An inaugural essay on the action of poisons / by Henry William Ducachet.

Contributors

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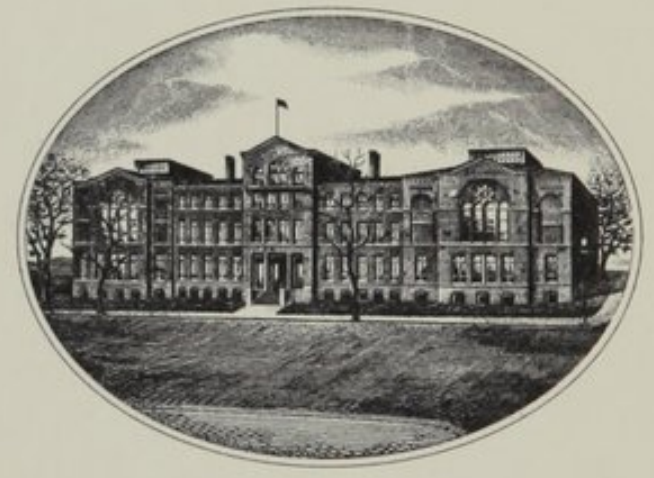

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Public Health Service

AN

INAUGURAL ESSAY

ON

THE ACTION OF POISONS.

BY

HENRY WILLIAM DUCACHET,

NEW-YORK:

Printed by Van Winkle, Wiley, & Co., Printers to the University.

1817.

To/ The Revd Dr Inglis—

with the respects of

the author.

ESSAY

ON

THE ACTION OF POISONS.

AN

INAUGURAL ESSAY

ON

THE ACTION OF POISONS.

BY

HENRY WILLIAM DUCACHET,

(Of South-Carolina,)

Member of the Medico-Chirurgical Society of the University of the State of New-York.

Hæc, ut potero, explicabo; nec tamen, certa ut sint et fixa, quæ dixero; sed ut Homunculus unus, e multis probabiliora conjecturâ sequens.—CICERO.

Prove all things—hold fast that which is good.—ST. PAUL.

NEW-YORK:

Printed by Van Winkle, Wiley, & Co., Printers to the University.

1817.

AN

INAUGURAL ESSAY

ON

THE ACTION OF POISONS.

SUBMITTED

TO THE EXAMINATION

OF

SAMUEL BARD, M. D. LLD. PRESIDENT,

AND THE TRUSTEES AND PROFESSORS OF THE COLLEGE OF PHYSICIANS AND SURGEONS OF THE UNIVERSITY OF THE STATE OF NEW-YORK,

AND

PUBLICKLY DEFENDED

FOR THE

DEGREE OF DOCTOR OF MEDICINE

On the 6th day of April, 1817.

TO

THE RIGHT REV.

JOHN HENRY HOBART, DD.

BISHOP OF THE PROTESTANT EPISCOPAL CHURCH

IN THE STATE OF NEW-YORK, &c. &c.

THIS DISSERTATION

IS

RESPECTFULLY DEDICATED,

AS A PUBLIC EXPRESSION OF THE ESTEEM AND VENERATION

I ENTERTAIN FOR HIS CHARACTER.

HENRY WILLIAM DUCACHET.

TO

DAVID HOSACK, M. D.

F. R. S. L. & E. F. L. S.

Professor of the Theory and Practice of Physic, and of Midwifery, in the University of New-York, &c. &c.

SIR,

I prefix your name to this Essay, not in compliance with the requisitions of custom, nor with the desire of courting favour by servile adulation; but publicly to testify my gratitude for the instructions I have received from you during the period of my studies, and to express my exalted estimation of your professional abilities.

Receive this imperfect testimonial of the gratitude and esteem of

AN AFFECTIONATE PUPIL.

TO

JOHN W. FRANCIS, M. D.

Professor of the Institutes of Medicine in the University of the State of New-York, &c. &c.

THIS ESSAY IS INSCRIBED,

As an acknowledgment of the many obligations which his kindness has imposed, and an expression of the personal regard of

THE AUTHOR.

[Printed under the authority of the College of Physicians and Surgeons, as the Statute directs.]

PREFACE.

When the author began to investigate the subject of the following Essay, he was, indeed, aware of the diversity of opinion which prevailed, and of the danger of attempting to establish points upon which the most learned and ingenious disagree. He intended to give merely a summary of the opinions which have been entertained upon this subject, without enumerating or amplifying the arguments upon which they rest. But he was confident that in this subject, as in every other, truth is immutable and one—although errors might be diversified and numerous. He therefore determined to avail himself of the facts and experiments which he had collected from the best authorities, and to adopt such conclusions as might result from a just and legitimate induction. He was confident, that although he might not attain and establish the truth, he would make, at least, nearer advances to it by detecting error. This has been his object; this, he trusts, is the result of his inquiry. In conducting this investigation, he has en-

deavoured to divest himself of the prejudices that he might have imbibed, to secure himself against those which he might derive from the high authorities whom he has consulted, and to subject every fact to the test of experience, and every opinion to analysis. Conforming to the great first rule of philosophizing, "to admit no cause that does not actually exist, and which is not sufficient to produce the effect assigned to it," he has endeavoured to exclude every explication that is inadequate to explain the phenomena we observe.

