Hunter on Animal Œconomy
Pharmacopoeia (London)

OBSERVATIONS

ON

CERTAIN PARTS

OF THE

ANIMAL ŒCONOMY.

By JOHN HUNTER.

LONDON,
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TO

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,

JOHN HUNTER.

LEICESTER-SQUARE,
NOVEMBER, 9, 1786.

THE

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On

ERRATA.

Page 14, line 2, for *invariable*, read *invariably*.
28, 25, for *falling a*, read *falling from a*.
63, 23, for *principle*, read *principal*.
81, 15, for *the humeri*, read *the os humeri*.
88, 1, for *freeze*, read *froze*.
101, 1, for *affect*, read *effect*.
118, 19, for *principle*, read *principal*.
120, laſt line, for *rouſes and depreſſes*, read *rouſe and depreſs*.
187, line 13, for *ſwallows*, read *they ſwallow*.

A DESCRIPTION

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

A Diſcovery in any art not only enriches that to which it immediately belongs, but elucidates all thoſe to which it has any relation. A knowledge of the conſtruction of the human body is eſſential to medicine, therefore every improvement in anatomy muſt throw new light on that branch of ſcience; and theſe improvements are more ſtriking when they are new; which is well illuſtrated by the advantages derived to pathology, from the diſcovery of the lymphatics being the abſorbent ſyſtem; and is no leſs evident in the hernia, where the inteſtine lies in contact with the teſticle, which by the diſcovery of the original ſeat of the teſticle is perfectly explained.

Several years before Haller's Opuſcula Pathologica were publiſhed, my brother informed me, that in examining the contents of the abdomen of a child, ſtill born, about the ſeventh or eighth month, he found both the teſticles lying in that cavity, and mentioned the circumſtance with ſome degree of ſurpriſe. We could never explain this matter 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 1755, when I firſt had the pleaſure of reading Baron Haller's obſervations on the hernia congenita,[a] it ſtruck my imagination that the ſtate of the teſtis in the fœtus and its deſcent from the abdomen into the ſcrotum would explain ſeveral things concerning ruptures and the hydrocele, and particularly that obſervation which Mr. 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 opportu-

[a] Alberti Halleri Opuſcul. Patholog. Lauſan. 1755, 8vo. page 53, &c.

nity 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 are taken from my notes, publiſhed by Dr. Hunter, in his commentaries, to which I have ſince 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 full-grown 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 fœtus than in the adult; the ſigmoeide flexure of the colon, part of the rectum, the greateſt part of the bladder, the fundus uteri, the fallopian tubes, &c.

&c. being placed in the fœtus above the hollow of the pelvis, in the common or great abdominal cavity.

At this time the ſhape or figure of the teſtis is much the ſame as in the adult, and its poſition or attitude is 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. But as the teſtis is leſs connected with the ſurrounding parts while it is in the loins, its poſition may be a little variable. The moſt natural ſeems to be when the anterior edge is turned directly forwards; but the leaſt touch of any thing will throw that edge either to the right ſide, or to the left, and then the flat ſide of the teſtis is turned forwards.

It is attached to the pſoas muſcle all along its poſterior edge, except juſt at its upper extremity. 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 in older bodies, but is larger in proportion, and adheres backwards to the pſoas. When the fœtus 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 fœtus 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. Both theſe arteries run in a ſerpentine direction, making pretty large but gentle turnings; both are ſituated behind the peritonæum, and both run into the poſterior edge of the teſtis, between the two reflected laminæ of that membrane, much in the ſame manner as the veſſels paſs to the inteſtines between the two reflected laminæ of the meſocolon or meſentery.

The veins of the teſtis are analogous to its arteries. The ſuperior ſpermatic vein, to begin with its trunk, riſes commonly in the following manner; on the right ſide from the trunk of the 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, 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, attending upon 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 fœtus; 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 ſympathiſing ſo readily with it and

and its particular ſenſation, with 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. There 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 it 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 fœtus there is a cavity or pouch formed between the middle part of the teſtis, and the middle part of the epididymis, which is more conſiderable than what is commonly obſerved in full-grown ſubjects. As the body grows, the epididymis adheres more cloſely to the ſide of the teſtis. The greateſt part of the epididymis is made up of one convoluted canal, which becomes larger in ſize and leſs convoluted towards the lower end of the epididymis, 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.

This duct 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 at a more advanced period of life, in the fœtus at this age 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 of theſe is, 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.

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The uſe of this muſcle, when the teſticle is in the ſcrotum, appears to be evidently that of a ſuſpenſory: but what purpoſe it anſwers in the fœtus, or in animals whoſe teſticles remain in the abdomen, is not eaſily immagined, 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 inſtead of going acroſs to the pubis and ſpreading upon the external ſurface of the gubernaculum immediately under the peritonæum; it appears to be loſt on the peritonæum, a little way from the teſticle; this 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 obeying the ſame command of the will.

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 directs its courſe in its deſcent. 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 ſpermatic veſſels begin to paſs through the muſcles. Here 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

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part of the round ligament of the uterus in a fœtus 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 the ſtructure or compoſition of this ligament may be. It is certainly vaſcular and fibrous, and the fibres run in the direction of the ligament itſelf. It is covered by the fibres of the cremaſter or muſculus teſtis which is placed immediately behind the peritonæum; this 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 full-grown.

In the hedge-hog the teſtes continue through life to be lodged within the abdomen, in the ſame ſituation as in the human fœtus; 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 in 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, where 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 remains ſuſpended in the abdominal cavity, I find that the ſame cremaſter 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, and which in this ſtate of that animal is about an inch and an half in length. In the human fœtus, while the teſtis is retained in the cavity of the abdomen,

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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 thoſe 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 paſſing out of the abdomen, and then upwards, ſo as to cover more of the ligament than what 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 doubled 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 fœtuſes I found the aperture ſo large, that I could puſh the teſtis into it, as far as the tendon of the external oblique muſcle.

From

From this original ſituation within the abdomen the teſtis is afterwards moved to its deſtined ſtation in the ſcrotum. It is the more difficult to aſcertain the exact time of this motion, 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 is the principal means of preventing the hernia congenita.

At the before mentioned period, the teſtis moves downwards till its lower extremity comes into contact with the lower part of the abdominal parietes. By this time the upper part of the ligament, which hitherto was within the abdomen, has ſunk downwards, lies in the paſſage from the abdomen to the ſcrotum, and lies in that paſſage which is afterwards to receive the teſtis. As the teſticle paſſes out, it in ſome degree inverts the ſituation of the ligament paſſing down behind 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 readily paſs from the abdomen to the ſcrotum. 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 who have 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 poſſeſſing its determined ſtation; though it commonly remains for ſome time by the ſide of the penis, and by degrees only 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 removal 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 ſac of the hernia congenita in ſome bodies, and the tunica vaginalis propria in others.

When the teſtis is deſcending, and when it has even paſſed into the ſcrotum, it is ſtill covered by the peritonæum, exactly in the ſame manner as when it was within the abdomen; and the ſpermatic veſſels run down behind the peritonæum there, as they did when the teſtis lay before the pſoas muſcle; and that lamella of the peritonæum is united behind with the teſtis, the epididymis, and the ſpermatic veſſels (beſides the vas deferens) as it was in the loins; and the teſtis is fixed backwards to the parts againſt which it reſts, and is unconnected and looſe forwards, as it was when 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, and 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 it ſlides down from the loins, carrying the peritonæum with it; and both itſelf 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 I ſhould ſuppoſe that it may not be ſo very intelligible, becauſe I find ſtudents very generally puzzled with it, and imagine that, when the teſtis comes firſt down, it ſhould be looſe all around, 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, and to all the other parts

around

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 it can only happen in thoſe which have adheſions to the loins. This I ſuſpect to be rather the conſequence of a rupture already formed, than the cauſe of the firſt formation of the hernial ſac, in which the inteſtine lies, and I ſhould ſuppoſe could only form very gradually. The cæcum has been ſometimes found to have deſcended into the ſcrotum, and to have brought along with it its 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 are not to be operated upon 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 be ſeen at the lower part of it; and the ſpermatic veſſels and the vas deferens will be ſeen 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. This opinion is grounded on the following obſervation.

 I have

I have ſeen 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 puſhed 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 the diſpoſition for ſuch an operation does not exiſt, leaving the lower part of the ſac open or looſe, even in the human ſubject, through life, and forms 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; but in ſome children this union ſeems not to take place regularly, but is interrupted in the middle, producing an hydrocele of the chord, which neither communicates with the abdomen or tunica vaginalis teſtis. This contraction and obliteration of the paſſage ſeems to be a peculiar operation of nature, depending upon ſteady and uniform principles, and not the conſequence of inflammation, or 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 part is much greater; as 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 baulked when ſhe was in the humour of doing 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. 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 cauſe to be the prevention of ruptures, to which men are ſo much more liable

than

than beafts, from their erect ftate of body. In fome cafes the aperture of the fac is not entirely clofed, allowing a fluid to pafs down and form a hydrocele, which fluid upon preffure can be fqueezed back into the belly; cafes of this kind fometimes give the idea of a gut being protruded, and make it difficult to determine the exact nature of the cafe.

What is the immediate caufe of the defcent of the teftis from the loins to the fcrotum? It is evident that it cannot be the compreffive force of refpiration, becaufe commonly the teftis is in the fcrotum before the child has breathed; that is, the effect has been produced before the fuppofed caufe has exifted. Is the teftis pulled down by the cremafter mufcle? I can hardly fuppofe that it is; becaufe, if that was the cafe, I fee no reafon why it fhould not take place in the hedge-hog, as well as in other quadrupeds; and if the mufculus teftis had this power, it could not bring it lower than the ring of the mufcle.

Why do the teftes take their blood-veffels from fuch diftant trunks? Thofe phyfiologifts, who have puzzled themfelves about the folution of this queftion, have not confidered, that in the firft formation of the body, the teftes are fituated, not in the fcrotum, but immediately below the kidneys; and that therefore it was very natural that their blood-veffels fhould rife in the fame manner as thofe of the kidneys, but a little lower. The great length of the fpermatic veffels in the adult body will no doubt occafion a more languid circulation, which, we may fuppofe, was the intention of nature.

The fituation of the teftis in the fœtus may likewife account for the contrary directions of the epididymis and of the vas deferens in adult bodies, though thefe two in reality make only one excretory canal. In the fœtus the epididymis begins at the upper end of the teftis; and it is natural, confidering it as an excretory tube, that it fhould run downwards. And it is as natural that the reft of the tube, which is called vas deferens, fhould turn inwards at the lower end of the teftis, becaufe that is its moft direct courfe to the neck of the bladder. Thus we fee that in the fœtus the excretory duct is always paffing downwards. But the teftis is directed in its defcent by the gubernaculum; and this is firmly fixed to the lower parts of the teftis and epididymis, and

and to the beginning of the vas deferens, and thence muſt keep thoſe parts invariable 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 bubonocele and hydrocele, 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, that it would ſeem 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 antient times, when the parts of the human body were deſcribed from diſſections and obſervations made principally upon brutes: and the circumſtances in the ſtructure of the parts, which are peculiar to the fœtus, having been imperfectly underſtood, we may ſuppoſe, has likewiſe contributed to make perplexity and contradiction among authors.

Baron Haller, in his Opuſcula Pathologica, has obſerved, that in infants, ſometimes the inteſtine 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 very peculiar circumſtances in a rupture of this kind; the inteſtine is always in immediate contact with the teſtis, and there is no tunica vaginalis propria teſtis. The ſtructure of the parts in the fœtus explains, in the moſt ſatisfactory manner, both thoſe circumſtances, however extraordinary they muſt appear to a man who is only converſant with the ſtructure of the parts in ſubjects of a more advanced age: and indeed it is ſo clear that it needs no illuſtration. I may obſerve, however, that the hernia congenita may happen, not only by the inteſtine falling down to the teſtis before the aperture of the ſac be cloſed,

cloſed, 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 that tunic 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 given an account of the ſituation of the teſticles in the fœtus, of their deſcent, and the circumſtances attending it, I ſhall next conſider the caſes where this change takes place in one or both teſticles later than the uſual or natural time: and having taken notice of the conſequences of this deſcent when it happens at ſo late a period, I ſhall proceed to mention thoſe caſes in which the teſticles never paſs out of the abdomen.

I have ſaid, that the early coming down of the teſticles, and cloſing of the mouth of the ſac, by uſually taking place before birth, hinder 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 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 compleated; 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; it is however certain that the teſticle which has com-

pleated

pleated 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 compleated before birth, often requiring years for that purpoſe; it ſometimes never reaching 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 immagined; they are 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 ſome meaſure loſt or not; but when it comes down after birth, we can eaſily ſuppoſe ſome portion of inteſtine or epiploon more ready to deſcend and prevent the cloſing of the mouth of the ſac, than before the child was born when no ſuch actions had 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 compleating 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

doubt often ariſes in the mind, whether it is better entirely to prevent its paſſage, or to aſſiſt it by exerciſe or other means; it would certainly be the beſt practice to aſſiſt it if that could be done effectually. When it has got upon the outſide of the tendon it in general can be eaſily puſhed up again into the abdomen; and in theſe two ſituations it will ſometimes play backwards and forwards, for years, without ever coming low enough to allow of the uſe of artificial means to prevent 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 and give 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 perceived that any inconvenience aroſe 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 above the teſticle; taking care the teſticle is not injured by it: but as this generally happens at too early a period for the patients themſelves to be able to attend to it, it becomes the more neceſſary that the ſurgeon who is employed ſhould be very attentive, and that thoſe who have the charge of their education ſhould watch them with particular care.

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

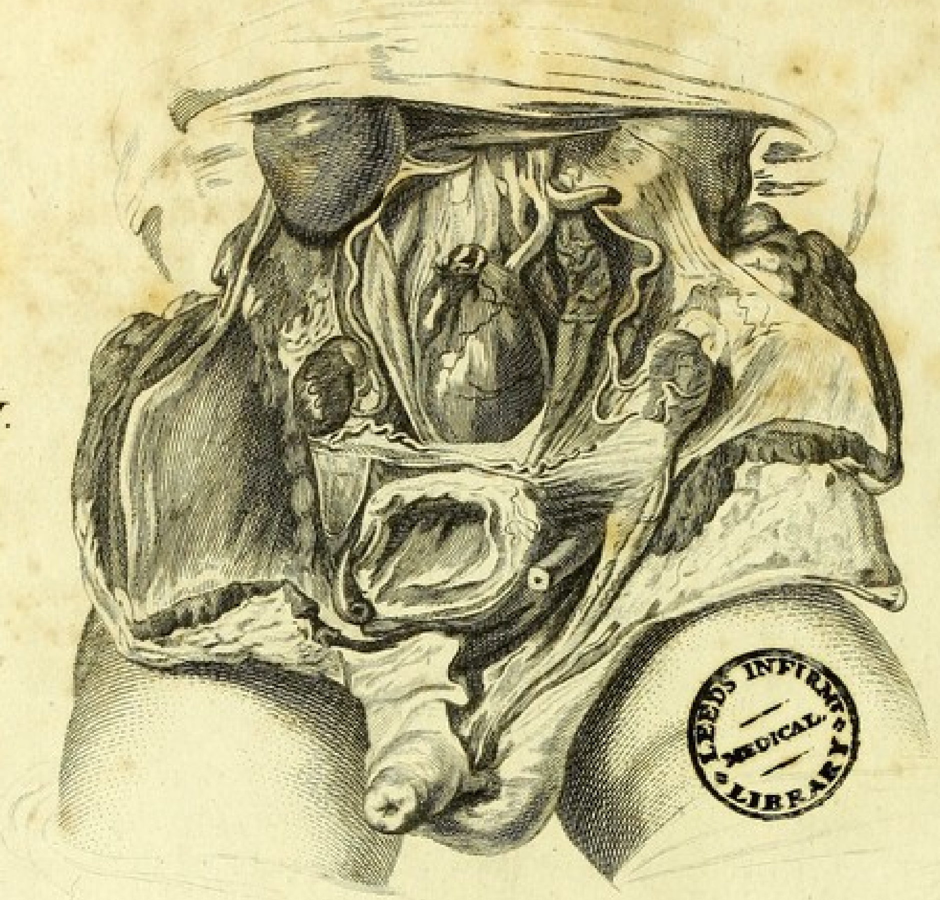
It ſometimes happens that one of the teſticles remains in the cavity of the abdomen through life, never aſſuming the diſpoſition to change its ſituation; when this happens, the perſon naturally concludes that he has only one teſticle; and it can only be known that he had two by an examination of theſe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

When both teſticles remain through life in the belly, I believe that they are exceedingly imperfect, and incapable of performing the natural functions of thoſe organs; and this imperfection prevents the diſpoſition for their deſcent taking place. They are more defective than thoſe which are late in coming into the ſcrotum. This is very evident in the quadruped, for in them the teſticle which has reached the ſcrotum is conſiderably larger than that which remains in the abdomen. It is probable that it is a tendency towards an hermaphrodite, the teſticle ſeldom being well formed. In ſuch caſes nothing is to be done by art; as it is not poſſible to give the ſtimulus of perfection to ſuch teſticles, which I believe is neceſſary to make them aſſume the diſpoſition which is requiſite for their deſcent; and the ring of the external oblique muſcle is probably leſs liable, in theſe inſtances, to allow of the deſcent of a portion of inteſtine than where the teſticles have paſſed through it; ſuch perſons being perhaps more ſecure from accidents of this kind than if they had been more perfectly formed.

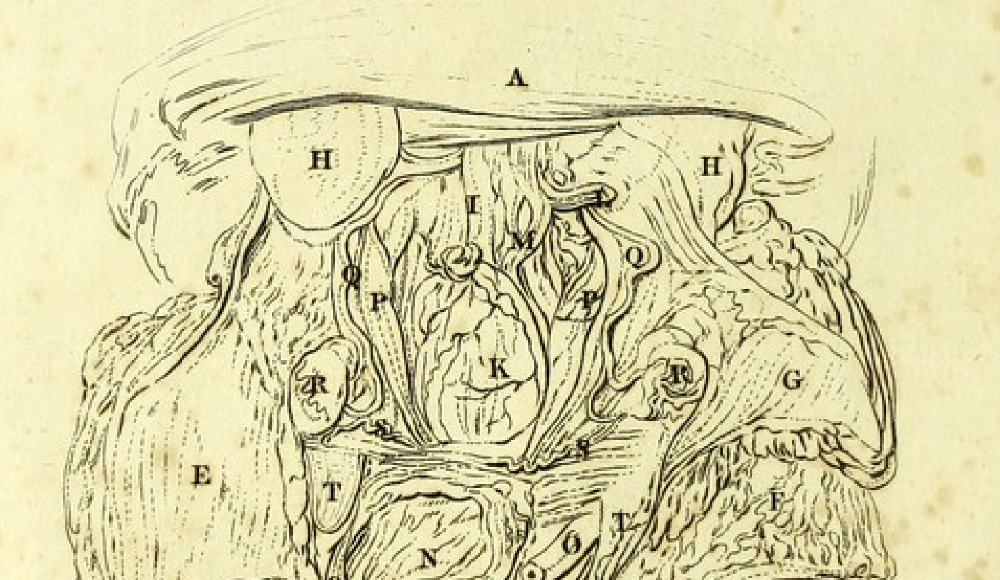
That the deſcriptions which I have given may be better underſtood I have annexed three figures that were carefully taken from nature.

PLATE I.

Fig. I.



Out-lines of Fig. I.



J. V. Riemsdyk del.

P. C. Canot 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.

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, turned back over the right os iliûm 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 iliûm. The lower part of this flap is cut away, in order to ſhew 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 lower part of the bladder, that part of it 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 its 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 ſeminalis.

TT What I have called the gubernacula or ligaments of the teſtes in a fœtus. On the left ſide this ligament is entire, ſo that it is ſeen going down from the lower end of the teſtis, through the ring of the muſcle, 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.

PLATE II.

Out-lines of Fig. II.

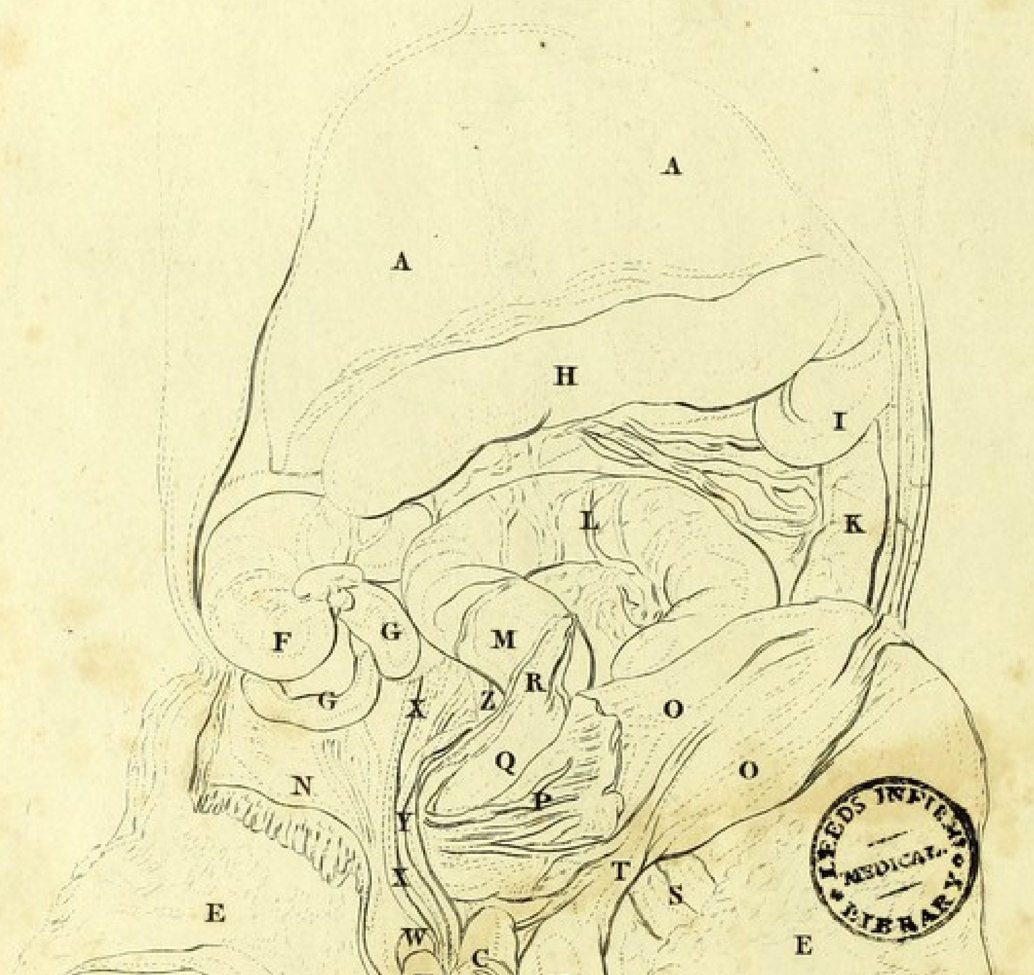
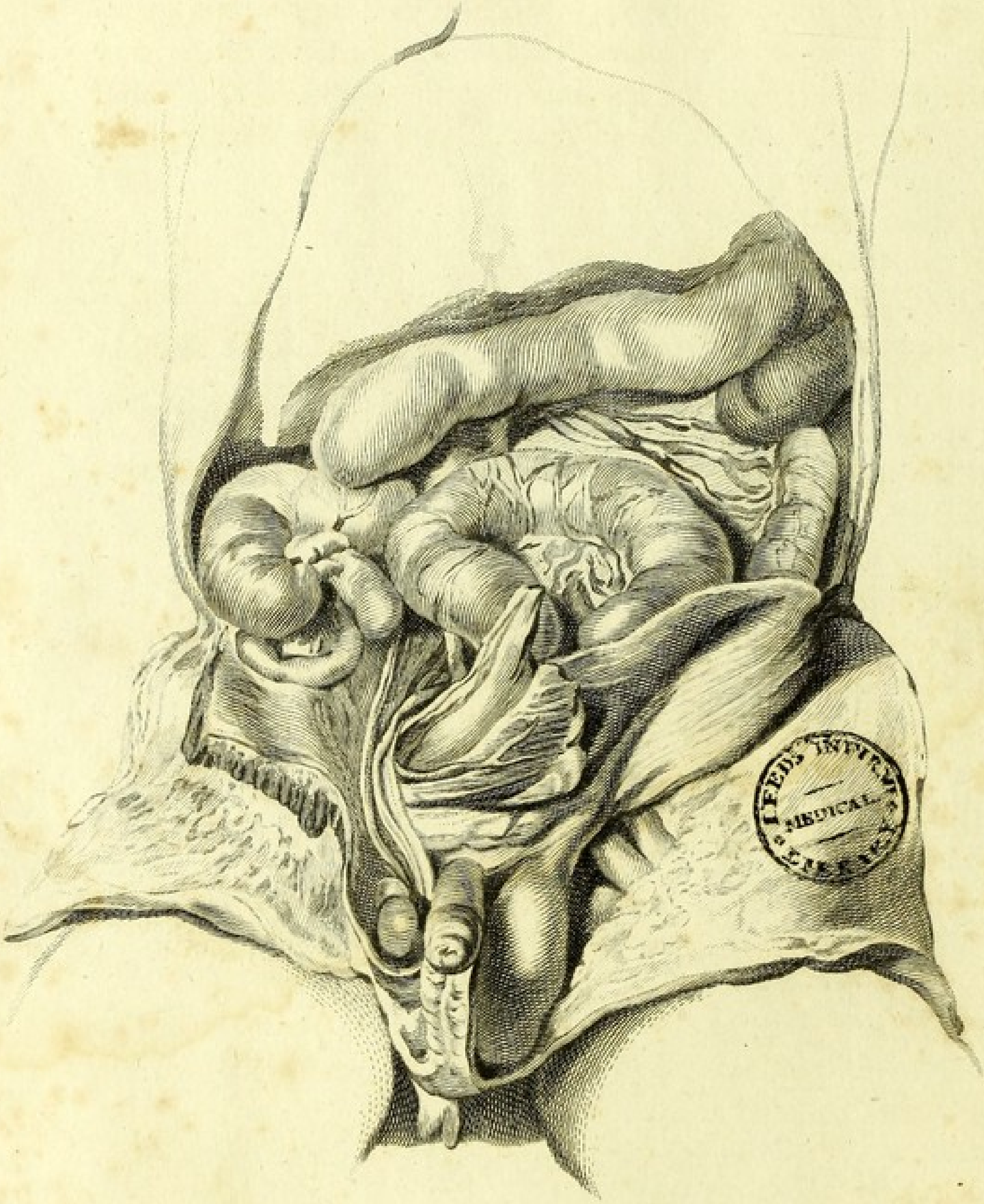


Fig. II.



J. V. Riemsdyk del. — P. C. Canot sculp.

PLATE II.

THE ſecond figure repreſents nearly the ſame parts in a fœtus, ſomewhat older, in order to ſhew 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.

AA The liver, in out-lines.

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 ſigmoeid 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 back over the ſpine of the os iliûm.

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

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 epididymis of the ſame ſide.

XX The ſpermatic veſſels.

Y The vas deferens. *N. B.* The peritonæum lies before the ſpermatic veſſels and vas deferens, or covers them within the abdomen; and its proceſs or elongation covers them in the ſame manner all the way from the abdominal muſcles downwards; ſo that if the inteſtine ſlips down after the teſtis in a fœtus it muſt be placed before the ſpermatic veſſels and vas deferens.

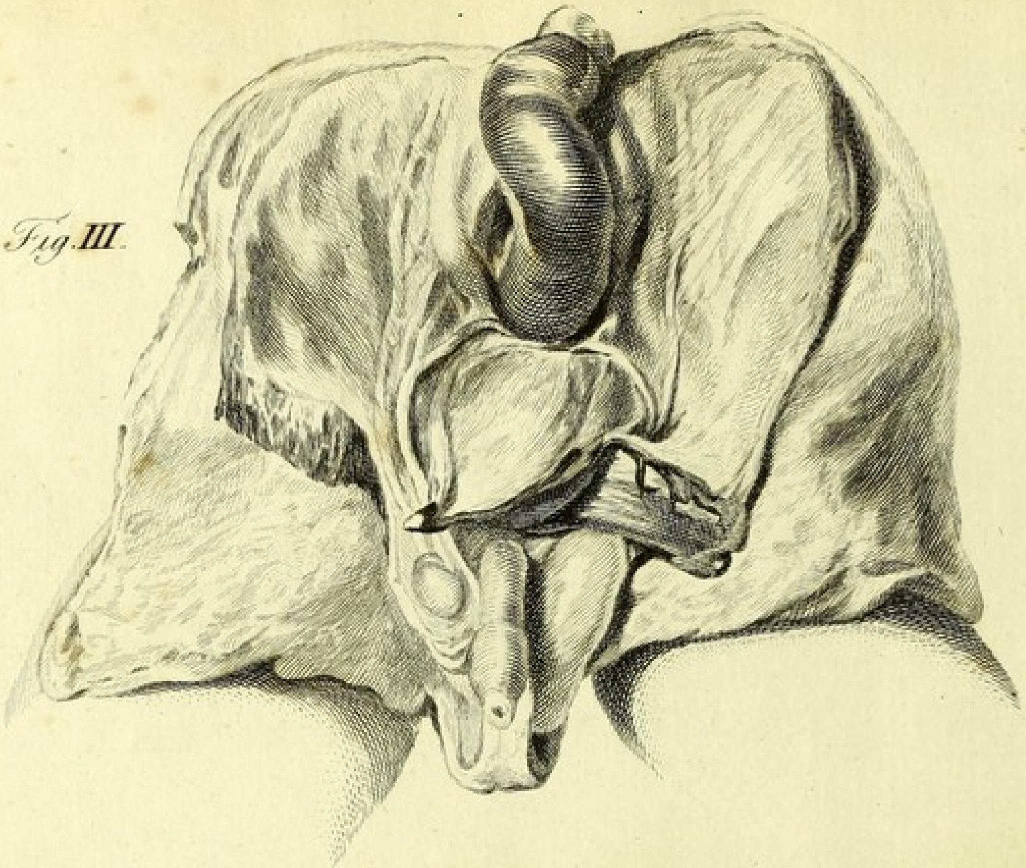
Z The ureter.

& 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 a 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.

PLATE III.

Fig. III.



Out-lines of Fig. III.

J. V. Riemsdyk del.   P. C. Canot sculp.

PLATE III.

THE third figure reprefents the teftes, &c. in the fame fubject; all the parts above the offa iliûm being cut away, and the abdominal mufcles and the bladder being turned downwards.

AA The thighs, unfinifhed.

B The penis.

C The middle part of the fcrotum, its lateral parts being removed to fhow the teftes.

DD The fkin and cellular membrane of the abdomen turned down over the thighs.

EE Part of the abdominal mufcles and peritonæum turned down at each groin.

FF The peritonæum covering the iliacus internus mufcle of each fide.

G The inteftinum rectum filled with meconium.

H The bladder with the umbilical artery on each fide of it, turned a little forwards over the fymphyfis of the pubes.

II The ureters paffing over the iliac veffels to the pelvis.

K The right teftis expofed, as in Fig. II. V. W. XX. Y.

L The left teftis inclofed in the procefs of the peritonæum, See Fig. II. U.

M The fpermatic veffels of the left fide, feen through the peritonæum which covers them, in their defcent through the abdominal mufcles at the groin.

N The left vas deferens feen through the peritonæum, in its paffage from the mouth of the fac to the pofterior part of the bladder.

O The mouth or aperture of the procefs of the peritonæum, whereby its mouth or cavity communicates with the general cavity of the belly. This aperture clofes up, and the membrane becomes fmooth at this place, when the fœtus grows a little older; unlefs when the gut falls down after the teftis, and keeps it open. In that cafe it makes the mouth of the hernial fac.

P The left epigaftric artery branching upon the infide of the rectus mufcle, which is here turned downwards and outwards. This artery is always fituated, as in this figure, on the infide of the mouth of the hernial fac, or paffage of the fpermatic veffels.

PLATE IV.

Plate IV.

PLATE 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 half from the ring, I found the termination of the cremaſter, which was a tolerable 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 back part 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.

F G 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

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

THE bags ſituated between the bladder and rectum in the male of ſome animals, which are commonly called veſiculæ ſeminales, have been conſidered as reſervoirs for the ſemen, ſecreted by the teſticles in the ſame manner as the gall-bladder is ſuppoſed to be a reſervoir for the bile. Phyſiologiſts muſt have been led to 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 theſe 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 ſimilar parts in other animals ſuppoſed to anſwer the ſame 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 do not ſerve the purpoſe of 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 appears 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 vaſa 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 cheſnut. The taſte is at firſt inſipid; yet there is ſomewhat of pungency in it, which after ſome little time ſtimulates and excites a degree of warmth in the mouth. The fluid contained in theſe veſiculæ, in a dead body, is of a browniſh colour, and often varies in its conſiſtence in different parts of the bag, as if not well mixed. Its ſmell does not reſemble that of the ſemen; and it does not become more fluid by being expoſed to the air.

It may however be objected, 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 vaſa 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 the contents of theſe veſiculæ, 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 is uſually 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 a conſiderable height, and whoſe body I inſpected ſoon after the accident, I found the contents of the veſiculæ 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 ſeminal weakneſs, and is I believe commonly ſuppoſed to be the ſemen[a]; but in all the caſes of

[a] Vide Treatiſe on the Venereal Diſeaſe, page 197.

this

this kind in which I have been conſulted, it nearly reſembled the contents of the veſiculæ in the dead body; 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.[a]

In this country eunuchs ſeldom come under our examination. We have ſometimes however 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 only loſt one teſticle are more to our preſent purpoſe 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, and which had remained there ever ſince. But as caſtration is 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 they will neither be ſo large nor ſo full in them 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, they will not grow to the full ſize. But where only one teſticle is removed, its loſs does not in the leaſt affect

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

generation,

generation, therefore does not produce any change in the veficula of that fide from which the tefticle is taken; becaufe the veficula does not depend upon the tefticle for its fecretion, but upon the conftitution, and the perfon being capable of the action of generation.

A man who was under my care in St. George's Hofpital for a venereal complaint, died there, and was difcovered to have loft his right tefticle. From the cicatrix being hardly obfervable, it muft have been removed fome confiderable time before his death; and the complaint for which he was received into the hofpital is a convincing proof that he had connection with women after that period.

I infpected the body in the prefence of Mr. Hodges, the houfe-furgeon, and feveral of the pupils of the hofpital. Upon diffecting out and examining the contents of the pelvis, with the penis and fcrotum, I found that the vas deferens of the right fide was fmaller and firmer in its texture than the other, efpecially at that end next to the abdominal rings, near to the part that had been cut through in the operation. The cellular membrane furrounding the duct on the right fide was not fo loofe as on the left; neither were the veffels which ramified on the right veficula fo full of blood. But upon opening the veficulæ, both appeared to be filled with a kind of mucus fimilar to that which is found in other dead bodies; and the veficula of the right fide was rather larger than that on the left. Whatever therefore may be the real ufe of thefe veficulæ, we have a proof from this diffection, that in the human fubject they do not contain the femen.

In a man who died in St. George's Hofpital with a very large bubonocele, the tefticle of that fide was difcovered to have almoft loft its natural texture from the preffure of the hernial fac; and upon examining the tefticle with attention, there was no appearance of vas deferens till we came near the bladder, where it was almoft as large as ufual. The veficula of that fide was found to be as full as the other, and to contain the fame kind of mucus.

I extirpated the left tefticle of a Frenchman on account of its being difeafed. He was a married man, and died about a year afterwards, having been extremely ill for feveral months before his death. On examining

amining the body, the veſiculæ were both found nearly full; more eſpecially that of the left ſide, which I ſuppoſe might be accidental. But upon examining the vas deferens of the left ſide, where it lies along the ſide of this bag, and where it has a ſimilar ſtructure with the veſiculæ, I found it filled with the ſame kind of mucus; and this I believe is always the caſe, whether the teſticle has been removed or not.

A young man, a coachman, who had a diſeaſe in his left teſticle, 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, and for which he requeſted to be put into the warm bath. But as he was going from the ward to the bath he dropped down and died almoſt immediately. The body was inſpected, with a view to diſcover the cauſe of his death; and upon an 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 diſſecting a male-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 found 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 the vas deferens for nearly the whole length

length of the ſpermatic chord of the right ſide.[a] On the left ſide the vas deferens begun where the epididymis commonly terminates; and there was a deficiency of nearly an inch of the extremity of the epididymis.[b] I then diſſected the bag above mentioned, 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 diſcover no duct leading from them to the proſtate gland, nor any remains of one.

In this ſubject it was evident 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 ſeen. The teſticles were very ſound, and the ducts from them to the epididymis were very manifeſt and full of ſemen.[c]

[a] Vide plate I, fig. 1. [b] Vide plate I, fig. 2.

[c] 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 were 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, and commonly terminates 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, there being 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 to believe, from analogy, 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; and afterwards become a ſtraight canal, paſſing along with it for ſome way, when they are commonly obliterated.

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

From

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 the quantity of ſemen ſecreted and the encreaſed 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 is ſtill kept up, and the pain in the teſticles will in general continue till the paroxyſm and evacuation of the ſemen is brought on, which renders the act complete; without which a ſtop cannot be put to the action of the veſſels that produce the ſecretion, nor the parts be allowed to fall back into their natural ſtate. There is at this time no ſenſation of any kind felt in the ſeat of the veſiculæ ſeminales. The pain in the teſticles, in conſequence of being filled with ſemen and 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 ſtrong robuſt bodies where death has happened from 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 other animals; confining myſelf to thoſe which 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 whofe ufes in different animals are the fame; while the femen in moft of thofe which I have examined may be faid to be fimilar.

The analogy which obtains between thefe bags and the gall-bladder, in the human fubject, by no means holds equally good when applied to other animals. In the horfe they are two bags like fmall urinary bladders, almoft loofe and pendulous, with a partial coat from the peritonæum, under which there are two layers of mufcular 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 clofe to the vafa deferentia, do not communicate with them. The feptum between the two ducts is not continued on quite to the urethra, fo that they cannot, in ftrict language, be faid to enter that paffage feparately; but there is not length of common duct fufficient to admit of regurgitation from the vafa deferentia into thefe bags. They are not of the fame fize in the gelding and in the ftone-horfe, being largeft in the laft. Their contents in both are exactly fimilar, and nearly equal in quantity; but in no way refembling the femen emitted by the ftone-horfe in the coitus, or what is found in the vas deferens after death.

In the boar thefe bags are extremely large, and divided into cells of a confiderable fize; or they may more properly be faid to form ramifications clofely connected with one another, and having a large canal or duct common to the whole. The ducts contain a whitifh fluid, very unlike what is found in the vafa deferentia of the fame animal, with which they have not the leaft communication.

In the rat the bags are large and flat, with ferrated edges, and lie fome way within the abdomen, containing a thick afh coloured mucus, nearly of the confiftence of foft cheefe, very different from what is found in the vafa deferentia of the fame animal, with which they do not communicate.

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

In the Guinea-pig they are compofed of long cylindrical tubes, and lie in the cavity of the belly; are fmooth on their external furface, and do not

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 would appear to be the part which 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 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; and it is natural to ſuppoſe that if the veſiculæ were reſervoirs of ſemen, they would be more neceſſary in birds; the power of repeating the act of copulation being in them infinitely greater than in quadrupeds: and indeed we find that in birds there are reſervoirs which will 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, but ſimply a groove, as the drake and gander; and many are even without a groove, as the common fowl, it was abſolutely neceſſary there ſhould be ſuch a reſervoir ſomewhere.

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 uſe 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 in proportion as the parts connected with ſuch feelings are formed, and are in a ſtate to act; and the diſpoſition for action in theſe parts is alſo in proportion to their formation 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 heat ſuitable to that claſs to which the animal belongs. As in moſt parts of the globe the ſeaſons vary in their temperature; the cold in ſome of them is ſo conſiderable as to prevent thoſe feelings or diſpoſitions in animals from taking place; and in many ſituations the general influence

of

of the cold upon them is ſo great, as during its continuance to deprive them of theſe feelings and diſpoſitions, and to render them, for the time, unfit for the purpoſes of generation.[a]

The teſticle becomes at this ſeaſon ſmall, a fact 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[b]; 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.[c] 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; but in the land-mouſe, mole, &c. this diminution is ſtill more remarkable. Thoſe animals, on the contrary, who are not in a ſtate of nature, have no ſuch change take place in their teſticles; and from not being much affected by ſeaſons are conſequently always in good condition, and in that 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 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 given 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 diſcernible in the winter; and 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, admitting them to be ſeminal reſervoirs: but what happens to the proſtate gland, which

[a] 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.

[b] Vide plate III. fig. 1. [c] Vide plate III. fig. 5.

has

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 obſervable, 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, depend 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 effect the growth of the other parts. In the perfect male the penis is large; the corpora cavernoſa[a] being capable of dilatation. The corpus ſpongioſum is very vaſcular[b]; and that part of the canal which is called the bulb is conſiderably enlarged, forming a cavity; the muſculi acceleratores urinæ, as they are termed, are ſtrong and healthy. In many animals which have long peniſes, they are continued forwards to the end of it; and in others they are not extended ſo far, but 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

[a] 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.

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

muſcles

mufcles are white, fmall, and have a ligamentous appearance. The fame obfervations are true if applied to the erectores penis.

The penis of the perfect male is of a fufficient length, when erected, to reach to the further end of the vagina of the female. In the caftrated animal it is much fhorter; and erections having then become unneceffary, the parts which fhould project, often adhere to the infide of the prepuce. The erectores mufcles in the perfect male are ftrong enough to fqueeze at once the blood out of the crura into the body of the penis, fo as to ftraiten and contract the urethra inftantaneoufly, and the acceleratores urinæ[a] have fufficient power to throw out the femen that is gradually accumulated at the bulb for ejection.

The proftate gland[b], Cowper's glands, and the glands along the urethra, (of which the lacunæ are the excretory ducts) in the perfect male are large and pulpy, fecreting a confiderable quantity of a flimy mucus, which is falt to the tafte, is moft probably for the purpofe of lubricating thofe parts, and is only thrown out when in vigour for copulation: while in the caftrated animal they are fmall, flabby, tough and ligamentous, and have little fecretion. From this account a confiderable difference in appearance is diftinguifhable between the parts connected with generation of the perfect male, and thofe which remain in one that has been caftrated;

[a] I fhall call thefe mufcles, expulfores feminis, as I apprehend their real ufe to be for the expulfion of that fecretion: thefe mufcles likewife throw out thofe drops of urine which are collected in the bulb from the laft contractions of the bladder; and they have been from this circumftance named, acceleratores urinæ; but if a receptacle had not been neceffary for the femen, thofe mufcles had probably never exifted, and the laft drops of urine would have been thrown out by the action of the bladder and urethra, as in fome meafure is the cafe in the caftrated animal. That the urethra has the power of contraction is evident upon the application of any ftimulus; for I have feen the urethra refufe to allow an injection to pafs on: and in that part where the injection ftopped, a fulnefs was felt which terminated at once: this contraction is moft probably in the internal membrane.

[b] The proftate gland is not common to all animals. It is wanting in the bull, buck, and moft probably I believe in all ruminating animals. In this clafs the coats of the veficulæ are much thicker, and more glandular, than in thofe who have proftate glands; it is therefore natural to fuppofe that the veficulæ anfwer nearly the fame purpofes as the proftate gland.

The proftate gland, and Cowper's glands, as well as the veficulæ, are wanting in birds, in the amphibious animals, and in thofe fifh which have tefticles, as all of the ray kind.

more

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 the powers remained; therefore whatever parts had a connection with theſe powers would ſtill have the ſtimulus of perfection given to them.

The difference of appearance of the bulb, and the muſcles, would ſeem to point out, that in the perfect male the enlargement of the former is 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 moſt acquainted with, that the bulb is intended for this 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.[a] This accumulation of blood in the corpus ſpongioſum diminiſhes

[a] 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.

