistence. But substances may be rendered too soft; for a fluid is difficult of digestion; and we may observe that, Nature having given us very few fluids as articles of food, to render these few fitter for the action of the digestive powers, a coagulating principle is provided to give them some degree of solidity*. It is not easy to assign a reason why fluidity should be unfavourable to the process of digestion; more especially as it seems essential to those of fermentation and chymical solution. The requisite degree of solidity, I should suppose to be, that of curd, or what is produced by the coagulation of animal mucilages, as of the white of an egg; but this is only supposition, founded on the idea that Nature's general principles are right, and all the corresponding parts adapted to one another, except when monstrous, either in form or action.

Mastication is the effect of a mechanical power, produced by parts particularly provided for that purpose, which are of various kinds, fitted for that sort of food on which the animal is by Nature intended to live; and may be imitated with equal advantage by many other pieces of mechanism.

The masticating powers are of three kinds. The first is that which merely fits the substance for deglutition, as in the lion, and many other carnivorous animals; and which, in the ruminating tribe, renders the food fit to be swallowed, that it may undergo such preparation in the first sto-

* The circumstance of the chryalline humour, which is solid, being coagulated, prior to its being digested, renders it probable that all animal substances go through that process; and that the loss of texture, which they undergo, arises from coagulation.

mach as is necessary before it is further masticated for digestion. The second is that, which not only fits the food for deglutition, but exposes it to the action of the gastric juice, by breaking the shells or husks in which the nourishment is contained, and in which it would be defended from the powers of digestion. And the third is, that which divides and bruises the food, before it is received into the stomach; which mastication is of considerable service, by producing a saving in food.

The husk, of the seeds of plants, although a vegetable substance, appears to be indigestible in its natural state; whether this arises from the nature of the husk itself, or from its compactness, I am not quite certain, but am inclined to suppose the last; as we find the cocoa, which is only a husk, to be digestible when ground to a powder and well boiled. We know likewise, that cuticle, horn, hair, and feathers, although animal substances, are not affected, in the first instance, by the gastric juice; yet if reduced in Papin's digester to a jelly, that jelly can be acted upon in the stomach; we must therefore suppose that, a certain natural degree of solidity in animal and vegetable substances renders them indigestible. This compactness in the husk seems to be intended to preserve, while under ground, the farinaceous part of the seed, in which the living principle is placed; the husk having probably no other power of resisting putrefaction than what arises from its texture; but whatever may be the use of the husk, it must be connected with the vegetative process of the plant. The same purpose of preservation is probably answered by the shells of all ova. Although husks are not capable of being dissolved in the gastric juice, they allow of transudation; and that the seed is in some degree affected by it, is known by its swelling in the stomach; yet it can only take up a certain proportion of it, and that not sufficient to convert it into chyle; the gastric juice having no power of action upon the husks themselves; therefore we see grain of all kinds, when swallowed whole, pass through entire, though swelled; and even the kernels of some nuts, as chestnuts, are not digestible when eat raw.

The essential oils of vegetables and animals are indigestible; but being soluble either in the gastric juice or chyle, they become medicinal, from

their stimulating powers. The essential oil of vegetables, but more particularly that of animals, seems to pervade the very substance of those animals whose food contains much of this oil. Thus, we find sea-birds, whose constant food is fish, taste very strongly of fish; and those who live on that kind of food, only during certain times of the year, as the wild duck, have that taste only at such seasons. This fact is so well known, that it was hardly necessary to put it to the test of an experiment; yet I took two ducks, and fed one with barley, the other with sprats, for about a month, and killed both at the same time; when they were dressed, the one fed wholly with sprats was hardly eatable, it tasted so strongly of fish.

Although bones are in part composed of animal substance, and so far digestible, yet they require stronger powers to digest them than common meat, from the animal substance being guarded by the earth. Thus the animal part of a bone is less easily soluble in an alkali than flesh, or than even the animal part when deprived of its earth by an acid; nor will a bone, being guarded by the calcareous earth, submit to putrefaction so readily as meat; therefore animals who live upon others, and swallow them whole, as the heron, digest bone with more ease, than the crow or magpye that are not accustomed to swallow bones, but commonly pick the flesh only.

The degree of ease, or difficulty, with which substances are digested, will not only arise from a difference in solidity, but from a difference in the structure of the parts themselves: brain, liver, muscle, and tendon, being digestible in the order here put down.

There is not only a difference in the degree of facility with which the various kinds of natural food are digested; but these can also be made to undergo changes by art, which render them still more easy of digestion: for it appears from my experiments, that boiled and roasted, and even putrid, meat is easier of digestion than raw, which in the two first, may be supposed to arise from their juices being coagulated; but the same reason will not hold good with regard to the putrid. A raw egg is thought more easy of digestion than an egg hard boiled, although the raw one must be coagulated in the stomach before it can be digested: and it has like-

wife been observed, that what is easy of digestion in one stomach will not be so in another ; but such cases may probably arise from the stomach not being in a healthy state.

In many animals the whole of the food does not appear to be digested, the substance in part being found in the fæces ; for if a dog is fed with tallow, his excrements will consist of a somewhat firm unctuous substance ; so that the oil is only digested in part. The circumstance of some part of the food, though digestible, not being acted upon by the gastric juice, may arise from two causes ; first, from many parts of vegetables being too firm in texture to be digested in the same time with the other food, and being therefore carried along in a crude state, together with the chyle, into the duodenum ; and secondly, from the stomach at the time being so much disordered as to digest imperfectly. We know that food may lie a considerable time in the stomach, when it is diseased, without being digested. Food has been retained in the stomach twenty-four hours, and thrown up without being in the least altered ; the animal at the time not requiring nourishment : this often arises from disease, and is also the case with those who go to rest in the winter.

The powers of digestion may, in some instances, be estimated by the appearance of the excrement, in which if the food appears not to be much altered, we may conclude, that digestion has had little or no influence on it. Thus the excrement of a flea, that has lived on blood, is nearly to appearance pure blood, not having even lost its colour.

Animals take food in proportion to the quantity of nourishment contained in it, of which the stomach appears from instinct, to be capable of judging ; and also in proportion to the powers they possess of converting what they eat into chyle. A caterpillar, perhaps, eats more in proportion to its size than any other animal that lives on the same kind of food ; for not having the power of dissolving the vegetable, but only of extracting a juice or infusion from it ; the bit of leaf comes away entire, coiled up and hardened ; but by being put into water unfolds like tea.

There are few animals that do not eat flesh in some form or other, while there are many who do not eat vegetables at all ; and therefore the

difficulty to make the herbivorous eat meat, is not so great, as to make the carnivorous eat vegetables. Where there is an instinctive principle in an animal, directing it either to the one species of food or the other, the animal will certainly die, rather than break through, of its own accord, that natural law; but it may be made to violate every natural principle by artificial means. That the hawk tribe can be made to feed upon bread, I have known these thirty years; for to a tame kite I first gave fat, which it eat very readily; then tallow and butter; and afterwards small balls of bread rolled in fat or butter; and by decreasing the fat gradually, it at last eat bread alone, and seemed to thrive as well as when fed with meat. This, however, produced a difference in the consistence of the excrements; for when it eat meat they were thin, and it had the power of throwing them to some distance; but when it eat bread, they became firmer in texture, and dropped like the excrement of a common fowl. Spallanzani attempted, in vain, to make an eagle eat bread by itself; but by enclosing the bread in meat, so as to deceive the eagle, the bread was swallowed, and digested in the stomach.

The excrements of animals we may suppose to be that part of the common food which is indigestible; and as food is either animal or vegetable, and each different kind adapted to distinct classes of animals, it is natural to believe that the excrementitious part of each will be different; and where the animal feeds upon both, that the excrement will be of a mixed nature. Although this appears probable, it is only true in a certain degree; for the mode of digestion, and whether the animal has a cæcum and colon, with their peculiar form, have all an influence in the changes which the food undergoes. Vegetable food produces more excrement than animal, and this according to the kind or parts of vegetables that are eaten. The woody parts and husks, which are indigestible, produce the most; the true farinaceous part the least: why there should be any at all from the farinaceous, and animal substance, except what has eluded the action of the digestive organs, is not easily accounted for.

All fæces have a tendency to putrefaction, but least in those animals who feed on vegetables. Indeed, the excrement from vegetable food alone

could hardly ever become putrid, if it was not mixed with the mucus of the intestines; and would even then be kept sweet by the tendency which undigested vegetables have to take on the vinous and acetous fermentation. But the fæces of those which live entirely on animal food, in general very soon become putrid; and indeed often before they are voided: but such animals are either without cæcum or colon; or if not, what they have is very short; so that the excrement not being long retained, has less time to become putrid. When the fæces stagnate so as to take on either the vinous or putrefactive fermentation, air is let loose, which will be according to the nature of the fermentation; most probably, from the vegetable it will be fixed; and from the animal, inflammable air.

The fæces of the greatest number of animals are tinged by the bile, which in some gives them a yellowish green colour; in the bird they are generally green, but sometimes white, from being mixed with the urine. The fæces of the maggot appear to be loaded with bile; for besides being yellow, they are extremely bitter; which is known by eating the kernel of a nut that has a maggot in it. Some kinds of food, when not wholly digested, give a tinge to the fæces, as grass to the excrement of cows.

The animals which feed upon vegetables alone, commonly have their fæces somewhat solid; but the degree will vary according to the state of the vegetable, whether green or dried; and therefore the particular state of the fæces will depend on the nature of the indigestible part of the food, and must be different according to the digestive powers in different animals. An animal that feeds upon grass, has the fæces much softer than when fed on the same kind of grass made into hay; and therefore the fæces of the herbivorous animals are softer in the summer than the winter: but green vegetable food does not produce soft fæces in all animals; for the caterpillar, which lives upon the leaves of vegetables, has its fæces almost dry; and we find in some ruminating animals, as sheep, that the difference in the fæces, during summer and winter, is inconsiderable. The quadrupeds, and birds, that live principally upon vegetables, generally have their cæca large, and the colon long, as we see in many of the ruminating animals. Some have the colon both long and large, as the horse,

and those of the rat tribe ; which circumstance has considerable effects in allowing the fæces to become dry : in a few of the ruminating animals, and of the rat kind, they are formed into small portions.

The fæces of quadrupeds, living upon animal food, are commonly soft, and in birds are fluid ; but in such as live on both animals and vegetables, they are in consistence of a mixt nature, and will be more or less soft, according to the food. If a dog is fed entirely on animal substance, its fæces will be soft ; if wholly on vegetable, as on bread, they will become so hard as to be expelled with difficulty.