He is aware that he exposes himself to disapprobation, by advocating doctrines to which the most eminent physiologists of the day are opposed. But in the defence of opinions which he considers true, and which a patient and laborious investigation have led him to adopt, he will even incur disapprobation; nay, he will defy censure rather than resign them to any authority, but a conviction of their fallacy.

He indeed has a deference for superior talents: he respects those who have laboured in the search of truth, and whose exertions have advanced the cause of science: but he will not suffer hypotheses, however admired, to captivate his reason; assertions, however bold, to impose restraints upon truth; nor authority,

however great, to interdict examination. He would rather be led astray by the dictates of his own judgment than be deluded by the errors of another.

But, did all learned and ingenious men agree upon this point, he should then distrust his own reasoning, and be willing to ascribe his difference in opinion to a want of that superior perception by which they had discovered the truth. But it is manifest that they entertain various and contradictory sentiments; they cannot all be right, and therefore no individual authority is sufficient evidence of truth, nor is entitled to implicit confidence.

Had this principle been permitted to sway the mind, the luminous discoveries which the progressive improvement of the human understanding, and the bold exertions of its energies, have brought to light, would never have been developed; and the world never would have attained the elevated and glorious station, that now bids defiance to the annals of history to produce a parallel example of human greatness.

Disdaining that servile adherence to received opinions, and that undue reverence for authority, which are calculated to injure the cause of truth, and retard the

progress of science, he has "proceeded to examine systems by experiments; he has gained all the light he could from the observations of others, but he has trusted to his own."*

It is humbly hoped that the errors and inaccuracies, which may be detected by the intelligent reader, will be charitably overlooked; and that the consideration that it is a juvenile performance, "will restrain the rude hands that are ever ready to pluck up the tender plants of science, because they do not bear fruit at a season when they can only be putting forth *blossoms*."

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* Anonymous Life of Boerhaave. London, an. 1743.

AN ESSAY, &c.

It is difficult to define a *poison*. This is evident from the various and inconsistent definitions of authors. According to some, there are no poisons in *quality*, and all things are poisonous or not poisonous, depending upon the *quantities* in which they are employed. Others have attached to them a certain *mysteriousness in their action;* and others have designated them by the *rapidity* with which they manifest their deleterious operation. But these definitions are by no means satisfactory. The *first* includes substances the most harmless in their action, and useful in their properties, and does not comprehend many of the most deadly poisons: the *second* does not designate them from medicines, the operation of which is equally mysterious: and the *last* is limited in its application; for many poisons require a considerable time to produce their effects.

A poison has been defined, "that which in its nature, or in its most remarkable properties is so hostile to animal life, as in a SMALL quantity to be destructive to it."

But even this general definition is evidently *imperfect:* for a dose, which at one time may be attended with fatal consequences, under different circumstances will produce no effect: thus, in torpor of the intestines from paralysis, incredible quantities of mercury may be administered without any perceptible operation; and to compose the frantic ravings of mania, or to allay the violent spasms of a convulsion, requires a quantity of opium which, under other circumstances, might produce death. A quantity may be taken by individuals whose susceptibility is destroyed or diminished by *habit*, which, under ordinary circumstances, would inevitably be fatal. Thus THEOPHRASTUS informs us of a shepherd who could eat hellebore. GALEN tells us of a woman who had accustomed herself to cicuta. Thus MITHRIDATES, the tyrant of Pontus, by gradually habituating himself to large doses of poison, secured himself

against the attempts of his oppressed and discontented subjects; and hence the Turks and Persians, who are in the habitual use of opium, require, for their exhilaration, quantities of this article which, at first, would have occasioned their death. SCHENKIUS relates many cases of persons who could bear considerable doses of poison.* We may here notice the power of habit in securing the body against the poison of contagious fevers. "Infection, like some other poisons, does not so readily affect those who are accustomed to it, and, therefore, those who are in the habit of being exposed to it, frequently escape its bad effects."† This observation is corroborated by the experience of Professor HOSACK, and is ably illustrated in his "Observations on the Laws governing the communication of Contagious diseases, &c."‡

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* Observat. Medicin. lib. vii.

† Blane, Diseases of Seamen, p. 223. Adams, Morbid Poisons, p. 8. Haygarth on the Prevention of Infectious Fevers, p. 44. MS. Notes on Hosack's Lectures, &c. Thomas' Practice, p. 40. Pringle, Diseases of the Army, p. 289. Cullen, First Lines, vol. 2. p. 246.

‡ See Observations, p. 33. 74. 76.

But the definition is further objectionable, inasmuch as some substances which prove poisonous to one animal, may be perfectly innocuous, and sometimes even salutary, to another: thus aloes is poisonous to the dog, pepper to the hog, parsley to the parrot; goats feed on hellebore; hogs eat hyosciamus; arsenic is harmless to the wolf and to the horse,* and hemlock and the manchineal apple, which to man are fatal poisons, sheep eat with impunity. Even the poison of the viper† has no effect upon some animals; while human saliva, so bland and useful a secretion to man, is destructive to serpents.‡ Nay, food grateful to some, will produce vomiting and deliquium in others;§ some are thrown into convulsions by the fragrancy of an apple; and others will actually "die of a rose, in aromatic pain."

