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EXPERIMENTS & OBSERVATIONS

ON

THE BILE,

By JARVIS ROEBUCK,

OF THE ISLAND OF ST. CROIX;

Honorary Member of the Philadelphia Medical and Chemical Societies.

Conamur tenues, grandia.

HOR.

PHILADELPHIA:

PRINTED BY CARR & SMITH.

1801.

Mr. Ankrum

with the Compts.

of the

Author

AN

INAUGURAL

EXPERIMENTAL INQUIRY,

FOR THE DEGREE OF

DOCTOR OF MEDICINE.

SUBMITTED TO THE EXAMINATION

OF THE

REV. JOHN EWING, S. T. P. PROVOST;

THE

TRUSTEES AND MEDICAL FACULTY

OF THE

UNIVERSITY OF PENNSYLVANIA,

ON THE

EIGHTH DAY OF JUNE, 1801,

TO

BENJAMIN SMITH BARTON, M. D.

Professor of Materia Medica, Natural History, and Botany,

IN THE UNIVERSITY OF PENNSYLVANIA.

SIR,

AN acknowledgment of the many favours received at your hands, would require much more than the too proſtituted page of dedication. Nor would I wiſh them cancelled by a bare enumeration. Inſpired with a lively ſenſe of gratitude and affection, by the numerous teſtimonies of your attention in the double capacity of preceptor and friend; my ſenſations may more readily be conceived than expreſsed. Permit me the ſilent enjoyment of them. I regret that the preſent performance, is not more worthy of your patronage: you will readily perceive, that the proper conſideration of the ſubject, required more than the allotted time. That you may enjoy health, adequate to the continuance of thoſe labours, in the field of ſcience, which have been ſo productive of benefit to ſociety, and honour to yourſelf, is the fervent wiſh of

Your Friend and Pupil,

THE AUTHOR.

TO

DRS. JOHN & CHRISTOPHER JOHNSON,

OF

THE ISLAND OF ST. CROIX;

AS A TRIBUTE

OF

GRATITUDE AND RESPECT,

THE FOLLOWING PAGES

ARE ALSO

DEDICATED,

BY THEIR

FRIEND AND NEPHEW,

THE AUTHOR.

EXPERIMENTS, &c.

ON THE BILE.

IN the inveſtigation of a ſubject like this, to which much time, and great talents, have often been devoted, little elſe can be effected by the hand of inexperience, than to retrace the road, which towering and ſucceſsful genius has pointed out.

Little advantage, however, has reſulted from the inquiries of our predeceſſors, who (unaided by the rapid improvements which have recently been made in the ſcience of chemiſtry) deduced inferences, from thoſe reſults, alone, which they were capable of obtaining by the aſſiſtance of heat; the fallacy of which muſt be evident, in a minute analyſis; and from which many prejudices, at preſent exiſting, have originated.

To the mere ſpeculation of Boerhaave, are we indebted, for an opinion, which modern

chemiſtry has combated in vain: the ſaponaceous nature of the bile—founded on analogy alone, has been received by ſucceſſive generations as a well-eſtabliſhed point in phyſiology; and the reputation of that great man has ſanctioned the error. The ſupport which the opinion of Boerhaave has received, from the experiments of Cadet, are inſufficient, however, to eſtabliſh it on the firm baſis of truth. Reſting the reſult on the action of heat, he obtained by his experiment a quantity of oil, ſoda, &c. he, therefore, immediately concluded the bile to be a true animal ſoap. This was conteſted in a publication at Stratſburg, by a Mr. Roederer, who advanced the coagulation of milk by bile, as a ſufficient refutation of its alkaline nature. Without entering into the conſideration of this refutation, we may eaſily convince ourſelves of the fallacy of Cadet's inductions, by reflecting on the tendency of heat to produce different reſults, according to the mode of its application; but a ſole reliance on heat was improper: the ſalts, exiſting in the bile, muſt inevitably be decompoſed, their earthy, or alkaline baſes, will remain, and as there certainly does exiſt an oil in the bile, are we on ſuch ſlight foundation to infer the ſaponace

ous nature of it? The experiments which I have inſtituted, clearly evince the exiſtence of a conſiderable proportion of phoſphoric acid, and that combined with an earth and alkali; the union therefore of the oil and alkali will be prevented; the preſence of an acid, and the formation of ſoap being incompatible.

Of the various fluids, of the human body, none has engaged the attention of the phiſiologiſts, more, than the ſubject of the preſent eſſay. Its preſence, in almoſt all the claſſes of animals, muſt have indicated ſome ſalutatary effect; and the defect of its introduction, in the chylopoietic viſcera, by diſeaſe, was ſufficient to eſtabliſh the efficacy of its operation. But although all were willing to allow the advantages of an uninterrupted excretion; yet many regarded the bile, as a neceſſary agent, in the formation of chyle; while others adopted the opinion of its being merely excrementitious, and ſeparated from the blood as a maſs, unfit for the purpoſes of the animal economy.