Spallanzani made some experiments, to prove that digestion is carried on after death ; but they are not so conducted as to correspond with the appearances met with in the dead body, where that process has taken place, and the coats of the stomach itself have been in part digested. An experiment, although it may be very well and accurately made, so far as the experiment goes, if a close connection is not preserved with the purpose for which it was made, the conclusions to be drawn from it cannot correspond with the intention. This is exactly the case with the experiments of Spallanzani ; which, although they prove that meat was digested in the stomach after the animal was killed, (which no one doubted) yet are not at all calculated to show that the stomach itself may be digested. In fact, the mode in which they were managed, rather tended to prevent that effect from taking place, for the gastric juice, by having substances introduced on which it could act, was less likely to affect the coats of the stomach. That the digestion was not carried on merely by the gastric juice secreted before the animal was apparently dead, is evident from his own account, some of the food which had been introduced and digested, being found in the duodenum ; a thing that could not have happened, if a cessation of the actions of life in the involuntary parts had taken place when visible life terminated. There had been an action, and most probably a secretion, in the stomach. The only experiment that can be made with any probability of a decided result is, to kill the animal while the stomach is empty, and observe what afterwards takes place. There are few stomachs that do not shew, when examined after

death, some of the inner villous coat destroyed; which may have been done by the gastric juice in the ducts of the glands which secrete it.

Dr. Stevens, in an inaugural dissertation on this subject, published at Edinburgh 1777, gives a number of experiments, some of which are well devised, to ascertain the substances that are easiest of digestion; a thing in fact more wanted, than the cause of that process: but many of his experiments, more especially those on ruminating animals, were not made with sufficient accuracy. How the chopped hay and pot-herbs came to be so much changed in the first stomach of a ruminating animal I cannot conceive, as I have reason to believe it has not the least power of digesting; and should doubt very much that hay was capable of being wholly digested in any stomach. His experiment made on substances out of the body, proves that the gastric juice is not able in all cases to prevent the vinous and acetous fermentation in vegetables; and is a circumstance which I believe often takes place in the living body, when the stomach is weak. He seems to be in some apprehension for the safety of the stomach itself, from the action of so powerful a solvent as the gastric juice: but though inclined to suppose that the living powers of the animal may guard it against such effects; yet he is still disposed to fear that, in all cases they may not be sufficient.

The living power, in the stomach, must indeed be very weak to admit of its being digested; where that was likely to happen, I imagine the secretion of the gastric juice would be too defective to allow of the stomach being acted upon.

Dr. Stevens gives two cases, with the dissections, to prove that the living stomach has not always the power to resist the action of the gastric juice: but he has not made it clear, that those very stomachs might not have been digested after death. The appearance of the edges of the hole should have been more particularly described; for if it took place before death, it is probable it was owing to ulceration, which I have sometimes seen. Men should be very accurate in ascertaining the truth of facts, before they advance them, especially when they tend either to overturn a received opinion, or to establish a new one. As to the possibility of ani-

mals swallowed alive being digested, no fresh proofs are necessary, as we eat oysters every day ; but this does not prove that they are digested while alive. In his experiments made on ruminating animals, and the dog, as the vegetables were not so readily digested as the meat, he concludes, “ It is possible every species of animal has its peculiar gastric liquor, capable of dissolving certain substances only ” ; which is certainly not true.

Mr. Senebier relates some experiments made by Mr. Goffe, upon himself ; but which hardly contain any thing, except a curious conjecture of Mr. Senebier's, “ That distention of the stomach is the cause of the secretion of the gastric liquor.” He mentions the substances, both animal and vegetable, which are not digestible ; then those difficult of digestion ; afterwards, those easily digested ; also what substances facilitate digestion, and what retard it. But if we are to judge of the truth of these facts from a detail of the experiments which he made to ascertain them, I am inclined to believe that the experiments have not been made with sufficient accuracy to be depended upon.

ON THE DIGESTION OF THE STOMACH AFTER DEATH.

THE following account, of the Stomach being digested after Death, was drawn up at the desire of the late Sir John Pringle, when he was president of the Royal Society ; and the circumstance which led to it was as follows : I had opened, in his presence, the body of a patient who had been under his care, in which the stomach was found to be in part dissolved ; a thing that appeared to him very unaccountable, there having been no previous symptom which could have led him to suspect any disease in the stomach. I took that opportunity of explaining to him my ideas respecting it ; and that having long been employed in making experiments on digestion, I had been induced to consider this as one of the facts which proved a converting power in the gastric juice. I mentioned my intention of publishing the whole of my observations on digestion at some future period ; but he desired me, in the mean time, to give this

fact by itself, with my remarks ; as it would prove that there is a solvent power existing in the stomach, and would be of use in the examination of dead bodies.

An accurate knowledge of the appearances in animal bodies, where death has been the consequence of some violence while they were otherwise in health, ought certainly to be considered as necessary to qualify us to judge truly of the state of the body in those that die of diseases. An animal body undergoes changes after death ; but it has never been sufficiently considered what those changes are, or how soon they may take place ; yet till this be done, it is impossible we can form an accurate judgment of the appearances which present themselves at the time of inspection. The diseases of an animal body (mortification excepted) are always connected with the living principle, and are not in the least similar to the changes which take place in the dead body : without a knowledge of this, an opinion drawn from dissections must always be very imperfect, or very erroneous. Appearances which are in themselves natural, may be mistaken for those of disease : we may see diseased parts, and suppose them in a natural state ; we may consider a circumstance to have existed before death, which was really a consequence of it ; or we may imagine it to be a natural change after death, when it was in fact a disease of the living body. It is easy to see, therefore, how a man in this state of ignorance must blunder, when he comes to connect the appearances in a dead body with the symptoms that were observed in life ; and indeed, all the advantage to be derived from opening dead bodies, depends upon the judgment and sagacity with which this sort of comparison is made.

There is a case of a mixed nature, which can neither be reckoned a process of the living body, nor of the dead ; it participates of both, inasmuch as its cause arises from life, and the effect cannot take place till after death. To render this more intelligible, it will be necessary to state some general ideas concerning this cause and effect.

An animal substance, when joined with the living principle, cannot undergo any change in its properties but as an animal ; this principle al-

ways acting and preserving the substance possessed of it from dissolution, and from being changed, according to the natural changes which other substances undergo.

There are a great many powers in nature, which the living principle does not enable the animal matter, with which it is combined, to resist, viz. the mechanical and most of the strongest chymical solvents. It renders it, however, capable of resisting the powers of fermentation, digestion, (and perhaps several others) which are well known to act on this same matter, and entirely to decompose it, when deprived of the living principle. The number of powers which thus act differently on the living and dead animal substance not being ascertained, we shall only take notice of two, putrefaction and digestion, which do not affect this substance, unless when it is deprived of the living principle. Putrefaction is an effect which arises spontaneously; digestion is an effect of another principle, and shall here be considered a little more particularly.

Animals, or parts of animals, possessed of the living principle, when taken into the stomach, are not in the least affected by the powers of that viscus, so long as the animal principle remains; hence it is that we find animals of various kinds not only can live in the stomach, but are even hatched and bred there: yet the moment that any of these lose the living principle, they become subject to the digestive powers of the stomach. If it were possible for a man's hand, for example, to be introduced into the stomach of a living animal, and kept there for some considerable time, it would be found, that the dissolvent powers of the stomach could have no effect upon it; but if the same hand were separated from the body, and introduced into the same stomach, we should then find that the stomach could immediately act upon it. Indeed, if the first were not the case, the stomach itself ought to have been made of indigestible materials; for were not the living principle capable of preserving animal substances from being acted upon by the process of digestion, the stomach itself would be digested: and accordingly we find that the stomach, which at one instant, that is, while possessed of the living principle, was capable of resisting the digestive powers which it contained, the next moment, viz.

when deprived of the living principle, is itself capable of being digested, not only by the digestive powers of other stomachs, but even by the remains of that power which itself had of digesting other things.

These observations lead us to account for an appearance which we often find in the stomachs of dead bodies; and they at the same time throw considerable light upon the nature of digestion. The appearance we allude to, is a dissolution of the stomach at its great extremity; in consequence of which, there is frequently a considerable aperture made in that viscus. The edges of this opening appear to be half dissolved, very much like that kind of solution which fleshy parts undergo when half digested in a living stomach, or when acted upon by a caustic alkali, viz. pulpy, tender, and ragged.

In these cases the contents of the stomach are generally found loose in the cavity of the abdomen, about the spleen and diaphragm; and in many subjects the influence of this digestive power extends much further than through the stomach. I have often found, that after the stomach had been dissolved at the usual place, its contents let loose had come into contact with the spleen and diaphragm, had dissolved the diaphragm quite through, and had partly affected the adjacent side of the spleen, so that what had been contained in the stomach, was found in the cavity of the thorax, and had even affected the lungs to a small degree.

There are very few dead bodies in which the stomach at its great end is not in some degree digested; and one who is acquainted with dissections can easily trace these gradations. To be sensible of this effect, nothing more is necessary than to compare the inner surface of the great end of the stomach with any other part of its inner surface; the sound portions will appear soft, spongy, and granulated, and without distinct blood-vessels, opaque and thick; while the others will appear smooth, thin, and more transparent; and the vessels will be seen ramifying in its substance, and upon squeezing the blood which they contain from the larger branches to the smaller, it will be found to pass out at the digested ends of the vessels, and to appear like drops on the inner surface.

Though I have often seen such appearances, and supposed that they

must have been seen by others, yet I was quite at a loss to account for them. At first, I supposed them to have been produced during life, and was therefore inclined to look upon them as the cause of death; only that I never found they had any connection with the patient's symptoms: but I was still more at a loss to account for them, when I discovered they were most frequent in those who died by sudden violence; a circumstance which made me suspect that the true cause was not guessed at*.

