

**An essay on glandular secretion; containing an experimental enquiry into the formation of pus: and a critical examination into an opinion of Mr. John Hunter's, 'that the blood is alive!' / [James Hendy].**

**Contributors**

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A N  
E S S A Y  
O N  
GLANDULAR SECRETION;  
CONTAINING AN  
EXPERIMENTAL ENQUIRY  
INTO THE  
FORMATION OF P U S:  
AND A  
CRITICAL EXAMINATION  
Into an O P I N I O N of  
Mr. J O H N H U N T E R's,  
*"That the Blood is alive."*

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By J A M E S H E N D Y, M D.

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L O N D O N:

Printed for JOHN BELL, near Exeter-Exchange, Strand.

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EXPERIMENTAL ENQUIRY

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AND A

CRITICAL EXAMINATION

OF AN

MR JOHN HUNT

OF THE

BY JAMES H. D. D.

L O N D O N

Printed by J. G. Smith, at the Printing Office, No. 1, St. Paul's Churchyard, London.

1825

To the FACULTY of the  
MIDDLESEX HOSPITAL.

**P**ERMIT me, gentlemen, to dedicate this short treatise to your patronage. There is some propriety in my taking this liberty, as I wish this attempt to explain an intricate part of physiology, may be considered as the first fruits of a medical education, acquired principally, from the opportunities your kindness and friendship have given me.

I offer it as a small tribute for the innumerable obligations I lay under to you; and I hope you will accept it as a testimony of gratitude from,

Gentlemen,

your most respectful,

and very humble Servant,

London, January 1775.

JAMES HENDY.



## Advertifement.

THE Author humbly prefumes, that a preface to this publication is unnecessary. He takes this opportunity, however, to beg the candid correction of the public where he has erred ; and also to assure those who do him the honour of perusing this treatise, that, to meet their approbation is his chief ambition.

It is with great diffidence that he ventures to appear in public, and would by no means have done it, had he not been perswaded to it, by the advice of several medical gentlemen, on whose judgment he lays much stress.



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A N  
E S S A Y  
O N  
GLANDULAR SECRETION.

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*Definition of Glands.*

GLANDS are appendages to the sanguiferous and lymphatic systems, and have the power of inducing changes on the fluids that are brought to them, or separating particular parts from the general mass.

The manner, by which these effects are produced, will be treated of in this essay, not

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indeed



indeed from a presumption that I should excel in illustrating this subject, but because it appeared to be a part of physiology highly worthy of investigation.

In prosecuting this subject, I shall divide it into three sections: in the first, I shall treat of the general structure of glands, and the propriety of the preceding definition, subjoining a short view of the division of glands; I shall then proceed to speak more particularly of those glands I term appendages to the sanguiferous system; and next, of those that I suppose more properly to belong to the lymphatic system. In the second section, I shall give the most prevailing opinions, concerning the manner in which they produce alterations upon, or separations from, the general mass; and to each of these I shall point out the principal objections. In the third section, I shall offer what appears to me to be a *more* probable conjecture concerning the nature of secretion, and subjoin the reasons that tend to support this opinion.



## S E C T. I.

*Concerning the Structure of Glands,  
&c.*

## C H A P. I.

*Of the Structure of Glands in general.*

THERE is no part of animal mechanism that has employed the attention of anatomists more, than the structure of glands; and surely, from the importance of their functions to life and health, they justly merit a minute enquiry into their construction. For, unless this be known, it must be impossible to acquire any correct idea of the mode by which they produce such evident changes on our fluids. Indeed, every advance, without this foundation, must be vague and merely conjectu-



ral. I may remark, that this, as well as every endeavour of human industry to unfold the deep and hidden recesses of wise Nature, have always required the most unwearied diligence, united with the best capacity. But genius and application are separate gifts, and are seldom conjoined in the same individual; yet there are not wanting instances, where these have united in the same person, and where it has so happened, it has given birth to very important discoveries. This is particularly exemplified in several professors of the medical art. That the present investigation was arduous, and required both these human powers united, we may, I think, easily perceive from the history of this part of anatomy; and we may also learn, from the disputes of celebrated anatomists, that the minuteness and nicety of this subject rendered observers liable to many fallacies. For the history of medicine informs us, that at one period the medical world were ready to concur in opinion with the ingenious Malpighi, and admit his supposition that all glands were either vesicular  
or



or follicular. The arguments which this great man brought for his theory, and the support it received from Dr. Boerhaave, gave it great plausibility and weight ; in so much, that it is highly probable that we should have adopted and retained this opinion, had it not been for the celebrated Ruysch, who opposed Malpighi, and advanced an opinion, which though not totally new,\* successfully controverted his doctrine ; and this very accurate anatomist, by rendering the secrets of our organization evident and demonstrable, and by opposing obvious objections to the conclusions of Malpighi, and also by bringing convincing proofs in support of the opinion which he renewed, not only overturned the Malpighian doctrine, but at the same time established his own, upon the best and most infallible foundation, viz. clear and decisive experiments. For, by his very minute injections, he demonstrated

B 3

that

\* Edmundus King, Nehemiah Grew, and some others, who were prior to Ruysch, had the same ideas of Glands with those he advanced and supported.



that those appearances which were taken for follicles, vesicles, or *cryptæ*, were, in fact, a congeries of vessels. And, certainly, if this be the case, the arguments brought from the morbid state, must be fallacious. But suppose, for an instant, we were to admit the follicular structure of glands, and to allow proofs to be brought from the diseased state, we shall, even then, I am confident, find them very inadequate to the conclusion. Such arguments are to be found in Haller's *Elementa Physiologiæ*. In the first place, to invalidate this opinion, it is remarked, that these morbid vesicles are met with, where glands are not to be found, as in the cellular texture of different parts of the body. When they are found in glands, as indeed is very frequently the case, and if we suppose secretions are made in them, and obstructions formed to their fluids making their exit from them by the excretory duct, should we not expect to find the vesicles increased by the accumulation of the particular fluid which the gland secreted?

But



But this does not happen, as is evinced by hydatids of the liver, &c.

Should it be thought incumbent on the opposers of the Malpighian doctrine to prove from what sources these morbid vesicles proceed, since they deny that they spring from the distension of vesicles, which they also reject as unconnected with the structure of glands in their sound state; I can affirm, supported by the best authority, that they sometimes happen in *loculi* of cellular texture, and that they often proceed from the distension of the space between the valves of the lymphatic vessels. The only part where hydatids do occur, and where lymphatic vessels have not been demonstrated, is the brain; and it is rendered highly probable, that they are not wanting in this important organ, by arguments brought by Dr. Monro and Mr. Hewson.\* Tyson also advanced an opinion, that

B 4

they

\* Mr. Falconer, anatomist of London, informs me he has great reason to believe that he has injected lymphatic vessels in the brain of the cod-fish.



they, in some instances, proceeded from worms, which opinion some men of judgment have admitted.

Though the Malpighian doctrine was questioned by several authors, yet Ruyfch, as he compleated its overthrow, has deservedly acquired the principal honour. It is indeed to him that we are chiefly indebted; for if, by his art, he had not extricated us from those difficulties which accompany such a minute enquiry, we should have perhaps been still ignorant of this and many other parts of the *minutiæ* of anatomical structure. This art has been so considerably improved by modern anatomists, and more especially by the indefatigable Hewson, that I am enabled to speak with greater confidence on this subject.



## C H A P. II.

*Of the Variety of Glands, &c.*

**M**OST writers, that I have consulted, have defined glands, either from the external habit or the internal structure ; some from one, and some from the other: but it is evident, that a character taken from either of these, must be very imperfect. For if we only view the external figure of glands, the great diversity which we find among them would lead us to exclude a great number, which we very properly term glands. The liver and testicle are both glands, yet how different is their outward appearances? We shall find the same conclusion hold good, if we examine the internal construction ; for in this also there is a considerable diversity. Some essential parts are wanting to certain glands, as for instance, the spleen, &c. are without excretory ducts, at least what we  
call



call so, in the other glands. And indeed, where all the parts that enter the definition are present, *viz.* a congeries of minute arteries, veins, nerves, &c. they ramify in such a variety of ways, as scarcely to appear to be composed of the same parts; as may be observed by comparing the pancreas with the kidney or liver. In my opinion, this great variety is what we should naturally expect, when we consider that each organ is to have its own particular effect; for, if size and construction were the same, what else could be expected, but that they would all perform the same function? Besides, there are instances where a change is produced on the coagulable lymph, merely by an organic pore, or more probably by an exhalant artery, as I shall hereafter endeavour to prove.

