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AN
 E S S A Y
 ON
 GLANDULAR APPETENCY,
 OR THE
 ABSORPTION OF MEDICINES.

BY
 THOMAS WALMSLEY,
 OF PENNSYLVANIA.

✓

ONE OF THE VICE PRESIDENTS OF THE AMERICAN LINGUISTIC SOCIETY, AND HONORARY MEMBER OF THE PHILADELPHIA MEDICAL SOCIETY.

The wide, the unbounded prospect lies before me,
 But shadows, clouds, and darkness rest upon it.

ADDISON.

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—1803.—

AN
INAUGURAL DISSERTATION,
FOR
THE DEGREE
OF
DOCTOR OF MEDICINE ;
SUBMITTED TO THE EXAMINATION
OF THE
REVEREND JOHN ANDREWS, D. D.
(PROVOST PRO TEMPORE),
THE
TRUSTEES AND MEDICAL PROFESSORS
OF THE
UNIVERSITY OF PENNSYLVANIA,
ON THE
EIGHTH DAY OF JUNE, 1803.

AN
INAUGURAL DISSERTATION
FOR
THE DEGREE
DOCTOR OF MEDICINE
Submitted to the Examination
of the Faculty of the
UNIVERSITY OF PENNSYLVANIA
ON THE
EIGHTH DAY OF JUNE, 1803.

Sir John Lymb Doney M.D.

as a mark of sincere respect,

From

The Author
L

TO
BENJAMIN SMITH BARTON, M. D.

PROFESSOR OF

MATERIA MEDICA, BOTANY, AND
NATURAL HISTORY,

IN THE UNIVERSITY OF PENNSYLVANIA :

DEAR SIR,

DEDICATIONS, made up with professions of gratitude and esteem, have such a formal appearance, that they are now considered as *things* of course : but whilst I admit that the idea is not without foundation, I must declare it is not consonant to my present feelings. The opportunity thus afforded me, publicly to acknowledge, the many obligations your attention has laid me under, compensates for the disagreeable necessity of submitting to the world a very imperfect essay.

ACCEPT, then, my most sincere thanks, and permit me, when I shall no longer be your pupil, to call myself one of your friends.

T. WALMSLEY.

TO
DOCTOR MAHLON GREGG,
OF ATTLEBOROUGH.

DEAR SIR,

AS the first fruits of my medical education, this Essay can be dedicated to no one with more propriety, than to him from whom my first ideas of the science were derived. The many opportunities of improvement your practice afforded me, and the interest you took in the direction of my studies, will always be recollected with the greatest satisfaction.

THAT you may long continue affording relief to those in distress, and enjoy that prosperity you so justly merit, is the unfeigned wish of,

Your Friend,

And former Pupil,

THE AUTHOR.

TO
WILLIAM STEPHEN JACOBS, M. D.

AND

JOHN MOORE, M. D.

TWO GENTLEMEN, WHOSE FRIENDSHIP

IT DOES ME

HONOR TO POSSESS ;

AND,

WHO HAVE MERITED MY WARMEST REGARD, BY THEIR

DISINTERESTED FAVORS,

AND ASSISTANCE IN MY EXPERIMENTS :

THIS ESSAY,

IS VERY RESPECTFULLY INSCRIBED,

BY

THEIR MUCH OBLIGED FRIEND,

THE AUTHOR.

TO
WILLIAM STEPHEN JACOBS, M. D.

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TWO GENTLEMEN, WHOSE FRIENDSHIP

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THIS ESSAY,

IS VERY RESPECTFULLY INSCRIBED,

BY

THEIR MOST OBLIGED FRIEND,

THE AUTHOR.

INTRODUCTION.

PERHAPS Physiology does not involve a question more difficult of investigation, or of greater importance in a practical view, than the subject of the following sheets.

THAT we are very much influenced in practice by theoretical opinions, is a notorious fact; it becomes, of course a matter of the highest consequence that they should be founded in truth.

TRUTH, therefore, is the grand object of enquiry; and, any attempt to ascertain it, however feeble or unsuccessful, will be esteemed by the candid, as at least a laudable effort. To unveil the mysteries of nature, to point out the course she pursues, and dispel the clouds of prejudice which obscure half the knowledge we have already acquired, is not for me; some future Spallanzani, some second Hunter, will alone be adequate to this. 'Tis not to be supposed that a youthful arm should bend the bow of Ulysses, or that a novice in a science should develop its

principles. All that can be expected from me, is, a few undecisive experiments, and I can hope to do little more than relate them with candour.

THAT no absolute certainty, as regards a general doctrine, can be inferred from any experiments hitherto made on this subject, is not surprising, when we consider, that, by far the greater number of them are negative, and therefore objectionable for several reasons. In the first place, it is said that negative proof is no proof when opposed by positive; and there are facts upon record which deserve this name. Secondly, it is possible that a principle cause of our constantly failing to detect different substances in experiments of this kind, is the uncertainty and inaccuracy of our tests. Iron is that substance for which we have the most decisive tests, and the presence of which we are able to determine with the greatest certainty; yet, if it exist in the condition of an oxyd, or if it be super-saturated with an acid, the prussiates of lime and pot-ash, and the alcohol of galls, are equally ineffectual in detecting its presence.*

HENCE, when we experiment with iron, we are obliged to use those preparations which are best accommodated to our tests; and these are generally of the most active kind; of course, if

* See Section 3d. Experiments 1, 2, 3, 4 & 5.

a considerable quantity (of the sulphate for instance) be taken into the stomach, or thrown into the intestines, a violent inflammation is the consequence, and the healthy action of the absorbents being thereby destroyed, the experiment is unfair. Or if the quantity be so small as not to excite inflammation, we should hardly expect that in a mass of *transparent* fluid equal to the blood, any marks of its presence would be evident; and when we take into consideration that the blood is not transparent, but opake and of a dark colour, we shall at once be convinced, that to detect the largest portion of iron that can be supposed to enter the circulation, if not altogether impossible, is no easy matter.

THESE objections and many others, apply in a greater or less degree to almost every experiment hitherto made, on the question of absorption; my own are by no means an exception, but how far my endeavours to obviate some of them have succeeded, the candid reader must determine.

SEEING then, that our present knowledge of Physiology, and Chemistry more particularly, does not enable us to decide positively on this important question, we are obliged to make such experiments as we can make, and use such facts as are sufficiently authenticated, for the purpose of regulating our opinions; and leave the ulti-

mate decision to a future period, when Chemistry, as well as "Physiology, shall assume one of the highest stations in the range of *sure*, as well as splendid sciences."

At present, as "we cannot reason but from what we know," I am the less ambitious of giving even my own opinion. The facts we have upon record, and several of them from the most respectable authorities, are so contradictory that the whole subject is a complete chaos. I shall therefore give a brief statement of some of the more important of these facts, on both sides of the question; relate my own experiments in a manner as concise as possible, and leave the reader pretty much to the direction of his own good sense, in concluding upon the general doctrine.

ESSAY ON ABSORPTION.

SECTION I.

UPON WHAT THE DOCTRINE OF ABSORPTION IS
FOUNDED.

TO trace this opinion to its origin, is no difficult matter. Ever anxious to account for the phenomena which are presented to his senses, man has uniformly suffered his imagination to run at large; and thus it has so frequently happened, that systems have been formed of visionary materials, in addition to a few facts, and supported by arguments deduced from themselves.

THIS was precisely the case with the humoral pathology: the doctrine once conceived, omnipotent fancy could make every thing bend to its support; and when facts were too short, hypotheses supplied the deficiency.

IT is unnecessary to say any thing further on this subject; at present the Boerhaavian tenets have but few advocates. It is now much more fashionable to place every disease in a morbid action of the solids; and, of course, that on these, medicines must act. But how? Still they must be carried into the circulation.

HENCE, very cogent arguments are inferred to prove the doctrine of absorption; in this place, however, we shall pass them by, to take

notice of some *facts*, on which it more immediately rests.

CASTELLI tells us, that he found two ounces of mercury in the body of a woman who had been salivated; Wepfer* mentions having seen a large quantity flow from the occipital foramen; and Schenklius says, that a man, after three mercurial frictions, ejected a cup-full of the metal from his stomach. Still more wonderful is the experiment of Schneiderius: He fed a hen on gold and silver leaf of the thinnest texture, and found that she laid eggs with silver shells, and had her viscera gilded †. But such cases seem to us too much like miracles to enforce belief; we shall therefore only take notice of such as carry somewhat of probability with them.

INSTANCES of this kind have occurred so very seldom, that not a few are inclined to discredit them altogether, and attribute the whole to some fallacy of observation. But if we place no confidence in the authority of Lister, Musgrave, Haller, Broadbelt, and several others, we may discard every thing the records of medicine or history contain. We shall therefore proceed to give an abstract of the evidence afforded by these gentlemen.

“ DOCTOR LISTER caused a dog to be fed:
“ four hours after, he opened the abdomen and

* Wepfer is one of the best authorities in ancient medicine; his works on Apoplexy and Hemlock, are still deemed classic. He lived about the year 1680.

