An experimental inquiry into the function of the liver : in which the most popular doctrines respecting the function of this organ are examined, and that of Dr. Rush adopted and vindicated. Also, an experimental inquiry into the function of the spleen, gall bladder, pancreas, thyroid and thymus glands, and capsulae renales : being an inaugural dissertation submitted to the public examination of the Trustees and Professors of the College of Physicians and Surgeons in the University of the State of New-York ... for the degree of doctor medicine, on the sixth day of May, 1816 / by Luke Douglas.

AN

EXPERIMENTAL INQUIRY

INTO THE

FUNCTION OF THE LIVER,

BOTH IN THE FŒTUS AND ADULT;

IN WHICH THE MOST POPULAR DOCTRINES RESPECTING THE FUNCTION OF THIS ORGAN ARE EXAMINED, AND THAT OF DR. RUSH ADOPTED AND VINDICATED.

ALSO,

AN EXPERIMENTAL INQUIRY

INTO THE

FUNCTION OF THE SPLEEN, GALL BLADDER, PANCREAS, THYROID AND THYMUS GLANDS, AND CAPSULÆ RENALES:

BEING

AN INAUGURAL DISSERTATION,

Submitted to the public examination of the Trustees and Professors of the College of Physicians and Surgeons in the University of the State of New-York,

SAMUEL BARD, M. D. LL. D. President,

FOR THE DEGREE OF DOCTOR OF MEDICINE, ON THE SIXTH DAY OF MAY, 1816.

BY LUKE DOUGLAS, A. B.

Member of the American Æsculapian Society.

Every theory founded on experiment and not assumed, is always good for as much as it will explain. BURKE.

NEW-YORK:

PRINTED BY JOHN FORBES & CO.

78 WALL-STREET.

1816.

TO

VALENTINE MOTT, M. D.

Professor of the Principles and Operations of Surgery in the College of Physicians and Surgeons of New-York; Fellow of the Literary and Philosophical Society, and President of the Physico-Medical Society of New-York,

THIS TREATISE IS INSCRIBED,

AS A TESTIMONY OF RESPECT FOR YOUR

UNIFORM FRIENDSHIP, VIRTUES AND

PROFESSIONAL ABILITIES,

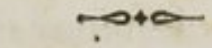
BY

YOUR SINCERE FRIEND

AND PUPIL,

THE AUTHOR.

INTRODUCTION.



The human body being a machine so infinitely complicated, that, although its anatomy, so far as the eye or glass can reach appears to be understood, a part of its physiology, or function of its various organs is still enveloped in profound darkness. If this knowledge is within the reach of human understanding, the reason why it is inexplicable, may be justly ascribed to the superstition which has pervaded all nations, till lately, respecting the cultivation of anatomy and physiology by dissection. Indeed, so great was the abhorrence to this only method of obtaining a knowledge of the laws of organic life, that martyrs to humanity have fallen, in daring to attempt it. And it is to be regretted that this black superstition still exists in many parts of this enlightened republic. But as true science and philosophy progressed, superstition was gradually stript of its tyranny, and anatomy and physiology began to flourish.

When it is considered that all our knowledge of the healing art is founded on these sciences, a

knowledge of them becomes indispensable. For health consists in an easy and regular performance of the various actions proper to the human body; and disease a difficulty in performing those actions, by which weariness and pain is produced, till at last a total stoppage of them is death. An attempt then to practice the healing art, without a knowlege of these sublime sciences, is chance, quackery, and absurdity in the extreme. But it is a lamentable fact, (and one that the eyes of the law ought to discover) that many do practice medicine without this requisite knowledge. Hence the degradation of the profession in the eyes of many, ignorant of natural knowledge. Having been educated in a school that strongly inculcates the necessity of anatomy and physiology, as laying the foundation of a medical education, it has claimed not a small share of my attention, and will be the subject of this essay.

From the first dawnings of physiological science to the present day, authors have paid particular attention to the liver, and have been variously exercising their ingenuity to explain its function. And if we consider that the adult liver is the largest gland of the whole body, and that of the fœtus composing more than one fifth of the whole mass, we are not surprised that the ancients should make it the supreme director of the animal system. Knowing of no other vessels by which absorption could take place than the veins, they imagined they

could see these absorbing the chyle, carrying it into the liver, there to be converted into blood. This blood they imagined was immediately transmitted by the vena cava hepaticæ to the right side of the heart, and from thence the veins carried it to every part of the body.

Previous to the discovery of the circulation of the blood, the absorbents, or structure of glandular bodies, these opinions were not surprising. With respect to glandular secretion, the older physiologists, being deficient in anatomy, and not having premises on which to found a reasonable hypothesis, were content with saying, that glands possessed an inherent power to separate from the blood the various secretions preexisting in it. When anatomical knowledge became more diffused, recourse was had to hypothesis; such as fermentation, filtration, &c. the former supported by Van Helmont, the latter by Borelli and others. These hypotheses, resting on authority and fashion alone, soon gave way to others not less fallacious. Secreting surfaces were supposed by Winslow and Helvetius, to be imbued with a fluid, corresponding in properties with that to be secreted; accordingly a watery fluid could not pass through an oily surface, &c. and vice versa. Upon this principle the difference of serous, mucous, and other secretions was founded. Other physiologists, among whom was Boerhaave, ascribed it to the impetus of the blood in the secreting organ. Others to the caliber,

length and convolutions of vessels. While others, disregarding hypothesis, ascribed it altogether to the vital action, disregarding essentially structure or organization. Secretion must undoubtedly be referred to vital action, but differently organized bodies seem to be essential to the different secretions, for we see a simple canal, a convoluted canal, cells, and a simple surface all pouring out their different secretions. Therefore glandular structure becomes an object of inquiry.

No correct notions were entertained of the internal structure of glandular bodies previous to the time of Malpighi. His experiments and dissections taught him to despise the scholastic learning of the time, and by anatomical investigation to establish doctrines more consonant with reason and true philosophy. Throwing in his injections, carefully dissecting and examining with the microscope, he saw the arteries, after making various contortions, terminate into little cells or follicles, from which the excretory duct originated. Ruysch, although in the early part of his life, was a disciple to the doctrines of Malpighi, yet from a more attentive observation, was led first to doubt, then to boldly reject them, and from new facts to establish a new theory, which proselyted a large part of the anatomical world. Ruysch, by pushing his injections more minutely and successfully, showed that the arteries, instead of terminating in a follicle, terminated in an excretory duct by a continuity of canal.

And what Malpighi called cells, were merely vessels convoluted upon one another. The great celebrity of Ruysch made his museum the resort of kings, embassadors, and all the learned of the age. Indeed, to such perfection did he carry the art of injecting and preparing the various parts of the human body, that no one since his time has surpassed him.

The theories of these two great men still divide the anatomical world, but perhaps the greater part are on the side of Ruysch. Some believe both to be right, contending that acini and penicilli, exist in the same gland. But it must be observed here, that although glandular structure at present seems to be pretty well understood, yet there exist many glands in the human body, the function of which are still unknown. The brain and spinal marrow as giving origin to the nerves of sense and motion, seem to hold the first rank among glandular bodies. The salivary glands pour their saliva into the mouth, to moisten its parietes, and mix with the comminuted aliment, giving it an easy passage into the stomach.

We see the absorbents bending their courses towards the lymphatic glands, to enter them in order to undergo some necessary change.

Arterial blood passes into the kidney, from which the urine is separated. The same blood goes to the testicle, from which that *divine* fluid is secreted, which alone can answer the purposes of generation.

The vena portæ, instead of emptying its blood into the ascending cava, pours it into the liver, there to be elaborated into bile. The function of the above, and many other glands appear to be obvious. But when we speak of the spleen; the capsulæ renales; the thyroid and thymus glands, no obvious use has yet been assigned to them. And although the enormous volume of the fœtal liver would seem to point out its function, yet if we except the theory of Doctor Rush, nothing satisfactory is known of it. By seeing the liver secrete bile after birth, authors were content with ascribing the same function to the fœtal liver, although no use has been pointed out by them for fœtal bile.

As reasoning from analogy without the aid of experiment is vain, I was led to see what facts would result from experimenting on the liver in living animals, and from these facts alone I have attempted to reason.

Experiment fairly and judiciously conducted is true, and was equally so with Hippocrates and Galen, as with Malpighi and Ruysch. But we are often deceived in the consequences deduced from the truest principles; owing, perhaps, in part, to the weakness of human reason, and in part, to precipitate judgment. But experiment ought to found the basis of all our reasoning, however plausible it might otherwise appear. The blood of the vena portarum from its colour, and its circulation over the surface of the intestinal canal, contiguous to the

excrementitious part of the aliment, was considered as having putrescent properties. And on this presumption alone was built an hypothesis, explaining the nature and uses of the bile, which has long existed, and believed by some at the present day, merely from the plausibility of the inference. Now, one single experiment, made by exposing the blood of the vena portæ and arterial blood to the same degree of temperature, to see which would putrify first, conclusively confuted this doctrine, which had occupied hundreds of pages in physiological writings. For it was found that the blood of the vena portæ was less putreseent than arterial blood. Reasoning therefore from inference, however plausible, is fallacious. And although reasoning from experiment is not always true, but experiment per se fairly conducted, is, and will be always true, however fallacious our reasoning. Hence the words of that illustrious chemist and philosopher, Lavosier. "We ought in every instance to submit our reasoning to the test of experiment, and never to search for truth but by the natural road of experiment and observation."