Baron Haller remarks the difficulty of forming a definition from any of these circumstances, and includes their functions. After treating of these, he adds, "*Ex his omnibus colligitur, glandularum & organorum secretoriorum magnam varietatem esse, & pa-*  
*rum*"



*rum omnino videri, quod diversæ glandularum classes commune habent, cum neque fabrica convenient, neque facie, neque functione; ut omnino ægrè definitionem invenias.*"\* If a definition cannot be obtained from any of these, glands must remain undefined; but I humbly conceive, that an idea of a gland may be given by considering their general function. I have therefore advanced a new definition, not that I think it perfect, but because it appears the least liable to error, and to be the most unexceptionable; for it is certain that we call those organs glands, which secrete. We extend this appellation indeed somewhat farther, *viz.* to those which we suspect to perform some such office, as the spleen, &c. But until their use be published, we only call them so presumptively, from their having somewhat of a similar appearance

\* "From all these circumstances, we may gather, that there is a great variety of glands and secreting organs, and that the different classes of them have few circumstances, in which they agree in common, as they differ much in structure, appearance and function. Hence it is with the utmost difficulty, that one can pitch on a proper definition of glands."



appearance to those organs which we know to be glands. I have said they are appendages to the sanguiferous and lymphatic systems, concluding that the one system does not merely serve to carry blood to every part of the body, or the other merely to absorb from the several cavities, and return the whole of this fluid to the heart again, unacted upon ; but that each of these systems has a power inherent in itself to act on the fluids which it carries. That this is the case, with respect to the arterial system, is successfully proved by the ingenious Hewson, who observed different appearances in the coagulable lymph, when the arteries were acting differently. It has indeed been opposed to this conclusion, that very trifling circumstances will cause remarkable diversity in the appearance, from whence these conclusions were made. But it should be remarked, at the same time, that all external circumstances were the same ; that the arteries themselves only were found to be acting with different strength at different times. The late most worthy professor,



at Edinburgh, Dr. Gregory,\* in his course of practice, confirmed this, by asserting that he had seen different appearances in the blood, received in different vessels, at the same bleeding, where the flow of the blood and every other apparent circumstance was the same. Hewson, by careful observation and experiment, found that the change in these cases depends on the arterial system's state of action being changed. I conclude, therefore, with him, that the arterial system is capable of changing the properties of a particular part of the blood, viz. the coagulable lymph. From hence, therefore, I think I am authorized to advance, that certain glands are added to this system, to effect farther changes on the blood, or separations from it.

I might perhaps with propriety have subdivided this part of my subject by saying, that glands are appendages to the arterial and to the venous system ; but I chose rather to be general, than run the hazard of  
erring

\* Professor of the practice of medicine.



erring by endeavouring to be too minute. But yet, I think, this opinion might be supported to a certain degree, for the liver undoubtedly appears to be a venous viscus. Indeed, it has been advanced, that the *venous* blood, which serves this organ, requires to be previously acted upon by the spleen. It is true, we observe, that the splenic vein, making the *vena portarum*, is, in a considerable degree, robbed of its coagulable lymph. Haller speaks as follows, in his *Primæ lineæ*, § DCLXXXI, \* “*Is sanguis vix unquam coagulatus est.*” But that the spleen has an important office of its own, is a discovery lately made by that very excellent anatomist Mr. Hewson. † Perhaps

\* “That blood is scarce ever coagulated.”

† Some persons, who were by no means masters of Mr. Hewson's reasoning, have nevertheless ventured to criticise his opinion concerning the use of the spleen, &c. by which they not only shewed a want of judgment, in attempting to impugn a doctrine which they did not understand, but at the same time exposed their ardent though fruitless endeavours to clip the wings of a *rising genius*. He however could have no victorious opposer



haps the reason of the splenic vein's passing into the liver is merely because it is the *nearest and most convenient* way to return the blood to the heart. For, though the liver may require venous blood, yet it is improbable that it should require another organ solely to prepare the blood for it; as we observe that the male *semen*, which perhaps is the most elaborate, at least the most important secretion, is formed in a gland independent of any other. In as far indeed, as  
every

poser to his towering greatness; he could have no dangerous enemy to his future fame, but one, and that was death.

There have not been wanting persons who have affirmed, that the use Hewson attributed to the lymphatic system was no real discovery; and have placed it amongst the ridiculous opinions of the ancients. They have laid much stress on the number of back-doors that Mr. Hewson left, that he might escape the artillery of medical critics, and defend his hypothesis.

Thus, say they, if it be advanced against Mr. Hewson, that several animals have been deprived of their spleen, and still that these particles have been completely formed, he immediately flies to the thymus gland. If it be then remarked, that after a certain age this gland is obliterated, he will retire to the lymphatic glands, and assure us that they are formed there. And lastly, if it be opposed to his doctrine, that some animals have



every part is subservient to the other, and altogether making one compleat whole, every part is dependent on the other, but no farther; and in this view I may say, that the kidneys prepare blood to be properly acted on by every gland in the body, by freeing it of its superfluous water.—I shall speak of the use of the spleen when I arrive at the lymphatic system.

I might, from this division of the subject, be led, by a distant analogy, to proceed still farther,

no lymphatic glands, he then takes his last subterfuge and defends himself by retiring to the lymphatic vessels themselves.

To avoid this crafty opposition, for I cannot even term it specious reasoning, they ought to be informed, that it is the *lymphatic system* which forms the red part of the blood, and that the spleen, thymus, and lymphatic glands are considered as parts of, or appendages to, this system.

I cannot avoid remarking, that one gentleman who opposes Hewson's doctrine, by the reasoning I have just related, advances or rather supports an opinion which is overturned by the above arguments, without leaving himself a single back-door to creep out at. He says, it is highly probable that the spleen is subservient to the liver, and that it prepares the blood for that viscus. He must permit me to ask, how the blood is prepared for the liver, when the spleen is cut out?



farther, and I shall venture to do it, but with the greatest diffidence. It is merely a supposition, viz. that certain glands, which continue to be the *opprobria* of the anatomist and the physiologist, may perhaps be considered as belonging to the nervous system, and their uses intimately connected with that system. I mean the *glandula pinealis*, the *glandula pituitaria*, the ganglions of the nerves, and the renal *capsula*. That the *glandulae pinealis* and *pituitaria*, and the nervous ganglions are so, we have some reason to suppose, from their situation and close connection : that the renal *capsula* also makes a part of this system, I am led to suppose from the perusal of a paper in the *Acta Petropolitana*, written by DuVerney, in which he endeavours to shew, that there is a similarity between this part and the ganglions of the nerves ; but I must confess that the analogy does not strike me.

This idea, I think, is better supported by a number of dissections of a late accurate

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observer,



observer, who found, on inspection, that those monsters, which are born with a diminution or total want of brain, have their renal *capsula* wanting in the same proportion.

According to the view I have now taken of this subject, the different glands of the human body may be arranged and enumerated in the following order :

Appendages to the sanguiferous system.

1st, *to the arteries ;*

All the mucous glands, those which moisten or lubricate the several cavities, the salivary glands, *pancreas*, kidney, *mammæ mulierum*, testicles, and perhaps the thyroid glands ; indeed every gland, except those mentioned under the other heads.

2dly, *to the venal system ;*

The liver.

Appendages to the lymphatic system.

The lymphatic glands, the *thymus* and the spleen.

And



And it is perhaps probable, that the following are parts belonging to the nervous system :

The brain, the *glandulæ pinealis* and *pituitaria*, the ganglions of the nerves, and the renal *capsula*.



## C H A P. III.

*Further Considerations on those Glands which are connected with the Sanguiferous System.*

**I**T is probable that those glands, which only separate, or produce a small change, are the most simple in their construction. The kidney, however, seems an exception to this notion, for it appears to be as complex as any gland whatever. The only sure method of judging when mere separation happens, is to compare the fluid secreted, and observe if it is similar in properties to any of the parts, which exist in the blood-vessels. Where we find this is the case, we may justly conclude that it is a mere separation. The tears, the fluid that is found in the ventricles of the brain, the matter of perspiration and urine, are saline fluids, which I believe existed in the sanguiferous system. The matter, which is found in the several cavities of the body, I except from



from the number of separated fluids. The *synovia*, which lubricates the joints, &c. is different in its general appearance from any part of the blood; it approaches nearer to a matter of an oily nature, which does not exist in the blood vessels, unless it is carried thither, after being secreted and re-absorbed from the adipose membrane: at least, this is rendered highly probable, from the third chapter of the first part of Hewson's Experimental Enquiry. That the interstitial fluids are not mere separations, I am led to conclude, from the following observations. Though they coagulate, on being exposed to the air, yet they differ greatly in the time required. The coagulable lymph of the blood completely jellies in seven or eight minutes, when exposed to the air; but these fluids require thirty minutes for their coagulation. Besides, there is another difference; for though exposure to the air is proved to be the chief agent in coagulating the blood, yet long rest will produce the same effect. Now, I will not assert, that the interstitial fluids are totally at rest; yet they surely are by no means so violently agitated, as in  
the



the course of circulation. From hence it should follow, that the delay in the cavities should dispose them to coagulate more speedily when they are exposed to the air, than the lymph taken from the vessels while in their usual motion. But this is found not to happen. The interstitial fluid differs from the coagulable lymph, more remarkably, in the time necessary for its coagulation, when neither is exposed to the air, but in a state of rest, as is proved by Hewson's experiments. [*See his lymphatic system.*] For the coagulable lymph in the blood-vessels was found to be coagulated at the end of six hours; whereas, the lymph in the lymphatic vessels, which is the same with the interstitial fluid, was perfectly fluid after resting twenty hours; and being then let out, jellied after being some time exposed to the air.