Numerous are the arguments which might be advanced in favour of either opinion: On the one hand, a deficiency of chyle has been attributed to the obſtruction of the biliary ducts, (for if the bile is a requiſite ingre-

dient in the compoſition of chyle, the want of it, will preſent a ſubſtance improper to repair the conſtant waſte incident to the human ſyſtem ; and its action on the alimentary canal in aſſiſting the protruſion of its contents, has long been conſidered, as indiſpenſable. On the other hand, may not its agency, in the formation of chyle, be juſtly called in queſtion, when we reflect on the ſupport which the ſyſtem receives, independently of the bile, during the diſeaſe of jaundice ; if chyle is not formed, the diſeaſe muſt neceſſarily be fatal; if it is formed, (which for the patient to ſurvive it certainly muſt be,) it is effected without the aſſiſtance of the bile, and as this takes place, in one inſtance, why not at all times ? But does not the retention of the fœces, ſtrongly exhibit the neceſſity of its operation ? Having at one time, exerted its influence on the viſcera, the ceſſation of that ſtimulus, muſt certainly be prejudicial ; does this however, evince any more, than that parts accuſtomed to the operation of any ſtimulus will be incapable of performing their accuſtomed functions when deprived of it ? Does it amount to a poſitive demonſtration of an original incapacity in the inteſtines, to be excited to a regular action, but by means of the

bile? Had this fluid ſome other outlet from the body, would not the contents of the ſtomach prove ſufficiently ſtimulant? For as the irritability of the parts would be leſs impaired, an inconſiderable ſtimulus muſt be capable of exciting a ſufficient degree of action. The ſtomach, unaided by the bile, drives onward its contents, for its occaſional preſence there muſt be conſidered an error loci, the conſtant effect of diſeaſe; and the ſtimulus of diſtention appears ſufficient to excite the muſcular fibres of the œſophagus to their accuſtomed duty. The important operation attributed to the bile, in the proceſs of chylification, as the medium of combination, between the oily and aqueous portions of the chyme, thereby forming that moſt important fluid, the chyle, and the precipitation of the fœcal part, for the purpoſe of its elimination, wears the garb of deceptive hypotheſis rather than of concluſive experiment. The ſaponaceous nature of the bile, we have already attempted to diſprove; but, admitting this property, and that nature had kindly afforded us this medium for the proper union ofthe repulſive parts of the unaffimilated chyme, to what muſt this union be attributed, when by diſeaſe the uſual ſupply has been prevented? To an improper ſtimulus in

the arterial ſyſtem, muſt the uneaſy ſenſations of the patient be aſcribed, and not to a defect of nouriſhment. Are not the fœces ſeparated in the diſeaſe of jaundice? The torpor, indeed, of the inteſtines, owing to the privation of an accuſtomed ſtimulus, prevents their ſpeedy expulſion, and the activity of the abſorbents deprives them of their uſual moiſture; but

"Non noſtrum, inter hos, tantas componere lites."

I muſt therefore wave the further conſideraton of theſe opinions; much learning and ingenuity has, without doubt, been diſplayed on both ſides. To experiment, the ſole, and never failing ſource of truth, muſt the deciſion be referred; reaſoning without its aid is the mere mantle of fallacy, and plauſibility alone has enſlaved whole ages in the bonds of error!

Altho' a conſiderable diverſity of ſentiment exiſted, reſpecting the nature of the operation of the bile, more unanimity prevailed as to the ſource of its ſupply. The peculiarity of the ſtructure of the liver, in the unuſual number and ſize of its veſſels, when compared with any other viſcus, and the neceſſity of the returning blood of the inteſtinal canal taking that route, previous to its being

reconducted to the general circulation, were too ſtriking to eſcape obſervation, and too important to be neglected by our ſpeculative predeceſſors. They ſuppoſed that this blood poſſeſſed ſome principle which the parts where it had been diſtributed were in a particular manner calculated to afford; and, which was not to be detected in any other portion of the vaſcular ſyſtem: To this, the ambiguous term phlogiſton was appended, a term of ſingular ſervice, in expreſſing whatever is little underſtood; and that this phlogiſton ultimately ſeparated from the blood in the liver, appeared under the form of bile. This idea, is, I believe, or ought to be,entirely diſcarded—when we conſider the great length of the inteſtinal tube; how numerous and minute muſt be the ramifications of the meſenteric arteries, for the purpoſe of their ſupply—we may readily conceive, that in this tedious route, the oxygenous principle which it had received in the lungs muſt be diſſipated; but, in no reſpect, does it differ from the refluent blood of the extremities.

But, is the preſence of oxygen unfavorable to the ſecretion of bile? By no means:—Accidental diſſections have ſatisfactorily eſtabliſhed this point; and the caſe of Mr. Aber-

nethy alone, wherein the vena portarum communicated immediately with the aſcending cava, and the hepatic artery was found adequate to the nouriſhment of the liver and the purpoſes of the bilious ſecretion; proves to a demonſtration, that the proceſs is carried on, though the parts be ſupplied ſolely with arterial blood.

The opinion which Maclurg maintained, in his celebrated treatiſe on the bile, namely, that its ſecretion was calculated to ſeparate that portion from the blood, which in the tedious courſe of the circulation, had acquired the putrefactive taint, has been ſufficiently refuted; not alone by oppoſing argument to argument, but by an appeal to the moſt deciſive tribunal, that of experiment.

In the frequent analyſes of the bile, which have been given to the world, the reſults have been very different. Thus, for inſtance, the bile has been conſidered a true ſoap, formed by the combination of an animal oil, and ſoda, to which a ſmall proportion of mucilage and reſin has been added. At another, we find the ſoda united with an acid, either the muriatic or phoſphoric, and ſometimes both. Some, more fortunate than the reſt, have detected the preſence of iron; while others, with the

ſame views, have experimented in vain. Nor muſt lime and ammoniac be omitted in this brief review ; to which, if the ſaccharine ſubſtance, ſimilar to the ſugar of milk, mentioned by Cadet, be added, we ſhall have nearly aſcertained the ſum total of opinions on this ſubject.

But are we to conclude from this, that the bile of the ſame ſpecies of animal, uninfluenced by diſeaſe, can, at different periods, afford ſuch varying reſults ? May we not with greater propriety, attribute them to ignorance, prejudice, or miſtake? A preconceived opinion too often influences the inductions from the beſt experiment; by ignorance, phantaſms are taken for realities ; and miſtake has been a frequent clog to the wheel of ſcience.

Although much remains to be done on this ſubject, little ſatisfaction can the reader derive from the peruſal of the following pages.