OBSERVATIONS

ON THE

FUNCTION OF THE LIVER.

HAVING in the introduction given a short outline of the history, fabric and action of glandular bodies, I now proceed to examine the most popular doctrines on the function of that great and important organ, the nature and use of the bile, and to offer my own opinion as dictated by experiments performed on living animals.

But previous to doing this, I wish it to be understood, that my experiments were made with the intent of establishing no particular theory. The function of the fœtal liver still being enveloped in darkness, the nature and uses of the adult bile, having for these forty years past, been the source of more learned physiological controversy than have existed of any other fluid of the human body; and observing that many of these theories, although learned, ingenious and profound, did not rest on experimental investigation, the only sure foundation on which a lasting physiological monument can be erected, I was led to submit the most popular theories to the test

of experiment, and the facts resulting therefrom, I designed to constitute the subject of this thesis, without forming or supporting any particular theory. Indeed, my original design was to confine my experiments and remarks exclusively to the fœtal liver, on the supposition of its having a peculiar function. But after satisfying myself that that organ carried on a chylopoetic process, I was led to believe, that the liver after birth, performed the same function; therefore, my inquiries became more general. Progressing farther, and finding experiment to accord generally with, and substantiate the doctrines delivered by the celebrated Rush on that organ, I was necessarily led to adopt and vindicate the theory of that philosopher. But the reader will soon discover, that experiment would not allow me to adopt the whole of this theory, especially with respect to the spleen, gall bladder, and other collateral opinions; but agree with him in the main and most essential parts, namely: That the fœtal liver secretes chyle from the maternal blood, transmitted to it through the medium of the placenta, which is poured into the duodenum by the biliary ducts; taken up by the lacteals; conveyed into the circulation; fitted and prepared to nourish and support the tender fœtus.

That neither the adult or fœtal liver should be considered as exclusively a secretory or an excretory organ, but partaking of both, more excretory in the former and less so in the latter.

That the adult liver performs the same chylopoetic function as the fœtal; but in a less degree, its product being comparatively less, and a greater proportion of it excrementitious.

That the pancreas is an auxiliary to the liver, and its secretion necessary to complete the formation of chyle.

(Whether Doctor Rush considered the stimulus of chyme, or bile necessary to excite the pancreatic secretion or not, I do not know; he has not mentioned it: but, I think the fact sufficiently proved by my experiments on that organ.) So far I agree with the theory of Dr. Rush, and think that it can be corroborated by experiment. But with respect to the gall bladder, my experiments so far from corroborating his theory, I think prove it to be incorrect. And I can by no means agree with him with respect to the function of the spleen. According to this theory it will be evident, that two chylopoetic processes are constantly going on in the human adult system. The one performed in the mouth, stomach and intestines by the action of their respective fluids upon the aliment; the other performed ostensibly by the liver and pancreas, agreeably to the theory of Rush, but in my opinion greatly aided by the spleen, as experiment will shortly show. It will also be evident, that the whole chylopoetic process of the fœtus is performed ostensibly by the liver, without the aid of the stomach, assisted according to my views, less by the pancreas and spleen, and more by the thymus gland and capsulæ renales. After birth, the solution of the aliment by the action of the saliva, gastric and intestinal juices and absorbents upon it, may be considered the primary or imperfect process, because the chyle although absorbed and carried into the circulation, is not sufficiently animalized to become pure blood, or to serve all the purposes of nutrition and secretion, but requires the second, or perfect process, to complete that deficiency.

As the fœtal liver was the first object of inquiry, I deem it necessary to make some remarks, particularly on that organ, before I proceed to those of a more gen-

eral nature. The attentive physiologist when viewing the fœtal liver, is struck with two remarkable circumstances: First—its immense volume when compared with the liver of the adult body, (composing more than one fifth part of the whole fœtal mass, and only about one twenty-fifth of the adult.) This circumstance alone, if compared with the other works of nature that has made nothing in vain, is enough to convince him that its function is highly important; for tracing the unerring laws of nature, he sees her invariably guard against unnecessary redundance. Those parts which have no office to perform until after birth, are comparatively small; and those useful only to the fœtus, are seen gradually to decrease after birth, and at last disappear. This law being equally manifest in the adult system, and knowing that the liver is also the largest viscus of that body; and add to this a knowledge, that this organ obtains in all animals having red blood, is necessarily urged to the strong belief, that the liver is an exceedingly important organ both in the fœtal and adult economy, but supremely so in the former.

Secondly; he sees the maternal blood, destined for fœtal nourishment, brought by the umbilical vein, through the medium of the placenta, instead of going directly to the heart, and from thence sent to every part of the body, as other decarbonized or arterial blood is, to nourish it; is poured first into the liver. Observing this wonderful peculiarity, and reasoning from analogy, is convinced of the importance of its function, but contenting himself that the use of the bile after birth, is sufficiently explained and very obvious, and knowing, that it is impossible for it to perform the same offices in the fœtus; there being no excrementitious discharge, no chyme to change and precipitate, no ca-

thartic or antiseptic wanted, he is forced to assign to this great viscus, some peculiar function, different from that which he supposes it to perform after birth. And my experiments were first directed to ascertain, what this function was.

On this ground different uses have been assigned to it by different physiologists; the most popular of which I shall mention.

It has been said by Haller, the greatest physiologist of the last century, to impede the velocity of the blood in the umbilical vein before it should be poured into the heart. He also says, that it secretes bile, which is bland and sweet, but says nothing of its use. By assigning a use to the bill after birth which it could not serve in the fœtus, physiologists were completely staggered with respect to the function of the fœtal liver: indeed so complete was their embarrassment, that vague and even frivolous uses have been assigned to that important viscus. This will explain the reason why Haller, that prince of physiologists, should assign to this viscus, a function, analogous to that performed by the foramen caroticum, of the petrous portion of the temporal bone. It appears to me very strange, that the umbilical vein, after receiving its blood slowly and gradually from the maternal part of the placenta, and running sometimes two feet in a contorted manner, sometimes taking two or three turns around the neck of the fœtus before it arrives at the liver; that the impetus of the blood in this vein should be so great, as to require an organ equal in bulk to one-fifth of the fœtus, to check its progress. We need not ask whether so great a man as Haller, was satisfied with this hypothesis. As a physiological writer, it was his duty to say something, and perhaps nothing more reasonable was left to be said, after assigning the above

uses to the bile. Throughout the whole treatise of Haller on the liver and bile, it seems to be evident, that he was not satisfied with the doctrines which he inculcates. He seems to think, that the purposes answered by the secretion of bile in the liver after birth; are too degrading for an organ of that magnitude. Therefore he labours to assign other uses to it; such as, the support of the diaphragm; that it pushes it up in expiration, receives its contraction in inspiration, so as to compress the abdominal viscera in a uniform manner; and that it foments, assists and imparts heat to the stomach, &c. And of the fœtal liver, he says: "When I reflect that there is no bile required in the fœtus, there being no food received; when again I see that the liver is of great size in the fœtus, and not small like the lungs, which are destined to an operation in the economy after birth; I cannot but suspect, that it has some other use in the fœtus, than the secretion of bile." Charles Bell, whose physiological opinions stand deservedly high, says: "that we must look upon the peculiarities of the circulation of the blood in the liver of the fœtus, as a provision against the secretion of stimulating bile." This doctrine I am totally at a loss to understand. Nature has formed an organ in the fœtus, which is to perform no function until after birth; but this organ, instead of lying dormant in the system, like the stomach, lungs, kidneys, &c. until birth shall call it into action, is more than five times larger comparatively, than the adult liver; has four-fifths of the blood destined to the fœtal economy circulating through its fabric, and all this wondrous organization, is merely to prevent it from performing a function, which is to answer no purpose until after birth: And from the time of birth, this organ begins to lessen, until it shall have acquired its due proportion in the

adult economy. Can any reflecting mind be satisfied with this doctrine? Impossible! especially when he reflects, that little stimulating bile is required, until the infant shall be weaned from the mother's breast; for while the infant is at the breast, the bile is but little stimulating. This circumstance induces the idea, that there is some analogy between the function of the fœtal liver and the mamma of the mother; the product of the former being less stimulating than the latter. And although the chemical analysis of fœtal bile and human milk is somewhat different, I know of no two fluids, which are more allied in their sensible properties.