† Gallinam bracteis tenuissimis argenteis aureisque pastam, posuisse argentea ova, et habuisse viscera inargentata et deaurata, non credas ex alio referenti Schneidero, cum per annum aliis in periculis, eadem aves hæc metalla fideliter per alvum reddiderent.—Halleri *Elementa Physiologiae*, vol. 6. p. 216.

“ injected into the jejunum an ounce or two of
 “ a clear tincture of indigo; this done, the in-
 “ testine was replaced and the wound stitched up.
 “ An hour and a quarter had elapsed when the
 “ abdomen was again opened; a copious distri-
 “ bution of chyle was observed, and very tur-
 “ gid lacteals, but as white as ever; and tho’
 “ he carefully searched the intestines, the inject-
 “ ed liquor was no where to be perceived.

“ HE gave another dog, which had been
 “ kept fasting forty hours, a very little flesh with-
 “ out water; about five hours afterwards twelve
 “ ounces of a warm tincture of indigo was in-
 “ jected as before; here the intestines were ob-
 “ served to be empty, and not the least appear-
 “ ance of any lacteals in the mesentery. When
 “ three hours had elapsed, the abdomen was
 “ again opened, and upon carefully examining
 “ the mesentery, many lacteals were observed of
 “ an azure colour; some of the larger ones be-
 “ ing divided a thick bluish chyle issued out*”

‘ DR. MUSGRAVE injected into the jejunum
 ‘ of a dog, that had been kept fasting for a day
 ‘ before, about twelve ounces of a solution of
 ‘ indigo; and three hours after, opening the dog,
 ‘ he saw several of the lacteals of a blue colour,
 ‘ which disappeared upon stretching the mesen-
 ‘ tery; an argument that the contents of the
 ‘ vessel, and not the vessel itself, was of a blue
 ‘ colour.

‘ A few days afterwards, repeating the ex-
 ‘ periment on a dog kept fasting for thirty-six
 ‘ hours, but with stone blue instead of indigo, he

* Baddam's Royal Society, vol. 2. page 169.

‘ saw several of the lacteals become of a perfect
 ‘ blue colour within a very few minutes after the
 ‘ injection. And again, the same experiment
 ‘ being repeated on a Spaniel, the doctor ob-
 ‘ served that the thoracic duct, as well as the
 ‘ lacteals, was of a blue colour; though the lat-
 ‘ ter, upon being cut, discharged a liquor of a
 ‘ deeper blue than the former*.

FROM these experiments Drs. Lister and Musgrave deduced some very wild theories, which in this place it is unnecessary to notice; suffice it to say, that their motive was not to prove absorption: this was taken for granted;—To prove therefore what no one doubted, would have been a superfluous undertaking, and of much less importance, than to establish some speculative notions concerning the pathology of diseases, which perhaps never existed. But an experiment of a similar kind was instituted by Dr. Fordyce, for the express purpose of ascertaining the power of the absorbent vessels. He injected indigo into the intestines of a sheep, after the manner of Dr. Lister, and found, in the course of a little time, that the chyle was rendered quite blue†.

ACCIDENT has also thrown into our way some facts, which are perhaps as pointed as the result of any experiment.

IN the winter of 1792, when Mr. Francis Rigbey Broadbelt was prosecuting his dissections at St. Thomas’s hospital, he happened on a subject who had been afflicted with Syphilis; but the death of the patient seemed evidently to have occurred from *universal dropsy*.

* Philosophical Transactions abridged, vol. v. p. 254.

† Fordyce on Digestion, p. 122.

AFTER having removed the muscular parts of the larynx, it was left during the night to dry; on the next morning he was greatly surprised to see it covered with globules of mercury. These were not observable the night before, or had by some means escaped his notice.

THE os hyoides, upon examination, was found much covered with quicksilver, as were likewise the thyroid and cricoid cartilages. By the assistance of a glass, small globules could be discerned in several other parts.*

HALLER himself, did not see mercury in the blood or bones; but from his manner of relating it, we can have little doubt of the authenticity of the fact, though his assertion is too broad. His words are, “Hydrargyrus per intestina immutatus descendit aut in sanguinem quidem recipitur, nihil tamen mutatus, cum fluidus in ossium cellulis, et redivivus reperiatur.”†

THIS is not the only sentence in Haller relative to the absorption of mercury, though others are less positive: he mentions cases on the authority of Jordan, Valvasor, Mead, &c. But knowing the credulity of some of these philosophers, we do not think it necessary to transcribe them; they are, however, to be found in volumes 5 and 6, of the *Elementa Physiologiæ*.

WE derive a principle from nature, which teaches us that the evidence of our own senses

* A prize medal was adjudged to Mr. Broadbelt for this discovery. See *Annals of Medicine*, vol. 5, page 112.

† *Elementa Physiologiæ Halleri*, vol. 6, page 215.

is paramount to the assertions of another. Directed by this principle, I am constrained to believe, that the absorption of mercury is not altogether a whimsical notion. During the last course of lectures, a subject was brought to the anatomical theatre for dissection; we were at that time entirely ignorant of every circumstance relative to his disease, or his treatment. Dr. Jacobs was preparing the larynx for demonstration, and observed on the thyroid cartilage, a singular appearance, which at the moment did not strike his attention very forcibly, as he had not the most distant idea of meeting with such a phenomenon; indeed, the doctor (being an unbeliever) would, *a priori*, have conceived it impossible. Observing, however, a number of small shining particles in the form of globules, he examined them more minutely, and found that by approximating two of them they ran into one, which he could again divide by a knife with a fine edge: in short, they resembled mercury in every particular, and no doubt remains in my mind, but that they were real quicksilver in its metallic state. Doctor Wistar, and several other gentlemen, were of the same opinion, after a very precise examination with a microscope. The quantity was too small to be treated chemically. We afterwards learnt that the patient had been under a heavy course of mercury for syphilis, and had died of *phthisis pulmonalis*.

It may, perhaps, be thought somewhat strange, that we should not have made more particular mention of the cases from Drs. Mead, Hamilton, &c. but we strongly suspect some fallacy of observation, and chose rather to relate only those in which we had less reason to sus-

pect a deception. Doctor Hamilton's experiment will probably be noticed in another place.

As we have nothing more to offer, which can be regarded as positive proof, we shall proceed to give presumptive evidence, from which the doctrine of absorption principally derives its support.

POWERFUL arguments are inferred on this side the question, from certain circumstances relative to the exhibition and effects of mercury. That this medicine, in order to excite ptyalism, must be administered *for a length of time*, and in small doses, thereby to avoid catharsis, is known to every one : it is likewise to be observed that the salivation frequently continues, but little diminished in violence, and in some cases even becomes more profuse, several days after the mercury is laid aside. And when we consider that other medicines, which are acknowledged to act on the stomach, produce their effects immediately, we cannot but allow, that the reasons for believing in the absorption of *mercury* are not very trifling.

IN addition to these, there are some *facts* which render the opinion still more probable. The case of Dr. Barton, as related by himself, is so very interesting, that we shall not fail to give it in full. The Doctor was attending a patient in a very obstinate remitting fever, for which he thought proper to prescribe mercury : the medicine was exhibited in the form of pills without any external application ; in a few days a profuse ptyalism came on ; the doctor visited him two and three times a day, and merely by breathing the air of the sick man's chamber, was himself

greatly salivated. The nurse was affected in the same way, but not so severely. Doctor Barton had not taken a grain of mercury for two years ; but it is proper to mention that he is remarkably disposed to be salivated by the most trifling quantity. Three or four grains taken over night will excite a ptyalism by the next morning.

THE case related by Doctor Cooper is little less decisive. In the year 1795, the doctor was attending a lady with remitting fever ; he salivated her with *calomel*. Her child was not with her except when sucking, but was violently affected with ptyalism, and finally fell a victim to the disease, though it was put to a nurse. Doctor Hodge accounts for this by supposing the mercury had been externally applied, but that was not the case.

I AM likewise informed by my fellow student, Mr. Grimes, that his former preceptor, Doctor Smelt, of Augusta, Georgia, is constantly in the practice of curing venereal affections in children, by administering mercury to the mother. This practice Mr. Grimes has seen so repeatedly among the negroes in Georgia, whose children are frequently born with the disease upon them, that he has no doubt of its general efficacy.*

SOME facts, apparently pretty decisive, have come under the observation of different persons, relative to the absorption of other articles ; a few of the most important will be mentioned.

* Since writing the above, I have learned that Dr. Wistar is in the same practice.

WE shall say nothing of madder, the cactus opuntia, asparagus, or of the alliacea, in this place; they are all nutritious articles; to the latter, however, we shall probably pay some attention elsewhere.

TURPENTINE will also come more properly under another head.

A FEW days since, Dr. A. Gregg, jun. had occasion to prescribe a cathartic for a woman who had a sucking infant at her breast. The prescription was Pulv. rad. podophyll. peltat. and Crem. tart. of each fifteen grains. This was divided in two doses, one of which she was to take immediately, the other in two hours. The first had no effect on the mother, but the child was considerably purged in about thirty-five minutes.*

DOCTOR BARTON relates † a case which is very much in point. A patient of his, in Typhus fever, had continued the use of camphor for a considerable time, when the smell of the medicine was observed in his perspiration. A case so singular could not fail to attract attention; the doctor therefore ordered the skin to be washed with warm water and wiped dry; when the skin had again become moist, the perspiration was wiped off with a clean linen cloth, which thereby became evidently impregnated with the odour of camphor. It is necessary here to observe that the

* Thirty-five minutes would seem a short time for the cathartic to pass the intestines, the lacteals, the routine of the circulation, the mammillary glands, and be secreted in the milk. It would almost lead us to suppose that there is a shorter passage to the mammillary glands than the one above mentioned.