I humbly presume these differences are sufficient to place this fluid among the altered secretions. I chuse the structure of this organ, which secretes the interstitial fluid, as an example of the most simple kind of gland,

And



And concerning the structure of this organ three opinions have been advanced; viz. that it is an inorganic pore, admitting of transudation; that it is an organised pore, which is capable of separating a particular part of the circulating fluid; and that it is an exhalant artery. The first of these opinions was advanced by Dr. Hunter, but is most completely overturned by Mr. Hewson in his Experimental Enquiry. It therefore only now remains to determine whether it is an organic pore or exhalant artery, that forms the interstitial secretion. I am led to believe, it is an exhalant artery: for a mere organic orifice does not seem adequate to effect those alterations, which have been observed to happen in an healthy state of the system; and it will appear less so, if we take into consideration the morbid changes, and particularly the effect of the vessels forming *pus*, of which I shall speak hereafter.

Those who have espoused the idea of its being a mere organic pore, have argued, from supposing the interstitial fluid to be a mere water,



water, saying that such a pore was adequate to such a function, and that the supposition of its being a vessel would account for nothing farther. But finding that, in fact, a certain alteration is produced on the fluids, this reasoning must be relinquished. I suppose this exhalant artery to be somewhat similar to those vessels which take their rise from the red arteries, that are found to surround the fatty substance contained in the articulations of bones, &c.—which vessels, it is very probable, secrete the *synovia* of the joints. I, the more readily, adopt this opinion of an exhalant artery, because it agrees with Dr. Haller's, who, in treating of the termination of arteries, speaks thus, § XLIII. “*Alius finis arteriæ est, quo in canalem exhalantem terminatur. Iste finis ubique in corpore frequens est.*”\*

Several instances of the more complex or compleat structure of glands, might be brought; but it will be sufficient for my purpose

\* “There is another termination of an artery, *viz.* in an exhalant canal. This sort of termination is every where common in the human body.”



to mention the liver, the kidneys, the testicles, &c. I do not think it necessary to relate the structure of these, because a much more perfect knowledge than I could give in this short treatise, may be acquired from many sources. I shall however observe, that the appearance of *cryptæ* found in the kidney, was much in favour of the Malpighian doctrine; but Ruyfch affirms, that this appearance is owing to the wonderful manner in which the vessels divide. Hewson also proved, by his injections, that this was the case; and demonstrated farther, that there is a serpentine vessel running between these *corpora globosa* and the excretory duct. Harderius, a cotemporary with Malpighi, in his *Exercitationes Anatomicae*, appears to have a very clear idea of the composition of the *corpora globosa* of the kidney. “*Existimo tamen glandulas illas e quibus exterior renum substantia conflata est, aliud nihil præ se ferre quam vasa variè gyrata et intorta, quæ post varios flexus et ambages tandem in papillas coeunt.*” \* I do not, indeed,

\* “I think however that those glands, of which the exterior substance of the kidneys is composed, have no other appearance than that of vessels differently contorted, which after various turnings and windings at length meet in *papillæ*.”



know a single exception to the vascular structure of glands. With respect to the termination of their excretory ducts, there is some diversity which it may not be improper to notice. They either end on the surface of some cavity, in a bladder or reservoir, or in both these ways. Of the first we have many instances, as in the *salivary* glands, the *pancreas*, &c.; of the second, we have examples in the kidneys, and perhaps also in the *lacunæ* of mucous glands. We find a termination in both these ways in the liver. I thought it necessary to mention these circumstances, because it has been an opinion, with some, that the secretions were in some instances carried on in the reservoirs: and it has been advanced that the *vesica fellea* formed bile. If these bladders were essentially necessary to secretions, no glands should be without them; but I have observed that glands open into cavities, and are without this part. There is surely no more reason to suppose the gall-bladder forms bile, than that the urinary-bladder separates urine, or that the *vesiculæ seminales* form *femen*, which we are certain is not the case. For, with respect to the last, if



if it was so, castrated males would still continue prolific, which is not even supposed. I must therefore relinquish such an opinion, and admit, with Dr. Haller, that these parts of certain glands are mere reservoirs, for storing up the secreted fluids, for the use or convenience of the œconomy ; and that the principal change which they undergo, is to have the more fluid parts or superfluous water, absorbed, which is necessary for their secretion.



## CHAP IV.

*Of the Glands connected with the lymphatic System.*

**T**HAT there is a propriety in calling these glands, of which I am now to speak, appendages to the lymphatic system, I am well persuaded; and though I cannot enter on a full detail of the proofs which may be brought in support of this opinion, as such an enquiry would far exceed the bounds of a pamphlet, and would be a dissertation that would require more time and recollection than I can bestow on it, I hope the particular experiments and judicious arguments which lead to this discovery will be published to the world among the posthumous works of their invaluable author. It will be sufficient to the present purpose, to mention a few leading facts, and the general conclusions. That this system takes its rise from the several cavities, and is  
by



by no means a continuation of the arteries, is ingeniously proved, by the celebrated anatomists Drs. Monro and Hunter, and Mr. Hewson. Yet, it is a fact, that the red particles of the blood are found in this system of vessels.† This has been noticed by several observers, and, I believe, is not doubted by any. A question naturally arises then, How should these particles come thither, if not formed in this system? It may perhaps be answered, that they are absorbed. But surely this cannot be the case; for, in the first place, let us consider and compare these particles with the scarcely visible lymphatic orifices, which appear on the *villi* of the intestines, where it is to be presumed they are as large as in any part of the body. We have no reason to suppose that capillary attraction should cause these vessels to take up particles at least as large as their orifices. Besides, before they could be absorbed, they should be poured out into the cavities of the body. But this is not the case; for they are not to be found mixed

† It is remarked by Dr. Haller, and brought as an argument to support the old idea, *viz.* that the lymphatic vessels are continued from the small arteries.



with the interstitial fluids, so far as they have been examined, although they might be imagined to happen, in consequence of the struggles of the animal while under the operation necessary for the experiment. But to shew that this is not in the least degree probable, I must add, that these particles appeared in the *thoracic duct* immediately, both above and below a ligature, that was instantly applied; which would not be the case, if the particles were absorbed. Is it not then with the highest probability, that I may answer the question, and say, that these particles are formed in this system, and in those parts which I term its appendages, *viz.* the lymphatic glands, the *thymus* and the spleen, because they are found to aid the lymphatic system in its office, and make the system compleat? I shall now proceed to speak of the structure of the lymphatic glands.

1. The lymphatic glands are found pretty constantly in certain parts of the human body. When their arteries and veins are injected with coloured liquor, these vessels are found to divide so very minutely, that they appear to be



be composed of nothing else. And the same appearance is seen, if we inject the lymphatic vessels with mercury. Hence I conclude that these two systems compose these glands principally; not that I mean to exclude, the nerves. Many anatomists have observed that they are replete with cellular texture, and are invested with a membranous tunic. The lymphatic vessels, which enter these glands, very frequently divide, as is observed by Nuck and others, and unite again into one or more vessels, at the part at which they make their exit. In some instances however, the gland is composed simply of a lymphatic vessel convoluted, as is proved by unravelling them, and after this convolution they pass on to the thoracic duct. These glands are observed to be larger in young animals than in old ones.

2. The *thymus* is similar in construction to the lymphatic glands, except this circumstance, that the lymphatic vessels do not enter and pass through, but take their rise from this gland. It is also larger in young animals,  
and



and gradually disappears as the animal advances in years, and is often obliterated in the adult state. I may remark also, that it is largest in some animals, not so much according to their present size, as in proportion to the speed with which they grow. Thus it is larger in proportion in a calf, than in the human *fœtus*. I need not say any thing of the situation of this or the other glands, as I know not whether this would afford any aid to our reasoning.

3. The spleen is remarked by most authors for the quantity of blood-vessels that pass into it, in proportion to its bulk. It is similar to the last mentioned gland in giving rise to numbers of lymphatic vessels. It is said also to be of a cellular texture.