Owing to the limited period in which I was to make an election, and conclude the conſideration of a ſubject—the period of a few weeks; I was obliged to confine myſelf to one view of the ſubject, viz. to the difference (if any) which exiſts between the bile,

of different animals. The number has unavoidably been ſmall.

My experiments were firſt made with the bile of the ox: To obtain the reſults more ſatisfactorily, I took advantage of the aſſiſtance of heat, and the requiſite chemical agents.

EXPERIMENT.

I expoſed 10 oz. of recent ox bile, in a retort (to which was appended a tubulated receiver) over Argand's lamp, to a low degree of heat; at the end of ſome hours, I obtained eight ounces of tranſparent colourleſs water, which was ſtrongly impregnated with the odor ſubmoſchatus, or aroma of the bile, but entirely free of bitterneſs: immediately after this, the retort was filled with a white denſe vapour, of ſuch gravity, that it fell to the bottom of the receiver, and was there condenſed. A ſmall quantity of water, which fell from the neck of the retort with this vapour was immediately mixt with it, and preſented a colour as white as milk: half an ounce of it was obtained; it was ſlightly empyreumatic, and being placed aſide for an hour, a thick white ſubſtance was precipitated; the ſupernatant liquor, however, remained ſtill ſomewhat coloured by it. This I conſider an

oil; the colour muſt be owing to the low degree of heat uſed. After which I obtained half an ounce of a brown oil ſtrongly empyreumatic: a hard bitter maſs remained in the retort.

Analysis of the bile of the Ox: by chemical tests.

Nine oz. of ox bile being taken, a quantity of alkohol was added: it was then laid by for 48 hours; at the end of which time, by means of filtering paper, a gluten was ſeparated, which after deſication weighed 12 grs.

The brown, tranſparent, ſaccharine and intenſely bitter liquor which paſſed the paper, was evaporated to the conſiſtence of a denſe extract; on this extract diſtilled water was poured, for the purpoſe of ſeparating that portion which has been called reſinous; this being again filtered, left nothing behind that could be denominated a reſin. Evaporation was a ſecond time performed, and carried ſo far that the reſiduum underwent an imperfect chryſtallization, when taken from the lamp, this was divided into ſeveral portions and treated with the following teſts:

To one, the oxalic acid was added, the precipitate which enſued demonſtrated the preſence of lime:

To a ſecond, the muriated barytes; the phoſphate of barytes was precipitated:

To a third, a few drops of the alkohol of galls; no change of colour enſued:

To a fourth, the pruſſiat of pot-aſh; the preſence of iron, however was not detected by it:

To a fifth, the nitrat of ſilver; a copious precipitate enſued: but are we from this experiment to conclude that the muriatic acid exiſts in combination in the bile?

Many, I am aware, have inferred the exiſtence of it, by this experiment alone; without conſidering, that, as the phoſphoric acid exiſts with the ſoda or lime, the addition of nitrated ſilver muſt cauſe a precipitation; owing to the ſtronger affinity which exiſts between the nitric acid of the ſilver, and the ſoda of the phoſphoric ſalt, whereby the phoſphoric acid ſeizes on the ſilver, and forms an inſoluble compound.

Does there exiſt any difference between the component ingredients of the ox's bile and that of the calf? are any of them wanting in the latter which age may be ſuppoſed to ſupply? or may they not all exiſt, and the only difference be in the proportions? To ſatisfy myſelf on this point, the bile of a calf, two weeks old, was ſubjected to examination.

Two ounces of this bile, with the addition of a ſufficient quantity of alkohol, after remaining the uſual time, for the proper ſeparation of the glutinous part, was ſubjected to the filter; this gluten which remained on the filter was in very ſmall quantity, adhering to the paper, extremely yellow, and very ſlightly bitter.* The filtered liquor was brown, diaphanous, and poſſeſſed of an aromatic bitterneſs not to be met with in the more pungent bile of the ox; diſtilled water was added to this, to ſeparate the reſin; no precipitate however was obtained: the liquor was evaporated: a portion of the extract diſſolved in diſtilled water, with the addition of the oxalic acid, produced a cloud barely to be diſtinguiſhed.

To a ſecond portion, the addition of the muriated barytes, cauſed a phoſphate of barytes to be precipitated.

Neither the pruſſiat of pot-aſh, nor alkohol of galls, produced a change in point of colour.

A drop of the nitric acid by chance fell in the glaſs where a ſmall portion of the extract was diſſolved, and a ſlight purple tint

* This gluten could not have exceeded a grain in weight.

was produced; this was greatly increaſed by the addition of two or three drops more.

By this we perceive that what is found in the bile of the ox, is alſo to be detected in that of the calf, the proportions however being much ſmaller in that of the latter.

Analysis of the Bile of the Dog.

Having obtained ſome dog's bile, to an ounce and a half of it were added two ounces of alkohol; it was laid by for two days, then committed to filtering paper; a dark green liquor was ſeparated, and left a light green ſubſtance on the paper, which had been precipitated by the alkohol. This ſubſtance carefully taken from the paper, was weighed when moiſt, and amounted to 22 grs. the external ſurface aſſumed a dark green appearance, by the action of the air: the ſurface which reſted on the paper, was of a light graſs green colour: applied to the tongue it was perfectly inſipid: when perfectly dry, it weighed but three grains: although the ſpecific gravity of this ſubſtance was ſo ſmall yet the bulk was conſiderable; ſo much ſo, indeed, that previous to my weighing it the ſecond time, I judged it could not be leſs than 15 or 20 grs.—when broken, the edges were

ſhining; the central parts of a duſky green, and very compact: one fourth of it was put in a ſmall quantity of water; the water was rendered turbid; and on reſting a few minutes the whole fell to the bottom in the form of a floccous precipitate.

Another portion was tried with the nitric acid, it was entirely diſſolved; the colour of the ſolution was of a reddiſh yellow.