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 are ſtill more diſtended, and conſequently the canal of the urethra in that proportion diminiſhed. The ſemen in the time of copulation, in ſuch 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 acts 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 moſt diſtant fibres 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 action ſqueezed forward, but requiring a greater impulſe to propel it, is rather later than the ſemen, on which it preſſes from behind; and 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, ſo that 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 of the ſemen in which it 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.

P L A T E I.

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

FIG. 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.

FIG. 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 vaginales is removed.

E The origin of the vas deferens.

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

THE

Plate I.

Fig. I.

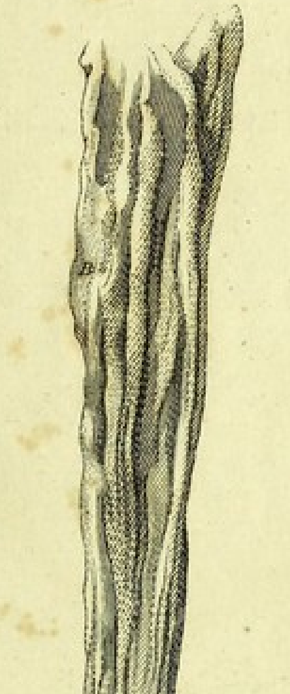
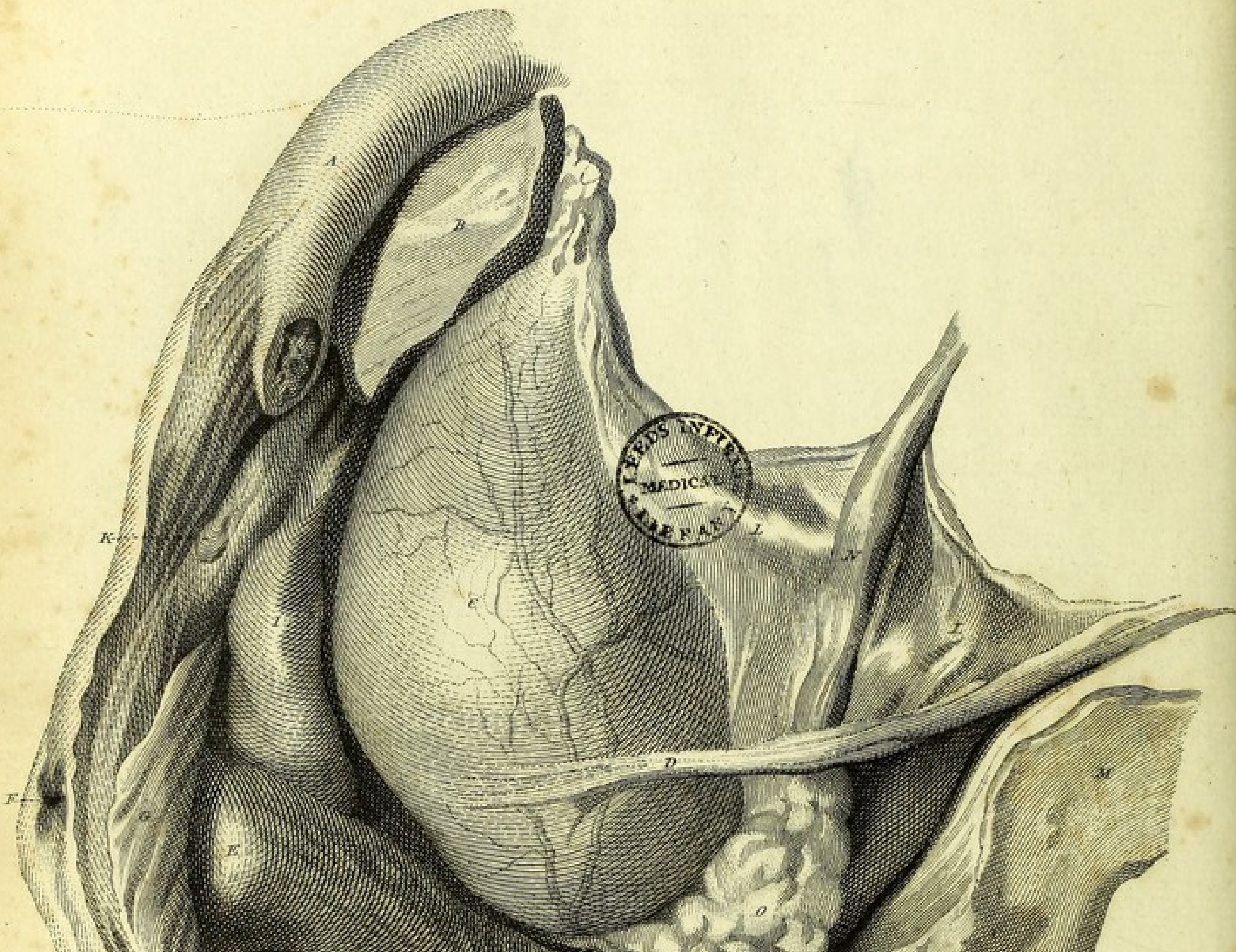


Fig. II.

J. V. Riemsdyk del.

Wm. Skelton sculp.

THE nine 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 ſuch of them as I conſider to be 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.



J. V. Riemsdyk del.   Wm. Skelton sculp.

THE EXPLANATION OF

PLATE II.

A Side view of the pelvis, taken from the ſame ſubject as plate I, 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 peritoneum, which lined the left ſide of the pelvis.
M The ſacrum, where it is articulated with the os ileum.
N The left vas deferens.
O The veſiculæ.

to be put at page 37

Plate III. p. 44.

1 January

2 Middle of February

3 Beginning of March

4 Latterend of March

5 Middle of April

ACCOUNT OF THE FREE MARTIN.

GENERATION, when produced from a ſeed, has two cauſes which concur towards its perfection; the one which forms the ſeed, the other which gives it the principle of action.[a]

The cauſe which forms the ſeed is called the female, the other cauſe is called 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 they were firſt conſidered in animals where thoſe parts were ſeparated, or in which the female parts were found complete in one animal 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 either the one ſex or the other were called either the female or the male parts of generation; but upon a more perfect knowledge of animals, and of thoſe parts, they were found in many of the inferior tribes to be united in the ſame animal, which, from poſſeſſing both parts, has got the name of hermaphrodite.

As both male and female parts are natural to moſt animals, as the union of them in the ſame animal is alſo natural to many, and as the ſeparation of them in diſtinct animals is only a circumſtance making no

[a] 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 that claſs of 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 takes place in many orders of animals, viz. the female produces 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.

eſſential

eſſential difference in the parts themſelves, it becomes no great effort or uncommon play in nature to unite them in thoſe animals in which they are commonly ſeparated.

And accordingly we find many of thoſe orders of animals, which naturally have them ſeparate, have them ſometimes united.

From this account hermaphrodites may be divided into two kinds, the natural and the unnatural.

The natural hermaphrodite belongs to the inferior and more ſimple genera of animals, of which there are a much greater number than of the more perfect; but 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 have alſo taken place.

The unnatural hermaphrodite, I believe, now and then occurs in every tribe of animals having diſtinct ſexes, and is to be met with in all its gradations, from the diſtinct ſex to the complete union of the male and female organs, but is more common in ſome than in others.[a] I fancy it is moſt rarely to be met with 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 expelling and conducting 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 can not poſſeſs in perfection that part which is common to both. For as this common

[a] 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

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 part which they have muſt of courſe partake of both ſexes, and conſequently render the hermaphrodite ſo far imperfect. But thoſe parts which are peculiar to each ſex, may be perfectly joined in the ſame animal, which will come up to the idea of the trueſt hermaphrodite; although it may not be neceſſary 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 happens in parts whoſe uſes are equally ſimilar, as the teſticle and ovarium, forming one body with the properties of neither; which as it approaches nearer to the teſticle or ovarium, will make the animal partake more of the one ſex than the other; and ſome of them, from the ſameneſs of their ſituation in the two ſexes, in many animals may interfere with one another.

The parts in 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, which is alſo the place allotted for the teſticles of the male of this tribe; (and probably of all thoſe in which they come out of the cavity of the belly) therefore in the hermaphrodite which has both theſe parts, the teſticles are in ſome degree obliged to come down into the udder, which does not receive them ſo readily as the ſcrotum.

The hermaphrodites, which I have ſeen, have always appeared externally, and, at firſt view, to be females: this ariſes 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 thoſe animals where the female is preſerved for breeding only, as in ſheep, goats, pigs, &c. they are generally kept, from their being ſuppoſed to be females.

Among horſes they 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 had the appearance of an udder, not being ſo pendulous as the ſcrotum in the perfect male of

ſuch

ſuch animals. There were alſo two nipples, of which the male have no perfect form, being blended in them 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, ſtood almoſt directly backwards.

I procured a foal aſs, very ſimilar in external appearance to the horſe, 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 aſs.

The external female parts were ſimilar to thoſe of the ſhe-aſs. Within the entrance of the vagina was placed the clitoris, but much longer than that of a true female, its length being about five inches. The vagina was pervious a little further than the opening of the urethra into it, and there it was obliterated; from thence up to the fundus of the uterus there was no canal. The common 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 were attached, there paſſed towards each groin a part ſimilar to the round ligament in the female, which were continued into the rings of the abdominal muſcles; but with this difference, that there was continued with them a proceſs or theca of the peritonæum, ſimilar to the tunica vaginalis communis in the male aſs, 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

dites 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, which appears to be almoſt an eſtabliſhed principle in the œconomy of propagation of that ſpecies of animal, and perhaps peculiar to them.

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 becomes a very proper bull. This cow-calf is called in this country a *free martin*; and is juſt 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 have reaſon to believe 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.

When they are preſerved 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.[a]

They are known not to breed: they do not ſhow the leaſt inclination for the bull, nor does the bull ever take the leaſt notice of them.[b]

[a] 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.

[b] Vide Leſlie on Huſbandry, p. 98, 99.

They very much refemble in form the ox or fpayed heifer, being confiderably larger than either the bull or the cow, the horns being very fimilar to the horns of an ox.

The bellow of the free martin is fimilar to that of an ox, having more refemblance to that of the cow than of the bull. Free martins are very fufceptible of growing fat with good food. The flefh, like that of the ox or fpayed heifer, is in common much finer in the fibre than either the bull or cow; and is fuppofed to exceed that of the ox and heifer in delicacy of flavour, and bears a higher price at market.

However, it feems that this is not univerfal; for I was lately informed by Charles Palmer, Efq. of Luckley in Berkfhire, that there was a free martin killed in his neighbourhood, and from the general idea of its being better meat than common, every neighbour befpoke a piece, which turned out nearly as bad as bull-beef, at leaft worfe than that of a cow. It is probable that circumftance might arife from this animal having more the properties of the bull than the cow, as we fhall fee hereafter that they are fometimes more the one than the other.[a] Although what I have faid with refpect to the productions of free martins is in general true, yet I was lately furnifhed with an inftance, by the affiftance of Benjamin Way, Efq. of Denham, near Uxbridge, who knew that I was anxious to afcertain this point, that it does not invariably hold good.

One of his cows having produced twins, which were to appearance male and female, upon a fuppofition that the cow-calf was a free martin, 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 defired that he would keep it; but unfortunately it died about a month old. Upon examining the organs of generation, they appeared to be thofe of the

[a] The Romans called the bull taurus: they however talked of tauræ in the feminine gender. And Stephen obferves, that it was thought the Romans meant by tauræ, barren cows, and called them by this name becaufe they did not conceive. He alfo quotes a paffage from Columella, lib. vi. cap. 22. "and like the tauræ, which occupy the place of fertile cows, fhould be rejected, or fent away." He likewife quotes Varro, De re Ruftica, lib. ii. cap. 5. "The cow which is barren, is called taura." From which we may reafonably conjecture that the Romans had not the idea of the circumftances of their production.

female,

female, and perfectly formed; but to make this more certain, I procured thofe parts of a common cow-calf, and compared them together, and found them exactly alike. This made us regret that the animal had not lived long enough for us to fee if it would breed; for the conftruction of the parts being to appearance perfect, is not fufficient of itfelf to ftamp it a true or perfect female; for I can fuppofe that the parts being perfectly formed, but without the power of propagation, may conftitute the moft fimple kind of hermaphrodite. It is however moft probable that this was a perfect female, which is an exception to the common rule; and if there are fuch deviations as twins being perfect male and female, why not fuppofe, on the other hand, that an hermaphrodite may be produced fingly, as in other animals; and I am the more inclined to this opinion, from finding a number of hermaphrodites among black cattle, without the circumftances of their birth being afcertained.

Hermaphrodites are to be met with in fheep; but from the account given of them, I fhould fuppofe that they are not free martins. I have feen feveral of them which appeared to be imperfect males, having the penis terminating in the perinæum; the orifice of which appeared like the bearing in the female. They are not naturally ftimulated to put themfelves in the pofition of the female when they void their water, fo that when they pafs it they wet all the furrounding parts, which being covered with wool, retains the urine, keeps them continually wet, and gives them a ftrong fmell. They are mentioned as both male and female, which is not reconcileable to the account given of the free martin.

I believe it had never been even conjectured, notwithftanding all thofe peculiarities, what was the true nature of the free martin.

From the fingularity of the animal, and the account of its production, I was almoft ready to fuppofe the whole a vulgar error; yet from the univerfality of the teftimony in its favour, it appeared to have fome foundation; and therefore I eagerly fought for an opportunity to fee and examine one. Since when I have fucceeded in this inquiry, and have feen feveral; the firft of which was one belonging to John Arbuthnot, Efq. of Mitcham, which was calved in his own farm. He was fo obliging as to allow me to fatisfy myfelf, by permitting me firft to have a drawing made of the

 animal

animal while alive, which was executed by Mr. Gilpin; and after death to examine the parts. When the drawing was made of Mr. Arbuthnot's free martin, John Wells, Efq. of Bickley Farm, near Bromley in Kent, was prefent, 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 defired Mr. Arbuthnot to fpeak to Mr. Wells to keep them, or let me buy them of him; but from his great defire for natural knowledge he very readily preferved both till the bull fhewed all the figns of a good bull; and when the free martin was killed he allowed me to infpect the parts.

Of all the free martins which I have diffected, I fhall only give the defcriptions of three, which point out moft diftinctly the complete free martin, with the gradations towards the male and female.

THE DESCRIPTION OF THE THREE FREE MARTINS.

Mr. Arbuthnot's free martin.[a]

The external parts were rather fmaller than in the cow. The vagina paffed on, as in the cow, to the opening of the urethra, and then it began to contract into a fmall canal, which paffed on to the divifion of the uterus into the two horns, each horn paffed along the edge of the broad ligament laterally towards the ovaria.

At the termination of thofe horns were placed both the ovaria and the tefticles; both were nearly of the fame fize, which was about as large as a fmall nutmeg.

To the ovaria I could not find any Fallopian tube.

To the tefticles were vafa deferentia, but they were imperfect. The left one did not come near the tefticle; the right only came clofe 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.

[a] This animal was feven years old, had been often yoked with the oxen; at other times went with the cows and bull, but never fhewed any defires for either the one or the other.

On

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 deſerving the name of hermaphrodite than the two following; for it had a mixture of all the parts, although all were imperfect.

Mr. Wright's free martin, five years old.

This animal had more the appearance and general character of the ox, or ſpayed heifer, than 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 thoſe bodies teſticles, are the following. Firſt, they were more than twenty times larger than the ovaria of the cow, and nearly the ſize of the teſticles of the bull, particularly thoſe of the ridgill, or bull whoſe teſticles never come down. Secondly, the ſpermatic arteries were ſimilar to thoſe of the bull, eſpecially of the ridgill. Thirdly, the cremaſter muſcle paſſed up from the rings of the abdominal muſcles to the teſticles, as it does in the ridgill.[a]

There were the two bags placed behind the bladder, between it 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

[a] Although I call theſe bodies teſticles for the reaſon given, yet they were only imitations of ſuch; 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.

cow;

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 was different in the cow part, in having nothing ſimilar to the ovaria; nor was the uterus a cavity.

Mr. Well's free martin.

This animal was never ſeen to ſhow any ſigns of a deſire for the male, although it went conſtantly with one. It looked more like an heifer than what they commonly do; but as it was only between three and four years old when killed, it is very probable that it was not ſufficiently old to have taken the characters of the ox; however this may be owing to another circumſtance that will be mentioned hereafter.

The teats and udder were ſmall compared with thoſe of an heifer, but rather larger than in either of the former; the beginning of the vagina was ſimilar to that of the cow, but ſoon obliterated beyond the opening of the urethra, as in the laſt deſcribed. The vagina and uterus to external appearance was 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 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 it and the vagina, were the bags called veſiculæ ſeminales; between which were the terminations of the two vaſa deferentia.

The ducts of the bags and the vaſa deferentia opened as in the former.

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

The

The female parts were imperfect, and there was the addition of part of the vafa deferentia, and the bags called veficulæ feminales.

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

PLATE

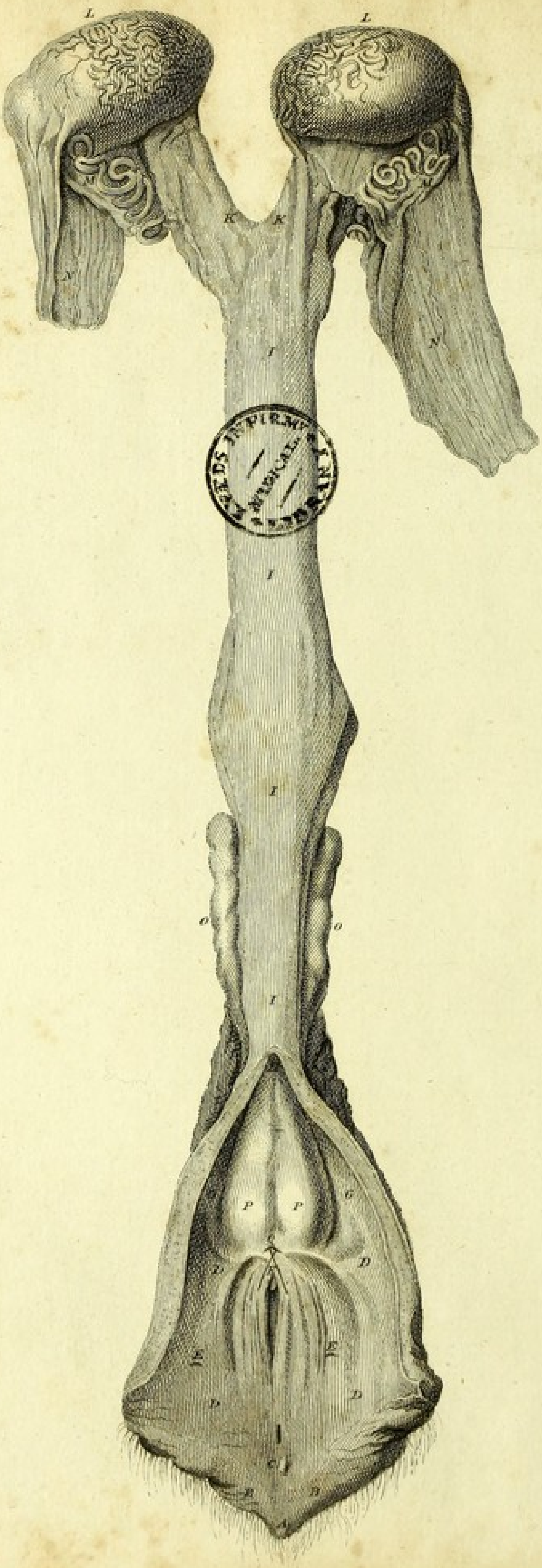
Plate I.

P L A T E I.

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 ſeminales.
Q Q The ducts of veſiculæ ſeminales ſeen through the vagina.
R Points to the openings of the vaſa deferentia and veſiculæ ſeminales.

Plate II.



Wm. Bell del.

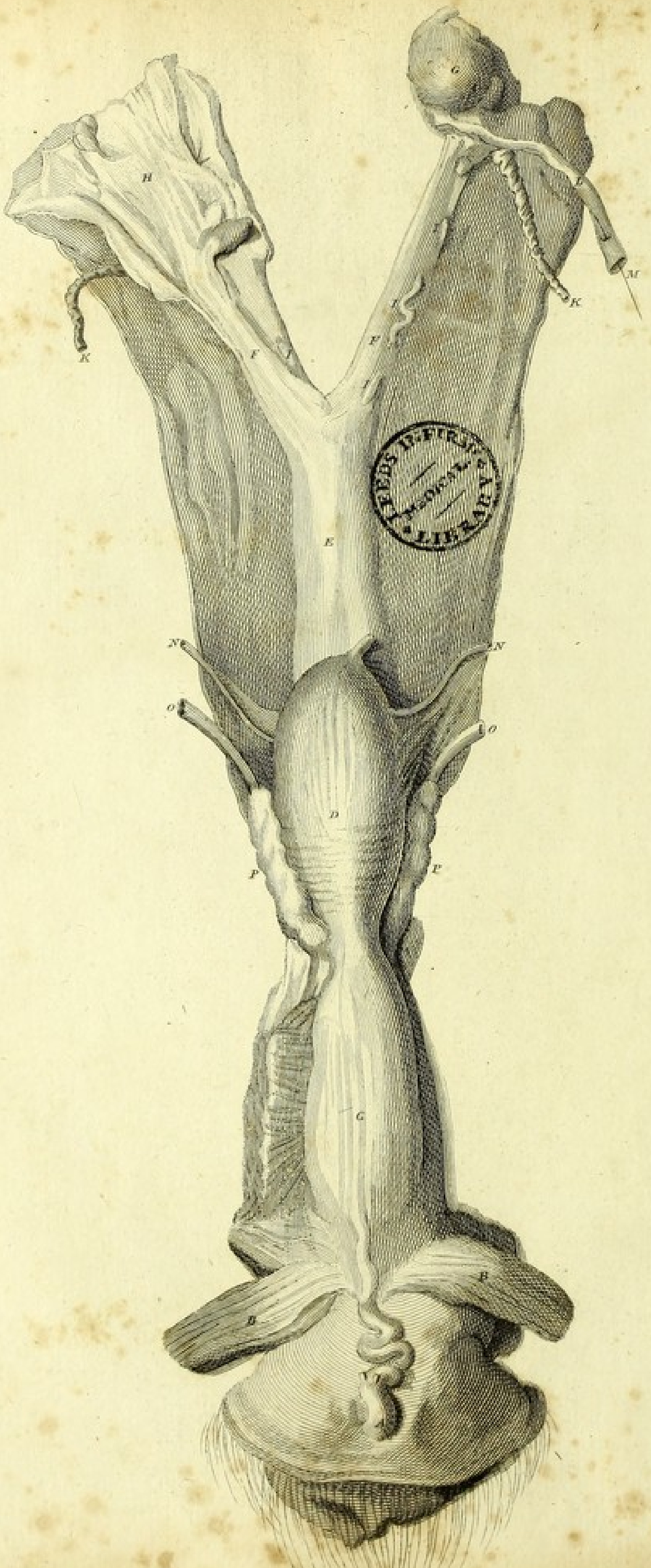
Wm. Skelton sculpt.

PLATE II.

THIS plate reprefents the organs of generation of Mr. Wright's free martin, which are more the parts of a bull than thofe 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.
B B The labia.
C The glans clitoridis.
D D D D The inner furface of the common vagina.
E E The orifices of the ducts of two glands
F Meatus urinarius.
G G The inner furface 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 tefticles.
M M The fpermatic veffels.
N N The cremafter mufcles.
O O The veficulæ feminales.
P P The ducts of the veficulæ feminales feen through the vagina.
Q Points to the ducts of ditto, into which are introduced briftles.

Plate III.



Wm. Skelton sculp.

PLATE III.

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 right 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 ſeminales.

PLATE III.

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 appearance.

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

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 found to be infinite. 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 extraordinary formation. Nor does this appear to be a matter of mere chance; for it may be obſerved, that every one 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 ſupernumerary parts of the ſame kinds, and nearly the ſame ſort of defects; but every part is not perhaps ſubject to a great variety of forms, each part of each ſpecies having its monſtrous form, as it were, originally impreſſed on it by the hand of nature.

It is well known, that there are many orders of animals which have the two parts deſigned for the purpoſe of generation different in the ſame ſpecies, by which they are diſtinguiſhed into male and female: but this is not the only mark of diſtinction in many genera of animals; in the greateſt part the male being diſtinguiſhed from the female by various marks. The differences which are found in the parts of generation themſelves, I ſhall call the firſt, or principle; and all others depending upon theſe I ſhall call ſecondary. The firſt belong equally to both; but the ſecondary will be found 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 to another, which part is that moſt immediately employed in fighting. This difference in external form is more particularly ſtriking in the animals whoſe females are of a peaceable

nature;

nature; ſuch are the greateſt number of thoſe which feed on vegetables, and the marks to diſcriminate the ſexes are in them very numerous. As the males of almoſt every claſs of animals are probably diſpoſed to fight, they are, as I have obſerved, ſtronger than the females. In many there are parts ſolely deſtined for that purpoſe, as the ſpurs in the cock, and the horns in the bull; on which 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 it requires ſtrength to kill, we do not find ſuch a difference in the form of the male and female; very little being diſcernable in that of 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, for inſtance, diſtinguiſhing him from the lioneſs: and the males of ſuch animals as neither fight nor feed on fleſh, are diſtinguiſhable from the female merely 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 thoſe orders of animals where the ſexes are diſtinct, we may not only obſerve the genital organs to be ſubject to a mal-conformation, ſimilar to a mal-conformation in any other part of the animal; but that an attempt is ſometimes made to unite the two parts in one animal body, producing an animal called an unnatural hermaphrodite.

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

The unnatural hermaphrodites appear to be governed by the ſame certain laws by which ſuch extraordinary formation of parts is effected; for it is obſervable, that theſe deviations obtain through a whole ſpecies of animals 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.

We find however, that there is often a change of the ſecondary properties of one ſex into thoſe of another; the female in ſuch reſpects now and

and then aſſuming the peculiarities of the male; and it may 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.

I here beg leave to premiſe, that in animals juſt born, or very young, there are no peculiarities of ſhape to diſtinguiſh one ſex from the other, excluſive of what relates to the organs of generation, and that towards the age of maturity the diſcriminating changes before mentioned appear; the male then loſing that reſemblance he had to the female in various ſecondary properties.[a] 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 the age of maturity. 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 is therefore more ſimilar to 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 a ſnail, or an animal of neither ſex, as the caſtrated male or ſpayed female.

But where the ſexes are ſeparate, and the animals have two characters, neither of them can be called the true one; the true diſtinguiſhing properties being thoſe peculiar to neither ſex, as above mentioned, which are likewiſe found in the monſtrous hermaphrodite. That theſe properties give the diſtinct character of ſuch animals is evident, for the caſtrated male and the ſpayed 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

[a] This is not common to all animals of diſtinct ſexes; for in the fiſh 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.

double ſex, are imitated by depriving the individual animal of its ſex by art, by which means it only preſerves the true properties of the ſpecies.

In ſome animals which have the ſecondary properties we have mentioned peculiar to the two ſexes, there is a deviation from all thoſe general rules, by a change of theſe ſecondary characters; the female perfect with reſpect to the parts of generation, aſſuming more or leſs of the ſecondary character of the male.

This however does not appear to ariſe from any action which takes place at the firſt formation of the animal, or grows up with it, but ſeems to be one of thoſe which is produced at certain periods of life, 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.

It is remarked by thoſe who are converſant with this bird, when wild, that there appears every now and then a hen pheaſant with the feathers of the cock; and all that they have decided 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 of the female peculiar to 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 happened to be then preſent, and was deſired to examine the bird. The following was the reſult of my examination.

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

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 ſhould ſeem probable that this character ariſes from a change at a late period of the animal's life, and does not grow up with it from the beginning

ginning. This lady had for ſome time bred pheaſants, and paid particular attention to them. One of the hens, after having produced ſeveral broods, moulted, and the ſucceeding feathers were thoſe of a cock. This animal was never afterwards impregnated. Hence it is moſt probable, that all thoſe hen pheaſants which are found wild, and have the feathers of a cock, were formerly perfect hens, but have become changed by age, and perhaps by certain conſtitutional circumſtances.

I having bought ſome pheaſants from a dealer in birds, among which were ſeveral hens, perceived that one of the hens, the year after, did not lay, and began to change her feathers; the year following ſhe had nearly thoſe of the cock, but leſs brilliant, eſpecially on the head. It is more than probable this was an old hen, nearly under ſimilar circumſtances 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 appearing like a pyed peacock. In this proceſs the tail, which became like that of a cock, appeared firſt after moulting. In the following year ſhe moulted again, and produced ſimilar feathers. In the third year ſhe did the ſame; then 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, namely, in the winter 1775-6. This bird is now preſerved in the muſeum of Sir Aſhton Lever.[a]

From what has been related of theſe three birds, may it not reaſonably be inferred, that all thoſe wild pheaſants of the female ſex, which are found reſembling the cock, have changed the nature of their feathers at a certain age? This not only obtains in the birds above mentioned, but perhaps to a certain degree in every claſs of animals. We find ſome-

[a] It might be ſuppoſed, that this bird was really a cock which had been changed for a hen; but the following facts put this matter beyond a doubt. Firſt, there was no other pyed pea-fowl in the country. Secondly, the hen had knobs 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.

thing 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 recedes from the female, and aſſumes 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; and even recedes from the original character of the animal, approaching in appearance towards the male, or perhaps more properly towards the hermaphrodite.

ACCOUNT OF THE ORGAN OF HEARING IN FISHES.

NATURAL hiſtory has ever been conſidered as worthy the attention of the curious philoſopher, and therefore 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 alſo that natural hiſtory has not been neglected; all Europe appears to be awake to it. In this iſland it has been purſued with more philoſophic ardour than what was ever known in any country. It has become the ſtudy of 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 purſuit of it, ſearching unknown regions to improve mankind, ſettling correſpondences every where, ſo as to bring in its materials into this country in order to make 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 induced me to publiſh this ſhort account of the Organ of Hearing in Fiſhes; for though the exiſtence of ſuch an organ is now more known, it is ſtill a ſubject of diſpute with many, whether they poſſeſs that ſenſe or not.

Some time before I quitted my anatomical purſuits in the year 1760, and went with the army to Belliſle, I had obſerved 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.[a] My reſearches, in that and in every other part of the animal

[a] I have injected theſe parts in other animals, both with wax and metals, and the bone being afterwards corroded in ſpirit of ſea-ſalt makes elegant caſts of theſe canals.

œconomy,

œconomy, have been continued ever ſince that time; yet I am ſtill inclined to conſider whatever is uncommon in the ſtructure of theſe parts in fiſhes, as only a link in the chain of varieties diſplayed in the formation of this organ of ſenſe, in different animals, deſcending from the moſt perfect to the moſt imperfect, in a regular progreſſion.[a]

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 to be true, or really found it to be ſo, would be very ready to believe the diſcovery their own, and to aſſume all the merit of it to themſelves. My attention was more ſtrongly recalled 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 conceived that it would be only juſtice to myſelf to deliver a ſhort account of that organ to the Royal Society, of which I had made a diſcovery more than twenty years before. I have thought it proper to reprint it here, without adding any thing to what I had before written on the ſubject; reſerving a more compleat inveſtigation of this part of natural hiſtory for a larger work on the 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 chuſe to purſue this branch of the animal œconomy may think it deficient 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 there is in fact nothing more eaſy.

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

[a] 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.

The

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 the organ, as it does in the quadruped and the bird, the organ itſelf being a diſtinct and detached part. In ſome fiſhes the organ is wholly ſurrounded by the parts compoſing the cavity of the ſkull, as in the ray kind.

In others this organ is in part within the ſkull, or that 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 with the animal, for it is nearly in the ſame proportion with that of the animal; 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.

It is much more ſimple in fiſhes than 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 orders of fiſhes; but in all it conſiſts of three curved tubes which unite with one another; this union forms in ſome only one canal, as in the cod, ſalmon, ling, &c. and in others a pretty large cavity, as in the ray kind. In the jack there is an oblong bag, or blind proceſs, which is an addition to thoſe canals, and which 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 appear to anſwer the ſame purpoſe with the cavity in the ray or cartilaginous fiſhes, which is 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 which in ſome fiſhes is cruſted over with a thin bony lamella, ſo as not to allow it to collapſe; for as the ſkull does not form any part of thoſe canals or cavities, they muſt be compoſed of a ſubſtance capable of keeping its form.

Each

Each tube defcribes more than a femi-circle. This refembles in fome refpect what we find in moft other animals, but differs in the parts being diftinct from the fkull.[a]

Two of the femi-circular canals are fimilar to one another, may be called a pair, and are placed perpendicularly; the third is not fo long; in fome it is placed horizontally, uniting as it were the other two at their ends or terminations. In the fkate this is fomewhat different, being only united to one of the perpendicular canals. The two femi-circular canals whofe pofition is perpendicular, are united at one end laterally, forming one canal; at their other extremities they have no connection with each other, but are joined to the terminations of the horizontal one, near its entrance into the common cavity. Near the union of thofe canals into the common, they are fwelled out into round bags, becoming there much larger.

In the ray kind they all terminate in one cavity; and in the cod they terminate in one canal, placed upon the additional cavity or cavities, in which there is a bone or bones. In fome 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 fubftance.[b]

In fome fifhes the external communication, or meatus, enters at the union of the two perpendicular canals. This is the cafe with all the ray kind, the external orifice being fmall, and placed on the upper flat furface of the head; but it is not every genus or fpecies of fifhes that have the external opening.

The nerves of the ear pafs outwards from the brain, and appear to terminate at once on the external furface of the fwelling of the femi-circular tubes above defcribed. They do not appear to pafs through thefe fo as to get on the infide, as is fuppofed to be the cafe in quadrupeds; I fhould therefore very much fufpect, that the lining of the tubes in the quadruped is not nerve, but a kind of internal periofteum.

[a] The turtle and the crocodile have a ftructure fomewhat fimilar to this; and the intention is the fame, for their fkulls make no part of the organ.

[b] This chalky fubftance is alfo found in the ears of amphibious animals.

As

As it is evident that fiſhes poſſeſs the organ of hearing, it becomes unneceſſary to make or relate any experiment made with live 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. Its bottom was level with the ground, and was made by forming a bank all round; with a ſhrubbery cloſe to it. Whilſt I was lying on the bank, obſerving the fiſh ſwimming about, I deſired a gentleman, who was with me, to take a loaded gun and go behind the ſhrubs and fire it. The reaſon for going behind the ſhrubs was, that there might not be the leaſt reflection of light. The inſtant the report was made, the fiſh ſeemed to be all of one mind, for they vaniſhed inſtantaneouſly, raiſing as it were a cloud of mud from the bottom. In about five minutes afterwards they began to appear, and were ſeen ſwimming about as before.

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 ſemi-circular canals in other animals, together with their union into one cavity; and mentions the chalky ſubſtance contained in that cavity and the nerves. But it is by no means clear, that he was acquainted with the external opening which leads to thoſe 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; neither does that which he deſcribes 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 that to be compenſated by the medium in which they live, and by which ſound is conveyed to them being more denſe than that of the air, by which ſound is communicated to

L animals

animals living on the land; and of this idea he is certainly the author. Monſ. Geoffroi cannot indeed be ſaid to have given a perfect account of the organ of hearing in fiſhes, yet on the whole he ſhould be conſidered as a diſcoverer. For though he had only made his obſervations on the ray, as belonging to the claſs of reptiles; yet as it may be properly conſidered of the fiſh kind, he has a juſt claim to the diſcovery. Had I formerly been acquainted with this author's reſearches and pretenſions, I ſhould not have claimed a diſcovery to which I had not a prior right; nor ſhould I have held the diſcovery of the external communication alone, an object of conſequence enough to induce me to diſpute the honour with Monſ. Geoffroi.

In looking over the works of the different authors who have treated of the organ of hearing in fiſhes, I find from a paſſage in Willoughby,[a] who publiſhed prior to Mr. Geoffroi, and indeed is quoted by him, that my claim, even to the diſcovery of the external opening, is not ſo ſtrong as I believed it to be; for he mentions an external orifice in the ſkate, contiguous to what he ſuppoſes the organ of hearing in that fiſh. If what he alludes to is really the external opening of the ear, it gives him a prior claim to the diſcovery of that part of the organ; although from his account, he cannot be ſaid to have been acquainted with the organ itſelf: but as we find in deſcribing the external ear of the thornback that he has evidently miſtaken the noſe for it, of which he gives a tolerable full account, it is very obvious that he was ignorant of the opening into the ear.[b]

Although profeſſor Camper publiſhed an account of the organ of hearing in fiſhes ſo late as 1774, he did not ſeem at that time to have been acquainted with the external opening of the ear in the ray. After giving a deſcription of the organ of hearing in the pike, he makes ſome general obſervations on the ſimilarity of this organ in other fiſhes; but

[a] Willughbeii Hiſtoria Piſcium, Oxonii 1686, lib. iii. cap. viii.

[b] Lib. iii. cap. xiv.

excepts

excepts the ſhark and ray.[a] This exception we might ſuppoſe alluded to the auditory canal; but further on he explains what he 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.[b]

[a] "Il eſt très-vraiſemblable que toutes les autres eſpèces de poiſſons, tant *malacopterygii* 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." Memoires Etrangers de l'Academie des Sciences, 1774, tom. 6, page 190.

[b] "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 oſſeuſe 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 des 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." Memoires Etrangers de l'Acadamie des Sciences, 1774, tom. 6, page 190.

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 diſſections 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 this claſs of animals. Theſe latter obſervations were not however made in conſequence of any regular deſign to inveſtigate this ſubject further. For 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 will 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.

To make this matter more intelligible, I muſt previouſly give an idea of the difference between the particular parts in queſtion, and thoſe of other animals who are not endowed with this property; and firſt, the conſtruction of the lungs, and then of thoſe receptacles.

The

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

It has been afferted that birds have no diaphragm; but this opinion muft have arifen either from a want of obfervation, or from too confined an idea of a diaphragm; for there is a moderately ftrong, but thin and tranfparent, membrane which covers the lower furface of the lungs, adheres to them, and affords infertion to feveral thin mufcles which arife from the inner furfaces of the ribs. The ufe of this part feems to be that of leffening the concavity of the lungs towards the abdomen, at the time of infpiration, and thereby affifting to dilate the air-cells; for which reafon it is to be confidered as anfwering one main purpofe of a diaphragm. Befides this attachment of the lungs to the diaphragm, they are alfo connected to the ribs, and to the fides of the vertebræ.

Thefe adhefions are peculiar to this tribe of animals, and are of fingular ufe, nay in fact are abfolutely neceffary in fuch lungs as thofe of birds; out of which it is intended the air fhould find a paffage into other cavities. For if the lungs were loofe in the cavity of the thorax, as is the cafe in many other animals, thefe cells could not be expanded, either by the depreffion of the diaphragm, or the elevation of the ribs; fince the air rufhing in to fill up the vacuum produced in the cavity of the cheft by thefe actions would take the ftreight road from the trachea through thefe paffages, and of confequence would expand no part of the lungs which lay out of that line, whereby refpiration would be totally prevented, and an effect produced exactly fimilar to what happens in other animals where the lungs are fo much wounded as to allow a free exit to the air at that part.

The cells in birds which receive air from the lungs, are to be found both in the foft parts, and in the bones; and have no communication with the cavity of the common cellular membrane. Some of thefe air-bags are placed in the larger cavities, as the abdomen; and others are fo lodged in the interftices, as about the breaft, axilla, &c. as at firft to give the appearance of the common connecting membrane. Some of them communicate immediately with one another; and all may be faid

to

to have a communication by means of the lungs. They are of very different ſizes, juſt as beſt ſuits 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, have their internal ſubſtance divided into innumerable cells, whilſt others, as the os humeri, and the os femoris, are hollowed out into one large canal, with ſometimes a few bony columns running acroſs at the extremities. Bones of this kind may be diſtinguiſhed from thoſe that do not receive air, by ſeveral marks: 1ſt, By their leſs ſpecific gravity: 2dly, By being leſs vaſcular than the others, and therefore whiter: 3dly, By their containing little or no oil, and conſequently being more eaſily cleaned; and when cleaned appearing much whiter than common bones: 4thly, By having no marrow, or even any bloody pulpy ſubſtance in their cells: 5thly, By not being in general ſo hard and firm as other bones[a]; and 6thly, By the paſſage that allows the air to enter the bones, which can eaſily be perceived. In the recent bone we may readily diſcover holes, or openings, not filled with any ſuch ſoft ſubſtance as blood-veſſels or nerves; and it happens that 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 become emphyſematous.

There are openings in the lungs, by which they tranſmit air to the other parts; and the membrane or diaphragm above mentioned 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 being afterwards continued through the whole of the

[a] 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.

abdomen

abdomen are attached to the back and ſides of that cavity, by which the bags are kept firm 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 ſay that they extend over the whole abdomen.

The lungs at the anterior part, contiguous to the ſternum, open 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 opening of the lungs is into the large cells of a looſe net work, through which the trachea, œſophagus, and large 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 in general is 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 breaſt is united to the parts underneath by means of thoſe cells, which are pretty equally formed; and when the ſkin is removed, the two ſeparated ſurfaces appear as if honey-combed. When theſe 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. The oſtrich however is an exception. In ſome birds they are continued down the wing, where they communicate with the ulna and radius, and in others they go even as far as the pinions.

The poſterior edges of the lungs (which lie on the ſides of the ſpine and project backwards between the ribs) open into the cells of the bodies of the vertebræ, into thoſe of the ribs, the canal of the medulla ſpinalis, 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

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we generally find; though fome birds have more, and fome fewer of thefe communications; for in the oftrich no air gets into the os humeri, but it enters into every other part, as defcribed above, 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 firft bone of the wing, or in the thigh bones. On the other hand, in the pelican the air paffes on to the ulna and radius, and into thofe bones which anfwer to the carpus and metacarpus of quadrupeds.

Thus the cells of the abdomen, thofe furrounding the pericardium, thofe fituated at the lower and forepart of the neck, and in the axilla, thofe in the cellular membrane under the pectoral mufcles, as well as in that which unites the fkin to the body, all communicate with the lungs, and are capable of being filled with air; and again from thefe the cells of the fternum, 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 furnifhed with air.

This fupply of air is not conveyed to the bones folely by means of the lungs; for the cells of the bones of the head in fome birds are likewife filled with it, of which the owl is a remarkable inftance. In this bird the diploe between the two plates of the fkull is cellular and admits a confiderable quantity of air, which is furnifhed by the Euftachian tube.

Some authors confidered the diploe in the cranium of a bird as a continuation of the mamillary procefs,[a] and have looked upon it as a circumftance peculiar to finging birds, but this is not really the cafe. The lower jaw of many birds, but more particularly the pelican, is furnifhed with air, which is fupplied by means of the Euftachian tube.[b]

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[a] The only thing fimilar 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 Euftachian tube.

[b] When I wrote this account to fend it to the Royal Society, I did not then know by what means this was done; for in that I faid, " but by what means I do not know;" that is, I did not know whether it was conveyed by the trachea, where it paffes along the neck, or the Euftachian tube. Profeffor Camper, when he did me the honour to call upon me, was fo

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

Firſt, I made an opening into the belly of a cock, and having introduced a ſilver canula, 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 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 nearly the like ſucceſs. But the paſſage of air through the parts in both, eſpecially in the laſt experiment, was attended with more difficulty than in the firſt; it was indeed ſo great as to render it impoſſible for the animal to live longer than to prove evidently 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; then the whole being put into ſpirit of ſea-ſalt, and corroded, the caſt of injection came out entire.

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

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.