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* Parr, Medical Dictionary, art. venenum, vol. 2.

† Vid. Fontana on the Viper, vol. 1. p. 34.

‡ Plin. Hist. Natural, lib. viii. cap. 2. et Aristot. Hist. Animal, lib. viii. cap. 29.

§ Boyle on the strange subtility of Effluvium. Boerhaave de morbis nervorum. Trotter on the Nervous Temperament. MS. Notes on Hosack's Lectures.

It may here be remarked, in further illustration of this position, that many of those poisons which produce *specific diseases*, do not operate upon every species of animals: thus the venereal disease is peculiar to the human race, and cannot be communicated to brutes by any means soever.* The same may be said of the virus of small-pox. With regard to the hydrophobic poison, it is remarked, that it exhibits its morbific effects only in carnivorous animals, and is generally confined to the canine and cat species.†

Boerhaave defines a poison, "that which, applied to the living human body, so alters its actions that it is not overcome by the vital powers, but destroys the principle of life."‡ This is excep-

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* Vide, Thomas's Practice. Hunter on Lues Venerea.

† Vide, Thomas's Practice. Bardsley's Reports. Moseley on Hydrophobia. Gillman on the bite of a Rabid Animal, &c. It has been observed to originate, de novo, in rats. Vide Linnean Transactions.

‡ Quæ corpori humano viventi applicata actiones omnes ita mutant, ut per vim vitalem non superentur, sed ut illa vis vitalis destruatur. De Morb Nerv. tom. 2. p. 447.

tionable, inasmuch as it is restricted to those substances which are injurious to the *human* body only, and which are inevitably *fatal.*

The most unexceptionable definition is that given by PLENCK, viz. "that which, in a very small quantity, either taken internally, or externally applied to the human body, by some peculiar action occasions a serious disease, or produces death.* But even this evinces the difficulty of defining that of which we have no precise idea.

Physiological Action of Poisons.

The *manner* in which poisons produce their effects upon the system, has afforded much matter for speculation and controversy. But all the ingenuity that has been directed to this subject, and all the disputations to which it has given rise, have not determined the question to general satis-

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* Ens quod perexigua dosi, corpori humano ingestum aut extus applicatum, vi quadam peculiari morbum gravem vel mortem causat, venenum seu toxicum audit. Toxicolog, p. 9.

faction. It is not my intention to attempt the accomplishment of a task, the difficulty of which has been experienced and acknowledged by the most able physiologists; such presumption would ill comport with the humble object of an inaugural thesis. I shall, however, examine the various opinions which have been entertained upon this subject, and discard those which seem discordant with facts, or inadequate to the explication of the phenomena resulting from the action of the poisons. It shall be my endeavour to avoid the imputation of presumption in advancing what may have neither plausibility to recommend, nor authority to sanction it; and I am equally desirous to avoid the appearance of appropriating to myself what justly belongs to another. I, therefore, claim no originality, and deprecate all other commendation, but that of patient investigation, and candid acknowledgment to the authors whom I have consulted.

Let us now proceed to examine the various opinions that have been entertained concerning the *action of poisons.*

I. The ancients thought that all poisons operated upon the *innate heat* of the system in which resided the vital principle. They accordingly considered all poisons as *hot* or *cold*. Those of the *first* class caused death by raising the animal temperature to a degree incompatible with life, or by their acrimony and inordinate activity counteracting or suffocating the genial warmth of the *calidum innatum*. The *cold* poisons possessed a refrigerant operation, and reducing the principle of life below the natural point, benumbed its energy and arrested its action. In conformity with this view, their *alexipharmics*, or antidotes, were such as they supposed were calculated to restore the vital warmth, and by renewing the action of the vital principle, to expel from the system the noxious materials which threatened its disorganization.

But the moderns, guided by a more correct physiology, have attained more accurate views of the animal œconomy, and the principles by which its actions are regulated and preserved; assisted by the light which modern chemistry has afforded.

they have better ascertained the nature and properties of the poisons, and the antidotes which they suggest.

II. At a time when the laws of *mechanics* were had recourse to, to account for the phenomena of the living system, the action of poisons also was explained upon the same principles. But, though it could not be demonstrated that such is not their mode of action, the absurdity of applying mechanical principles to vital operations would be sufficient refutation of this theory. That the action of poisons is *wholly mechanical*, is an unwarrantable idea; for the single fact, that their operation is most speedy and violent,* when administered in circumstances the most unfavourable to such action, immediately overturns all the speculations that can be adduced in its support: and if such were their *modus agendi*, the laws of gravitation and motion being invariably the same, their effects, depending upon these laws, must likewise

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* Thus arsenic, corrosive sublimate, opium, &c., act more speedily when taken in *solution* than in a concrete state.

be the same; but this is contradicted by daily experience.