A third in the muriatic acid; it was alſo diſſolved completely, and was of a beautiful bottle-green colour; when water was added to this, the whole aſſumed a beautiful blue colour; and a floccous ſubſtance fell to the bottom: ſome of the phoſphate of lime, was added to a portion of that ſolution; the colour was ſtill preſerved.

To a fourth portion ſome concentrated alkohol was added: it had no action on it.

The liquor which had paſſed the filtre was next tried. It was expoſed in an open veſſel to a gentle heat, and the alkohol driven off.

To the reſidue, about an ounce of diſtilled water was added, for the purpoſe of obtaining the reſinous part which the alkohol had diſſolved: the liquor after filtration was clear, ſomewhat ſweet, intenſely bitter, and of a dark green colour; the reſinous part which

remained on the paper was very ſmall in quantity, brown and ſlightly bitter. The liquor was again evaporated, the extract was of a ſweet taſte, ſtill extremely bitter (although the reſinous part, in which the bitterneſs is ſaid to reſide, was ſeparated) and very tenacious or viſcid. After further ſolutions and evaporations, the extract was analyſed to aſcertain the nature of the ſalts.

To 5 grains of this extract, diſſolved in diſtilled water, two or three drops of the oxalic acid were added: a precipitate was obtained, which evinced the preſence of lime in no inconſiderable quantity. The proportion was much greater in this bile than in that of the ox; it was impoſſible for me to aſcertain the exact quantity, as it readily paſſed the filter, and by reſt was converted into a pretty ſtrong pellicle, adhering to the ſides of the glaſs.

To a ſolution of the above quantity of the extract in diſtilled water, a few drops of the nitrate of ſilver were added; a copious precipitate was the conſequence, a duſky red ſubſtance adhered to the ſides of the glaſs. This precipitate was principally, if not entirely, a phoſphate of ſilver.

The phoſphoric acid was clearly indicated to be preſent by two other experiments, wherein precipitates were obtained by the addition of muriated barytes and the acetate of lead, or ſaccharum ſaturni.

To a ſolution of the extract, a ſmall quantity of the alkohol of galls was added, no black colour was produced. Neither was a change of colour obſerved when the pruſſiat of pot-aſh was made uſe of.

Analysis of the bile of the acepenser sturio? or sturgeon.

The bile of fiſhes being known to be much more acrid, in general, than that of terreſtrial animals; it was a matter of curioſity to aſcertain whether this analyſis, would preſent me with the ſame reſults as thoſe I had obtained in the preceding inveſtigations.

The colour of this bile when taken from the gall bladder of the fiſh, was of a ſtrong and as elegant a green as I had ever obſerved, and to the taſte, it was intenſely bitter.

To an ounce meaſure of it, was added the ſame quantity of highly concentrated alkohol, it remained unmoleſted for two days, at the end of which time a conſiderable precipitate,

occupied the bottom of the glaſs: the whole was thrown on filtering paper.

The glutinous part which remained on the paper, did not amount to more than half a grain. The liquor which had paſſed the filter was evaporated: to the extract ſome diſtilled water was added; the whole was taken up by the water; on filtering it no reſinous ſubſtance remained on the paper; the liquor was again evaporated, and the extract was treated in the following manner; after being re-diſſolved in diſtilled water.

To a portion of it, a drop or two of muriated barytes were added: a precipitate was obtained: this was without doubt the phoſphate of barytes.

To a ſecond, the addition of the oxalic acid, evinced the preſence of lime in a very ſmall proportion.

To a third, I added the alkohol of galls, no change of colour was produced; nor did the pruſſiat of pot-aſh evince the preſence of iron.

Examination of the Human Bile.

Having obtained the bile of a perſon, who afflicted with chronic mania for a number of years, ultimately fell a victim to pulmonary conſumption; I wiſhed to aſcertain whether

any particular change is effected in this fluid by diſeaſe; conſequently kept it ſeparate from the bile of another perſon who had been differently affected. The quantity I obtained rather exceeded an ounce; the colour was of a dark brown, edged with a ſhining yellow; the taſte moderately bitter: a couple of ounces of alkohol being added, it remained undiſturbed for ſeveral hours, at the end of which a very copious precipitate fell to the bottom. It was now committed to the filter; the gluten which was ſeparated, was of a yellowiſh green colour, in the courſe of a few hours it became dry; I found it weighed ten grains. Externally it was of a deep green, frequently interrupted by a yellow ſpot or ſtreak and interſperſed with a number of thin ſcales of a micacious brilliancy; the internal ſurface when expoſed by fracture, evinced a greater portion of the yellow, than the external; the ſcales were diſtributed, but not very thickly, thro' the entire ſubſtance. This ſubſtance was ſoluble in the ſaliva, but of no perceptible bitterneſs when applied to the tongue. The paper through which the mixture of the bile with alkohol had paſſed, and on which the glutinous part remained, preſented an intereſting appearance, which was,

that on the evaporation of the alkohol, white brilliant plates, ſimilar to thoſe in the glutinous portion, but infinitely more abundant; were ſpread upon it. Was this a portion of the reſin held in ſolution by the alkohol, which the paper had abſorbed, and which, after an expoſure to the atmoſphere, and the conſequent diſſipation of the ſpirituous part, remained on the paper in a chryſtallized form? Or was it the ſize, with which the paper was impregnated? No ſuch appearance, it muſt be confeſſed, was obſerved in any experiments with the bile of other animals: If it proceeded from the paper, it ſhould have taken place in every experiment. Some of theſe plates when taken up by the point of a knife, and applied to the tongue, were perfectly inſipid or without taſte. The proportion of this ſubſtance was very ſmall, probably not more than 1½ or two grains.