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ſpecific gravity between the external and internal air. This opinion was ſtrengthened, by obſerving that the feathers of birds contain alſo a conſiderable quantity of air and in the very part which requires the greateſt ſtrength: the analogy to which ſeems to hold good between this mechaniſm in birds; and what is diſcoverable in moſt kinds of fiſhes, rather favours this opinion. For theſe 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.[a] But when I found that the oſtrich (which is not intended

[a] When we conſider that the elevating and ſuſpending apparatus is much leſs in fiſhes than in birds, we might 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 a ſhark, which has no air-bag, ſhould be lighter than that of the ſalmon and cod, which have: 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 two ſituations of oil in fiſhes; in one it is diffuſed through the body, as in the ſalmon, herring, &c. In the other it is in the liver, as in all of the ray kind, cod, &c. and thoſe who have it in one part have none in the other. 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

M 2 liver.

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 with air: when I ſaw that the wood-cock, which flies, and is even 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 parts were to be conſidered as an appendage to the lungs; to which I was led by the analogy obſervable in amphibious animals. For in the ſnake, viper, and many others of them, the lungs are continued down through the whole belly, in form of two bags; of which 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 cannot however be ſuppoſed, that the air may 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 great ſimilarity 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 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; for it is in conſequence of this ſtructure that they require to breathe leſs frequently than others. Even conſidering the matter in this

liver of the cod; and in the ſalmon, &c. there is a great deal of oil diffuſed through the whole; therefore I am affraid we are not yet acquainted with the full effect of the air-bladder in fiſhes.

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light, it may ſtill, in birds, have ſome connection with flying, as that motion may eaſily be imagined to render frequency of reſpiration inconvenient, and a reſervoir of air may therefore become ſingularly uſeful. Although we are not to conſider this ſtructure in birds to be an extention of lungs, yet I can eaſily conceive this accumulation of air to be of great uſe in reſpiration; for as we obſerved in the viper, 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.[a] And this opinion will appear not 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 this extenſion of cavity. It is alſo a corroborating circumſtance, that in the fowl the air could have paſſed by a much readier way than through the lungs, into all the cells about the breaſt, neck, axilla, wings, &c. that 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 in 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 extenſion, or diffuſion of air is carried further than in the others; and this opinion is ſtrengthened by comparing this ſtructure with the reſpiratory organs in the flying inſects, which are compoſed of cells diffuſed through the whole body; and theſe are extended even into the head and down the extremities; while there is no ſuch ſtructure in thoſe 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 as to require ſuch an increaſe of body without increaſe of weight?

[a] 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 comes, as well thoſe of the ſoft parts as of the bones, are very little vaſcular.

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How far this conſtruction of the reſpiratory organs may aſſiſt birds in ſinging, is deſerving of notice; 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. Theſe experiments were not originally inſtituted with any expectation of the event which reſulted from them, but for a very different purpoſe; which was no other than to ſatisfy myſelf, whether an animal could retain life after it was frozen, as had been confidently aſſerted both of fiſhes and ſnakes. If I had ſucceeded, I meant to have tried the effects of freezing, on living animals, to a much greater extent than ever can happen accidentally. For that ſnakes and fiſhes, after being frozen, have ſtill retained 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. I mention theſe circumſtances, to account for what might otherwiſe be attributed to negligence and inattention; namely, the little nicety that was uſed in meaſuring the preciſe degree of cold applied in theſe experiments. Accuracy in this particular was not aimed at, as it was 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: then ice was 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. The animal juices, the blood for example,

ample, freeze at 25°; ſo that a piece of dead fleſh could be frozen in ſuch an atmoſphere.

EXPERIMENTS.

I. The firſt experiment was made on two carp. They were put into a glaſs veſſel with common river water, and the veſſel put into the freezing mixture; the water did not freeze faſt enough; and therefore to make it freeze ſooner we put in as much cooled ſnow as to render the whole thick. The ſnow round the carp melted: we put in more freſh ſnow, which melted alſo; and this 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. This was done very gradually; but the animals did not, with flexibility, recover life. While in this cold they 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 pretty deep, that the cold of the atmoſphere round the animal might not be diminiſhed faſt by the warmth of the open air, which would have ſpoiled it as a conductor.

II. The ſecond experiment was upon a dormouſe. The veſſel was 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

fine its heat; but finding that ineffectual, it then endeavoured to make its efcape[a]; its motions became lefs violent by the finking of the vital powers; and its feet were frozen; but we were not able to keep up the cold a fufficient time to freeze the whole animal, its hair being fo bad a conductor of heat, that the confumption was not more than the animal powers were capable of fupporting.[b]

III. The third experiment was made upon another dormoufe. From the failure of the laft experiment I took care that the hair fhould not a fecond time be an obftruction to the fuccefs of our experiment. I therefore firft made it wet all over, that the heat of the animal might be more inftantaneoufly carried off; and then it was put into a leaden veffel. The whole was put into the cold mixture as before. The animal foon gave figns of its feeling the cold, by repeated attempts to make its efcape. The breath, and the evaporating water from its body were foon frozen, and appeared like a hoar-froft on the fides of the veffel, and on its whifkers; but while the vigour of life lafted, it defied the approach of the cold. However, from the hair being wet, and thereby rendered a good conductor of heat, there was a much greater confumption of it than in the former experiment. This haftened on a diminution of the power of producing it. The animal died, and foon became ftiff; upon thawing it, we found it was quite dead.

IV. The fourth experiment was upon a toad. It was put into water juft deep enough not to cover its mouth, and the whole was put into a cold mixture, now between 10° and 15°. It allowed the water to freeze clofe to it, which as it were, clofed it in; but the animal did not die, and therefore was not frozen: however, it hardly ever recovered the ufe of its limbs.

[a] This fhows, that cold carried to a great degree rather roufes the animal into action than depreffes it; but it would appear from many circumftances and obfervations, that a certain degree of cold produces inactivity both in the living and fenfative principle, which will be further illuftrated hereafter.

[b] Thefe experiments were made in prefence of Dr. George Fordyce and Dr. Erwin, teacher of Chymiftry at Glafgow; the latter of whom came in accidentally in the middle of our operations.

V. The fifth experiment was with a ſnail, which froze very ſoon, in a cold between 10° and 13°; but theſe two laſt experiments were made in the winter, when the living powers of thoſe animals are very weak: they might have reſiſted the cold more ſtrongly in the ſummer. Why the animals mentioned in theſe 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, that moſt probably the animals were deprived of life before they were frozen. Secondly, that there was an exertion or expence of animal powers 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. It might alſo perhaps depend in ſome degree on other circumſtances not hitherto obſerved: for from experiment II. and III. upon dormice, I found that in theſe 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. in 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 quick-

ſilver

ſilver to 64°. But to what degree the more imperfect animals are capable of being rendered hotter and colder at one time than another, I have not been able to determine. 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 ages of parts in the ſame animal; for an animal is naturally alike 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 able to ſupport life equally with the old; 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 power of generating heat ſeems to be a property in an animal while alive. It is 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. 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: neither can it be ſaid to depend upon the nervous ſyſtem, for it is found in animals that have no brain or nerves. It is then 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

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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 maſter 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 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 objection 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 the 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 greateſt power of retaining a certain degree of heat, which may be called,

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 them, are incapable of keeping conſtantly to one degree; but may be altered from their ſtandard heat, either by external applications, or diſeaſe. However, 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: we are 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 likewiſe 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 therefore in its different degrees muſt be preſent. 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 are ſubject to variations in their degrees of heat and cold from external applications, they 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,

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 proportionable 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 them. 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, in ſome degree, 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 ths following experiments.

Experiment I. I placed the ball of my 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°; but this being continued, it roſe no higher. I then took ſeveral pieces of ice, about the ſize of walnuts, and put them in the ſame ſituation, allowing them only

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melt in part, that the application of cold might be better kept up, occasionally ſpitting out the water ariſing from the ſolution : this I continued for ten minutes, and 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 to any greater 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.

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 matter was in any degree ſubject to the common laws of matter in this reſpect, the urethra would be readily affected: for this reaſon I got 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; after it had remained there about a minute, the quickſilver roſe only to 92° : at two inches it roſe to 93°; at four inches the quickſilver roſe 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

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: this was repeated ſeveral times with the ſame ſucceſs. As the urethra ſtill appeared to be the part of an animal beſt calculated for experiments of this kind; to find if there was any difference in the quickneſs of the tranſition of heat and cold in living and dead parts, and if the extent to which each would go, was likewiſe different, I procured a dead penis to make the following comparative experiments; being clearly of opinion that all ſuch trials ſhould be as ſimilar as poſſible, excepting 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 having heated the dead one to the ſame degree, I had both immerſed in the ſame veſſel, with the water at 50°, where introducing the thermometers different times, I obſerved the comparative quickneſs with which they cooled from 92°. The dead cooled ſooner by only two or three degrees. The living came down to 58°, and the dead to 50°. After having continued the thermometer there ſome time longer, it fell no lower. I repeated the ſame experiment ſeveral times, with the ſame ſucceſs; although at one time there might be a ſmall difference in the degrees of heat from that of another, the heat of the water alſo differing; 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 we find very little difference between the cooling of a part of a dead and of a living body; but we cannot ſuppoſe this to take place uniformly through the whole body, as in that caſe a living man would always be of the ſame degree of heat with the aſmoſphere in which he lives. The man not chooſing to be cooled lower than 53° or 54°, prevented my ſeeing if the powers of generating heat were exerted in a higher 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.

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From the experiments related I found, that parts of an animal were capable of being reduced below the common or natural heat; I therefore made other experiments, with a view to fee whether the fame parts were capable of becoming much hotter than the ftandard heat of animals. The experiments were made in the fame 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 immerfed for two minutes in water heated to 113°, and the thermometer being introduced as before, the quickfilver rofe to 100° and a half. This experiment I alfo repeated feveral times, but could not raife the heat of the penis beyond 100° and a half: this was probably owing to the perfon not being able at this time to bear the application of water warmer than 113°. As thefe were only fingle experiments, I made a comparative one with the dead part.

Experiment VI. Both the living and dead part being immerfed in water, gradually made warmer and warmer from 100° to 118°, and continued in this heat for fome minutes, the dead part raifed the thermometer to 114°, while the living raifed it no higher than 102° and a quarter. It was obferved, by the perfon 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 fo hot that he could hardly bear it. Upon applying the thermometer to the fides of the living glans, the quickfilver immediately fell from 118° to about 104°, while it did not fall more than a degree when put clofe to the dead; fo that the living glans cooled the furrounding water to a certain diftance.[a]

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

In the fecond, third, fourth, fifth, and fixth experiments, an internal cavity, which is both very vafcular and fenfible, was evidently influenced

[a] This might furnifh an ufeful hint refpecting bathing in water, whether colder or warmer than the heat of the body: for if intended to be either colder or hotter, it will foon be of the fame temperature with that of the body; therefore in a large bath, the patient fhould move from place to place; and in a fmall one, there fhould be a conftant fucceffion of water of the intended heat.

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

Experiment VIII. In a room, in which the air was at between 50° and 60° of temperature, a ſmall opening was made in the belly of a dormouſe, of a ſufficient ſize to admit the ball of my thermometr, 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 again introduced, the quickſilver 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 was at the bottom of the diſh, and almoſt frozen; the whole of the animal was numbed, and a good deal weakened. When the thermometer was introduced, the heat varied in different parts of the belly; in the pelvis, near the parts moſt expoſed to the cold, it was as low as 62°;

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in the middle, among the inteftines, about 70°; but near the diaphragm it rofe to 80°, 82°, 84°, and 85°; fo that in the middle of the body the heat had decreafed 10°. Finding a variation in different parts of the fame cavity in the fame animal, I repeated the fame experiments upon another dormoufe.

Experiment XII. I took a healthy dormoufe, which had been afleep from the coldnefs of the atmofphere, and brought it into a room in which there was a fire (the atmofphere at 64°); I put the thermometer into its belly, nearly at the middle, between the thorax and pubis, and the quickfilver rofe to 74° or 75°; when I turned the ball towards the diaphragm, it rofe to 80°; and when I applied it to the liver, it rofe to 81° and a half.

Experiment XIII. The moufe was put into an atmofphere at 20°. and left there half an hour; when taken out it was very lively, much more fo than when put in. I introduced the thermometer into the lower part of the belly, and it rofe to 91°; and upon turning it up to the liver, to 93°.

Experiment XIV. The animal was put back into the cold atmofphere at 30° for an hour, when the thermometer was again introduced into the belly; at the liver it rofe to 93°; in the pelvis to 92°; it was ftill very lively.

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

Experiment XVI. It was put into its cage, and two hours after the thermometer, placed at the diaphragm, was at 93°.

As I was unable to procure hedge-hogs in the torpid ftate, to afcertain their heat during that period, I got my friend, Mr. Jenner, furgeon, at Berkley, to make the fame experiments on that animal, that I might compare them with thofe in the dormoufe; and his account is as follows.

" Experiment I. In the winter the atmofphere 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 atmofphere 26°, the heat of a torpid hedge-hog, in the cavity of the abdomen, was reduced fo low as 30°.

Experiment III. The hedge-hog was expofed to the cold atmofphere of 26° for two days; and the heat of the rectum was found to be 93°; the wound in the abdomen being now fo fmall that it would not admit the thermometer.

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

In fummer the atmofphere 78°, the heat of the hedge-hog, in an active ftate, in the cavity of the abdomen, towards the pelvis, was 95°; at the diaphragm 97°."

We find from thefe experiments, that the heat of an animal is increafed under the circumftances of cold, whenever there are actions to be carried on for which heat is neceffary.

In the experiments on the firft dormoufe, the heat of the animal was 80°; which is below the ftandard heat of the actions of that animal; and after being put into the cold mixture, its heat was raifed to 85°. In the fecond dormoufe the heat was raifed by repeated experiments from 75° to 93°. A queftion naturally occurs here, was the increafe of heat owing to the animals being put into a cold atmofphere, and therefore generated to refift the cold? Or was it owing to a wound having been made into the cavity of the abdomen, which required an exertion of the animal powers to repair the injury; and which actions could not take place without the neceffary degree of heat? That it was in confequence of the wound, appears evident from the experiment made upon the fecond hedge-hog; for in an atmofphere of 26° of heat, it was in a very torpid ftate, and did not raife the thermometer higher than 30°; but after being put back into the cold, and kept there for two days, its heat in the rectum was 93°; and fo far from being torpid, it was lively, and the bed in which it lay felt warm.

As this animal allowed its heat to come fo low as 30°, when there was no neceffary action to take place, the increafed heat cannot be attributed to the effects of cold; but muft be referred to the wound made into the belly, that called forth the powers of the animal to repair an

injury

injury which they could not affect in a degree of heat below the ſtandard heat of the animal; and this ſtimulus of neceſſity for action cauſed an exertion of the powers of generating heat, even in a degree of cold that would have otherways brought the animal ſo low as 30°.

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 upon the principle of ſleep). But I ſhould very much ſuſpect, that ſleep, ſimply conſidered, is out of the queſtion, as ſleep is an effect that takes place in all degrees of heat and cold. In thoſe animals where the voluntary actions are ſuſpended by cold, it appears to produce the effect by acting in a certain degree as a ſedative, in conſequence of which the animal faculties are proportionably weakened, but 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 every order has its degree of cold by which the voluntary actions muſt be ſuſpended.

When a man is aſleep, he is colder than when awake; and I find, in general, that the difference is about one degree and an 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 is 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 animals.

To ſee how far the reſult of theſe experiments upon dormice was peculiar to them, I wiſhed to repeat the ſame experiments upon common mice; for which purpoſe I procured two, one ſtrong and vigorous, the other weakened by faſting.

Experiment XVII. The common atmoſphere being at 60°, I introduced the thermometer into the abdomen of the ſtrong mouſe; the ball being

being at the diaphragm, the quickfilver was raifed to 99°, but at the pelvis only to 96° and three quarters.

Here there was a real difference of about 9° in two animals of the fame fize, in fome degree of the fame genus, and at the fame feafon of the year, and the atmofphere of nearly the fame temperature.

Experiment XVIII. The fame moufe was put into a cold atmofphere of 13°, for an hour, and then the thermometer was introduced as before; the quickfilver at the diaphragm was raifed to 83°, in the pelvis only to 78°.

Here the real heat of the animal was diminifhed 16° at the diaphragm, and 18° in the pelvis.

Experiment XIX. In order to determine whether an animal that is weakened has the fame powers, with refpect to preferving heat and cold, as one that is vigorous and ftrong, I introduced the ball of the thermometer into the belly of the weak moufe; the ball being at the diaphragm, the quickfilver rofe to 97°; in the pelvis to 95°: the moufe being put into an atmofphere as cold as the other, and the thermometer again introduced, the quickfilver ftood at 79° at the diaphragm, and at 74° in the pelvis.

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

Here was a diminution of heat in the fecond greater than in the firft, we may fuppofe proportional to the decreafed power of the animal, arifing from want of food.

To determine how far different parts of other animals than thofe 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 quickfilver rofe to 100° and a half exactly. The cheft 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 quickfilver rofe to 101° exactly. A wound was next made fome way into the fubftance of the liver; and the ball being introduced, the quickfilver rofe to 100° and three quarters. It was next introduced into the

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 the experiments on mice, and thoſe 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 imperfections, I made the following experiments upon fowls, which I conſidered to be one remove below what are commonly called quadrupeds.

Experiment XXIII. I introduced the ball of the thermometer ſucceſſively 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.

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It may be aſked, whether the increaſe of three or four degrees of heat, which is found in the fowl more than in the quadruped, is for the purpoſe of incubation? We found that the heat in the eggs, which was cauſed and ſupported by the heat of the fowls, was not above the ſtandard of the quadrupeds; and that 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 is thoſe commonly called amphibious.

Experiment XXVIII. I took a healthy viper, and introduced the thermometer into its ſtomach, and afterwards into its anus; the quickſilver roſe from 58° (the heat of the atmoſphere in which it was) to 68°; ſo that it was 10° warmer than the common atmoſphere.

Experiment XXIX. The viper being put into a pan, and the pan into a cold mixture of about 10°; after remaining there about ten minutes, its heat was 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 XXX. 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

Experiment XXXI. 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 introduced the thermometer into its ſtomach; the quickſilver roſe to 69°; ſo that the difference between the water and the fiſh was only 3° and a half.

Experiment XXXII. 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 alive the next day.

Experiment XXXIII. 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 XXXIV. 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 the imperfect claſſes of animals will bear to have their heat reduced to that point at which the ſolids and fluids freeze when dead, but if much below it, death muſt be the conſequence, I wiſhed to determine to what degree of heat the animal could be raiſed.

Experiment XXXV. A healthy viper was put into an atmoſphere of 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 heat. The ſame experiment was made upon frogs with nearly the ſame reſult.

Experiment XXXVI. 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 of the ſame degree of heat with the water.

Experiment XXXVII. 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 theſe inferior claſſes of animals, I made comparative trials between living and dead ones.

Experiment XXXVIII. 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 I conceived, that the ſame principle exiſted in animal ſubſtances, devoid of apparent organization and motion, where the power of preſervation was ſimply required.

I was led to this notion twenty years ago, when I was 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 part of the albumen, which is not 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 how far eggs would ſtand other teſts of a living principle, I made the following experiments.

Experiment XXXIX. Having put an egg into a cold about 0, which froze it, I then allowed it to thaw; from this proceſs I imagined that the preſerving powers of the egg muſt be deſtroyed. I next put this egg into the cold mixture, and with it one newly laid; and the difference in freezing was ſeven minutes and a half, the freſh one taking ſo much longer time in freezing.

Experiment XL. A new laid egg was put into a cold atmoſphere, fluctuating between 17° and 15°; it took above half an hour to freeze;

but

but when thawed and put into an atmoſphere at 25°, it froze in half the time. This experiment was repeated ſeveral times with nearly the the ſame reſult.

To determine the comparative 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 XLI. 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 after the dead one, it roſe to 32°, and began to ſwell and freeze.

The reſult of this experiment upon the freſh egg was ſimilar to the above experiments upon the frog, eel, ſnail, &c. where life allowed the heat to be diminiſhed two or three degrees below the freezing point, and then reſiſted all further decreaſe; but the powers of life were expended by this exertion, and then 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,[a] and propagating the ſpecies, both which, eſpecially the laſt, require the greateſt powers an animal can exert. The animals which we call imperfect being commonly 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

[a] How far this idea holds good with fiſhes I am not certain.

as to call forth the powers neceffary for the propagation of the fpecies.[a]

Therefore, whenever thefe imperfect animals are expofed to a cold fo great as to weaken their powers, and difable them from performing the firft of thefe fecondary actions, they in fome meafure ceafe to be voluntary agents, and remain in a torpid ftate during that degree of cold which always occurs during fome part of the winter in fuch countries as they inhabit; and the food of fuch animals not being in general produced in the cold feafon, is a reafon why this torpidity becomes in fome meafure neceffary.

From this circumftance of the heat of fuch animals being allowed to fink to the freezing point, or fomewhat lower, and then becoming ftationary; and the animal not being able to fupport life in a much greater degree of cold for any length of time, we fee a reafon why they always endeavour to procure places of abode in the winter where the cold feldom finks to that point. Thus we find toads burrowing, frogs living under large ftones, fnails feeking fhelter under ftones and in holes, and fifhes having recourfe to deep water; the heat of all which places is generally above the freezing point in our hardeft frofts; which however are fometimes fo fevere, as to kill many whofe habitations are not well chofen.

[a] The hedge-hog may be called a truly torpid animal, and we find that its heat is diminifhed when the actions are not vigorous. From a general review of this whole fubject it would appear, that a certain degree of heat in the animal is neceffary for its various œconomical operations, among which is digeftion; and that neceffary heat will be according to the nature of the animal, and probably the nature of the neceffary operations to be performed. A frog will digeft 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, dormoufe, bat, &c. is reduced to 70°, 75°, or 80°, they lofe their power of digeftion; or rather that the body, in fuch a degree of cold, has no call upon the ftomach. That animals, in a certain degree of heat, muft always have food, is further illuftrated by the inftance of bees. The conftruction of a bee is very fimilar to a fly, a wafp, &c. A fly and a wafp can allow their heat to diminifh as in the fifh, fnake, &c. without lofing life, but a bee cannot; therefore a bee is obliged to keep up its heat as high as what we call its digeftive heat, but not its propagating; for which purpofe they provide againft fuch cold as would deprive them even of their digeftive heat, if they had not food to preferve it.

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, and the water freezes: the advantage ariſing from theſe two circumſtances are great; the ſnow ſerving as a blanket to the earth, and the ice to the water.[a]

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 made upon whole ones, and as I never ſaw life return by thawing,[b] I wiſhed to ſee how far parts were ſimilar to the whole in this reſpect; eſpecially as it is aſſerted, and with ſome authority, that parts of a man may be frozen, and afterwards recover: for this purpoſe I made 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; on the ſide of the veſſel was a hole, through which I introduced the ear of a rabbit. 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

[a] 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; ſo that 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 came on, the ſurface of the ground was frozen; but a conſiderable fall of ſnow fell, and continued ſeveral weeks; the atmoſphere at this time was often at 15°, but it was allowed to affect the ſurface of the earth ſo inconſiderably, that the ſurface of the ground thawed, and the earth retained the heat of 34°, in which beans and peas 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 fell and covered the ice; the heat of the water was preſerved and thawed the ice, 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 worthy of the attention of the philoſopher, to inveſtigate the cauſe of the heat of the earth, upon what principle it is preſerved, &c.

[b] Vide Phil. Tranſ. for the year 1775, vol. LXV. part II. p. 446.

of

of the ear projecting into the veffel became ftiff, and when cut did not bleed; and the part cut off by a pair of fciffars, flew from between the blades like a hard chip.

The ear remained in the mixture nearly an hour: when taken out it foon thawed, and began to bleed, and became very flaccid, fo as to double upon itfelf, having loft its natural elafticity. When out of the mixture nearly an hour, it became warm, and this warmth increafed to a confiderable degree, and alfo began to thicken, in confequence of inflammation, while the other ear continued in its ufual cold. The day following the frozen ear was ftill warm; and two days after it retained its heat and thicknefs, which continued for many days after.

About a week after this, the mixture being the fame as the former, I introduced both ears of the fame rabbit through the hole, and froze them both: the found one however froze firft, probably from its being confiderably colder at the beginning. When withdrawn they foon thawed, and both foon became warm, and the frefh ear thickened as the other had done before.

Thefe changes in the parts do not always fo quickly take place; for on repeating thefe experiments on the ear of another rabbit till it became as hard as a board, it was longer in thawing than in the former experiment, and much longer before it became warm; however in about two hours it became a little warm, and the day following it was very warm and thickened.

In the fpring 1776, I obferved that the cocks I had in the country had their combs fmooth with an even edge, and not fo broad as formerly, appearing as if near one half of them had been cut off. Having inquired into the caufe of this, my fervant told me, that it had been common in that winter during the hard froft. He obferved, that they had become in part dead, and at laft dropped off: alfo, that the comb of one cock had dropped entirely off; this I did not fee, as by accident he burnt himfelf to death. I naturally imputed this effect to the combs having been frozen in the time of the fevere froft; and having, confequently, loft the life of that part by this operation. I endeavoured to try the folidity of this reafoning by experiment.

I attempted

I attempted to freeze the comb of a very large young cock (which was of a confiderable breadth) but could only freeze the ferrated edges (which proceffes were full half an inch long) ; the comb itfelf being very thick and warm refifted the cold. The frozen parts became white and hard; and when I cut off a little bit it did not bleed, nor did the animal fhow any figns of pain. I next introduced into the cold mixture one of his wattles, which was very broad and thin; it froze very readily: upon thawing both the comb and wattle, they became warm, but were of a purple colour, having loft the tranfparency 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 firft neareft to the found parts, increafing gradually till the whole had acquired a healthy appearance.

There was a very material difference in the effect between thofe fowls, the ferrated edges of whofe combs I fufpected to have been frozen in the winter of 1765-6, for they muft have dropped off. The only way in which I can account for this difference is, that in thofe fowls the parts were kept fo long frozen, that the unfrozen or active parts had time to inflame, and had brought about a feparation of the frozen parts, treating them exactly as dead, fimilar to a mortified part; and that before they thawed, the feparation was fo far compleated as to deprive them of further fupport.

As it is confidently afferted, that fifhes are often frozen and come to life again, and as I had never fucceeded in any of my experiments of this kind upon whole fifhes, I made fome partial experiments upon this clafs of animals; being led to do this by having found a material difference in the refult of the experiments made upon the whole animal, and of thofe made only 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 it 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

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, and they had almoſt loſt the power of motion; at laſt they died. The water in which they were kept was New River water, ſhifted every day, and about ten gallons in quantity.

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.

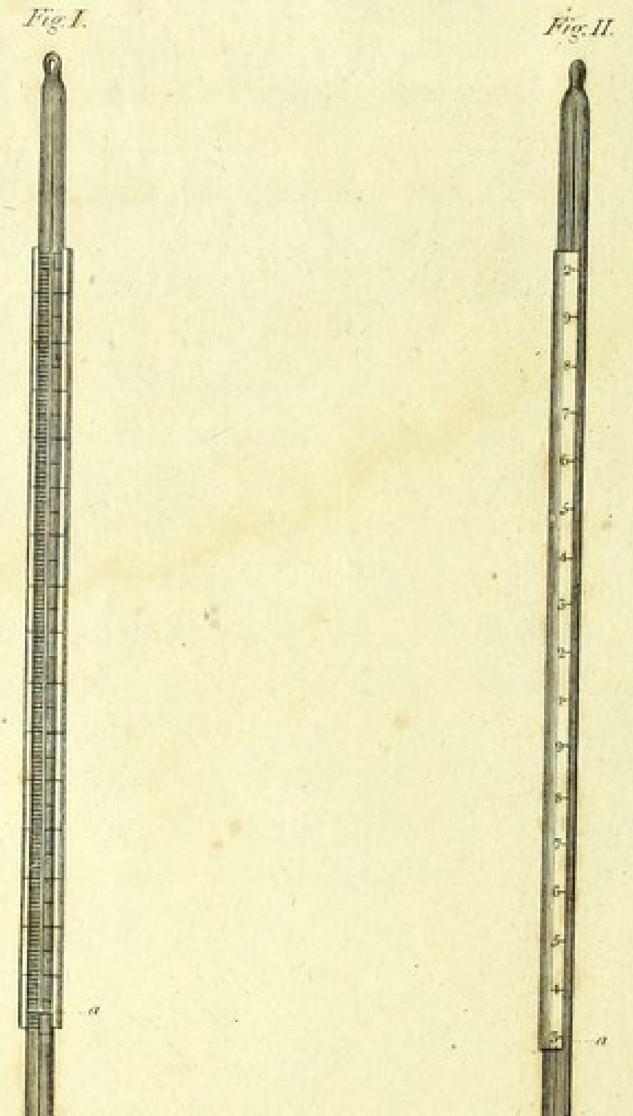
I then froze the anterior half of another earth-worm; but the whole died.

I next froze the poſterior half of an earth-worm; the anterior half lived, 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°; and 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.

EXPLANATION

p. 113.



EXPLANATION OF THE PLATE.

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 with his requeſt, 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 favourite buſineſs and 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 practice is new, and has furniſhed as yet few important and clear facts. If we judge of the queſtion by our general knowledge of the animal œconomy, I am afraid that it is ſo imperfectly underſtood, that our reaſoning from it alone cannot be relied on: neverthelefs, on a ſubject ſo intereſting to humanity, we muſt not be idle; we muſt throw out our obſervations, and reaſon as well as we can from the few data we have, in hopes that the ſubject, thus put fairly into the hands of the public, may in time, by their united endeavours, become 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.[a] This,

[a] 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; the animal becomes relaxed, in which ſtate it naturally comes out of the ſhell, and the body appears large, giving the full ſize of the animal, but without any motion; all its actions being ſuſpended, and continue to be ſo till either the cauſe of the ſuſpenſion is removed, or ſome other ſtimulus ſhall bring the parts into action; but in this ſtate life cannot be preſerved for any conſiderable length of time; and when the ſtimulus of death takes place, the whole animal is thrown into action, in which contracted ſtate abſolute death is produced. A ſtate of relaxation ſhould therefore (in caſes where an univerſal violence has not been committed) be conſidered as a criterion of life; and even in them it ſhould be for ſome time admitted as a probable reaſon for ſuppoſing life ſtill to exiſt.

probably, is the caſe in the beginning of all violent deaths, except thoſe cauſed by lightning, electricity, an univerſal ſhock, by which abſolute death may be inſtantaneouſly produced; or a blow on the ſtomach which appears to act in the ſame way, producing abſolute death immediately: for in all thoſe 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 without any rigor of muſcles; 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 this viſcus and every part of the body, at leaſt with vital parts; the blow moſt probably producing inſtant death in that organ, in conſequence of which the whole animal dies.[a]

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 powers, though deprived of the action of life, the cauſe of that privation may frequently be removed; but, when the powers of life are 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.[b] The laſt propoſition I have to eſtabliſh is, that the ſtomach ſympathizes with every part of an

[a] 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.

[b] 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 reſults of many obſervations and experiments. But it may be thought neceſſary here to give a definition of what I call the living principle: ſo far 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.

animal,

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. This ſympathy is ſtrongeſt with the vital parts. Beſides this univerſal ſympathy between the ſtomach and all parts of the body, there are peculiar ſympathies; for inſtance, the heart ſympathizes immediately with the lungs. 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, 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, probably 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 on the brain or ſpinal marrow, ſufficient life remaining in the ſolids, if actions 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; and, as I believe, happens by a blow on the ſtomach; in all which caſes the muſcles remain flexible.[a]

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

[a] 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.

have

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.

The preſent conſideration is, which of the kinds of violent death drowning comes under? I ſuppoſe it moſt commonly comes under the firſt; and upon that ground I ſhall principally conſider the ſubject.

The loſs of motion in drowning ſeems to ariſe from the loſs of reſpiration, and the immediate effects which this has upon the other vital motions of the animal: except what may have ariſen from the affections of the mind; however, the privation of breathing appears 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 a ſufficiency of life ſtill exiſts to produce 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 principle effect depends upon air being thrown into the lungs, is, what happens in the birth of children, when too much time has been ſpent after the interruption of that life which is peculiar to the fœtus; they then loſe altogether the diſpoſition for the 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 die, if air were not thrown into its lungs, and by this 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,

before, only the frequency of its action was confiderably increafed. I then ftopped the motion of the bellows, and the heart became gradually weaker and lefs frequent in its contraction, till it left off moving altogether: by renewing my operation, the heart begun again to move, at firft very faintly, and with longer intermiffions; but by continuing the artificial breathing its motion became as frequent and as ftrong as before. This procefs I repeated upon the fame dog ten times, fometimes ftopping for five, eight, or ten minutes. I obferved that every time I left off working the bellows, the heart became extremely turgid with blood, and the blood in the left fide became as dark as that in the right; which was not the cafe when the bellows were working. Thefe fituations of the animal appear to me exactly fimilar to drowning.

The lofs of life in drowned perfons has been accounted for, by fuppofing that the blood rendered unfit by want of the action of the air in refpiration, is fent in that vitiated ftate to the brain and other vital parts, by which means the nerves lofe their effect upon the heart, and the heart in confequence its motion. This however I am fully convinced is falfe: firft, from the experiments on the dog, in whofe cafe a large column of bad blood, viz. all that was contained in the heart and pulmonary veins was pufhed forward without any ill effect being produced; and next, from the recovery of drowned perfons and ftill-born children, which, under fuch circumftances, never could happen, unlefs a change of the blood could take place in the brain, prior to the reftoration of the heart's motion: thefore the heart's motion muft depend immediately upon the application of fuch air to the lungs, and not upon the effects which air has upon the blood, and which that blood has upon the vital parts. Thefe are only fecondary operations in the animal œconomy. However, if the affections of the mind have had any fhare in the ceffation of actions in the heart, that will not be fo eafily reftored as it would otherwife be: therefore in our attempts to recover perfons drowned, it might be proper to inquire if there had been time fufficient for the perfon to form any idea of his fituation, previous to his being plunged into the water. It is more than probable, in fuch a cafe, that the agitated ftate of mind might affift in killing him; and I fhould very much doubt the probability of recovering

fuch

ſuch a perſon. In the hiſtory of thoſe who have, and who have not been recovered, could the difference be aſſigned to any ſuch cauſe, it might lead to ſomething uſeful.

It frequently happens in the caſe of drowning, that aſſiſtance cannot be procured till a conſiderable time after the accident; every moment of which 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.[a] But if a conſiderable time, ſuch as an hour, has been loſt, this will ſeldom be ſufficient; the heart, in all probability, having by this time loſt its nice connection with the lungs. It will therefore be proper to apply ſuch ſtimulating medicines, as the vapour of volatile alkali, mixed with the air; 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 are known to rouſe the living principle and put the muſcles of reſpiration into action, and are therefore likely to excite the action of the heart: Beſides affections of theſe nerves more immediately affect the living principle, for while a ſtrong ſmell of very ſweet flowers, as orange flowers, ſhall in many cauſe fainting, the application of vinegar will immediately reſtore the powers to action again. All perfumes in which there is ſome acid, rather rouſes than depreſſes, as the ſweet-brier, eſſence of lemon, &c. If during

[a] 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.

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the operation of the bellows, the larynx be gently preffed againft the œfophagus and fpine, it will prevent the ftomach and inteftines being too much diftended by the air, and leave room for the application of more effectual ftimuli to thofe parts. This preffure however muft be conducted with judgment and caution, fo that the trachea and the aperture into the larynx may both be left perfectly free. While this bufinefs is going on, an affiftant fhould prepare bed-cloaths, carefully brought to the proper degree of heat. I confider heat as congenial with the living principle; increafing the neceffity of action it increafes action: cold, on the other hand, leffens the neceffity, and of courfe the action is diminifhed; to a due proportion of heat, therefore the living principle owes its vigour.

From obfervations and experiments it appears to be a law of nature in animal bodies, that the degree of external heat fhould bear a proportion to the quantity of life; as it is weakened, this proportion requires great accuracy in the adjuftment; while greater powers of life allow it greater latitudes.[a]

I was led to make thefe obfervations by attending to perfons who are froft-bitten; the effect of cold in this cafe being that of leffening the living principle. The powers of action remain as perfect as ever, only weakened; and heat is the only thing wanting to put thefe powers into action; yet heat muft at firft be gradually applied, and proportioned to the quantity of the living principle; but as that increafes you may increafe the degree of heat. If this method is not obferved, and too great a degree of heat is at firft applied, the perfon or part lofes entirely the living principle, and mortification enfues. This procefs invariably takes place with regard to men. The fame thing, I am convinced, happens to other animals. If an eel, for inftance, is expofed to a degree of cold fufficiently intenfe to benumb it till the remains of life are fcarcely perceptible, and ftill retained in a cold of about 40°; this fmall proportion of living principle will continue for a confiderable time without diminution or increafe; but

[a] It is upon thefe principles that cold air is found of fo much fervice to people who are reduced by difeafe, as the confluent fmallpox, and fevers, by diminifhing heat in proportion to the diminution of life; or leffening the neceffity of the body's producing its own cold.

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 phænomena 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 become, before the ſeaſon permits it to come out, very conſiderably weakened, perhaps ſo much as not to be again reſtored. If animals, in ſuch a 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, any of them that had been caught, and 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.[a] The ſame effects probably take place even in perfect health. It appears 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.[b]

If bed-cloaths are put over the drowned perſon ſo as ſcarce to touch him, ſteam of volatile alkali, or of warm balſams and eſſential oils, may

[a] It is upon this principle that parts mortify in conſequence of inflammation.

[b] Vide Phil. Tranſ. for the year 1775, vol. 65. p. 111.

be

be thrown under ſo as to come in contact with many parts of his body. It will certainly prove advantageous if the ſame 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, an effect I ſhould chuſe to avoid. Some of the ſtimulating ſubſtances, which are of a warm nature, and have an immediate effect, may be thrown into the ſtomach in a fluid ſtate, viz. as ſpirits of hartſhorn, peppermint-water, juice of horſe-raddiſh, and many others which produce a more laſting ſtimulus, as balſams and turpentines, which are found to quicken the pulſe of a man in health; but the quantity muſt be ſmall, as they have a tendency to produce ſickneſs. The ſame ſteam and ſubſtances ſhould alſo be thrown up by the anus. The proceſs recommended under the firſt head of treatment ſhould ſtill be continued, while that recommended under the ſecond is putting in practice, as the laſt is 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 part of the proceſs.[a] I would recommend to the operator the ſame care in regulating the proportion of every one of theſe methods, as I did before in the application of heat; as every one of them may poſſibly have the ſame property of entirely deſtroying the feeble action which they have excited, if adminiſtered in too great a quantity; 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 leſſened, that their increaſe afterwards may be directed,

[a] Electricity has been known to be of ſervice, and ſhould be tried when other methods have failed. It is probably 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.

as nearly as poſſible, by the quantity of powers as they ariſe. As the heart is commonly the laſt part that 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 obſerving 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 to be careful not to call forth any diſpoſition that might depreſs, by introducing any thing into the ſtomach, which ordinarily creates nauſea, as that alſo will have a ſimilar effect, except it can be carried ſo far as to relieve itſelf by exciting vomiting, and would therefore avoid throwing any thing in by the anus which is likely to produce an evacuation that way, as every ſuch evacuation alſo tends 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 purpoſes 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 neceſſary, ſo that no time may be loſt between the operations; and the more ſo, 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; by this the artificial breathing

may

may be continued, while the other operations, except the application of the ſtimuli to the ſtomach, are going on, which cannot conveniently be done if the muzzle 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 pair of ſmall 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.

ON THE STRUCTURE OF THE PLACENTA.

THE connection between the mother and fœtus 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 until 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;[a] 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 had 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.[b]

Having opened the abdomen, and exposed the uterus; he made an incision into the fore part, quite through its substance, and came to somewhat having the appearance of an irregular mass of injected matter, which afterwards proved to be the placenta. This appearance being new, he stopped, and greatly obliged me, by desiring my attendance to examine the parts, in which there appeared something so uncommon. This examination was made in his presence, and in the presence of several other

[a] 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.

[b] Dr. Mc. Kenzie being then an assistant to the late Dr. Smellie, his 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.

gentlemen

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

I firſt raiſed, with great care, part of the uterus from the irregular maſs above mentioned; in doing which I obſerved regular pieces of wax paſſing obliquely between it and the uterus, which broke off, leaving part upon this maſs; and when they were attentively examined, towards the uterus, plainly appeared to be a continuation of the veins paſſing from it to this ſubſtance or 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 I 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

[a] If I ſhould be ſo fortunate as to have this publication fall into any of thoſe gentlemens 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.

next examined the arteries, and tracing them in the ſame manner toward the placenta, found that they made a twiſt, or cloſe ſpiral turn upon themſelves, and then 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 obſerved, and was intermixed with the red injection.

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. This 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 a natural cellular ſtructure, fitted to be a reſervoir for blood.

In ſome of the veins leading from the placenta to the uterus, I perceived that the red injection of the arteries, (which had been firſt injected) had paſſed into them out of the ſubſtance of the placenta, mixing itſelf with the yellow injection. I alſo obſerved, that the ſpongy chorion, called the decidua, by Dr. Hunter, was very vaſcular, its veſſels coming from and returning to 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, was ſoon convinced. 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 placentas, to ſee 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.

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 ſhall 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 fœtus, 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 modes by which they produce the ſame effect.[a] In the firſt diviſion is included the human ſpecies,

[a] 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

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.[a]

In the human ſpecies the anatomical ſtructure of the mother and embryo, relative to fœtation, being well known, it will only be neceſſary fully to deſcribe the nature of that 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 fœtus is to be derived; which will lead us laſtly to a conſideration of thoſe peculiarities by which the fœtus is to receive its nouriſhment, and that conſtitute its immediate communication likewiſe 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; but all theſe lobes are united into one uniform ſurface next to the child, on which its umbilical veſſels ramify. When we cut into the placenta, its whole ſubſtance appears to be little elſe than a net-work, or ſpongy maſs, through which the blood veſſels of the fœtus ramify, and indeed it ſeems to be principally formed by the ramifications of thoſe veſſels; exhibiting hardly any appearance of connecting membrane: but we can hardly 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

[a] Vide page 45.

the whole mafs of placenta, and all the cells have a communication at the common bafe.

This ftructure of the placenta, its reciprocal communication with the two bodies with which it is immediately connected, and the ufe arifing from this arrangement, form the union between the mother and fœtus for the fupport of the latter. Prior to the time I have mentioned above, anatomifts feem to have been wholly unacquainted with this ftructure of placenta. By notes taken from Dr. Hunter's lectures, in the winter, 1755-6, it appears that he expreffed himfelf in the following manner.[a] "The fubftance of the placenta is a flefhy mafs, which feems to be formed entirely of the veffels of the umbilical rope." In another part, mentioning the appearances when injected, he fays, "and upon a flight putrefaction coming on, you will find the whole appearing like a mafs of veffels"; then fays, "there is always a white uninjected fubftance between the veffels, but whether lymphatics or what I cannot tell." This uninjected fubftance, mentioned by Dr. Hunter, is what forms the cellular ftructure.

The placenta feems to be principally compofed of the ramifications of the veffels of the embryo, and may have been originally formed in confequence of thofe next to the uterus laying hold by a fpecies of animal attraction of the coagulable lymph which lines the uterus. This might take place in a manner refembling what happens when the root of a plant fpreads on the furface of moift bodies; with this difference, that in the prefent inftance the veffels form the fubftance through which they ramify, as in the cafe of granulations.