It can hardly be expected, that the saliva and gastric juice of the infant immediately after birth, can act much on its aliment; the smallness and delicate texture of the stomach; its thin and tender parietes, and the minuteness of its blood vessels, prevent the opinion. The infant, then, after birth, receiving (though less refined) the product of the fœtal liver immediately from the mother, has less use for this organ; hence its immediate decrease after birth, and hence too the smallness of the fœtal spleen and vena portæ.

Richerand thinks that the liver performs the same function to the fœtus as the lungs do after birth: that is, to separate from the blood, its superabundant hydrogen and carbon; and attempts to account for the large size of this viscus by supposing, that these obnoxious principles are absorbed and retained in its fabric. Now, there is nothing in the colour or composition of this viscus which leads to such a conclusion. I have made repeated trials, but could never discover any more oileaginous or fatty matter in the fœtal, than in the adult liver: its structure being comparatively finer, more friable and delicate, perhaps may have led to this

opinion. It is as minutely vascular as the adult liver; by injecting it with water, and sticking the point of a needle into any part of it, this fluid will exude; therefore, if these excrementitious principles are absorbed and retained by this viscus, they must form a part of its organic structure. It seems to be remarkable, that a gland should have the function of separating from the blood a substance unfit for nourishment, and that this very substance should form the greatest part of its delicate fabric, which is as finely organized, as any other part of the fœtus. This doctrine must lead to the conclusion, that the greatest part of the liver, is made up of a carbo oileaginous mass; the contrary of this, however, is the fact. I know of no fact tending to prove this hypothesis, and its author has given us none; therefore it stands merely as a matter of opinion. Since finishing my experiments on the liver, and writing the above, I received a number of the Edinburgh Medical and Surgical Journal for January, 1815, from the Professor of Surgery of this College, in which was contained an ingenious treatise on the function of the fœtal liver, by James Bryce, F. R. S. E. fellow of the Royal College of Surgeons, Edinburgh.

He supposes, first—That the collapsed fœtal lungs completely fill the cavity of the thorax, the muscles of respiration being in a state of relaxation.

Secondly—That the great size of the fœtal liver, depends on the quantity of blood sent to it by the umbilical vein, and the distention of the biliary ducts by viscid bile. Admitting that no bile is poured into the intestines during the fœtal state, but the whole secreted becomes viscid, distends and remains in the bile ducts until birth. Now, the two great fœtal cavities being completely filled, and the respiratory muscles relaxed,

he thinks it impossible for the fœtus to inspire, unless some diminution takes place in the cavity of the abdomen, in order to admit the contraction of the diaphragm. The first contraction of the diaphragm, causing the first inspiration, presses on the liver, squeezes out this viscid bile, which is the meconium, by which the liver is lessened. Shortly after the umbilical cord is tied; or the blood ceases to circulate in it by the new action of respiration; the blood which before went to the liver, now goes to the lungs; the consequence is, an immediate decrease of the liver, which admits of a permanent dilatation of the lungs, thereby preventing, what he calls, "the loss of tone and debility."

Thirdly—That little or no blood goes to the lungs by the pulmonary artery.

This doctrine of the function of the fœtal liver, to say the least of it, is very ingenious, and at first sight appears to be plausible, but when submitted to the test of experiment and careful examination, it will be found hypothetical, and unsupported by good logic. The opinion, that all the bile secreted in the fœtal state remains in the bile ducts, there becomes inspissated meconium, distending not only the coats of the ducts, but enlarging the liver, I can confidently assert is without foundation. I have now before me a number of fœtal animals, taken from the uterus before birth; meconium is in the intestines. No appearance of viscid bile in the bile ducts: but these contain a little thin, pellucid, sweet bile, with little or no viscidity. Even if this viscid bile did exist in the bile ducts, it would not help the above doctrine; for the cavity of the abdomen would not be diminished by its expulsion into the intestines. The cavity of the chest does not seem to be completely filled by the lungs. I can blow into the

trachea of one of these animals before me, considerable air without distending either the abdomen or the chest, sufficient in my opinion, for what our author calls, 'a permanent dilatation of the lungs.' The diaphragm is natural, not at all saculated towards the chest, the only way that this cavity can be made smaller by the liver. Some blood does go to the lungs by the pulmonary artery, for blood is now running from the pulmonary veins of an animal that has never breathed. The great size of the liver of these animals before me, does not depend on the blood sent to it by the umbilical vein; or inspissated bile in the bile ducts, as the following experiment will show.

The contents of a fœtal liver being squeezed gently out, pipes were inserted into the vessels and stopped perfectly tight; the liver was then immersed into a vessel of water, and the height of the water accurately marked; it was then taken out and injected as hard as its fabric would bear, and again immersed in the same vessel of water; the rise of the water was too small to be mentioned, or hardly perceptible, it could not have exceeded thirty grains in a liver that weighed seventeen drams: therefore the fœtal liver cannot be much diminished in volume, at the moment when the change of circulation takes place. The structure of the liver itself, without the aid of experiment, forbids the opinion. A section of the liver shows all its vessels with open mouths, the outer coats of which are firmly attached to its parenchyma.

Is a diminution of the size of the abdomen or liver necessary to the longitudinal enlargement of the thorax? Does not the abdomen readily and easily adapt itself to its contents? A man that can eat five pounds at a meal, feels little or no inconvenience from a full

or an empty stomach. Our female aborigines, know nothing of bandaging after parturition, and their two hours confinement, shows what little inconvenience the abdomen suffers, from its great and sudden diminution.

Nor is the pregnant woman much incommoded by the large size of the uterus, in the latter months of gestation. Let us admit for a moment, that the liver diminished greatly at the moment of respiration; the thorax of a human fœtus would be little enlarged by it: for the middle of the diaphragm is tightly tied upward by the mediastinum; it is difficult to move it either upward or downward without laceration; the enlargement, therefore, would consist only of the difference between its natural curve, and a straight line drawn from its middle, to its insertion into the margin of the ribs. It is impossible for the thorax to be enlarged in any other way, by the reduction of the liver; for suppose it tends to push the ribs more at a right angle with the spine, by bringing the ribs to their natural situation, the thorax will be lessened in capacity. If the liver were like the lungs, and the diaphragm saculated towards the chest, some probability might be attached to this hypothesis; but as the liver may be considered a solid gland, not susceptible of dilatation or contraction, by the fluid circulating through it, I see not a shadow of probability, or even plausibility to this hypothesis; but must look upon it, as the offspring of a fruitful imagination, unsupported by experiment, fact, or good logic.

The facts which dictated to me the theory of the fœtal liver here inculcated are the following. First. Its great comparative volume: composing one-fifth part of the whole fœtus. Second—Four-fifths of the blood des-

tined to nourish the fœtus, circulates through its fabric. Third—The sensible properties of the bile: being bland, sweet and pellucid, greatly resembling the first milk secreted by the mother. Fourth—The quantity of bile secreted in a given time: which is at least ten ounces in twenty-four hours in an animal weighing less than three pounds.

Fifth—Proof of its being absorbed by the lacteals.

Sixth—Proof that the bile is pouring into the intestines in a small but perpetual streamlet.

Previous to my departure from Europe in the latter part of the year 1815, for the United States, I procured a bitch, that would pup on the passage for the purpose of experiment. The duodenum of one of these pups was open, while the circulation was going on in the funis umbilicalis, and the bile was seen flowing into it in a small but perpetual streamlet, adhering to the parietes of the intestine. I judged by the quantity poured in, in five minutes, that at least half an ounce must have been secreted in an hour, in this small animal, weighing less than three pounds. The absorbents of the misentery could be seen distended with white chyle, and by the aid of a microscope, its motion was observable in these vessels. The meconium of this pup weighed fourteen drams, by which it appears, that in four hours, more bile is poured into the intestines of a matured fœtus, than would compose the whole of the meconium.

I am aware, that the above experiment does not conclusively prove the exact quantity of fœtal bile secreted in a given time; neither is it easy to say how this can be proved. But if a rational man, sees a fluid running into a receiver, in a small, equable and perpetual streamlet, a given time, and compares this with other artificial

streamlets, &c. it seems to be impossible that he should be much deceived, respecting the quantity secreted. But waving all argument of this kind, and admitting that only six ounces of bile, is poured into the intestines of a matured human fœtus in twenty-four hours, and allowing the meconium to be four ounces, which has been gathering during the whole time of gestation, I ask, what becomes of all this surplus bile? No other answer can be given, since the doctrine of transudation during life is done away, than that it must be absorbed. Well, if absorbed, and carried into the circulation, what purpose does it answer? Is this the course of obnoxious carbon, or other excrementitious particles? If this is the passage of a fluid unfit for nourishment in the fœtus, why not so after birth, or in the adult body? I know of no good reason why it should not. It is a common idea, since the explosion of the theory, that the fœtus was nourished by the liquor amnii, by absorption, that the absorbents, and especially the lacteals, lay pretty much dormant in the fœtal system. Bloomenbach expresses this as his opinion. But I should like to know how these physiologists account for the growth, nourishment, and constant changes going forward in the fœtus, without these vessels. That this opinion is founded in error, is proved, not only by ocular demonstration, but by dissection; for the thoracic duct of the fœtus is comparatively larger than that of the adult. In the anatomical museum attached to the medical university in Dublin, are many preparations establishing this fact. And I am convinced of it myself, by the dissection of a fœtal dog. While observing this circumstance in Dublin, previous to imbibing my present opinions, the idea occurred to me, that the fœtus must

either be nourished by the liquor amnii, or that something must be absorbed from the intestinal canal.