III. Some have supposed that they act *chemically* upon the animal body. This idea, although more plausible, is not more true. It is, indeed, true, as a general fact, that of *mineral* substances, those which abound most in oxygen, and which most readily yield it, possess the most active poisonous properties. But this is not invariable;* nor are their effects always the same, which they should be—affinities, under similar circumstances being always the same. But the mineral poisons are not necessarily decomposed in the *primæ viæ;* they are, frequently, upon inspection after death, found in the same state, nay, even in the same quantities, in which they were taken, and no lesion of the organic texture is to be discovered.† How then can any chemical action have taken place?

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* Iron, in all degrees of oxydation, is inert—even its salts possess no poisonous property.

† Brodie's Experiments. Morgagni de sed. et caus. vol. 3. p. 374. Fodére, Médecine Legale, tom 4. Belloc, cours de Méd. Lég. p. 163. Orfila, Toxicolog. Générale; passim.

The action of the *vegetable* poisons will not admit of explanation on chemical principles. It is true that the fluids are sometimes dissolved, and sometimes coagulated in death from these poisons; but these appearances can more rationally be accounted for by the deranged actions of the system itself, or the natural and spontaneous consequences of the extinction of life.

With regard to the action of the *animal* poisons, chemical principles cannot be admitted. The dissolved state of the blood, and the putrescency of the humours, which are observed in those diseases particularly that arise from a specific virus, are not the *immediate* consequences of the poison, but the result of the action of the system, or the assimilation of the mass of fluids to the nature of the ferment, which is exclusively a vital, not a *chemical* process.

It appears, then, that the action of the poisons *cannot be explained on mechanical or chemical principles.* The *spiculæ*, which were formerly devised to disunite the animal fibre, and irritate the animal

spirits; the *organic particles*, which fancy had created to supply the deficiency of more obvious causes; and the *acrid salts*, which were supposed to attenuate, to inspissate, or decompose the humours, are not only unnecessary, but totally inadequate to the solution of the phenomena, and must be discarded in this enlightened age, in which facts and experiments take the precedence of conjecture and assertion.

IV. That *all poisons act primarily upon, and exclusively through, the medium of the blood*, is an opinion which has been advanced by high authority. Let us respect authority, but let us respect truth more. Let us examine the action of the several classes of poisons, and see how far it can be reconciled with this doctrine, and in what manner it can more satisfactorily be explained.

1. *Of the* Mineral *Poisons.*

Mr. Brodie* injected into the stomach of a rabbit, by means of an elastic catheter, six grains of

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* Vid. Philosophical Transactions for 1811 and 1812.

corrosive sublimate, dissolved in six ounces of distilled water. *Three* minutes after the injection, the animal, without the least previous suffering, became insensible, experienced some convulsive movements, and died in 4 ½ *minutes* from the time of injection. From this experiment, and many others, which afforded the same results, this distinguished physiologist concluded that the action of the poison upon the stomach, is by sympathy communicated to the heart and brain, and that death is the consequence of the suspension of their functions. He conceives it impossible that, in so short a time, the poison should be *absorbed.*

It would be unnecessary to detail a series of experiments to illustrate this fact. The quickness with which all the metallic poisons operate is daily exemplified in the treatment of diseases. The speedy inverted action of the stomach consequent upon the exhibition of *antimony* and the *vitriol* emetics is familiar to all, and *cannot be ascribed to their absorption into the blood.*

2. *Of the* Vegetable *Poisons.*

Let us now examine the action of the *vegetable* poisons, and see how far it can be reconciled with the doctrine of absorption.

Dr. Alston, who made many experiments on *opium*, has observed, and, indeed, the remark is corroborated by daily experience, that it *almost instantly* produces sleep, relief from pain, vomiting, &c., and hence concludes that it operates upon the system through the medium of the nerves.*

Dr. Whytt found that opium, injected into the large intestines of a dog, *quickly* paralyzes the posterior extremities, and that applied to a naked muscle, it destroys its contractile powers. "It remains, therefore, that opium, by affecting the extremities of the nerves of the part, to which it is applied, does, by means of their connection and sympathy with the brain and spinal marrow,

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* Medical Essays and Observations, vol. 5.

destroy or prevent, the operation of that power upon which depend sensation and motion in the bodies of animals."*

This conclusion is substantiated by the experiments of Dr. MUNRO.† He found that opium, applied to the brain, *instantly* excites violent and universal spasm; and that by injecting it in such manner as to make it pass along the spinal marrow, the animal is *instantly* killed. He observes that its "effects are more speedy where the dose is equal, when the opium is applied inwardly, than when applied outwardly, as might have been presumed from the greater sensibility and delicacy of the inward organs."

FONTANA gives us the following results of his experiments on opium:‡ viz. guinea-pigs which he made to swallow a spirituous solution of opium‖

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* Whytt's Works.

† Essays and Observations Physical and Literary, vol. 3.

‡ Fontana on the Viper, vol. 2. p. 362.

‖ The aqueous solution produced the same effects, though not in so short a time, owing to the greater activity of the spirituous menstruum.

became motionless in a *few minutes*, and died in less than twenty-seven. Those into the bellies of which the spirituous solution was injected became motionless in an *instant*, and all, without exception, died in less than half an hour. Those beneath the skin of which he injected the same preparation, died in less than half an hour; and he had scarcely made the injection when they could no longer stir their hinder feet.