At the time, or very probably before the female feed enters the uterus, coagulable lymph, from the blood of the mother, is thrown out every where on its inner furface, either from the ftimulus of impregnation taking place in the ovarium, or in confequence of the feed being expelled from it. When the feed has entered the uterus, it attaches itfelf to that

[a] Thefe 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 occafion, with the fame ufe of his notes. Vide Dr. Hunter's Commentaries.

lymph

lymph, by which it becomes covered and immediately ſurrounded.[a] This coagulable lymph forms a ſoft pulpy membrane, the decidua, which is, I believe, peculiar to the human ſpecies, and to monkeys, having never 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 into the cellular ſubſtance of the placenta, as before deſcribed. As this membrane lines the uterus and covers the ſeed, 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, and the decidua to be a production of the mother; and an additional proof of both theſe may be drawn from the circumſtance of the decidua paſſing between the placenta and uterus. 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 parts diſtinct, and at a diſtance from the placenta, ſhould be plainly ſeen

[a] This is exactly 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 incloſed, hydatids detached, and afterwards incloſed.

paſſing

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.

The veſſels of the fœtus adhering, by the intervention of the decidua, to a certain ſpace 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 of the uterus which are not immediately employed in conveying nouriſhment to it, 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, 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 as it approaches the ſpongy ſubſtance of the placenta, and is a ſtructure which muſt leſſen the quick motion of the blood in a part where a quick motion of this fluid was not wanted. The ſize of theſe curling arteries at this termination is about that of a crow's quill.

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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 thoſe veins bear no proportion to their circumference, the veins being very much flattened.

This ſtructure of parts points out at once the motion of the blood 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 chord, 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 terminates in 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.

OBSERVATIONS

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 the one I am going to give an account of.

A female monkey, belonging to Mr. Enderſbay, 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. However, ſome time after, ſhe appeared to be bigger in the belly, which 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, and it was now certain 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 after-birth 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 after-birth 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 above mentioned, 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 receives 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, and 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 treminated in the placenta.

The principal veins in general aroſe 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

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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 very 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.

EXPLANATION

Wm. Bell del. Wm. Skelton sculp.

EXPLANATION OF THE PLATE.

A Part of a 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

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

ONE of the digeſtive organs of this trout being ſo very remarkable as to have given name to the fiſh, and to be looked upon as its diſtinguiſhing characteriſtic; it will be neceſſary to take a general view of the varieties in the digeſtive organs of animals, to be able to determine what place the ſtomach of this particular trout holds among them, and to throw ſome light upon the queſtion, whether its reſemblance to a gizzard be ſuch, as to render the name of gizzard trout a proper appellation. For this purpoſe let me ſtate ſome general facts. Food of animals may be divided into two kinds, what does, and what does not, require maſtication to facilitate digeſtion. All animal food is of this latter kind. But grain, and many other ſubſtances which ſerve for aliment, require a previous grinding or trituration; and therefore animals which live on ſuch 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 a ſet of teeth of a particular form, which ſerve as ſo many grindſtones for reducing their food to a powder, before they convey it into the ſtomach for digeſtion. When ſo prepared, it is, with regard to the digeſtive power, rendered ſimilar to animal food: therefore in many ſuch animals the ſtomach is ſimilar to that of the carnivorus; and whenever the ſtomach in granivorous quadrupeds departs from this general rule, there is a ſingularity in the operations of digeſtion. Such birds as live upon food, for the digeſtion of which trituration is indiſpenſably neceſſary, have the powers of maſtication and digeſtion united in one part, the gizzard; which is peculiarly conſtructed for that purpoſe. 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 point out the food of the ſpecies as clearly as the teeth and ſtomach together do in thoſe animals in which the two offices of maſtication and and digeſtion are not joined together in the ſame part.

As it appears then to be the difference of the ſtomachs only, that fits birds for their different kinds of food, it is evident that every gradation of ſtomach muſt be found among them, from the true gizzard which is one extreme, to the mere membranous ſtomach which is the other; ſince the food of different ſpecies is of every different kind, from the hardeſt grain to the ſofteſt animal matter. In conſequence of this, it muſt be as difficult to determine the exact limits of the two different conſtructions, 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 are quite imperceptible. Similar gradations are obſerved 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, as 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 filled up 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 ſofter 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, they both act, in ſome degree, as a hinge, on which the two muſcles may be ſaid to move, by means of the middle tendon allowing of a free motion of the grinding ſurfaces on each other, which is neceſſary for the comminution of food.

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The two flat lateral fides of the grinding cavity are lined with a thick horny fubftance fimilar to a hard and thick cuticle: the narrow anterior and pofterior tendinous parts are alfo lined with a cuticle, but not fo ftrong as the former: this horny fubftance is gradually loft at one end in a very thin cuticle, which lines the paffages of the œfophagus and inteftine for a little way; and at the other end is loft in the fame manner in the membranous bag.

The two large mufcles may be confidered as a pair of jaws, whofe teeth are taken in occafionally, being fmall rough ftones or pebbles which the animal fwallows: and from the feeling on the tongue, it can diftinguifh fuch of thefe as are proper, from thofe which are fmooth or otherways unfit for the purpofe, which laft it inftantly drops out of its mouth.

Some birds, with gizzards, have a craw or crop alfo, which ferves as a refervoir, and for foftening the grain; but as all of them have not this organ, it is not to our prefent purpofe.

There are other animals befides that clafs of birds, which mafticate their food in their ftomach, but their teeth are placed there by nature: crabs and lobfters are of this kind.

The gradation from gizzard to ftomach is made by the mufcular fides 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 confidered as a firft principle, with refpect to which the digeftive powers, with their appendages, are as fecondary parts, being adapted to and determined by the food, as the primary object.

We find then that in granivorous animals of all forts, there is an apparatus for the maftication of the food, although of different kinds and differently placed. But in true carnivorous animals of whatever tribe, maftication is not fo neceffary, and therefore they have no apparatus for that purpofe. The teeth of fuch quadrupeds, as are carnivorous, ferve chiefly to procure food and prepare it for deglutition. The fame thing holds in the true carnivorous bird, the office of whofe beak and talons is to procure the aliment, and fit it for deglutition, correfponding in this refpect to the teeth of the others. Applying this to fifh, it feems, at firft fight, that there is no occafion in them for that variety of ftructure in the

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digeftive organs, which is found in the before mentioned quadrupeds and birds; the food of fifh being principally of one fort, namely, animal, which however with regard to the digeftive powers, is to be diftinguifhed into two kinds, viz. common foft fifh and fhell-fifh. Such fifh as live on the firft kind, have like the carnivorous quadrupeds and birds, no apparatus for maftication; their teeth being intended merely for catching the food and fitting it to be fwallowed. But the fhells of the fecond kind of food render fome degree of mafticating power neceffary, and accordingly we find in certain fifh a ftructure fuited to this purpofe.

Thus the mouth of the wolf-fifh is almoft paved with teeth, by means of which it can break any fhells to pieces, and fo effectually difengage the food for digeftion, that though it lives upon fuch hard food, the ftomach does not differ from that of other fifh: the organs of maftication and digeftion therefore in this animal exactly correfpond to thofe of many granivorous quadrupeds.

Other fifh, on the contrary, approach nearer to the ftructure of birds, in having their ftomach furnifhed with fome degree of mafticating power; this in many is very imperfect, compared with the gizzards of fowls, though perhaps the difference is fuch only as the difference of food will properly allow: for in thofe fifh who have this power, the food being ftill animal; and in general but imperfectly covered with the fhell, it perhaps wants only to be broken; however, in the bulla lignaria of Linnæus, this apparatus is more perfect, confifting of two bones, which we muft fuppofe capable of grinding hard fhells; whereas the food of granivorous birds requires to be ground into a kind of meal.

Of all the fifh I have feen, the mullet is the cleareft inftance of this ftructure; its ftrong mufcular ftomach being evidently adapted, like the gizzard of birds, to the two offices of maftication and digeftion. The ftomach of the fifh now before us holds the fecond place.

But ftill neither of thofe ftomachs can be juftly ranked as gizzards, fince they want fome of the moft effential characters, viz. a power and motion fitted for grinding, and the horny cuticle. The ftomach of the Gillaroo trout is however more circumfcribed than that of moft fifh, better adapted for fmall food, and endued with fufficient ftrength to break the fhells ſhells of ſmall ſhell-fiſh; which will moſt 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 Engliſh trout is exactly of the ſame ſpecies with that of the Gillaroo, but its coat is not ſo thick by two-thirds.[a] How far this difference in thickneſs of ſtomach is ſufficient to make a diſtinct ſpecies, or barely a variety of the ſame, is only to be determined by experiment.[b]

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.[c]

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.

[a] The Engliſh trout ſwallows ſhell-fiſh, and alſo pretty large ſmooth ſtones, which ſerve as kind of ſhell-breakers.

[b] 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.

[c] I chuſe to give this name to the pancreas from its appearance.

SOME

SOME OBSERVATIONS ON DIGESTION.

THE paper which I formerly prefented to the Royal Society, on the ftomach being digefted after death, and which was publifhed in 1772, in the 62nd volume of the Philofophical Tranfactions, feems to have attracted the attention of Spallanzani, and others. I fhall therefore make fome remarks upon the experiments and opinions of thofe gentlemen; compare them with thofe of Reaumur, and give fome general facts and obfervations of my own upon digeftion, and fhall conclude by adding a copy of the above mentioned paper, with the hope that others will take up the fubject in a more enlarged point of view, and profecute an inquiry which is of fo much confequence in the inveftigation of the operations of the animal œconomy. I cannot, at prefent, fpare fufficient time to give my opinions at large on this fubject, with all the experiments and obfervations I have made upon it; but as foon as I have leifure I fhall lay them before the public.

The difcovery of parts has been a principal object in the refearches of the young or practical anatomift; but the connection, arrangement, mode of action, and ufes of the whole, or of particular organs, have more commonly been referved for the confideration of thofe whofe views were extended further; and whofe powers of reafoning had been enlarged by habits of obfervation and inquiry. Curious and fpeculative men have likewife made attempts in this way, often without being fufficiently acquainted with the ftructure of the parts they were about to confider; and confequently ignorant of their relations and connections with one another. They have not been contented to fpeculate concerning thofe which were moft obvious, which might have led to ufeful knowledge; but directed by what beft fuited their fancy, they have principally attempted the moft obfcure and intricate. Generation, or the mode of continuing the fpecies, and digeftion, or the means of preferving the individual, have been with them the great objects of inquiry; but it does not appear that they have been very fuccefsful. Digeftion, as being one of the moft important operations of the animal œconomy, and moft obvious in its effects, fupplies a number of facts to aſſiſt in aſcertaining its powers; yet 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, having 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 theſe laſt conſidered fermentation as the great agent in digeſtion: but as they were rather ſpeculative philoſophers, than practical anatomiſts, they 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 phænomena in the operation of digeſtion? But we can have no very high idea of experiments made by gentlemen and *prieſts*, who, for want of anatomical knowledge, have not been able to purſue their reaſoning even 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 not being univerſal; and thoſe gentlemen who conſidered 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 at the ſame time ſupplied the chymiſt with arguments in favour of the proceſs of fermentation.

It

It is indeed a more difficult matter than thofe gentlemen imagine, to acquire on this fubject a knowledge fufficiently accurate, to be able to explain a procefs fo complicated as that of digeftion. There are in Nature's operations always two obvious extremes; and the mind of man eagerly adopts that which accords with fome principle he is fond of, and with which he is beft acquainted; but the intermediate connections and gradations being lefs ftriking, do not fo forcibly affect the fuperficial inquirer.

It happens unfortunately that thofe who from the nature of their education are beft qualified to inveftigate the intricacies, and improve our knowledge of the animal œconomy, are compelled to get their living by the practice of a profeffion which is conftant employment. The only idle profeffional men are thofe of the church; and we therefore frequently fee them becoming philofophers and phyfiologifts, as it were inftinctively, without having had that kind of education which might direct their purfuits. Experiments, it is true, may be made by fuch men; but they muft not be complicated, nor having any immediate relation to a branch of knowledge, with which they cannot be much acquainted; and experiments fo made, will feldom go further than perhaps to explain a fingle fact. They may look through microfcopes and examine the red globules of blood; they may view animalculæ and give us a candid relation of what they fee; but fhould not prefume to carry their reafoning into a fcience of which they can know nothing; or hope to throw light on a fubject which it is impoffible they can underftand. It fhould be remembered, that nothing in nature ftands alone; but that every art and fcience has a relation to fome other art or fcience, and that it requires a knowledge of thefe others, as far as this connection takes place, to enable us to become perfect in that which engages our particular attention.

Thefe obfervations are applicable to all thofe who have made experiments to explain digeftion. The mechanical powers being eafily underftood, thofe who confidered digeftion mechanically have in general explained their effects juftly, as far as they applied to the gizzard; but their powers of reafoning went no further, and they fuppofed thefe effects to be digeſtion. But thoſe who took it up chymically being leſs acquainted with chymiſtry, and totally ignorant of the principles of the animal œconomy, have erroneouſly referred the operations of the animal machine to the laws of chymiſtry.

The firſt inquirers into digeſtion were ſtruck only by the extremes, the gizzard, and membranous ſtomach, paying no regard to the gradations leading from the one to the other; which if properly examined would have aſſiſted them more effectually 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 he is entitled to no merit from this idea, as the premiſes are falſe: however, this opinion of Vallisneri ſet Reaumur to work, and has been the means of bringing ſeveral curious facts to light. His experiments were firſt made with a view to contradict that idea; and were therefore made upon birds that had gizzards, as beſt adapted to his purpoſe. In this purſuit he only attended to thoſe parts of the experiments which beſt accorded with his own opinion; yet carefully guarded againſt every poſſible accident that might affect their accuracy. If trituration was the immediate cauſe of digeſtion, his making experiments on the gizzards of birds was unneceſſary; it would have been ſufficient to have examined the food after it had been maſticated by the teeth of animals who have grinders; for the teeth and gizzard anſwer 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

other preparation? Several reafons feem to oppofe this; trituration alone might reduce the grain into a flour; but flour alone is not chyle." "From the fmell of the aliment (taken from the gizzards of birds) are we not led to conclude that it undergoes a fermentation? This fmell may be faid to arife from the liquor with which the aliment is mixed; but is it likely that juices do not difpofe to fermentation, fuch fubftances in which it is fo eafily excited? Flour made into a pafte and fruit, require little more than heat to make them ferment." From thefe very experiments made with a view to prove that digeftion is carried on by trituration, Reaumur was led to fuppofe a folvent. But as there are fome birds whofe ftomachs do not feem fufficiently ftrong to have the power of trituration, he felected the buzzard, as being of that kind, and the fitteft, for the fubject of his experiments, from the circumftance of its throwing up whatever is folid and indigeftible; therefore without killing the bird, he could know the refult, and repeat the experiment as often as he thought neceffary.

From the ftomach in the buzzard being incapable of trituration, he concluded that a folvent was neceffary for digeftion; but to preclude all mechanical effects of the ftomach, in his experiments, he employed tin tubes filled with meat, which, after the tubes had remained twenty-four hours in the ftomach of the buzzard, was reduced to three-fourths of its fize was like threads, and was neither putrid, four, nor volatile, but infipid. On this effect he made his remarks, which are very pertinent. In another experiment which was more accurate and conclufive, he is convinced of the action of a folvent. He then tried the foft bones of young animals, and found they were digefted; and that though the hard bones were not acted on fo readily; yet by returning the fame bones feveral times into the ftomach, they were at laft digefted.

Reaumur was next anxious to know, if fuch birds as were intended by nature to live upon meat, could alfo digeft vegetables; but the refult was not fo fatisfactory. He gave bread to his buzzard, which upon being returned had the appearance of having been chewed. He next tried a piece of a ripe pear; which, after having been twenty-four hours in the ftomach it had loft fome of its weight, and had the appearance of

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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 this liquor which had ſuch powers, he taſted the jelly to which the meat and bone had been reduced, conceiving it muſt be well impregnated with this fluid, but could only diſtinguiſh a bitter or ſaltiſh taſte. To have an opportunity of more fully 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 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 has a ſolvent power; but it did not digeſt bread, although 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 graſs, when incloſed in tubes.

Reaumur's experiments, although not compleat, yet paved the way for future inveſtigation; and Spallanzani proceeding on the ſame ground, has

has confirmed them by his own, and has filled up ſeveral blanks not compleatly 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 did not poſſeſs general knowledge ſufficient to direct him in his purſuits, which neceſſarily confined him to what he was moſt maſter of, the mere making experiments. He was neither an anatomiſt, nor a phyſiologiſt; nor can he be ſaid to have been perfectly juſt in his deſcription of parts, having conſidered the crop, and the œſophagus leading from it to the gizzard, as being two diſtinct ſtomachs. This however is only to be ſet down as a piece of anatomical ignorance, not affecting the ſubject in the leaſt. Spallanzani is alſo incorrect in his anatomical knowledge; but it muſt be owned, that his experiments, as far as they go, are in themſelves concluſive; but like all mere makers of experiments, he is not ſatisfied 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 merely tend 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 moſt animals 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, however unneceſſary or fatiguing to the reader, muſt be pleaſed with Spallanzani; but he muſt

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tire even thofe whom he informed, and much more thofe who only read his works in expectation of fomething new. Reaumur, feemed indeed willing to give up the idea of trituration being the fole caufe of digeftion; but Spallanzani perfifts in proving that it is not performed by trituration.

To make comparative experiments upon the digeftive power of different animals, they fhould be under the fame circumftances relative to digeftion: they fhould be equal in age; for the growing eat more than the full-grown, and of courfe digeft fafter; and this point therefore can be beft afcertained in each clafs of animals, by felecting thofe which have attained their full-growth. They fhould be equal in fatnefs, for this makes a very material difference in the powers of digeftion in the fame animal; and they fhould be equal in health; which laft circumftance, of all others, probably makes the greateft difference in the powers of the ftomach. In comparing animals of the fame clafs, the atmofphere fhould likewife be of the fame temperature; for the different claffes of animals are varioufly affected by the fame degree of heat. Experiments made upon fnakes and lizards in the winter, will differ greatly from thofe made in the fummer, while fimilar experiments made on dogs will have nearly the fame refult in both feafons. Nor will the powers of the ftomach be found equal in the fame clafs; for fleeping animals, of the quadruped kind, as hedge-hogs, do not digeft in the winter, but in the fummer only; therefore the conclufions to be drawn from experiments made refpecting the digeftive powers in one, are not at all applicable to thofe made in the other feafon.

Spallanzani obferved that the fnake digefted food fafter in June, when the heat was at 82° and 83°, than in April, when it was only 60°; from whence he concludes, that heat affifts digeftion; but this heat is not the immediate, but the remote caufe of the increafed power: heat having produced in the animal greater neceffity for nourifhment; and of courfe greater powers; gaftric juice was therefore fecreted fafter or in greater quantity.

As a proof that heat does not act as an immediate, but only as a remote caufe in affifting digeftion, I fhall mention the effect it produced

upon

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 volume[a].

"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 ſlow digeſtion in ſerpents, and quotes Bomare; who gives an account of a ſerpent at Martinico, which after having retained a chicken in its ſtomach for three months, it was not completely digeſted, the feathers ſtill adhering to the ſkin[b]. 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 wanted; unleſs in Martinico, as in colder climates, there is a torpid ſeaſon, in which the act of digeſtion is not neceſſary: but in that caſe the ſerpent would not have ſwallowed the chicken. When 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 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 allowed to live, 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 requiring action ſuitable to that warmth; the body requiring nouriſhment ſuitable to that action; and the ſtomach being called upon, obeys.

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 break-

[a] Vide page 100. [b] Bomare Dict. d'Hiſtoire Nat.

ing or grinding down the grain; and that they are picked up accidentally. Theſe ſtones have long been ſuppoſed to anſwer the purpoſes of trituration: they have been conſidered as an aſſiſtance to the ſtomach, in the manner of teeth, and of courſe neceſſary for digeſtion. Spallanzani combats this opinion; but as ſtones are univerſally found in gizzards, he found it neceſſary to account for the mode of their being conveyed there, and 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 to be 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 put tubes, needles, and lancets, in gizzards in which there were but very few ſtones, and found them broken; but in this experiment they 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 thoſe ſubſtances 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 where he had taken care there ſhould be no ſtones, yet 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 found that the inner ſurface of the ſtomach was not hurt by ſuch ſubſtances. Indeed it is ſcarcely poſſible for the inner coat of the ſtomach of a fowl to be pierced by ſharp pointed ſubſtances; its quantity of motion being ſo inconſiderable, as hardly to make a body paſs through its inner coat. But the principal reaſon is, that this motion being lateral, it does not preſs perpendicularly to its axis, but is one ſurface ſliding in a contrary direction to another; and this 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

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: he 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 that uſe, although the part can do its office without them?

To account for pebbles being found in the gizzards of birds, he ſuppoſes them picked up by chance, or by their not diſtinguiſhing between food and ſtones. It appears ſingular, that only thoſe which have gizzards ſhould be ſo ſtupid: he owns, that Redi and himſelf found that birds died of hunger, without having picked up more ſtones than uſual, which we might ſuppoſe would not have been the caſe if they had no choice; nor can ſtones be confounded with the grain on which theſe birds feed.

The ſtones aſſiſt in grinding down the grain, and by ſeparating its parts allow the gaſtric juice to come more readily in contact with it; they alſo rub off the digeſted ſurface, by which means the remainder is ſooner brought into contact with the gaſtric juice.

It has been mentioned, that the motion of the gizzard is hardly obſervable, and cannot be felt by the hand; but for the purpoſe of trituration that is not neceſſary; for its cavity is very ſmall, and adapted to its contents, which it muſt always be, or it cannot poſſibly grind; and therefore they require but little motion to affect them. A ſwelling and collapſing, like the motion of the heart, would have no effect. The extent of motion in grind-ſtones 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 we can hear the ſtones 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 motion. The food paſſes ſlowly along the œſophagus; and even in that of a man, fluids which might be expected to act by their own gravity, deſcend but ſlowly. I imagine, however, 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, we do not find it much agitated or affected; not even by handling or being irritated. The ſame thing may be obſerved in the whole tract of inteſtines; and we find that when the fæces are expelled by the action of the gut alone, that this expulſion is ſlow; the ſtomach and rectum, however, can be emptied at once; but this 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 by the ſame action the contents of the rectum can be expelled.

We need not ſeek for another power to empty the ſtomach in vomiting, theſe muſcles being often capable of forcing the bowels themſelves out of the abdomen, 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. Since the lungs do not act in the leaſt of themſelves, to throw up any extraneous matter; and a cough to the lungs is ſimilar to a vomit to the ſtomach. The muſcles of reſpiration are the active parts in emptying the lungs, and 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

There is reafon to believe that the natural motion in all ftomachs is regular. What makes me of this opinion, is that appearance which takes place in the ftomach of animals who are covered with hair, and who lick their own bodies; and of fuch as feed on whole animals, who are likewife covered with hair. In the calf, for inftance, who licks his fkin with his tongue, and fwallows whatever is attached to the rough furface of that organ, balls of hair are often found in the cavity of the ftomach; on examining their furface, the hairs in each hemifphere feem to arife from a centre, and to have the fame direction, which is circular, and correfponding to what would appear to be the axis of this motion, refembling what we fee in different parts of the fkin of animals whofe hair take different turns. This regularity in the direction of the hair, in fuch balls, could not be produced if there was not a regular motion in the ftomach. This motion is alfo proved in the dog; for I have feen a ball of this kind, that had been thrown up from a dog's ftomach, where the fame regularity in the turns of the hair was very evident and compleat. The fame motion feems alfo to take place in the bird kind: in the cuckoo, for inftance, which in certain feafons lives on caterpillars, fome of whom have hairs of a confiderable length on their bodies, the ends of thefe are found fticking in the inner horny coat of the ftomach or gizzard, while the hairs themfelves are laid flat on its furface; not in every direction, which would be the cafe if there was no regular motion, but all one way, arifing from a central point placed in the middle of the horny part; and this appearance on the furface of both fides of the gizzard correfponding. Thefe two facts prove, in my opinion, a regular and circular motion taking place in the gizzard and membranous ftomach; and therefore, moft probably, fomething fimilar is carried on in all the various kinds of ftomachs. Indeed this motion in the ftomach is fo confiderable, than when there is no horny defence, we find the coats of the ftomach fometimes pierced by hard pointed bodies. Thus the cows who feed on the grafs of bleaching grounds have their ftomachs, efpecially the fecond, ftuck full of pins; and fifh who prey upon, and fwallow other fifh entire, often have their ftomachs pierced by the bones.

Spallanzani

Spallanzani calls the inner coat, cartilaginous; whereas, in fact, it is a horny fubftance, forming an inner cuticle, but differing in fome refpects from the common cuticle. This horny fubftance not only differs in ftructure from the common cuticle, but in its attachment, from both cuticle, nails, and hoofs. The cutis, where it is covered by fuch fubftances, has a vaft number of villi on its furface, which pafs into correfponding perforations in the cuticle, by which ftructure of parts, when the cuticle, nails, or hoofs are feparated, their inner furface appears to be full of fmall perforations; and the cutis from which they have been removed is villous; and thefe villi are more numerous in fome parts than in others, where the fenfe of touch is required to be delicate or acute. But the inner lining of the gizzard is juft the reverfe; that furface of the horny fubftance which is in contact with the gizzard being villous; and when feparated, the inner furface of the gizzard appears perforated. Thefe villi are the laft formed parts of this horny fubftance, or are the fibres of which the horny coat is compofed. It is probable, that this horny fubftance takes the form of villi that it may be more firmly connected with the inner furface of the ftomach; there being no occafion for acute fenfation in the ftomach.

We may remark here, that the experiments made on the digeftion of ruminating animals have been deficient, which arifes from this procefs in them being more complicated than in the ftomachs of other animals; requiring attention to be paid to certain circumftances, which cannot take place in ftomachs of only one cavity.

The circumftance mentioned by Spallanzani, of the ruminating animals voiding the tubes, fhows that they are not careful the whole food fhould be returned into the mouth to be chewed a fecond time; for if they were, the tubes would certainly come up likewife, and would as certainly be thrown out of their mouths as improper to be chewed, which very often happened. But it was hardly neceffary to make experiments to afcertain whether ruminating animals digefted meat, when we know that in fome cold countries, the cattle are fed on dried fifh; and moft animals eat their own fecondines: indeed the circumftance of animals living upon both animal and vegetable food might have taught us, that the mode of digefting both (whatever it is) was the fame;

therefore

therefore it could only be neceſſary to diſcover that mode; except we 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 this happens it ariſes from the powers of digeſtion being defective. It is often found that 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 certain 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[a]; 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,

[a] Phyſical and chymical Eſſays by Sir Tobern Bergman.

AS

as the fermentation advances towards the putrefactive, the meat at laſt begins to acquire the ſame putrid diſpoſition. Of this Sir John Pringle was not aware in making his experiments on this ſubject. Yet this laſt part of the proceſs cannot, I think, take place in the ſtomach; for 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 probably 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.

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

getable nor animal ſubſtances can undergo their ſpontaneous changes, while digeſtion is going on in them; 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. 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, prevents the ſpontaneous change from taking place. In moſt ſtomachs there is an acid, even although the animal has lived upon meat for many weeks; this, however, is not always the caſe, therefore 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; for animals in health ſeem to have the power of ſecreting ſugar, as we find in the milk, and ſometimes in the urine, from diſeaſe. The acid prevails ſometimes 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: the ſugar of vegetables, and in ſome ſtomachs even vinous ſpirits, turning directly into acid. To aſcertain whether there is an acid naturally in the ſtomach, it will be proper to examine the contents before the birth, when the digeſtive organs are perfect, and when no acid can have been produced by diſeaſe, or any thing that has been ſwallowed. In the ſlink calf, near the full time, there is no acid found in the ſtomach; although the contents have the ſame coagulating powers with thoſe of animals who have ſucked.

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; but the laſt effect, I believe, ariſes from diſeaſe in that viſcus. It may be difficult to account for the formation of this air; and as the ſtomach is a reſervoir for ſubſtances diſpoſed to ferment, it might 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 ſome ſtomachs, even where food has not been ſwallowed for a conſiderable time, and where digeſtion appeared to have been compleated; which we muſt conclude to have been the caſe, from the food not having diſagreed with either ſtomach or bowels, and from the ſtools being good. When the gout falls on the ſtomach, the quantity of air thrown up is often immenſe. The ſame thing may be obſerved in ſome caſes which are commonly called nervous; yet the proceſs of digeſtion will not account for this formation of air, as no air is to be found[a] 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, as a kind of ſecretion: we can give no 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 air being detected in the uterus or vagina, without the perſons themſelves knowing any thing of it, except by not having at the time of its paſſing the ſame power to prevent its eſcape, as when it is in the rectum; from which circumſtance they were always alarmed leaſt it might make a noiſe in its paſſage. This fact being ſo extraordinary, rendered me ſomewhat incredulous; but made me more inquiſitive, with 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 ſenſible of it till it paſſes. A woman, whom I attended with the late Sir John Pringle, informed us of this fact; but mentioned it only as a diſagreeable thing. I was anxious to determine, if there were any communication

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

between

between the vagina and rectum, and was allowed to examine, but difcovered nothing uncommon in the ftructure of thefe parts. She died fome time after; and being permitted to open the body, I found no difeafe either in the vagina or uterus. Since that time I have taken opportunities of inquiring of a number of women, concerning this circumftance, and by three or four have been informed exactly of the fame fact, with all the circumftances abovementioned: how far they are to be relied upon I will not pretend to determine. I have likewife found air in the cellular membrane, in fome gun-fhot wounds that had paffed fome way under the fkin, 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 loofe by fome action of the veffels both naturally and from difeafe, is an undeniable fact. We find air formed in fome fifhes to anfwer natural purpofes; for in thofe fifhes whofe air-bladders do not communicate externally (many of which there are) we muft fuppofe it to have been formed there. We alfo find it in animals after death; and I have a piece of inteftine of a hog which has a number of air-bladders upon it[a]. I have often feen fuch vesicles on the edges of the lungs; but thefe may be fuppofed to have been kind of aneurifmal air-cells filled from the trachea, which may poffibly be the cafe; but they are circumfcribed and impervious, fo that in the ftate we find them, they have no communication with the external air. In one inftance I have difcovered air in an abfcefs, which could not have been received from the external air; nor could it have arifen from putrefaction; the cafe 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 fymptoms, her difeafe was fuppofed by fome to be the ftone; but upon examination no ftone was found. She had alfo an umbilical hernia, for which I had been confulted. She grew gradually worfe; and from being a lufty became a thin woman. A fmall tumor appeared in the groin, and the fkin over it became red, fimilar to an abfcefs when the matter is beginning to point

[a] Vide Plate.

 externally;

externally; but before her death this fubfided. A few days before fhe died, I was defired to examine a fwelling on the lower and right-fide of the belly, extending nearly from the navel to the fpine of the ilium on the right-fide, and almoft of the fame width. It was a tenfe fwelling, but evidently contained air, and could be made to found almoft like a drum. It had come on within a very few weeks, and I felt myfelf puzzled to account for it, there being clearly no connection between that tumor and the umbilical hernia. I was inclined to fuppofe it to be a ventral hernia, containing the cæcum and part of the colon, filled with air. But as fhe had ftools; as there were no fymptoms of a ftrangulated gut, nor any uneafinefs in the bowels, as I could not make the air recede, but felt it as if confined to that part, I own I could not conjecture what the cafe 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-fide of the linea alba; and on examining the cavity of the abdomen, found every thing natural, except a fmall portion of the epiploon adhering to the infide of the navel; but oppofite to the tumor, the parietes of the abdomen were in a natural ftate. On preffing the tumor by the hand, air was heard to make its efcape; but whether by the vagina or anus was at firft doubtful. On examining with more attention, it was difcovered to come from between the two labia. I now opened the tumor externally, and let out the air, which was not in the leaft putrid, and was contained in a fac tolerably fmooth on its infide, made up of compreffed cellular membrane, the abdominal mufcles and tendons forming the pofterior furface, which extended as low as the inferior edge of Poupart's ligament. The contents of the abdomen were tolerably found; but when I infpected the vifcera 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 thefe adhefions, I got into a cavity between the bladder, uterus, and vagina, on the right-fide fomething like an abfcefs. From the right-fide of this cavity there was a canal afcending to the brim of the pelvis, in the courfe of the round ligament, as far as to the going out of the iliac veffels, veſſels, which it ſeemed to accompany, and when it paſſed from behind Poupart's ligament, communicated with the tumor abovementioned. I next 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 tumor before death, and the particular account I have given of the diſſection, the reader may be able to make his own obſervations, and 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-houſz[a].

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

[a] Exp[ts] upon Vegetables; proving their great Power of purifying the common Air, &c.

air

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, commonly, contains a great deal of air; therefore the globules of air might as readily come from the water as from the body; this circumſtance 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[a], 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°, 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

[a] "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 during bathing 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. During bathing in the bath of Buxton, the human body, after being immerſed, and kept at reſt in it for ſome time, is covered with air-like bubbles; but theſe bubbles appear, in the ſame manner, on any ſolid body whatever that may be 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."

further

further proved, by making an experiment, with only this difference, that the iron is to be ten degrees colder than the water; in this cafe little or no air will be feparated, and of courfe no bubbles be obferved. The bubbles of air do not appear to arife entirely from the degree of warmth of the water, but alfo in fome meafure from a folid body being immerfed in it, which appears to have a power of attracting the air, whofe affinity to the water is now weakened by heat; for fimply heating the water to the fame degree will not feparate the air, as we find, by experiment, that no bubbles are produced: and this power of attracting the air appears, in fome meafure, to depend upon the folidity of the body immerfed; at leaft, bodies have a greater number of bubbles in proportion to their folidity: for upon making comparative experiments between iron, ftone, wood, and cork, the air feparated from the water upon the furface of the iron and ftone is in confiderable quantity; that upon the wood very fmall, and fcarcely any at all upon the cork.

It is perhaps impoffible to determine, with abfolute certainty, the feat of digeftion; but it is more than probable it is principally in the ftomach: this, however, will not be equally the cafe in all animals. We may venture to affirm that, in the long contracted œfophagus of the quadruped, digeftion does not take place; and that the fecretion of this part is a flimy mucus, poffeffed of no power fimilar to that of the gaftric juice, being only intended to promote the eafy paffage of the food; while the lower end of the œfophagus in birds is exceedingly glandular, fecretes the principal part of the gaftric juice, and is a fubftitute for the deficiency of the fecretion in the ftomach of this clafs of animals, which in fome is lined with a horny fubftance, and in others with a cuticle. But even in birds, the feat of digeftion is chiefly in the ftomach; the juice fecreted in the lower part of the œfophagus being conveyed into that cavity. The mucus fecreted by the other parts of the œfophagus, fuch as the crop in thofe who have one, has no fuch power. It is poffible, however, that digeftion may go on in the lower part of the œfophagus; for if any digeftible fubftance fhould be retained in it, as may happen in many of thofe who receive whole animals into the ftomach, as the gull and heron, who fwallow fnakes and fifh entire, the tails of which may remain in the œſophagus till the head is digeſted; in ſuch a caſe the tail itſelf may likewiſe be acted upon. 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; it 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, it may be ſet down as a fact, that digeſtion goes on in the fourth cavity. This is beſt proved by feeding the animal with a ſubſtance that does not require any kind of preparation for digeſtion, ſuch as milk. Let 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, and 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 probable that digeſtion is likewiſe carried on in the duodenum, eſpecially in its upper part; which may ariſe from two cauſes; one, the inteſtine moſt probably ſecreting the ſame juice with the ſtomach; the other, ſome of the gaſtric juice, and alſo part of the food, having paſſed into the inteſtine before it had been converted into chyle.

Although the ſtomach is the ſeat of digeſtion, it is not ſolely appropriated to that purpoſe: in many animals it is 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

merely a reſervoir, and in this reſpect is 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 they return the food, as they have not the power to maſticate. In ſome animals, who do not ruminate, there is not the ſame neceſſity for diſtinct pouches ; the ſtomach therefore conſiſts either of one bag, ſingly ; or with appendages, as in the pecari. But the whole of this bag 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, which is 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 inſtances of this. This circumſtance takes place, in a ſmaller degree, in the horſe. This increaſe of the cavity of the ſtomach, and its having appendages, beyond what is neceſſary for digeſtion alone, is peculiar to thoſe animals who take in more food than what is immediately wanted, or which requires a certain degree of preparation prior to digeſtion. The crop in the eagle is of the firſt kind ; and the crop in the gallinaceous fowls, and the firſt ſtomach in the ruminating animals and porpus, is of the ſecond. It is the diſpoſition of ſuch animals to fill theſe cavities, and the quantity they contain, makes them ſeldomer require being filled ; it is probably the ſenſation produced from this fulneſs which gives ſatisfaction to the animal, and takes off the further deſire for food, ſimilar to what happens from filling the ſtomach itſelf of other animals, who having no ſuch proviſion, are longer and oftener employed in purſuit of food.

I ſhould be apt to ſuppoſe the power of the gaſtric juice to coagulate milk and ſome other animal mucilages[a], is 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 ; as milk, and many other natural ſubſtances, require being co-

[a] 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, the ſame as if boiled ; the cryſtalline humour, in the ſtomachs of fiſhes, is likewiſe found coagulated as if boiled.

agulated before they can be digefted. I have found this coagulating power in the ftomach of every animal which I have examined for that purpofe, from the moft perfect down to reptiles; and thefe appendages which I have confidered only as refervoirs preparatory to digeftion, as the firft ftomach in the ruminating animal, and the crop in birds, have no fuch power.

The gaftric juice is a fluid fomewhat tranfparent, and a little faltifh or brackifh to the tafte. Whether this faltifhnefs is effential, or only accidental, is not eafily determined. Indeed, there are very few of our fecretions which have not fome falt in them; for it is found in the tears, the faliva, the fecretion of the glans penis, of the glands of the urethra, and in the firft and the laft milk fecreted in the udders of animals.

I fhould not be inclined to fuppofe that there is any acid in the gaftric juice as a component or effential part of it, although an acid is very commonly found, even when no vegetable matter has been introduced into the ftomach[a]. The acid is increafed in fome difeafes, and in others the difpofition to form it may be deftroyed; which may be the reafon why, by a kind of inftinctive principle, many girls are fond of eating four fruit, and of drinking vinegar; while others, on the contrary, from a different caufe, often eat chalk, lime, and other fubftances of that kind: but the acid not being always found, it is not yet determined on what occafions it is formed, or in what manner it is deftroyed.

[a] The only trial to which I ever put the gaftric juice, (to afcertain if it was acid) was with the fyrup of violets; and in many of the trials the colour of the mixture was changed to red: but it is neceffary for the accuracy of the experiment, which is to determine this fact, that the animal fhould not be fed upon vegetables for fome time before the trial is made, they being liable to become in fome degree four; therefore it is hardly fair to make the experiment on the contents of the ftomach of animals who live upon vegetables. In many trials of this kind, we may be deceived, and led to fuppofe an alkali. For certain animal fecretions being of a yellow caft, when fuch are mixed with the fyrup 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 fcarlet, by being then a mixture of red and yellow; but if the fecretion is not only of a yellow colour, but of an alkaline nature, it will alfo continue green; and by adding a little more acid than what faturates the alkali, the colour will then become that of orange.

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The procefs of digeftion differs from every other natural operation in the change of bodies. It is by no means fermentation, though it may fomewhat refemble it. For fermentation is a fpontaneous procefs, and is that natural fucceffion of changes by which vegetable and animal matter is reduced to earth; therefore muft be widely different from digeftion, which converts both animal and vegetable fubftances into chyle; in the formation of which there cannot be a decompofition, fimilar to fermentation.

Digeftion is very different from chymical folution, which is only an union of bodies by elective attraction, not a real change of the fubftances themfelves, but of their properties. But digeftion is an affimilating procefs, and in this refpect is fomewhat fimilar in its action to morbid poifons. It is a fpecies of generation, two fubftances making a third; but the curious circumftance is its converting both vegetable and animal matter into the fame kind of fubftance or compound, which no chymical procefs can effect. The chyle is compounded of the gaftric juice, and digeftible fubftances when perfectly converted; and it is probable that the quantity of gaftric juice is nearly equal to that part of the food that is really converted into chyle; if fo, it demonftrates the neceffity of a very quick fecretion, to fupply a quantity fo very confiderable; but it is not loft to the conftitution.

The progrefs of the converfion of food into chyle, is often well feen in the ftomach of animals at different times after feeding, or even in the fame meal. Fifhes are good fubjects on which to make obfervations for this purpofe, as they fwallow their food whole; that food is commonly fifh, and often too large to be completely admitted into the ftomach. As they do not mafticate their food, it is not adapted to the cavity of the ftomach; and we therefore often find part of it lying in the œfophagus, a circumftance from which the comparative progrefs of digeftion becomes more obvious.

It may alfo be well obferved in the ftomach of a dog, in which the whole that it contains has been fwallowed at the fame time. In the great end the food will be but little altered; towards the middle, more; and towards the pylorus, will be fimilar to what is found in the duodenum.

From the ftructure of the ftomach in ruminating animals, they are badly adapted to affift our inquiries on this fubject; becaufe whatever is fwallowed in a hard folid form, and unfit for digeftion, requiring to be ruminated, as metalic balls, will often be thrown out when returned into the mouth to be mafticated; or it may lie a long time in the firft ftomach without being either thrown up or paffed into the fourth, as I have frequently feen; therefore the chance of its getting into the fourth ftomach in a proper time to fit it for the object of an experiment, being very uncertain, no great light can be derived from trials on animals of this clafs.

Live or frefh vegetables, when taken into the ftomach, are firft killed, by which a flabbinefs in their texture is produced, as if boiled, and then they begin to be acted upon by the gaftric juice.

Meat appears to undergo no change, as preparatory to digeftion, but feems at once to fubmit to its union with the gaftric juice: for after it has been acted upon, firft, it appears to lofe its texture; then becomes cineritious in colour; next gelatinous; and laft, chyle. The firft change made upon milk, and fome other fecretions, as the yolk and white of an egg, is coagulation; after which the gaftric juice begins to acquire a power of union with them.

The firft change which is produced on animal fubftances, out of the body, when either expofed to heat, or becoming putrid, is fimilar to the fecond of the three changes which takes place in digeftion, and is only preparatory to the complete change, whether digeftion or putrefaction.

It appears from many experiments, that the digefted or animalized part, when carried into the inteftine, is attracted by, or clings to its villous coat as if entangled among the villi; while the excrementitious part, fuch as bile, is found lying unconnected in the gut, as if feparated from the other.

The food of moft animals confifts either of vegetable or animal fubftances; and vegetables feem intended to fupport one clafs, with a view to its being the food of another. Although there are claffes of animals intended to fubfift on each particular kind of food, yet they do not all invariably keep to the fame kind in every ftage of life; many being nou-

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riſhed 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 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 more than of one kind. This is the caſe with inſects in general, and the ſilk-worm will ſcarcely touch any thing but mulberry-leaves. 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, they 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: we may obſerve that, nature has given us very few fluids as articles of food; and to render thoſe few fitter for the action of the digeſtive powers, a coagulating principle is provided to give them ſome degree of ſolidity[a]. It is not eaſy to aſſign a reaſon for a fluid ſtate being unfavorable to digeſtion; more particularly as it ſeems eſſential to fermentation and chymical ſolution. The neceſſary degree of ſolidity is, I ſhould ſuppoſe, 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, all the correſponding parts being 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

[a] The circumſtance of the cryſtalline 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.

that ſort of food the animal is by nature intended to live upon; 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 in the ruminating tribe renders the food fit to be ſwallowed, to undergo that preparation in the firſt ſtomach which 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 defended from the powers of digeſtion. And the third is, that which bruiſes and divides the food by chewing, before it is received into the ſtomach, as happens to moſt vegetables; which maſtication, although of conſiderable ſervice, is not abſolutely neceſſary. It however produces great ſaving in food.

The huſk, of all the ſeeds of plants, although a vegetable ſubſtance, appears to be indigeſtible in a 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, is 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 digeſtor 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, as the huſk has probably no other power of reſiſting putrefaction but what ariſes from its texture. 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, yet they allow of tranſudation, and the ſeed is in ſome degree affected by it, which is known by its ſwelling in the ſtomach; yet it can only take up a certain proportion of it, but not ſufficient to con-

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vert it into chyle, the gaſtric juice having no power of action upon the huſks themſelves.