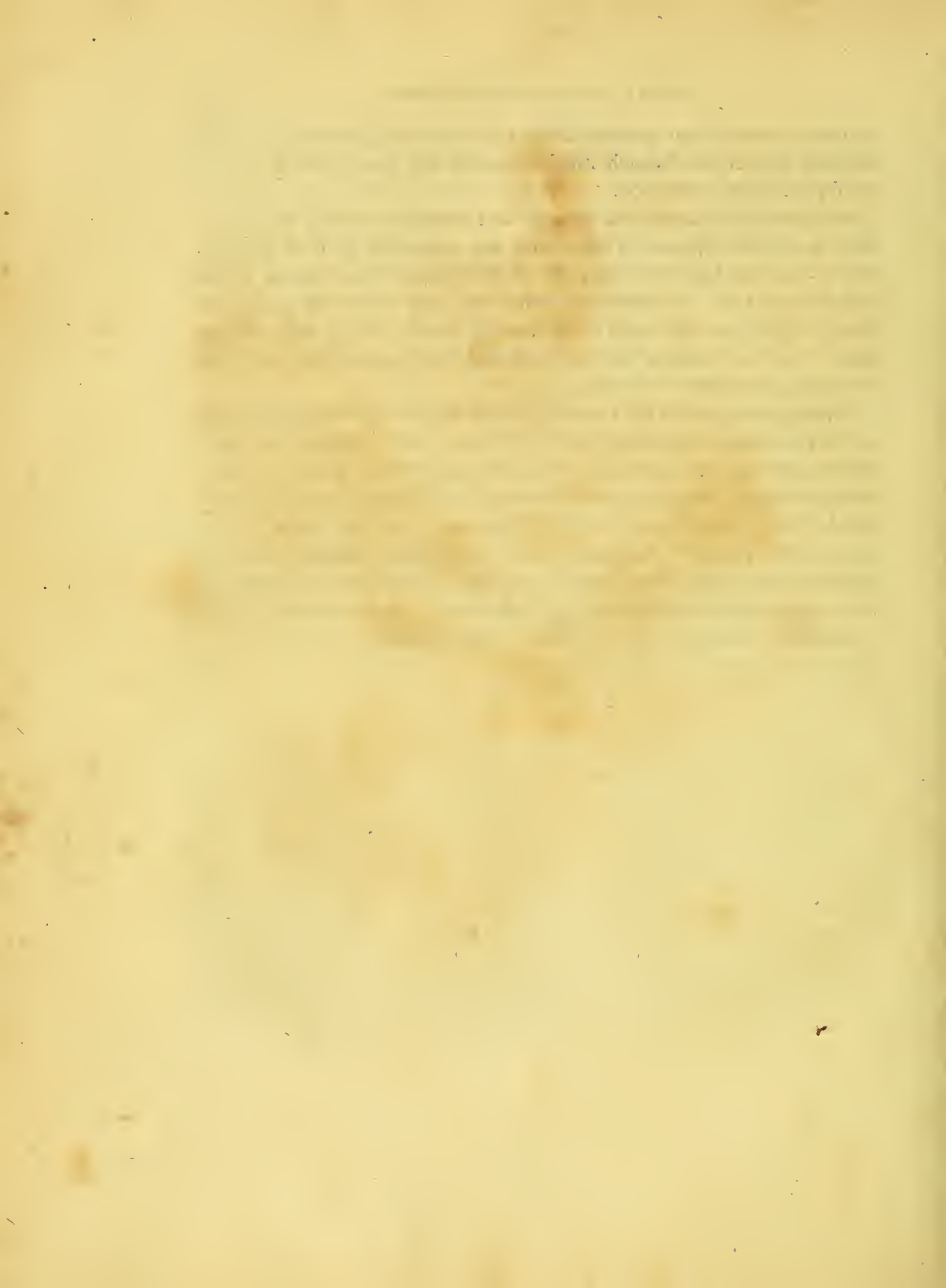
At this time I was employed in making experiments upon digestion in different animals; all of which were killed at different times, after having been fed with various kinds of food; many of these were not opened immediately after death, and in some of them I found the above described appearances in the stomach. The better to pursue my inquiry on the subject of digestion, I procured the stomachs of a vast variety of fishes, whose deaths are always violent; and who may be said to die in perfect health, with their stomachs usually full. In them we can observe the progress of digestion most distinctly; the shape of their stomachs being very favourable for that purpose. They likewise swallow their food whole, that is, without mastication; and swallow fish that are much larger than the digesting part of the stomach can contain; therefore in many instances the part swallowed, which was lodged in the digesting part of the stomach, was found more or less dissolved, while that which remained

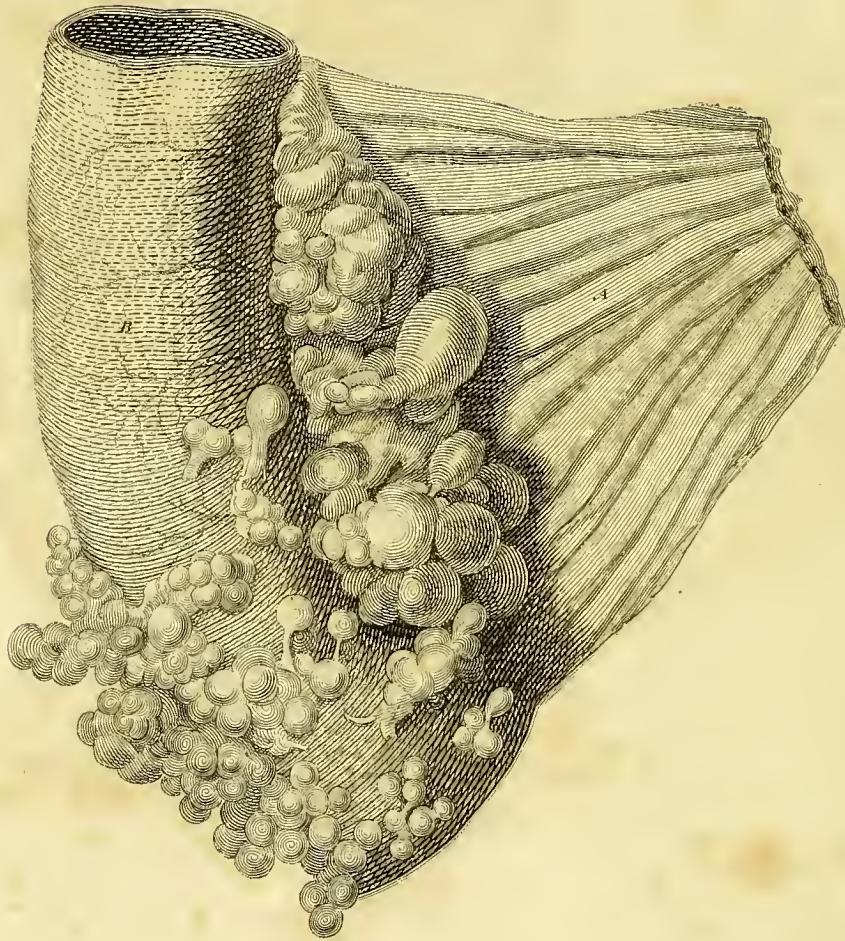
* The first time that I had occasion to observe this appearance, where death had been produced by violence, and where it could not therefore easily be supposed to be the effect of disease, was in a man who had his skull fractured by one blow of a poker. Just before this accident he had been in perfect health, and had taken a hearty supper of cold meat, cheese, bread, and ale. Upon opening the abdomen, I found that the stomach, though it still contained a good deal, was dissolved at its great end, and a considerable part of its contents lay loose in the general cavity of the belly; a circumstance which puzzled me very much. The second instance was in a man who died at St. George's Hospital, a few hours after receiving a blow on his head, which fractured his skull. From these two cases, among various conjectures about so strange an appearance, I began to suspect it might be peculiar to cases of fractured skull; and therefore, whenever I had an opportunity, I examined the stomach of every person who died from that accident; but I found many of them which had not this appearance. I afterwards met with the same appearance in a man who had been hanged.

in the œsophagus was perfectly found: and in many of these I saw the digesting part of the stomach itself reduced to the same dissolved state as the digested part of the food.

Being employed upon this subject, and therefore enabled to account more readily for appearances which had any connection with it, and observing that the half-dissolved parts of the stomach were similar to the half-digested food, it immediately struck me, that it was the process of digestion going on after death; and that the stomach, being dead, was no longer capable of resisting the powers of that menstruum, which itself had formed for the digestion of food.

These appearances of the stomach after death, throw considerable light on the principles of digestion, and show, that it neither depends on a mechanical power, nor contractions of the stomach, nor on heat, but something secreted in the coats of the stomach, and thrown into its cavity, which there animalises the food, or assimilates it to the nature of the blood. The power of the gastric juice is confined or limited to certain substances, generally of the vegetable and animal kingdoms; and although this menstruum is capable of acting independently of the stomach, yet it is indebted to that viscus for its existence and continuance.





P L A T E XIV.

A portion of intestine of a hog, the peritoneal coat of which is covered in several places with small pellucid cysts, containing air.

It was sent to me by my friend Mr. Jenner, surgeon at Berkley, who informed me, that this appearance is found very frequently upon the intestines of hogs that are killed in the summer months.

A The portion of the mesentery.

B The portion of intestine on which the air-cells are situated.

H h

ON A SECRETION IN THE CROP OF BREEDING PIGEONS, FOR THE NOURISHMENT OF THEIR YOUNG.

THE nourishment of animals admits, perhaps, of as much variety in the mode by which it is to be performed, as any circumstance connected with their œconomy; whether we consider their numerous tribes, the different stages through which every animal passes, or the food adapted to the support of each, in their distinct conditions and situations. We are likewise to include in this view, that endless variety, in the means by which this food is procured, according to the class of the animal and the particular stage of its existence. If the food was the same through every period of the life of an animal; if every individual of a tribe lived on the same kind, and procured it by the same mode, our speculations would then admit of a regular arrangement. But when we see that the food adapted to one stage of an animal's life is rejected at another; and that animals of one class in some respects resemble those of another, by hardly having any food peculiar to themselves, the subject becomes so complicated, that it is not surprising if we are at a loss to arrange the various modes by which animals are nourished.

Animal life may not improperly be divided into three states, or stages. The first comprehends the production of the animal and its growth in the fœtal state: the second commences when it emerges from that state by what is called the birth; yet for a certain time must, either mediately or immediately, depend on the parent for support; the third may be said to take place when the animal is fit, and at liberty, to act for itself. The first and third stages are perhaps common to all animals; but there are some classes, as fishes, spiders, &c. which seem to have no second stage, but pass directly from the first to what is the third in other animals. Of

those requiring a second stage, the polypus and the viviparous animals continue to derive their nourishment immediately from the parent; while the oviparous are for some time supported by a substance originally formed with them, and reserved for that purpose.