After repeated additions of diſtilled water, and evaporations of the liquor which paſſed the filter, the ſalts which remained were coloured with a ſmall portion of a brown extractive matter, very ſimilar in conſiſtence to a denſe ſyrup, which made them adhere to every thing that came in contact with them, which extractive matter I could not ſeparate

from them, as it was equally ſoluble, in alkohol or water, the ſalts, together with this brown ſubſtance (which was in very ſmall quantity) weighed only 2 grs.

The whole was diſſolved in diſtilled water, and divided into ſeveral portions.

As I wiſhed to aſcertain the preſence of the muriatic acid, which has been mentioned by ſome writers as exiſting in the bile, and knowing that the ordinary method was inaccurate, I ventured to try the following: To a portion of the ſaline ſubſtance diſſolved in diſtilled water, a quantity of the ſulphate of iron was added, ſufficient to decompoſe all the ſalts; by it I expected to have formed a phoſphate of iron; and (if the muriatic acid exiſted in combination in any of the ſalts) a muriat of iron. The phoſphate of iron, I knew, was inſoluble; it would conſequently be detained in the filter. The muriate of iron is ſoluble, that neceſſarily would exiſt in the filtered liquor; and the ſulphate of lime or gypſum could be ſeparated; by this means I expected, with the nitrate of ſilver, to detect the muriatic acid, if it exiſted.

After the addition of the green vitriol, the liquor was turbid; on reſting a brown ſubſtance was precipitated, which was the

phoſphate of iron. The whole was thrown on filtering paper: the brown, extractive ſubſtance, mentioned above, adhered to the precipitate; and the filtered liquor was not in the leaſt diſcouloured: a thin pellicle on the ſurface of the liquor, gave me reaſon to ſuppoſe that the ſulphate of lime had paſſed through the paper—this I was more inclined to believe was the caſe, as in ſome former experiments I found the oxalate readily paſſed through. I therefore doubled my paper, and refiltered it: the nitrat of ſilver was now added, and a turbidneſs in the liquor was the conſequence—From this, therefore, I think, the concluſion ought to be, that the muriatic acid exiſts in combination in the bile.

A ſecond portion of the ſaline matter being diſſolved, as above, a drop or two of the muriated barytes were added: the phoſphate of barytes was precipitated.

To a third, the addition of the oxalic acid produced an oxalate of lime. No change of colour was produced on the addition of the alkohol of galls, or pruſſiat of pot aſh.

From theſe experiments it is neceſſary to draw ſome inference. I am ſorry that it is not in my power to add an analyſis of the

bile of an animal purely carniverous; it might have given ſtrength to the opinion which thoſe I have performed enable me to advance, on the nature of bile in general. An opinion deduced from experiments, has, I hope, more than plauſibility, to recommend it.

I ſhall here attempt a ſeparate conſideration of ſome of the moſt important parts of the bile. The colour of the bile as immediately attracting obſervation, I ſhall firſt ſpeak of.

This in different animals varies conſiderably; it is obſerved in animals of the ſame claſs. The bile of the ox differs from that of the ſheep: the one is of a deep brown, the other of a greener hue. Human bile approaches nearer to that of the ox; and that of the dog is a deep bottle green; how different alſo is the black bile of the cuttle fiſh, from the lively green of the ſturgeon, and many others. To what is this owing? ſome there are who attribute the colour to the preſence of iron; and Cadet thought it probable, that a ferruginous calcareous earth, together with a peculiar animal oil, were the cauſes of bitterneſs and colour.*

To the firſt (the opinion of M. Durade of Geneva,) I muſt anſwer, that as I have been

* Med. Commen. vol. 1. p. 69.

conſtantly unſucceſsful in my attempts to detect the preſence of iron, and as the evaſive manner in which it has been mentioned by this very author, induced Maclurg to ſuſpect that ſome pre-poſſeſſion was neceſſary to detect it: From theſe conſiderations, I am unwilling to aſcribe an effect ſo material to a cauſe, the exiſtence of which is far from being undiſputed. If the preſence of iron, ſhould be proved, could analogy favour us with one argument in ſupport of the opinion? Experiment I have reaſon to ſuppoſe could not. The hypotheſis of Cadet has ſomething more to recommend it; the ferruginous earth, to which he attributes ſo important an operation in the economy of this ſecretion, taken in this view, does not deſerve the leaſt attention. Calcareous earth, certainly does exiſt in the bile, and admitting it to be ferruginous, no advantage can be derived from it when an acid is preſent. Neutral ſalts, when they exiſt, are incapable of effecting a change; they are not neceſſary to the formation of bile; their preſence muſt be conſidered fortuitous, but conſtant. Is there a perſon who conſiders carbonic acid a conſtituent part of atmoſpheric air? Yet who has ever failed in detecting it? Neutral ſalts have their definite

characters, and as long as theſe characters are preſerved, no change can be produced; this muſt only be effected by decompoſition, which argues a non-exiſtence; and when cauſes ceaſe to exiſt, are effects to be looked for? This reaſoning is not applicable to the experiments of Cadet, he does not ſuſpect the exiſtence of a neutral ſalt. As far as he went his analyſis is juſt; to be ſatisfactory, it ſhould have been more minute. His inductions were erroneous, becauſe his view of the ſubject was incomplete.

Although my acquaintance with the ſubject is but partial, it yet affords me ſufficient grounds for theſe arguments; the preſence of neutral ſalts, cannot be denied: the experiments are eaſy, and any perſon may ſatisfy himſelf on this ſcore; this being the caſe, the opinion of Cadet muſt fall! He has mentioned an oil; this, I have reaſon to believe muſt materially influence the colour of the bile; but not as he ſuppoſed, when united with an earth.