Before I speak of the use of these parts, I must refer to the Philosophical Transactions, where proof will be met with, to establish an opinion, which Mr. Hewson advanced, *viz.* that the red particles of the blood are composed of two parts, a central or middle solid part,  
and



and a furrounding vesicle, or external covering. Mr. Hewson, in the ciii. lecture of his anatomical course, made it appear extremely probable, that the lymphatic vessels themselves were capable of forming both these parts ; but that, for the more compleatly performing this function, the lymphatic glands were found in the more perfect animals. That the vessels themselves are endowed with this power, is proved, by observing that some animals, that have no lymphatic glands, have this particle compleat.

In the infant state there seems to be a greater demand, in the animal œconomy, for these particles ; and, on this account, perhaps, young animals have an additional organ, which is obliterated as they advance in life : I mean the *thymus*. This gland supplies the central part, as appears by observation ; for a number of these particles are brought from this organ by the lymphatic vessels, which I said, derive their origin from thence.



Though, from experiment, the spleen appears to be an important organ to sanguification, yet several anatomists, and among these Mr. Hewson, have cut out this viscus, and the animal has continued to live; but whether with or without apparent diminution of this part of the blood, we have not had an opportunity of ascertaining: we must leave this to future experiment. It is probable, however, that as there are other organs answering the same office, that they will, in some degree, compensate for its loss. For, not only in this, but in other parts of the œconomy, we perceive that nature has more methods than one of producing the same effect. The function of this viscus seems to be, to add the flat vesicle to the central globule; for Mr. Hewson observes, that the lymphatic vessels, coming out from the spleen, are replete with these perfectly finished particles: and what more proof can we have of the function of any viscus? Do not we say that the liver forms bile, because we perceive bile come from it? It appears farther,



farther, that this vesicular sheath is formed from the coagulable lymph ; for we have observed above, that the blood contained in the splenic vein, scarcely coagulates ; and, that the coagulation depends on the presence or absence of this part of the blood, is too plain to be doubted.



## S E C T. II.

*Concerning the Physiology of Glands.*

## C H A P. I.

*The most remarkable Opinions concerning the Manner in which Glands produce their Effects on the general Mass : with Objections to these Theories.*

**I**N viewing the diversity of hypotheses, that have been framed, to explain the phænomena of secretion, I think it sufficient to select the most plausible ; and I shall first speak of the opinion which Des Cartes supported, for it deserves notice, both on account of its simplicity and ingenuity. This philosopher supposed, that the secretory organs were similar to sieves, and that the several pores of different glands  
were



were adapted to the magnitude and figure of the particles which the organ was to separate from the blood. However plausible this theory may appear, at first sight, the least attention is sufficient to refute it.

For, in the first place, it supposes that there are particles, of different size and shape, existing in the blood-vessels; but of this there is not the least proof: for, on viewing the blood with a microscope, we only observe the red particles, and some of the central parts without their vesicular coat. These red particles are all equal in shape and size, in the same animal, at the same period of life; but they differ in the same animal, at different periods of life, both in figure and magnitude, as may be noticed in the Philosophical Transactions, before cited. Therefore, if these particles were separated from the blood, they would require the secreting vessels to be larger, and to have different forms at different times, which is not probable. The fact is,



we do not observe these particles universally in the secreted fluids.

Secondly, it presumes that all the secreted liquors should previously exist in the blood-vessels. If so, we should find *bile*, *semen*, &c. circulating with the blood, which we do not. Besides, if it depended on mere filtration, every smaller particle, and every more subtile fluid, would accompany the larger particles, or more viscid fluids unacted upon: but, I believe, this is not found to be the case. Milk consists of certain solid globules; but can we expect to find all the fluids with it unchanged? No; we find the caseous matter of milk, which is supposed to be the most analogous to the coagulable lymph of the blood, to differ widely from it in certain properties.

Lastly, according to this doctrine, the secreting vessels of glands ought to be shaped differently; but the contrary of this is observed: for our vessels, in a transverse section,



## GLANDULAR SECRETION. 39

fection, are found to be universally circular, excepting only the sinuses of the brain.

Another theory, equally simple with the former, supposes, that the particular fluids which the several glands secrete, existed there *ab origine* ; and that, as the blood passes through the blood-vessels of the glands, each fluid attracts the parts which are similar to itself, and repels the rest. This hypothesis, like the last, supposes that the secreted fluids were in the sanguiferous system *in propria forma*, which is disputed, though Dr. Haller seems to think that they are. His words are these : “ *Diximus in sanguine omnes humorum classes reperiri, quas diversa per cola de sanguine oportet separari.*” \* That the elementary parts of all secreted liquors are contained in the blood, is abundantly probable ; but that they are acted upon and changed very

\* “ We have said, that all the classes of fluids are found in the blood, which must be separated from it by different strainers.”



considerably, is proved from the dissimilarity of some of the secreted fluids from any part of the blood. But farther, this supposition is contradicted by fact; for, in an embrio, as Dr. Haller himself remarks, *lib. VII. sect. III. § xxxi.* that the bile differs from what it is afterwards, both in colour and in taste.

The third, and last opinion, I shall mention, is that which supposes glandular secretion to be performed by fermentation. In treating of this, I shall, first, endeavour to ascertain what is meant by fermentation, according to the best definition of this operation.

Chemists do not seem to be agreed in their ideas of fermentation. It is, however, certainly necessary for me, in treating this subject, to discover what is meant by this operation; and, it appears to me, that the best and easiest method of attaining this knowledge, is to examine the definitions generally given. I will first speak  
of



of that which is given by a very excellent chemist, and which Dr. Pearson has adopted in his Inaugural Dissertation. “ *Si qua in materie motus intestinus suboriatur, et per totam massam serpat, donec tota homogenea fiat, et ita mutetur ut quælibet pars ejus recenti materie, ejusdem generis ac illa ab initio fuerat, addita, motum intestinum in hac concitat, et universam sibi omni ex parte similem reddat; hi motus, et hæc sub iis facta mutatio, fermentatio, et materies addita fermentationem ciens, fermentum dicuntur.*”\*

In the first place, I must examine, if there is any *motus intestinus*; and, in order to this, I must find out, on what this phænomenon in fermentation depends. It is

\* “ If an intestine motion arise in any matter, and diffuse itself through the whole mass, until the whole become homogeneous, and be so changed that any part of it applied to recent matter of the same sort, of which it had originally been, excite an intestine motion in it, and render the whole perfectly similar to itself; these motions, and the change produced by their agency, are called *fermentation*, and the matter exciting the fermentation, a *ferment*.”

proved,



proved, by Dr. Black, in his Chemical Prælections, that it is the consequence of the extrication of fixed air. Is it probable that any such thing happens in our secretory organs? I should presume not. According to this definition, it is necessary, that the matter changed by fermentation, applied to fresh matter, should convert it to its own likeness; that is, it should act as a ferment. It is demonstrable that this is not the case with our secretions. For, in cases of *icterus*, the bile is evidently conveyed into the blood-vessels, perhaps by absorption; and every circumstance favours fermentation equally, as heat, &c. yet, our fluids are not converted into bile. In cases of *variolaë*, the *pus* is often absorbed from the pustules, and evacuated by the intestinal tube, and in this case, the *pus* is even supposed to have a particular ferment, though taken into the blood-vessels; yet it does not act so as to convert our fluids into a similar matter. Its action seems to me, to be confined to the small vessels on the surface; for, if this particular

lar



lar matter had the power of acting as a ferment on our fluids, I cannot conceive why the disease should ever stop, for fresh fluids are continually formed for it to act upon, and the variolous matter is present in a greater quantity, in the pustules, at the end of the disease, than when it was first applied, in cases of inoculation, to produce the disease. These facts I cannot reconcile with the idea of a fermentation.—I shall prove hereafter, that *pus* is not capable of acting on any of our fluids as a ferment, and of converting them by that means into *pus*.

Another very able chemist defines fermentation to be, “an operation, by which one compound is altered into a different compound, by a new mode of combination in the same elements.” It would be very difficult to prove that something of this sort does not take place in our fluids, though, at the same time, no proofs can possibly be brought to shew that it does happen. But then this is too vague a definition to be admitted, for it does not agree properly with the principal and best known



known species of fermentation, *viz.* the vinous; in which it is certain, that the same elements do not constitute the new compound, as is well known, from the copious separation of fixed air, and deposition of lees and other feculent matter, which happens during this fermentation.