There is infinite variety in the means by which Nature provides for the support of the young in the second stage of animal life. In many insects it is effected by the female instinctively depositing the egg, or whatever contains the rudiments of the animal, in such a situation that, when hatched, it may be within reach of proper food: others, as the humble-bee and blackbeetle, collect a quantity of peculiar substance, which both serves as a nidus for the egg, and nourishment for the maggot, when the embryo arrives at that state. Most birds, and many of the bee tribe, collect food for their young; when at a more advanced period, the task of feeding them is performed by both male and female, with an exception in the common bee, the young ones of which are not fed by either parent, but by the working bees, who act the part of the nurse. There is likewise a number of animals capable of supplying immediately from their own bodies, the nourishment proper for their offspring, during this second stage; a mode of nourishment which has hitherto been supposed to be peculiar to that class of animals which Linnæus calls Mammalia; nor has it, I imagine, been ever suspected to belong to any other.

I have, however, in my inquiries concerning the various modes in which young animals are nourished, discovered that all of the dove kind are endowed with a similar power. The young pigeon, like the young quadruped, till it is capable of digesting the common food of its kind, is fed with a substance secreted for that purpose by the parent animal: not as in the Mammalia by the female alone, but also by the male; which, perhaps, furnishes this nutriment in a degree still more abundant. It is a common property of birds, that both male and female are equally employed in hatching, and in feeding their young in the second stage; but this particular mode of nourishment, by means of a substance secreted in their own bodies, is peculiar to certain kinds, and is carried on in the crop.

Besides the dove kind, I have some reason to suppose parrots to be

endowed with the same faculty, as they have the power of throwing up the contents of the crop, and feeding one another. I have seen the cock parouquet regularly feed the hen, by first filling his own crop, and then supplying her from his beak. Parrots, macaws, cockatoos, &c. when they are very fond of the person who feeds them, may likewise be observed to have the action of throwing up the food, and often do it. The cock pigeon, when he caresses the hen, performs the same kind of action as when he feeds his young; but I do not know, if at this time, he throws up any thing from the crop.

During incubation, the coats of the crop, in the pigeon, are gradually enlarged and thickened, like what happens to the udder of females of the class Mammalia, in the term of uterine-gestation. On comparing the state of the crop when the bird is not sitting, with its appearance during incubation, the difference is very remarkable. In the first case it is thin and membranous; but by the time the young are about to be hatched, the whole, except what lies on the trachea, becomes thicker and takes on a glandular appearance, having its internal surface very irregular*. It is likewise evidently more vascular than in its former state; that it may convey a quantity of blood, sufficient for the secretion of the substance, which is to nourish the young brood for some days after they are hatched.

Whatever may be the consistence of this substance, when just secreted, it most probably very soon coagulates into a granulated white curd; for in such a form I have always found it in the crop: and if an old pigeon is killed just as the young ones are hatching, the crop will be found as above described, and in its cavity pieces of white curd mixed with some of the common food of the pigeon, such as barley, beans, &c. If we allow either of the parents to feed the brood, the crop of the young pigeons, when examined, will be discovered to contain the same kind of curdled substance as that of the old ones; which passes from thence into the stomach, where it is to be digested.

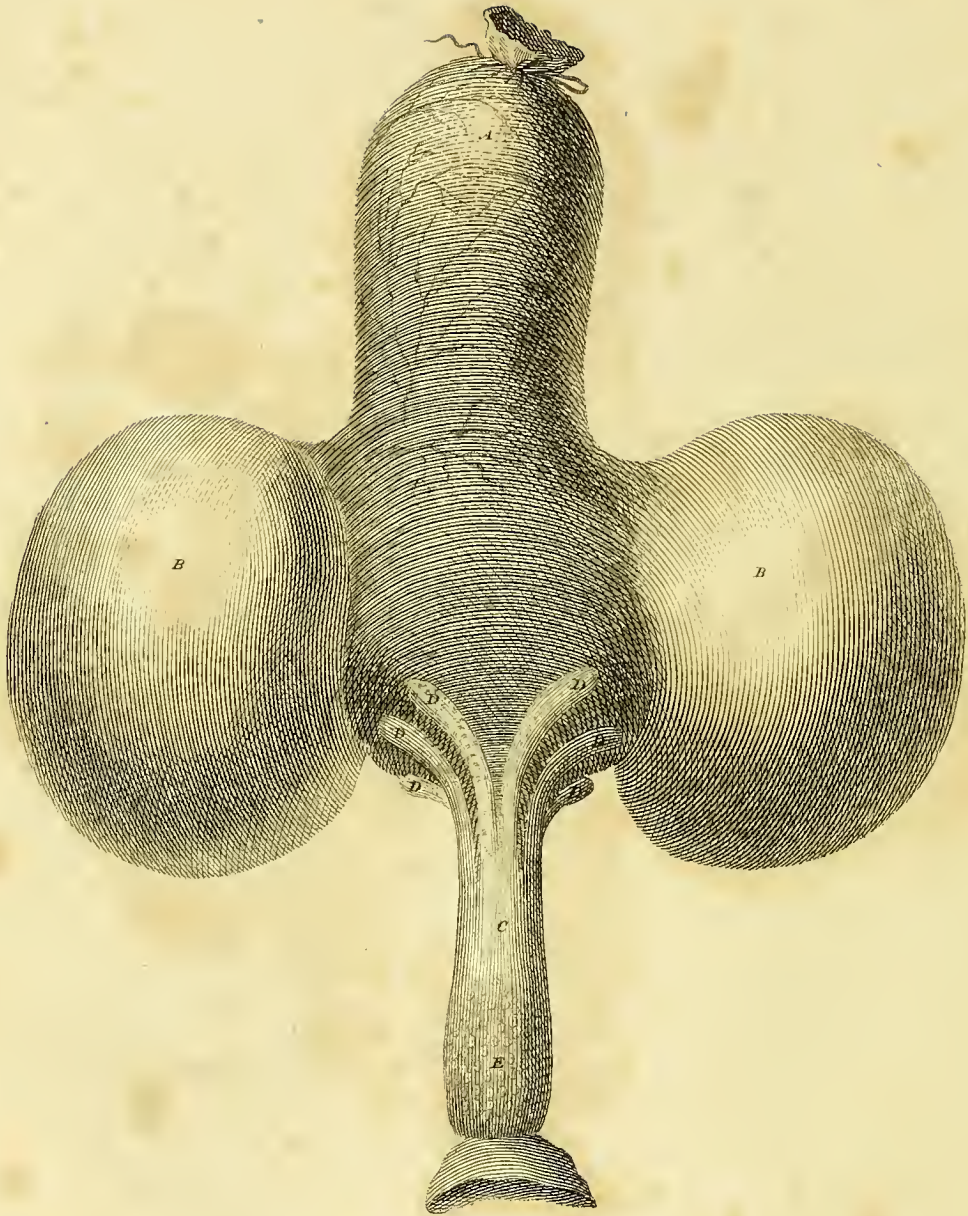
The young pigeon is fed for a little time with this substance only; as

* Vide Plate XVI.

about the third day, some of the common food is found mingled with it; as the pigeon grows older, the proportion of common food is increased; so that by the time it is seven, eight, or nine days old, the secretion of the curd ceases in the old ones, and of course no more will be found in the crop of the young. It is a curious fact, that the parent pigeon has at first a power to throw up this curd without any mixture of common food, although afterwards both are thrown up according to the proportion required for the young ones.

I have called this substance curd, not as being literally so, but as resembling that more than any thing I know; it may, however, have a greater resemblance to curd, than we are perhaps aware of; for neither this secretion, nor curd, from which the whey has been pressed, seem to contain any sugar, and do not run into the acetous fermentation. The property of coagulating is confined to the substance itself, as it produces no such effect when mixed with milk.

This secretion in the pigeon, like all other animal substances, becomes putrid by standing; though not so readily as either blood or meat, it resisting putrefaction for a considerable time; neither will curd, much pressed, become putrid so soon as either blood or meat.



P L A T E XV.

THE crop, taken from a pigeon when it had no young ones.

The crop of the pigeon appears to be placed more in the middle of the neck than in any other bird, being two equal bags, annexed, laterally, to the œsophagus; whereas in most other birds, it is single, and a little on one side. The œsophagus of those birds which have crops, may be divided into two portions, a superior and inferior. The superior is that which leads from the mouth to the crop; the inferior, from the crop to the gizzard.

The crop, represented in the plate, had been turned inside out, and distended with spirits. It shows the appearance of the internal surface.

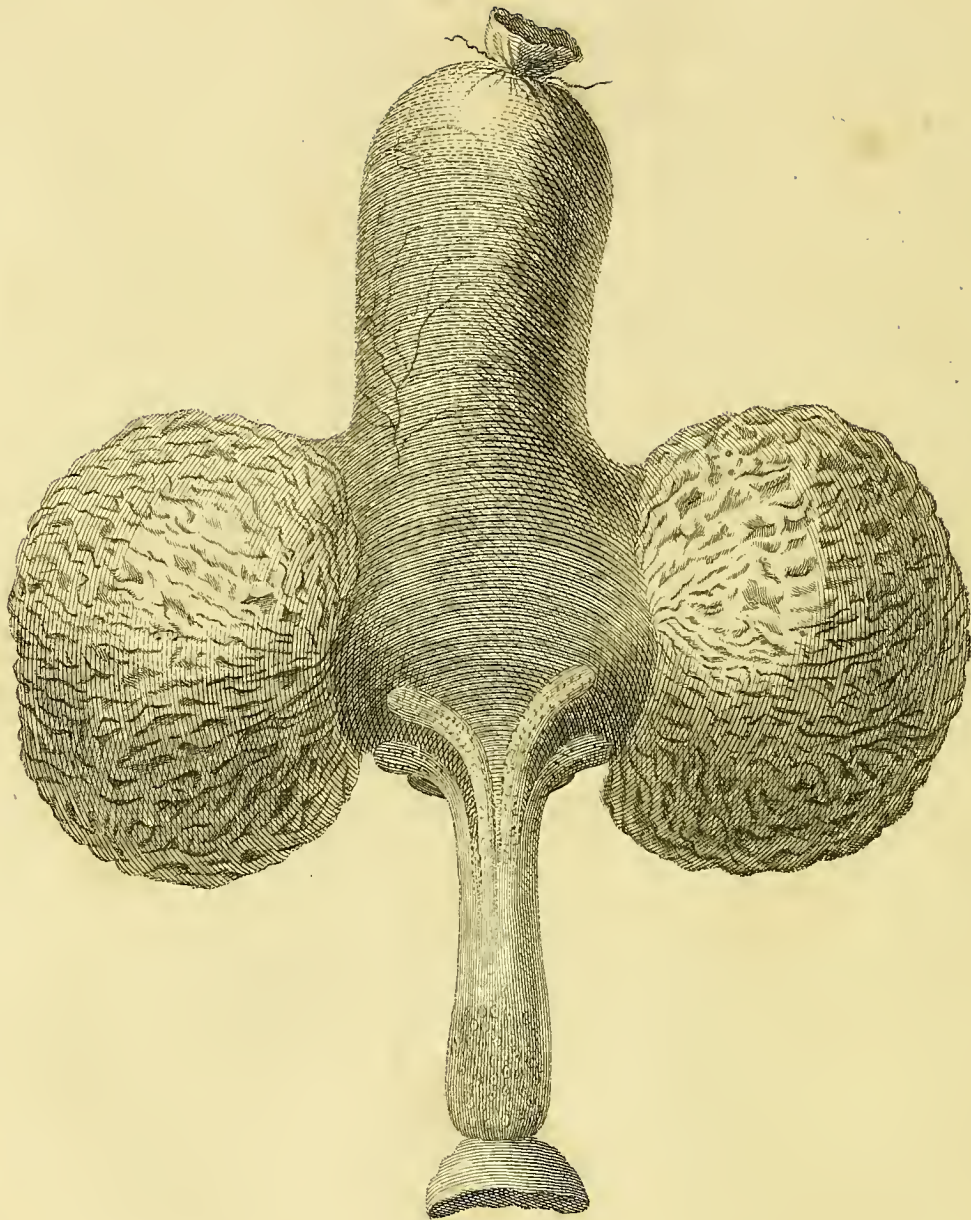
A The inner surface of the superior œsophagus.