I have hinted above, that the preſence of ſalts is not required in the formation of bile; they exiſt in every animal fluid, and characteriſe none. What are the more prominent features of this ſecretion? I anſwer, colour,

taſte, conſiſtence and ſmell; the principles which, when united, produce theſe, are its component ingredients, and theſe conſiſt in a gluten, a volatile and fixed oil, and an acid. I ſhall endeavour to account for the colour, by an union of ſome of theſe; although ſupported by experiment, I may be miſtaken. After the gluten had been ſeparated from the bile of a dog, it was divided into ſeveral portions, and an acid added to two of theſe, the nitric to one, the muriatic to the other; in both caſes it was diſſolved; the firſt was of a reddiſh yellow colour, the laſt a beautiful bottle green; when water was added to the laſt, the colour was changed to a blue. From this, I conceive that an acid, acting on an animal ſubſtance, produces a colour, which is conſiderably modified by the preſencc of an oil, and that the difference of colour, is owing to the proportion of each; this is rendered probable, by the following experiment; the gluten of ſheep's bile is very yellow, I took a few grains of it, and added a tea ſpoon-full of ſpermaceti oil; they were intimately mixed; it was of a deep chocolate colour. This was increaſed by the addition of the muriatic acid; but on the addition of water, the whole put on a light green appearance.

I had an idea that this acid might be ſupplied by the decompoſition of ſea ſalt, and that thoſe animals which make the greateſt uſe of it, are poſſeſſed of a brighter coloured bile; but there may be many unanſwerable objections to this, and as the phoſphoric exiſts in greater quantity than the muriatic, and the ſame phenomena occur when it is uſed, it is immaterial which of the two be preſent; but probability ſeems more in favour of the former.

The aroma, I am apt to believe, depends on a volatile oil, which comes over with the water in diſtillation; but on the addition of alkohol, acids, &c. is deſtroyed; to the preſence of this oil, and even one of a more fixed nature; I aſcribe the formation of the reſin mentioned by all writers; I know no proof of its formal exiſtence; do we detect it when fire alone is uſed? Some have called the reſiduum after the diſtillation of bile has been carried to a certain length; "a brittle reſinous or pitchy maſs"; atmoſpheric air aided by heat, may have inſpiſſated the remaining oil; but by increaſing the heat, it comes over in a highly empyreumatic ſtate: How do chemiſts explain the formation of vegetable reſins; is it not by the oxygenation of the volatile oils. "By expoſure to the air," ſays Four-

croy, " they become thick, and in proceſs of time aſſume the character of reſin."

On the addition therefore, of alkohol, or an acid, with the intention of ſeparating the gluten, a partial decompoſition enſues, and a ſufficient quantity of oxygene is afforded to thicken the volatile oil, this change we have reaſon to ſuppoſe, takes place, as the aroma is deſtroyed. Now, as this aroma forms an eſſential part of ſuch an oil, and as we know that this principle can only be loſt (when it is ſo by long keeping) by a change being effected in the nature of the oil; is it irrational to conclude, that when a greater quantity of oxygen is afforded, the ſame thing ſhould take place in a ſhort ſpace of time. In recent bile therefore, I have no idea of the exiſtence of a reſin; but in what manner are we to account for the ſtrongeſt characteriſtic of the bile, I mean its bitterneſs. Authors have laid great ſtreſs on its reſiding in the reſin; but when the reſinous part is ſeparated by the addition of water to the alkohol, which holds it in ſolution, and the liquor which paſſes the filter is evaporated, the extract when formed, is very bitter. How are we to account for this? Ought bitterneſs toremain when the principle in which it conſiſts is taken away? I have obſerved in

all the extracts I made, a dark coloured viſcid ſubſtance, tenaciouſly adhering to the ſaline portion; this was equally ſoluble in water or alkohol, and while it remained the reſiduum was intenſely bitter. Was this a reſin? a reſin is inſoluble in water; a mucilage; to have anſwered this character it could not be diſſolved in alkohol; was it a ſaccharine matter; a pungent bitter, and ſtrongly ſaline taſte alone was perceptible; I conceived it to be an oil, ſomewhat changed however, by the treatment it had undergone, and principally to this fixed oil, I think the bitterneſs of the bile is to be aſcribed.

The conſiſtence of the bile is much increaſed by remaining in the gall bladder; this has properly enough been aſcribed to an abſorption of the aqueous parts. This viſcidity, may be referred to the gluten, in a great meaſure, but the quantity of oil preſent muſt have a conſiderable influence. I have deviated from the general opinion of thoſe who have written on this ſubject, in conſidering the ſubſtance ſeparated from the bile, by alkohol, a gluten. They have denominated it a mucilage; the characters of theſe ſubſtances are ſufficiently marked to prevent one being taken for the other, and as che-

miſts have written on the ſubject; want of conſideration, may probably be imputed to me, in deviating from received opinions: although I may be miſtaken, as to the nature of the ſubſtance, ſtill I muſt confeſs, that I never have detected any thing which anſwers my idea of a mucilage. I formed my opinion of the nature of this ſubſtance, by ſome experiments on the precipitate, afforded by the mixture of dog's bile and alkohol—they are related in the analyſis of the bile of that animal; by them, I found that acids diſſolved it completely after ſome time; that water rendered it ſoft, but did not diſſolve it, and that it was inſoluble in alkohol. I was diſſatisfied with the name of mucilage, when applied to a ſubſtance inſoluble in water. The appearance of that of the dog, when perfectly dry, was denſe and uniform; that of the ſheep was granulated. I could not detect the ſaccharine matter mentioned by Cadet—ſimilar to the ſugar of milk. The taſte of recent bile, is however conſiderably ſaccharine; that of the ox, more particularly. To the preſence of ammoniac, I have paid little attention, it is very frequently a creature of our own formation; and a variation in ſome circumſtances, is adequate to its production.