Besides, if the other definition was correct, or, in other words, agreeable to the phænomena of fermentation; this cannot be so, as may appear by comparing them. For though, in the former, a decomposition and different arrangement may probably take place; yet, from certain matters, we always have the same result. Thus, the farinaceous matter of vegetables, is converted into sugar, sugar into wine, and so on, till at last it putrifies, and these appear to me, to be only different stages of the same operation, which invariably happen. But the contrary of this takes place in secretions, and, there is by no means this steadiness; for, though they are all formed from



from the blood, yet, they are wonderfully varied in their qualities.—It may, perhaps, be said, that animals have the power of converting vegetable matter, by the process of digestion, and other animal processes, into an animal nature, without admitting these several stages, which indeed, does not appear to be the case. But then, it must be remarked, that animal matters, in their progress to decay, pass into the putrefactive fermentation. The living principle of animals has a power, however, of resisting this disposition to decay, in a considerable degree; though many circumstances in life are highly favourable to this operation, as the degree of heat, &c. Putrefaction being the next step which animal matter is disposed to pass into, it would undoubtedly be expected, that every change by any process, in the least similar to fermentation, should verge towards putrefaction, and the degree of putrefaction should be in proportion to the change. Our fluids therefore, that are most altered, should, under equal circumstances, run faster into putrefaction,



faction, or, in other words, should sooner putrefy than those which are less changed. The following experiments determine the fact to be otherwise.

It has been pretty generally believed, that *pus* in particular, was formed by fermentation, till some gentlemen, whom I shall mention hereafter, controverted it. However formed, it is manifestly, a change produced on our fluids by the suppuratory inflammation, and therefore, proper for the experiment.

#### EXPERIMENT I.

I took three vials of equal size, No. 1. a little more than half-filled with fresh serum; No 2. filled to the same height with serum, mixed with red particles; No. 3. filled in the same manner, with bland *pus*, which was given me by Mr. Hewson, and which had the same day been taken from the cavity of the *pleura*. These I placed, unstopped, all under the same circumstances,



cumstances, in my window, and examined them from time to time, and observed they putrified in the following order. No. 2, was fætid first, and after it No 1, emitted a fætid smell, while No. 3, was without any, and continued so for some time after. This would not have happened, if the *pus* had been formed by a process, similar to the putrefactive fermentation.

As this, and indeed all the experiments, were made merely for my private satisfaction, without the least idea that they were to appear in print, I was not perhaps, sufficiently exact in noting down the particulars, but paid the greatest attention to the general conclusions they lead to. As, however, I had an opportunity of repeating this experiment, and observing the particular time that each began to putrify, with greater nicety, I determined to do so.

#### EXPERIMENT II.

Edinburgh, June 21, 1774, I obtained about two ounces of good *pus*, taken  
from



an abscess, which, unfortunately however, was mixed with some of the red particles. I also got the same quantity of fresh serum, separated from red particles, and an equal quantity of serum, loaded with red particles. The blood, from whence I took these parts, was taken from a patient, labouring under an inflammatory disease, two or three hours later than the abscess was opened, and the *pus* obtained. The three vials containing these, I marked A, *pus*; B, serum; C, serum with red particles. These I placed in a basin with Fahrenheit's thermometer, and exposed them to the sun. The degree of heat, for the most part, was in the morning, about 58; between twelve and one, from 66 to 70; and in the evening, 58. I also, at times, when the sun did not shine on them, endeavoured to promote the putrefaction, by placing the basin in a water-bath, in a degree of heat as near 98 as possible, but it scarcely ever reached so high.

June 26. In the evening C shewed a slight degree of fætor, which was increased during



during the night, so as to be very evident the next morning. A, and B, quite sweet. June 27th, ditto.

June 28th. C, fætid ; A, and B, sweet. 29th, C, fætid, A, and B, as yesterday. 30th, as yesterday. July 1st. in the morning, A, emitted a disagreeable smell, B, still sweet ; C, more fætid. July 2d. A, was evidently fætid ; B, gave an unpleasant smell ; C, more fætid. July 3d. A, more fætid ; B, evidently putrid ; C, very fætid.

Though, in this experiment the result is different, yet, when the cause of this difference is ascertained and explained, we shall find it will in no wise contradict the conclusion drawn from our former experiment.—For, we must consider, that, as the addition of the red particles caused C to putrify such a length of time before the pure serum B ; so also the red particles, that were unavoidably mixed with the *pus* in opening the abscess, must also promote the putrefaction of A ; and it leaves us not the least reason to doubt, that, if it were not



for the mixture of the red particles, which forwarded the putrefaction of the *pus*, that it would have remained much longer sweet than the serum; at any rate, it proves, that *pus*, even when containing the red particles, is not so putrescent as the serum with red particles. So this experiment also is satisfactory.



## C H A P. II.

*An Endeavour to determine whether Pus is formed by a Fermentation ; and whether it can possibly act as a Ferment, either on the Solids or Fluids.*

**T**HAT *pus*, does not in the least depend on fermentation, and also, that it is incapable of acting as a ferment on the effused fluids, I am convinced, from the experiments, I shall now relate, and which were made while I was studying at the Middlesex Hospital, at London.

## EXPERIMENT III.

I took a thin slice of mutton, and placed it in a deep ulcer of the leg, which was in a granulating state, and produced laudable *pus*, leaving some of the *pus* to act as a fer-



ment. I covered the ulcer with a piece of smooth lead, and bandaged it up. I viewed it sometime after, and found that the piece of mutton was every way lessened, but it was not converted into *pus*; on the other hand, it was very fætid, differing exceedingly from the secretion that the ulcer formed at that time.—From this experiment, it appears, that *pus* is not produced by a fermentation of the solids; and also, that the cause of lost substance, in cases of abscesses, is not owing to the substance being converted into *pus*, and therefore that it must depend on some other cause. For though the piece of flesh, in this experiment, was so acted upon by the *pus*, or more probably by putrefaction, I cannot conclude that this effect would happen in an equal degree on a living part. For, on the contrary, we know that there is a power in life, of resisting the action of any cause that tends to its destruction, proved by applying causticks of equal strength and size, one to the living body, another to the dead: we shall find the effect



fect on the dead body much more considerable than on the living. Mr. Hewson, in the 7th chapter of his Lymphatic System, hath advanced, that the waste of solid parts in abscesses, may depend on the pressure of the fluid contained in the cyst, which pressure will increase, in proportion to the accumulation of the matter within, till at last it bursts where there is the least resistance. I may observe, in confirmation of this, that pressure seems to have this effect in a very considerable degree, as we may perceive by what happens in aneurisms, where there is pressure against a bone; for it destroys the bone, as I have seen, particularly in a case of aneurism of the curvature of the *aorta*, which destroyed a large portion of the *sternum*.—This fact, I believe, cannot be accounted for from any idea of fermentation; but pressure, in both instances, appears to be the cause, in whatever manner it produces the effect.

With a view to ascertain whether the effused fluids were fermented into *pus*, I made the following experiment.



## EXPERIMENT IV.

Into the same ulcer, which continued to form good *pus*, I poured a quantity of fresh serum, which was separated from the blood of a patient labouring under an acute rheumatism, and exhibited an inflammatory buff. I left, as in the last experiment, a small quantity of *pus*, to act as a ferment, and covered it with lead. When I examined it, I found it was become very fætid. At first I made this experiment with the serum of blood which was not inflammatory ; but Mr. Hewson suggested to me, that perhaps, in order to the formation of *pus* by fermentation, it required that the serum should be acted upon by inflamed vessels, and desired me to repeat the same experiment, with blood which shewed an inflammatory crust. The result of both, however, according to expectation, was the same.

In order to determine whether it was the coagulable lymph that was changed into  
to



to *pus*, the following experiment was made.

### EXPERIMENT V.

In the same ulcer, with all circumstances as recited above, I placed a slice of the buffy coat of inflamed blood, *i. e.* the coagulable lymph coagulated. As in the other cases, so in this, it became fætid. This experiment, however, is not conclusive; for it may be said, that the lymph is poured out in its fluid state, and is acted upon before it coagulates. The following therefore was made, which is more decisive.

### EXPERIMENT VI.

From the blood of a patient affected with an acute rheumatism, I collected the lymph by little and little, taking advantage of the slowness with which the blood coagulates, and placed it in the ulcer, covering it up from the action of the air. The



quantity of lymph was small; but, as in the other experiments, so in this, it gave a degree of *fætor*.