† Lectures on Materia Medica, article Camphor.

medicine was not kept in the patient's room, but mixed below stairs, and carried to him by his mother.

WE have now related a sufficient number of well authenticated facts, to convince any one that mercury, and certain other active substances, have been, and are occasionally, taken up by the absorbent vessels, carried into the circulation, and thrown out by the secretions, or deposited in different parts of the body. On these few facts, with some probabilities, the doctrine of absorption rests.

SECTION II.

WE come now to consider the objections to the doctrine of absorption. We shall first take notice of such as apply generally; and secondly, such as more especially apply to particular articles.

IN our explanations, “ we are to admit of no causes but what are true, and no more than are sufficient to explain the phenomena.”

As we are obliged, in very many cases, to admit of a sympathetic action, which we shall endeavour to make appear, this axiom will of course, stand amongst the most powerful objections.

THE largest quantity of a substance that can be supposed to enter the circulation, is so small, that when it becomes diffused through the mass of blood, the portion applied to a particular part

is too insufficient to produce any effect.* And the almost instantaneous operation of many substances, excludes the possibility of their having been absorbed, previous to the exertion of their powers. Hence we are necessitated to allow, that an action on the stomach is capable of influencing the whole system ; and as certain articles produce their effects only in particular parts, we are also obliged to admit of *specific action*.

SEEING, therefore, that specific sympathy does exist, we cannot strictly adhere to the rules of philosophizing, and call in absorption to assist us in explaining the *modus operandi* of medicines, until it is no longer doubted ; especially, as the difficulty is not thereby diminished.

TAKE, for instance, cantharides ; a very ordinary effect of which is to produce strangury. If we admit that they are absorbed, we must also acknowledge that they are specifically determined to the bladder ; and specific determination is not accounted for with less difficulty than specific action. If cantharides were absorbed to produce strangury, it would seem strange that a second blister should remove the affection ; this, however, is the constant practice of Doctor Barton.

OR, if we take the *Uva Ursi* as an instance, we shall find ourselves in the same predicament. Admit that it is absorbed, it does not act by chemical combination, or attraction, otherwise it would dissolve calculi already formed, which is

* Cullen's *Materia Medica*, vol. 2d. page 5. 4to edition.

not the case.* But it is well known that Uva Ursi alleviates the symptoms of lithiasis, and prevents the deposition of calculous matter; this, then, it must do by altering the secretion of urine, which can only be affected by a specific action on the kidneys. The question then, is, whether ought we to say, that a specific action is produced in the kidneys through the medium of the stomach; or, that a specific determination carries the medicine to the kidneys, where (still by a specific stimulus) the same action is excited, and a change effected in the secretion of urine.

WE cannot but consider it as a powerful objection to the doctrine of absorption, that medicines *do not always* produce the same effects. If they were specifically determined to any part, they ought uniformly to act upon that part, at least more especially than on any other, which is not the fact. But as sympathy depends, in some inexplicable manner, on the sensorial power, it is easy to conceive, that the different degrees of accumulation, which may obtain at different times, and in different parts, would have the effect of deriving action into these parts.

CATHARTICS frequently purge, though almost immediately returned by vomiting.† We learn from this, that the presence of a medicine

* Gerardi says, he obtained an acid from the Uva Ursi by distillation, and to this he attributes all the good effects of the medicine. I have had an opportunity of seeing this experiment several times repeated by my fellow graduate Mr. Mitchell, but never saw any evidence of an acid. And, indeed, were we disposed to give full credit to Gerardi's experiment, we might, with some propriety, doubt his opinion, that the efficacy of the medicine resided in the acid; for the carbonate of soda, lime water, the beards of leeks, and even pure alkaline salts, have occasionally been found to relieve the disease as readily as Uva Ursi.

† Facts, tending to show the connection of the stomach with life, disease and recovery.

in a particular part, is not necessary to produce its ordinary effects, provided a part of the system has been under its influence.

BUT the great objection is, that ninety-nine experiments in the hundred, which have been made with a view to decide the agitated question, have resulted in the negative; perhaps even a greater proportion than this: and we are far from being certain, that a number of those which seem to favour the affirmative, are entirely accurate. Any one who is in the least acquainted with the casualties to which experiments are subject, cannot be surprized at this; nor shall we be accused of impiety for suggesting, that even Hamilton *might* be mistaken. This gentleman tells us, that he obtained mercury in globules, by slowly evaporating the milk of a woman, under a salivation.

OUR reasons for doubting this, are, 1st. That it is a solitary case. 2d. The same experiment has been performed on the saliva, blood, &c. of patients with ptyalism, but without detecting mercury.* 3d. An appearance very easily mistaken for mercury, is sometimes produced, when the saliva of a person in health is used.†

THE objections to Dr. Hamilton's experiment apply only to mercury; we shall now en-

* See experiments by Dr. Slare. See also section 3d. experiments 31 and 34.

† See Dr. Hodge's Inaugural Thesis, pages 36 and 38.

My fellow graduate, Mr. Downey, who is experimenting on the *Sanguinaria Canadensis*, a few days since, had a profuse discharge of viscid saliva, excited by that root. Without exactly knowing why, he spit several times on a hot shovel; after the water was driven off, he examined the residuum, and perceived little globules, not to be distinguished from mercury, but with the greatest difficulty. He had not taken a particle of mercury.

deavour to point out why certain facts, which have been adduced on the affirmative, if they do not operate the other way, are at all events equivocal.

WHEN mercury has been used for some time, whether by external or internal application, the gums change their colour, and previous to a considerable increase of saliva, a preternatural taste is excited in the mouth, which continues for some time after the medicine is laid aside : hence it is inferred, that the mercury must be absorbed. Unfortunately, however, it is not the taste of mercury, but of *copper*, that is perceived ; and when we recollect that the same sensation is produced by other substances, we certainly ought not to regard the taste of *copper* as a proof of the presence of *mercury*.

THAT lead, antimony, &c. do excite the taste of copper, is a fact too well known to be insisted on here ; yet we shall give an instance of each.

PROFESSOR Thunberg, during his voyage to the Cape of Good Hope, was accidentally salivated by lead, and experienced the same copper taste that is so constantly produced by mercury.*

DOCTOR Barton was salivated by tartar emetic, and experienced a similar effect : as is related by himself.†

CERTAIN vegetable substances which have occasionally effected a salivation, have also in-

* Voyage to the Cape of Good Hope. C. P. Thunberg.

† Lectures on Materia Medica.

duced the same taste. Whether this holds good with Hemlock, Digitalis, Polygala Senega, Opium, Camphor, &c. I am unable to determine; but it was verified in the case of Mr. Downey, who had not only the copper taste, but that peculiar odour in his breath, which every one associates with mercury; except, perhaps it might be rather more fetid.*

HENCE it is sufficiently evident, that the copper taste is not to be referred to an absorption of mercury, but to a peculiar action excited in the organs of taste.

IN illustration of this position, we might mention a fact relative to a new species of *Lacerta*, an account of which Dr. Barton has lately presented to the Philosophical Society. This animal upon being irritated, discharges from several parts of his body, a very viscid white matter; which, when applied to the tongue, produces a sensation not unlike that we have so often had occasion to mention. It is, however, to be observed, that for the first ten or twelve seconds, nothing but the insipid viscid matter is perceived, when the taste is suddenly excited, as I have more than once witnessed.

Now, if there was any positive taste in the matter itself, sensation ought to be coeval with the application of the fluid, as is the case with other sapid bodies: but, as this does not happen, and as the sensation, when produced, is over the whole surface of the tongue, we must account for the phenomenon by supposing that

* We have mentioned this case page 25 in a note.

organ to be specifically stimulated ; for a sufficient length of time does not elapse to allow the matter to be absorbed.

It is unnecessary in this place to dwell longer on the absorption of mercury ; if it has ever whitened a piece of money in the mouth, or been detected in the urine* (which, from our experiments, we are inclined to doubt) such instances are too uncommon to be made the ground work of a general doctrine.

SEVERAL objections that we have made to the absorption of mercury, apply with equal force to turpentine. The exhibition of this substance, in whatever manner applied, is constantly followed by a peculiar smell in the urine, which is considered as a proof of its absorption. Here again, however, 'tis not the smell of turpentine, but of *violets* ; and nothing surely can be more preposterous, than to regard this as an indication of the presence of turpentine ; otherwise we might expect to find an abundance of that substance in our meadows, during the spring season.

It is, nevertheless, very rational to consider the violet smell of the urine as the *effect* of terebinthinate medicines, because it so constantly follows their use : we object only to the manner in which it is explained. Almost every one believes the tides to be caused by the moon ; yet, no one pretends to say, that a portion of the moon has fallen into the water, and thus, by giving it additional bulk, encreases its height. This phenomenon is very generally allowed to be the ef-

* Haller's *Elementa Physiologiae*, vol. 5, page 85.

fect of attraction, or gravitation ; about which we know just as much as we do of sympathy ; that is, we know such powers exist.