Thus, to conclude the inveſtigation which was the object of theſe experiments, I am of opinion, that the bile of all animals is alike, in thoſe parts which are moſt material or requiſite for its formation; the principles which I have mentioned above, are the ſame in all; the difference, I conceive, is to be found only in the proportions; this I grant may be very great in point of colour, taſte, &c. In the elaborate compilation of Haller on this ſubject, can there be found any two perſons who exactly correſpond in opinion.

In fine, this performance has been executed in haſte: To do juſtice to the ſubject, time and indefatigable perſeverance are required.

While I was engaged in the foregoing experiments, a moſt inveterate caſe of jaundice occurring under the care of my preceptor, preſented me with an opportunity of inſtituting ſundry experiments, in order to detect the preſence of the bile in the blood, as characteriſing the diſeaſe of jaundice.

Many there are, who do not conſider the bile as capable of exiſting in the blood-veſſels

without occaſioning death, and others, on the contrary, are too apt to aſcribe a yellowneſs of ſkin, &c. ſo frequently in many diſeaſes to the abſorption of it.

On this diſeaſe it is not my intention to ſay any thing; all I ſhall endeavour to prove will be, whether the bile can exiſt in the blood veſſels with impunity; for this purpoſe I obtained a quantity of jaundiced blood. After it had ſeparated, the ſerum was remarkably yellow, of a ſaline taſte, perfectly void of bitterneſs.

EXPERIMENT.

A table ſpoonfull of the ſerum was expoſed to the heat of a candle; the albuminous part after the evaporation of the ſerous portion was very yellow; this on being taſted betrayed not the leaſt ſenſible bitterneſs.

EXPERIMENT II.

Some of the craſſamentum was next taken, it was freed from the coagulable lymph by waſhing; the lymph was ſomewhat yellow, no taſte of the bile, however, was perceptible in it. That part of the coagulum which remained after the ſeparation of the lymph, was expoſed to heat, the aqueous part being diſ-

ſipated, the reſiduum was ſomewhat ſalt, not bitter.

EXPERIMENT III.

A portion of the craſſamentum, freed from coagulable lymph, was put in a wine glaſs, to this ſome alkohol was added, a partial coagulation was effected, which was ſoon after diſſolved, a little water was next added, which occaſioned the precipitation of a coagulated ſubſtance; a part of the ſupernatant liquor, holding the red globules in ſolution, was carefully poured off and expoſed to a gentle heat; after the evaporation, a ſcum of a brown colour remained, of a ſaline taſte, but not in the leaſt bitter. Finding that it would in this way, be impoſſible to detect its preſence, I reſolved to avail myſelf of the aid of chemical teſts.

EXPERIMENT IV.

A table ſpoonfull of the jaundiced ſerum was poured in a glaſs, three or four drops of the muriatic acid, were added, an immediate green colour was produced.

To aſcertain whether the marine acid is a good teſt for bile, 12 drops of that of the ox, were mixed in a little water, to this a few drops of the acid were added; the green colour was produced.

I found that the nitric acid, alſo produced a green with the jaundiced ſerum, and when added to bile mixed with water, the ſame thing took place.

The muriatic acid however, is much the better teſt, as the green is more rapidly evolved by its uſe.

The ſulphuric, cauſed a yellow colour.

To eſtabliſh the reſult of the above experiments, it was requiſite to aſcertain the action of the acids, on healty ſerum.

Having obtained ſome blood from a friend who was in perfect health, on the ſeparation of the ſerum, a portion of it was taken, to which I added a few drops of the muriatic acid, a white coagulum alone was the reſult.

The ſame took place on the addition of the nitric acid.

A patient labouring under pneumonia whoſe ſerum was remarkably yellow, afforded me an opportunity of aſcertaining how far a mere colouring matter might tend towards the production of the green, with the nitric or muriatic acids.

For this purpoſe, a quantity of it was obtained; to an ounce of this ſerum, a few drops of ſtrong nitric acid were added; an entire coagulation enſued, the coagulum, was at firſt

white, ſtanding for ſome minutes it aſſumed a yellow tinge. The ſerum, on the contrary, of the jaundiced perſon, inſtantaneouſly on the contact of the acid, formed a deep green coagulum.

With the muriatic, a ſlightly yellow firm coagulum enſued.

This is ſufficient to eſtabliſh the abſence of bile in the inflammatory ſerum; and alſo, that the preſence of it was abſolutely neceſſary to the production of the green colour, exemplified in the foregoing experiments, on the ſerum of the jaundiced patient. I found a very ſmall quantity of bile, ſufficient to produce the colour: for on adding 4 drops of ox bile to a table ſpoonful, or ½ an ounce of the inflammatory ſerum on the immediate contact of the marine acid, the green was ſtrong, but on being ſhaken it was faintly diſcernable in the yellow coagulated maſs.

When I uſed 12 drops of the bile, with the above quantum of ſerum; the muriatic acid produced a permanent green.

Having procured ſome of the urine of the ſame perſon, I was deſirous of ſeeing how far that ſecretion was affected by the diſeaſe. The contraſt between this and healthy urine was very remarkable: the one was viſcid,

and of a dark yellow, the other free from viſcidity, and of a ſtraw colour.

EXPERIMENT V.

To ½ a wine glaſs of urine, were added about 10 drops of concentrated nitric acid; a very deep green colour was immediately produced; on agitating the glaſs a ſlight effervescence took place, attended with the ſudden deſtruction of the green colour.

EXPERIMENT VI.

To a portion of the ſame urine were added a few drops of the muriatic acid; the green colour was immediately produced: no effervescence enſued: the colour was permanent.

When the ſulphuric acid was uſed, a black colour was the conſequence, attended with the diſengagement of calorique.

The following experiments were required, to aſcertain the action of the acids on healthy urine.

EXPERIMENT VII.

To half an ounce of healthy urine, 10 drops of the nitric acid were added; a conſiderable effervescence followed; the liquor was of its priſtine colour.

EXPERIMENT VIII.