From these experiments I am led to conclude, that *pus* is not the consequence of fermentation; and if a morbid alteration does not depend on this chemical process, I think there is less reason to suppose that the natural secretions are produced by any such means. I shall hereafter say, that *pus* is probably formed by the exhalant arteries, at least in some instances. The only fermentation which uncontrovertibly takes place in the animated system, is the putrefactive, and this can only happen, in any considerable degree, where the *vis vitæ* is feeble, and at any rate cannot proceed far, consistent with life. Perhaps those diseases, which are said to be produced by contagious ferment, may probably depend on the specific *stimulus* of each matter, causing morbid alteration in the action of the moving fibre or nervous system, and by this means alone affecting our fluids, and by

no



no means deriving their origin from fermentation. In both ideas there are difficulties, but to me it is more conceivable, and more consistent with animal life, that various alterations or deviations from the proper action of the vessels should happen, than to suppose that there are so many different fermentations; especially, when I reflect on the great diversity there is in the *succus proprius* of vegetables, and consider that the most mild and the most acrid receive nourishment from the same water, as is experimentally proved by Du Hamel, &c. The honourable Mr. Boyle, in his work concerning the usefulness of experimental natural philosophy, *part II. essay ii.* says, “that rain water, which in its passage through a vine or an apricot-tree, or the like plants, is turned into a sweet liquor; in its passage through those plants that bear lemons and barberries, is transmuted into a liquor sharp enough to corrode not only pearls, but coral, *lapides cancerorum*, and other hard concretes, as *spiritus vitrioli* would do.” Dr. Hope also observes, that the mallow and the arum grow-

ing



ing in the same garden-pot, and supplied with the same water, &c. retain their proper and distinct natures; for while the one is mucilaginous, the other is acrid. I am led to ask, therefore, whether it is probable, or even possible, that this variety of changes on water should be effected by fermentation? I should suppose not; for, as every circumstance is the same, except the difference of vegetable organization, I cannot conceive that the changes can depend on any thing but the different constructions of the different vegetables; which from hence derive the power, they possess, of changing the water they imbibe.



## C H A P. III.

*An Enquiry whether the Blood is alive.*

**I**T might be expected, that, whilst I argue against fermentations taking place in our secretions, I should take advantage of an ingenious supposition, which I lately met with, in the Medical Commentaries, I mean an idea of Mr. Hunter's, viz. "that the blood is really alive:" for it certainly would be of use to me, in the course of this treatise, if I could admit the fact. But I avoid founding the least upon so hypothetical an idea; and, indeed, as in my next general division I mean to consider the blood as altogether passive, and merely acted upon by the living solid, I think myself obliged to endeavour to refute this opinion, before I can possibly be at liberty to proceed to reason as though the blood be really not alive.

With



With this intent, I will examine Mr. Hunter's proofs of this opinion separately, and in order.

*First*, “ Because it unites living parts, in some circumstances, as certainly as the yet recent juices of the branch of one tree unite it with that of another.” If the juices of vegetables were proved to be alive, such analogous reasoning might be admitted ; but no one ever supposed it. If we attend to the operation of grafting, we shall not conclude with Mr. Hunter that it is the recent juices that is the cause of the union in trees ; for the gardener does not depend on the juice, but is chiefly careful to oppose certain parts of the graft to similar parts of the stock. He applies these together, as soon as the necessary incisions are made ; not that he requires the yet recent juices to unite the parts, but because the vegetating powers are still vigorous : and on this account he prefers a particular season of the year.

*Secondly*,



*Secondly*, “Were either of these fluids to be considered as extraneous or dead matters, they would act as *stimuli*, and no union would take place either in the vegetable or animal kingdom.” This argument, Mr. Hunter imagines, is still farther established by the following experiment, *viz.* the testicle of a living cock, being introduced into the belly of a living hen, upon injecting the liver of the hen, was injected along with the liver, as it had come in contract with it and adhered to it. As Mr. Hunter supposes that the fluids, or any matter whatever, if dead or extraneous, should act as *stimuli*, *i. e.* according to his idea, should produce a particular process, which must terminate in throwing off the dead or extraneous part from the living or sound parts; I would ask, why the testicle of the cock did not cause this process to begin? I avoid engaging in the dispute, whether a part of an animal when severed from a living animal, may or may not be said to have life.



In some of the less perfect animals, it is a fact, that the separated part is capable of becoming a complete animal; also that the muscular parts of the perfect animals retain their irritability some time after respiration has ceased.

We therefore will not venture to affirm, that the testicle of the cock ought to be considered as a dead part, but surely it must be allowed, that it was very extraneous with respect to the liver of the hen.

But Mr. Hunter observes, "that this union of living parts, takes place in certain circumstances." I can only find that it happens in cases of inflammation, and the phænomena of inflammations are sufficient to explain this union, without the supposition of the fluids being alive. For we observe, that inflammation disposes the inflamed vessels to elongate, and the union of parts depends on this elongation; as we may observe in adhesions of the inflamed *pleura*. Du  
Hamel



Hamel, relates an instance, where the spurs of a cock placed upon the comb, when it was inflamed, fixed there, and grew. From these and all the other proofs, which Mr. Hunter himself has brought, there is much more reason, I think, for supposing that the union of parts depended rather on the life and elongation of the vessels, than to suppose that the fluids are alive which these vessels carry.

*Thirdly*, “The blood becomes vascular, like other living parts, and the *coagula*, as Mr. Hunter affirms, in the extremities of arteries, after amputations, may be injected by injecting these arteries.”

This indeed may be the case, and nevertheless, the fluids which these vessels carry, may not be alive. For I have just observed, that inflammation causes the vessels to elongate, and after amputation the stump is inflamed; hence vessels may arise and pass through this *coagulum* and anastomose; but they by no means take their origin  
from



from the *coagulum*. For if this were possible, I should expect that those clots, that are termed false conceptions, should be vascular, which is by no means the case; and that the blood should become vascular in every instance, where it is extravasated. Besides, the blood, in its coagulated state, cannot possibly be said to possess life, according to Mr. Hunter's own idea of life. In order to prove this, I must employ his arguments against himself.

In the first place, Mr. Hunter, in considering the blood as alive, views it as a fluid; as is evidenced by his having alledged, "that in the nature of things, there is not a more intimate connection between life and a solid, than between life and a fluid." By coagulation, the blood loses its fluidity and becomes solid. Mr. Hunter seems to admit that the coagulation of the blood is a morbid change, produced by the stimulus of exposure which happens, he says, as certainly, as the cavity of the  
*thorax*



*thorax* or *abdomen* inflame, from the same cause. I alledge, admitting for an instant, that the blood whilst fluid was really alive, that it must, according to Mr. Hunter's own words, be considered, when in its coagulated state, as being really and certainly dead. If this be the case, it undoubtedly is a very unfavourable circumstance, to be brought as an illustration of the blood possessing life. But what is life, in the sense in which Mr. Hunter adopts that term? It may be gathered, from observing that he considers a muscle, cut out of the body, to be alive as long as it continues capable of being acted upon by *stimuli* of any kind; that is, as long as a part retains any irritability. Mr. Hunter agrees to this, by saying that the muscles of a turtle continue alive a great while after the animal, as one whole, is dead. Query, does the *coagulum* possess the least degree of this kind of irritability, or life, as Mr. Hunter is pleased to term it? No, it certainly has lost all irritability; and therefore Mr. Hunter himself, not only agrees



that coagulation is a morbid change, but by his own arguments, proves that the *coagulum* of blood is really a dead matter.

*Fourthly*, “ Blood taken from the arm, in the most intense cold which the human body can bear, raises the thermometer to the same height, as blood taken in the most sultry heat.” This Mr. Hunter considers, as a strong proof of the blood’s being alive, as living bodies alone have the power of resisting great degrees both of heat and cold, and of maintaining, in almost every situation while in health, that temperature which we distinguish by the name of animal heat. This is indeed, a proof that life can resist the action of external cold; but is by no means a proof, that this power existed in the blood. On the contrary, blood drawn in several different circumstances of the air, in less than two hours will be cooled to the degree of the surrounding atmosphere, as I am convinced from often repeated experiments. I therefore aver, that the blood, independent



dependent of the animal system, cannot retain heat, which indeed, seems to be connected with life. Now as, in fact, we find it cannot retain its heat, it cannot be alive.

*Fifthly*, “ Blood is capable of being acted upon by a *stimulus*; for it coagulates on exposure, as certainly as the cavity of the *thorax* or *abdomen* inflames from the same cause.” Any person would be laughed at, if he were to affirm that a jelly of any kind were alive, because in a certain heat it continues fluid, and on diminution of this heat, or on exposure to cold and air, it coagulates.

*Sixthly*, “ The more it is alive, *i. e.* the more the animal is in health, the sooner it coagulates on exposure; and the more it has lost of its living principle, as in the case of violent inflammation, the less it is sensible to the *stimulus* produced from its being exposed, and the later it coagulates.” It should seem, from this, that it is a



mere passive kind of life that the blood enjoys; for it is totally dependent on the animal powers, which cause it to circulate. I must also remark, that it must be a very curious kind of life which it possesses; as the more it is alive, the easier it is affected by a *stimulus*: now, it is the reverse in every other kind of life; for the weaker the principle of life is, the more violently it is affected by slighter *stimuli*.