IN opposition to the absorption of lead, a number of arguments may be adduced ; some of which are generally considered as favorable to the opinion.

THE workers of the lead mines in Sweden were formerly so subject to cholica pictonum,* and so large a number of them fell victims to the disease, that government was obliged to replenish the mines every two or three years ; at present, however, the disease is scarcely known among them ; the reason of which is, that the workmen are now in the habit of eating large quantities of lard or butter.†

A POTTER in this city makes it a constant practice to give his workmen fat soup, whenever they are glazing their vessels. He is completely convinced of its efficacy.‡

THAT the bad effects of lead, when they are general, are produced by its action in the stomach, would appear from the prophylactic powers of fat. It is to be recollected, that fat is a digestible and nutritious substance ; of course, if it even entered the absorbents combined with the lead, so as to *obtund its acrimony*, the assimilating power of these vessels would soon leave the lead by itself, to produce all its bad effects.

* De Haen's Ratio Medendi.

† Professor Barton's lectures on Materia Medica.

‡ Do.

Do.

WE can state a few facts relative to the operation of lead, which amount to a positive proof, that it is not absorbed previous to its action on the system.

DOCTOR BARTON cured a profuse uterine hæmorrhage, which threatened life, by two grains of *saccharum saturni*: this was effected within half an hour after the exhibition of the medicine. The Doctor likewise stopt a very alarming epistaxis, in a few minutes by the same means. I have seen many instances of this kind; where the small quantity, its sudden operation and powerful effects, convinced me that nothing was to be attributed to its absorption.*

ALTHOUGH we have mentioned, in a former part of this essay, a decisive instance of the possibility of a child being affected by a cathartic given to its mother, such instances are very rare. Dr. Young says, he never saw a single case; † and Dr. Cullen says, that infants are not affected by sucking intoxicated nurses. ‡ Hence, we are justified in an opinion, that wherever such cases do occur, they are preternatural, and we are to suspect something morbid.

IT is, nevertheless, a well known fact, that not only the milk, but the very flesh of animals feeding on garlic, has both the smell and taste of their aliment. But garlic is nutritious; and we shall admit the absorption of all “such matters” as are in any manner qualified to supply the

* See page 1st. section 2d.

† See his treatise on milk.

‡ Cullen's *Materia Medica*, vol. 1st. page 324, 4to edition.

“ substance of the body, whether solid or fluid.” We are yet ignorant how far the process of assimilation must be carried to effect this purpose ; certain it is, however, that in the less perfect animals, assimilation is by no means so complete as in man. Innumerable facts might be brought in proof of this ; as well as to prove that the absorbent system is possessed of an appetency, either sentient or mechanical.* Whence it happens, that while the vessels properly perform their functions, no substance incapable of assimilation, or in other words, no substance, which if admitted into the circulation, would be productive of evil consequences, is suffered to pass.

CONCERNING the poisonous quality of certain fish, as the Baracuta, the Sparus erythrinus, † &c. as well as some birds, we have but little to say. Doctor Hodge has treated the subject very ingeniously indeed, and his explanation is much more than probable. Should any future experiments prove, that those fish derive their poisonous quality from their *proper* food, it would only serve still more clearly to convince us, that in man, the process of assimilation is carried much farther than in many other animals.

BUT our experiments on frogs, which are hereafter to be mentioned, give us good reason to suspect, that in certain conditions of the system, *transudation may take place during the life of the animal* (at any rate in the lower order of animals.) This being the case, I confess I should not be very confident of escap-

* Some of our experiments we hope will shew this.

† This is the red Sea-Bream, by eating which, the crew of the ship Resolution was salivated. BARTON'S Lectures on Materia Medica.

ing all danger, were I to eat the Baracuta, after every precaution had been taken, as mentioned by Dr. Hodge.

THE fish, it seems, is poisonous only at one season of the year. Transudation only happened in Frogs, at one season of the year. There appears to be a great analogy in this respect; I have not had an opportunity of seeing the fish, and cannot determine with certainty.

SECTION III.

Non fingendum aut excogitandum, sed inveniendum,
quod natura faciat vel fecit.

LORD BACON.

HAVING, in the preceeding pages, given an imperfect sketch of the facts and arguments on both sides of the disputed question, we shall relate some experiments; the greater number of which were made whilst the author *implicitly* believed in the doctrine of absorption; on which account he may at any rate, aspire to the merit of being candid.

WE do not mean to take up much of our time by inductions as we go on, but shall relate the experiments as they were made; and subjoin a few general conclusions.

THE urine, as it is secreted by a gland highly organized, and peculiarly adapted to the purpose, is one of those fluids in which we could with least probability expect to find iron, mercury, &c. But again, it is such an heterogenous compound, that one entirely ignorant of its analysis, would not be more surprized at meeting with these metals, than with many substances which it does actually contain. It is however, asserted by Mr. Lorry, that the urine of persons who have taken large quantities of chalybeate medicines, will be turned black by an infusion of galls. Mr. Cruikshank on the contrary, believes this circumstance never to happen. Having, as I thought, a very favourable opportunity, I was resolved to put it to the test of experiment.

EXPERIMENT I.

FROM a patient of Dr. Barton's, who had taken, in the course of a month or six weeks, 1150 grains of rubigo ferri, I obtained a quantity of urine; to one portion of which I added prussiate of lime, to another prussiate of pot-ash, to a third alcohol of galls; no change of colour was observable in either case.

EXPERIMENT II.

TO one ounce of urine, I added 5 grains of rubigo ferri, and kept it at the temperature of 96 degrees for 24 hours. The result of this experiment in no wise differed from the former.

OXYDS, 'tis true, are insoluble in water, and the rubigo ferri is an oxyd: on this account objections might be made to the above experiments, were it not that in the same urine, an excess of acid was proved to obtain, by the simple test of litmus paper. This acid was, in all probability, the phosphoric, or lithiasic; we ought, therefore, to have suspected iron present in the form of a neutral salt (rather than an oxyd) from which it would have been precipitated by the alcohol of galls.

WILLING, however, to remove every objection, as far as lay in my power, I thought it might be prudent to try the effect of some acids, whose affinities are better understood than those of the above mentioned.

EXPERIMENT III.

Two ounces of urine, obtained from the patient before alluded to, was evaporated to dryness; twelve drops of muriatic acid were added to the residuum, which was suffered to stand for half an hour, in a warm temperature, at which time I added two ounces of water, and applied a low heat for three hours longer; after which the whole was divided into several portions; to the first I added alcohol of galls, to the second prussiate of pot-ash, to the third prussiate of lime; the galls produced no change, but the prussiates struck a dark blue colour.

THIS experiment was repeated with the sulphuric acid; the result was precisely the same. It was also repeated (on a smaller scale) by adding the acid previous to evaporation; still no change was effected by the alcohol of galls: of course, I concluded the test was inaccurate, and that I had certainly detected iron by the prussiates. But upon adding some of the same test to a quart of water containing one grain of copperas, a dark colour was instantly produced; which convinced me it was not so bad, as I had been led to believe.

THIS naturally induced me to suspect some deception in the experiments, made with the prussiates of lime and pot-ash: the following experiment was therefore made.

EXPERIMENT IV.

To two ounces of pure water, containing three drops of muriatic acid, I added the prussiate of lime, and was immediately surprized by the appearance of a blue colour. The sulphuric acid was found to have the same effect; though in a less degree; nor was the result different when I used the prussiate of pot-ash.

HENCE it is evident, that to determine concerning the presence of iron, is not so easy as is generally imagined; for, if it be in the condition of an oxyd, or super-saturated with an acid, the alcohol of galls will afford no criterion; and the prussiates of lime, and pot-ash, strike a blue colour with the muriatic and sulphuric acids, when no iron is present.

EXPERIMENT V.

I TOOK two portions of urine, obtained from Dr. Barton's patient; to one I added a few grains of rubigo ferri, to both ten drops of muriatic acid, and applied heat for three hours; the acid was then accurately saturated by quick-lime, and the different portions tested by alcohol of galls; the first threw down a precipitate somewhat coloured, the other was completely white.

THEREFORE, as it is possible to detect iron by the above process, even though it exist in the condition of an oxyd, I am obliged to suppose that it was not present in the urine of the patient who had taken it in such large quantities.

EXPERIMENT VI.

My next object was, if possible, to detect certain articles in the thoracic duct, or blood itself. With a view to this effect, I threw into the rectum of a kitten one ounce of the watery solution of gamboge* containing a portion of the sulphate of iron.† Four hours afterwards, a small quantity of chyle was obtained from the thoracic duct, by the assistance of Dr. Jacobs; alcohol of galls caused immediate coagulation, but no black tinge; nor was the colouring matter of the gamboge any where to be perceived, except in the alimentary canal.

EXPERIMENT VII.

HALF an ounce of ink was used in this experiment, and the kitten suffered to live twenty-one hours. The intestines as far as the valve of the ilium, were found distended to their full size, and appeared of a black colour; when they were opened nothing was found but a small quantity of yellow mucus, the coats being considerably thickened: the black colour was evidently derived from the injection, though somewhat of sphacelation had began. The lacteals were not in the least altered in appearance; nor could we discern any portion of colouring matter in the thoracic duct.