To the above quantity of the ſame urine; 10 drops of muriatic acid were added; a ſlight purpliſh tint was obſervable; 10 more of the acid being added, the whole was changed to a ſlightly purple hue; no effervescence took place.

The effect of a little ox bile mixt with the healthy urine, expoſed to the action of the acids, was next tried to aſcertain the affinity of the reſult between this and the jaundiced urine.

EXPERIMENT IX.

To half a wine glaſs of healthy urine, 12 drops of the ox bile, with the ſame proportion of nitric acid were added; the green colour was produced; an effervescence then took place, which immediately deſtroyed the above colour.

EXPERIMENT X.

The ſame proportions of urine and bile being mixt, 12 drops of the muriatic acid were added; the green colour was produced, and was permanent; no effervescence took place.

From theſe experiments, I think we may ſafely conclude, that a conſiderable quantity

of bile muſt be preſent in the urine of the perſon the ſubject of them.

By the experiments alſo, we find that the muriatic is the beſt teſt. The reaſon why the nitric is not ſo good, muſt proceed from the ſtronger affinity which that acid has for the baſes of the urinary ſalts, whereby, although the green colour is at firſt produced, yet the affinity it has for theſe baſes being greater than that, with the bile, it ſoon quits the bile for them; efferveſcence is produced, and the green colour loſt. With the muriatic this does not take place; no efferveſcence enſues; owing to this acid being a component part of ſome of the urinary ſalts, or other acids in combination, with an alkaline, or earthy baſe having a ſtronger affinity for them than the muriatic has.

The event of the above experiments, being ſufficiently ſatisfactory to eſtabliſh the preſence of ſomething more than the colouring matter of the bile in the blood; I yet determined on performing an experiment which has been mentioned by ſeveral authors, namely the tying up of the hepatic and cyſtic ducts, in order to produce the diſeaſe of jaundice, and to aſcertain the preſence of the bile, not by the mere colour as they have done,

but in a more unequivocal manner by the aid of chemical teſts. Two experiments were performed on dogs, but as my intention was to preſerve them ſeveral days after the operation, in order to effect more completely what I had in view; ſome accident or other generally occurred which entirely defeated my deſigns. The following experiment, however, may not be entirely unworthy of notice. The great hardineſs of hogs was an inducement, for me to try the above mentioned experiment on one of this kind. For this purpoſe, a pig being obtained, ligatures were made on the cyſtic and common ducts, on the 5th of April; the gall bladder was well diſtended with bile; the flow of it into the ductus choledichus was prevented by the ligature nearly at the mouth of the cyſt. He continued very well until the 22d of April, when, by an unfortunate accident, he was killed. On examination, the wound in his abdomen had entirely healed. On opening, the inteſtines were firmly aglutinated to the peritoneum; the parts being carefully ſeparated, the liver appeared of a colour nearly natural, and the gall-bladder, which, when tied up, contained a conſiderable quantity of bile, appeared now to be filled with a

tranſparent liquor; and that portion of the duct included between the two ligatures, where the hepatic joined the cyſtic, to form the common duct, was diſtended almoſt to the ſize of the gall bladder, and contained a quantity of genuine bile :—Here then was the formation of a new cyſt; and by ſome means or other, the bile opened a paſſage into the duodenum. Both, the ligatures were found on the ducts, covered with coagulable lymph: no jaundice was produced in this caſe: the urine when tried by the nitric and muriatic acids, evinced no change of colour. Although I was diſappointed in the object of the experiment, yet the appearance of this white tranſparent fluid, in the old cyſt and the formation of a new one, were phenomena in ſome degree calculated to leſſon my regret. I ſhall however forbear any comments: when the liquor of the old cyſt was received in a glaſs, a quantity of a mucous ſubſtance immediately fell to the bottom, which left the ſupernatant liquor clear; the liquor was very viſcid, for on the application of the finger it was drawn out into ſmall threads, and when taſted perfectly void of bitterneſs, and inconſiderably ſaline.

A part of this liquor was taken and mixed with diſtilled water, and on the addition of the muriated barytes, a precipitate was the conſequence, which indicated the preſence of the phoſphoric acid. Another portion was tried with the oxalic acid, and lime was detected; indeed the ſalts, in this fluid, appeared to be exactly the ſame as thoſe which exiſt in the bile.

To the mucous ſubſtance (which we mentioned above to be precipitated from the tranſparent liquor) was added a little muriatic acid, it was turned green by the action of it; when water was added, the colour was loſt.

Although a want of ſucceſs attended my experiments on the dogs and pig, there ſtill remained one, which, if ſucceſsful, would inconteſtibly eſtabliſh the poſſibility of the bile exiſting in the blood-veſſels without a fatal conſequence, and that was, by injecting it into them. For this purpoſe a dog was obtained. An inciſion being made through the integuments of the neck, and the external jugular expoſed, two drachms of dog's bile, previouſly diluted with the ſame quantity of water, were injected. This was conducted in ſuch a manner that not a drop of the

bile was loſt. The vein and integuments being properly ſecured, the dog was let looſe.

A few hours after he appeared very dull, would not quit the place in which he lay; and refuſed the meat that was offered him. This continued for two or three days: he then gradually recovered his appetite, and is now perfectly well.

Thus we have proved the ſtimulus of the bile in the blood veſſels, not to be neceſſarily attended with fatality: and as the comparative experiments between the inflammatory and jaundiced ſerum, indicate ſomething in the latter, which in the reſult with the acids, does not in the leaſt differ from that of healthy ſerum with the addition of bile: I will venture, unfaſhionable as it may appear, to aſcribe to abſorption, what modern theoriſts might attribute to ſympathy: In embracing an antiquated doctrine, I am ſanctioned by experiment. This ſurely ought not to be deſerted for the deceptive fluſhings of a diſeaſed imagination.

FINIS.