The eſſential oils of vegetables and animals are indigeſtible; but are ſoluble either in the gaſtric juice or chyle, by which means they become medicinal, from their ſtimulating powers. The eſſential oil of vegetables, but more particularly that of animals, would ſeem 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 of digeſtion than common meat, from the animal ſubſtance being guarded by the earth. Thus the animal part of a bone is leſs readily ſoluble in an alkali than fleſh, or even the animal part when deprived of its earth by an acid; nor will a bone ſubmit to putrefaction ſo readily as meat, being guarded by the calcarious earth; therefore animals who live upon others, and ſwallow them whole, as the heron, more eaſily digeſt bone than thoſe who are not accuſtomed to ſwallow bones, as the crow and magpye, who commonly only pick the fleſh.

The degree of eaſe, or the contrary, 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, are digeſtible in the order here put down.

There is not only a difference in the degree of readineſs with which the various kinds of natural food are digeſted; but theſe can alſo be made to undergo changes by art, that render them ſtill more eaſy of digeſtion. For it appears that both boiled and roaſted, and even putrid, meat is eaſier of digeſtion than raw; at leaſt I have found it ſo in my experiments.

ments. This may be ſuppoſed to ariſe in the two firſt, from their juices being coagulated; but 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. It may be obſerved, that what is eaſy of digeſtion in one ſtomach will not be ſo in another; in which laſt caſe the ſtomachs, I believe, are not healthy.

The whole of the food in many animals appears not 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. This circumſtance of ſome part of the food, though digeſtible, not being acted upon by the gaſtric juice, may ariſe from two cauſes; firſt, many parts of vegetables being too firm in texture to be digeſted in the ſame time with the other food, are 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 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, as is the caſe with thoſe who go to reſt in the winter.

The powers of digeſtion may, in ſome inſtances, be aſcertained by the appearance of the excrement, in which if the food appear not to be much altered, we may conclude, that this power 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 eat in proportion to the quantity of nouriſhment contained in the food, of which the ſtomach, from inſtinct, appears to be ſenſible; and alſo in proportion to the powers 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; not having the power of diſſolving the vegetable, only of extracting a juice or infuſion

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from it; for the bit of leaf comes away entire, being coiled up and hardened; but by being put into water unfolds like tea.

There are few animals that do not eat animal food 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 that natural law, but 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; 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, each of which is 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 ſome degree; for other circumſtances muſt be attended to, as the mode of digeſtion, and whether the animal has a cæcum and colon, with their peculiar form, all which varieties have a connection with the changes the food undergoes. Vegetable food produces more excrement than animal, and this according to the kind or parts of vegetables. 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 had 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: however, ſuch animals have either no cæcum or colon; or if they have, it is very ſhort, ſo that the excrement is not long retained, therefore 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 moſt 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.

Thoſe animals which feed upon vegetables alone, commonly have their fæces ſomewhat ſolid; but this will vary according to the ſtate of the vegetable, whether green or dried; therefore the kind of fæces would ſeem to depend on the nature of the indigeſtible part of the food, and muſt vary according to the digeſtive powers in different animals: an animal that feeds upon graſs, has the fæces much ſofter than the ſame animal when fed on the ſame kind of graſs made into hay, the fæces of the herbivorous animals being 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

fæces, during ſummer and winter, is inconſiderable. Moſt quadrupeds, and birds, that live principally upon vegetables, have their cæca large, and the colon long, as many of the ruminating animals. Some have the colon both long and large, as the horſe, and the rat tribe; which circumſtance has conſiderable effects on the fæces, allowing them 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, that live 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 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 in the dead body. An experiment, although it may be very well and accurately made, ſo far as the experiment goes, if it does not preſerve a cloſe connection 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, the gaſtric juice 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 death, 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 very few ſtomachs that have not, 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, publiſhed at Edinburgh 1777, gives a number of experiments on digeſtion, ſome of which are well deviſed, to aſcertain the ſubſtances that are eaſieſt of digeſtion, the thing in fact more wanted, than the cauſe of that proceſs; but many of his experiments, more eſpecially thoſe on ruminating animals, are 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 could have been wholly digeſted in any ſtomach. His experiment which was 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; which circumſtance 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: he is inclined, however, to ſuppoſe that the living powers of the animal may be a guard againſt ſuch effects; but is ſtill diſpoſed to fear that, in all caſes theſe may not be ſufficient.

The living power, in the ſtomach, muſt be indeed very weak to allow of its being digeſted; and in that caſe I ſuſpect the ſecretion of the gaſtric juice would be ſo defective, as to prevent ſuch effects being produced.

Dr. Stevens gives two caſes, with the diſſection, 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 facts, before they advance them, eſpecially when theſe are either to overturn a received

received opinion, or to eſtabliſh a new one. As to the fact, of living animals being ſwallowed and digeſted, no freſh proofs are neceſſary, as we are eating 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. Goſſe, upon himſelf, and 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 theſe facts from the experiments he has made to aſcertain them, I am inclined to believe they 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 this is as follows. I was opening, in his preſence, the body of a patient of his own, where the ſtomach was in part diſſolved, which appeared to him very unaccountable, as there had been no previous ſymptom that could have led him to ſuſpect any diſeaſe in the ſtomach. I took that opportunity of giving him my ideas reſpecting it; and told him that, I had long been making experiments on digeſtion, and conſidered 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 this ſubject at ſome future period; but he deſired me to give this fact by itſelf, with my remarks, as it would prove that there is a ſolvent power in the ſtomach, and be of uſe in the examination of dead bodies.

An

An accurate knowledge of the appearances in animal bodies that die of a violent death, that is, in perfect health, or in a found ftate, ought to be confidered as a neceffary foundation for judging of the ftate of the body in thofe that are difeafed.

But as an animal body undergoes changes after death, or when dead, it has never been fufficiently confidered what thofe changes are; and till this be done, it is impoffible we fhould judge accurately of the appearances in dead bodies. The difeafes which the living body undergoes (mortification excepted) are always connected with the living principle, and are not in the leaft fimilar to what may be called difeafes or changes in the dead body: without this knowledge, our judgment of the appearances in dead bodies muft often be very imperfect, or very erroneous; we may fee appearances which are natural, and may fuppofe them to have arifen from difeafe; we may fee difeafed parts, and fuppofe them in a natural ftate; and we may fuppofe a circumftance to have exifted before death, which was really a confequence of it; or we may imagine it to be a natural change after death, when it was truly a difeafe of the living body. It is eafy to fee therefore, how a man in this ftate of ignorance muft blunder, when he comes to connect the appearances in a dead body with the fymptoms that were obferved in life; and indeed, all the ufefulnefs of opening dead bodies depends upon the judgment and fagacity with which this fort of comparifon is made.

There is a cafe of a mixed nature, which can neither be reckoned a procefs of the living body, nor of the dead; it participates of both, inafmuch as its caufe arifes from life, and the effect cannot take place till after death. To render this more intelligible, it will be neceffary to give fome general ideas concerning the caufe and effects.

An animal fubftance, when joined with the living principle, cannot undergo any change in its properties but as an animal; this principle always acting and preferving the fubftance, poffeffed of it from diffolution, and from being changed according to the natural changes which other fubftances 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 refift, viz.

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, when deprived of the living principle, and entirely to decompoſe it. The number of powers, which thus act differently on the living and dead animal ſubſtance, is not aſcertained: we ſhall take notice of two, which can only affect this ſubſtance when deprived of the living principle; which are, putrefaction and digeſtion. Putrefaction is an effect which ariſes ſpontaneouſly; digeſtion is an effect of another principle acting upon it, 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 living in the ſtomach, or even hatched and bred there: but the moment that any of thoſe 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 would immediately act upon it.

Indeed, if this were not the caſe, we ſhould find that the ſtomach itſelf ought to have been made of indigeſtible materials; for if the living principle was not capable of preſerving animal ſubſtances from undergoing that proceſs, the ſtomach itſelf would be digeſted.

But we find on the contrary, 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, either by the digeſtive powers of other ſtomachs, or by the remains of that power which it had of digeſting other things.

From theſe obſervations we are led to account for an appearance which we often find in the ſtomachs of dead bodies; and at the ſame time they throw

throw a conſiderable light upon the nature of digeſtion. The appearance which has been hinted at, is a diſſolution of the ſtomach at its greateſt extremity; in conſequence of which, there is frequently a conſiderable apperture made in that viſcus. The edges of this opening appear to be half diſſolved, very much like that kind of diſſolution which fleſhy parts undergo when half digeſted in a living ſtomach, or when diſſolved 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. In many ſubjects this digeſtive power extends much further than through the ſtomach. I have often found, that after it had diſſolved the ſtomach at the uſual place, the contents of the ſtomach had come into contact with the ſpleen and diaphragm, had partly diſſolved the adjacent ſide of the ſpleen, and had diſſolved the diaphragm quite through; ſo that the contents of the ſtomach were found in the cavity of the thorax, and had even affected the lungs in a ſmall degree.

There are very few dead bodies in which the ſtomach is not, at its great end, in ſome degree digeſted; and one who is acquainted with diſſections, can eaſily trace the gradations from the ſmalleſt to the greateſt.

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 the inner ſurface; what is found will appear ſoft, ſpongy, and granulated, and without diſtinct blood-veſſels, opake and thick; while the other 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 appear like drops on the inner ſurface.

Theſe appearances I had often ſeen, and I do ſuppoſe that they had been ſeen by others; but I was at a loſs to account for them; at firſt, I ſuppoſed them to have been produced during life, and was therefore diſpoſed to look upon them as the cauſe of death; but I never found that they had any connection with the ſymptoms: and I was ſtill more at a loſs to account for theſe appearances, when I found that they were moſt frequent

frequent in thoſe who died of violent deaths, which made me ſuſpect that the true cauſe was not even imagined[a].

At this time I was employed in making many experiments upon digeſtion, in different animals, all of which were killed, at different times, after being fed with various kinds of food; ſome of them were not opened immediately after death, and in ſome of them I found the appearances abovedeſcribed in the ſtomach. For purſuing the inquiry about digeſtion, I procured the ſtomachs of a vaſt variety of fiſhes, which all die of violent deaths, and may be ſaid to die in perfect health, with their ſtomachs commonly full. In them we ſee the progreſs of digeſtion moſt diſtinctly; for as they ſwallow their food whole, that is, without maſtication, and ſwallow fiſh that are much larger than the digeſting part of the ſtomach can contain, the ſhape of the fiſh which ſwallows being very favourable for this inquiry, we find in many inſtances the part ſwallowed, which is lodged in the digeſting part of the ſtomach, is more or leſs diſſolved, while that which remains in the œſophagus is perfectly ſound: and in many of theſe I found, that the digeſting part of the ſtomach was 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

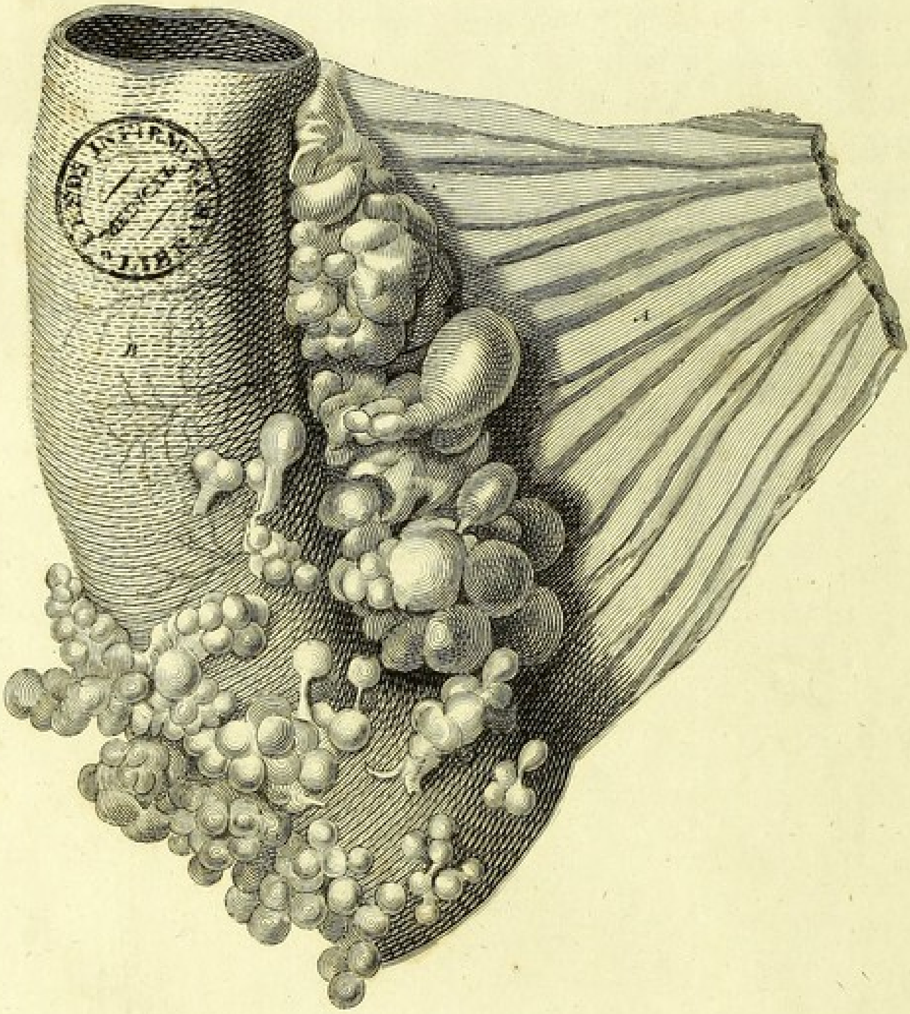
[a] The firſt time that I had occaſion to obſerve this appearance, in ſuch as died ſuddenly from violence in the living body, and in whom therefore I could not eaſily ſuppoſe it to be the effect of diſeaſe, was in a man who had his ſkull fractured, by one blow of a poker. Juſt before this accident, he 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 theſe 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, wheenver 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.

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. With this idea, I ſet about making experiments to produce theſe appearances at pleaſure, which would have taught us how long the animal ought to live after feeding, and how long it ſhould remain after death before it is opened; and above all, to find out the method of producing the greateſt digeſtive power in the living ſtomach.

Theſe appearances throw conſiderable light on the principles of digeſtion, and ſhow, that it is neither a mechanical power, nor contractions of the ſtomach, nor 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 this juice is confined or limited to certain ſubſtances, eſpecially 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 continuance.

EXPLANATION

Plate



W^m. Bell del.   W^m. Skelton sculp.

EXPLANATION OF THE PLATE.

A Portion of inteftine of a hog, the peritoneal coat of which is covered in feveral places with fmall pellucid cyfts, containing air.

It was fent to me by my friend Mr. Jenner, furgeon, at Berkley, who informed me, that this appearance is found very frequently upon the inteftines of hogs that are killed in the fummer months.

A The portion of the mefentery.

B The portion of inteftine on which the air-cells are fituated.

EXPLANATION OF THE PLATE.

A [illegible] (inter)ſtices of a Hog, the [illegible] of which is [illegible] natural place with [illegible] [illegible] [illegible] was ſent to me by my friend [illegible] [illegible] [illegible] informs me, that this appearance is found very frequently upon the in[testines] of Hogs that are killed in the ſummer months.

A [illegible]. The portion of the inteſtine.
B [illegible]. The portion of inteſtine on which the [illegible] are formed.

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 in which it is 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 are in ſome reſpects ſimilar to thoſe of another, having hardly 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

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 this 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 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 nouriſhment proper for their offspring, during this ſecond ſtage, immediately from their own bodies; which mode of nouriſhment has hitherto been ſuppoſed to be peculiar to that claſs of animals which Linnæus calls Mammalia; nor has it, I imagine, been even 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

paroquet

paroquet 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 thickened, and takes on a glandular appearance, having its internal ſurface very irregular[a]. It is likewiſe evidently more vaſcular than in its former ſtate, that it may convey a quantity of blood, ſufficient for the ſecretion of this 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 young, its crop, when examined, will be diſcovered to contain the ſame curdled ſubſtance; which paſſes from thence into the ſtomach, where it is to be digeſted.

The young pigeon is fed for ſome time with this ſubſtance only, and about the third day, ſome of the common food is found mingled with it; and 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

[a] Vide Plate II.

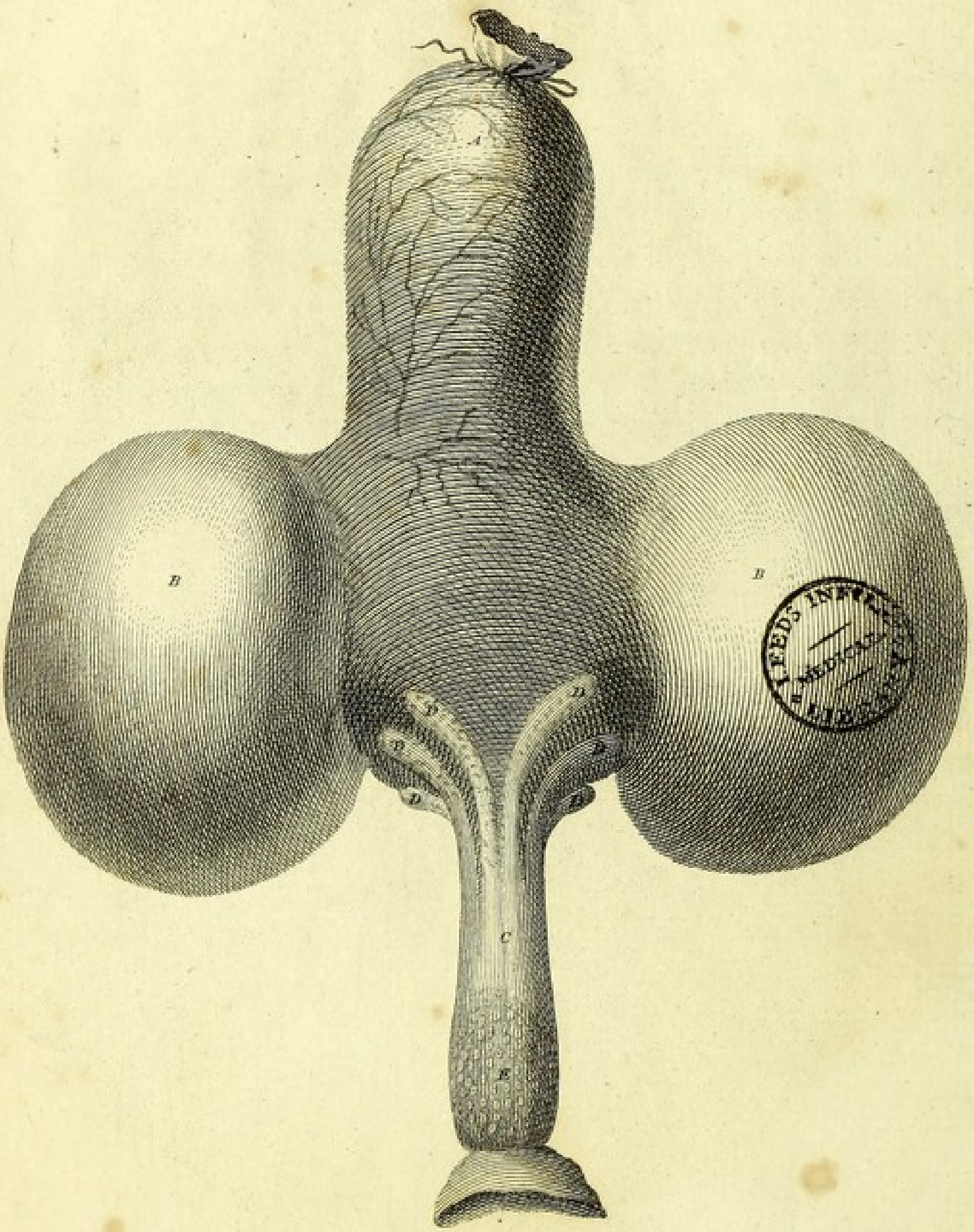
be

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.

Plate I.



Wm. Bell del.

Wm. Skelton sculp.

PLATE I.

THE crop, taken from a pigeon when it had no young ones. The crop in the pigeon is probably more in the middle of the neck than in any other bird, being two equal bags, as it were, paffing out, laterally, from the œfophagus; while in moft other birds it is a little on one fide. The œfophagus of thofe birds who have crops, may be divided into two, a fuperior and inferior. The fuperior is that which leads from the mouth to the crop; the inferior, from the crop to the gizzard.

The crop was inverted and diftended with fpirits. It fhows the appearance of its internal furface.

A The inner furface of the fuperior œfophagus.

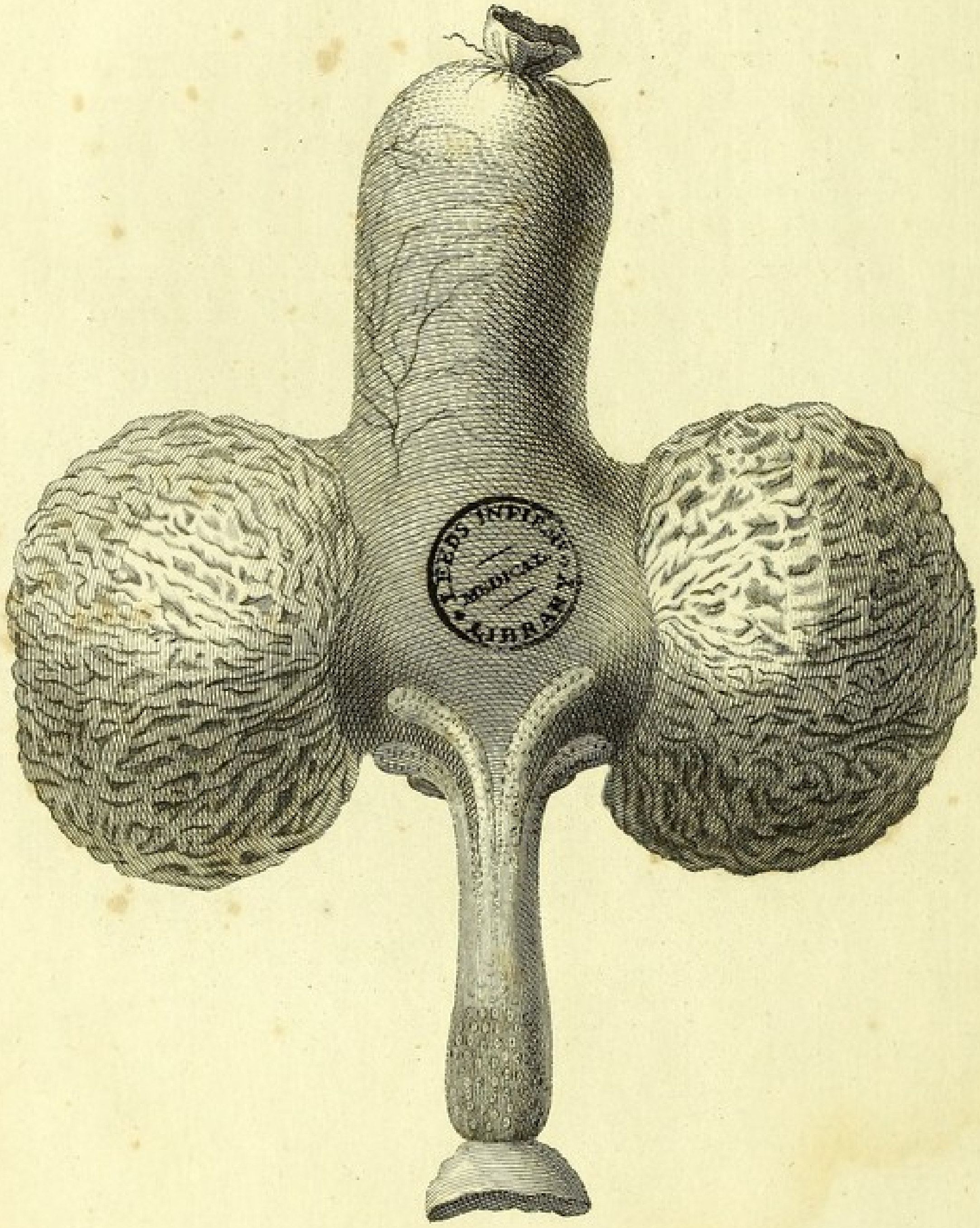
B B The infide of the two projecting bags of the crop.

E The inferior œfophagus, leading from the crop to the gizzard.

D D D D Glands fituated on the lower part of the crop, and continued into the inferior œfophagus.

E A glandular ftructure upon the inner furface of this œfophagus, juft before it terminates in the gizzard, for the purpofe of fecreting a fubftance analogous to the gaftric liquor.

Plate II.



Wm. Bell del. Wm. Skelton sculp.

PLATE II.

THE crop, from a male pigeon, while the female was breeding, to ſhow 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 I; 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.

PLATE [illegible]

THE crop, from a male pigeon, when the [illegible], to shew the change which takes place at that time [illegible] the internal surface, for the purpose of secreting a [illegible] in the [illegible]

[illegible] in the [illegible] in the appearance of the [illegible] of the [illegible] which is not [illegible]

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 in 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 ſubſtance, and its uſe, confining myſelf to the conſideration of that portion which lines the tunica choroides in the claſs Mammalia and in birds: in doing this I ſhall alſo take notice of the difference of colour occurring in animals of the ſame ſpecies. Although the appearance of this ſubſtance in the eyes of ſome fiſhes might illuſtrate the ſubject, we cannot avail ourſelves of it, as we are not 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[a]. Conſiderable varieties are produced under ſuch

[a] 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

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 is 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. As this alteration in colour is 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 black of that kind, 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[b].

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[c]. The colour differs in different claſſes of animals, often in the ſame claſs, and even in the ſame ſpecies. In the human ſpecies it is moſt commonly dark; in the ferret kind always light: and 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

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.

[b] In vegetables, I believe, it invariably holds good, that however improved by culture, if neglected, they ſoon degenerate into their firſt ſtate.

[c] 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.

partly

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 deviations are ſo extraordinary, as with propriety to be denominated monſtrous[a].

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 in individuals of the ſame ſpecies, and this difference is ſometimes ſtarting up in the offspring 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; 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 cha-

[a] 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.

racteriſtics, 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 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 ſpecies, 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 fœtal, 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 fœtus; the 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[a]. This difference in the colour of the hair, on different parts of the body, is not ſo obſervable 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,

[a] The hair growing grey, is not in the leaſt to the preſent purpoſe.

the

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 eyelids 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

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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 luſus 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 to 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 juſt 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 the nerve. This ſhape is peculiar to the cat, lion, dog, and moſt of the carnivorous tribe; in the herbivorous, the upper edge being irregular; however, in the ſeal, the light part of this pigmentum is equally diſpoſed all round the optic nerve,

nerve, and is, on the whole, broader than it is commonly found in quadrupeds. How far this increafed furface is an approach towards the fifh kind, in which it is wholly of this metallic white, I will not pretend to fay; but it is probable that, as the animal is to fee under water as well as in the air, its being circular, may be for its better correfponding with the form of the eyelids, which open equally all round; this feems alfo to correfpond with what is obfervable in fifhes, as they are without eyelids.

The colour of the pigmentum, whether white, or green, or both, has always a bright furface, appearing like polifhed metal; which appearance, animal fubftance is very capable of taking on, as we fee in hair, feathers, filk, &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 vifion, in both cafes; whether thefe effects are fimilar, or if one cafe illuftrates the other.

It may be afferted 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 vifion of thofe of the fame fpecies who have the pigmentum wholly dark, with thofe who have it perfectly white; and fomething may be learned, by a fimilar comparifon of animals who have it only in part white, with thofe which have it entirely dark, although they are of different fpecies, as it is reafonable, from analogy, to fuppofe that fome fuch effect is produced in the eye which is poffeffed of both.

I fhall firft confider the effect produced when the white or light colour makes only part of the pigmentum. This will lead me to obferve, that all animals having the pigmentum diverfified, though they are capable of bearing as much light as others, and can fee as perfectly when light is in an equal degree; can likewife fee very diftinctly when the light is much lefs than ferves the purpofes of animals having it wholly dark. May we not, therefore, afcribe this advantage to the pigmentum being partly white? One might be almoft tempted to fuppofe, that fuch 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 inlarged and illuminated; but in a full light that 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; in the cow it is a light green.

I ſhall ſecondly, conſider thoſe which have the whole pigmentum of a white colour, whether it is accidental or natural, and which 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; this colour, moſt probably, ariſes from the blood in the veſſels of the choroid coat; I have 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[a]. This ſeemed to be the caſe in a boy at Shepperton, when about three years of age, and 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 twi-

[a] 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 conceived I ſaw the light paſſing through the ſubſtance of the iris.

light

light is lefs offenfive. When in a room, he turns his eyes from the window; and when made to expofe his face to the light, or when out in the open air, he knits his eyebrows, half fhuts his eyelids, and bends his head forwards, or a little down: yet the light appears lefs obnoxious to him now than formerly, probably from habit. Such perfons appear to be nearer fighted than people in common; but I apprehend that arifes from the pofition into which the eyelids and brows are thrown, which not only in a great degree excludes the light, making the object faint in proportion to the contraction of the pupil and fhade made by the eyelids and eyebrows; but at the fame time fits the eye to fee near objects: for if we nearly clofe our eyelids, and knit our eyebrows, we can fee a fmall object much nearer than if we did not perform fuch actions; and it will make above a foot difference in the focal diftance of the eye. In many rabbits who have white eyelafhes, and in white mice, the pigmentum is entirely white. The fame thing is to be obferved in a certain diftinct fpecies of animals, the ferret, which we have adduced as an example of the pigmentum being naturally white: thefe animals being intended to fee in the dark, and their mode of life not expofing them to the light, they are liable, in a much greater degree, to be affected by ftrong light.

If it is allowed as probable that, in animals having the pigmentum diverfified, the object to be viewed is thrown upon the lighter coloured portion; how does it happen that fuch are able to bear the light better than thofe who have the pigmentum altogether of a light colour? Perhaps it is not the illuminated object itfelf that is offenfive to the retina; but that diffufion of light in the one kind of eye, which does not happen in the other.

Having ftated the facts, and the general effect arifing from the diverfified pigmentum, let us next confider the manner in which the effect is produced;—That fuch animals fee better with little light, than thofe which have the pigmentum wholly black.

Let us here fuppofe the retina to be the organ of fight; and that by the rays which fall upon it being properly refracted, it gives or conveys to the mind an idea of a diftinct object, correfponding with the fenfation of touch. This is the moft common and fimple manner in which vifion

is

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 passes through the pupil, and 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

from whence they came. This ſeems to be in a great meaſure the caſe, by the conſiderable degree of illumination from 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 moſt parts requiring a variety of motions, it became neceſſary to have a variety of muſcles ſuited to theſe 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 happens in moſt joints. And in a whole part, deſtined to a variety of motion, 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 from one body to another, as would permit its being impreſſed

by

by the various ſurrounding objects; and it was alſo neceſſary, that there ſhould be a power capable of keeping the eye fixed, 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 which are 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, but alſo neceſſary that there ſhould be a power to keep it fixed on any one object the mind might be attentive to; 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 progreſſively 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 all which 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

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 commands 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 reſpecting the pupil of the eye, and not in the leaſt allowed to move over its ſurface[a]. 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

[a] 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.

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 heads 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 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 compounded.

A DESCRIPTION

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. The fourth and ſixth pair of nerves are remarkable inſtances of this; and we may reaſonably conclude, that every part has its particular branch allotted to it; 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 more than mere mechanical convenience. For many variations have been deſcribed in the diſſections of nerves, which I believe to have ariſen from the blunders of the anatomiſt, rather than from any irregularity their number, mode of ramifying, courſe, diſtribution, or connection[a] 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 ſo neceſſary it ſhould be uniform; it not being very material to a part by what channel the blood is conveyed; 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 ariſing from the brain, and how different the circumſtances attending them, we muſt ſuppoſe a variety of uſes to ariſe out of this peculiar 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, muſt be exact, as there are parts whoſe actions immediately depend upon ſuch circumſtances. The brain may be ſaid to have an intelligence with the body; but no ſuch intercourſe ſubſiſts between the different parts of the body and the heart.

[a] 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

In the ſummer of 1754, I employed myſelf principally in diſſecting the nerves paſſing out of the ſkull; in doing which 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[a]. 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; eſpecially as it was ſummer, the acid was not diluted with water, but with ſpirit. When the bones were rendered ſoft, I purſued my intention, and in the courſe of the diſſection diſcovered the firſt pair of nerves; 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[b]. 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 thoſe have nothing to do with the ſenſe of ſmelling; for it is 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 ſenſation; and alſo

[a] 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.

[b] 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 ſhowed 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 ſhows 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.

different

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, which only convey common ſenſation, we may ſuppoſe are 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 theſe 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[a].

The

[a] 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

of

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[a]." 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 ſubſtance, while the others appear to conſiſt of medullary alone; and

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.

[a] Elementa Phyſiologiæ, vol. 5. page 151.

ſecondly,

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 its lying in a groove, made by two convolutions of the brain. Its 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 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 origins. As the branches of the nerve paſs through this bone, they ſeem to take proceſſes from the dura mater along with them; they then become 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. Theſe 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 in 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 theſe 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

down a little radiated, and the branches, eſpecially at the upper part, or at their firſt ſetting out, join 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 curvated pendulous edge to the concave ſide; but ſome paſſing through the ſubſtance of that bone, 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 it is reaſonable to ſuppoſe that they go through the inferior turbinated bone in the ſame manner, ſince we find ſimilar holes there.

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, it 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 paſſes backwards and inwards, through the foramen commune, between the orbitar proceſs of the palate, and the root of the ala of the ſphenoid bone, a branch of which gets into a fiſſure, and 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 contrary direction 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, then 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 makes 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

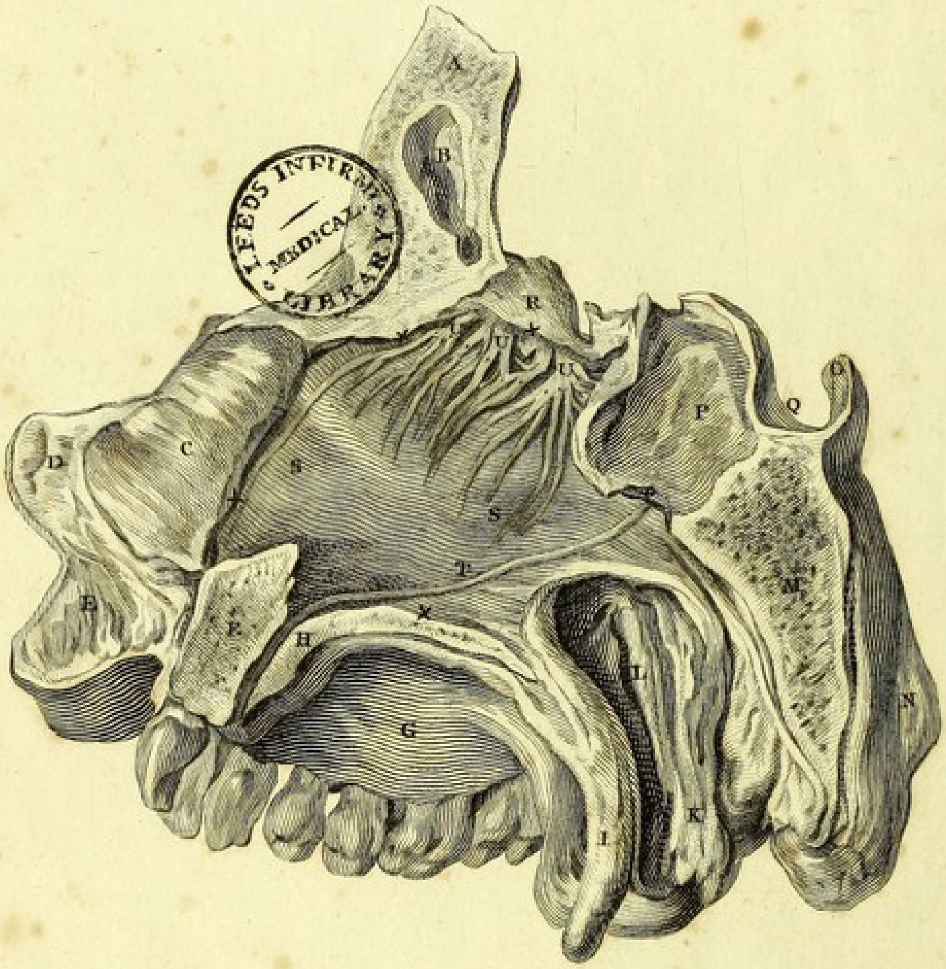
branch, called chorda tympani, I ſhall not deſcribe; I am almoſt certain it is not a branch of the ſeventh pair of nerves, but the laſt-deſcribed branch from the fifth pair; for I think I have been able to ſeparate this branch from the portio dura, and have found it lead to the chorda tympani; perhaps, is continued into it; but this is a point very difficult to determine, as the portio dura is a very 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.

PLATE I.

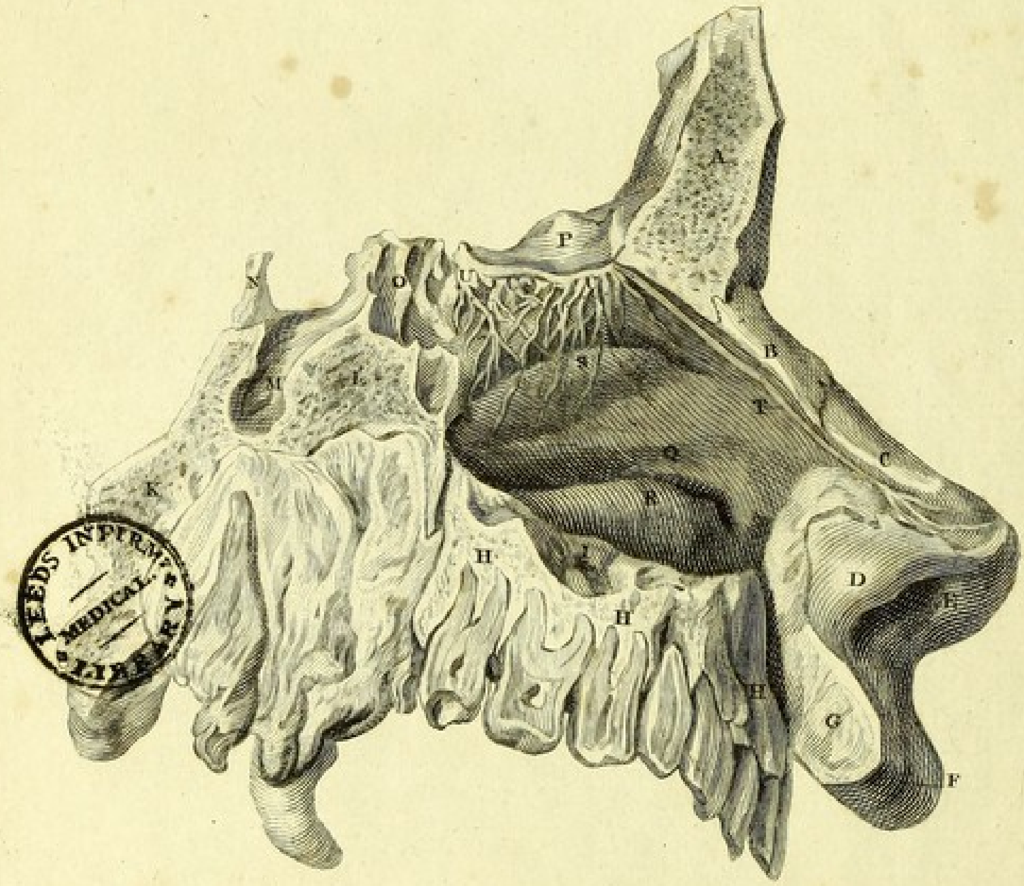
THE olfactory, or firſt pair of nerves, as they are ſeen upon the membrane of the ſeptum narium.

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 ſella 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 ſpheno-palatinum.
UUU The firſt pair of nerves, having paſſed through the cribriform plate of the ethmoid bone, ramifying on the membrane of the ſeptum.



J. V. Reimsdyk del. W. Sharp sculp.



J. V. Reimsdyk del. W. Sharp sculp.

PLATE II.

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 I.

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 Sneider's membrane that covers the turbinated bones.

THE END.

PLATE II.

This [illegible], [illegible] part of the [illegible], as they are [illegible] upon the [illegible] of the [illegible], [illegible] the [illegible] bones; the [illegible] parts of the [illegible] removed.

This engraving [illegible] head [illegible] plate I.

A The os frontis.
B [illegible]
C The [illegible] and [illegible] part of the [illegible].
D The [illegible]
E [illegible]
F The upper lip.
G H H The [illegible] process of the superior maxillary bone.
I [illegible]
K The [illegible]
L The [illegible]
M The groove [illegible]
N The posterior [illegible]
O The [illegible]
P [illegible]
Q The [illegible]
R The membrane, a little [illegible] where the inferior turbinated bone is situated.
S The line where the superior turbinated bone is situated.
T The [illegible] that was supposed to be left on the membrane of the [illegible].
U U U The trunk of the [illegible] which is afterwards left upon that part of the [illegible] the turbinated bones.

THE END.

PHARMACOPOEIA

COLLEGII REGALIS

MEDICORUM

LONDINENSIS.

LONDINI:

APUD JOSEPHUM JOHNSON.

M.DCC.LXXXVIII.

Imprimatur

Hic liber, cui titulus, PHARMACOPOEIA COLLEGII REGALIS MEDICORUM LONDINENSIS.

Datum ex ædibus collegii in comitiis censoriis, Octobris *Mensis* 5^to^ 1787.

GEORGIUS BAKER, *Præses.*

Henricus Revell Reynolds,
Jacobus Hervey,
Jacobus Robertson,
Georgius Fordyce,
} *Censores.*

SACRÆ MAJESTATI

SERENISSIMI CELSISSIMIQUE

PRINCIPIS

GEORGII

TERTII,

DEI GRATIA

Magnæ Britanniæ, Franciæ, et *Hiberniæ*

REGIS,

FIDEI DEFENSORIS, &c.

Ducis *Brunſvicenſis* et *Lunenburgenſis*,

S. R. I.

ARCHITHESAURARII et ELECTORIS,

HOC OPUS,

DENUO RECOGNITUM AC RETRACTATUM,

IPSI

TANTO GRATIUS ATQUE ACCEPTIUS FUTURUM,
QUANTO AD SALUTEM PUBLICAM ACCOMMODATIUS
SUMMO CUM STUDIO ET REVERENTIA
IN MANUS ET CLIENTELAM TRADIT

COLLEGIUM MEDICORUM LONDINENSE.

At the Court at *St. James's*, the 16th of *January*, 1788.

PRESENT

The King's Moſt Excellent MAJESTY.

Lord Chancellor.
Lord Privy Seal.
Lord Steward.
Marquis of *Carmarthen*.
Viſcount *Weymouth*.
Lord *Herbert*.
Lord *Onſlow*.
Lord *Sydney*.
Lord *Hawkeſbury*.
Mr. *Pitt*.