Now, I ask, what conclusion can be drawn from these facts in favour of the doctrine of *absorption?* Do they not incontestably prove that this article *acts directly upon the nerves?*

With regard to the action of the *cherry-laurel*, Fontana has instituted many experiments which, it seems to us, lead to the same conclusion. He tells us that he has seen rabbits, after taking the laurel-water, fall into convulsions in less than 30 *seconds*, and die in *less than a minute.** He

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* Vol. 2. p. 144.

found that if swallowed in large quantities, they die *almost instantly;* and that its action is so *sudden* and *violent* that the animals give evidence of its operation at the *moment* of its being swallowed. Dr. MEAD says, that *immediate* totterings and convulsions are produced, which are soon followed by total paralysis; nay, he has known a dog to be killed in the *very act* of swallowing it.*

In short, many experiments are detailed by FONTANA,† the result of which was, that many animals, the most tenacious of life, are *instantly* convulsed and killed by this poison.‡

The natives of Java and Borneo use the extract of the *Upas Tieute* to poison the points of their arrows, a wound from which, we are told, is *speedily* fatal. M. M. MAGENDIE and DELILE reported, to the Institute of Paris, several experiments on this poison, which show, that a puncture made in the thigh of a dog with a bit of wood dipped in a so-

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* Vid. Works, p. 97. † Vol. 2. p. 156. ‡ Vol. 2. p. 345.

lution of this extract, or the solution itself injected into the peritoneal cavity of this animal, *instantly* induces convulsions, to which death succeeds in the space of *three minutes.*

Some tribes of Indians, we are told, poison their weapons of war with the expressed oil of *tobacco*, and that a wound from these instruments is attended by *instantaneous* vomiting, convulsions, stupor, and death. This account is confirmed by the experiments of FONTANA on this article. He found that a piece of wood covered with the oil of tobacco, and introduced into the pectoral muscle of a pigeon, produced a total insensibility in a *few seconds.**

The same *speedy* consequences attend a wound from the arrows pointed with the *Woorara* poison,† or the Indian *ticunas.*‡

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* Vol. 2. p. 154.

† Bancroft's History of Guiana, vol. 2. p. 85.

‡ Fontana, vol. 2. p. 112.

Some experiments performed by Mr. Brodie are in point, viz.: the oil of *bitter almonds* applied to the tongue of a cat, *instantly* produced convulsions; at the end of five minutes, the animal had so far become insensible as to be apparently dead, and was actually so at the expiration of six or seven more. Two drops of the same instilled into a wound in the thigh of a rabbit, produced convulsions in four minutes—apparent death in two more—actual death in several. The same results followed the injection of eight drops of the oil of *bitter almonds*, the juice of *aconite*, and the oil of *tobacco*, into the rectum of dogs and cats, with the addition of immediate sickness, when the last article was employed. Mr. Brodie touched his own tongue with a very small quantity of oil of bitter almonds, at the end of a probe, and *instantly* he experienced its effects; he felt an indescribable sensation *immediately* come on, which was succeeded by a sudden weakness and inability to stand.*

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* Philosophical Transactions for 1811.

We may now be permitted to ask, on what known law of the animal economy can these phenomena be explained, but that of a *direct action upon the nerves of the part* to which the poison is applied, and a *sympathetic* operation (which must depend on nervous intercommunion) upon the *sensorium* and *spinal marrow*, from which all vital energy proceeds? Can we admit those "*passages* in the living body we are yet ignorant of," "through which the most nimble and active particles of the poisons may readily introduce themselves into the blood;" and "those *unknown powers* and *latent principles*, requisite to detach the most active parts of these substances?"* Can we, in spite of the facts before us, believe that these poisons are taken into the blood by the *slow process of absorption*, and produce their effects upon the system *secondarily* only, and *through the medium of the circulation?* Or, shall we cut the matter short, and agree with Mr. Brodie, that they *pass directly into the blood through the divided vessels*, involving ourselves

* Fontana, vol. 2. p. 199. 379.

in difficulty to account for their action, when applied to surfaces in which there is no lesion or solution of continuity?

Let us rather reason from *known* principles, than assume, as data, hypotheses which are as unfounded in nature as they are inadequate to explain her phenomena: "for it is, indeed, much better to be ignorant of a truth, than to admit an error."

3. *Of the* Animal *Poisons.*

We now proceed to examine the action of the *Animal* Poisons.

In many parts of India persons working in the fields are frequently bitten by venomous serpents: in a *few minutes* distressing sickness, stupor, and convulsions supervene, which terminate in death in a *very short time.* Few, we are told, survive *half an hour.**

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* Oriental Field Sports. Plenck's Toxicologia, p. 33.