* The gamboge was added to render the fluid visible in the lymphatics, in case it should be absorbed.

† Recto ligato, injectio non redderetur.

EXPERIMENT VIII.

AN incision being made through the abdominal muscles and peritoneum of a kitten, I injected one ounce of ink into the cavity ; but the struggles and scratching of the animal, prevented me from closing the aperture as completely as I could have wished. An accident obliged me to leave the place for two or three hours, and when I returned I found the kitten dead. On removing the skin, which, for some distance round the incision was completely dyed, I found the muscles on the anterior part of the abdomen quite black ; this appearance was not confined to their superficies, but obtained through their whole substance. The lacteals were as transparent as usual, and were in every respect entirely natural : the intestines were black, but when I wiped them with a sponge the colour disappeared, which was not the case with the muscles. The cavity of the abdomen was nearly full of a black fluid, more diluted than the ink injected.

EXPERIMENT IX.

ONE ounce of water saturated with copperas, and as much solution of gamboge was administered per rectum, to a large snapping tortoise which was not ejected : the next day I gave him two ounces of the sulphate, and one of gamboge, using the same precaution as in experiment sixth ; seven hours after this, I proceeded to dissection. The lymphatics are naturally large and numerous in this animal, which induced me to make it the subject of an experiment : they were

in the present case, not at all coloured by the gamboge, nor did the alcohol of galls alter the fluid which they contained, except by coagulation. After much difficulty, the thoracic duct was secured, and punctured; a quantity of chyle issued forth, which was quite transparent, and simply coagulated by the addition of alcohol of galls, without the slightest tinge; although a black colour instantly appeared when I added the smallest quantity of the sulphate of iron. Thro' the greater part of the intestines the iron was easily detected.

EXPERIMENT X.

HAVING kept a dog thirty-six hours without food, an incision was made into the cavity of the abdomen, and three ounces of copperas water thrown into the jejunum; the intestine being secured by ligatures was returned, and the lips of the wound brought together by stitches. Six hours afterwards the dog was hung, and the thoracic duct, lymphatics and mesenteric glands carefully examined; in none of which was there any indication of the presence of iron, when the proper tests were applied.

EXPERIMENT XI.

A DOG was kept upon a quantity of food barely sufficient to sustain life, for near two months; during which time he was frequently bled. At nine o'clock, A. M. four ounces of ink were thrown into the jejunum, after the manner of Doctors Lister and Musgrave. When six hours

had elapsed, the dog was killed; a very large quantity of *transparent* chyle was obtained from the thoracic duct. The lymphatics were, certainly, not all of the same colour, but this did not depend upon the contained fluid; for when that was pressed away, the difference was still observable, and when the chyle was let out, it was quite limpid.

THE foregoing experiments, in addition to those of Doctor Hodge, were sufficient to convince me, that if the absorbents could, by any process, be made to take up iron, it was not to be done in this way.* Still, however, I could not give up the idea: by using small doses I might succeed. We know not the utmost extent of habit; many nations live upon food, which, it would seem, nature had never designed for them. Certain animals, by custom, are brought to digest substances, which, at first, the stomach even rejected. It is now well known, that the horses at Hudson's Bay subsist upon flesh; † and perhaps no animal is more purely phytivorous than the horse. Daily observation teaches us, that food, to which we are not accustomed, is frequently productive of disagreeable effects, and rarely admits of a healthy digestion till its use has been continued for some time; but, at length, becomes salutary.

REASONING therefore, from analogy, I became much more confident of success: Doctor Barton's experiments were directly in point; a variety of vegetables, on which he experimented,

* See introduction, page 11.

† Barton's lectures on Natural History.

refused to take up iron at first, but after a while, absorbed it in abundance.* An ingenious philosopher, Dr. George Pearson, has lately discovered, by some very interesting experiments, that sulphate of iron is a most powerful manure : and the most accurate naturalists, have never yet been able to draw a line, which should separate animals from vegetables. In the following experiment, therefore, I endeavoured to avail myself of the power of habit.

EXPERIMENT XII.

ON Monday the 10th of May, I gave a dog, apparently in perfect health, ten grains of an oxyd of iron, with bread and butter. 11th. gave him twenty grains. 12th. twenty-five grains. 13th. thirty grains, and thus increased the dose five grains per day, till Thursday the 24th, when his dose was eighty-five grains; having taken 745 grains since the 10th. I now began with the sulphate, for reasons before mentioned : of this, I gave my dog ten grains, on a piece of bread and butter, with two grains of opium. On Saturday the 25th, he took fifteen grains, with two of opium ; the dose was increased five grains per day, without affecting him considerably ; sometimes he would appear sick and refuse his food when offered him, but always ate it in the course of the day. June 5th, his dose was sixty grains ; above which I could not raise it, but continued to give him one dram till the 19th, when I drenched him with three drams in solution ; in the afternoon he ate one hundred grains of the powder at two doses. From this time till the 24th, he took

* Dr. Barton's lectures on Botany.

in divided doses, one hundred grains a day. The whole quantity of sulphate amounted to 1945 grains, or four ounces and twenty-five grains.

ON the 24th. I put an end to his existence. The contents of his stomach struck a deep black with galls ; those of the duodenum and jejunum underwent the same change, but in a less degree. The whole tract of intestines was thicker than natural, and somewhat indurated. The lacteals contained a fluid, which was not altered by any of the tests ; the mesenteric glands were carefully examined, both at the time, and on the succeeding day, but nothing evinced the presence of iron. Several of them were cut in two, and suffered to stand during the night, immersed in water containing alcohol of galls.

A LIGATURE had been placed on the thoracic duct, and a large quantity of chyle was now collected ; I tested it in various ways, but was only able to produce coagulation, without a change of colour.

THE bladder was partially distended with urine ; this I subjected to the different reagents, but my success was still the same.

IN order to examine the blood, I had killed the dog by cutting his carotids. A portion was caught in a wine glass, and a few drops of the tincture of galls added ; a brown colour was produced.* This, with two other portions, was set by to stand till the next day : one was blood by itself, the other contained pure alcohol. In the morning

* See experiment 15.

they were all coagulated; the portion to which had been added the tincture of galls was a solid mass, of a brown colour on its surface; the one with alcohol was likewise solid, but of a different appearance; that without addition had separated into serum and crassamentum, the latter was a florid red. The serum was simply coagulated by alcohol of galls, without the slightest tinge.

EXPERIMENT XIII.

ON the 20th of July, I gave another dog ten grains of the *oxy-sulphate* of iron,* which he ejected per orem, before noon. 21st. he took ten grains. 22d. fifteen grains, which operated somewhat as a cathartic. 23d. twenty grains; after this the dose was regularly increased five grains a day, without producing any effect, till the 21st. of August, at which time he had taken 2810 grains; his last dose was one hundred and fifty grains. 22d. I offered him one hundred and sixty-five grains, but he refused it, and appeared sick; I bled him, and repeated the experiment on the blood with the same result as before. The next day he was quite well, and took one hundred grains in divided doses. 24th. he took the same quantity, above which I did not attempt to raise it. During the whole period I allowed him no food but the vehicle of his medicine; consequently he became very poor. September 1st. I bled him again, the blood presented the same phenomena as before.

* Dr. Woodhouse says, that *sulphate* of iron will not strike a black colour with the gallic acid. Copperas is composed of two salts, sulphate and oxy-sulphate. In my last experiment I used the copperas of the shops. In this I preferred the oxy-sulphate, as being milder and better calculated to strike a black colour.

Sept. 3d. I cut the carotids, and caught a large quantity of blood. The above experiments were again repeated upon a more extensive scale, but I do not think it necessary to specify particulars, as they differed in nothing from those already mentioned.

A LIGATURE was passed round the thoracic duct, and a large quantity of chyle or lymph obtained, in which we could by no means detect the presence of iron, though repeated trials were made. The mesenteric glands were of their natural colour, and when suffered to stand till the next day, were only affected by the astringency of the galls. The lymphatics between the glands and the intestines were the largest and most beautiful I had ever seen, but contained no iron. The intestines changed their colour as in the former experiment. None of the secretions were affected by the reagents; the urine and bile were particularly examined.

THE spleen was much harder than natural, though but little different in size; a portion of it was examined without detecting iron.

THE intestines, through their whole course were small, and their coats somewhat indurated.

THE gall bladder was very large and full of green bile.

EXPERIMENT XIV.

As even the oxy-sulphate is so acrid as to excite considerable inflammation, the experi-

ment could not be very fair ; it would be a desirable thing to substitute some mild preparation of iron, which might easily be detected. Accordingly, in this experiment, I used the sulphate of iron repeatedly washed in boiling water.

THE first dose was five grains ; the dog appeared healthy : his dose was increased as in the last experiment. When I killed him he had taken five ounces and twenty-five grains. It would only be repetition to detail the particulars of this experiment : I was still unable to detect the iron by whatever means I used. The blood indeed was turned brown by alcohol of galls ; but the following experiment convinced me that this change could not be attributed to any absorption of iron.