WHEREAS there was this day read at the Board, the humble memorial of Sir *George Baker*, Baronet, Phyſician to Their Majeſties, and Preſident of the College or Commonalty of the Faculty of Phyſic in *London*, ſetting forth, that the ſaid Preſident and College have, with great care, pains, and induſtry, reviſed, corrected,

corrected, and reformed a book by them formerly publiſhed, intituled *Pharmacopœia Collegii Regalis Medicorum Londinenſis*, preſcribing and directing the manner of preparing all ſorts of medicines therein contained, together with the true weights and meaſures by which they ought to be made: which book is now perfected and ready to be publiſhed, and, it is conceived, will contribute to the public good of His Majeſty's Subjects, by preventing all deceits, differences, and uncertainties in making or compounding of medicines, if, for the future, the manner and form preſcribed therein ſhould be practiſed by Apothecaries and others in their compoſitions of medicines: The Memorialiſt therefore moſt humbly prays, that His Majeſty will be graciouſly pleaſed to enforce the obſervance thereof in ſuch manner as to His Majeſty ſhall ſeem meet:— His Majeſty this day took the ſaid memorial into His Royal Conſideration, and being deſirous to provide in all caſes for the common good of his people, and being perſuaded that the eſtabliſhing the general uſe of the ſaid book may tend to the prevention of ſuch deceits in the making and compounding medicines, wherein the lives and health of His Majeſty's Subjects are ſo highly concerned,

hath

hath therefore thought fit, by and with the advice of His Privy Council, hereby to notify to all Apothecaries and others concerned, to the intent they may not pretend ignorance thereof, that the ſaid book, called *Pharmacopœia Collegii Regalis Medicorum Londinenſis*, is perfected and ready to be publiſhed: and His Majeſty doth therefore ſtrictly require, charge and command all and ſingular Apothecaries and others, whoſe buſineſs it is to compound medicines, or diſtil oils or waters, or make other extracts, within any part of His Majeſty's kingdom of *Great Britain* called *England*, dominion of *Wales*, or town of *Berwick* upon *Tweed*, that they, and every of them, immediately after the ſaid *Pharmacopœia Collegii Regalis Medicorum Londinenſis* ſhall be printed and publiſhed, do not compound or make any medicine or medicinal receipt or preſcription, or diſtil any oil or waters, or make other extracts that are or ſhall be in the ſaid *Pharmacopœia Collegii Regalis Medicorum Londinenſis* mentioned or named, in any other manner or form than is or ſhall be directed, preſcribed, and ſet down in the ſaid book, and according to the weights and meaſures that are or ſhall be therein limited, except it ſhall be by the ſpecial direction or preſcription of of ſome learned Phyſician in that behalf. And His Majeſty doth hereby declare, that the offenders to the contrary ſhall not only incur His Majeſty's juſt diſpleaſure, but be proceeded againſt for ſuch their contempt and offences, according to the utmoſt ſeverity of Law.

W. FAWKENER.

NOMINA

NOMINA SOCIORUM
ET
PERMISSORUM
Collegii Regalis Medicorum
LONDINENSIS.

SOCII.

GEORGIUS BAKER, *Baronettus,*
Regis et Reginæ Medicus,
PRÆSES.

Ambroſius Dawſon.

Ruſſel Plumptre, *Medicinæ Regius Profeſſor Cantabrigiæ.*

Gulielmus Heberden.

Gulielmus Muſhet.

Gulielmus Pitcairn, *Electus.*

Joannes Monro, *Electus.*

Thomas Wharton.

Antonius Addington.

Ricardus Brockleſby, *Electus.*

Joannes Clerke.
Noah Thomas, *Eques Auratus*, *Medicus Regius*.
Gulielmus Cadogan, *Electus*.
Thomas Gisborne, *Electus: Medicus ad Regis et ad Reginæ Familiam*.
Thomas Healde.
Ricardus Warren, *Electus: Regis et Principis Walliæ Medicus*.
Robertus Glynn Clobery.
Clifton Wintringham, *Baronettus*, *Medicus Regius*.
Robertus Thomlinson, *Electus*, *et Thesaurarius*.
Joannes Turton, *Medicus Regis Extraordinarius*, *et Medicus Reginæ*.
Donaldus Monro.
Henricus Revell Reynolds, *Censor*.
Lucas Pepys, *Baronettus*, *Medicus Regius Extraordinarius*.
Joannes Burges.
Joannes Rawlinson.
Ricardus Budd.
Franciscus Milman, *Medicus Extraordinarius ad Regis Familiam*.
Isaac Pennington.
Carolus Elsden Bagge.
Jacobus Hervey, *Censor*, *et Registrarius*.

Devereux

Devereux Mytton.
Joannes Matthews.
David Pitcairn, *Principis Walliæ Medicus Extraordinarius.*
Franciſcus Riollay.
Robertus Knox.
Jacobus Robertſon, *Cenſor.*
Martinus Wall.
Joannes Littlehales.
Georgius Fordyce, *Cenſor.*
Gulielmus Auſtin.
Paul Jodrell, *Eques Auratus.*
Joannes Aſh.

PERMISSI.

Carolus Morton.
Joannes Baptiſta Silveſter, *Eques Auratus.*
Georgius Lamont.
Jacobus Dargent.
Edvardus Archer.
Chriſtopherus Kelly.
Thomas Milner.
Michael Morris.
Hugo Smith.
Jacobus Ford, *Reginæ Medicus Extraordinarius.*

Maxwell Garthſhore.
Joannes Morgan.
Samuel Chapman.
David Orme.
Thomas Manningham.
Joannes Hill.
Hugo Alexander Kennedy, *Principis Walliæ Medicus Extraordinarius.*
Joannes Ford.
Joſephus Allen.
Jacobus Walker.
Franciſcus de Valangin.
Gulielmus Vaughan.
Joannes Leake.
Rowland Jackſon.
Alexander Hay.
Gulielmus Saunders.
Georgius Hicks.
Joannes Coakley Lettſom.
Gilbertus Thompſon.
Jacobus Carmichael Smyth, *Medicus Regius Extraordinarius.*
Iſaac Henricus Sequeira.
Nathaniel Hulme.
Ricardus Gulielmus Stack.

Henricus Krohn.
Robertus Robertſon.
Andreas Douglas.
Joannes Hunter.
Joannes Lee.
Jacobus Sims.
Samuel Foart Simmons.
Seguin Henricus Jackſon.
Joannes Sims.
Antonius Fothergill.
Jacobus Ford, *junior.*
Stephanus Pellett.
Thomas Clerk.
Gulielmus Paine.
Gilbertus Blane, *Principis Walliæ Medicus Extraordinarius, et ad Familiam.*
Joannes Whitehead.
Gulielmus Liſter.
Robertus Freer.
Robertus Hallifax, *Principis Walliæ Medicus.*
Adair Crawford.
Gulielmus Rowley.
Joannes Meyer.
Georgius Pearſon.
Joannes Cooke.

Thomas

Thomas Watſon.
Gulielmus Woodville.
Joannes Relph.
Robertus Willan.
Gulielmus Mackinen Fraſer.
Gulielmus Robertſon.
Joannes Potter.
Samuel Ferris.
Gulielmus Fordyce, *Eques Auratus.*
Joannes Grieve.
Joſephus Phelan.
Georgius Buxton.
Gulielmus Butter.
Theodorus Forbes Leith.
Georgius Sandeman.
Gulielmus Lowder.
Thomas Dale.
Gulielmus Blackburne.
Joannes Macnamara Hayes.
Robertus Bland.
Gulielmus Hamilton.
Joannes Hamon.
Gulielmus Black.
Andreas Thynne.
David Bayford.

Benjamin Mofeley.
Thomas Skeete.
Gulielmus Macnaven.
Jofephus Hart Myers.
Lawrentius Nihell.

PERMISSI AD ARTEM OBSTETRICIAM EXERCENDAM.

Thomas Denman.
Gulielmus Ofborn.
Michael Underwood.
Carolus Combe.
Thomas Savage.
Joannes Cooper.
Joannes Squire.
Joannes Clarke.

PERMISSI EXTRA URBEM.

Meffenger Monfey.
Henricus Richardfon.
Jofephus Davifon.
Joannes Manning.
Joannes Mather.
Gulielmus Farr.
Thomas Dimfdale, *Imperii Roffici Baro.*

Gulielmus

Gulielmus Norford.
Jacobus Vaughan.
Ricardus Pultney.
Daniel Bridges.
Gulielmus Falconer.
Edvardus Spry.
Nicholſon Doubleday.
Petrus Hooke.
Jonathan Binns.
Edvardus Whitaker Gray.
Samuel Daniel.
Robertus Bree.
Chriſtopherus Mann Torre.
Joannes Fox.
Robertus Hamilton.
Carolus Brown.
Thomas Karr.
Gulielmus Lanſdale.
Joannes Atkinſon.
Julian Gartner Hall.
Edvardus Long Fox.
Joannes Ford.

PRÆFATIO.

PRÆFATIO.

HAUD multum abeſt quin dimidium ſæculum effluxerit, ex quo anteceſſores noſtri, cum ſumma diligentiæ et judicii laude, id expleverunt munus quo nunc fungimur. Interea temporis Medicina, ſi cum aliis bonis artibus non pari paſſu proceſſerit, adjumenta tamen, nec pauca nec parvi æſtimanda, tum aliorum induſtriæ et inventis accepta retulit, tum eorum, egregie et præter cæteros, qui nuperis abhinc annis in chemiam altius ſubtiliuſque excolendam acriori ſtudio incubuerunt. Itaque cum officii noſtri ratio poſtulare tandem viſa eſt, ut hæc communia artis medicinalis inſtrumenta de integro revocarentur ad examen, haud nos operi iſti atque officio ſatisfacturos exiſtimavimus, niſi quidquid ab hodierna chemicorum diſciplina hauriendum erat auxilii huc transferremus, et noſtram facem ex collatitio eorum lumine claram magis et illuſtriorem exhiberemus. Id proinde primum habuimus in votis, ut materia omnis chemica, medicinæ faciendæ accommodata, in

c medium

medium a nobis proferretur non ſolum erroribus purgata, ſed etiam perfectior politiorque, et artificioſe magis digeſta ac diſtributa, quam quæ apud nos antea invaluerat. Nec tamen in hac difficillima operis parte adeo omnis conſumta eſt cura, ut alia jacerent tanquam neglecta, aut obiter tantum ac curſim perſtringerentur; ſed et medicamenti uniuscujuſque compoſitio reddita eſt quaſi trutinæ ſingillatim et curioſe perpenſa; ut ſive deficeret aliquid, adjectum eſſet, ſive ſuperaret et redundaret, reciſum. Nec ſane dubitavimus, in hoc obeundo munere, integras medicamentorum formulas et amputare inutiles, et feliciores inſerere; ita tamen, ut nihil novi cupide arreptum eſſet, uſitati nihil ac conſueti temere repudiatum. Data opera eſt ſedulo, ut veterum aviarum veſtigia quam pauciſſima manerent: quod ſi ſupervacanei quidpiam aut parum utilis hinc inde ſparſum ſit, id nobis ſatius viſum eſt poſteris relinquere ſive corrigendum, ſive delendum, quam opinionibus etiam pravis, dum innoxiis, nimium pertinaciter adverſari. Simplicitati ubique, qua licuit, proſpeximus; cautumque præcipue eſt, ne in unum miſcerentur niſi quæ apte inter ſe cohærerent, et uni eidemque inſervirent propoſito. Inde fit, ut portentoſa quædam et enormia *antidota*, quæ profecto in in ſe neque conſilium neque modum habent ullum, quippe ex rebus collectis undique et inter ſe pugnantibus conflata, nunc demum moveantur loco: et vel hoc argumento patet, nec inveteratæ conſuetudinis auctoritatem, nec reverentiam antiquitatis in nos amplius ultra modum dominari.

Timore et fuga venenorum miſeræ laborarunt antiqui; quorum tamen non niſi perpauca admodum iis innotuiſſe pro comperto habemus. Noſtris temporibus alia eſt, et longe diſſimilis venenorum fortuna; neque enim ab iis, tanquam prorſus inimicis, abhorrere videtur Medicina, ſed ea ad partes ſuas traducere, et opem eorum ſociam et adjutricem expoſcere. Ex his unum atque alterum (quorum ſcilicet feceramus ipſi periculum) in tabulas noſtras conſcribi voluimus, alia protinus civitate donaturi, ſi qua alia experimentum non fallax oſtenderit, in ipſis morborum curationibus comprobata. Ea, quæ nondum ſatis ſunt explorata, pro cognitis amplecti, certe nimiæ eſſet temeritatis.

Ingens ille rerum numerus, quas nominibus incognitis et nuper fictis inſignitas dedimus, anſam fortaſſe alicui ad reprehendendum dabit; quandoquidem nemo fere ſit qui non nominibus, quibus conſuevit, libentius utatur quam novis. Iſtæ autem immutationes huc fere ſpectant; primum, ut vana quædam et inſulſa vocabula, ſive a veterum chemicorum commentis, ſive aliunde deducta (quantum commode fieri poterit) in deſuetudinem et oblivionem abeant; tum præterea, ut per titulos ipſos deſignetur quælibet compoſitio quid ſit magis quam quo valeat; et potius ex quibus conſtet principiis, quam quibus in caſibus proficere ſoleat, aut cui parti corporis opitulari; poſtremo, ut nihil quidpiam medicamenti ſub titulo non ſuo occultetur. Quod ad nomina attinet, *tribus ſalibus alkalinis* pro arbitrio noſtro impoſita, (quorum unum quidem jam olim uſu receptum fuerat, duo autem altera ab uſitatis non niſi parce detorta ſunt) ea ſane tantum in ſe commoditatis ac compendii præ ſe ferunt, ut veniam ſaltem apud medicos non immerito ſibi vendicent. Neutiquam tamen eſt diſſimulandum, tot novitates rem pharmaceuticam tractantibus, dum incipiant ii, dumque ignorent, graves fore; ea vero moleſtia, quantaquanta fuerit (niſi nos animus fallit) facile devorabitur, ceſſura nimirum ultro, et locum datura conſuetudini magis idoneæ, magis jucundæ, magis utili.

Haud

Haud ſumus neſcii, quantæ fuiſſet magnitudinis ac difficultatis, pharmacopœiam adornare in omni genere perfectam et abſolutam,—omnibus hominibus arriſuram, quam prorſus inſperabile. Nihil quidpiam tale ſuſcipimus aut profitemur; nobiſmet ſerio gratulaturi, ſi opera, paginis hiſce in ſalutem publicam impenſa, id aliquatenus effectum reddiderit, ut mala adverſæ valetudinis leviora fiant, curatio promtior et expeditior.

O R D O

C A P I T U M.

PHARMACOPOEIA

LONDINENSIS.

PONDERA, MENSURÆ, &c.

APUD noſtrates uſu recepta ſunt duo ponderum genera, unum, quo aurum et argentum veneunt, alterum, quo cæteræ fere merces; prius vocamus *Troy weight*, poſterius *Avoirdupois weight*. Horum libræ diverſo modo dividuntur; illius enim libra duodecim tantum uncias habet, hujus vero ſedecim. Pondere quoque inter ſe differunt tum libræ, tum unciæ; libra enim aurifabrorum minor eſt altera, uncia major. Nos libra aurifabrorum utimur; quam tamen non ut iſti, ſed in hunc modum dividimus.

Libra	habet	uncias duodecim,
Uncia	habet	drachmas octo,
Drachma	habet	ſcrupulos tres,
Scrupulus	habet	grana viginti.

Menſura etiam liquidorum apud noſtrates diſpar eſt: alia eſt cereviſiæ, alia vino propria. Poſteriorem nos adhibemus, eam menſuram pro libra uſurpantes quæ Anglice appellatur *A wine pint*.

Hanc libram hac ratione dividimus:

Libra	habet	uncias ſedecim,
Uncia	habet	drachmas octo.

Congius habet libras octo.

Quoniam nullius fere liquoris pondus librale menſuram, quam vocamus libram, adimplet, ubique præpoſuimus P. vel M. pröut pondere, vel menſura ſingula præſcribuntur.

Mortaria, ex ære aut orichalco facta, medicamentis parandis minus apta eſſe cenſemus. Volumus etiam, ut menſuræ, infundibula, et vaſa exhalationi liquorum inſervientia, quæ ex ære, e plumbo, vel e miſtura metallica fiunt, cujus pars aliqua eſt æs aut plumbum, cautim evitentur.

Thermometro Fahrenheitiano utimur.

Per

Per CALOREM FERVENTEM intelligi volumus calorem a 200° ad 212°.

Per CALOREM LENEM intelligi volumus calorem a 90° ad 100°.

Quoties vocabulis PONDUS SPECIFICUM utimur, ponimus materiam, de qua agatur, eſſe caloris 55°.

MATERIA MEDICA.

A.

ABrotonum, *folium*.	Artemisia *Abrotanum*. Linnæi Species Plantarum.
Absinthium maritimum, *cacumen*.	Artemisia *maritima*. L.S.P.
Absinthium vulgare, *herba*.	Artemisia *Absinthium*. L. S. P.
Acetosa pratensis, *folium*.	Rumex *Acetosa*. L. S. P.
Acidum vitriolicum. Hujus pondus specificum est ad pondus aquæ distillatæ ut 1,850 ad 1,000.	
Aconitum, *herba*.	Aconitum *Napellus*, L.S.P.
Adeps suilla.	
Allium, *radix*.	Allium *sativum*. L. S. P.
Aloë barbadensis. socotorina.	Aloë *perfoliata*. L. S. P.
Althæa, *radix*, *folium*.	Althæa *officinalis*. L. S. P.
Alumen.	Argilla vitriolata.
Ammoniacum, *gummi-resina*.	

Amygdala

Amygdala amara, dulcis, } *nucleus.*	Amygdalus *communis.* L. S. P.
Anethum, *ſemen.*	Anethum *graveolens.* L.S.P.
Angelica, *radix, caulis, folium, ſemen.*	Angelica *Archangelica.* L. S. P.
Aniſum, *ſemen.*	Pimpinella *Aniſum.* L. S. P.
Antimonium.	Antimonium Sulphuratum.
Arabicum Gummi.	Mimoſa *nilotica.* L. S. P.
Argentum.	
Arnica, *herba, flos, radix.*	Arnica *montana.* L. S. P.
Arum, *radix recens.*	Arum *maculatum.* L. S. P.
Aſa fœtida, *gummi-reſina.*	Ferula *Aſſa fœtida.* L.S.P.
Aſarum, *folium.*	Aſarum *europæum.* L. S. P.
Avena, *ſemen.*	Avena *ſativa.* L. S. P.
Aurantium hiſpalenſe, *folium, flos, fructus ſuccus, et cortex exterior.*	Citrus *Aurantium.* L. S. P.

B.

Balſamum canadenſe.	Pinus *Balſamea.* L. S. P.
copaiva.	Copaifera *officinalis.* L.S.P.
peruvianum.	Myroxylon *peruiferum.* Linnæi Supplementum Plantarum.
tolutanum.	Toluifera *Balſamum.* L.S.P.
Bardana, *radix.*	Arctium *Lappa.* L. S. P.

Barilla.

Barilla.	Natron impurum.
Becabunga, *herba*.	Veronica *Beccabunga*. L.S.P.
Benzoë, *reſina*.	Styrax *Benzoë*. Acta Philoſophica Londinenſia.
Biſtorta, *radix*.	Polygonum *Biſtorta*. L.S.P.
Bolus gallicus.	
Borax.	Natron boracicatum.

C.

Calamus aromaticus, *radix*.	Acorus *Calamus*. L. S. P.
Calx.	Lapis calcareus purus recens uſtus.
Camphora.	Laurus *Camphora*. L. S. P.
Canella alba, *cortex*.	
Cantharis.	Meloë *veſicatorius*. Linnæi Syſtema Naturæ.
Cardamine, *flos*.	Cardamine *pratenſis*. L.S.P.
Cardamomum minus, *ſemen*.	Amomum *repens*. Sonnerati iter.
Carduus benedictus, *herba*.	Centaurea *benedicta*. L.S.P.
Carica.	Ficus *Carica*. L. S. P.
Caruon, *ſemen*.	Carum *Carui*. L. S. P.
Caryophyllum aromaticum, *et oleum ejus eſſentiale*.	Caryophyllus *aromaticus*. L. S. P.
Caryophyllum rubrum, *flos*.	Dianthus *Caryophyllus*. L. S. P.

Caſcarilla,

Cascarilla, *cortex.*	
Cassia fistularis, *fructus.*	Cassia *Fistula.* L. S. P.
Castoreum russicum.	
Catechu, vulgo terra japonica.	Mimosa *Catechu.* L. Suppl. P.
Centaureum minus, *cacumen.*	Gentiana *Centaurium.* L. S. P.
Cera flava.	
alba.	
Chamæmelum, *flos simplex.*	Anthemis *nobilis.* L. S. P.
Chelæ cancrorum.	Cancer *Pagurus.* L. S. N.
Cicuta, *herba, flos, semen.*	Conium *maculatum.* L.S.P.
Cinara, *folium.*	Cynara *Scolymus.* L. S. P.
Cineres clavellati.	Kali impurum.
Cinnamomum, *cortex, et ejus oleum essentiale.*	Laurus *Cinnamomum.* L. S. P.
Coccinella.	
Cochlearia hortensis, *herba.*	Cochlearia *officinalis.* L. S. P.
Colchicum, *radix recens.*	Colchicum *autumnale.* L. S. P.
Colocynthis, *fructus medulla.*	Cucumis *Colocynthis.* L. S. P.
Colomba, *radix.*	
Contrayerva, *radix.*	Dorstenia *Contrajerva.* L. S. P.

Corallium

Corallium rubrum.	Iſis nobilis. L. S. N.
Coriandrum, *ſemen.*	Coriandrum *ſativum.* L.S.P.
Cornu cervi.	
Creta.	
Crocus, *floris ſtigma.*	Crocus *ſativus.* L. S. P.
Cubeba.	Piper *Cubeba.* L. Suppl. P.
Cucumis agreſtis, *fructus recens.*	Momordica *Elaterium.* L. S. P.
Cuminum, *ſemen.*	Cuminum *Cyminum.* L.S.P.
Cuprum.	
Ærugo.	
Vitriolum cœruleum.	Cuprum vitriolatum.
Curcuma, *radix.*	Curcuma *longa.* L. S. P.
Cydonium malum, *ejuſque ſemen.*	Pyrus *Cydonia.* L. S. P.
Cynoſbatus, *fructus.*	Roſa *canina.* L. S. P.

D.

Daucus ſylveſtris, *ſemen.*	Daucus *Carota.* L. S. P.
Digitalis, *herba.*	Digitalis *purpurea.* L. S. P.

E.

Elemi, *reſina.*	Amyris *Elemifera.* L. S. P.
Enula campana, *radix.*	Inula *Helenium.* L. S. P.
Eryngium, *radix.*	Eryngium *maritimum.* L. S. P.

F.

F.	
Ferrum.	
Vitriolum viride.	Ferrum vitriolatum.
Filix, *radix.*	Polypodium *Filix mas.* L. S. P.
Fœniculum dulce, *ſemen.*	Anethum *Fœniculum.* L. S. P.
Fœnum græcum, *ſemen.*	Trigonella *Fœnum græcum.* L. S. P.
G.	
Galbanum, *gummi-reſina.*	Bubon *Galbanum.* L. S. P.
Galla.	
Gambogia, *gummi-reſina.*	
Geniſta, *cacumen, ſemen.*	Spartium *ſcoparium.* L.S.P.
Gentiana, *radix.*	Gentiana *lutea.* L. S. P.
Ginſeng, *radix.*	Panax *quinquefolium.* L. S. P.
Glycyrrhiza, *radix.*	Glycyrrhiza *glabra.* L.S.P.
Granatum, *flos* Balauſtium *dictus, cortex fructus.*	Punica *Granatum.* L. S. P.
Gratiola, *herba.*	Gratiola *officinalis.* L. S. P.
Guaiacum, *lignum, cortex, gummi-reſina.*	Guaiacum *officinale.* L.S.P.
H.	
Helleboraſter, *folium.*	Helleborus *fœtidus.* L.S.P.
Helleborus albus, *radix.*	Veratrum *album.* L. S. P.
niger, *radix.*	Helleborus *niger.* L. S. P.

Hordeum, *ſemen.*	Hordeum *diſtichon.* L.S.P.
ſemen perlatum.	
Hydrargyrus.	
Cinnabaris.	Hydrargyrus ſulphuratus.
Hypericum, *flos.*	Hypericum *perforatum.* L. S. P.

J.

Jalapium, *radix.*	
Ichthyocolla.	
Ipecacuanha, *radix.*	
Iris, *radix.*	Iris *florentina.* L. S. P.
Juglans, *fructus immaturus.*	Juglans *regia.* L. S. P.
Juniperus, *bacca, cacumen.*	Juniperus *communis.* L.S.P.

K.

Kino, *reſina.*	Gummi gambienſe.

L.

Ladanum, *reſina.*	Ciſtus *creticus.* L. S. P.
Lavendula, *flos.*	Lavandula *Spica.* L. S. P.
Laurus, *folium, bacca.*	Laurus *nobilis.* L. S. P.
Lignum campechenſe.	Hæmatoxylum *Campechianum.* L. S. P.
Limon, *ſuccus, cortex exterior, et oleum* eſſentia *dictum.*	Citrus *Medica.* L. S. P.

Linum,

Linum, *femen.*	Linum *ufitatiffimum.* L.S.P.
Lujula, *folium.*	Oxalis *Acetofella.* L. S. P.
M.	
Majorana, *herba.*	Origanum *Majorana.* L.S.P.
Malva, *folium*, *flos.*	Malva *fylveftris.* L. S. P.
Manna.	
Marrubium album, *herba.*	Marrubium *vulgare.* L.S.P.
Marum fyriacum, *herba.*	Teucrium *Marum.* L.S.P.
Maftiche, *refina.*	Piftacia *Lentifcus.* L. S. P.
Mel.	
Meliffa, *herba.*	Meliffa *officinalis.* L. S. P.
Mentha piperitis, *herba.*	Mentha *piperita.* L. S. P.
fativa, *herba.*	Mentha *fpicata.* Hudfoni Flora Anglica.
Mezereum, *cortex radicis.*	Daphne *Mezereum.* L. S. P.
Millepeda.	Onifcus *Afellus.* L. S. N.
Morum, *fructus.*	Morus *nigra.* L. S. P.
Mofchus.	
Myrrha, *gummi-refina.*	
N.	
Nafturtium aquaticum, *herba recens.*	Sifymbrium *Nafturtium aquaticum.* L. S. P.
Nicotiana, *folium.*	Nicotiana *Tabacum.* L.S.P.
Nitrum.	Kali nitratum.

Nux moſchata, *Oleum eſſentiale*, *Oleum expreſſum* Oleum macis *vulgo dictum*, *Macis*.	Myriſtica *Moſchata*. Acta Holmienſia.

O.

Olibanum, *gummi-reſina*.	Juniperus *lycia*. L. S. P.
Oliva, *Oleum*.	Olea *europæa*. L. S. P.
Opium.	
Opopanax, *gummi-reſina*.	Paſtinaca *Opopanax*. L.S.P.
Origanum, *herba*.	Origanum *vulgare*. L.S.P.
Ovum.	Ovum gallinaceum.

P.

Papaver album, *caput*.	Papaver *ſomniferum*. L.S.P.
erraticum, *flos*.	*Rhœas*. L. S. P.
Pareira brava, *radix*.	Ciſſampelos *pareira*. L.S.P.
Parietaria, *herba*.	Parietaria *officinalis*. L. S. P.
Pentaphyllum, *radix*.	Potentilla *reptans*. L. S. P.
Peruvianus cortex.	Cinchona *officinalis*. L. S. P.
Petroleum.	Bitumen *Petroleum*. L.S.N.
Petroſelinum, *radix*, *ſemen*.	Apium *Petroſelinum*. L. S. P.

Pimento, *bacca.*	Myrtus *Pimenta.* L. S. P.
Piper indicum, *fructus.*	Capſicum *annuum.* L. S. P.
longum, *fructus.*	Piper *longum.* L. S. P.
nigrum, *bacca.*	Piper *nigrum.* L. S. P.
Pix burgundica.	
liquida.	
Plumbum.	
Ceruſſa.	
Lithargyrus.	
Minium.	
Prunum gallicum.	Prunus *domeſtica.* L. S. P.
ſylveſtre.	Prunus *ſpinoſa.* L. S. P.
Pulegium, *herba*, *flos.*	Mentha *Pulegium.* L. S. P.
Pyrethrum, *radix.*	Anthemis *Pyrethrum.* L. S. P.

Q.

Quaſſia, *lignum*, *cortex*, *radix.*	Quaſſia *amara.* L. S. P.
Quercus, *cortex.*	Quercus *Robur.* L. S. P.

R.

Raphanus ruſticanus, *radix.*	Cochlearia *Armoracia.* L. S. P.
Rhabarbarum, *radix.*	Rheum *palmatum.* L. S. P.

Ribes

Ribes nigrum, *fructus*.	Ribes *nigrum*. L. S. P.
rubrum, *fructus*.	*rubrum*. L. S. P.
Ricinus, *ſemen*.	Ricinus *communis*. L. S. P.
Roſa damaſcena, *petalum*.	Roſa *centifolia*. L. S. P.
rubra, *petalum*.	Roſa *gallica*. L. S. P.
Roſmarinus, *cacumen*, *flos*.	Roſmarinus *officinalis*. L. S. P.
Rubia, *radix*.	Rubia *tinctorum*. L. S. P.
Rubus idæus, *fructus*.	Rubus *idæus*. L. S. P.
Ruta, *herba*.	Ruta *graveolens*. L. S. P.

S.

Sabina, *folium*.	Juniperus *Sabina*. L. S. P.
Saccharum non purificatum.	
purificatum.	Saccharum bis coctum.
Sagapenum, *gummi-reſina*.	
Sal amarus.	Magneſia vitriolata.
Sal ammoniacus.	Ammonia muriata.
Sal muriaticus.	Natron muriatum.
Salvia, *folium*.	Salvia *officinalis*. L. S. P.
Sambucus, *cortex interior*, *flos*, *bacca*.	Sambucus *nigra*. L. S. P.
Sanguis draconis, *reſina*.	
Santalum rubrum, *lignum*.	Pterocarpus *Santolinus*. L. Suppl. P.
Santonicum, *ſemen*.	Artemiſia *Santonicum*. L. S. P.

Sapo.

Sapo.	Sapo ex oleo olivæ et natro confectus.
Sarcocolla, *gummi-resina*.	
Sarsaparilla, *radix*.	Smilax *Sarsaparilla*. L.S.P.
Sassafras, *lignum*, *radix*, *ejusque cortex*.	Laurus *Sassafras*. L. S. P.
Scammonium, *gummi-resina*.	Convolvulus *Scammonia*. L. S. P.
Scilla, *radix*.	Scilla *maritima*. L. S. P.
Scordium, *herba*.	Teucrium *Scordium*. L.S.P.
Senna, *folium*.	Cassia *Senna*. L. S. P.
Seneka, *radix*.	Polygala *Senega*. L. S. P.
Serpentaria virginiana, *radix*.	Aristolochia *Serpentaria*. L. S. P.
Sevum ovillum.	
Simarouba, *cortex*.	Quassia *Simarouba*. L. Suppl. P.
Sinapi, *semen*.	Sinapis *nigra*. L. S. P.
Sium, *herba*.	Sium *nodiflorum*. L. S. P.
Sperma ceti.	
Spigelia, *radix*.	Spigelia *marilandica*. L. S. N.
Spina cervina, *bacca*.	Rhamnus *catharticus*. L. S. P.
Spiritus vinosus rectificatus continet alkoholis par-	

tes

tes 95 et aquæ diſtillatæ partes 5 in partibus 100. Hujus pondus ſpecificum eſt ad pondus aquæ diſtillatæ ut 835 ad 1,000.

Spiritus vinoſus tenuior continet alkoholis partes 55 et aquæ diſtillatæ partes 45 in partibus 100. Hujus pondus ſpecificum eſt ad pondus aquæ diſtillatæ ut 930 ad 1,000.

Spongia.	Spongia *officinalis*. L. S. N.
Stannum.	
Staphiſagria, *ſemen*.	Delphinium *Staphiſagria*. L. S. P.
Styrax, *reſina*.	Styrax *officinalis*. L. S. P.
Succinum.	
Sulphur.	
Sulphuris flores.	

T.

Tamarindus, *fructus*.	Tamarindus *Indica*. L.S.P.
Tanacetum, *flos*, *herba*.	Tanacetum *vulgare*. L.S.P.
Taraxacum, *radix*, *herba*.	Leontodon *Taraxacum*. L. S. P.

Terebinthina

Terebinthina vulgaris.	
chia.	
Teſtæ oſtreorum.	Oſtrea *edulis*. L. S. N.
Thus, *reſina*.	
Tormentilla, *radix*.	Tormentilla *erecta*. L. S. P.
Tragacantha, *gummi*.	Aſtragalus *Tragacantha*. L. S. P.
Trifolium paludoſum, *herba*.	Menyanthes *trifoliata*. L. S. P.
Triticum, *farina*.	Triticum *hybernum*. L.S.P.
amylum.	
Tuſſilago, *herba*.	Tuſſilago *Farfara*. L. S. P.

V.

Valeriana ſylveſtris, *radix*.	Valeriana *officinalis*. L. S. P.
Viola, *flos recens*.	Viola *odorata*. L. S. P.
Vitis.	Vitis *vinifera*. L. S. P.
Uva paſſa.	
Vinum.	
Tartarum.	Tartarum impurum.
Tartari cryſtalli.	purificatum.
Acetum.	
Ulmus, *cortex interior*.	Ulmus *campeſtris*. L. S. P.
Urtica, *herba*.	Urtica *dioica*. L. S. P.
Uva urſi, *folium*.	Arbutus *Uva urſi*. L. S. P.

Z.

Zedoaria, *radix*.	Kaempferia *rotunda*. L. S.P.
Zincum.	
Lapis calaminaris.	Lapis calaminaris uſtus.
Tutia.	
Vitriolum album.	Zincum vitriolatum.
Zingiber, *radix*.	Amomum *Zingiber*. L.S.P.

PRÆPARATIONES SIMPLICIORES.

QUORUNDAM AQUA NON SOLUBILIUM PRÆPARATIO.

HÆC primum in mortario contunde in pulverem; quem deinde, affuſa paulula aqua, læviga ſuper lapidem expolitum et durum, non autem calcareum, ut fiat quam maxime ſubtilis. Hunc pulverem ſuper cretam, charta bibula interpoſita, exſicca; et in loco tepido, vel ſaltem ſicco, ad aliquot dies ſepone.

Ad hunc modum præparari debent

Antimonium,
Chelæ cancrorum,
Corallium,
Creta,
Lapis calaminaris,
Succinum,

Teftæ oftreorum fordibus prius purgatæ,
Tutia.

Chelæ cancrorum, prius minutim fractæ, aqua fervente lavari debent, antequam lævigentur.

Eodem modo præparari debet
Ærugo.

ADIPIS SUILLÆ, SEVIQUE OVILLI PRÆPARATIO.

Hæc in fruftula concide, et lento igne liqua; deinde colando a membranis fepara.

AMMONIACI PURIFICATIO.

Ammoniacum gummi, fi non purum effe videatur, coque in aqua donec mollefcat, et prelo exprime per pannum cannabinum; dein fepone ut pars refinofa fubfidat. Aquam per vaporationem abfume, fub finem exhalationis partem refinofam redde, et parti gummofæ immifce.

Eodem modo purificantur afafœtida et fimiles gummi-refinæ.

Gummi

Gummi etiam quodvis facile liquefcens, quale eft galbanum, purificare potes id immittendo in vefi-cam bubulam, tenendoque in aqua fervente, donec adeo molle fiat, ut per pannum cannabinum a for-dibus prelo feparari poffit.

CORNU CERVI USTIO.

Cornu cervi frufta ure, donec penitus albefcant; deinde tere in pulverem fubtiliffimum.

HERBARUM ET FLORUM EXSICCATIO.

Hæc, leviter ftrata, leni calore exficca.

MELLIS DESPUMATIO.

Mel in balneo aquofo liqua, et fpumam aufer.

MILLEPEDÆ PRÆPARATIO.

Millepedas, panno cannabino rariori inclufas, fufpende in vafe claufo fuper fpiritum vinofum tenuiorem calefactum, ut, a vapore necatæ, friabiles fiant.

PULPARUM EXTRACTIO.

Fructus pulpofos, fi immaturi fint, vel maturi et ficci, fepone in loco humido, ut mollefcant; deinde pulpas per cribrum fetaceum exprime; poftea coque lento igne, crebro agitans donec debitæ fint craffitudinis.

Item caffiæ fiftularis pulpam e filiqua exime, et ad idoneam craffitudinem decoque.

Fructuum maturorum et recentium pulpas per cribrum exprime, nulla coctione adhibita.

SCILLÆ EXSICCATIO.

Scillam, ablato externo cortice, tranfverfim in laminas tenues feca, et leni calore exficca.

SPONGIÆ USTIO.

Spongiam, in fruſtula conciſam, contunde, et a lapillis ſeparatam ure in vaſe ferreo clauſo donec nigreſcat et friabilis fiat; deinde tere in pulverem ſubtiliſſimum.

STYRACIS PURIFICATIO.

Styracem in ſpiritu vinoſo rectificato ſolutum, cola; deinde per lenem calorem fiat debitæ craſſitudinis.

CONSERVÆ.

Conſerva Lujulæ,
Abſinthii maritimi,
Roſæ rubræ,
Corticis exterioris aurantii hiſpalenſis.

Folia e pediculis, petala nondum explicita e calycibus decerpe, ungues adimens; corticem exteriorem aurantiorum radula abrade; tum ſingula piſtillo ligneo contunde in mortario marmoreo, primum per ſe, deinde cum ſacchari purificati triplo pondere, donec miſceantur.

CONSERVA ARI.

℞ Radicis ari recentis contuſæ P. libram dimidiam.
Sacchari purificati P. libram unam cum ſemiſſe.

Simul in mortario contunde.

CONSERVA

CONSERVA CYNOSBATI.

℞ Pulpæ fructus cynosbati maturi P. libram unam,
Sacchari purificati in pulverem triti P. uncias viginti.

Misce in conservam.

CONSERVA PRUNI SYLVESTRIS.

Pruna in aquam immissa igni impone, ut mollescant, cura adhibita ne rumpantur; dein, prunis aqua exemtis, pulpam exprime, et cum triplo pondere sacchari purificati misce in conservam.

CONSERVA SCILLÆ.

℞ Scillæ recentis P. unciam unam,
Sacchari purificati P. uncias quinque.

Simul in mortario contunde in conservam.

Conservas omnes, præcipue ari et scillæ, in vasis clausis sepone.

S U C C I.

SUCCUS COCHLEARIÆ COMPOSITUS.

℞ Succi cochleariæ hortensis M. libras duas,
becabungæ,
nasturtii aquatici, singulorum M. libram unam,
aurantiorum hispalensium M. uncias viginti.

Misce, et, postquam fæces subsederint, effunde liquorem, vel cola.

SUCCUS BACCÆ SAMBUCI SPISSATUS.

℞ Succi baccarum sambuci expressi et defæcati M. libras duas.

In balneo aquæ, sale muriatico saturatæ, spissa.

Eodem modo succum ribis nigri, limonis, et cicutæ floribus jamprimum apparentibus collectæ, spissa.

EXTRACTA

EXTRACTA et RESINÆ.

Extractum Chamæmeli,
Cacuminis geniſtæ,
Gentianæ,
Glycyrrhizæ,
Hellebori nigri,
Rutæ,
Sabinæ.

In aqua diſtillata coque, exprime, cola, et ſepone ut fæces ſubſidant; deinde in balneo aquæ, ſale muriatico ſaturatæ, ad craſſitudinem pilulis fingendis aptam decoque.

Eodem balnei genere utendum eſt, ut vaporatio rite fiat, in extractorum omnium præparatione.

EXTRACTUM COLOCYNTHIDIS COMPOSITUM.

℞ Medullæ colocynthidis minutim incisæ P. drachmas sex,
Aloës socotorinæ in pulverem tritæ P. unciam unam cum semisse,
Scammonii in pulverem triti P. unciam dimidiam,
Seminum cardamomi minoris in pulverem tritorum, demtis capsulis, P. drachmam unam,
Spiritus vinosi tenuioris M. libram unam.

Colocynthidem in spiritu vinoso per quatriduum digere leni calore. Tincturæ expressæ adde aloën et scammonium. His solutis spiritum distillatione separa, ut materia crassitudinis fiat pilulis fingendis aptæ. Sub finem adde semina.

ELATERIUM.

Cucumeres agrestes maturos scinde, et succum levissime expressum trajice per cribrum setaceum tenuissimum in vas vitreum; deinde ad aliquot horas sepone, donec crassior pars subsederit. Partem tenuiorem

orem ſupernatantem effunde, et reliquam colando ſepara. Craſſiorem vero partem, quæ poſt colandum reſtat, panno linteo coopertam, leni calori ſubjice donec exaruerit.

EXTRACTUM LIGNI CAMPECHENSIS.

℞ Ligni campechenſis raſi P. libram unam.

Coque quater vel ſæpius in aquæ diſtillatæ congio uno ad dimidium; deinde liquores ſimul miſtos et colatos ad idoneam craſſitudinem decoque.

EXTRACTUM CORTICIS PERUVIANI.

℞ Corticis peruviani in pulverem craſſum triti P. libram unam,
Aquæ diſtillatæ M. libras duodecim.

Coque per unam vel alteram horam, et liquorem effunde; qui, durante calore, ruber erit et pellucebit, ceſſante autem, flaveſcet, et turbidus fiet. Eadem aquæ menſura iterum affuſa, denuo coque, ut prius, et et coctionem repete, donec liquor frigefactus maneat limpidus. Deinde liquores hos omnes miſtos et colatos ad idoneam craſſitudinem per vaporationem redige.

Hoc extractum ſub duplici forma præparari debet; MOLLI ad pilulas fingendas apta, et DURA quæ in pulverem teri poſſit.

EXTRACTUM
CORTICIS PERUVIANI CUM RESINA.

℞ Corticis peruviani in pulverem craſſum triti P. libram unam,
Spiritus vinoſi rectificati M. libras quatuor.

Digere per dies quatuor, et tincturam effunde. Reſiduum in aquæ diſtillatæ libris decem decoque ad libras duas. Dein tincturam et decoctum ſeparatim cola; et hoc vaporet, illa diſtilletur, donec utrumque ſpiſſari incipiat. Poſtremo extractum reſinæ miſce, et pilulis fingendis aptum redde.

Eodem modo præparari debent

Extractum Caſcarillæ,
Jalapii.

EXTRACTUM

EXTRACTUM SENNÆ.

℞ Sennæ p. libram unam,
Aquæ diſtillatæ congium unum.

Decoque ſennam in aqua diſtillata, addito poſt coctionem ſpiritus vinoſi rectificati pauxillo. Liquorem colatum ad idoneam craſſitudinem redige.

OPIUM PURIFICATUM.

℞ Opii in fruſtula conciſi p. libram unam,
Spiritus vinoſi tenuioris m. libras duodecim.

Digere leni calore, ſubinde agitans donec ſolvatur opium, quod per chartam cola. Diſtilla tincturam ita paratam ad idoneam craſſitudinem.

Opium purificatum ſub duplici forma ſervetur, MOLLI ad pilulas fingendas apta, et DURA quæ in pulverem teri poſſit.

NOTA.

Omnia extracta, dum ſpiſſeſcunt, ſæpius agitari debent.

Omnibus extractis aquoſis mollioribus paululum ſpiritus vinoſi inſperge.

OLEA EXPRESSA.

OLEUM AMYGDALÆ.

Amygdalas, vel dulces vel amaras, recentes in mortario tere; deinde prelo frigido exprime oleum.

Eodem modo exprimendum eſt

Oleum e ſeminibus Lini contuſis,
Ricini demto prius cortice,
et Sinapeos contuſis.

OLEA DISTILLATA.

Oleum Essentiale Anisi,
Carui,
Lavendulæ,
Menthæ piperitidis,
sativæ,
Origani,
Pulegii,
Rorismarini,
Baccæ juniperi,
Radicis sassafras.

Hæc olea eliciantur distillatione ex alembico cum vase frigidario satis amplo. Ad præcavendum autem empyreuma, materiæ addenda est aqua, in qua maceranda est illa ante distillationem.