Many of the best physiologists allow that the fœtal liver secretes sweet bile, and pours it into the duodenum. The quantity secreted in a given time, they have not told us. But I believe that every one will readily allow, that the quantity of fluid secreted by this great organ, during the long period of gestation, will greatly exceed the small quantity of meconium found in the intestines. Now, the mystery is, what becomes of this excess, since we know of no other way that it can get out of the intestines, than by absorption. Authors being completely at a loss with respect to the solution of this question, have here let the matter rest. And no one before our countryman Rush, has told the world that the fœtal liver secretes chyle, which is absorbed by the lacteals; carried into the circulation; there becomes blood; which is destined to the growth and nourishment of the fœtus. It is to be regretted that this philosopher did not support his theory by direct experiment. But he having formed it from his minute acquaintance with the laws governing the animal economy, it gives us a more sublime idea of his penetrating genius. I do not mean to say that all the product of the fœtal liver is nutritious; a very small portion of it is excrementitious, and this latter portion I believe, forms the meconium.

CONCERNING THE LIVER AFTER BIRTH, AND ITS SECRETION.

Physiologists are generally agreed in this—that the liver after birth secretes cystic bile. For of all the physiological and chemical writers that I have examined, not one can be found, who has made any experiments on the hepatic bile, with a view to ascertain its chemical analysis ; or pointed out any but its sensible properties ; nor but few who have mentioned the striking differences existing between them. Haller says, that the cystic bile is bitter, and the hepatic sweet, but draws all his conclusions with respect to its use from the former.

Boerhaave says, that bile is properly of two kinds, cystic and hepatic, the former is thicker, darker coloured and more bitter than the latter, which is sweeter, thin and pellucid, and suggests the propriety of calling the hepatic bile, lympha hepatica. But notwithstanding this, the bitter acrid, viscid bile of the gall-bladder, forms the basis of all his reasoning.

Doctor Saunders of London, who has written a popular work on the liver, made his experiments, and drew all his conclusions from the more concentrated, acrid, green and bitter cystic bile of the ox, between which, and the human hepatic bile there is no analogy. The result of his inquiries led him, of course, to the conclusion, that the bile was wholly excrementitious.

If Doctor Saunders had measured the difference between the quantity of bile secreted in a given time, and the excrementitious discharge, perhaps, it would have capsized his theory.

Fourcroy also confined his experiments to the cystic ox bile.

It is to Thenard alone, and that very recently, that the world is indebted, for the first analysis of human cystic bile. But in vain do we look for a chemical analysis of hepatic bile, notwithstanding the obvious difference existing between them.

Here then are two sources of error.

First—In identifying human cystic bile with the cystic bile of the ox.

Second—In identifying cystic and hepatic bile of the same animal, and supposing them to answer one, and the same purpose in the animal economy, when there is but little analogy between them. These errors, I consider, as having laid the foundation of all the physiological controversies, which have existed, respecting this too much degraded fluid. The more ancient physicians seem to have been better acquainted with the difference existing between cystic and hepatic bile, than those of the present day. Revenherst obtained the hepatic bile by an experiment made on a living dog, and says, that it is dilute, not much bitter, very different from the bile in the gall bladder. Many anatomists before Boerhaave, believed that the gall bladder secreted its own bile; so great in their opinions was the difference between cystic and hepatic bile.

As the theory of Doctor Saunders respecting the function of the liver, and uses of the bile, is perhaps as popular as any other now extant, and he considering the bile to be altogether excrementitious, my first experiments

were directed, either to approve or disprove this opinion. If the bile be wholly excrementitious, it follows, that it must be all evacuated, together with a portion of the aliment taken in by the mouth, per annum. Now, if it can be proved, that the quantity of bile alone poured into the alimentary canal, exceeds the fœcal discharge, this opinion, I think, must be erroneous. How far I have succeeded in proving this, the following experiments will show.

Experiment, to ascertain the quantity of bile secreted in a given time. The duodenum of a living dog was opened, and a tube with a small bladder appended to one end of it, was inserted into the ductus communis choledochus; in two hours the cyst was examined, and four drams of bile was found in it. This dog weighed thirty pounds: allowing the human adult subject to weigh one hundred and fifty pounds, and make weight the ratio of our calculations, thirty ounces of fluid will be poured into the intestines of a small sized man in twenty-four hours. But if the difference of weight of the liver be made the ratio, twenty-nine ounces and two drams will be the quantity. Making the calibre of the common duct the ratio, thirty-six ounces and four drams. Making vascularity the ratio, ascertained by the quantity of fluid that each viscus will contain, twenty-eight ounces and six drams will be the quantity. As these varied, comparative calculations so nearly agree, I think it may fairly be concluded, that twenty-nine ounces of bile is discharged from the bile ducts of a common sized man in twenty-four hours. Considering this experiment important, it was repeated with nearly the same result. The quantity of excrementitious discharge from the intestinal canal was ascertained by the following experiment. The fœces of a common sized healthy labouring man, aged twenty-six years, whose appetite was unusually good, were care-

fully weighed after each evacuation for a week together; the seventh part of which weighed five ounces, two drams. The same experiment was made on another man twenty-seven years old, equally healthy, but accustomed to a studious and sedentary life, and perhaps finer food, and the daily quantity was three ounces, five drams. By these experiments, it appears, that twenty-nine ounces of bile are poured into the intestinal canal in twenty-four hours, and but five ounces discharged per annum in the same time. Allowing three ounces of the daily fœcal discharge to be formed from the aliment taken in by the mouth, (which will be small, considering the quantity and kind of aliment,) there will be a daily excess of bile in the intestines, over the fœcal discharge per annum, of twenty-seven ounces. If these experiments be correct, do they not conclusively prove that the bile is not wholly excrementitious? And do they not as clearly prove that the greatest quantity of the bile is absorbed by the lacteals?

Most of the physiologists who have written within the last century, teach, that one important use of the bile is, to convert chyme into chyle by combining with it, and precipitating its fœcal particles. My next experiments were directed to corroborate or disprove this opinion. If it can be proved that chyme or chyle is absorbed without the aid of bile and vice versa, then a union of them is unnecessary to insure their absorption, or to constitute what is understood by chyle.

Experiment, to ascertain whether chyme is absorbed without the aid of bile. A dog was kept starving twenty-four hours; a ligature was then passed round the common duct; the next day it eat animal food greedily; two hours after this the absorbents of the mesentery were examined, and was found distended with chyle. The

chyme was seen to pass along the whole length of the jejunum; the intestine occasionally contracting upon it, and forcing it onward, by which the most fluid or chylous portion seemed to be pressed out, and adhere to the parietes of the intestine; as it passed along, the chyme grew more viscid, and when arrived at the lower part of the ileon, it pretty much lost its fluidity. While observing the operations of nature in the intestines of this living animal, I conceived the opinion, that as the absorbents had the power of abstracting the nutritious particles of the chyme as it passed along, that if a piece of solid aliment were in this intestine, it would contract upon it, so as that the absorbents could abstract its nutritious juices without previous solution. I accordingly made the experiment by introducing a piece of beef steak, weighing two drams, into the jejunum of a cat. An oblique incision, half an inch long, was made in the gut, which was afterwards closed with three sutures, and the ligatures cut close down to the knot. The animal was kept confined forty-eight hours, so as to examine the fœces, but no traces of indigested beef could be discovered in them. This animal has got entirely well, and is now (two weeks after the experiment) running about the gardens.