EXPERIMENT XV.

I PROCURED a dog that I am well convinced had taken no preparation of iron. His blood presented the same phenomena that appeared in the above experiments ; a few drops of alcohol of galls caused a dark brown colour, which upon standing some time became almost black. Simple alcohol only caused the serum to coagulate, which remained of a turbid white. The brown colour must, therefore, have been occasioned by a precipitation of the tanning from the galls.

EXPERIMENT XVI.

THE subject of this experiment was a pig. I took the little animal from its mother when it was only a week old, and fed it upon milk, hold-

ing a portion of copperas in solution : at first the dose was small, but was gradually increased till it became of considerable size. At the end of a month the pig was killed ; but I was not more successful in this case than heretofore. Still I could detect no iron.

THESE experiments excited some doubts of the absorption of iron, by the animals I had hitherto subjected to its effects ; but the authority of Doctors Monroe and A. P. Wilson, induced me to believe I should certainly detect it in frogs. 'Tis true, these gentlemen did not experiment with iron, but they used articles not less active, which I thought proper to try myself, before I proceeded farther.

EXPERIMENT XVII.—Friday, July 24.

AT 11 o'clock P. M: I applied pledgits of lint dipt in a strong spirituuous solution of camphor, to both hind legs of a pretty large frog. (*rana pipiens*) Saturday morning at 7 o'clock, I found him dead. The skin was carefully taken off; that part to which the camphor had been applied, was removed by instruments which were not again used ; the different parts were dissected by different instruments (this is a precaution I used in all my experiments)—The smell was very distinct even in the liver, as was observed by several persons, some of whom were entirely ignorant of the experiment. The taste I thought I could perceive ; perhaps this might have been an idea associated with the smell.

EXPERIMENT XVIII.—Sunday, July 26.

HAVING about three ounces of strong copperas water in a quart tumbler, I put into the solution a large frog, (*rana ocellata*) and covered the tumbler with a piece of gauze. He immediately showed signs of great uneasiness; in 15 minutes he was more composed; in 35 minutes I observed the vessels of his feet to be very distinct; in 50 minutes the vessels over the whole body were very evident; as yet he was quite lively. In 65 minutes his eyes began to grow dim; in an hour and an half he did not move unless touched, but still respired; in 2 hours he did not stir when pricked pretty smartly. At this time I took him out, and removed the skin as before: The abdomen was opened with a lancet; several of the viscera were touched with alcohol of galls; the stomach and rectum contracted very strongly, and shortly after became quite black; the other viscera also changed colour. The stomach contained a quantity of fluid which was not altered by the tests; but when they were applied to the internal surface, abraded of its mucus, a black colour in the one case and a blue in the other, was instantly produced.

WHEN the cavity of the thorax was opened I found the heart still beating.

ALCOHOL of galls was applied to the surface immediately under the skin; the cellular membrane and muscles for some depth became black, which gradually disappeared towards the bone, round which not the slightest change could be observed.

THE gall bladder was much larger, and more distended than natural.

HERE, although the frog was completely immersed, it is hardly propable that any of the solution found its way into the stomach, otherwise the fluid contained in this viscus should have changed colour by the tests. This might be put out of the question.

EXPERIMENT XIX.—August 24.

A frog was suspended by a piece of tape fastened to his superior extremities, and extended by a half pound weight. Being placed in a tumbler, a strong solution of copperas was made to reach just as high as the point of the sternum. He struggled violently but was obliged to submit; I observed however, that by his exertions the solution was splashed over his whole body. At the end of three hours he became pretty quiet, yet when released was capable of motion. His skin was taken off, and the dissection performed as before related: The heart was still in action. The stomach contracted strongly when touched by alcohol of galls; the change was more evident on that part which had been below the surface of the water, but could be perceived for some distance above, and more evidently in places where, by the struggles of the frog, the water had been thrown on his body, and prevented from running off by a horizontal position. When the stomach was opened, the contained fluid and internal surface evinced the presence of iron more strongly than the external; the œsophagus through its whole length had a purple tinge, when alcohol of

galls was applied. The surface of the limbs immediately under the skin, presented the same phenomena that were related in our last; round the bone no change could be perceived. In every part the vessels were much distended, and the gall bladder contained a greater quantity of bile than natural.

THIS experiment suggested some curious doubts. I had detected the iron in various parts; much could not have passed by the mouth, though some of it did in spite of my precautions. Could this be absorption? If so, the appearances were very singular: but we have been taught from our infancy to believe that *transudation* cannot take place in the *living* animal. Further experiments, alone can determine.

EXPERIMENT XX.—September 15.

IN a strong solution of copperas, a large frog (*Rana ocellata* of Linneus) was suspended in the following manner; having a piece of thick paste-board, with a hole in its centre just large enough to admit the posterior half of the frog, he was drawn through as far as the superior extremities; to his hind legs was appended an half pound weight, and to prevent his retracting through the aperture, his fore legs were secured above. Thus accoutred, the paste-board was laid on the top of a glass tumbler, containing a sufficient quantity of the solution, to reach within a small distance of the top. He shewed as many signs of uneasiness as his situation would admit, for a considerable time. After three hours I released him, and

found that he still respired, and had the power of drawing in his legs when touched.

THE skin was removed and the abdomen laid open as before; when the sternum was raised I found the heart pulsating very strongly. Alcohol of galls was dropt on the external surface of the stomach, a violent contraction ensued, and an almost instantaneous change of colour; but it is remarkable that the black tinge extended no higher than the surface of the water, so that the part of the frog which had been immersed, could readily be distinguished from that which had not, merely by the application of the alcohol of galls. The anterior part of the stomach, or the surface in contact with the abdominal muscles, received a much deeper black from the alcohol, than did the surface opposed to the spine. When this viscus was opened, the test was applied to the fluid it contained, but no change was effected; the internal coat being abraded, an evident colour was produced in that part nearest the abdominal muscles; whilst the other next the spine, suffered no alteration. When the muscles of the lower extremities were cut into, and the test applied, the tinge extended from the surface about half way to the bone, gradually becoming less evident.

THIS experiment, the reader may be sure, did not serve to silence my doubts. 'Tis true, the anatomy of the frog is but imperfectly understood, and I know as little of it as most people. If these are absorbents leading from the skin to the stomach on its anterior surface, they must be very delicate, as I never could perceive any thing of the kind when removing the abdo-

minal muscles, though assisted by a glass. But admitting them to exist, (as indeed would seem probable, from some experiments made by my fellow graduate, Mr. Young) their action will not explain the appearance we have described, According to Mr. Young's experiments, these vessels carry water, &c. *into the cavity of the stomach.* In mine, the liquor in the stomach was never coloured when nothing had passed by the mouth; of course the iron could not have been carried by the absorbent vessels, for in this case it should have been deposited in the cavity of the stomach, and coloured the liquor and the internal surface in every part alike,

EXPERIMENT XXI.—September 18.

A frog prepared by means of paste-board, &c. as particularized in the last, was suspended in a strong solution of copperas. This frog remained in the sulphate but little more than half as long as the last; accordingly when I released him he made several attempts to escape, and seemed inconsiderably affected by his severe treatment. At the end of two hours the skin was removed with every necessary caution. When alcohol of galls was dropt on the external surface of the stomach, a powerful contraction was produced, and an immediate change of colour, but less evident than the last; owing probably to the difference of time during which they were respectively suspended. A transverse ring, above which no colour could be observed, marked the height to which the solution had extended. The internal surface and the fluid contained were not in the least altered; the œsophagus and mouth

were particularly examined, but I am sure no iron was there.

THE surface immediately under the skin turned as black as ink, but the muscles at a little depth, remained of their natural colour. Most of these experiment, especially the two last were several times repeated, but with so little variation, that I think it unnecessary to relate them.

EXPERIMENT XXII.—September 20.

PLEDGITS of lint were confined on the inside of a frog's thigh; the frog was bound on a board so as to prevent motion, and the lint kept constantly wet with solution of copperas. At the end of six hours the skin was taken off; no change of colour was perceptible on any of the viscera, but the muscles immediately under the lint turned black to a considerable depth; the colour extended a little on every side, but not more in the course which the absorbents are observed to take in the human subject, than elsewhere.

EXPERIMENT XXIII.

A frog being confined to a board on his back so as to prevent motion, a piece of lint laid over his stomach was kept constantly wet with the solution as above. When five hours had elapsed, I proceeded to examination. That part of the stomach directly under the lint, turned black by the application of the usual test; the colour was a little diffused, but on the opposite side not the

smallest change was evident. The internal surface and the contained fluid, were unaffected.

THE two last experiments were not repeated: I intended to have made several others which I had reason to hope would have given a more decisive result; but a variety of circumstances prevented any farther prosecution of the subject till the following spring.

EXPERIMENT XXIV.—March 20, 1803.

I SUSPENDED a frog in a solution of the sulphate of iron, as in my former experiments, and suffered him to remain two hours in that condition. When he was taken out, I observed the cutaneous vessels to be inflamed; not the smallest degree of torpor was evident. He was then washed in clear water, and wiped dry; alcohol of galls *applied to the skin* caused violent convulsions, but no change of colour. This being the case with the skin, it was not worth while to examine the viscera; I therefore replaced him for three hours longer. Upon taking him out a second time whilst still vigorous, a slight tinge was produced on the skin, but every other part was incapable of receiving a colour from the test, though carefully and repeatedly tried.