Aqua, quæ inter distillandum prodit cum oleo, in usum servari debet.

OLEUM PETROLEI.

Petroleum distilla balneo arenæ.

OLEUM

OLEUM TEREBINTHINÆ.

℞ Terebinthinæ vulgaris P. libras quinque,
Aquæ M. libras quatuor.

Terebinthinam cum aqua diſtilla ex alembico cupreo.

RESINA FLAVA

Poſt diſtillationem olei terebinthinæ remanet.

OLEUM TEREBINTHINÆ RECTIFICATUM.

℞ Olei terebinthinæ P. libram unam,
Aquæ M. libras quatuor.

Diſtilla.

OLEUM ANIMALE.

℞ Olei cornu cervi P. libram unam.

Ter diſtilla.

OLEUM SUCCINI RECTIFICATUM.

℞ Olei ſuccini P. libram unam.

Ter diſtilla.

OLEUM VINI.

℞ Alkoholis,
Acidi vitriolici, ſingulorum M. libram unam.

Gradatim miſce, et diſtilla, cavens ne ſpuma nigra in receptaculum tranſeat. Liquoris diſtillati partem oleoſam ſepara ab acido vitriolico volatili. Parti oleoſæ adde aquæ kali puri quantum ſatis ſit ad odorem ſulphureum corrigendum; dein diſtilla ætherem leni calore. Manet oleum vini in retorta liquorem aquoſum ſupernatans, a quo ſeparandum eſt

S A L E S.

ACIDUM VITRIOLICUM DILUTUM.

℞ Acidi vitriolici P. unciam unam,
Aquæ diſtillatæ P. uncias octo.

Paulatim miſce.

ACIDUM NITROSUM.

℞ Nitri purificati P. uncias ſexaginta,
Acidi vitriolici P. uncias viginti novem.

Miſce, et diſtilla.

Hujus pondus ſpecificum eſt ad pondus aquæ diſtillatæ ut 1,550 ad 1,000.

ACIDUM NITROSUM DILUTUM.

℞ Acidi nitroſi,
Aquæ diſtillatæ, ſingulorum P. libram unam.

Miſce.

ACIDUM MURIATICUM.

℞ Salis muriatici exſiccati P. libras decem,
Acidi vitriolici P. libras ſex,
Aquæ P. libras quinque.

Sali adde gradatim acidum cum aqua prius miſtum: tum diſtilla.

Hujus pondus ſpecificum eſt ad pondus aquæ diſtillatæ ut 1,170 ad 1,000.

ACETUM DISTILLATUM.

℞ Aceti M. libras quinque.

Diſtilla lento igne in vaſis vitreis quamdiu guttæ cadunt ab empyreumate puræ.

ACIDUM ACETOSUM.

℞ Æruginis in pulverem craſſum tritæ P. libras duas.

Balneo aquæ, ſale muriatico ſaturatæ, penitus exſicca; dein diſtilla in balneo arenæ, et liquorem iterum diſtilla.

Hujus pondus ſpecificum eſt ad pondus aquæ diſtillatæ ut 1,050 ad 1,000.

SAL et OLEUM SUCCINI.

℞ Succini P. libras duas.

Diſtilla balneo arenæ calore gradatim aucto: aſcendent liquor acidus, oleum, et ſal oleo inquinatus.

SAL SUCCINI PURIFICATUS.

℞ Salis ſuccini P. libram dimidiam,
Aquæ diſtillatæ M. libram unam.

Salem coque in aqua diſtillata, et ſepone ut fiant cryſtalli.

FLORES BENZOËS.

℞ Benzoës in pulverem tritæ P. libram unam.

Ollæ figulinæ, in arena locatæ, immitte; et lento igne flores ſublima in chartæ conum ollæ aptatum.

Flores, ſi flavi ſint coloris, cum argilla alba miſtos denuo ſublima.

KALI PRÆPARATUM.

℞ Cinerum clavellatorum P. libras duas,
Aquæ diſtillatæ ferventis M. libras tres.

Solve,

Solve, et cola per chartam; dein liquor vaporet donec in ſuperficie appareat pellicula. Tum per noctem ſepone, ut ſales neutri in cryſtallos abeant. Poſtea liquorem effunde, et aquam totam decoque aſſidue agitans ne ſal ollæ adhæreat.

Eodem modo purificatur Kali impurum ex cineribus quorumcunque vegetabilium.

Idem ſal præparari poteſt ex tartaro uſto donec cinerei ſit coloris.

AQUA KALI.

℞ Kali P. libram unam.

Sepone in loco humido donec liqueſcat, et cola.

AQUA KALI PURI.

℞ Kali P. libras quatuor,
Calcis P. libras ſex,
Aquæ diſtillatæ congios quatuor.

Calci adde aquæ libras quatuor, et ſtent ſimul per horam, dein adde kali et aquæ quod reliquum eſt. Tum coque per horæ quadrantem; ſine liquorem frigeſcere, et cola. Hujus liquoris menſura libralis uncias

uncias ſedecim pendere debet. Si liquor ullam effervefcentiam cum acido quocunque cieat, plus calcis adde.

KALI PURUM.

℞ Aquæ kali puri congium unum.

Vaporet ad ſiccitatem; dein ad ignem liqueſcat, et effundatur.

CALX CUM KALI PURO.

℞ Calcis P. libras quinque cum unciis quatuor,
Aquæ kali puri P. libras ſedecim.

Decoque aquam kali ad partem quartam; deinde calcem affuſa aqua in pulverem diſſolutam inſperge. Hanc in vaſe obturato ſerva.

NATRON PRÆPARATUM.

℞ Barillæ in pulverem tritæ P. libras duas,
Aquæ diſtillatæ congium unum.

Coque barillam in aquæ M. libris quatuor per horam dimidiam, et cola. Partem, quæ poſt colandum reſtat, coque cum reliqua aqua, et cola. Liquores miſti vaporent ad libras duas, et ſeponantur ad dies octo. Hunc liquorem iterum cola; et, poſt debitam coctionem, ſepone ut fiant cryſtalli. Cryſtallos in aqua diſtillata ſolve, liquorem cola, decoque, et ſepone ut fiant cryſtalli.

AMMONIA PRÆPARATA.

℞ Salis ammoniaci in pulverem triti P. libram unam,
Cretæ præparatæ P. libras duas.

Miſce, et ſublima.

AQUA AMMONIÆ PURÆ.

℞ Salis ammoniaci P. libram unam,
Calcis P. libras duas,
Aquæ congium unum.

Calci adde aquæ libras duas; ſtent ſimul horam, dein adde ſalem ammoniacum et aquæ ferventis libras ſex, et vas ſtatim operi. Liquorem frigefactum effunde, et lento igne diſtilla libram unam.

AQUA

AQUA AMMONIÆ.

℞ Salis ammoniaci P. libram unam,
Cinerum clavellatorum P. libram unam cum ſemiſſe,
Aquæ M. libras quatuor.

Diſtilla lento igne M. libras duas.

LIQUOR VOLATILIS, SAL, ET OLEUM CORNU CERVI.

℞ Cornu cervi P. libras decem.

Diſtilla igne gradatim aucto. Aſcendent liquor volatilis, ſal, et oleum. Oleo et ſale ſeparatis, liquorem ter diſtilla.

Sali adde æquale cretæ præparatæ pondus; et ſublima ter, vel donec candidus fiat.

Idem liquor volatilis, ſal, et oleum elici poſſunt e partibus quibuſvis quorumcunque animalium, adipe excepta.

KALI VITRIOLATUM.

℞ Salis, qui reſtat poſt diſtillationem acidi nitroſi, P. libras duas,
Aquæ diſtillatæ congios duos.

Igne forti in vaſe aperto acidum ſupervacaneum abſume; dein coque pauliſper in aqua, cola, et ſepone ut fiant cryſtalli.

NATRON VITRIOLATUM.

℞ Salis, qui reſtat poſt diſtillationem acidi muriatici, P. libras duas,
Aquæ diſtillatæ M. libras duas cum ſemiſſe.

Igne forti in vaſe aperto acidum ſupervacaneum abſume; dein coque pauliſper in aqua, cola, et ſepone ut fiant cryſtalli.

NITRUM PURIFICATUM.

℞ Nitri P. libras duas,
Aquæ diſtillatæ M. libras quatuor.

Coque nitrum in aqua donec ſolvatur, cola, et ſepone ut fiant cryſtalli.

KALI

KALI ACETATUM.

℞ Kali P. libram unam.

Coque lento igne in aceti diſtillati quadruplo vel quintuplo. Effervescentia ceſſante, plus aceti diſtillati diverſis vicibus adjiciatur, donec, priore aceto fere per exhalationem abſumto, plus aceti additum nullam cieat effervescentiam; quod fiet conſumto aceti diſtillati quaſi pondere librarum viginti: poſtea lente exſiccetur. Relinquetur ſal impurus, qui lento igne aliquantiſper liquefiat; deinde aqua ſolvatur et per chartam coletur. Si liquefactio rite peracta ſit, liquor colatus expers coloris erit; ſin aliter, coloris fuſci. Denique lento igne vaporet hic liquor ex vaſe vitreo minime profundo, ſale, dum areſcit, ſubinde moto, ut citius ad ſiccitatem perveniat; qui vaſe obturato aſſervari debet. Sal candidiſſimus eſſe debet, totuſque tum aqua tum ſpiritu vinoſo ſolvi, nullis fœcibus relictis. Si ſal, etiamſi albus ſit, in ſpiritu vinoſo fæces aliquas demiſerit, eo ſpiritu ſolutus per chartam colandus eſt, iterumque exſiccandus.

AQUA AMMONIÆ ACETATÆ.

℞ Ammoniæ P. uncias duas,
Aceti diſtillati M. libras quatuor, vel quantum ſatis ſit ut ammonia ſaturetur.

Miſce.

KALI TARTARISATUM.

℞ Kali P. libram unam,
Cryſtallorum tartari P. libras tres,
Aquæ diſtillatæ ferventis congium unum.

Sali, in aqua ſoluto, tartari cryſtallos in pulverem tritas, gradatim adjice. Liquorem, poſtquam refrixerit, per chartam cola, et poſt idoneam exhalationem ſepone ut fiant cryſtalli.

NATRON TARTARISATUM.

℞ Natri P. uncias viginti,
Cryſtallorum tartari in pulverem tritarum P. libras duas,
Aquæ diſtillatæ ferventis M. libras decem.

Natron in aqua ſolve, et gradatim adde cryſtallos tartari. Liquor per chartam coletur, vaporet, et ſeponatur ut fiant cryſtalli.

ALUMINIS PURIFICATIO.

℞ Aluminis P. libram unam,
Cretæ P. drachmam unam,
Aquæ diſtillatæ M. libram unam.

Coque pauliſper, cola, et ſepone ut fiant cryſtalli.

ALUMEN USTUM.

℞ Aluminis P. libram dimidiam,

In vaſe fictili ure quamdiu ebullit.

Cryſtallos ſalium, ſi fæcibus ullis inquinantur, primum liquore relicto, dein paululo aquæ diſtillatæ, vel ſpiritus vinoſi rectificati lava.

Quum ſalis cujuſvis concreverint cryſtalli ex quocunque liquore, quod reliquum liquoris eſt effunde, et, ſi opus ſit, cola. Vaporatione iterata partem liquoris abſume, et ſepone ut fiant cryſtalli. Hoc repete quamdiu cryſtalli prodeunt puræ.

MAGNESIA

MAGNESIA.

MAGNESIA ALBA.

℞ Salis amari,
Kali, ſingulorum P. libras duas,
Aquæ diſtillatæ ferventis M. libras viginti.

Salem amarum et kali ſingulatim ſolve in aquæ libris decem, et per chartam cola; dein miſce. Coque liquorem pauliſper, et adhuc calentem cola per linteum, ſuper quod reſtabit MAGNESIA ALBA; dein aqua diſtillata ſæpius affuſa kali vitriolatum ablue.

MAGNESIA USTA.

℞ Magneſiæ albæ P. uncias quatuor.

Magneſiam igni forti duas horas ſubjice. Hanc frigefactam in vaſe vitreo obturato repone.

PRÆPARATA e SULPHURE.

FLORES SULPHURIS LOTI.

℞ Florum ſulphuris P. libram unam,
Aquæ diſtillatæ M. libras quatuor.

Flores ſulphuris coque pauliſper in aqua diſtillata; deinde hanc aquam effunde, et, frigida affuſa, acidum ablue: denique exſicca.

KALI SULPHURATUM.

℞ Florum ſulphuris P. unciam unam,
Kali P. uncias quinque.

Salem ſulphuri, lento igne liquefacto, aſſidua agitatione immiſce, donec in unum coëant.

OLEUM SULPHURATUM,
et
PETROLEUM SULPHURATUM.

℞ Florum ſulphuris P. uncias quatuor,
Olei olivæ P. uncias ſedecim.

Flores ſulphuris coque cum oleo in olla parum operta, donec in unum coëant.

Itidem fit petroleum ſulphuratum.

SULPHUR PRÆCIPITATUM.

℞ Kali ſulphurati P. uncias ſex,
Aquæ diſtillatæ P. libram unam cum ſemiſſe,
Acidi vitriolici diluti quantum ſatis ſit.

Coque kali ſulphuratum in aqua diſtillata donec ſolvatur. Liquorem per chartam cola, cui adde acidum vitriolicum. Pulverem præcipitatum lava affuſa ſæpius aqua donec inſipidus fiat.

PRÆPARATA ex ANTIMONIO.

ANTIMONIUM CALCINATUM.

℞ Antimonii in pulverem triti P. uncias octo,
Nitri in pulverem triti P. libras duas.

Misce, et gradatim in crucibulum igne rubens immitte; ure materiam albam circiter horam dimidiam, et frigefactam in pulverem tere; deinde aqua distillata lava.

CROCUS ANTIMONII.

℞ Antimonii in pulverem triti,
Nitri in pulverem triti, singulorum P. libram unam,
Salis muriatici P. unciam unam.

Misce, et gradatim in crucibulum igne rubens injice, et calore aucto liqua. Materiam liquatam effunde, et frigefactam a scoriis separa.

ANTIMONIUM MURIATUM.

℞ Croci antimonii in pulverem triti,
Acidi vitriolici, ſingulorum P. libram unam,
Salis muriatici ſiccati P. libras duas.

Acidum vitriolicum retortæ infunde, paulatim addens ſalem muriaticum et crocum antimonii prius miſta: dein balneo arenæ fiat diſtillatio. Materia diſtillata plures dies aëri pateat; tum effundatur e fæcibus pars liquida.

PULVIS ANTIMONIALIS.

℞ Antimonii in pulverem craſſum triti,
Cornu cervi raſi, ſingulorum P. libras duas.

Miſce, et injice ollæ ferreæ latæ, ad rubedinem calefactæ, et aſſidue agita, donec colore cinereo fuerint. Materiam refrigeratam in pulverem tere, et crucibulo loricato immitte. Crucibulum aliud inverſum, cui parvum ſit in fundo foramen, luto conjunge. Ignem ſubminiſtra, quem ad rubedinem ſenſim auge, et ita auctum ſerva per horas duas. Denique materiam frigefactam in pulverem ſubtiliſſimum tere.

SULPHUR

SULPHUR ANTIMONII PRÆCIPITATUM.

℞ Antimonii in pulverem triti P. libras duas,
Aquæ kali puri M. libras quatuor,
Aquæ diſtillatæ M. libras tres.

Miſce, et coque igne lento horas tres, aſſidue agitans, et addens aquam diſtillatam proüt opus fuerit. Lixivium calidum cola per linteum duplex; et liquori adhuc calenti inſtilla paulatim acidi vitriolici diluti quantum ſatis ſit ad dejiciendum ſulphur. Aqua calida kali vitriolatum ablue.

ANTIMONIUM TARTARISATUM.

℞ Croci antimonii in pulverem triti P. libram unam cum ſemiſſe,
Cryſtallorum tartari P. libras duas,
Aquæ diſtillatæ congios duos.

Coque in vaſe vitreo circiter horæ quadrantem; per chartam cola, et liquorem colatum ſepone ut fiant cryſtalli.

ANTIMONIUM

ANTIMONIUM VITRIFICATUM.

℞ Antimonii in pulverem triti P. uncias quatuor.

In vaſe fictili lato ure igne ſenſim aucto, bacillo ferreo agitans, donec fumum ſulphureum non amplius emittat. Hunc pulverem immitte crucibulo, ita ut duas tertias partes ejus occupet. Operculo aptato, ignem ſubminiſtra, primum modicum, deinde fortiorem, donec liquetur materia. Vitrum liquefactum effunde.

PRÆPARATUM ex ARGENTO.

ARGENTUM NITRATUM.

℞ Argenti P. unciam unam,
Acidi nitroſi diluti M. uncias quatuor.

Argentum acido nitroſo in vaſe vitreo ſuper arenam calidam ſolve; deinde, calore leniter aucto, exſicca: poſtea in crucibulo liqua, ut in formas idoneas effundatur, cautim evitato nimio calore.

PRÆPARATA e FERRO.

FERRUM AMMONIACALE.

℞ Ferri ramentorum P. libram unam,
Salis ammoniaci P. libras duas.

Miſce, et ſublima. Quod reſtat in vaſis fundo cum materia ſublimata ſimul terendo miſce, iterumque ſublima.

FERRI RUBIGO.

℞ Ferri ramentorum P. libram unam.

Ferrum, aëri ſubjectum, humecta ſæpius aqua, donec in rubiginem exeſum fuerit; deinde tere in mortario ferreo, et affuſa aqua diſtillata pulverem ſubtiliſſimum ablue: reſiduum vero, quod modico tritu in pulverem facile abluendum non redigitur, humectatum aëri diutius ſubjice; et denuo in mortario tritum ablue, ut prius. Pulvis ablutus ſiccetur.

FERRUM TARTARISATUM.

℞ Ferri ramentorum P. libram unam,
Cryſtallorum tartari tritarum P. libras duas.

Miſce cum aqua diſtillata in materiam craſſam, quam in patulo vaſe figulino dies octo aëri expone; dein materiam balneo arenæ arefactam in pulverem ſubtiliſſimum tere.

FERRUM VITRIOLATUM.

℞ Ferri ramentorum,
Acidi vitriolici, ſingulorum P. uncias octo,
Aquæ diſtillatæ M. libras tres.

Miſce in vaſe vitreo, et quum ceſſaverit ebullire, ſiſte aliquamdiu ſuper arenam calidam; deinde liquorem effuſum per chartam cola, et poſt idoneam exhalationem ſepone ut fiant cryſtalli.

PRÆPARATA ex HYDRARGYRO.

HYDRARGYRUS PURIFICATUS.

℞ Hydrargyri,
Ferri ramentorum, ſingulorum P. libras quatuor.

Tere ſimul, et diſtilla e vaſe ferreo.

HYDRARGYRUS ACETATUS.

℞ Hydrargyri purificati P. libram unam,
Acidi nitroſi diluti P. libras duas,
Aquæ kali quantum ſatis ſit.

Hydrargyrum cum acido miſce in vaſe vitreo, et in balneo arenæ ſolve: tum paulatim inſtilla aquam kali ut calx hydrargyri dejiciatur. Hanc aqua diſtillata plurima lava, et leni calore ſicca. His peractis,

℞ Calcis hydrargyri modo defcriptæ P. libram unam,
Acidi acetofi quantum calci folvendæ fufficiat.

Mifce in vafe vitreo, et, hydrargyro foluto, liquorem per chartam cola; dein vaporet liquor donec pellicula appareat, et feponatur ut fiant cryftalli. Has in vafe obturato repone.

HYDRARGYRUS CALCINATUS.

℞ Hydrargyri purificati P. libram unam.

Hydrargyrum in cucurbita vitrea, cui fundus planior fit, in balneo arenæ calori 600° circiter expone, donec in pulverem rubrum concreverit.

HYDRARGYRUS CUM CRETA.

℞ Hydrargyri purificati P. uncias tres,
Cretæ in pulverem tritæ P. uncias quinque.

Tere fimul donec globuli vifum fugerint.

HYDRARGYRUS MURIATUS.

℞ Hydrargyri purificati,
Acidi vitriolici, fingulorum P. libras duas,
Salis muriatici ficcati P. libras tres cum femiffe.

Misce hydrargyrum cum acido in vase vitreo, et coque in balneo arenæ donec materia exsiccata fuerit. Materiam frigefactam misce in vase vitreo cum sale muriatico; tum in cucurbita vitrea sublima calore sensim aucto. Dein materia sublimata a scoriis separetur.

CALOMELAS.

℞ Hydrargyri muriati P. libram unam,
Hydrargyri purificati P. uncias novem.

Tere simul donec globuli visum fugerint, et sublima; dein materiam totam iterum tere, et sublima. Eodem modo sublimationem quater repete. Postea materiam in pulverem subtilissimum tere, et affusa aqua distillata fervente lava.

HYDRARGYRUS MURIATUS MITIS.

℞ Hydrargyri purificati,
Acidi nitrosi diluti, singulorum P. libram dimidiam.

Misce in vase vitreo, et sepone donec hydrargyrus solvatur. Ferveant, ut solvatur sal. Liquorem

rem ferventem effunde in vas vitreum, cui prius inditus ſit alius liquor fervens, qui conſtat ex

Salis muriatici P. unciis quatuor,
Aquæ diſtillatæ M. libris octo.

Poſtquam pulvis albus ad fundum vaſis deſcenderit, effundatur liquor limpidus ſupernatans; et pulvis remanens affuſa ſæpius aqua calida lavetur donec inſipidus ſit. Tum charta bibula exceptus leni calore exſiccetur.

HYDRARGYRUS NITRATUS RUBER.

℞ Hydrargyri purificati,
Acidi nitroſi, ſingulorum P. libram unam,
Acidi muriatici P. drachmam unam.

Miſce in vaſe vitreo, et in balneo arenæ ſolve hydrargyrum; tum ignem auge, donec materia in cryſtallos rubras abeat.

CALX HYDRARGYRI ALBA.

℞ Hydrargyri muriati,
Salis ammoniaci,
Aquæ kali, ſingulorum P. libram dimidiam.

Solve

Solve primo ſalem ammoniacum, et dein hydrargyrum muriatum, aqua diſtillata, et adde aquam kali. Pulverem præcipitatum lava donec ſapore careat.

HYDRARGYRUS CUM SULPHURE.

℞ Hydrargyri purificati,
Florum ſulphuris, ſingulorum P. libram unam.

Tere ſimul donec globuli viſum fugerint.

HYDRARGYRUS SULPHURATUS RUBER.

℞ Hydrargyri purificati P. uncias quadraginta,
Sulphuris P. uncias octo.

Sulphuri liquefacto hydrargyrum immiſce. Si miſtura flammam concipiat, eam extingue vas tegendo; deinde in pulverem tere materiam, et ſublima.

HYDRARGYRUS VITRIOLATUS.

℞ Hydrargyri purificati,
Acidi vitriolici, ſingulorum P. libram unam.

Miſce in vaſe vitreo, et gradatim incaleſcant donec in materiam albam coëant, quæ igne forti penitus exſiccanda eſt. Hæc materia, plurima aqua calida diſtillata affuſa, ſtatim flaveſcet, et in pulverem fatiſcet. Pulverem tere diligenter cum hac aqua in mortario vitreo. Poſtquam pulvis ſubſederit, aquam effunde; et, plus aquæ diſtillatæ aliquoties adjiciens, lava materiam donec ſaporis expers ſit.

PRÆPARATA e PLUMBO.

CERUSSA ACETATA.

℞ Ceruſſæ P. libram unam,
Aceti diſtillati congium unum cum ſemiſſe.

Coque ceruſſam cum aceto donec acetum ſaturetur; deinde per chartam cola, et poſt idoneam exhalationem ſepone ut fiant cryſtalli.

AQUA LITHARGYRI ACETATI.

℞ Lithargyri P. libras duas cum unciis quatuor,
Aceti diſtillati congium unum.

Miſce, et decoque ad libras ſex, aſſidue agitans; dein ſepone. Poſtquam fæces ſubſederint, cola.

PRÆPARATUM e STANNO.

STANNUM PULVERATUM.

℞ Stanni P. libras ſex.

In vaſe ferreo liqua, et agita baculo ferreo donec pulvis ſupernatet. Pulverem aufer, et frigefactum per cribrum trajice.

PRÆPARATA e ZINCO.

ZINCUM CALCINATUM.

℞ Zinci in fruſtula fracti P. uncias octo.

Zincum candenti crucibulo capaci alto et inclinato per vices injice, crucibulum aliud ſuperimponens, adeo ut aëri liceat zincum candens attingere. Calcem, ſimul ac illa appareat, exime, et partem ejus albam et leviorem per cribrum trajice.

ZINCUM VITRIOLATUM PURIFICATUM.

℞ Vitrioli albi P. libram unam,
Acidi vitriolici P. drachmam unam,
Aquæ diſtillatæ ferventis M. libras tres.

Miſce, et per chartam cola; poſt idoneam exhalationem ſepone in loco frigido ut fiant cryſtalli.

AQUÆ DISTILLATÆ.

AQUA DISTILLATA.

℞ Aquæ fontanæ congios decem.

Diſtilla primum libras quatuor; quibus abjectis, diſtilla congios quatuor. Aquam hanc ſerva in lagena vitrea, vel fictili, vitreo epiſtomio obturata.

AQUA ANETHI.

℞ Seminum anethi contuſorum P. libram unam,
Aquæ quod ſatis ſit ad præcavendum empyreuma.

Diſtilla congium unum.

AQUA CINNAMOMI.

℞ Corticis cinnamomi contuſi P. libram unam,
Aquæ quod ſatis ſit ad præcavendum empyreuma.

Macera per horas quatuor et viginti, et diſtilla congium unum.

AQUA FOENICULI.

℞ Seminum fœniculi dulcis contuſorum P. libram unam,
Aquæ quod ſatis ſit ad præcavendum empyreuma.

Diſtilla congium unum.

AQUA MENTHÆ PIPERITIDIS.

℞ Herbæ menthæ piperitidis exſiccatæ P. libram unam cum ſemiſſe,
Aquæ quod ſatis ſit ad præcavendum empyreuma.

Diſtilla congium unum.

AQUA MENTHÆ SATIVÆ.

℞ Herbæ menthæ ſativæ exſiccatæ P. libram unam cum ſemiſſe,
Aquæ quod ſatis ſit ad præcavendum empyreuma.

Diſtilla congium unum.

AQUA PIMENTO.

℞ Baccarum pimento contuſarum P. libram dimidiam,
Aquæ quod ſatis ſit ad præcavendum empyreuma.

Macera per horas quatuor et viginti, et diſtilla congium unum.

AQUA PULEGII.

℞ Herbæ pulegii exſiccatæ P. libram unam cum ſemiſſe,
Aquæ quod ſatis ſit ad præcavendum empyreuma.

Diſtilla congium unum.

AQUA ROSÆ.

℞ Petalorum roſæ damaſcenæ recentium, demtis unguibus, P. libras ſex,
Aquæ quod ſatis ſit ad præcavendum empyreuma.

Diſtilla congium unum.

Aquas

Aquas ex herbis exſiccatis diſtillandas eſſe præcepimus, quoniam recentes omni anni tempore non præſto ſunt. Quandocunque hæ in uſum veniunt, pondera ſunt augenda. Sive autem recentes, ſeu exſiccatæ adhibeantur, opifici licet pondus variare, ratione habita ad cœli tempeſtatem, ſub quo herbæ ſuccreverint et fuerint collectæ.

Herbæ et ſemina, ultra unius anni ſpatium ſervata, minus idonea ſunt quæ aquarum diſtillationi inſerviant.

Singulis harum aquarum congiis adde ſpiritus vinoſi tenuioris M. uncias quinque.

SPIRITUS DISTILLATI.

ALKOHOL.

℞ Spiritus vinoſi rectificati congium unum,
Kali calidi P. libram unam cum ſemiſſe,
Kali puri P. unciam unam.

Miſce ſpiritum vinoſum cum kali puro; et dein adde kali calidi libram unam, agita, et digere per horas quatuor et viginti. Effunde ſpiritum, cui adde kali quod reliquum eſt, et diſtilla balneo aquoſo. Servetur in vaſe bene obturato.

Alkoholis pondus ſpecificum eſt ad pondus aquæ diſtillatæ ut 815 ad 1,000.

SPIRITUS ÆTHERIS VITRIOLICI.

℞ Spiritus vinoſi rectificati,
Acidi vitriolici, ſingulorum P. libram unam.

Paulatim

Paulatim acidum ſpiritui infunde, et agitando miſce; dein ex retorta in receptaculum tubulatum, cui aptetur vas recipiens, lento igne diſtilla ſpiritum ætheris vitriolici, donec vapores ſulphurei inceperint oriri.

ÆTHER VITRIOLICUS.

℞ Spiritus ætheris vitriolici P. libras duas,
Aquæ kali puri M. unciam unam.

Agita ſimul, et leni calore diſtilla M. uncias quatuordecim.

SPIRITUS ÆTHERIS NITROSI.

℞ Spiritus vinoſi rectificati M. libras duas,
Acidi nitroſi P. libram dimidiam.

Miſce affundendo acidum ſpiritui, et diſtilla leni calore P. libram unam cum unciis decem.

SPIRITUS AMMONIÆ.

℞ Spiritus vinoſi tenuioris M. libras tres,
Salis ammoniaci P. uncias quatuor,
Cinerum clavellatorum P. uncias ſex.

Miſce, et diſtilla lento igne M. libram unam cum ſemiſſe.

SPIRITUS AMMONIÆ FOETIDUS.

℞ Spiritus vinoſi tenuioris M. libras ſex,
Salis ammoniaci P. libram unam,
Aſæ fœtidæ P. uncias quatuor,
Cinerum clavellatorum P. libram unam cum ſemiſſe.

Miſce, et diſtilla lento igne M. libras quinque.

SPIRITUS ANISI COMPOSITUS.

℞ Seminum contuſorum aniſi,
angelicæ, ſingulorum P. libram dimidiam,
Spiritus vinoſi tenuioris congium unum,
Aquæ quod ſatis ſit ad præcavendum empyreuma.

Diſtilla congium unum.

SPIRITUS CARUI.

℞ Seminum carui contuſorum P. libram dimidiam,
Spiritus vinoſi tenuioris congium unum,
Aquæ quod ſatis ſit ad præcavendum empyreuma.

Diſtilla congium unum.

SPIRITUS CINNAMOMI.

℞ Corticis cinnamomi contufi P. libram unam,
Spiritus vinofi tenuioris congium unum,
Aquæ quod fatis fit ad præcavendum empyreuma.

Diftilla congium unum.

SPIRITUS JUNIPERI COMPOSITUS.

℞ Baccarum juniperi contufarum P. libram unam,
Seminum carui contuforum,
Seminum fœniculi dulcis, fingulorum P. unciam unam cum femiffe,
Spiritus vinofi tenuioris congium unum,
Aquæ quod fatis fit ad præcavendum empyreuma.

Diftilla congium unum.

SPIRITUS LAVENDULÆ.

℞ Florum recentium lavendulæ P. libram unam cum ſemiſſe,
Spiritus vinoſi tenuioris congium unum.

Diſtilla balneo aquoſo M. libras quinque.

SPIRITUS MENTHÆ PIPERITIDIS.

℞ Herbæ menthæ piperitidis exſiccatæ P. libram unam cum ſemiſſe,
Spiritus vinoſi tenuioris congium unum,
Aquæ quod ſatis ſit ad præcavendum empyreuma.

Diſtilla congium unum.

SPIRITUS MENTHÆ SATIVÆ.

℞ Herbæ menthæ ſativæ exſiccatæ P. libram unam cum ſemiſſe,
Spiritus vinoſi tenuioris congium unum,
Aquæ quod ſatis ſit ad præcavendum empyreuma.

Diſtilla congium unum.

SPIRITUS

SPIRITUS NUCIS MOSCHATÆ.

℞ Nucis moſchatæ contuſæ P. uncias duas,
Spiritus vinoſi tenuioris congium unum,
Aquæ quod ſatis ſit ad præcavendum empyreuma.

Diſtilla congium unum.

SPIRITUS PIMENTO.

℞ Baccarum contuſarum pimento P. uncias duas,
Spiritus vinoſi tenuioris congium unum,
Aquæ quod ſatis ſit ad præcavendum empyreuma.

Diſtilla congium unum.

SPIRITUS PULEGII.

℞ Herbæ pulegii exſiccatæ P. libram unam cum ſemiſſe,
Spiritus vinoſi tenuioris congium unum,
Aquæ quod ſatis ſit ad præcavendum empyreuma.

Diſtilla congium unum.

SPIRITUS RAPHANI COMPOSITUS.

℞ Radicis raphani ruſticani recentis,
Corticis exterioris aurantiorum hiſpalenſium exſiccati, ſingulorum P. libras duas,
Herbæ cochleariæ hortenſis recentis P. libras quatuor,
Nucis moſchatæ contuſæ P. unciam unam,
Spiritus vinoſi tenuioris congios duos,
Aquæ quod ſatis ſit ad præcavendum empyreuma.

Diſtilla congios duos.

SPIRITUS RORISMARINI.

℞ Cacuminum roriſmarini recentium P. libram unam cum ſemiſſe,
Spiritus vinoſi tenuioris congium unum.

Diſtilla balneo aquoſo M. libras quinque.

DECOCTA

DECOCTA et INFUSA.

DECOCTUM CORNU CERVI.

℞ Cornu cervi uſti et præparati P. uncias duas,
Arabici gummi P. drachmas ſex,
Aquæ diſtillatæ M. libras tres.

Decoque aſſidue agitans ad libras duas, et cola.

DECOCTUM CORTICIS PERUVIANI.

℞ Corticis peruviani in pulverem triti P. unciam unam,
Aquæ diſtillatæ M. libram unam cum unciis tribus.

Coque per horæ partem ſextam in vaſe operto, et liquorem adhuc calentem cola.

DECOCTUM pro ENEMATE.

℞ Foliorum exſiccatorum malvæ P. unciam unam,
Florum chamæmeli exſiccatorum P. unciam dimidiam,
Aquæ M. libram unam.

Coque, et cola.

DECOCTUM pro FOMENTO.

℞ Foliorum abrotoni exſiccatorum,
Cacuminum abſinthii maritimi exſiccatorum,
Florum chamæmeli exſiccatorum, ſingulorum P. unciam unam,
Foliorum lauri exſiccatorum P. unciam dimidiam,
Aquæ diſtillatæ M. libras ſex.

Pauliſper coque, et cola.

DECOCTUM HELLEBORI.

℞ Radicis hellebori albi in pulverem tritæ P. unciam unam,
Aquæ diſtillatæ M. libras duas,
Spiritus vinoſi rectificati P. uncias duas.

Aquam cum radice decoque ad libram unam, et liquori frigefacto et colato adde ſpiritum.

DECOCTUM

DECOCTUM HORDEI.

℞ Hordei perlati P. uncias duas,
Aquæ diſtillatæ M. libras quatuor.

Res alienas, hordeo adhærentes, aqua frigida primum ablue; deinde, affuſa aquæ libra circiter dimidia, hordeum pauliſper coque. Hac aqua abjecta, hordeo aquam diſtillatam ferventem adde; decoque ad libras duas, et cola.

DECOCTUM HORDEI COMPOSITUM.

℞ Decocti hordei M. libras duas,
Uvarum paſſarum, acinis exemtis,
Caricarum inciſarum, ſingulorum P. uncias duas,
Radicis glycyrrhizæ inciſæ et contuſæ P. unciam dimidiam,
Aquæ diſtillatæ M. libram unam.

Decoque ad libras duas, et cola.

DECOCTUM SARSAPARILLÆ.

℞ Radicis ſarſaparillæ inciſæ P. uncias ſex,
Aquæ diſtillatæ M. libras octo.

Macera per duas horas calore circiter 195°; dein radicem exime, et contunde; contuſam liquori redde, et iterum macera per horas duas. Deinde liquorem, ad menſuram quatuor librarum decoctum, exprime, et cola.

DECOCTUM SARSAPARILLÆ COMPOSITUM.

℞ Radicis ſarſaparillæ inciſæ et contuſæ P. uncias ſex,
Corticis radicis ſaſſafras,
Ligni guaiaci raſi,
Radicis glycyrrhizæ contuſæ, ſingulorum P. unciam unam,
Corticis radicis mezerei P. drachmas tres,
Aquæ diſtillatæ M. libras decem.

Macera leni calore per horas ſex; dein decoque ad menſuram librarum quinque, ſub finem coctionis addens corticem radicis mezerei. Cola liquorem.

DECOCTUM ULMI.

℞ Corticis interioris recentis ulmi contusi p. uncias quatuor,
Aquæ distillatæ m. libras quatuor.

Decoque ad libras duas, et cola.

MUCILAGO AMYLI.

℞ Amyli p. drachmas tres,
Aquæ distillatæ m. libram unam.

Amylum tere, paulatim addens aquam distillatam; dein coque paulisper.

MUCILAGO ARABICI GUMMI.

℞ Arabici gummi in pulverem triti p. uncias quatuor,
Aquæ distillatæ ferventis m. uncias octo.

Tere gummi in aqua donec solvatur.

MUCILAGO SEMINIS CYDONII MALI.

℞ Seminum cydonii mali P. drachmam unam,
Aquæ diſtillatæ M. uncias octo.

Coque lento igne donec aqua lenteſcat; dein per linteum trajice.

INFUSUM GENTIANÆ COMPOSITUM.

℞ Radicis gentianæ P. drachmam unam,
Corticis exterioris limonum recentium P. unciam dimidiam,
Corticis exterioris aurantiorum hiſpalenſium exſiccati P. drachmam unam cum ſemiſſe,
Aquæ ferventis M. uncias duodecim.

Macera per horam unam, et cola.

INFUSUM SENNÆ SIMPLEX.

℞ Sennæ P. unciam unam cum ſemiſſe,
Zingiberis in pulverem triti P. drachmam unam,
Aquæ diſtillatæ ferventis M. libram unam.

Macera per horam unam in vaſe operto, et liquorem frigefactum cola.

INFUSUM SENNÆ TARTARISATUM.

℞ Sennæ P. unciam unam cum ſemiſſe,
Seminum coriandri contuſorum P. unciam dimidiam,
Cryſtallorum tartari P. drachmas duas,
Aquæ diſtillatæ M. libram unam.

Cryſtallos tartari in aqua coquendo ſolve; deinde liquorem adhuc ferventem ſennæ et ſeminibus affunde; macera per horam unam in vaſe operto, et frigefactum cola.

AQUA

AQUA CALCIS.

℞ Calcis P. libram dimidiam,
Aquæ diſtillatæ ferventis M. libras duodecim.

Miſce, et ſepone in vaſe operto per horam unam; dein effunde liquorem, quem in vaſe obturato ſerva.

INFUSUM ROSÆ.

℞ Petalorum roſæ rubræ nondum explicitorum, demtis unguibus, P. unciam dimidiam,
Acidi vitriolici diluti P. drachmas tres,
Aquæ diſtillatæ ferventis M. libras duas cum ſemiſſe,
Sacchari purificati P. unciam unam cum ſemiſſe.

Aquam primo petalis affunde in vaſe vitreo; dein adde acidum vitriolicum dilutum, et macera per horam dimidiam. Liquorem frigefactum cola, et adjice ſaccharum.

ACETUM SCILLÆ.

℞ Scillarum recens exſiccatarum P. libram unam,
Aceti M. libras ſex,
Spiritus vinoſi tenuioris M. libram dimidiam.

Macera ſcillas in aceto leni calore in vaſe vitreo per horas quatuor et viginti. Deinde liquorem exprime, et ſepone ut fæces ſubſidant; denique liquori effuſo adde ſpiritum.

VINA MEDICATA.

VINUM ALOËS.

℞ Aloës ſocotorinæ P. uncias octo,
Canellæ albæ P. uncias duas,
Vini albi hiſpanici M. libras ſex,
Spiritus vinoſi tenuioris M. libras duas.

Aloën et canellam albam ſeparatim in pulverem tere; miſtis vinum affunde. Deinde digere per dies quatuordecim ſubinde agitans; et poſtremo cola.

Non alienum erit arenam albam, ſordibus purgatam, pulveri miſcere, quo minus concreſcat aloë madefacta.

VINUM ANTIMONII.

℞ Antimonii vitrificati in pulverem triti P. unciam unam,
Vini albi hiſpanici M. libram unam cum ſemiſſe.

Digere per dies duodecim ſæpe agitans; et per chartam cola.

VINUM ANTIMONII TARTARISATI.

℞ Antimonii tartarifati P. fcrupulos duos,
Aquæ diftillatæ ferventis M. uncias duas,
Vini albi hifpanici M. uncias octo.

Solve antimonium tartarifatum in aqua diftillata ferventi, et adde vinum.

VINUM FERRI.

℞ Ferri ramentorum P. uncias quatuor,
Vini albi hifpanici M. libras quatuor.

Digere per menfem fæpe agitans, et cola.

VINUM IPECACUANHÆ.

℞ Radicis ipecacuanhæ contufæ P. uncias duas,
Vini albi hifpanici M. libras duas.

Digere per dies decem, et cola.

VINUM RHABARBARI.

℞ Rhabarbari incisi P. uncias duas cum semisse,
Seminum cardamomi minoris contusorum, demtis capsulis, P. unciam dimidiam,
Croci P. drachmas duas,
Vini albi hispanici M. libras duas,
Spiritus vinosi tenuioris M. uncias octo.

Digere per dies decem, et cola.

TINCTURÆ.

TINCTURA ALOËS.

℞ Aloës ſocotorinæ in pulverem tritæ P. unciam dimidiam,
Extracti glycyrrhizæ P. unciam unam cum ſemiſſe,
Aquæ diſtillatæ,
Spiritus vinoſi tenuioris, ſingulorum M. uncias octo.

Digere in balneo arenæ vas ſubinde agitans donec ſolvatur extractum, et cola.

TINCTURA ALOËS COMPOSITA.

℞ Tincturæ myrrhæ M. libras duas,
Croci,
Aloës ſocotorinæ, ſingulorum P. uncias tres.

Digere per dies octo, et cola.

TINCTURA ASÆ FOETIDÆ.

℞ Aſæ fœtidæ P. uncias quatuor,
Spiritus vinoſi rectificati M. libras duas.

Digere leni calore per dies ſex, et cola.

TINCTURA BALSAMI PERUVIANI.

℞ Balſami peruviani P. uncias quatuor,
Spiritus vinoſi rectificati M. libram unam.

Digere donec ſolvatur balſamum.

TINCTURA BALSAMI TOLUTANI.

℞ Balſami tolutani P. unciam unam cum ſemiſſe,
Spiritus vinoſi rectificati M. libram unam.

Digere donec ſolvatur balſamum, et cola.

TINCTURA

TINCTURA BENZOËS COMPOSITA.

℞ Benzoës P. uncias tres,
Styracis colati P. uncias duas,
Balſami tolutani P. unciam unam,
Aloës ſocotorinæ P. unciam dimidiam,
Spiritus vinoſi rectificati M. libras duas.

Digere leni calore per triduum, et cola.

TINCTURA CANTHARIDIS.

℞ Cantharidum contuſarum P. drachmas duas,
Coccinellarum in pulverem tritarum P. drachmam dimidiam,
Spiritus vinoſi tenuioris M. libram unam cum ſemiſſe.

Digere per dies octo, et cola.

TINCTURA CARDAMOMI.

℞ Seminum cardamomi minoris contuſorum, demtis capſulis, P. uncias tres,
Spiritus vinoſi tenuioris M. libras duas.

Digere per dies octo, et cola.

TINCTURA

TINCTURA CARDAMOMI COMPOSITA.

℞ Seminum cardamomi minoris, demtis capſulis,
carui,
Coccinellarum, ſingulorum in pulverem tritorum, P. drachmas duas,
Corticis cinnamomi contuſi P. unciam dimidiam,
Uvarum paſſarum, demtis acinis, P. uncias quatuor,
Spiritus vinoſi tenuioris M. libras duas.

