

Experiments & observations on the bile / by Jarvis Roebuck.

Contributors

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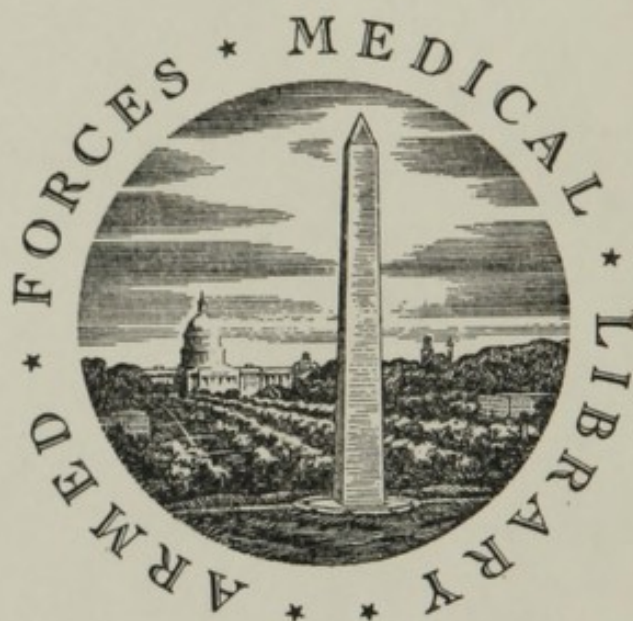
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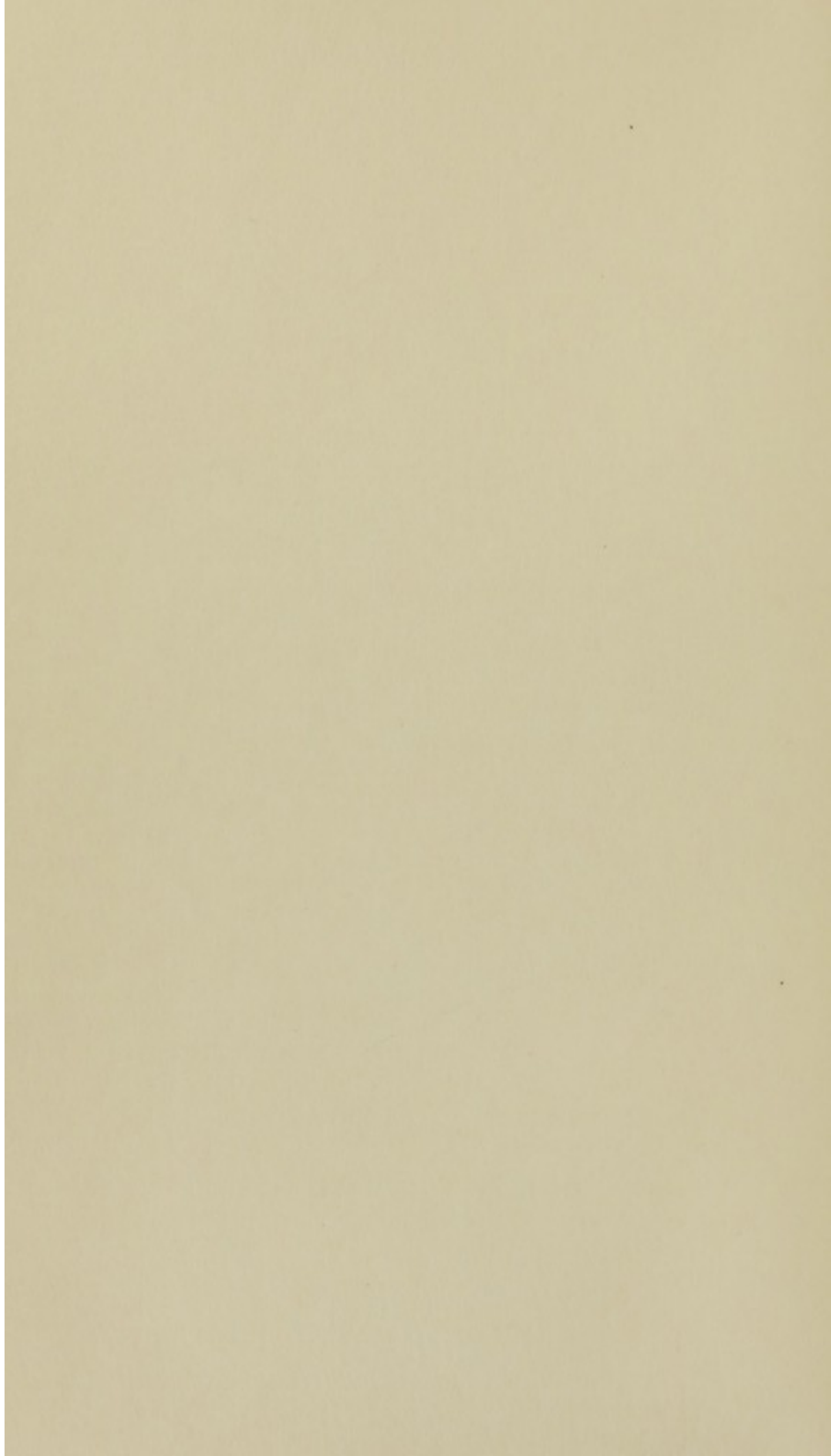
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EXPERIMENTS & OBSERVATIONS