It was possible that this failure might depend upon the solution, since we are taught that nothing but an oxy-sulphate will strike a black colour with galls; but I convinced myself that the solution was a proper one, by rendering one drop of it sensible in an ounce of water.

EXPERIMENT XXV.—March 21.

THE last experiment was repeated, with this difference, that the frog remained four hours undisturbed. The result was the same.

EXPERIMENT XXVI.—March 28.

A frog remained eight hours in the solution before he was subjected to dissection. The result did not vary.

EXPERIMENT XXVII.—March 30.

I KEPT a frog suspended as before, for ten hours ; but still did not succeed. The frog was taken out alive and active.

EXPERIMENT XXVIII.—April 1.

A FROG was accoutred with paste-board, &c. and placed in a solution of copperas, at 10 o'clock, A. M. At 8 in the evening I found him quite lively ; at 7 the next morning he was not dead. The result of this experiment differed in nothing from the last.

EXPERIMENT XXIX.—April 5.

I CONFINED a frog as before, and kept him so till he was entirely dead, which was thirty-six hours ; and now I was just able to dis-

cover a slight tinge by the application of the tests ; but nothing when compared with my former experiments.

It is to be observed that all these frogs happened to be pregnant, and how far that might influence the experiment I was unable to determine. At any rate, I was obliged to refer the difference between these experiments and those made the preceeding summer and autumn, to some condition of the system, because the frogs lived so much longer. Since then, however, I have had an opportunity of trying a male frog, with no better success. But the season of their amours is not yet over : most animals seem to enjoy life in a more perfect state during this period ; and it is but reasonable to attribute something to that circumstance. We may without hazard, conclude that the season of the year is an important consideration in every experiment upon animals, as well as vegetables ; more especially cold blooded animals.

HITHERTO my experiments have been pretty much confined to iron. Of this I have nothing more to say ; but shall relate some facts, &c. relative to mercury. As the limits of a dissertation necessarily confined me to a few articles, these two were selected ; the former because it is most decisively detected ; the latter because of all others it has the most plausible arguments in favour of its absorption.

DURING the winter I had an opportunity of seeing three subjects who had died whilst under a ptyalism. In one of these Doctor Jacobs observed mercury on the larynx. The other two

were subjected to very minute examination, both anatomical and chemical, particularly the larynx, the salivary glands, the pancreas, &c. In these, however, we could discern nothing like mercury.

A QUANTITY of saliva was obtained from a patient who had been a long time on the use of mercury, and was labouring under a profuse ptyalism when the saliva was reserved.

EXPERIMENT XXX.

ONE pint of the above saliva was kept over a low heat for forty-eight hours; at which time it was evaporated to dryness. Nothing like mercury appeared in the residuum; nor did a piece of copper, which had been suspended over the vessel, exhibit any signs of amalgamation. Conceiving it possible for mercury to exist in some form not susceptible of amalgamating, I added to the residuum a few drops of nitric acid, with twice as much water. After suffering it to remain in the same heat for half an hour, I rubbed it on a cent with a piece of leather; not the most trifling alteration was produced in the colour of the copper, except what would naturally result from friction

EXPERIMENT XXXI.

To four ounces of saliva, I added twelve drops of nitric acid: a piece of copper was placed in the vessel and another over it. A low heat was applied for thirty-six hours; at which time

the fluid was of the consistence of an extract. The copper was now examined, but it remained in statu quo, or perhaps a little brightened by the acid, the action of which had given the liquor a green colour ; the suspended piece was entirely unaltered. The copper was replaced, and the heat raised till the whole mass became ignited ; which happened at a low temperature, owing to a crack in the glass. After this was over, and the vessel had cooled, the copper was again examined, and found to be somewhat whitened. The residuum was now of a colour not unlike colcothar of vitriol, and the glass vessel in some places *appeared* to be gilt. The suspended copper was not changed.

HERE seemed something like mercury ; the following experiment I thought would detect it more completely.

EXPERIMENT XXXII.

A PINT and an half of the saliva was exposed to a low heat for three days, without effecting the smallest alteration on a piece of copper. The saliva was confined in an oil flask, over which was placed a cent with a small hole in it. The glass now contained a matter like an extract which was poured into an open dish and evaporated to dryness. No mercury could be seen as yet. The residuum was then triturated with powder of charcoal, and exposed to a red heat. But I succeeded no better than before.

THE mercury could not have been volatilized by the heat, whilst the saliva was in the open

dish, for the heat at this time was no greater than when applied to the flask, in which case it certainly did not rise, or the copper on the top would have shown it.

EXPERIMENT XXXIII.

HAVING put one grain of fulminating mercury into four ounces of the saliva, and agitated them completely, a piece of copper was added, and a low heat applied. At the end of three hours the copper was taken out evidently gilt. The colour here was vastly different from that observed in experiment thirty-one.

EXPERIMENT XXXIV.

THREE ounces of saliva obtained from a healthy person, was treated after the manner specified in experiment thirty-one, except that in this case the glass was sound, and therefore I could not produce ignition. No change was effected on the copper; the glass, however, showed nearly the same appearance as before; and I am disposed to believe the whole was owing to the mucus of the saliva.

I procured saliva from two other patients, and treated it as above; the result was not more successful.

EXPERIMENT XXXV.

ON Thursday the 24th February, I gave a young pig, four grains of blue ointment. Fri-

day he took six grains ; he did not appear to dislike it. The dose was gradually increased, without producing any evident effect till the 22d of March ; at which time a diarrhœa supervened, and the animal died in two days. The whole quantity of the ointment which he had taken amounted to two ounces and an half. Certain circumstances rendered it impossible for me to examine the body for several days, of course the blood was lost.

BEFORE opening either the thorax or abdomen, the skin was removed from the muscles of the neck, and the larynx taken out ; the flesh was in a great measure dissected off, and the cartilages with their connecting membranes, after a careful examination, placed to dry. The salivary glands were then cut away ; nothing like mercury having been discerned, they were divided into minute portions, and put in glass vessels with gold and copper ; the metals were not altered two days afterwards. The muscles likewise were carefully examined, but we found nothing præternatural. The bones were partially cleaned and put away, to be examined, again when dry.

THE stomach contained a considerable portion of black matter, which was the ointment mixed with food. About the pylorus a few very small globules of revived mercury were to be seen ; the small intestines were nearly empty ; the colon was filled with a substance resembling that contained in the stomach ; here however the mercury was much more evident, appearing in a great number of pretty large globules.* The

* It would seem that the digestive powers had separated the fat from the mercury, and that by the time the ointment had reached the colon, a portion of nutritious matter was selected and taken up by the lymphatics. This is an argument in support of our opinion, as the same observation does not hold in the next experiment.

lymphatics and mesenteric glands were perfectly natural.

THE dried larynx had no appearance of mercury. To this I paid particular attention; because it was here that Broadbelt saw it, and it was here I saw it myself in the dissection made by Doctor Jacobs; of course 'twas here I expected to find it, if any where.

THE bones were examined every day till they became quite dry; they were then broken to pieces, but we found no mercury even with a glass.

EXPERIMENT XXXVI.

THE subject of this experiment was a pig which had never enjoyed perfect health, in other words *a runt*. On Thursday the 24th of March, being just twenty-six days old, he took one scruple of mercurial ointment; the next day he took twenty-seven grains; the dose was gradually increased for several weeks, without producing much effect. On the 22d. of April, when his dose was three drams a day, a diarrhœa came on. The whole of what he had taken, amounted, at this time, to four ounces and twenty grains.

As the last had died of diarrhœa in spite of opium, &c. I was determined to prevent that accident from happening again, by killing the pig myself; so I cut his carotids, and caught the blood. This was divided into several portions; into one was put a cent, to a second a piece of gold, to a third a piece of copper, with one grain

of fulminating mercury ; I had also two portions of blood from a dog that had taken no medicine ; into one of these I put a cent, into the other a cent and one grain of fulminating mercury. When I had done this I proceeded to dissect the larynx as before ; removing the muscles of the neck one by one. Having observed no preternatural appearance; the cartilages with their *proper* muscles, &c. were put carefully away to dry. The skin was next taken off: in doing this I met with a singular appearance about the head, which I cannot account for ; the skin itself was perfectly sound, but between this and the cranium was a large quantity, I suppose at least three ounces, of white curd-like substance, giving out a most disagreeable odour, scarcely to be supported. The bone underneath was very uneven, and irregular in places, especially round the edges of that part, where the fetid matter lay ; it was considerably eroded, and partially divested of pericranium, but I could no where perceive an entire perforation, nor was there any appearance of mercury. I then proceeded to examine the thorax very minutely, but perceived nothing worthy of notice.