But perhaps Mr. Hunter will advance, that the blood *which really is alive*, when acted upon by inflamed vessels, and violently agitated by this means, falls then into a comatous state, so that it is not so much affected by *stimuli*. I might also add, that the blood is found coagulated, where the *stimulus* of exposure is not applied, as is proved by effusions in the brain. The coagulation here, takes place merely from rest, which Mr. Hunter, I presume, will maintain to be a *stimulus*.

*Seventhly,*



*Seventhly*, “ The blood preserves life in different parts of the body. When the nerves going to a part are tied or cut, the part becomes paralytic, and loses all power of motion, but it does not mortify. If the artery be cut, the part dies, and mortification ensues.” What keeps it alive in the first case? Mr. Hunter believes, “ that it is the living principle alone which can keep it alive; and he thinks that this phenomenon is inexplicable on any other supposition, than that it is supported by the blood.” I believe no one ever doubted that blood was essentially necessary to the life of a part. If this experiment proves any thing, it is that nourishment is chiefly conveyed by the arteries, and not by the nerves; but even in this view it is hardly satisfactory. For though the larger branches of nerves were tied or cut, yet the almost infinite division of nerves would lead us to conclude, that a number of these small branches were still left, at least sufficient to continue the circulation of the fluids, which



depends on the action of the solids produced by nervous influence.

And though a part deprived of blood does mortify, yet the fluid, which sustains it, may not be alive itself. At least this seems by no means necessary, for the whole animal body cannot exist long, independent of certain *ingesta*, taken into the stomach. But because this is highly necessary to preserve life, we cannot from thence infer that the *ingesta* must have life.

To conclude this chapter, I affirm, that the blood, considered as a fluid, and in this light merely Mr. Hunter views it, cannot, in the nature of things, have life: for life evidently consists in the performing [certain functions of an active kind; and for the performance of these, a certain organization is absolutely necessary. Fluids do not admit of organization, therefore they cannot be alive.



## S E C T. III.

*A more probable Conjecture concerning Glandular Secretion.*

## C H A P. I.

*The Arguments which support this Conjecture.*

**A**Lthough the opinions which I have mentioned, concerning glandular secretion, have, upon being attentively examined, proved altogether unsatisfactory, yet I do not think this intricate part of the physiology, totally incapable of being explained. I propose, in the next place, to offer some attempt towards an explication of it.

With this intent, I must premise that all secretions are produced in consequence



of life, and that, independent of it, they cannot proceed. From hence we may justly infer, that secretions are very intimately connected with the living principle or nervous system.

The animal machine is wonderfully constructed of solids and fluids: the former possess the living or active principle, the latter are passive, and totally dependent on the solids, which are endowed with the power of deriving nourishment from the fluids, for their increase, and the other purposes of the machine.

The vascular systems, which contain the fluids, can only be affected by nerves, in as far as they change the state of motion. This, I believe, cannot be doubted of by any. If then our secretions depend totally on the influence of the nervous system, and this system gives the power of action to the vessels; it appears, that it must be the effect of this action that determines the nature of our secretions. And  
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in the presumption that secretions depend on the influence of the nervous system, I am supported by some of the greatest authorities in medicine, viz. Hoffman, &c.

I derive proofs that secretions depend on the action of the vessels, in the first place, from considering the secretions in their sound state, and the alteration produced on them, without the least morbid change in their construction. For this purpose, I may observe how instantaneously the milk in the *mammæ* is changed by sudden fright; and also that the liver, as may be learned from the very ingenious Dr. Maclurg, has its secretion increased by anger; and that, in general, the secretion of bile has a considerable relation to the passions. I would here ask, what possible effect the passions can have on the fluids which are carried by the vessels, except by their influence in changing the state of action of the vessels, which contain the fluids? Now, that emotions of the mind, and passions, have a great power on the actions  
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of vessels, is a universal observation, and demonstrated by the effects of modesty or shame, in producing blushing, and by fear, producing paleness. It is from hence that the passions are properly distinguished into sedative and stimulant. The glandular parts on which the sedative passions exert their influence have the action of their vessels diminished, and in consequence of this effect, the quantity of secreted fluid of such parts is also lessened. The contrary of this takes place from the stimulant passions.

Sedatives and stimulants applied to the body, where glands are situated, produce effects on the secreted fluids, by diminishing or increasing the action of the discerning vessels ; thus cold lessens the quantity of perspiration, while heat increases it.

The variety of motions which the nerves are capable of causing, in the larger arteries, are almost infinite, as we learn from those writers, who treat of the pulse. And  
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that an equal variety may take place in the action of the small vessels we have not the least reason to doubt. Indeed, I have proof of it in the phænomena of topical inflammations ; for we feel pulsations, when a part is inflamed, which were not felt before.

As it is fully ascertained, that the nerves give origin to animal motion, is it not probable that these motions are variously modified, according to the structure of the organ, on which the nerves exert their influence ? Thus, if that coat of the vessels, which has a muscular power, is weaker in certain parts than in others, such parts must of course have a weaker action. And also, if the elastic coat, which appears to assist in the action of the vessels, by counteracting the contraction of the muscular power differs in strength in different parts, (for it is probable the alteration of distension depending on the elastic coat, \* and the

\* If the vessels are ever distended beyond their elasticity, by an over quantity of blood, as is the case, perhaps



the contraction depending on the muscular coat that constitutes the action of the vessels in every part of the body does the same) variety of actions in the vessels will undoubtedly arise from this source. Hence, therefore, it may justly be presumed, that a variety of motions should be produced, depending on the relative strength of these coats : and it is undoubtedly probable that the coats of the secretory vessels do differ, and most likely in those circumstances I have mentioned. Dr. Haller seems to be somewhat of this opinion, and on his judgment I always lay the greatest stress. His words § CCXXI. are as follow :  
*“ Densitates arteriarum minimarum & densitates pariter osculorum secretoriorum diversas esse posse, nihil repugnat & suadet in majoribus ramis certa observatio.”\** And

perhaps sometimes, in plethoric habits, then the elastic becomes an assistant power to the muscular coat, by endeavouring to resume its proper diameter.

\* Nothing hinders the densities of the smallest arteries, and the densities of the secerning orifices too, from being different ; and certain observation persuades us, that the case is so in the greater branches.

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## GLANDULAR SECRETION. 77

in the ramifications of the vessels I have noticed an extensive variety, which I have often seen in most beautiful microscopical preparations, made by Mr. Hewson. Dr. Haller has also observed, this great variety,

§ CCXIX. “ *Aspergillum in liene referunt arteriolæ rubræ, confertim ex trunculis oriundæ; pencillum in intestinis, serpentis in renibus, stellam in hepate, cincinnum in teste, circulum in uvea efficiunt. Non frustra vero fabricæ hanc diversitatem a Creatore factam esse, non obscura conjectura est.*”\*

This diversity of ramification, no doubt, has its effect, in determining the nature of our secretions; but it is chiefly to the action of the vessels in the different glands that I attribute the greatest power of changing

\* The minutest arteries resemble a watering-pan, in the spleen, arising in crouds from small trunks; a brush in the intestines; serpents in the kidneys; a star in the liver; a curl in the testicle; a circle in the *uvea*. Now it is perfectly obvious, that this diversity of structures was not given in vain by the Creator.



our fluids. Dr. Cullen † inclines to this opinion, as appears from § CCLXXXI. of his *Physiology*; speaking of secretions, he says, “till we can discover these more clearly, we may, in the mean time, observe, that the action of the vessels of the secretory organ, has a considerable share in determining both the quantity and quality of the secreted fluid.

Indeed the capacity of the fecerning orifices of glands merely, and the velocity of motion, which the blood acquires by the action of the large arteries, seem to have considerable influence in determining the quantity, and perhaps, in some instances, the quality of those fluids, which are considered as separations from the general mass. To illustrate this, it is observed, that the matter of perspiration is increased by exercise, and that the kidney in a spasmodic state of its vessels, separates a limpid fluid, without the admixture of the muc-

† Professor of the practice of Medicine at Edinburgh.



lage, which is supposed to be generated by a small degree of putrefaction taking place in the blood. These circumstances, however, assist us very little in accounting for the phænomena of secretions; for, in general, they are independent of the velocity of the blood in the course of circulation.



## CHAP. II.

*The Subject continued.*

FROM the reasoning in the preceding chapters, I humbly presume, that the secreting vessels of glands have a plastic power on the fluids, and by this power produce their effect on them.

I am confirmed in this opinion, by reflecting on the power of the large vessels, in producing changes according to their mode of action, and by considering the different state of some of the interstitial fluids, which vary in properties, according to the healthy state of the animal. What may be used as a further argument for this opinion, is, that the different appearances of *pus*, depend totally on the different degrees of inflammation, or, in other words, on the different action of inflamed vessels.