The southern parts of Indostan abound in serpents, the bite of which is speedily mortal.*

The bite of a rattlesnake uniformly produces death in a *few minutes*. Dr. MEAD tells us it will kill a dog in a *quarter of a minute*.†

He tells us, that "he caused several animals, dogs, cats, and pigeons, to be bit by an enraged *viper*, and he constantly observed that they all, *immediately* upon the bite, showed, with signs of acute pain, marks of their life being affected, by sickness, faintings, convulsions," &c. That great practical observer, FONTANA, who performed upwards of 6000 experiments on the venom of the viper, says, "I have seen animals, even pretty large ones, such as dogs, fall prostrate on being bit by a viper." "I have seen others void their urine and

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* M. M. Bruce & Paterson. The aspic which killed Cleopatra was probably of this kind, for the venomous reptiles found in India agree with the description given of the African Serpents; and particularly as described by Lucan. vid. Pharsal. lib. IX.

† Mead's Medical Works, p. 16. Plenck's Toxicologia, p. 30. Huxham on Fevers, p. 106.

excrements, *at the very instant*, as if their sphincters had become paralytic at the *moment* of their being bit. It is not a rare case to observe men fall into a swoon almost *immediately* after they have received a bite from a viper."* He found that a pigeon could be killed in *twenty seconds*, and that of twelve pigeons upon which he made direct experiments, all died within *two* or *fourteen minutes* from the time they were bitten.†

It may, perhaps, be said, that like all experiment-makers, he found exactly what he wished to find, and that he instituted experiments, not so much to deduce legitimate inferences from them, as to bend them to coincidence with his preconceived notions. But these important facts were in direct opposition to the opinion which he adopted: and although the Abbé drew from them a conclusion very different from that which seems to us the only legitimate one, this does not invalidate their force.

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* Fontana, vol. 1. p. 299. † Vol. 1. p. 297.

The same experimentalist observed, that a ligature applied to a limb which has been bitten by a viper will "prevent the complaint from communicating itself to the animal, no internal disease supervening during the time it remains on. It is, likewise, an experienced truth, and equally important, that at the end of a certain time, the venom does not communicate itself to the system." And he admits, "that it is not necessary to stop the circulation totally, and in the very small vessels:" and "that as to the pressure of the ligature, it was so very slight, that he could not think it capable of preventing the progress of the venom."*

These facts seem to prove that this poison acts upon the system through the medium of the *nerves*, and *not* of the *blood*. For, did the viperine poison operate upon the blood, what impediment was there to its action when the circulation was not interrupted, nor the progress of the venom prevented? The fact is, that the pressure,

* Vol. 2. p. 26. 33.

although it was so slight as not to interrupt the circulation, was sufficient to interrupt the *influx of nervous energy* into the limb to which it was applied: the sensibility of the nerves being thus suspended, the animal was secured against the action of the poison. This explanation is warranted by the fact, that the *violence of the action of poisons upon animals is in direct ratio to their irritability.* Hence animals in a torpid state have been bitten by a viper without any bad effects;* and opium† will render the body insensible to the action of poisons. Mons. Richerand tells us, that, "a dog, cæteris paribus, is much less in danger from the bite of a viper, when suddenly bitten, than when he has been some time gazing at the reptile, and is more or less terrified by the sight."‡ And FONTANA also remarks that "animals in general, which seem the most to dread the

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* MS. Notes on Professor M'Neven's Lectures on Materia Medica.

† Boerhaave de Morb. Nerv. It is a fact well known to every medical man, that opium will restrain the action of medicines.

‡ Elements of Physiology, p. 159.

bite of the viper, and which tremble at the very sight of this creature, die soonest."* This fact is important in its application. It seems that the terror with which the animal is seized, increases the sensibility of the nervous system, and thus renders it more liable to the deleterious operation of the venom. Thus, also, it is observed, that fearful, timorous persons are most subject to pestilential diseases; this has been particularly remarked with regard to the plague, to which they become powerfully predisposed by this debilitating passion.†

But when Fontana applied a ligature to the *blood-vessels themselves*, he found that the effects of the venom were not prevented. "We may deduce with certainty from these experiments, that the venom of the viper produces its usual effects,

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* Vol. 2. p. 52.

† Vide Valli on the plague of Constantinople of 1803. Also, Dr. Clarke's Travels to the Holy Land. MS. Notes on Hosack's Lectures on the Theory and Practice of Physic, &c. Baron De Tott's Memoirs, vol. 2. p. 83.

even when the parts bitten no longer participate of the circulation of the blood in the animal machine."*

Now, if the blood be the *only vehicle* of the poison, and the *only medium* through which it can act upon the system, how can we account for its effects when the circulation is interrupted?

He found that although the *absorbent trunks* be tied, so as to stop the circulation of the lymph and chyle, the venom still exercises its powers.† This then cannot be the medium through which, *necessarily*,‡ the poison operates upon the system.

That a *ligature on the nerves*, or what is essentially the same, a ligature applied to a limb in such manner as to *interrupt the influx* of *nervous energy*, will prevent the action of the poison, I think has been clearly shown before; (p. 41.) and

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* Vol. 1. p. 372. † Vol. 1. p. 376.