THE abdomen was next opened ; the stomach was distended considerably ; the small intestines contained little else than air, the large ones were pretty full, and of a dark colour, and so strongly agglutinated, that I found much difficulty in separating them. The lymphatics, particularly of the small intestines, were much larger than I had ever seen, even in a tortoise. The blood vessels were greatly inflamed, so as to give the whole mesentery a most elegant radiated appearance. The mesenteric glands were prodigi-

ously enlarged : the liver and spleen were likewise unnatural ; the pancreas was nearly twice its ordinary size, and contained a much greater quantity of fluid than I recollect before to have observed ; perhaps this was a principle source of the diarrhœa ; according to an ingenious idea suggested by Doctor Wistar. But in none of these could I suspect the presence of mercury, though I spared no endeavours to detect it.

THE stomach was now opened, and found to contain the ointment, but little if at all changed. The small intestines we have before observed were distended with air ; the dark colour of the large intestines was owing to the ointment within ; I could discern a few small particles of revived mercury here, though not a fourth part as many as in the former case ; in fact, the principle difference between the contents of these intestines and the ointment out of the body, was, that the former appeared mixt with other matters.

THE skeleton was carefully preserved, and examined every day till it became perfectly dry. The bones were then reduced to powder, but no mercury could be discerned.

THE morning after the dissection, I was greatly astonished at the appearance of the larynx ; on the hyoid bone, the membranes of the thyroid cartilage, the ligaments between the two, &c. were a great number of shining particles ; I could not hesitate a moment concerning what they were, especially when Dr. Barton, Dr. Seybert, Dr. Jacobs, and several other gentlemen pronounced them mercury. By a microscope they became much more evident ; some of them

indeed, were so large as to admit of being collected, several were divided, and others put into one. Not a gentleman saw them but declared it was impossible to be mistaken.

When the blood had been standing thirty six hours, the gold and coppers were taken out evidently changed by quicksilver; that piece however, which had been in the blood with fulminating mercury, showed stronger marks than the others. The pieces which had been immersed in the dogs blood served as a standard of comparison; the portion which contained the fulminating mercury communicated a silver appearance to the copper, the other portion produced no change. The effect of one grain of mercury in the dogs blood was far from equivalent to that quantity in the pigs blood, yet was somewhat more evident than that of the pigs blood without the fulminating mercury.

It was impossible that the globules should have got on the larynx by accident from the stomach, or intestines; because it was removed and placed at a distance, before the abdomen or any other part was touched. Besides, the experiments on the blood put it beyond a doubt.

THIS experiment was too interesting not to be repeated; but the time allotted for writing our dissertations being nearly expired, I could not bestow upon it that attention which it deserved.

EXPERIMENT XXXVII.

A pig, apparently in perfect health, was fed with the same mercurial ointment; but being desirous of giving a large quantity in a short

time, I suffered him to eat it without weighing; whenever he chose; a portion of it stood constantly by him. No effect was produced till the twelfth day, when a diarrhæa began to make its appearance; to stop which I killed him. The dissection and examination was conducted in every particular, as in the last, but I found no mercury. The larynx, the blood, the glands, the bones, &c. were all attended to; nothing preternatural was observed in any part, except, perhaps a little enlargement of the mesenteric glands and pancreas.

THE ointment which was used in all these experiments, contained one fourth of mercury by weight, and was well made.

I have now given an abstract of all my experiments; they are too incomplete to justify a decisive inference on the general position. Much yet remains to be done ere the question will be finally decided. A well conducted sett of experiments on lactescent animals, would probably add to our stock of knowledge. At present this is altogether impossible for me, as well as many other experiments which I intended to have made; at a future period however, I hope to prosecute the enquiry.

CONCLUSION.

SEEING things as they really are, I would willingly omit any speculations on the subject; for what I can now offer, will amount to but little more than downright speculation. But to conclude without hazarding any opinion, would seem almost as far from the mark as to draw a decisive one.

MY experiments with iron have been pretty numerous, and after every probable objection has been made on the score of the fallacy of tests, they authorize me in believing, that iron is not *generally* absorbed.

WITH mercury I have also experimented, and in a solitary instance, have detected it; but what shall I infer from this? Not that it is universally or necessarily absorbed; for who would think of forming a theory on a single fact, or even half a dozen?

Unable to prove the affirmative, it would be very naturally supposed I should endeavour to establish the contrary opinion; for enthusiasm is a trait so common in the human mind, that it might almost be accounted one of the passions. Hence it happens, that not only politicians, but philosophers, disappointed on the one side, frequently become the warmest advocates of the other.

THIS by all means I would wish to avoid. To say that iron, mercury, and other substances,

are never absorbed, would be as far from truth as to assert that they are constantly carried into the circulation. But it follows of course, that the absorption of these articles is not necessary, or essential to their action on the system. We have already, in a former part of this essay, given our reasons for adopting such an opinion. The short time which elapses between the exhibition of iron, and the perception of its effects, is a powerful argument, but does not apply to mercury; here we rested more on the similarity of taste excited by mercury, copper, lead, &c. And if we can render it probable that ptyalism is a disease of association, we shall thence derive an argument of still greater weight. Every one must have experienced the immediate flow of saliva at the sight or smell of food on certain occasions; nausea has the same effect, as to the quantity of the discharge, but it seems different in its nature.—The reader will easily perceive how I would apply these facts; for particulars however, he is referred to Dr. Darwin.

WE have admitted that a powerful objection will be made to such an explanation, in as much as a salivation does not soon follow the exhibition of mercury; but it is worthy of notice, that if the medicine does not act as a cathartic, a disposition to spit is its common effect. By continuing the mercury, in doses too small to effect the bowels, we confirm this disposition into a habit; which, like every other habit, will remain for a time after the cause is removed. Hence it is easy to account for its *modus operandi*, agreeable to the doctrine of John Hunter*.

* Hunter on the blood, vol. 1. page 3.

Thus far, as relating to iron and mercury, my experiments sanction with some degree of probability; any conclusions more general, must be mere conjectures; a few of which I shall subjoin, and beg the reader to regard them as they are.

I. That the absorbent vessels are endowed with a peculiar kind of appetency, either sentient or mechanical, being the result of organization. The experiments of Dr. Fordyce*, Dr. Hodge, &c. as well as my own, render this as probable as many positions in medicine, which no one doubts.

II. That during the healthy action of these vessels, whilst their functions are properly carried on, such substances *only* are absorbed, as are capable of being converted into a part of the animal; or in other words, no substance which may not be converted into nutritious chyle previous to its entering the blood vessels. We are not yet capable of drawing a discriminating line between nutritious and non-nutritious articles: But for what purpose could the latter be absorbed? The animal machine is so constructed, that all its functions contribute to some good purpose; but extraneous matters in the blood vessels, as far as we can judge from experiments, are productive of very disagreeable consequences, if not death. And as we *know* that many substances produce their effects without entering the circulation, we have good reason to believe, that no part of the *modus operandi* of medicines is to be explained by first admitting their absorption.

* Fordyce on digestion, page 123.

III. THAT this appetency of the absorbents, like taste, smell, &c. is subject to derangement; or partial or temporary destruction. By supposing this, we only allow those vessels to be on an equal footing with other parts of the system.

IV. That certain substances taken into the stomach, as well as a disease of the system, occasionally have produced such derangement; and that mercury is more apt to have this effect than other articles. With respect to the first part of this inference, we judge from analogy and probability. In support of the second, it may be observed that mercury is generally allowed to exert a specific influence on the glandular system, of which the absorbent vessels are a part; and secondly, it has more frequently been detected in different parts, than any other active medicine.

V. THAT the healthy appetency being destroyed, by whatever cause; certain substances, not capable of being converted into chyle, are sometimes taken up by the absorbent vessels, and have been occasionally found in the thoracic duct, in the blood, in different parts of the body, or in the secretions. Why we ought to ascribe the absorption of such articles to a morbid appetency is evident from the following reasons. 1st. Because instances are so very rare. 2d. Because in the case related by Broadbelt, in the dissection by Doctor Jacobs, and in my own experiment with the pig, a disease of the lymphatic system certainly existed. 3d. Because we are so little acquainted with the pathology of the absorbent system as to render it very possible for a disease to make a great alteration in those vessels, without our being able to discern it.

STILL, I can conceive it possible that the *powers of assimilation* may be *morbid*, whilst the appetency is in no way impaired. This may be the case when certain articles are detected in the secretions, blood, &c. not absolutely incapable of forming chyle.

VI. THAT *in frogs*, it is not impossible for TRANSUDATION TO TAKE PLACE DURING THE LIFE OF THE ANIMAL. No comment is necessary here, the inference is drawn from direct experiment. 'Tis true indeed, that a certain condition of the system is necessary, to admit of transudation; what it is we know not; one thing however is certain, it is not incompatible with life.

IF this be extended by analogy, to the human system, we shall readily explain those curious facts relative to musk, camphor, &c. We are by no means disposed to insist upon it, as it regards man; but conceive that no impropriety would be apparent in its application to all cold blooded animals.

HERE then I leave the subject. In relating my experiments, for the sake of brevity, and to avoid repetition, I have not in every instance, mentioned who were present; but it may not be amiss to observe, that in no case did I depend solely on my own senses and judgment. The gentlemen to whom I am particularly indebted for their assistance, are Dr. Barton, Dr. Gregg, Dr. Jacobs, Mr. Grimes and Mr. Mitchell; each of whom I hope will accept of my sincere thanks, and rest assured that I only wait for an opportunity to serve them.

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