Mr. Gaber is the author of an ingenious paper in the *Miscellanea Taurinensia*, in which he endeavours to prove, that *pus* may be formed by the putrefaction of serum. In repeating this gentleman's experiments, I find the phænomena, in as far as I attended to them, as he has related: but yet, I can by no means agree with him, that the deposition of putrid serum is *pus*: his ingenuity has indeed carried the analogy a great length; yet, I humbly think, that the sediment of serum differs from *pus* in several essential circumstances. A considerable time is required for this deposition, whereas *pus* is completely formed in a few hours, a short time only being necessary for the fluid parts to be absorbed; and it is from this part, being absorbed, that it acquires its opaque and viscid appearance.

It is true indeed, that \* "*Pus laudabile fere semper fætet, sed parum, et vix sensibili-*

\* Laudable *pus* almost constantly has a disagreeable smell, though in a small degree, and sometimes hardly perceptible.



*ter*:" yet, it must have happened, to every observer on this subject, to have been frequently found perfectly sweet, (particularly in the large cavities) and having very much the appearance of good cream. Serum does not deposit its sediment until there is a very perceptible *fætor*, that is, not until putrefaction is advanced a considerable way; I cannot therefore suppose *pus* to be produced by putrefaction, until it can be proved, that it is always fetid; which cannot be done.

I know very well that *pus* will become putrid, as well as every animal fluid; but I have already proved, that it is not so putrescent as serum.

It may indeed be advanced, in favour of *pus* being formed by this process, that, in some instances, where it remains but a short time, it is found to be very fetid; and an example may be brought from the very fetid smell, which a stump gives, in several of the first dressings after amputa-



amputation. I think, however, that *pus* acquires this smell, in consequence of the red particles oozing out from the small divided arteries, and promoting putrefaction. This idea is proved by noticing, that, after a certain time, when these vessels may be supposed to be closed, the *fætor* goes off.

As I have taken pains, therefore, to ascertain that the formation of *pus* does not, in the smallest degree, depend on a fermentation of the solids or fluids, this is the proper place to mention on what I think it does depend. And here, I exactly coincide in opinion with those who have affirmed that *pus* is secreted by inflamed vessels. The first person who advanced this very ingenious doctrine, was, I believe, Dr. Thomas Simpson, professor of medicine and anatomy, at St. Andrew's. For this gentleman, in a public oration, *anno* 1726, *de re medica*, says, "*In medicorum disceptationibus, nihil est celebrius, quam ut tumores, fluores, morbosque omnes,*



quiquidem evacuationibus omnino cedunt, ad humores statim pravos & malignos referant; nempe quia nescii sint, quomodo liquores aliam atque aliam induere possint formam, nisi vel fermentum aliquod, vel alia quæpiam nova insolensque materies interveniat. Verum, ut de hac re clarius certiusque constituatur, ex familiarissimis maximeque pervagatis corporis humani casibus; posulo, ut mecum velint vulneris, quod musculosæ alicui corporis benè sani parti infligitur, ordinem & progressum paulisper expendere.—Primo, igitur ex hiantium vasorum osculis pleno rivo profluit sanguis sincerus; deinde, iisdem visua contrahente paulatim conniventibus, liquor tenuior & pallidior, donec tandem omnino coeant, cum levi partis sauciæ inflammatione; quam sequitur mox sanguinis confluentis remora, & ex ea passim febricula, donec vasa dudum contenta denuo se sponte sua remittant, sinantque in communi vulneris alveo deponi liquida, ubi stagnantia & a calore corporis modicè feta, in unam albidam sui que similem materiem, quam vulgus pus appellat, concrecant. Quod si jam pisum,  
vel



*vel ejusmodi quid in plagæ fauces injiciatur, simulque arceatur aeris contagium, efficitur ut pus illud quamdiu voles fluat; prorsus ut hoc pacto repente existat emergatque nova quædam glandula. Contra, si vulnus quoquo modo irritatur, nimiumve comprimitur, liquida ex integro mutabuntur, aliamque speciem ferent, id quod chirurgi optimè norunt.——*  
*Hinc sequitur facilius esse nihil, quam ut corporis secretiones humoresque, sine ulla novæ materiæ vel fermenti accessione, mutata solum vasorum secernentium diametro numero, immutentur.”\**

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\* “ Nothing is more common, in the writings of physicians, than to ascribe, without hesitation, tumors, discharges, and all diseases which yield, in any manner, to evacuations, to vitiated and corrupted fluids; for which no other reason can be assigned, than that they do not understand how fluids can assume different forms, unless through the intervention of some ferment, or some other new and unusual matter. But that this point may be determined, with greater clearness and certainty, from the most familiar and common cases that occur in the human body, let them attentively consider, for a little, the order and progress of the appearances of a wound, which is inflicted on any muscular part of a sound body.— Now, first of all, there issues, in a full stream, from the mouths of the yawning vessels, pure blood; then,  
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Since this Dr. Morgan, has laboured the same subject at greater length, and with the greatest success, as appears from his excellent inaugural dissertation *De Puopioesi* anno 1763, to which I refer the reader for satisfactory arguments.

An additional proof, that *pus* is formed by a secretion depending on the action of

the vessels contracting by little and little by their own contractive power, a thinner and paler liquor; until, at length, they entirely shut, while a slight inflammation takes place, in the wounded part, which is soon succeeded by a cessation of the efflux of the blood determined to the part, and that by a slight fever, until the vessels, formerly stretched, spontaneously relax themselves, and deposit their liquid contents in the common cavity of the wound; where stagnating, and moderately cherished by the heat of the body, they are converted into a whitish and perfectly uniform matter, commonly called *pus*. But now, if a pea, or any thing of that sort, is introduced into the mouth of the wound, the consequence is, that the *pus* discharges as long as you please; so that, in this manner, a new kind of gland, as it were, suddenly starts into existence; on the contrary, if the wound is in any way irritated, or too much compressed, the fluids will be entirely changed, and assume another appearance, as surgeons well know.—— Hence it follows, that there is nothing easier than to change the secretions and fluids of the body, without any addition of new matter, or a ferment, by only altering the diameter of the discerning vessels.”

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the vessels, may be derived from the surgical treatment of inflamed parts. Experience has convinced surgeons, that a certain condition of *pus*, is connected to a certain state of the part inflamed; and accordingly, to abate or encrease the inflammation, they apply, either sedative or stimulant applications; whereas, if the fluid was fermented into *pus*, after its extravasation, we should not expect that they would proceed in such manner; but, that instead of applying those applications, which exert their influence, in altering the state of action of the inflamed vessels, they would be chiefly solicitous to employ such methods as might promote the fermentation.

Mr. Hewson has adopted this opinion, (see p. 117, 118, 119, of his Lymphatic System) and has farther advanced, that in some instances, it is secreted by the exhalant arteries, C. 9, where *pus* is found in the cavities without the least exulceration. The doctrine of fermentation being previously exploded, this fact amounts almost  
to



to a demonstration, that *pus* is a secretion, depending totally on the suppuratory inflammation, and also that in such instances it must be formed by the exhalant arteries.

Finding then, that the exhalant arteries, under different circumstances, produce different effects, as well as do the larger arteries by their alteration in this mode of action, all that remains, in order to establish that secretions, in general, are performed by the action of the secreting vessels, is to transfer this reasoning to those glands, which I treated of as more complex or compleat glands. This I have in part done, where I affirmed that they were universally vascular; for though, at the same time, I remarked that there was a difference in the ramifications of the vessels in different glands, &c. yet this, and other varieties, which no doubt exist in their construction, are only such as are proper for them, as they are to effect a greater change on the general mass. But that there is probably a similarity between the exhalant



lant vessels, and the fecerning vessels of glands, appears to be the opinion of the illustrious Haller, by his asking § XLIII.

*“Annon adfinis est exhalanti fabricæ omnis secretio in glandulas veras sive cryptas?”*

Now, as the nature of the interstitial fluids, as I have endeavoured to prove, depends on this simple organization of an exhalant vessel; so perhaps, the specific qualities of all the secretions are formed by those arteries, which, in their continuation, constitute the beginning of the excretory ducts of glands.

I humbly presume, that this opinion is somewhat supported by remarking, that secretions are not necessarily affected by changes in the general circulation; because the excretory duct is removed at a greater distance, and the velocity of the blood is previously much diminished, by the various contorsions of the minute arteries. And perhaps, this opinion may be confirmed by the observation, that the secretions are increased



creased by stimulating their excretory ducts, that is, by increasing their action.

The lymphatic vessels of the lymphatic glands, with those of the spleen and *thymus*, according to this view, are to be considered as adequate to the excretory ducts of other glands; for these vessels appear to answer a similar use.

### F I N I S.