‡ I say necessarily, because I believe that the venom *may* be taken up by the absorbents, although its action, even in such case, is not independent of the *nervous* system; nor its *absorption necessary* in order that its effects should be produced.

the practice of applying ligatures to limbs that have been bitten by venomous serpents, was pursued with distinguished success by Professor KEMPFER, during his voyages in India.*

There are some phenomena which tend to prove that even the *hydrophobic virus* in some cases acts primarily and exclusively upon the nerves. For sometimes no morbid appearances soever have been observed in the organs after death—no alteration has been detected in the fluids; in short, no marks of disease whatever.†

4. *Of* Aerial *Poisons.*

Gases or *Aerial* substances produce death, either by *their incapacity to support respiration*, or

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* Vid. Amenitat. Exot. &c. Vid. et Galen de Locis Affectis, lib. iii. cap. xi. Celsus, lib. v. cap. xxvii. Medicin.

† Thomas' Practice, p. 359. Gillman on the bite of a rabid animal, p. 83. Boyer's Surgery, p. 211. Plenck's Toxicolog. p. 68. Dr. Mease, Dr. Percival, Dr. Hamilton, and others equally distinguished, have entertained this idea. Dr. Rush tells us he has known it to be produced by a mere contusion with the teeth of a rabid animal, vol. 2. p. 326. of Med. Inq. and Obs.

by some *peculiar deleterious principle.* This distinction is founded on the fact that some gases, as hydrogen and nitrogen, may be breathed several times without any injury; death is at length produced not by any direct action of these airs, but by their unfitness to effect the necessary changes of decarbonization and oxydation of the blood; the consequent retention of the carbon paralyzing the heart, and arresting the circulation.

These gases, then, possessing no positive qualities, cannot be ranked among the poisons, any more than a mechanical interruption of respiration.

But other gases are *not only unrespirable* from their negative properties, but possess also *a peculiar noxious quality,* by which animals, that can long bear the want of respiration, are frequently *instantly* killed. In further confirmation of this distinction we may adduce the fact that animals will live longer *in vacuo* than in these airs; and persons whose respiration has been suspended by drowning, &c., will retain the vital

principle a longer time than those that have been exposed to the action of these noxious gases *

Dr. Black observes, that when the nostrils of birds are closed with suet, they live longer in *carbonic acid gas*, than when they are suffered to inhale it.† Mr. J. Hunter says, that fishes will live longer in water deprived of air, than in water impregnated with *fixed air*. These experiments show, that in the first cases they die merely for want of respiration, whereas in the latter they are killed by the *peculiar noxious principle* of the gas. Dr. Priestley observes, that animals which are made to breathe air infected with animal respiration, are sometimes affected so *suddenly*, that they are irrecoverable after a single

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* Life has been restored after it had been suspended for *more than three quarters of an hour*, in one case, after *six hours* immersion, (See Transactions of the Humane Society of London. Curry on Apparent Death, p. 107.) whereas a *very few minutes* will completely exhaust the vital principle, in cases of suffocation by noxious vapors.

† Black's Chemistry, vol. 2. p. 87.

inspiration, though they be withdrawn immediately, and every means be taken to bring them to life.*

If it be objected to a peculiar noxious principle in these gases, that carbonic acid gas, sulphurated hydrogen, &c., are as innocuous, when taken into the stomach, as hydrogen or atmospheric air, let it be remembered that every organ has a peculiar irritability to which peculiar stimuli are adapted; and that many substances, the poisonous nature of which the most sceptical have never doubted, are frequently innocent in the stomach.

Dr. Percival, speaking of the action of noxious vapours, observes, that "their effects are often *instantaneous;* they immediately destroy the action of the brain and nerves, and in a moment arrest the vital functions. Their effects evidently mark out a direct affection of the nervous system. Sometimes they occasion instant death; at others,

* Priestley on Air, vol. 1. p. 71. 77. 194.

the various symptoms of a debilitated nervous system, according as the poison is more or less concentrated."

With regard to the operation of the *effluvia* of *noxious plants*, as the bohon upas, the manchineal-tree, the poison-tree of Macassar, the yew, catalpa, &c., it may be remarked, that they owe their noxious properties chiefly to the *carbonic acid* gas which they evolve, and which constitutes these emanations.*

We have, as was proposed, examined the action of the several classes of poisons; we have seen that the phenomena which they produce *cannot be reconciled with the doctrine of their absorption into the blood;* and have endeavoured to explain them more satisfactorily, on the principle of a *direct action upon the nervous system.* Let us now proceed to test the validity of the *objections* to this theory, and to notice those poisons which must be consi-

* See Dr. Inghenhouz's Experiments on Vegetation, vol. 1. p. 69.

dered as *exceptions* to the general principle laid down.

1. *Objections to the Nervous Theory.*

Objection 1. That the *viperine virus*, and a *solution of opium*, or any other poisonous substance, when *injected into the blood-vessels*, will produce convulsions, and even death, are facts which cannot be denied;* but this by no means proves that these substances produce their deleterious effects upon the system by the *medium of the blood*—as has been triumphantly asserted by Fontana, and many succeeding physiologists; for it is a fact equally well established, that *any foreign* substance whatsoever *directly* introduced into the circulation, will be attended with the same consequences. *Wine*, *spirits of wine*,† *distilled vinegar*, *salt of tartar*,‡ nay, *the most bland*

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* Fontana, vol. 1. p. 318. et vol. 2, p. 383, in supplement, &c. Also, experiments of M. M. Magendie and Delille.