This experiment I deem important both to physiology and surgery. To the former it proves, that digestion can be carried on in the intestines without the aid of mastication, saliva, gastric juice, or bile, that these are merely auxiliaries, or solvents, and that digestion essentially consists in the power of the absorbents to abstract the nutritious, and reject the excrementitious parts of the aliment. I have also seen by experiment, that in half an hour after food was taken into the stomach of an animal, the absorbents of the stomach were distended with white chyle. The opinion, therefore, that perfect

chyle is formed in the intestines previous to absorption, is chimerical. Will not these phenomena also explain why the properties of chyle are so little understood? Because it never could be obtained in a pure state. The mixture of bile, therefore, with chyme in the duodenum, does not constitute chyle. The only experiments which I know to have been made to ascertain the correctness of this opinion, are those made by Doctor Saunders, which led him to reject the doctrine. If this union is necessary, how does chylification go on in jaundiced persons? If the common duct of a living animal be obstructed, chyle is absorbed but of too imperfect a nature for complete sanguification, and the animal in a short time dies from this cause. But if the cystic duct only is obstructed, which is generally the case in jaundice, chylification is perfect, but the bowels become torpid, and impacted with indurated fœces, which may be relieved by hydrogogue cathartics. If the cause, however be not removed, unnatural irritation will ultimately produce death; but it will be brought on slowly, and the patient may live a long time. Another fact, in proof of the theory that chyle is not formed by the mixture of chyme and bile, is, the alone absorption of bile, proved by the following experiment:

A dog, having been previously well fed, was kept starving twenty-four hours; the abdomen was then opened; the stomach was perfectly empty, but the lacteals below the perforation of the common duct into the duodenum, were filled with white chyle.

The next use of the bile, of which we shall speak, is one, I believe, universally admitted, namely: its liquefying, stimulating and cathartic effect, by which the peristaltic motion of the intestines is excited, and the fœces eliminated from the intestines. By the ready

passage of chyme along the jejunum, without bile, I received the impression that bile may assist only by its liquefying effect, but of the incorrectness of this opinion, I was convinced by the following experiments on myself. A dram of human cystic bile proved gently cathartic, and a dram of that of the ox much more so. The cystic duct of a living animal was tied; costiveness was the consequence, although the aliment was exclusively fluid. These experiments, I think, are sufficient to substantiate its cathartic effect; but to its antisceptic properties as particularly taught by Doctor Saunders, I cannot subscribe. Now, this author admits that hepatic bile becomes cystic by stagnation in a passive receptacle. Such being the fact, can this change be induced in any other way than by putrefaction? Admitting that a part of its water is absorbed, and that a little mucus is mixed with it, (of which he has no evidence) can this account for its acrid, pungent bitterness, not before existing? Why are the fœces of jaundiced persons almost void of smell? they remain much longer in the intestines, and when evacuated from it have no tendency to putrefaction. It is a known fact, that the more yellow or dark coloured the fœces (which colour is imparted to them by the cystic bile) the more putrid they are. When I tied the common duct of the animal to ascertain the alone absorption of chyme, three days after, the fœces remaining in this animal, were indurated, earthy, nearly without odour, and when removed from the intestines, had no more tendency to putrify than an earth or a metal.

Physiologists have long sought for that peculiar principle in the blood of the vena portæ, which fitted it better for the secretion of bile than artereal blood. Had they known that the liver secreted chyle as well as bile,

this property would have been manifest; for it is very evident, that a part only of the arterial blood is fitted for nutrition; after these particles are given off, it is received into veins, no longer capable of performing the office of nutrition, although, still containing nearly an equal quantity of fibrin, gelatine, lymph and albumen, with arterial blood, and more carbon. In order, therefore, to become arterial blood again, it is as necessary that the four first should be farther elaborated, as it is for the latter to be lessened. For the whole mass of venous blood is unfit for nourishment. Accordingly the lungs abstract its carbon, and the liver elaborates the remainder, and renders it fit for complete sanguification.

The excess of carbon in venous blood, however it may be obtained, I think answers an important purpose. Every chemist will allow that carbon is a very powerful antiseptic; and the proneness of arterial blood to putrefaction. Considering the slow and equable motion of venous blood, and the length of time that it remains in the veins, such an antiseptic seems to be indispensable. Will not carbon alone explain the reason why venous blood is less putresent than arterial? It may be asked why the blood of the vena portæ has the preference of other venous blood, in the secretion of bile or chyle? This question, with respect to that portion of the vena portæ formed by the spleenic veins, is answered in our observations on the spleen. Respecting the other part, it may be answered, that although no difference can be detected between this and other venous blood, it is probable, that the mesenteric arteries, by ramifying over the immense surface of the intestinal canal, may be more completely deprived of its nutritious particles, and therefore, require the first elabora-

tion. This will be obvious, with respect to the stomach and pancreas after secreting their respective fluids. And although the doctrine of absorption by veins has at present but few advocates, yet there is no fact to prove the contrary, but that the mesenteric veins may absorb such particles of the chyle as are too coarse to be received by the absorbents. Some distinguished physiologists were of this opinion. But whether either of the above opinions be correct or not, it is known that the liver is already the largest gland of the human body, and that it receives as much blood as it can elaborate: more blood would require a larger viscus, which would be unnecessary, because the elaboration of the present quantity completely serves the purpose for which it is designed. Then why not receive it from the mesenteric veins as well as others. By considering the quantity of blood going to the liver by the vena portæ (which is great) we can easily discover how the whole venous blood becomes ultimately elaborated. Suppose a family to have a cistern containing ten measures of water, one of which being filtered, is sufficient to serve them a day; if every day one measure is filtered, and one unfiltered measure added to the cistern, the family would be supplied, and the cistern kept full of sweet water. A larger filtering-stone would be unnecessary to this family.

I pass on now, to say something of the sensible and chemical differences existing between cystic and hepatic bile of the same animal. Both kinds of bile having been procured from the same dogs, the cystic was of a greenish or brownish yellow colour, very viscid, not near so bitter as ox or fish bile, tenacious so that it would not drop from a phial, ropy, slightly acrid and nauseous. While the hepatic bile was of a white slightly yellowish colour, little viscid, slightly bitter, little or no tenacity, of a sweet

mawkish and rather disagreeable taste, very analogous to human milk with a little yellow colouring matter in it.—By the aid of heat and chemical reagents I endeavoured to ascertain the properties of this cystic bile in the following way. On four ounces of bile was poured an equal quantity of alkohol, after standing forty-eight hours it was filtered through paper, a gluten was separated, which, after being dry, weighed five grains; this substance resembling albumen, or animal glue was without smell and nearly without taste. There adhered to the edges of the paper a brownish yellow matter, in small flakes, with a resinous matter adhering to it, having a nauseous and acrid taste; this substance was soluble in water and in alkohol; the latter abstracts the resin adhering to it and leaves it without bitterness; in this state it has an unctuous feel and a nauseous taste, something like tartarized antimony, insoluble in acids, and extremely soluble in alkalis. To the brown, yellowish, transparent liquor that passed the filter, after being evaporated nearly to the consistence of honey, distilled water was added, no precipitate could be discovered after standing twenty-four hours—the filter separated nothing. It was then again submitted to heat; very little odour and none of the properties of bile could be discovered in the vapour that passed over. Although heat was very moderately applied, no precipitate took place in any degree of concentration, although the liquor was repeatedly cooled. When the water was driven off, the residuum was of a brown or yellowish black colour, as hard, and very much resembling burgundy pitch, semi-transparent, and a glossy fracture, having not so much odour and antimonial taste, but in other respects retaining all the sensible properties of recent bile in a concentrated state.

This substance, composing one thirtieth part of the whole bile, was extremely and perfectly soluble in water, less quickly in alkohol, and still less in alkalis; insoluble in acids and in oils; precipitable from its solutions by acetate of lead, and more quickly and copiously by water of acetated litharge, in soft white flakes: this precipitate, although a new compound, changed in colour and rendered insoluble; still retained the essential properties of bile. If there be a resin in bile, analogous to a vegetable resin, insoluble in water and dissolved by a peculiar substance, not soda, which renders it soluble in water, &c. I could not obtain it in the cystic bile of the dog.

Equal parts of the resinous base of bile were dissolved in equal portions of water.

To the first syrup of violets was added, which, after standing some time, was turned green; indicating the presence of an uncombined alkali, no precipitate took place with the nitro muriate of platina, by which I infer that the alkali is soda.

To a second was added oxalic acid, a slight precipitate indicated the presence of lime.

To a third muriate of barytes was added; a pretty copious precipitate ensued, soluble in dilute muriatic acid, without effervescence. I therefore consider it to be phosphate of barytes, because sulphate of barytes is insoluble in dilute muriatic acid.

To a fourth was added tincture of galls, then prussiate of potash, but no trace of iron was indicated by either.

To a fifth was added nitrate of silver, a very copious white flaky precipitate took place. As the phosphoric acid exists in bile combined with soda or lime, and as the nitric acid of the silver has a stronger affinity for the soda than for silver, phosphate of silver might have been precipitated by this test. But by fusing this precipitate

with a gentle heat, luna cornea was formed, which indicated the presence of a muriate. No other precipitate could be obtained.