ON

THE BILE,

By JARVIS ROEBUCK,

OF THE ISLAND OF ST. CROIX;

*Honorary Member of the Philadelphia Medical and Chemical
Societies.*

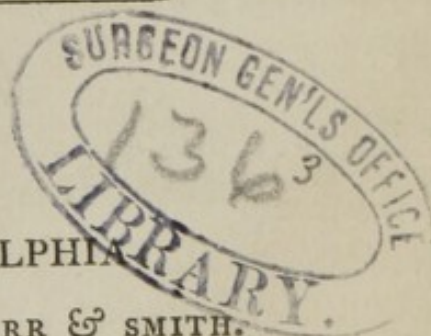
Conamur tenues, grandia.

HOR.

PHILADELPHIA

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.....
1801.



EXPERIMENTS & OBSERVATIONS

ON

THE

THE BILE,

IN CONNECTION WITH THE

BY JARVIS ROBERTS,

OF THE LIAISON OF THE

RESEARCHES OF THE PHYSIOLOGICAL AND CHEMICAL

SCIENCE

IN THE

RESEARCHES OF THE PHYSIOLOGICAL AND CHEMICAL

SCIENCE

AND

IN THE

PHILADELPHIA:

PRINTED BY CARR & SMITH.

1801.

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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,

404511

AN

INAUGURAL

EXPERIMENTAL INQUIRY,

FOR THE DEGREE OF

DOCTOR OF MEDICINE

BY JOHN EWING, M.D.

TRUSTEES AND ALUMNI ASSOCIATES

UNIVERSITY OF PENNSYLVANIA

PHILADELPHIA, PA. 1891

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.

BENJAMIN SMITH BARTON, M.D.

Professor of Medicine in the University of Pennsylvania

and Fellow

of the University of Pennsylvania

in

AN ESSAY ON THE NATURE AND HISTORY OF THE

FEVER WHICH PREVAILED IN THE CITY OF PHILADELPHIA

AND IN THE VICINITY THEREOF IN THE YEAR 1800

BY BENJAMIN SMITH BARTON, M.D.

OF THE UNIVERSITY OF PENNSYLVANIA

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1800

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 Strasburg, 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 saponaceous

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 physiologist, more, than the subject of the present essay. Its presence, in almost all the classes of animals, must have indicated some salutary 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 fæces, 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 chyliification, 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 fecal 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 of the repulsive parts of the unaffiliated 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 fæces 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 consideration 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 reflux 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 alcohol 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 defication 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 crystallization, 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 alcohol, 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 alcohol 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 alcohol 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 alcohol driven off.

To the residue, about an ounce of distilled water was added, for the purpose of obtaining the resinous part which the alcohol 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 alcohol 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 acipenser 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 alcohol, 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 refinous 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 alcohol of galls, no change of colour was produced; nor did the pruffiat 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, fimilar to thofe in the glutinous portion, but infinitely more abundant; were fpread upon it. Was this a portion of the refin held in folution by the alkohol, which the paper had abforbed, and which, after an expofure to the atmofphere, and the confequent diffipation of the fpirituofus part, remained on the paper in a chryftallized form? Or was it the fize, with which the paper was impregnated? No fuch appearance, it muft be confeffed, was obferved in any experiments with the bile of other animals: If it proceeded from the paper, it fhould have taken place in every experiment. Some of thefe plates when taken up by the point of a knife, and applied to the tongue, were perfectly infipid or without tafte. The proportion of this fubftance was very fmall, probably not more than $1\frac{1}{2}$ or two grains.

After repeated additions of diftilled water, and evaporations of the liquor which paffed the filter, the falts which remained were coloured with a fmall portion of a brown extractive matter, very fimilar in confiftence to a denfe fyrup, which made them adhere to every thing that came in contaét with them, which extractive matter I could not feparate

from them, as it was equally soluble, in alcohol 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 muriate 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 discoloured: 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 alcohol 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 carnivorous; 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 presence 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 alcohol, 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 to remain 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 viscosity, 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 fundry 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 healthy 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 $\frac{1}{2}$ 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 vividity, and of a straw colour.