BB The inside of the two projecting bags of the crop.

E The inferior œsophagus, leading from the crop to the gizzard.

DDDD Glands situated on the lower part of the crop, and continued into the inferior œsophagus.

E A glandular structure upon the inner surface of this œsophagus, just before it terminates in the gizzard, for the purpose of secreting a substance analogous to the gastric liquor.



P L A T E X V I.

THE crop, from a male pigeon, while the female was breeding, to shew the change which takes place at that time, on its internal surface, for the purpose of secreting a substance which is to nourish the young.

The crop is prepared in the same way as in Plate XV; and the only difference in the appearance is the glandular structure on the inner surface of the two lateral projecting bags, which is not seen at any other time.

ON THE COLOUR OF THE PIGMENTUM OF THE EYE IN DIFFERENT ANIMALS.

IN the eyes of all animals which I have examined, there is a substance approaching to the nature and appearance of a membrane, called the pigmentum, which lines the choroid coat, and is somewhat similar to the rete mucosum which lies under the cuticle of the human body; and there is also some of the same kind of substance diffused through the cellular-membrane, which unites the choroid with the sclerotic coat. My intention, at present, is only to communicate the observations I have made on this subject, and its use, confining myself to the consideration of that kind of it which lines the tunica choroides of the class Mammalia, and of birds: in doing which I shall also take occasion to speak of the difference of colour occurring in animals of the same species. Although an accurate examination of the appearances of a similar substance in the eyes of some fishes might illustrate the subject, we cannot avail ourselves of that, as from not being sufficiently acquainted with the effects of light on the eyes of that class of animals.

The propagation, or continuance of animals in their distinct classes, is an established law of Nature; and, in a general way, is preserved with a tolerable degree of uniformity: but in the individuals of each species, varieties are every day produced in colour, shape, size, and disposition. Some of these changes are permanent with respect to the propagation of the animal; becoming so far a part of its nature as to be continued in the offspring.

Animals living in a free and natural state are subject to few deviations from their specific character; but Nature is less uniform in its operations

when influenced by culture*. Considerable varieties are produced under such circumstances; of which the most frequent are changes in the colour. These changes are always, I believe, from the dark to the lighter tints; and the alteration very gradual in certain species, requiring in the Canary-bird several generations; while in the crow, mouse, &c. it is completed in one. But this change is not always to white, though still approaching nearer to it in the young, than in the parent; being sometimes to dun, at others to spotted, of all the various shades between the two extremes. This alteration in colour being constantly from dark to lighter, may we not reasonably infer, that in all animals subject to such variation, the darkest of the species should be reckoned nearest to the original; and that where there are specimens of a particular kind, entirely black, the whole have been originally black? Without this supposition, it will be impossible, on the principle I have stated, to account for individuals of any class being black. Every such variety may be considered as arising in the cultivated state of animals: but whether, if left to themselves, they would in time resume their original appearance, I do not know†.

The colour of the pigmentum of the eye always corresponds, I believe, with that of the hair and skin, especially if the animal be only of one colour, but is principally determined by the hair; and the most general colour is a very dark brown, approaching to black, from whence it had the name, *nigrum pigmentum*‡. The colour differs in different classes of animals, often in the same class, and even in the same species. In the human, it is most commonly dark; in the ferret kind, always light: and

* From the variations produced by culture, it would appear, that the animal is so susceptible of impression, as to vary Nature's actions; and this is even carried into propagation. Whether this takes place at the very first union of the principles of the two parents, so as to derive its existence from both; or, whether it takes its formation from the mother, after the first formation of the embryo, are, perhaps, not easily determined.

† In vegetables, I believe, it invariably holds good, that however improved by culture, if neglected, they soon degenerate into their first state.

‡ As the colour of this membrane corresponds with the colour of the skin and hair of the person, it is probable that the people, among whom it first got the name, were dark.

its difference of colour in the same species is evident from the variety observable in the eyes of different people. There is even a difference of colour in the same eye in many classes of animals: in all of the cat and dog kind; and perhaps in most part of the granivorous. In some it is partly black, and partly of the appearance of polished silver: and in many classes, the variation from dark is of two colours; for in the cow, in sheep, deer, horses, and I believe in all animals feeding on grass, there are, in the same eye, certain portions of it white, and others of a fine green colour. The difference in colour of this pigmentum, in the eyes of different animals of the same species, is very remarkable; in the human species it is of all the different shades between black and almost white; and the same variety is seen in rabbits, mice, crows, blackbirds, &c. but in these it is of one colour only in the same eye. Every species is, perhaps, subject to such variations; and some of these are so extraordinary, as with propriety to be denominated monstrous*.

The variation in the colour of the pigmentum in different species of animals, seems to depend on a fixed law of Nature; but the varieties which are met with in the same species are much less constant, being merely different shades, approaching to black or white. But the extraordinary circumstance is, its being sometimes unusually lighter or darker in individuals of the same species; and this difference not seldom starting up in the young without any hereditary principle to account for it.

The human species is a striking example of the colour of the pigmentum corresponding with that of the skin and hair; and though the skin and hair of one person differs very considerably from the skin and hair of another, yet it is not in so great a degree as in many animals. There are cattle perfectly white, white sheep, white dogs, white cats and rabbits;

* Perhaps the word, monstrous, is too strong, or not exactly just. It certainly may be laid down as one of the principles or laws of Nature, to deviate under certain circumstances. It may also be observed, that it is neither necessary, nor does it follow, that all deviations from the original must be a falling off; it appears just the contrary, therefore we may suppose that Nature is improving its works; or, at least, has established the principle of improvement in the body as well as in the mind.

but there are few of the human species that we can say are perfectly white. They rather pass from the black into the brown, red, and even light yellow; and we find this pigmentum, although only of one colour, varying through all the different corresponding shades. In the African negro, the blackness of whose hair and skin are great distinguishing characteristics, this pigmentum is also very black. In the mulatto, who has not the skin so dark as the African, but the hair nearly as black, this pigmentum is of a shade not quite so deep, yet still it does not approach so near to the middle tint as the skin, rather following the colour of the hair. In people of a swarthy complexion, as Indians, Turks, Tartars, Moors, &c. we find the hair always of a jet black; and this substance of a much darker brown than in those that are fair. In those of very dark complexions, and having very black hair, although descended from fair parents, the same thing holds good. There are few species of animals, or even individuals of a specie, whose bodies are only of one colour. Crows, and some others, are exceptions; but the greatest number are of two or more, being variously spotted or streaked, either with different colours, or with shades of the same. Many species are constantly lighter in some parts of the body than in others; and, with a few exceptions, animals are generally lighter, as to colour, on the lower, or what may be called the foreparts, than on the upper or backparts. The fair man or woman may strictly be considered as a spotted or variegated animal. In many persons, the hair of the head, eyebrows, eyelashes, beard, and hair on the pubes, all vary in colour. The hair of the three first may be called foetal, and are oftener all of the same, than of a different colour; the two last are to be considered as adult hair, and are commonly alike in colour, which yet frequently varies from that of the foetus; which last is more liable to change its colour than the other; and the change is generally that of growing darker, especially on the head and the eyelashes*. This difference in the colour of the hair, on different parts of the body, is not so observa-

* The hair growing grey, is not in the least to the present purpose.

ble in those nations who are dark or swarthy, as in people inhabiting many of the northern climates.

In animals which are variegated, let us observe the colour of this pigmentum, and we shall find it regulated by some general principle, and corresponding with the colour of the eyelashes. The magpie, for instance, is nearly one-third, or fourth part, white; and the two colours, if blended, would make the compound grey; but the eyelashes being black, the pigmentum is black also. We sometimes meet with people whose skin and hair are very white, and yet the iris is dark, which is a sign of a dark pigmentum; but if we examine more carefully, we shall also find that the eyelashes are dark, although the eyebrows may be the colour of the common hair.

As the colour of the iris in the human species is probably a presumptive, though not a certain sign of the colour of this pigmentum, we may be led to suppose, that in those who have the iris in one eye different from that of the other, this substance will likewise differ: but this I cannot determine, never having examined the eyes of any person with such a peculiarity. It is not an uncommon circumstance in some species of animals; the Angola cat seldom having the colour of the iris the same in both eyes.

In people remarkably fair, whether they are of a race that is naturally so, or what may be called monstrous in respect to colour, as white Æthiopians, still we find this pigmentum following the colour of the skin and hair; being in some of a light brown, and in others almost white, according to the colour of the hair in such people.

All foals are of the same colour; and whatever that may be, as they grow older it generally becomes lighter; therefore the pigmentum in them is almost always of the same colour, and does not seem to change with the hair. This change, however, is only in the hair, and not in the skin; the skin of a white or grey horse being as dark as the skin of a black one: yet there is a cream-coloured breed which has the skin of the same colour, whose foals are also of a cream-colour; and by inspecting the parts not covered with hair, such as the mouth, anus, sheath, &c. these, and the

pigmentum of the eyes of such horses, are found of a cream-colour likewise.