On two ounces of hepatic bile, obtained from dogs, was poured as much concentrated alcohol; after standing forty-eight hours, a gelatinous substance was separated by the filter, which after desiccation weighed eight grains: small fibrous flocculi formed on the filter: very little yellow, and no resinous matter were observed. The liquor that passed the filter was of a white brownish colour, of a sweet, and at the same time slightly bitter taste. This, being nearly four ounces, was evaporated by a gentle heat to about one ounce. At this time, in the evening, being called away, the vessel was taken from the lamp. In the morning there adhered to the bottom and sides of the vessel white, shining, chrystalline needles, of a sweet, honey-like taste, without smell. When viewed with a microscope, they appeared to be parallelopepods, terminating in six-sided prisms. I could account for this sweet salt in no other way than that it must have been insoluble in alcohol, but required a greater degree of concentration, and a longer time for its precipitation than albumen or yellow matter, or that these substances were incompatible with its precipitation. On the decanted liquor distilled water was poured, but no precipitate ensued. It was then evaporated to dryness. The residuum was of a greenish, or yellowish white colour, but in other respects resembling exactly the resinous base of cystic bile. The quantity however was much smaller, being only the one hundred and fiftieth part of the whole bile, whereas that of the cystic bile, was a thirtieth part. This residuum was treated with the various tests, to ascertain its salts. Phosphates of soda and lime were

discoverable, and muriate of soda. No uncombined soda or iron were indicated.

The above analysis, although imperfect, owing to the great difficulty of procuring the cystic bile of dogs in sufficient quantities for chemical experiment, (for the contents of the gall-bladders of seven large dogs only afforded four ounces) I think, will sufficiently point out the striking differences existing between cystic and hepatic bile of the same animal; the only design of this analysis.

CONCERNING THE GALL-BLADDER AND NATURE OF ITS CONTENTS.

Most authors, popular at the present day, consider the gall-bladder as a mere receptacle of hepatic bile; holding in reserve a sufficient store of this fluid against the time of need; filling by regurgitation, when from any cause the passage of bile into the duodenum is obstructed, or when the stimulus of chyme in the duodenum is wanted, to draw forth the biliary secretion.

Some suppose it to be a mere passive receptacle, discharged only by surrounding viscera pressing upon it, making it a mere organ of chance, influenced by every cause tending to press upon it.

Its viscid, resinous and bitter properties are accounted for by stagnation, and absorption of its aqueous part.

Doctor Rush believes it to serve as a receptacle for redundant bile in the bile ducts, from what cause soever this may happen, thereby preventing its return upon the liver. Experiment has taught me to differ from all these opinions. If the gall bladder was merely a receptacle of the hepatic bile, receiving this fluid when its passage into the duodenum is obstructed, and pouring it out again when such obstruction is removed, why does it not sometimes contain hepatic bile? Why does it always contain that peculiar fluid, cystic bile? Why not sometimes found empty, as in diminished secretions of bile? Why not surcharged in redundant secretions or obstructions,

with the same kind of bile contained in the hepatic duct? Suppose the gall-bladder to be empty, and that a gall stone should obstruct the passage of bile into the duodenum, at the place where the common duct perforates the intestine. Now, if this organ was a receptacle only, beyond all contradiction it would now be distended with hepatic bile. But this is not the case, as repeated experiment will show. The mere fact, if duly considered, that the gall-bladder always, under every circumstance and condition of the biliary organs, contains a peculiar fluid, differing as much from hepatic bile as chyle does from blood, is sufficient proof in my mind that this organ is not a receptacle only, but has its own function to perform. Another conspicuous and important fact rebutting this opinion of regurgitation is, the valvular structure of the cystic duct, and its peculiar contorted situation, and termination, with respect to the hepatic duct; which, I cannot look upon in any other light, than as a provision against the regurgitation of hepatic bile. A probe soon after death cannot perforate it without breaking down its valvular structure; nor can water be injected into it from the common duct during life. After death this can be accomplished, though sometimes with difficulty: so also can the veins and lymphatics be injected, contrary to the natural course of their fluids after death, owing to the flaccid state of their valves. But independent of these facts, will this doctrine bear candid criticism? It is said by its supporters that the bile does not flow in a perpetual and equable current into the duodenum, but is influenced altogether by the stimulus of chyme. When this stimulus is absent, the bile, instead of passing onward into the duodenum, in which it is not wanted, regurgitates into the gall-bladder; which serves as a

reservoir, until such time as the stimulus of chyme shall call it forth. Now, every one who has examined the biliary organs will allow, that the secretion of bile goes on while any blood remains in the system; and instances have been known of men to go without food a week, in which time the liver must secrete at least twelve pounds of bile. What a wondrous gall-bladder, then, here must be to contain this quantity of fluid! equal at least to the stomach of a bullock. Again, let us reason upon this economy in healthy men, who receive regular food. Chyme is not always passing the duodenum: at these intervals the gall-bladder has a chance to fill. Suppose now, the gall-bladder to be filled with cystic bile; the first meal or chyme produced by it, would draw a portion of it out, and let in a portion of hepatic bile, every meal after would do the same. This process, alternately continued, would at last exhaust the gall-bladder of every drop of cystic bile. This reasoning may be farther illustrated, by supposing a vessel to contain a gallon of wine; every hour a pint of this wine is drawn out and replaced by the same quantity of water; not many hours would elapse, before unmixed water would be drawn off. Therefore hepatic bile alone ought to be found in the gall-bladders of all healthy regular feeders. But according to this doctrine I cannot tell, under what circumstances cystic bile ought always to be found. It is impossible for a moment to suppose, that hepatic bile becomes cystic by stagnation, &c. the short time that it remains in the gall-bladder, in those who live regularly. I believe that no absorption takes place from the gall-bladder, unless it is obstructed or greatly distended. With equal propriety we may say, that the aqueous part of the urine is absorbed when that organ is not distended. Does

not the circumstance that the gall-bladder always contains its peculiar fluid, except in cases of obstruction or distention, prove that no absorption goes on from it in its natural state? In fine, the idea that stagnation and natural absorption will account for the difference of cystic and hepatic bile, is to me equally absurd, as that entertained formerly, by those who supposed the gall-bladder to secrete its own bile from its own blood.

But reasoning without the aid of experiment, that sole and never-failing source of truth, is at best mere plausibility or shadow of truth; but experiment instituted only with a view to establish a preconceived opinion, is not much better than analogical reasoning. I now proceed to offer my opinion respecting the function of the gall-bladder, as dictated by experiment, and in this I hope, not to be accused of possessing preconceived opinions.

The gall-bladder, in my opinion, has the function of secreting or separating from the hepatic bile, its bitter, resinous, yellow, viscid, or excrementitious principles, holding them in reserve, till the stimulus of chyme in the duodenum, calls for their stimulating and cathartic assistance.

The following facts led me to this belief. First—The gall-bladder fills slowly and gradually, and not by regurgitation. The abdomen of a living dog was opened and the contents of the gall-bladder squeezed gently out, a ligature was now passed round the common duct close to the duodenum. Six hours after the parts were examined, the common duct was greatly distended with bile, but the gall-bladder was only about one third full, and its contents had the same properties as the cystic bile of a long starving animal. Thinking that

some obstruction might possibly happen to the cystic duct, the experiment was repeated, but with the same result. Admitting these experiments to be correct, are they not conclusive proof against the regurgitation of hepatic bile? The conclusion appeared to me so palpable, that I was obliged to abandon the theories hitherto taught respecting the function of the gall-bladder, more especially that of Dr. Rush, for if the gall-bladder served merely as a waste gate, or reservoir to the liver, to shield it from harm in case of obstruction, or redundant secretion of bile; it certainly would have been now distended.

Secondly—It always contains the same kind of bile, which is very different from hepatic bile. This fact, I satisfactorily proved, not only in the last experiment, but by tasting both kinds of bile, of different animals, under different circumstances.

Thirdly—By the valvular structure and very acute termination of the cystic into the hepatic duct. Every anatomist knows this fact.

Fourthly—The inability to inject water into the gall bladder, from the common duct in a living animal.

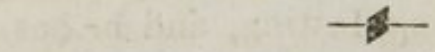
Fifthly—The difficulty of introducing a probe into the gall bladder from the common duct after death.

Sixthly—When there is no chyme in the duodenum, the gall bladder gradually fills and enlarges. At the same time hepatic bile is profusely poured into the intestines. A dog was kept starving three days; the abdomen was then opened, and the gall bladder was enlarged and distended with bile. The duodenum was then opened, and hepatic bile, having only a slight bitterness was constantly flowing into it. By observing the flow of hepatic bile into the duodenum under so many circumstances and conditions of an animal body, I feel myself warranted in

saying that the flow of hepatic bile into the duodenum, is as constant as the circulation of the blood: that chyme never presses on the duodenal canal of the common duct so as to impede its flow; and that the secretion of hepatic bile is not influenced by the presence of chyme in the duodenum. Boerhaave says, that the gall bladder is always distended with bile by long fasting, and he has seen this surprisingly illustrated in swine that have been kept several days, designedly, without food, before being slain. These facts I think sufficient to prove that the stimulus of chyme, draws forth only cystic bile. All physiologists agree that it is difficult to explain the manner in which regurgitation of hepatic bile into the gall bladder takes place; and I could mention may profound anatomists who disprove it altogether, but have not room here. Neither have I a disposition to make quotations, and a recapitulation of the doctrines of others, with such ideas and opinions as may arise from them, answer in place of experimental truths. An ingenious and fruitful mind will in a few hours deduce a plausible hypothesis, from the knowledge and opinions of others, which experiment will as soon overturn. Doctor Maclurgh of Virginia wrote a long and very ingenious treatise on the bile, and founded his whole theory on the putrescent nature of the blood of the vena portæ. Had he made only one experiment he would have found the blood of the vena portæ less putrescent than other blood, and his whole ingenious plan would have been overthrown.