Digere per dies quatuordecim, et cola.

TINCTURA CASCARILLÆ.

℞ Corticis caſcarillæ in pulverem triti P. uncias quatuor,
Spiritus vinoſi tenuioris M. libras duas.

Digere leni calore per dies octo, et cola.

TINCTURA

TINCTURA CASTOREI.

℞ Castorei russici in pulverem triti P. uncias duas,
Spiritus vinosi tenuioris M. libras duas.

Digere per dies decem, et cola.

TINCTURA CATECHU.

℞ Catechu P. uncias tres,
Corticis cinnamomi contusi P. uncias duas,
Spiritus vinosi tenuioris M. libras duas.

Digere per triduum, et cola.

TINCTURA CINNAMOMI.

℞ Corticis cinnamomi contusi P. unciam unam cum semisse,
Spiritus vinosi tenuioris M. libram unam.

Digere per dies decem, et cola.

TINCTURA

TINCTURA CINNAMOMI COMPOSITA.

℞ Corticis cinnamomi contuſi P. drachmas ſex,
Seminum cardamomi minoris, demtis capſulis, P. drachmas tres,
Piperis longi,
Zingiberis, ſingulorum in pulverem tritorum, P. drachmas duas,
Spiritus vinoſi tenuioris M. libras duas.

Digere per dies octo, et cola.

TINCTURA COLOMBÆ.

℞ Radicis colombæ in pulverem tritæ P. uncias duas cum ſemiſſe,
Spiritus vinoſi tenuioris M. libras duas.

Digere per dies octo, et cola.

TINCTURA CORTICIS AURANTII.

℞ Corticis exterioris aurantiorum hiſpalenſium recentis P. uncias tres,
Spiritus vinoſi tenuioris M. libras duas.

Digere per triduum, et cola.

TINCTURA

TINCTURA CORTICIS PERUVIANI.

℞ Corticis peruviani in pulverem triti P. uncias quatuor,
Spiritus vinoſi tenuioris M. libras duas.

Digere leni calore per dies octo, et cola.

TINCTURA CORTICIS PERUVIANI COMPOSITA.

℞ Corticis peruviani in pulverem triti P. uncias duas,
Corticis exterioris aurantiorum hiſpalenſium exſiccati P. unciam unam cum ſemiſſe,
Radicis ſerpentariæ virginianæ contuſæ P. drachmas tres,
Croci P. drachmam unam,
Coccinellarum in pulverem tritarum P. ſcrupulos duos,
Spiritus vinoſi tenuioris M. uncias viginti.

Digere per dies quatuordecim, et cola.

TINCTURA FERRI MURIATI.

℞ Ferri rubiginis P. libram dimidiam,
Acidi muriatici P. libras tres,
Spiritus vinosi rectificati M. libras tres.

Ferri rubigini affunde acidum muriaticum in vase vitreo; et mista per triduum subinde agita. Sepone ut fæces subsidant, dein liquorem effunde. Hunc per exhalationem ad libram unam absume, et frigefacto spiritum vinosum adde.

TINCTURA GALBANI.

℞ Galbani minutim incisi P. uncias duas,
Spiritus vinosi tenuioris M. libras duas.

Digere leni calore per dies octo, et cola.

TINCTURA GENTIANÆ COMPOSITA.

℞ Radicis gentianæ incisæ et contusæ P. uncias duas,
Corticis exterioris aurantiorum hispalensium exsiccati P. unciam unam,
Seminum cardamomi minoris contusorum, demtis capsulis, P. unciam dimidiam,
Spiritus vinosi tenuioris M. libras duas.

Digere per dies octo, et cola.

TINCTURA GUAIACI.

℞ Gummi guaiaci P. uncias quatuor,
Spiritus ammoniæ compositi M. libram unam cum semisse.

Digere per triduum, et cola.

TINCTURA HELLEBORI NIGRI.

℞ Radicis hellebori nigri in pulverem crassum tritæ P. uncias quatuor,
Coccinellarum in pulverem tritarum P. scrupulos duos,
Spiritus vinosi tenuioris M. libras duas.

Digere leni calore per dies octo, et cola.

TINCTURA JALAPII.

℞ Radicis jalapii in pulverem tritæ P. uncias octo,
Spiritus vinosi tenuioris M. libras duas.

Digere leni calore per dies octo, et cola.

TINCTURA LAVENDULÆ COMPOSITA.

℞ Spiritus lavendulæ M. libras tres,
rorifmarini M. libram unam,
Corticis cinnamomi contufi,
Nucis mofchatæ contufæ, fingulorum P. unciam dimidiam,
Santali rubri P. unciam unam.

Digere per dies decem, et cola.

TINCTURA MYRRHÆ.

℞ Myrrhæ contufæ P. uncias tres,
Spiritus vinofi tenuioris M. libram unam cum femiffe,
rectificati M. libram dimidiam.

Digere leni calore per dies octo, et cola.

TINCTURA OPII.

℞ Opii purificati duri in pulverem triti P. drachmas decem,
Spiritus vinofi tenuioris M. libram unam.

Digere per dies decem, et cola.

TINCTURA

TINCTURA OPII CAMPHORATA.

℞ Opii purificati duri,
Florum benzoës, ſingulorum P. drachmam unam,
Camphoræ P. ſcrupulos duos,
Olei eſſentialis aniſi P. drachmam unam,
Spiritus vinoſi tenuioris M. libras duas.

Digere per triduum.

TINCTURA RHABARBARI.

℞ Rhabarbari inciſi P. uncias duas,
Seminum cardamomi minoris contuſorum, demtis capſulis, P. unciam dimidiam,
Croci P. drachmas duas,
Spiritus vinoſi tenuioris M. libras duas.

Digere per dies octo, et cola.

TINCTURA

TINCTURA
RHABARBARI COMPOSITA.

℞ Rhabarbari incisi P. uncias duas,
Zingiberis in pulverem triti,
Croci, singulorum P. drachmas duas,
Radicis glycyrrhizæ contusæ P. unciam dimidiam,
Aquæ distillatæ M. libram unam,
Spiritus vinosi tenuioris M. uncias duodecim.

Digere per dies quatuordecim, et cola.

TINCTURA SABINÆ COMPOSITA.

℞ Extracti sabinæ P. unciam unam,
Tincturæ castorei M. libram unam,
myrrhæ M. libram dimidiam.

Digere donec solvatur extractum sabinæ, et cola.

TINCTURA

TINCTURA SCILLÆ.

℞ Scillarum recens exſiccatarum P. uncias quatuor,
Spiritus vinoſi tenuioris M. libras duas.

Digere per dies octo, et liquorem effunde.

TINCTURA SENNÆ.

℞ Sennæ P. libram unam,
Seminum carui contuſorum P. unciam unam cum ſemiſſe,
cardamomi minoris contuſorum, demtis capſulis, P. unciam dimidiam,
Uvarum paſſarum, demtis acinis, P. uncias ſedecim,
Spiritus vinoſi tenuioris congium unum.

Digere per dies quatuordecim, et cola.

TINCTURA SERPENTARIÆ.

℞ Radicis ſerpentariæ virginianæ P. uncias tres,
Spiritus vinoſi tenuioris M. libras duas.

Digere per dies octo, et cola.

TINCTURA VALERIANÆ.

℞ Radicis valerianæ ſylveſtris in pulverem craſſum tritæ P. uncias quatuor,
Spiritus vinoſi tenuioris M. libras duas.

Digere leni calore per dies octo, et cola.

TINCTURA VALERIANÆ VOLATILIS.

℞ Radicis valerianæ ſylveſtris P. uncias quatuor,
Spiritus ammoniæ compoſiti M. libras duas.

Digere per dies octo, et cola.

NOTA.

Tincturæ omnes in vaſe obturato fieri debent; tinctura ferri muriati excepta.

MISTURÆ.

MISTURÆ.

MISTURA CAMPHORATA.

℞ Camphoræ P. drachmam unam,
Spiritus vinosi rectificati guttas decem,
Sacchari purificati P. unciam dimidiam,
Aquæ distillatæ ferventis M. libram unam.

Camphoram primum cum spiritu vinoso rectificato tere, deinde cum saccharo; denique aquam sensim adde, et misturam cola.

MISTURA CRETACEA.

℞ Cretæ præparatæ P. unciam unam,
Sacchari purificati P. drachmas sex,
Arabici gummi in pulverem triti P. uncias duas,
Aquæ distillatæ M. libras duas.

Misce.

MISTURA MOSCHATA.

℞ Aquæ roſæ M. uncias ſex,
Moſchi P. ſcrupulos duos,
Arabici gummi in pulverem triti,
Sacchari purificati, ſingulorum P. drachmam unam.

Moſchum tere primum cum ſaccharo, dein cum gummi; et gradatim adde aquam roſæ.

LAC AMYGDALÆ.

℞ Amygdalarum dulcium P. unciam unam cum ſemiſſe,
Sacchari purificati P. unciam dimidiam,
Aquæ diſtillatæ M. libras duas.

Amygdalas cum ſaccharo contunde; dein, ſimul terens, aquam gradatim adjice, et liquorem cola.

LAC AMMONIACI.

℞ Ammoniaci P. drachmas duas,
Aquæ diſtillatæ M. libram dimidiam.

Tere gummi-reſinam cum aqua gradatim affuſa donec in lac abeat.

Eodem modo fiat laç aſæ fœtidæ, et cæterarum gummi-reſinarum.

SPIRITUS ÆTHERIS VITRIOLICI COMPOSITUS.

℞ Spiritus ætheris vitriolici P. libras duas,
Olei vini P. drachmas tres.

Miſce.

SPIRITUS AMMONIÆ COMPOSITUS.

℞ Spiritus ammoniæ M. libras duas,
Olei eſſentialis limonis,
nucis moſchatæ, ſingulorum P. drachmas duas.

Miſce.

SPIRITUS AMMONIÆ SUCCINATUS.

℞ Alkoholis P. unciam unam,
Aquæ ammoniæ puræ M. uncias quatuor,
Olei ſuccini rectificati P. ſcrupulum unum,
Saponis grana decem.

Digere ſaponem et oleum ſuccini in alkohole donec ſolvantur; dein adde aquam ammoniæ puræ, et miſce agitando.

SPIRITUS CAMPHORATUS.

℞ Camphoræ P. uncias quatuor,
Spiritus vinosi rectificati M. libras duas.

Misce ut camphora solvatur.

SYRUPI.

In ſyrupis conficiendis, ubi nec ſacchari pondus nec modum, quo id ſolvi debeat, præcepimus, hæc ſit norma.

℞ Sacchari purificati P. uncias novem et viginti,
Liquoris cujuſcunque M. libram unam.

Solve ſaccharum in liquore balneo aquoſo; dein ſepone per horas quatuor et viginti. Spumam aufer, et e fæcibus, ſi quæ ſint, effunde ſyrupum.

SYRUPUS ALTHÆÆ.

℞ Radicis recentis althææ contuſæ P. libram unam,
Sacchari purificati P. libras quatuor,
Aquæ diſtillatæ congium unum.

Decoque aquam cum radice althææ ad dimidium; et liquorem frigefactum exprime. Sepone per horas duodecim, et, poſtquam fæces ſubſederint, liquorem effunde. Addito ſaccharo, ad ſex librarum pondus decoque.

SYRUPUS CARYOPHYLLI RUBRI.

℞ Florum caryophylli rubri recentium, demtis unguibus, P. libras duas,
Aquæ diſtillatæ ferventis M. libras ſex.

Macera per horas duodecim in vaſe vitreo; et in liquore colato ſolve ſaccharum purificatum ut fiat ſyrupus.

SYRUPUS CORTICIS AURANTII.

℞ Corticis exterioris aurantiorum hiſpalenſium recentis P. uncias octo,
Aquæ diſtillatæ ferventis M. libras quinque.

Macera per horas duodecim in vaſe operto; et in liquore colato ſolve ſaccharum purificatum ut fiat ſyrupus.

SYRUPUS

SYRUPUS CROCI.

℞ Croci P. unciam unam,
Aquæ diſtillatæ ferventis M. libram unam.

Macera crocum aqua per horas duodecim in vaſe operto; liquore colato ſolve ſaccharum purificatum ut fiat ſyrupus.

SYRUPUS SUCCI LIMONIS.

℞ Succi limonum, poſtquam fæces ſubſederint, colati M. libras duas,
Sacchari purificati P. uncias quinquaginta.

Solve ſaccharum ut fiat ſyrupus.

Itidem confice ſyrupum ſucci fructus mori, rubi idæi, et ribis nigri.

SYRUPUS PAPAVERIS ALBI.

℞ Capitum papaveris albi exſiccatorum, ſeminibus exemtis, P. libras tres cum ſemiſſe,
Sacchari purificati P. libras ſex,
Aquæ diſtillatæ congios octo.

Capita concide et contunde; dein in aqua decoque ad congios tres, balneo aquoſo ſale muriatico ſaturato; et liquorem exprime. Hunc coquendo redige ad quatuor circiter librarum menſuram, et adhuc ferventem cola, primum per cribrum, deinde per pannum laneum tenuem; et ſepone per horas duodecim ut fæces ſubſidant. Liquorem fæcibus effuſum decoque ad libras tres, et ſolve ſaccharum ut fiat ſyrupus.

SYRUPUS PAPAVERIS ERRATICI.

℞ Florum papaveris erratici recentium P. libras quatuor,
Aquæ diſtillatæ ferventis M. libras quatuor cum ſemiſſe.

Aquæ ferventi in balneo aquoſo flores gradatim adjice crebro agitans. Dein, vaſe e balneo exemto, macera per horas duodecim; tum exprime liquorem, et ſepone ut fæces ſubſidant. Denique cum ſaccharo purificato fiat ſyrupus.

SYRUPUS ROSÆ.

℞ Petalorum roſæ damaſcenæ exſiccatorum P. uncias ſeptem,
Sacchari purificati P. libras ſex,
Aquæ diſtillatæ ferventis M. libras quatuor.

Macera roſæ petala in aqua per horas duodecim, et cola. Liquorem colatum per exhalationem abſume ad libras duas cum ſemiſſe; et adde ſaccharum ut fiat ſyrupus.

SYRUPUS SPINÆ CERVINÆ.

℞ Succi baccarum ſpinæ cervinæ maturarum et recentium congium unum,
Zingiberis contuſi P. unciam unam,
Pimento in pulverem triti P. unciam unam cum ſemiſſe,
Sacchari purificati P. libras ſeptem.

Sepone

Sepone ſuccum per aliquot dies ut fæces ſubſidant, et cola. In ſucci colati libra una macera zingiber et pimento per horas quatuor et cola. Succi quod reliquum eſt ad menſuram trium librarum decoque; dein adjice ſucci partem in qua zingiber et pimento macerata fuerint; denique adde ſaccharum ut fiat ſyrupus.

SYRUPUS TOLUTANUS.

℞ Balſami tolutani P. uncias octo,
Aquæ diſtillatæ M. libras tres.

Coque per duas horas. Liquori, poſtquam refrixerit, colato admiſce ſaccharum purificatum ut fiat ſyrupus.

SYRUPUS VIOLÆ.

℞ Petalorum violæ recentium P. libras duas,
Aquæ diſtillatæ ferventis M. libras quinque.

Macera per horas quatuor et viginti; deinde cola liquorem per linteum tenue ſine expreſſione. Adde ſaccharum purificatum ut fiat ſyrupus.

SYRUPUS ZINGIBERIS.

℞ Zingiberis contusi P. uncias quatuor,
Aquæ distillatæ ferventis M. libras tres.

Macera per horas quatuor, et cola; deinde saccharum purificatum adde ut fiat syrupus.

MELLA MEDICATA.

MEL ROSÆ.

℞ Petalorum rosæ rubræ nondum explicitorum, demtis unguibus, prius exsiccatorum, P. uncias quatuor,
Aquæ distillatæ ferventis M. libras tres,
Mellis despumati P. libras quinque.

Macera petala rosæ in aqua per sex horas; deinde mel liquori colato admisce, et misturam decoque ad syrupi crassitudinem.

MEL SCILLÆ.

℞ Mellis despumati P. libras tres,
Tincturæ scillæ M. libras duas.

Decoque in vase vitreo ad syrupi crassitudinem.

OXYMEL ÆRUGINIS.

℞ Æruginis præparatæ P. unciam unam,
Aceti M. uncias ſeptem,
Mellis deſpumati P. uncias quatuordecim.

Solve æruginem aceto, et cola per linteum; dein adde mel, et decoque ad idoneam craſſitudinem.

OXYMEL COLCHICI.

℞ Radicis colchici recentis in laminas tenues ſectæ P. unciam unam,
Aceti diſtillati M. libram unam,
Mellis deſpumati P. libras duas.

Macera radicem colchici cum aceto in vaſe vitreo leni calore per horas octo et quadraginta. Liquorem radice fortiter expreſſum cola, et adde mel. Denique miſturam, cochleari ligneo ſæpe agitans, decoque ad ſyrupi craſſitudinem.

OXYMEL

OXYMEL SCILLÆ.

℞ Mellis deſpumati P. libras tres,
Aceti ſcillæ M. libras duas.

Decoque in vaſe vitreo lento igne ad ſyrupi craſſitudinem.

OXYMEL SIMPLEX.

℞ Mellis deſpumati P. libras duas,
Aceti diſtillati M. libram unam.

Decoque in vaſe vitreo lento igne ad ſyrupi craſſitudinem.

P U L V E R E S.

PULVIS ALOËTICUS.

℞ Aloës ſocotorinæ P. libram unam,
Canellæ albæ P. uncias tres.

Separatim in pulverem tere, dein miſce.

PULVIS ALOËTICUS cum GUAIACO.

℞ Aloës ſocotorinæ P. unciam unam cum ſemiſſe,
Guaiaci gummi P. unciam unam,
Pulveris aromatici P. unciam dimidiam.

Aloën et guaiaci gummi ſeparatim in pulverem tere; dein omnia miſce.

PULVIS ALOËTICUS cum FERRO.

℞ Aloës ſocotorinæ in pulverem tritæ P. unciam unam cum ſemiſſe,
Myrrhæ in pulverem tritæ P. uncias duas,
Extracti gentianæ exſiccati,
Ferri vitriolati, ſingulorum in pulverem tritorum P. unciam unam.

Miſce.

PULVIS AROMATICUS.

℞ Corticis cinnamomi P. uncias duas,
Seminum cardamomi minoris, demtis capſulis,
Zingiberis,
Piperis longi, ſingulorum P. unciam unam.

Simul in pulverem tere.

PULVIS ASARI COMPOSITUS.

℞ Foliorum exſiccatorum aſari,
majoranæ,
mari ſyriaci,
Florum exſiccatorum lavendulæ, ſingulorum P. unciam unam.

Simul in pulverem tere.

PULVIS e CERUSSA.

℞ Ceruſſæ P. uncias quinque,
Sarcocollæ P. unciam unam cum ſemiſſe,
Tragacanthæ P. unciam dimidiam.

Simul in pulverem tere.

PULVIS e CHELIS CANCRORUM COMPOSITUS.

℞ Chelarum cancrorum præparatarum P. libram unam,
Cretæ præparatæ,
Corallii rubri præparati, ſingulorum P. uncias tres.

Miſce.

PULVIS CONTRAYERVÆ COMPOSITUS.

℞ Contrayervæ in pulverem tritæ P. uncias quinque,
Pulveris e chelis cancrorum compoſiti P. libram unam cum ſemiſſe.

Miſce.

PULVIS e CRETA COMPOSITUS.

℞ Cretæ præparatæ P. libram dimidiam,
Corticis cinnamomi P. uncias quatuor,
Tormentillæ,
Arabici gummi, ſingulorum P. uncias tres,
Piperis longi P. unciam dimidiam.

Separatim in pulverem tere, et miſce.

PULVIS e CRETA COMPOSITUS cum OPIO.

℞ Pulveris e creta compoſiti P. uncias octo,
Opii purificati duri in pulverem triti P. drachmam unam cum ſemiſſe.

Miſce.

PULVIS IPECACUANHÆ COMPOSITUS.

℞ Ipecacuanhæ,
Opii purificati duri, ſingulorum in pulverem tritorum P. drachmam unam,
Kali vitriolati in pulverem triti P. unciam unam.

Miſce.

PULVIS e MYRRHA COMPOSITUS.

℞ Myrrhæ,
Sabinæ exſiccatæ,
Rutæ exſiccatæ,
Caſtorei ruſſici, ſingulorum P. unciam unam.

Simul in pulverem tere.

PULVIS OPIATUS.

℞ Opii purificati duri in pulverem triti P. drachmam unam,
Cornu cervi uſti et præparati P. drachmas novem.

Miſce.

PULVIS e SCAMMONIO COMPOSITUS.

℞ Scammonii,
Extracti jalapii duri, ſingulorum P. uncias duas,
Zingiberis P. unciam dimidiam.

Separatim in pulverem tere, et miſce.

PULVIS

PULVIS e SCAMMONIO cum ALOË.

℞ Scammonii, P. drachmas ſex,
Extracti jalapii duri,
Aloës ſocotorinæ, ſingulorum P. unciam unam cum ſemiſſe,
Zingiberis P. unciam dimidiam.

Separatim in pulverem tere, et miſce.

PULVIS e SCAMMONIO cum CALOMELANE.

℞ Scammonii P. unciam dimidiam,
Calomelanos,
Sacchari purificati, ſingulorum P. drachmas duas.

Separatim in pulverem tere, et miſce.

PULVIS e SENNA COMPOSITUS.

℞ Sennæ,
Cryſtallorum tartari, ſingulorum P. uncias duas,
Scammonii P. unciam dimidiam,
Zingiberis P. drachmas duas.

Scammonium ſeparatim, cætera ſimul in pulverem tere, et omnia miſce.

PULVIS e TRAGACANTHA COMPOSITUS.

℞ Tragacanthæ in pulverem tritæ,
Arabici gummi,
Amyli, ſingulorum P. unciam unam cum ſemiſſe,
Sacchari purificati P. uncias tres.

Simul in pulverem tere.

TROCHISCI.

TROCHISCI AMYLI.

℞ Amyli P. unciam unam cum ſemiſſe,
Glycyrrhizæ P. drachmas ſex,
Iridis P. unciam dimidiam,
Sacchari purificati P. libram unam cum ſemiſſe.

Hæc in pulverem tere, et ope tragacanthæ in aqua ſolutæ fiant trochiſci.

Fiant etiam, ſi cui lubet, ſine iride.

TROCHISCI GLYCYRRHIZÆ.

℞ Extracti glycyrrhizæ,
Sacchari purificati, ſingulorum P. uncias decem,
Tragacanthæ in pulverem tritæ P. uncias tres.

Adjecta aqua fiant trochiſci.

TROCHISCI e NITRO.

℞ Nitri purificati in pulverem triti P. uncias quatuor,
Sacchari purificati in pulverem triti P. libram unam.
Tragacanthæ in pulverem tritæ P. uncias ſex.

Adjecta aqua fiant trochiſci.

TROCHISCI e SULPHURE.

℞ Florum ſulphuris lotorum P. uncias duas,
Sacchari purificati P. uncias quatuor.

Tere ſimul, et, mucilagine ſeminis cydonii mali ſubinde adjecta, fiant trochiſci.

TROCHISCI e CRETA.

℞ Cretæ præparatæ P. uncias quatuor,
Chelarum cancrorum præparatarum P. uncias duas,
Corticis cinnamomi P. unciam dimidiam,
Sacchari purificati P. uncias tres.

His in pulverem tritis mucilaginem arabici gummi adde et fiant trochiſci.

TROCHISCI e MAGNESIA.

℞ Magneſiæ ustæ ~~calcinatæ~~ P. uncias quatuor,
Sacchari purificati P. uncias duas,
Zingiberis in pulverem triti P. ſcrupulum unum.

Mucilagine arabici gummi addita fiant trochiſci.

P I L U L Æ.

P I L U L Æ ex A L O Ë.

℞ Aloës ſocotorinæ in pulverem tritæ P. unciam unam,
Extracti gentianæ P. unciam dimidiam,
Syrupi zingiberis quantum ſatis ſit.

Simul contunde.

PILULÆ ex ALOË cum MYRRHA.

℞ Aloës ſocotorinæ P. uncias duas,
Myrrhæ,
Croci, ſingulorum P. unciam unam,
Syrupi croci quantum ſatis ſit.

Aloën et myrrham ſeparatim in pulverem tere; deinde omnia ſimul contunde.

P I L U L Æ e G U M M I.

℞ Galbani,
Opopanacis,
Myrrhæ,
Sagapeni, ſingulorum P. unciam unam,
Aſæ fœtidæ P. unciam dimidiam,
Syrupi croci quantum ſatis ſit.

Simul contunde.

P I L U L Æ ex H Y D R A R G Y R O.

℞ Hydrargyri purificati,
Extracti glycyrrhizæ, mellis craſſitudinem habentis, ſingulorum P. drachmas duas,
Glycyrrhizæ in tenuem pulverem tritæ P. drachmam unam.

Hydrargyrum cum extracto glycyrrhizæ tere donec globuli viſum fugerint; deinde, addito glycyrrhizæ pulvere, omnia ſimul miſce.

P I L U L Æ ex O P I O.

℞ Opii purificati duri P. drachmas duas,
Extracti glycyrrhizæ P. unciam unam.

Contunde donec in unum miſceantur.

PILULÆ e SCILLA.

℞ Scillæ recens exſiccatæ in pulverem tritæ P. drachmam unam,
Zingiberis in pulverem triti,
Saponis, ſingulorum P. drachmas tres,
Ammoniaci P. drachmas duas,
Syrupi zingiberis quantum ſatis ſit.

Simul contunde.

ELECTUARIA.

ELECTUARIUM e CASSIA.

℞ Pulpæ caſſiæ recens extractæ P. libram dimidiam,
Mannæ P. uncias duas,
Pulpæ tamarindorum P. unciam unam,
Syrupi roſæ P. libram dimidiam.

Mannam contunde, et lento igne ſolve in ſyrupo roſæ; deinde adde pulpas; et, continuato calore, per vaporationem perfice electuarium idoneæ craſſitudinis.

ELECTUARIUM e SCAMMONIO.

℞ Scammonii in pulverem triti P. unciam unam cum ſemiſſe,
Caryophyllorum aromaticorum,
Zingiberis, ſingulorum P. drachmas ſex,
Olei carui eſſentialis P. drachmam dimidiam,
Syrupi roſæ quantum ſatis ſit.

Aromata ſimul trita ſyrupo admiſce; deinde ſcammonium adde, et poſtremo oleum carui.

ELECTUARIUM

ELECTUARIUM e SENNA.

℞ Sennæ p. uncias octo,
Caricarum p. libram unam,
Pulpæ tamarindorum,
caſſiæ,
prunorum gallicorum, ſingulorum p. libram dimidiam,
Seminum coriandri p. uncias quatuor,
Glycyrrhizæ p. uncias tres,
Sacchari purificati p. libras duas cum ſemiſſe.

Sennam cum ſeminibus coriandri tere, et per cribrum ſepara pulveris miſti uncias decem. Reſiduum cum caricis et glycyrrhiza in quatuor librarum aquæ diſtillatæ menſura decoque ad dimidium; deinde exprime, et cola. Liquorem colatum per vaporationem abſume ad pondus libræ circiter unius cum ſemiſſe. Poſtea adde ſaccharum ut fiat ſyrupus. Hunc ſyrupum adde gradatim pulpis; et poſtremo immiſce pulverem.

CONFECTIONES.

CONFECTIONES.

CONFECTIO AROMATICA.

℞ Zedoariæ in pulverem craſſum tritæ,
Croci, ſingulorum P. ~~unciam~~ dimidiam, *Libram*
Aquæ diſtillatæ M. libras tres.

Macera per horas quatuor et viginti; dein exprime et cola. Liquorem colatum per vaporationem abſume ad libram unam cum ſemiſſe, cui adde ea quæ ſequuntur in pulverem ſubtiliſſimum trita —

Pulveris e chelis cancrorum compoſiti P. uncias ſedecim,
Corticis cinnamomi,
Nucis moſchatæ, ſingulorum P. uncias duas,
Caryophyllorum aromaticorum P. unciam unam,
Seminum cardamomi minoris, demtis capſulis, P. unciam dimidiam,
Sacchari purificati P. libras duas.

Fiat confectio.

CONFECTIO

CONFECTIO OPIATA.

℞ Opii purificati duri in pulverem triti P. drachmas ſex,

Piperis longi,

Zingiberis,

Seminum carui, ſingulorum P. uncias duas,

Syrupi papaveris albi, ad mellis craſſitudinem decocti, triplum omnium pondus.

Opium purificatum ſyrupo calefacto diligenter immiſce; tum adde cætera in pulverem trita.

AQUÆ MEDICATÆ.

AQUA ALUMINIS COMPOSITA.

℞ Aluminis,
Zinci vitriolati, ſingulorum P. unciam dimidiam,
Aquæ diſtillatæ ferventis M. libras duas.

Aquam ſalibus affunde in vaſe vitreo, et cola.

AQUA CUPRI AMMONIATI.

℞ Aquæ calcis M. libram unam,
Salis ammoniaci P. drachmam unam.

Stent ſimul in vaſe cupreo donec ammonia ſaturetur.

AQUA LITHARGYRI ACETATI COMPOSITA.

℞ Aquæ lithargyri acetati P. drachmas duas,
Aquæ diſtillatæ M. libras duas,
Spiritus vinoſi tenuioris M. drachmas duas.

Miſce ſpiritum vinoſum cum aqua lithargyri acetati; dein adde aquam diſtillatam.

AQUA ZINCI VITRIOLATI cum CAMPHORA.

℞ Zinci vitriolati P. unciam dimidiam,
Spiritus camphorati M. unciam dimidiam,
Aquæ ferventis M. libras duas.

Miſce, et per chartam cola.

EMPLASTRA.

EMPLASTRUM AMMONIACI cum HYDRARGYRO.

℞ Ammoniaci colati P. libram unam,
Hydrargyri purificati P. uncias tres,
Olei ſulphurati P. drachmam unam, vel quod ſatis ſit.

Hydrargyrum cum oleo ſulphurato tere donec globuli viſum fugerint: deinde adde paulatim ammoniacum liquefactum, et miſce.

EMPLASTRUM CANTHARIDIS.

℞ Cantharidum P. libram unam,
Emplaſtri ceræ P. libras duas,
Adipis ſuillæ præparatæ P. libram dimidiam.

Emplaſtro et adipi liquatis, paulo antequam concreſcant inſperge et immiſce cantharides in pulverem ſubtiliſſimum tritas.

EMPLASTRUM CERÆ.

℞ Ceræ flavæ,
Sevi ovilli præparati, ſingulorum P. libras tres,
Reſinæ flavæ, P. libram unam.

Liqua ſimul, et miſturam adhuc fluidam cola.

EMPLASTRUM CUMINI.

℞ Seminum cumini,
carui,
Baccarum lauri, ſingulorum P. uncias tres,
Picis burgundicæ P. libras tres,
Ceræ flavæ P. uncias tres.

Pici cum cera liquefactæ cætera in pulverem trita immiſce, et fiat emplaſtrum.

EMPLASTRUM LADANI.

℞ Ladani P. uncias tres,
Thuris P. unciam unam,
Corticis cinnamomi in pulverem triti,
Olei expreſſi, olei macis dicti, ſingulorum P. unciam dimidiam,
Olei menthæ eſſentialis P. drachmam unam.

Thuri liquefacto adde primum ladanum igne emollitum; deinde oleum macis. Hæc poſtea, cum cinnamomo, oleo menthæ immiſce, et in mortario tepido ſimul contunde in emplaſtrum. In vaſe operto ſervetur.

EMPLASTRUM LITHARGYRI.

℞ Lithargyri in pulverem ſubtiliſſimum triti P. libras quinque,
Olei olivæ congium unum.

Coque lento igne cum aquæ menſura librarum circiter duarum, perpetuo agitans, donec oleum et lithargyrus coëant, et emplaſtri craſſitudinem habeant. Oportebit autem plus aquæ ferventis addere, ſi ea fere omnis, quæ in principio adhibita fuerit, ante finem coctionis abſumta fuerit.

EMPLASTRUM

EMPLASTRUM LITHARGYRI cum GUMMI.

℞ Emplaſtri lithargyri P. libras tres,
Galbani colati P. uncias octo,
Terebinthinæ P. drachmas decem,
Thuris P. uncias tres.

Galbano, lento igne cum terebinthina liquato, immiſce pulverem thuris, deinde emplaſtrum lithargyri lentiſſimo igne liquatum, et fiat emplaſtrum.

EMPLASTRUM LITHARGYRI cum HYDRARGYRO.

℞ Emplaſtri lithargyri P. libram unam,
Hydrargyri purificati P. uncias tres,
Olei ſulphurati P. drachmam unam, vel quod ſatis ſit.

Hoc emplaſtrum confice eodem modo quo emplaſtrum ammoniaci cum hydrargyro.

EMPLASTRUM LITHARGYRI cum RESINA.

℞ Emplaſtri lithargyri p. libras tres,
Reſinæ flavæ p. libram dimidiam.

Emplaſtro lithargyri, lentiſſimo igne liquato reſinam in pulverem tritam immiſce, et fiat emplaſtrum.

EMPLASTRUM PICIS BURGUNDICÆ.

℞ Picis burgundicæ p. libras duas,
Ladani p. libram unam,
Reſinæ flavæ,
Ceræ flavæ, ſingulorum p. uncias quatuor,
Olei expreſſi, olei macis dicti, p. unciam unam.

Pici, reſinæ, et ceræ, ſimul liquatis, adde primum ladanum, deinde oleum macis.

EMPLASTRUM SAPONIS.

℞ Saponis p. libram dimidiam,
Emplaſtri lithargyri p. libras tres.

Emplaſtro lithargyri liquefacto ſaponem immiſce; et ad emplaſtri craſſitudinem decoque.

EMPLASTRUM

EMPLASTRUM THURIS.

℞ Thuris p. libram dimidiam,
Sanguinis draconis p. uncias tres,
Emplaſtri lithargyri p. libras duas.

Emplaſtro lithargyri liquefacto adde cætera in pulverem trita.

U N G U E N T A,

et

L I N I M E N T A.

UNGUENTUM ADIPIS SUILLÆ.

℞ Adipis ſuillæ præparatæ P. libras duas,
Aquæ roſæ M. uncias tres.

Adipem cum aqua roſæ contunde donec miſceantur; deinde lento igne liqua et ſepone ut aqua ſubſidat. Poſtea effunde adipem, aqua relicta, aſſidue agitans donec frigeat.

UNGUENTUM CALCIS HYDRARGYRI ALBÆ.

℞ Calcis hydrargyri albæ P. drachmam unam,
Unguenti adipis ſuillæ P. unciam unam cum ſemiſſe.

Miſce, et fiat unguentum.

UNGUENTUM CANTHARIDIS.

℞ Cantharidum in pulverem tritarum P. uncias duas,
Aquæ diſtillatæ M. uncias octo,
Unguenti reſinæ flavæ P. uncias octo.

Aquam cum cantharidibus decoque ad dimidium, et cola. Colato liquori adde unguentum reſinæ flavæ. Hæc miſtura balneo aquoſo, ſale muriatico ſaturato, ad unguenti craſſitudinem vaporet.

UNGUENTUM CERÆ.

℞ Ceræ albæ P. uncias quatuor,
Spermatis ceti P. uncias tres,
Olei olivæ M. libram unam.

Lento igne liquefacta aſſidue acriterque agita, donec refrixerint.

UNGUENTUM CERUSSÆ ACETATÆ.

℞ Cerussæ acetatæ P. drachmas duas,
Ceræ albæ P. uncias duas,
Olei olivæ M. libram dimidiam.

Cerussam acetatam in pulverem tritam cum aliqua olei parte contere; deinde eam ceræ cum oleo reliquo liquefactæ adde. Misturam agita donec refrixerit.

UNGUENTUM ELEMI.

℞ Elemi P. libram unam,
Terebinthinæ P. uncias decem,
Sevi ovilli præparati P. libras duas,
Olei olivæ M. uncias duas.

Elemi cum sevo liqua, et ab igne remotum statim misce cum terebinthina et oleo; deinde misturam cola.

UNGUENTUM HELLEBORI ALBI.

℞ Radicis hellebori albi in pulverem tritæ P. unciam unam,
Unguenti adipis ſuillæ P. uncias quatuor,
Eſſentiæ limonum P. ſcrupulum dimidium.

Miſce, et fiat unguentum.

UNGUENTUM HYDRARGYRI FORTIUS.

℞ Hydrargyri purificati P. libras duas,
Adipis ſuillæ præparatæ P. uncias tres et viginti,
Sevi ovilli præparati P. unciam unam.

Tere primum hydrargyrum cum ſevo, et tantillo adipis ſuillæ, donec globuli viſum fugerint; dein adde quod reliquum eſt adipis, et fiat unguentum.

UNGUENTUM HYDRARGYRI MITIUS.

℞ Unguenti hydrargyri fortioris partem unam,
Adipis ſuillæ præparatæ partes duas.

Miſce.

UNGUENTUM HYDRARGYRI NITRATI.

℞ Hydrargyri purificati P. unciam unam,
Acidi nitrosi P. uncias duas,
Adipis suillæ præparatæ P. libram unam.

Solve hydrargyrum in acido nitroso, et, dum adhuc calet, misce cum adipe suilla liquefacta et jam frigescente.

UNGUENTUM PICIS.

℞ Picis liquidæ,
Sevi ovilli præparati, singulorum P. libram dimidiam.

Liqua simul, et cola.

UNGUENTUM RESINÆ FLAVÆ.

℞ Resinæ flavæ,
Ceræ flavæ, singularum P. libram unam,
Olei olivæ M. libram unam.

Lento igne liqua resinam, et ceram: dein adde oleum et misturam adhuc calentem cola.

UNGUENTUM

UNGUENTUM SAMBUCI.

℞ Florum ſambuci P. libras quatuor,
Sevi ovilli præparati P. libras tres,
Olei olivæ M. libram unam.

Sevo cum oleo liquefacto incoque flores donec fere criſpentur; deinde exprime et cola.

UNGUENTUM SPERMATIS CETI.

℞ Spermatis ceti P. drachmas ſex,
Ceræ albæ P. drachmas duas,
Olei olivæ M. uncias tres.

Lento igne ſimul liquefacta aſſidue et acriter agita, donec refrixerint.

UNGUENTUM SULPHURIS.

℞ Unguenti adipis ſuillæ P. libram dimidiam,
Florum ſulphuris P. uncias quatuor.

Miſce, et fiat unguentum.

UNGUENTUM

UNGUENTUM TUTIÆ.

℞ Tutiæ præparatæ,
Linimenti ceræ albæ quod ſatis ſit.

Miſce ut fiat unguentum molle.

LINIMENTUM AMMONIÆ.

℞ Aquæ ammoniæ M. unciam dimidiam,
Olei olivæ M. unciam unam cum ſemiſſe.

Agita ſimul in phiala donec miſceantur.

LINIMENTUM AMMONIÆ FORTIUS.

℞ Aquæ ammoniæ puræ M. unciam unam,
Olivæ olei M. uncias duas.

Agita ſimul in phiala.

LINIMENTUM CAMPHORÆ.

℞ Camphoræ P. uncias duas,
Aquæ ammoniæ M. uncias ſex,
Spiritus lavendulæ ſimplicis P. uncias ſedecim.

Aquam ammoniæ cum ſpiritu miſce, et ex retorta vitrea diſtilla lento igne uncias ſedecim. Tum liquore diſtillato ſolve camphoram.

LINIMENTUM SAPONIS.

℞ Saponis P. uncias tres,
Camphoræ P. unciam unam,
Spiritus roriſmarini M. libram unam.

Digere ſaponem in ſpiritu roriſmarini donec ſolvatur, et adde camphoram.

CERATA.

CERATUM CANTHARIDIS.

℞ Cerati ſpermatis ceti igne emolliti P. drachmas ſex,
Cantharidum in pulverem tenuem tritarum P. drachmam unam.

Miſce.

CERATUM LAPIDIS CALAMINARIS.

℞ Lapidis calaminaris præparati,
Ceræ flavæ, ſingulorum P. libram dimidiam,
Olei olivæ M. libram unam.

Liqua ceram cum oleo, et, ſimul ac miſtura lenteſcere inceperit, immiſce lapidem calaminarem, et agita donec ceratum refrixerit.

CERATUM LITHARGYRI ACETATI.

℞ Aquæ lithargyri acetati M. uncias duas cum ſemiſſe,
Ceræ flavæ P. uncias quatuor,
Olei olivæ M. uncias novem,
Camphoræ P. drachmam dimidiam.

Camphoram cum tantillo olei tere. Liqua ceram cum reliquo oleo, et ſimul ac miſtura lenteſcere inceperit, affunde gradatim aquam lithargyri acetati, et aſſidue agita donec refrixerit; deinde immiſce camphoram cum oleo tritam.

CERATUM RESINÆ FLAVÆ.

℞ Unguenti reſinæ flavæ P. libram dimidiam,
Ceræ flavæ P. unciam unam.

Liqua ſimul, et fiat ceratum.

CERATUM SAPONIS.

℞ Saponis P. uncias octo,
Ceræ flavæ P. uncias decem,
Lithargyri in pulverem triti P. libram unam,
Olei olivæ M. libram unam,
Aceti congium unum.

Coque acetum cum lithargyro lento igne aſſidue agitans donec miſtura coëat et lenteſcat; deinde immiſce cætera, et fiat ceratum.

CERATUM SPERMATIS CETI.

℞ Spermatis ceti P. unciam dimidiam,
Ceræ albæ P. uncias duas,
Olei olivæ M. uncias quatuor.

Liqua ſimul, et agita donec ceratum refrixerit.

EPITHEMATA.

CATAPLASMA CUMINI.

℞ Seminum cumini P. libram unam,
Baccarum lauri,
Foliorum ſcordii exſiccatorum,
Radicis ſerpentariæ virginianæ, ſingulorum P. uncias tres,
Caryophyllorum aromaticorum P. unciam unam.

Omnia ſimul tere, et, addito triplo mellis pondere, fiat cataplaſma.

CATAPLASMA SINAPEOS.

℞ Seminum ſinapeos in pulverem tritorum,
Medullæ panis, ſingulorum P. libram dimidiam,
Aceti quantum ſatis ſit.

Miſce, et fiat cataplaſma.

COAGULUM ALUMINIS.

℞ Albi ovorum duorum.

Agita cum frusto aluminis donec abeat in coagulum.

FINIS.

TABULA

OSTENDENS, QUA RATIONE HYDRARGYRUS ET OPIUM IN MEDICAMENTIS COMPOSITIS CONTINENTUR.

PULVIS e creta compoſitus cum opio in granis circiter 43 continet opii granum unum.

Pulvis ipecacuanhæ compoſitus in granis decem continet opii granum unum.

Pulvis opiatus in granis decem continet opii granum unum.

Pulvis e ſcammonio cum calomelane in granis quatuor continet calomelanos granum unum.

Pilulæ ex opio in granis quinque continet opii granum unum.

Pilulæ ex hydrargyro in granis decem continet hydrargyri grana quatuor.

Confectio opiata in granis 36 continet opii granum unum.

Emplaſtrum ammoniaci cum hydrargyro in unciis quinque continet hydrargyri unciam unam.

Emplaſtrum

Emplaſtrum lithargyri cum hydrargyro in unciis quinque continet hydrargyri unciam unam.

Unguentum hydrargyri fortius in drachmis duabus continet hydrargyri drachmam unam.

Unguentum hydrargyri mitius in drachmis quinque continet hydrargyri drachmam unam.

Unguenti hydrargyri nitrati in drachma una continet hydrargyri nitrati grana duodecim.

Unguentum calcis hydrargyri albæ in drachma una continet calcis hydrargyri albæ grana quatuor cum ſemiſſe.

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