In the pigmentum of the rabbit kind, there are all the degrees of dark and light, corresponding with the colour of the hair; yet there seem to be exceptions to this rule in some white rabbits with black eyes, and therefore with black pigmentum; but in all such there is either a circle of black hair surrounding the eye, or the eyelashes, and the skin forming the edge of the lid, is also black. In many white cattle, this is also observable; and in that breed of dogs, called Danes, some have the hair surrounding one eye black, while the hair surrounding the other is white; and the iris of the one is often lighter than that of the other. This circumstance, of the iris of one eye being lighter in colour than that of the other, is a common thing in the human species; and sometimes only one-half of the iris is light, without any difference in the colour of the eyelash, or eyebrow. Whether this difference in the colour of the iris of the two eyes, in the same animal, is owing to the pigmentum being different in colour, I do not know; although I rather suspect it is something similar to the white iris in horses, which makes them what is called wall-eyed.

The variation of colour appears most remarkable when a white starts up, either where the whole species is black, as in the crow or blackbird; or where only a certain part of the species is black, (but permanently so) as a white child born of black parents; and a perfectly white child whose hair is white, and who has the pigmentum also white, though born of parents who are fair, should as much be considered as a play of Nature as the others. All these *lufus naturæ*; such as the white negro, the pure white child of fair parents, the white crow, the white blackbird, white mice, &c. have likewise a white pigmentum corresponding with the colour of the hair, feathers, and skin.

Besides the circumstance of animals of the same species differing from one another in colour, there are some distinct species which are, as far as we know, always of a light colour, and in them too this pigmentum is white: the animal I allude too is the ferret.

When the pigmentum is of more than one colour in the same eye, the lighter portion is always placed at the bottom of the eye, in the shape

of a halfmoon with the circular arch upwards; the straight line or diameter passing almost horizontally across the lower edge of the optic nerve, so that the end of the nerve is within this lighter coloured part, which makes a kind of semicircular sweep above it. This shape is peculiar to the cat, lion, dog, and most of the carnivorous tribe; in the herbivorous, the upper edge being irregular; in the seal, however, the light part of this pigmentum is equally disposed all round the optic nerve, and is, on the whole, broader than it is commonly found in quadrupeds. How far this increase of surface is an approach towards the fish kind, in which it is wholly of this metallic white, I will not pretend to say; but it is probable, as the animal is to see in the water as well as in the air, that it may be formed circular, the better to correspond with the form of the eyelids, which open equally all round; which seems to accord with what is observable in fishes, they being without eyelids.

The colour of the pigmentum, whether white, or green, or both, has always a bright surface, appearing like polished metal; which appearance animal substance is very capable of taking on, as we see in hair, feathers, silk, &c.

After having taken notice of the various colours of this pigmentum in different animals, both where permanent, and where it appears to be a play of Nature, let us next examine what effect it has upon vision in both cases; whether these effects are similar, or if one case illustrates the other.

It may be asserted as an undoubted fact, that the light which falls on the retina, covering a white pigmentum, has more effect than when it falls on the retina which covers a dark one: which is known by comparing the vision of those of the same species who have the pigmentum wholly dark, with those who have it perfectly white; and something may be learned, by a similar comparison of animals who have it only in different species; it being reasonable, from analogy, to suppose that some such effect is produced in the eye which is possessed of both.

I shall first consider the effect produced when the white or light colour makes only part of the pigmentum. This will lead me to observe, that

all animals having the pigmentum diversified, though they are capable of bearing as much light as others, and can see as perfectly when light is in an equal degree; can likewise see very distinctly when the light is much less than will serve the purposes of animals having it wholly dark. May we not, therefore, ascribe this advantage to the pigmentum being partly white? One might be almost tempted to suppose, that such animals have a power of presenting the different parts of the eye to the light, according to the quantity of light required; or of moving the chrystalline humour higher or lower: but we are at present unacquainted with any power in the eye by which these actions can be performed.

We may observe that when a cat or dog looks at us in the twilight, the whole pupil is enlarged and illuminated; but in a full light there is no such appearance. It is plain there must be a reflexion of light from the bottom of the eye, to produce the above effect; especially as the light reflected is always of the colour of the pigmentum in such animals; which in the cow is of a light green.

I shall secondly, consider those animals which have the whole pigmentum of a white colour, whether it is accidental or natural; and that see much better in the dark, or with less light than those in which it is of a dark colour: of the first of these I shall take my instance from the human species; of the second, the ferret will serve as an example.

Those of the human species, who have the pigmentum of a light colour, see much better with a less degree of light than those who have it dark; and this in proportion to their fairness: for when the hair is quite white, they cannot see at all in open day, without knitting their eyebrows, and keeping the eyelids almost shut. In many of these instances there is an universal glare of light from the pupil, tinged with a shade of red; which colour, most probably, arises from the blood in the vessels of the choroid coat. I have likewise observed that the pigmentum is thinnest when it is light; so that some of the light, which is reflected from the point of vision, would seem to be thrown all over the inner surface of the eye; which being white, or rather a reddish white, the light appears to be

again reflected from side to side*. This seemed to be the case in a boy at Shepperton, when about three years of age, of whom I have a portrait, to show that appearance. He is now about thirteen years of age†: the common light of the day is still too much for him; the twilight is less offensive. When in a room, he turns his eyes from the window; and when made to expose his face to the light, or when out in the open air, he knits his eyebrows, half shuts his eyelids, and bends his head forwards, or a little down: yet the light appears to be less obnoxious to him now than formerly, probably from habit. Such persons appear to be nearer sighted than people in common; but I apprehend that appearance to arise from the position into which they throw the eyelids and brows, which not only in a great degree excludes the light, making the object faint in proportion to the contraction of the pupil and shade made by the eyelids and eyebrows; but at the same time fits the eye to see near objects: for if we nearly close our eyelids, and knit our eyebrows, we can see a small object much nearer than if we did not perform such actions; and it will make above a foot difference in the focal distance of the eye.

In many rabbits who have white eyelashes, and in white mice, the pigmentum is entirely white; which is likewise observable in a certain distinct species of animals, the ferret, which we have adduced as an example of the pigmentum being naturally white: for these animals being intended to see in the dark, and their mode of life not exposing them to the light, they are liable to be affected by strong light, to a greater degree, than many others.

If it is allowed as probable that, in animals having the pigmentum diversified, the object to be viewed is thrown upon the lighter coloured portion; how does it happen that such are able to bear the light better than those who have the pigmentum altogether of a light colour? Perhaps it is not the illuminated object itself that is offensive to the retina; but

* How far this is really the case, I do not absolutely say. For whatever light comes through the pupil, must be reflected from the point of vision; but I imagined I saw the light passing through the substance of the iris.

† The period here alluded to is 1786, when the first edition of this work was published.

that diffusion of light in the one kind of eye, which does not happen in the other.

Having stated the facts, and the general effect arising from the diversified pigmentum, let us next consider the manner in which it is brought about, that such animals see better with little light, than those which have the pigmentum wholly black.

Let us then suppose the retina to be the organ of sight; and that by the rays which fall upon it being properly refracted, it gives or conveys to the mind an idea of a distinct object, corresponding with the sensation of touch. This is the most common and simple manner in which vision is performed, and is that mode which takes place where the pigmentum is black, or nearly so; and where the greatest quantity of external light is required.

The retina, although somewhat opaque, is yet so transparent as to allow a considerable quantity of light to pass through it. For if this was not the case, there could not be those differences in the appearance of the eye which I have been describing. The rays which pass through, we may suppose, do or do not give sensation in their passage; and we may also suppose, that only those which strike against the retina are the cause of sensation: but this is not the present inquiry; the rays which pass through the retina, are what I am alone to consider; which falling upon the pigmentum, are there disposed of according to the reflecting powers of that substance. If the pigmentum is black, the rays will then be absorbed and entirely lost; therefore in such eyes, vision can receive no assistance from it; and consequently a considerable quantity of light is required to produce distinct vision: but in those who have some part of this pigmentum white, we find that the rays of light which pass through the retina, are reflected back again; and in this case it is not unnatural to suppose that the reflected rays, in their passage back, will strike against the retina and increase the power of vision. It is evident that a considerable portion passes forwards through the retina, which, I suspect, is partly lost on the inner surface of the lateral and forepart of the eye, where the pigmentum is black, while the remainder passing

through the pupil, is again thrown on the object looked at. The next thing to be considered is, whether the shape of the eye is such, as will throw the rays, which passed through the retina, back upon that membrane, in the same or nearly in the same place as that through which they originally came. The eye being a sphere, or approaching to that figure, makes it probable; but whether the curve is such as will reflect the rays exactly in the same direction, is not so easily determined. If the curve be a true one, then the rays that are not obstructed in their return by the retina, must pass forwards through the pupil; and being refracted in their passage through the chrystalline humour, will be sent out of the eye in the same lines in which they entered, and be thrown on the very object from whence they came; which seems to be in a great measure the case, if we may judge by the degree of illumination in the cat's eyes. If the rays, reflected from the light part of the pigmentum, should not, in their return, strike exactly on the same points in the retina, through which they first passed; yet if they are thrown nearly on the same place it will be sufficient; for we know that our sensations are not capable of conveying to the mind mathematical exactness. And the same circumstance will be a sufficient answer, should it be objected that the time lost in the passing and repassing of the rays may prevent distinct vision; for it is known, that if an illumined body is made to move quickly in a circle, it will appear to the eye a circle of fire.

THE USE OF THE OBLIQUE MUSCLES.

MUSCLES are the active parts in an animal body, producing different effects, according to the circumstances in which they are placed; and the greater number of parts requiring a variety of motions, it became necessary to have a variety of muscles suited to such motions.

The function of a muscle depends on the contraction of its fibres; and the most general effect produced by this contraction, is to move some one part of the body upon another. But we may observe, that when motion,

in a part, is performed by one set of muscles, there are other muscles employed in regulating that motion, as in most joints: and in a whole part, destined to a variety of motions, and composed of smaller parts, intended likewise to have their distinct motions, we find muscles appropriated for the purpose of keeping some of those parts fixed in a particular position, while the whole part is to be moved by other muscles, according to the nature of the action to be performed. This will, perhaps, be best illustrated by attending to what takes place in the eye, considering it as part of the head.

The eye being an organ of sense, which is to receive impressions from without, it was necessary it should be able to give its motions that kind of direction as would permit its being impressed by objects whether at rest or in motion, or moving from object to object; and it was also necessary, that there should be a power capable of keeping the eye fixed upon an object when our body or head was in motion.