Such productions, although exalting mental ingenuity, ought to be looked upon as evils, tending more to confuse science, and fill the world with empty opinions than to advance it. It is to a minute knowledge of anatomy, and candid experiment alone, that we must look, for the advancement of the sublime science of physiology.

CONCERNING THE PANCREAS AND ITS SECRETION.



The pancreas has been considered by most physiologists, as analogous to the salivary glands both in structure and secretion. Although the pancreatic fluid has been the subject of much controversy, especially between Sylvius, his followers, and opposers, yet it is a fact, that little or nothing is known of it. De Graaf and Nuck say, they have gathered it in a dog weighing ten pounds, at the rate of from two to three drams, to an ounce in an hour. Ruysch and Fordyce have also gathered it, but have not told us the quantity secreted in a given time; indeed, it seems that the quantity obtained by Fordyce was so small, that he could not analyse it; yet, however strange as it may appear, he has told us all that is now known of the properties of this fluid, which are, that it is colourless and slightly saline. Muriate of soda was obtained by evaporation, and was also indicated by the nitrate of silver. This is the extent of our knowledge respecting this fluid.

It is very strange, if the pancreatic fluid is to be obtained as easily and abundantly as is represented by De Graaf, &c. that it has not been an object of animal chemistry. Brunner and Swalve say that it is insipid, and Sylvius declared it to be acid. Boerhaave says, that it is neither saltish, acid, nor alkaline. Haller says, that it is thin, watry and insipid, neither acrid, nor alkaline, the quantity secreted is uncertain, but thinks it great

from analogy. Richerand says that nothing particular is known of the pancreatic juice, and that we are ignorant of its quantity. Blumenbach says that it is procured with difficulty, and says nothing more of its properties, than their resemblance to saliva. Upon the whole, the present state of our knowledge with respect to the pancreatic secretion, may be summed up in this short sentence : " the nature and action of the pancreatic fluid, are analogous to those of saliva." From observing all these contradictory and jarring opinions, would not an honest man be inclined to doubt whether much or any pure pancreatic fluid has ever been obtained ? Although the authority of De Graaf and Nuck, are respectable, but if it be considered that others, equally respectable, have been unsuccessful in their attempts to obtain it, the undefined manner in which they express the result of their experiments; the quantity secreted in an hour, in an animal weighing ten pounds, and the object which they had in view ; I am forced to doubt it. They said, that they obtained an ounce in an hour from an animal weighing ten pounds; at that rate the pancreas of a common sized man ought to secrete thirty pounds in twenty-four hours. Can any person believe this ? My experiments on that organ may perhaps point out some of its peculiarities, and may explain the reason why the properties of this fluid are still enveloped in darkness. My own opinion is, that the pancreatic fluid serves to dilute both the bile and chyme, and that the stimulus of one of these, in the duodenum, is necessary to excite its secretion.

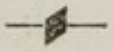
This opinion is founded on the following facts. First, The impossibility to obtain it by a tube inserted into its duct.

It is unnecessary to detail the various experiments that I made to obtain the pancreatic fluid; they were made by inserting a tube with a cyst appended to one end of it into the pancreatic ducts of living dogs and cats; but no fluid in either could be obtained. That the tube was fairly into the pancreatic duct of each of these animals, there can be no doubt; for the pancreatic duct in all the quadrupeds which I have examined, opens into the duodenum from one to two inches below the common duct, by a large and protuberant mouth; so that a tube is as easily inserted into this as into the common duct. If no fluid can be obtained from the pancreas by a tube inserted into its duct, and none can be seen flowing into an empty duodenum, how can the nature and use of the pancreatic fluid be known? When chyme was in the duodenum, so soon as I removed it, or kept it stationary, I could discover no secretion; the same was the case with bile; therefore I infer that it dilutes both chyme and bile; for the little that I could squeeze out of the duct appeared to be insipid and very dilute. By the termination of the pancreatic into the common duct in man, there is more reason to believe that it acts more essentially on the bile, being poured directly upon it. But even in man, the pancreatic duct does not always terminate into the common bile duct. I have generally found two ducts, one terminating below the common duct, and the other into it; and I have in two instances found the whole pancreatic duct terminate into the duodenum. The circumstance that the pancreatic duct terminates into the duodenum below the common duct, I think favours the idea that the juice dilutes chyme as well as bile; for the bile, after it has flowed through its duct, seems to adhere to the parietes of the intestine, and it is not easy to explain how the pancreatic fluid mixes with it, after passing over two inches

of intestine, before it arrives opposite to the mouth of the pancreatic duct.

Secondly. The structure of the pancreas and nature of its fluid, according to the general doctrine, seem to be analogous to the salivary glands and saliva.

OBSERVATIONS ON THE FUNCTION OF THE SPLEEN.



It was not my original intention to have said any thing on the function of this degraded organ, in this dissertation. But while experimenting on living animals for other purposes, and seeing the large quantity of blood which this viscus poured into the liver, a thought struck me to remove it, and see how it would effect the biliary secretion. The result of this experiment at once dictated to me the function of this organ, which was soon strengthened by subsequent experiments. Although many uses have been assigned to the spleen, they are all so unsatisfactory, that the function of this organ is still considered as being unknown: so much so, that the able professor of anatomy and physiology of this college, deemed it as time mispent to relate them. Instead therefore of relating the various and fanciful theories respecting the function of this organ, I shall preface my remarks with the following observations.

Most of the organs of an animal body seem to possess a peculiar, inherent, discriminating power, not under the control of the will, according to the different offices which they are designed by nature to perform. This faculty is peculiarly exemplified in the senses, but other organs seem also to possess it. The pylorus, like a watchman, admits the digested, and rejects the indi-

gested food. The kidney seems to have the faculty of selecting such blood as contain more abundantly the constituents of the urine; for without this faculty, it seems to me impossible to account for the quick and copious secretion of urine, immediately after drinking acidulous or diuretic liquors; unless there be a direct communication from the stomach to the kidney. The quantity of fluid secreted by the salivary glands during mastication, compared with that during sleep, or fasting, is so great, that, considering the size of the glands, it seems impossible to account for it on any other principle.

The same reasoning will apply to the pancreas, the lacteals, and various other parts of an animal body. But this faculty seems to be imparted to the spleen in a singular and striking manner. For I consider the spleen, as having the function to select imperfect blood from the arterial system, more abundantly loaded with the constituents of the green and bitter principles contained in cystic bile. How far I am warranted in assigning this function to the spleen, the following experiments will show.

The spleen of a living dog was carefully removed with little or no hæmorrhage: the contents of the gall-bladder was next squeezed out; the duodenum was then opened, and a tube inserted into the common duct; four hours after it was examined, and four drams of fluid found in it, having the following properties: sweet, slightly acrid animal-like taste, little or no viscidity; of a yellowish white colour; small yellowish white flakes floating on its surface, resembling milky water a little coloured with yellow matter, without odour. In fine, it appeared to be as different from other bile as arterial

is from venous blood; there seemed to be a manifest imperfection in its formation.

The gall-bladder had received in these four hours about a scruple. This bile compared with that squeezed out, had very different properties; the former having little or no bitterness, less odorous, equally viscid, and more opaque than the latter; the former being of a brownish opaque colour; the latter of a yellowish and more pellucid; the former having yellowish flakes floating on its surface; the latter none. But the greatest manifest difference was that of bitterness, being hardly sensible in the former. The weight of this dog was nineteen pounds; now, according to our former calculations, had the spleen of this animal been in, it should have secreted in four hours, five drams and two scruples; but instead of this, only four drams were secreted. This difference of secretion together with the remarkable sensible differences existing between the two fluids, was proof sufficient in my mind to point out the important function of the spleen, viz. to prepare the blood for the secretion of natural bile. This experiment was repeated with nearly the same result. Having progressed so far in my enquiries, and having met with good encouragement, I was stimulated to go further. Wishing to know in what manner the removal of the spleen would effect the system generally, the experiment was made. The animal refused meat or drink and on the fourth day died; having had during this time no evacuation per anum. The appearance of the abdomen, did not seem to indicate death by inflammation alone; the mesenteric glands were enlarged, and small balls of yellowish white indurated fœces were found in the ileon. The absorbents of the mesentery contained chyle. Except inflammation, I know not what to ascribe the death

of this animal to, except defective chylification, and a torpor of the alimentary canal. If this experiment proves no more, I think it proves the importance of this organ in an animal body. Some argue, that the function of the spleen cannot be very important, because animals may live some time with it removed. This argument has no weight. Animals may live with part of the lungs removed; the aorta tied; a limb excised, and indeed the most of the brain removed; but does this argue their little utility in an animal body? I was next led to ascertain what difference, if any, existed between spleenic venous blood, and other venous blood. Blood was taken from the splenic and jugular veins of the same animal, an equal quantity of each was set by to separate into its serum and crasimentum when this was accomplished, the serum of each was carefully separated. The splenic serum weighed thirteen drams and one scruple; the jugular weighed twelve drams and eighteen grains. By submitting these to heat, each yielded a proportionate quantity of albumin. By washing the crasimentum, the spleenic blood contained less fibrin.