EXPERIMENT V.

To $\frac{1}{2}$ 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 caloric.

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 hardness 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 agglutinated 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 lessen 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.

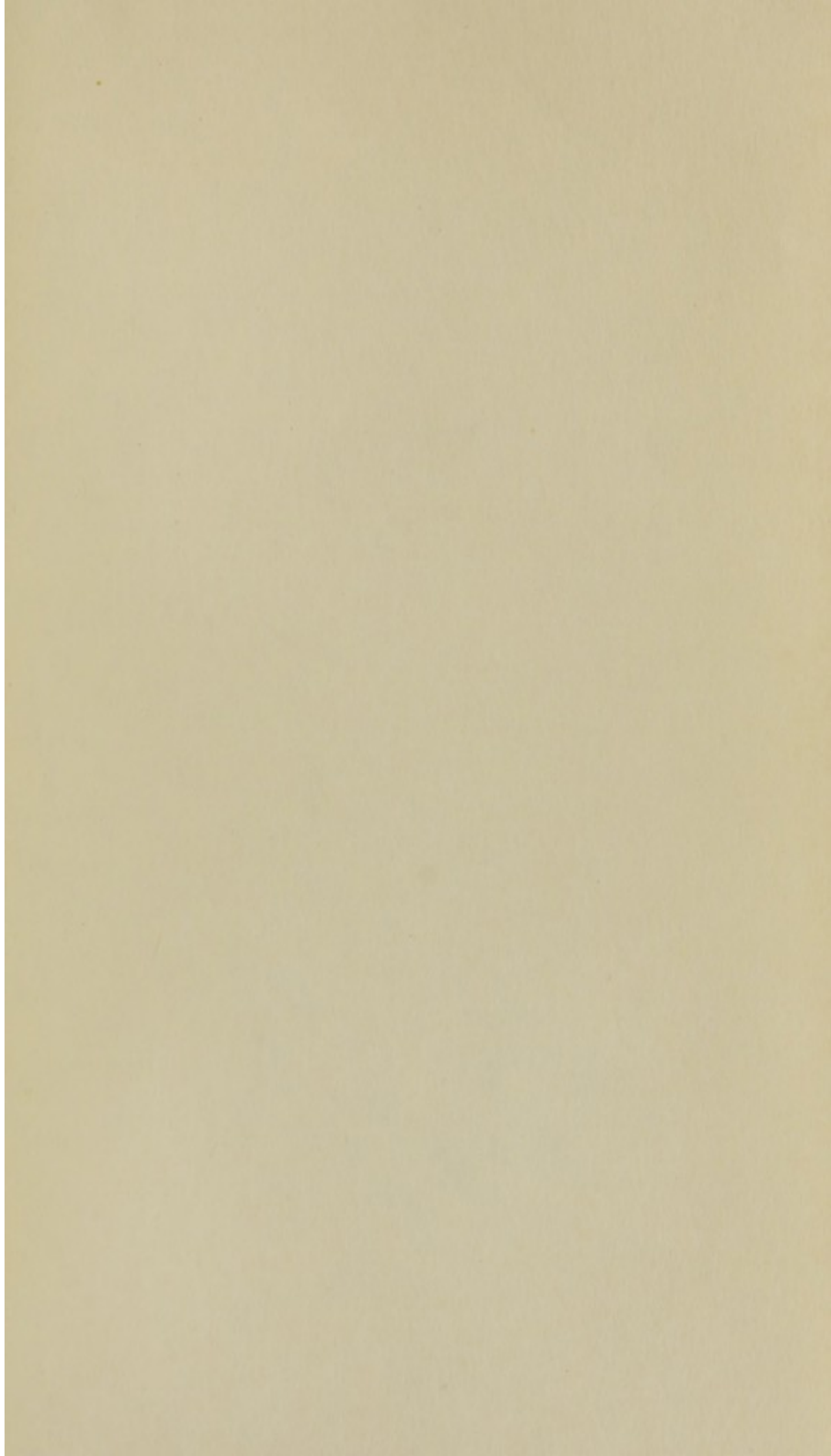
Although a want of success attended my experiments on the dogs and pig, there still remained one, which, if successful, would incontestibly establish the possibility of the bile existing in the blood-vessels without a fatal consequence, and that was, by injecting it into them. For this purpose a dog 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 flashings of a diseased imagination.

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