For the better understanding this action of pointing the eye towards objects under the various circumstances of vision, it will be necessary to mention, that the eye is furnished with muscles, some of which in the quadruped, bird, amphibia, and fishes, are called straight, from their being placed in the direction of, or parallel to, the axis of the eye: and two, I believe, have always been named oblique. Of the straight, some animals have more than others. There are four straight muscles common to most animals; and those which have more, have the additional muscles inserted immediately in the eyeball, on its posterior surface, and surrounding the optic nerve. The four straight muscles, which are common to all quadrupeds, pass further forwards, and are rather inserted towards the anterior surface of the eye. For vision, at large, it was not only necessary that the eye should be capable of moving from object to object, or of following any object in motion, but also necessary that there should be a power to keep it fixed on any one object to which the mind might be attentive; therefore the muscles are formed so as not only to be able to move the eye from object to object, but likewise to keep its point of vision fixed upon any particular one, while the eye is moving progres-

fively with the head or body. This is the use of these muscles, when the parts from whence they arise are kept fixed respecting the objects the eye is pointed to; but it is often necessary, while the eye is fixed upon a particular object, that the eyeball and the head in which it is fixed should shift their situation respecting that object; and this would alter the direction of the eye, if the muscles had not the power of taking up an action that produces a contrary effect, that is, keeping the point of insertion of the muscles as the fixed point, by causing their fibres to contract according as the origins of the muscles vary their position respecting the object. From this mechanism we find these three modes of action produced; first, the eye moving from one fixed object to another; then the eye moving along with an object in motion; and last, the eye keeping its axis to an object, although the whole eye, and the head, of which it makes a part, are in motion. From either of these motions taking place singly, or being combined, the eye is always kept towards its object. In the two first modes of action, the origins of the muscles are fixed points respecting the object; and, in the last, the object becomes as it were the centre of motion, or fixed point, commanding the direction of the actions of the eye, as the north demands the direction of the needle, let the box in which it is placed be moved in what direction it may. These two first modes of action are performed by the straight muscles; for the head being a fixed point, they are capable of moving the eye up and down, from right to left, with all the intermediate motions, which, taken together, constitute a circular movement; or, when the eye is to become the fixed point, then the head itself performs the circular movement. Thence appears the necessity why the object, the axis of the eye, and the point of sensation, should all three be in the same straight line. But this does not take place in all movements of that whole of which the eye makes a part; for besides those which we have already taken notice of, the head is capable of a motion from shoulder to shoulder, the axis of which is through the axis of the two eyes, from the fore to the backpart. It should be here observed, that for distinct vision, the object must be fixed as respecting the pupil of the eye, and not in the least allowed to move

over its surface*. To prevent any progressive motion of the object over the retina of the eye, either from the motion of the object itself, or of the head in some of the motions of that part, the straight muscles are provided as has been explained; but the effects which would arise from some other motion of the head, as from shoulder to shoulder, cannot be corrected by the action of the straight muscles, therefore the oblique muscles are provided. Thus when we look at an object, and at the same time move our head to either shoulder, it is moving in the arch of a circle whose centre is the neck; and of course the eyes would have the same quantity of motion on this axis, if the oblique muscles did not fix them upon the object. When the head is moved towards the right-shoulder, the superior oblique muscle of the right-side acts and keeps the right-eye fixed on the object; and a similar effect is produced upon the left-eye by the action of its inferior oblique muscle: when the head moves in a contrary direction, the other oblique muscles produce the same effect. This motion of the head may, however, be to a greater extent than can be counteracted by the action of the oblique muscles. Thus, for instance, while the head is on the left-shoulder, the eyes may be fixed upon an object, and continue looking at it while the head is moved to the right-shoulder, which sweep of the head produces a greater effect upon the eyeballs than can be counteracted by the action of the oblique muscles; and in this case we find that the oblique muscles let go the eye, so that it immediately returns into its natural situation in the orbit. Whether

* Optical writers seem to have been entirely ignorant of this; for they not only suppose distinct vision compatible with the object having a motion over the different parts of the retina, but even explain the effects which would be produced by it on the mind of the observer. Keill makes the following observation:

“Since opticks teach us, that every body which is visible, has by means of the rays which proceed from that object, its image painted on the bottom of the eye, or retina; it follows, that those objects will seem to be moved, whose images are moved on the retina, that is, which pass over successively the different parts of the retina, whilst the eye is supposed to be at rest: but those objects will be looked upon as being at rest whose images always occupy the same part of the retina, that is, when the motion of those images are not perceived in the bottom of the eye.” Keill’s Introduction to Natural Philosophy, page 79.

this is performed by the natural elasticity of the parts ; or, whether the antagonist oblique muscles take up the action and reinstate the eye, I do not know. If the head still continues its motion in the same direction, then the same oblique muscles begin to act anew, and go on acting, so as to keep the eyes fixed on the object. As this motion of the head seldom takes place uncombined with its other motions, some of the straight and oblique muscles will be employed at the same time, according as the motions are more or less compounded.

A DESCRIPTION OF THE NERVES WHICH SUPPLY THE ORGAN OF SMELLING.

THE nerves being in themselves perhaps the most difficult parts of an animal body to dissect, becomes a reason why we are still unacquainted with many of their minuter ramifications: yet if a knowledge of these, together with that of their origin, union and reunion, is at all connected with their physiology, the more accurately they are investigated, the more perfectly will the functions of the nerves be understood. I have no doubt, if their physiology was sufficiently known, but we should find the distribution and complication of nerves so immediately connected with their particular uses, as readily to explain many of those peculiarities for which it is now so difficult to account. What naturally leads to this opinion is, the origins and number of nerves being constantly the same; and particular nerves being invariably destined for particular parts, of which the fourth and sixth pair of nerves are remarkable instances. We may therefore reasonably conclude, that to every part is allotted its particular branch; and that however complicated the distribution may be, the complication is always regular. There are some nerves which have a peculiarity in their course, as the recurrent and chorda tympani; and others which are appropriated to particular sensations, as those which go to four of the organs of sense, seeing, hearing, smelling, and tasting; and some parts of the body having peculiar sensations, (as the stomach and penis) we may, without impropriety, include the fifth, or sense of feeling. This general uniformity, in course, connection and distribution, will lead us to suppose that there may be some other purpose to be answered than mere mechanical convenience; and many of the variations which have been described in the dissections of nerves, I believe to have arisen from the blunders of the anatomist, rather than from any irregularity in

their number, mode of ramifying, course, distribution, or connection* with each other. We observe no such uniformity in vessels carrying fluids; but find particular purposes answered by varying their origin and distribution: the pulmonary artery answers a very different purpose in the circulation of the blood, from that of the aorta; yet both arise from the same source, the heart. The course of the arteries is such as will convey the blood most conveniently, and therefore not necessarily uniform; it not being very material by what channel, provided the blood is conveyed to the part; though, in particular instances, certain purposes may be answered by a peculiarity in origin and distribution, as happens in the testicle of quadrupeds. This observation respecting arteries is likewise applicable to veins, and still more to the absorbent vessels, in which last, regularity is even less essential than in the veins. Whoever, therefore, discovers a new artery, vein, or lymphatic, adds little to the stock of physiological knowledge; but he who discovers a new nerve, or furnishes a more accurate description of the distribution of those already known, affords us information in those points which are most likely to lead to an accurate knowledge of the nervous system: for if we consider how various are the origins of the nerves, although all arise from the brain, and how different the circumstances attending them, we must suppose a variety of uses to arise out of every peculiarity of structure. Indeed, if we reflect on the actions arising immediately from the will, and affections of the mind, we must see that the origin, connection, and distribution of the nerves, ought to be exact, as there are parts whose actions immediately depend upon such circumstances. The brain may be considered as having an intelligence with the body; but no such intercourse subsists between the different parts of the body and the heart.

* Here it is to be understood I do not mean lateral connection; such as two branches uniting into one chord and then dividing; or a branch going to a part, either single or double, for still it is the same nerve; or whether a branch unites with another a little sooner or a little later, for still it is the same branch. Such effects may arise more from a variety in the shape of the bodies they belong to, than any variety in the nerves themselves.

In the summer of 1754, being much employed in dissecting the nerves passing out of the skull; I was, of course, led to trace many of their connections with those from the medulla spinalis; and was assisted by Dr. Smith, then pursuing his studies in London*. The better to trace these nerves through the foramina of the skull, I steeped the head in a weakned acid of sea-salt till the bones were rendered soft, and that the parts might be as firm as possible, and at the same time free from any tendency to putrefaction, (it being summer) the acid was not diluted with water, but with spirit. When the bones were rendered soft, pursuing my intention, I dissected the first pair of nerves, and discovered their distribution; and having made a preparation of the parts in which they were found, I immediately had drawings made from them, with a view to have presented the account to the Royal Society, but other pursuits prevented it†. Engravings were afterwards made from these drawings; and the preparation was repeatedly shown by Dr. Hunter, in his courses of anatomy, who, at the same time, pointed out that alteration in the mode of reasoning upon those nerves, which would naturally arise from this discovery. In this dissection I found several nerves, principally from the fifth pair, going to and lost upon the membrane of the nose; but suppose that those have nothing to do with the sense of smelling; it being more than probable, that what may be called organs of sense, have particular nerves, whose mode of action is different from that of nerves producing common

* Dr. Smith was afterwards teacher in chymistry and anatomy, in the university of Oxford; is now Savilian professor of geometry, and lecturer in physiology. This account of the first pair of nerves, as also of the branches of the fifth, is taken from the original description written by him, and taken from my dissection when I was tracing them.

† Dr. Scarpa, professor of anatomy at Pavia, while in London, in 1782, acquainted me that he had dissected the ramifications of the olfactory nerves; and that on his return to Italy he meant to publish an account of them. At this time I shewed him my drawings and engravings. I have lately been informed that he has published his account, but have not met with it: I have, however, seen one of his engravings, which was executed in London, and is very elegant. It only shews those on the septum narium, whose minuteness is rather carried further than the power of dissection, and the ramifications are more regular than we find them in Nature.

sensation ; and also different from one another ; and that the nerves on which the peculiar functions of each of the organs of sense depend, are not supplied from different parts of the brain. The organ of sight has its peculiar nerve ; so has that of hearing ; and probably that of smelling likewise ; and, on the same principle, we may suppose the organ of taste to have a peculiar nerve. Although these organs of sense may likewise have nerves from different parts of the brain ; yet it is most probable such nerves are only for the common sensations of the part, and other purposes answered by nerves. Thus we find nerves from different origins going to the parts composing the organ of sight, which are not at all concerned in the immediate act of vision ; it is also probable, although not so demonstrable, that the parts composing the ear have nerves belonging to them simply as a part of the body, and not as the organ of a particular sense : and if we carry this analogy to the nose, we shall find a nerve, which we may call the peculiar nerve of that sense ; and the other nerves of this part, derived from other origins, only conveying common sensation, and we may suppose only intended for the common actions of the part. This mode of reasoning is equally applicable to the organ of taste ; and if the opinion of peculiar nerves going to particular organs of sense, be well founded, then the reason is evident why the nose, as a part of our body, should have nerves in common with other parts, besides its peculiar nerves ; and, as the membrane of the nose is of considerable extent, and has a great deal of common sensation, we may suppose the nerves sent to this part, for that purpose, will not be few in number. It is upon this principle the fifth pair of nerves may be supposed to supply the eye and nose in common with other parts ; and, upon the same principle, it is more than probable, that every nerve so affected as to communicate sensation, in whatever part of the nerve the impression is made, always gives the same sensation as if affected at the common seat of the sensation of that particular nerve*.