Now, serum, or albumin appears to be the most unrefined part of the blood. It abounds most in diseased and starved constitutions. While the healthy and vigorous have a greater proportion of crasimentum. Fibrin and gelatine seem to compose the most refined parts of an animal body, while albumin seems to be more of a connecting medium. Albumin is found in nearly all the excretions of diseased persons, especially the dropsical and diabetic. In the former disease, Mr. Cruickshank found the urine so completely coagulable by heat and acids, as to differ little from the serum of the blood. But when dropsy arose from a morbid state of the liver, he found the urine not coagulable, small in quantity;

high coloured, and depositing a pink like sediment. These facts very much favour the doctrine here inculcated, both with respect to the spleen and liver. The fluid of dropsies is generally poured out by the exhalent arteries before being elaborated in the liver, and the urine is secreted directly from the arterial system. In this case, the cause appears to be, a redundant quantity of imperfect serum in the arterial system, causing a diseased action of the exhalents; in which case its origin may be looked for in the stomach. But if the liver is the cause of this disease, the blood will have passed from the arterial to the venous system, before the cause acts upon it. The albuminous portion of the blood then seemed to have been sufficiently elaborated in that diseased organ, as not to become extraneous; hence the fluid of dropsies arising from the liver is not albuminous. The pinklike sediment is likely to be owing to the imperfect formation of red globules. I made several other experiments on the spleen, but as they had no tendency to disprove, and but little to corroborate the above theory, I deem it unnecessary to mention them. But I believe *à priori* that the spleen may be safely considered as auxiliary to the liver in elaborating imperfect blood, and furnishing more abundantly the stimulating and cathartic principles of cystic bile.

In giving this function to the spleen, it seems to resemble that of the placenta, to which its structure is very analogous.

OBSERVATIONS ON THE CAPSULÆ RENALES AND THYMUS GLAND OF THE FŒTUS, AND THE THYROID GLAND OF THE ADULT.

My inquiries into the function of the spleen, have led me to believe that these organs perform an analogous function, viz. they concur with the liver in completing the assimilating process. Are not all the vascular and glandular parts of an animal body, except the brain and nerves, and the parts concerned in the preservation of the species, devoted exclusively to the assimilation and conveyance of the aliment, by which an animal is nourished and supported? What other office is there to perform? From the reception of the aliment by the mouth after birth, or by the umbilical vein in the fœtus, we see it undergoing changes by the different organs through which it passes, according to their different functions. In the mouth and stomach, by the action of the saliva and gastic juice, an imperfect chyle is formed. This is taken up by the lacteals, carried first through the lymphatic glands to be modified, then is poured into the circulation, to become pure blood, which alone is fit to nourish the animal. But in order to accomplish this, other important changes are necessary; its extraneous parts must be taken away, and its coarser ones refined. Its extraneous parts are taken away by the skin, lungs, kidnies, bowels, &c. and I believe its coarser parts are refined essentially by the li-

ver, and aided in the human adult subject by the spleen, thyroid gland, and perhaps other glands having no excretory ducts. The thyroid gland appears to be conveniently situated to perform the office of a refining organ to the blood going to the brain; its chief blood is received by a very large branch, arising from the internal carotid artery just above where the common carotid bifurcates. It is reasonable to suppose, that the primary and most delicate organ of the human body, requires the finest blood for its nourishment. Here then, the thyroid gland, like a watchman, is placed, to abstract the coarser particles and convey it back again to the venous system. The thymus gland of quadrupeds seem to perform an analogous function. But with the fœtus in utero it is different; having no organs in action to take away the extraneous parts of its aliment, this is done by the mother; but the maternal blood is too stimulating and unrefined for direct transfusion; its too stimulating and coarser particles must be taken away before it is fitted to nourish the delicate fabric of the fœtus. For it is presumed, that the blood of the mother is as unfit to nourish the fœtus, as the aliment of the mother. Indeed, by examining the laws of the animal economy, this presumption is self-evident. Why cannot the offspring, immediately after birth, receive the same stimulating juices for its nourishment, as those contained in the aliments of the mother? The answer is obvious; its delicate organs are incapable to act upon it, therefore, high excitement, inflammatory action, effusion and death would be the consequence. Why is the bland milk elaborated in the mammæ of the mother, alone fit to nourish the infant? because, of all other fluids it is less stimulant. Nature has therefore provided a remedy against the direct transfusion of stimulating maternal

blood to the fœtus. The placenta and fœtal liver are this remedy. No fact is more evident, than that the arterial blood of the mother undergoes a striking change in the fabric of the placenta; it goes to it of a red vermillion colour; comes from it of a dark venous colour and more dilute; (very analogous to what takes place in the spleen, which according to our doctrine has a similar function).

That the fœtus is nourished by the blood of the mother, I think there can be no doubt, when it is considered, that the uterine arterial vessels inosculate with those of the maternal part of the placenta, and that many diseases of the mother are transmitted to the fœtus in utero. And that, this blood undergoes a material change in the placenta is equally manifest by the striking differences existing between mother's blood, and that contained in the umbilical vein. Therefore I think it obvious, that the placenta performs a secerning function to the arterial blood of the mother. The fœtal liver has now to elaborate, and convert this placental blood into bland fœtal nourishment; and as this is no small office to perform, the fœtal liver is necessarily large. But as a portion of the placental blood passes immediately to the heart by the ductus venosus, there remains still in the fœtal circulation, a portion of unanimalized blood, which nature does not want to carry out of the system, and which remains still to be perfectly refined. Now, as the liver has done its office by elaborating four-fifths of the blood, and the spleen and vena portæ being designed to perform their offices after birth, it may be asked, what organs are there in the fœtal system to perform this important function.

A part of the unrefined blood, transmitted to the heart by the ductus venosus, goes through the foramen ovale,

and would be sent in its impure state to the depending brain, had not nature prepared an effectual remedy in the thymus gland. This gland very large in the fœtus, situated above the heart, in the anterior mediastinal space, filling nearly one third of the thorax, and receiving large quantities of blood from the carotid and subclavian arteries, is admirably situated to perform this secerning function, with respect to that portion of the blood going to the head. A part of this unrefined blood, escapes by the ductus arteriosus into the descending, or ascending aorta of the fœtus; therefore, other secerning organs are necessary. But the function of the spleen laying pretty much dormant in the fœtal system, because no cystic bile is wanted, the capsulæ renales of the fœtus, perform a vicarious function to the spleen after birth. The capsulæ renales of a fœtal dog are much larger than the kidnies themselves, being stretched entirely across the abdomen. They receive the principal part of their blood from the aorta itself—some branches from the emulgents, and pour their blood by large veins into the vena cava and renal veins. Now, could organs have been better situated and adapted to screen that portion of the blood above the curve of the aorta, than the capsulæ renales.

If the doctrines here inculcated be correct, will they not elevate the liver from its before humble and degraded office, and give it a rank in the animal economy second to none but the brain? that rank which its volume and omnipresence assigns to it.

And will not important advantages result from them, in their application to pathology? Will not this doctrine fully and completely explain why diseases of the liver alternate with those of the stomach and vice versa? Is it not because the stomach and liver perform vicarious offices? Will not this doctrine fully and amply explain

why the diseases of the liver are more frequent in hot than in cold climates ; in full and high feeders than in moderate ; in wine and dram drinkers than in the temperate ? Every author who has writen on the diseases of the liver of warm climates tell us that a diseased stomacch is one of its invariable symptoms.

Doctor T. Clark says, that a diseased stomach is not only the constant but often the first sign of a diseased liver. But this reciprocity of disease has been attempted to be explained by nervous connection. This nervous connection will equally apply to all the abdominal viscera, and indeed to every part of the human body.

Will not this doctrine also elevate the spleen from its non-organical state and give to it a function equal if not greater than that of the kidney ? Other organs both in the fœtal and adult state, equally necessary to the perfect harmony and completion of an animal body, will be raised from their physiological graves, and claim a share of no small importance in the animal economy.