* I knew a gentleman who had the nerves which go to the glans penis completely destroyed by mortification, almost as high as the union of the penis with the pubes ; and at the edge of the old skin, at the root of the penis, where the nerves terminated, was the peculiar sensation

The first pair of nerves arriving at the part of its destination as soon as it escapes from the skull, and immediately ramifying, has rendered its distribution more obscure than that of the others, whose course to the part to which they are allotted is visible and to be traced. As the body of the nerve, while within the skull, is pulpy and composed of the brain itself, it easily breaks off at the very division and exit of the small branches; it therefore becomes impossible to trace them, as we usually do other nerves; and they have by most physiologists been considered as never forming chords, but going on in their pulpy form to be distributed on the membrane of the nose, in a mode somewhat similar to that of the optic nerve; and to what is commonly supposed to take place with respect to the portio mollis of the seventh pair. Winslow has suggested an idea, that the first pair forms chords; but it is only as an assertion; and not having described them, that alone was not sufficient to alter the former mode of reasoning.

Haller, who is to be considered as the latest anatomist and physiologist, who has published on the subject, on whom we can depend, says, "That the first pair of nerves makes its way into the nose, covered by the pia mater only, very little altered from what it was when within the cavity of the skull*." This shows that Haller retained the old idea concerning these nerves: but we shall find that they become firm chords immediately upon piercing the dura mater and cribriform plate of the ethmoid bone.

The first pair, while within the skull, differs in some respects from all other nerves; firstly, it seems to be made up of a cortical and medullary

of the glans penis; and the sensation of the glans itself was now only common sensation; therefore the glans has, probably, different nerves, and those for common sensation may come through the body of the penis to the glans.

A serjeant of marines who had lost the glans, and the greater part of the body of the penis, upon being asked, if he ever felt those sensations which are peculiar to the glans, declared, that upon rubbing the end of the stump, it gave him exactly the sensation which friction upon the glans produced, and was followed by an emission of the semen.

* *Elementa Physiologiæ*, vol. 5. page 151.

substance, while the others appear to consist of medullary alone; and secondly, it is different, in that it does not seem to be composed of fasciculi, and has but one covering from the pia mater investing the whole nerve; whereas other nerves appear to have a covering round each fasciculus; and this is probably the reason why the first pair is weaker while within the skull, than the others. Its form is somewhat triangular, having three edges, from lying in a groove, made by two convolutions of the brain. The course is forwards, a little upwards and inwards; and where it lies upon the cribriform plate of the ethmoid bone, becomes somewhat larger and divides into a great many branches, like so many roots, answering to the number of holes in that plate, except one left for a branch of the fifth pair; but these divisions we cannot see, they being covered by the body of the nerve, which cannot be raised without breaking off the small branches at their origin. As the branches of the nerve pass through this bone, they seem to take processes from the dura mater along with them, then becoming firm chords, similar to other nerves. These branches, after they have got through the bone, form themselves into two planes or divisions, one passing on the septum, the other on the turbinated bones. Those of the septum narium, in their passage to the nose, are first continued a little way down, in bony canals of the perpendicular lamella of the ethmoid bone, which holes become small grooves in that bone; and those on the opposite side being more numerous and smaller, pass down through small holes that are on the inside plate of the ethmoid bone, which holes are likewise continued into grooves, for a little way, upon that plate. When the branches get upon the membrane of the nose, they subdivide into a great many smaller ones, which are somewhat flattened, and are only to be seen on that side of the membrane that adheres to the bones, not being visible at all on the other; so that the dissection of these nerves is no more than separating the membrane and bone from each other. They can hardly be dissected all round; and the further they are traced upon the membrane the fainter they become, and growing smaller, they sink deeper and deeper into the membrane to get on its outer surface, where we must suppose they terminate. Those upon

the septum pass down a little radiated, and the branches, especially at the upper part, or at their first setting out, unite with one another. Those on the side next the antrum, when they have reached the membrane of the nose, in their course to the superior turbinated bone, form a very considerable network or plexus; and when they reach that bone, do not all go round its convex curved pendulous edge to the concave side; but some passing through its substance, get immediately upon it; which is the reason why we find so many holes in that bone. It is difficult to trace them further; but we have reason to suppose that they go through the inferior turbinated bone in the same manner, since we find similar holes.

A DESCRIPTION OF SOME BRANCHES OF THE FIFTH PAIR OF NERVES.

IN tracing the course of the olfactory nerves, I also discovered several branches of the fifth pair, not commonly known, particularly two that were supposed to go to the membrane of the nose for the sense of smelling; but which only pass through that organ to their place of destination. The first is a small nerve from the first branch of the fifth pair; or, according to Winslow, the *nervus ophthalmicus Willisii*; which small nerve is called by Winslow, the *nasal*. This branch, after having passed out of the skull into the orbit, re-enters the cranium through the foramen orbitarium anterius, and gets on the cribriform plate of the ethmoid bone; from thence it passes down through one of the anterior holes of the cribriform plate, and after having continued its course in a groove on the nasal process of the frontal bone, runs forward and downward in a similar groove on the inside of the *os nasi*; from thence getting on the outside of the cavity of the nose, it runs along the cartilaginous part of the ala, and near the extremity of the nose mounts up upon the tip of the ala, and then dipping down between the two alæ, is lost on the anterior extremity of the cartilaginous septum. In its course it sends several small filaments into the alæ.

The second, is a branch of the superior maxillary nerve; for that nerve having passed through the foramen rotundum, divides and sends off several branches, one of which passing backwards and inwards, through the foramen commune, between the orbital process of the palate, and the root of the ala of the sphenoid bone, gives a branch which gets into a fissure that seems to separate the root of the ala from the body of the sphenoid bone, where that bone makes the roof of the nose. This branch then passes along the under surface of the body of the sphenoid bone, in its way to the septum narium, and getting upon that part, passes along between its membranes and the bone: its course is downwards, and forwards towards the foramen incisivum, through which it passes and is lost in the gum behind the first dentes incisores, and on the membrane of the roof of the mouth at that part.

There is another branch of the superior maxillary nerve, which comes off from a large branch that is going down to the mouth uvula, &c. and this branch, with its division into two, has been described by professor Meckel of Berlin; but after tracing one of these into the portio dura, he pursued the search no further. This branch of the superior maxillary nerve passes back through the foramen pterigoideum, accompanies the carotid artery as it passes across the posterior edge of the foramen, and there divides into two branches; one of which passes down along with the carotid artery, through the basis of the skull, and proceeding in a direction contrary to the course of the artery, in contact with that branch of the cervical ganglion that passes up with the carotid artery to join the sixth pair; then joins the first cervical ganglion. The other branch decussates that artery on its upper surface, and getting upon the anterior side of the petrous portion of the temporal bone, enters a small hole near the bottom of that large one which affords a passage to the seventh pair of nerves, joining the portio dura, just where that nerve making its first turn, passes along with it through what is called the aqueduct. This nerve, composed of portio dura, and the branch of the fifth pair, sends off, in the adult, the chorda tympani before its exit from the

skull; and in the fœtus, immediately after. The termination of the branch, called chorda tympani, I shall not describe; yet I am almost certain it is not a branch of the seventh pair of nerves, but the last-described branch from the fifth pair; for I think I have been able to separate this branch from the portio dura, and have found it lead to the chorda tympani; perhaps, is continued into it; but this is a point very difficult to determine, as the portio dura is a compact nerve, and not so fasciculated as some others are; however this may be, it is very reasonable to suppose that the chorda tympani is a branch of the fifth pair, as it goes to join another branch arising from the same trunk.







P L A T E XVII.

THE olfactory, or first pair of nerves, as they are seen upon the membrane of the septum narium.

The bony septum is removed to expose the nerves of the right nostril, as they pass at first between the membrane and bone.

A The os frontis.

B The frontal sinus.

C The cartilaginous part of the septum narium.

**** The cut edge, from which the septum has been separated all round.

D The surface of the common skin, where it is lost in the membrane of the nose.

E The upper lip.

F Part of the alveolar process of the maxillary bone next the symphysis.

G The roof of the mouth.

H The bony palate.

I The uvula, and palatum molle.

K The upper part of the fauces,

L The opening of the Eustachian tube.

M The cuneiform process of the os occipitis.

N The inside of the cuneiform process, near the foramen magnum occipitale.

O The posterior clinoid process.

P The sphenoid sinus, with its septum.

Q The fella Turcica.

R The crista galli.

SS The membrane of the right nostril that lined the septum; the septum being removed.

T A branch of the fifth pair of nerves, that comes through the foramen commune, or sphenopalatinum.

UUU The first pair of nerves, having passed through the cribriform plate of the ethmoid bone, ramifying on the membrane of the septum.



P L A T E XVIII.

THE olfactory, or first pair of nerves, as they are seen upon the membrane of the nose, which covers the turbinated bones; the exterior parts of the face being removed.

This engraving was taken from the same head as plate XVII.

- A The os frontis.
- B The os nasi.
- C The cartilaginous and membranous part of the nose.
- D The ala nasi, with the skin left on.
- E The septum narium.
- F The upper lip.
- HHH The alveolar process of the superior maxillary bone.
- I Part of the antrum.
- K The os occipitis.
- L The body of the sphenoid bone.
- M The groove made by the carotid artery.
- N The posterior clinoid process.
- O The sphenoid sinus.
- P The crista galli.
- Q The membrane of the nose.
- R The membrane, a little more convex, where the inferior turbinated bone is situated.
- S The same where the superior turbinated bone is situated.
- T The branch of the fifth pair of nerves that was supposed to be lost on the membrane of the nose.
- UUU The trunk of the first pair of nerves which is afterwards lost upon that part of Snecider's membrane that covers the turbinated bones.

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