† Fontana, vol. 2. p. 386.

‡ Sprægel Exp. circa venen. var.

fluid, even a drop of *milk*,* will be attended with the same results. *Air*, suddenly injected into the veins will likewise produce death.† In the middle of the 17th century, it was thought that the *transfusion of blood* superseded all other remedies, and that to restore life, it was only necessary to transfuse the blood of a healthy animal into the veins of the dying or the dead. But the result of such experiments proved the futility of the chimera; the operation *always proved fatal*,‡ and these murderous experiments were discontinued by legislative prohibition. The injection of *medicinal substances* into the veins, which was subsequently had recourse to, only establishes the truth of the position, that *any thing that will forcibly dilate the vessels, will cause death.*‖

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* Boerhaave, de Morb. Nerv. p. 217. Tom. 1.

† Rhedi—vena nempe jugularis vivi canis inflatur, protinus coagulatur sanguis, et cita mors sequitur liberum aëris per sanguinem iter. Opera. vol. 6. p. 223. et Sprœgel lib. cit.

‡ Richerand's Physiology.

‖ Il parait, par le résultat des différentes expériences, que la seule dilatation forcée des vaisseaux par des liquides quelconques, injectées, est suffisante pour causer la mort des animaux vivans sur lesquels on la pratique. Mahon, Méd. lég. tom. ii. p. 282.

Every organ of the system is endued with a peculiar excitability. The stomach is calculated to receive the various articles of food, but it will reject bile and blood. The bronchia and lungs are adapted to the impressions of atmospheric air; whereas a drop of water, &c., accidentally admitted into them, will produce violent coughing, and many gases are highly injurious. In like manner, the internal surface of the blood-vessels possesses a peculiar irritability, to which the blood alone is adapted.

But allowing that by the injection of the viperine venom, a solution of opium, &c., death is produced, independently of the natural consequences of the introduction of foreign substances into the circulation,* (which is very probable,) is it necessary that it should operate upon the *blood*, in order that death should be produced? Fontana concludes that it is—the inner membrane of the blood-vessels, according to him, being des-

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* *i. e.* By the peculiar operation of the poison.

titute of nerves. But granting, for a moment, that the internal membrane of the blood-vessels is *not* furnished with nerves, as advanced by FONTANA, is it necessary that such should be the case, in order that the *nerves* may be primarily affected? I think such an effect may be conceived without recurrence to this circumstance. For, may not these poisons, when injected into the blood-vessels, act upon the *nerves of the heart*, of which Professor SCARPA has observed many naked points upon its inner surface? or may they not act upon the *brain* or *spinal marrow*, the immediate sources of the nervous energy? Accordingly, Dr. MUNRO has found that a solution of opium thrown into the *heart* instantly paralyzes that organ; and that although the aorta be tied or cut, every part of the system is affected.* He found, that applied to the *brain*, it excites instanly violent and universal spasms, and that by injecting it in such manner as to make it pass along the *spinal marrow*, the animal is instantly killed.

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* The same experiment was repeatedly performed by Dr. Whytt, and uniformly with the same results.

But it is highly probable that the inner membrane of the blood-vessels is *not destitute* of *nerves.* They possess an acute sensibility to the stimulus of their contents; which, by causing their contraction, keeps up the circulation in the extreme vessels, into which the action of the heart alone could not propel the blood. Hence a paralytic limb, in which this irritability is destroyed, becomes cold, flaccid, and shriveled, because the circulation in it is passive, and wholly maintained by the *vis a tergo*, which is not sufficiently great to inject the capillaries; or it becomes œdematous, because the vessels have lost their power to propel it onward, and return it to the heart. It is certainly very irrational to deny the existence of nerves in the internal membrane of the blood-vessels *because they cannot be perceived.* This is a mode of reasoning which would inevitably lead to universal scepticism—and again involve philosophy in the chaos into which the absurdities of Cartesianism once plunged her. In the natural state of the transparent cornea, the pleura, the peritoneum, tendons, &c., even blood-vessels themselves cannot be seen: but their vascularity and acute sensibility in disease prove that they

are not destitute of blood-vessels or of nerves. In like manner, the most diligent and minute search has never yet detected absorbents in the brain; but that they do there exist, is not only proved by certain effects referable only to their action, but by the very existence of the organ itself—for blood-vessels, nerves, and absorbents must necessarily exist in every organized animal solid.*

Objection II. It is a fact well known, that some of the most active poisons are *innocuous when taken into the stomach.* Thus, repeated experiments have shown, that the *variolous matter* may be swallowed without any sensible effect upon the body.† The *virus of hydrophobia* may be taken into the stomach with impunity.‡ The *venereal virus* has been swallowed by accident,

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* Bell's Anatomy, vol. 4. p. 183. Richerand's Physiology, MS. Notes, Dr. Post's Lectures, &c.

† Rush. Foderé Méd. Lég. tom. iii. p. 474.

‡ Galen de temperamentis, lib, iii.. cap. ii. Mead's Works, p. 151. Rush's Inquiries, vol. ii. p. 326. et Medical Commentaries, vol. 8. p. 409.

and administered in experiments and no disease has been the consequence.* The *woorara poison* produces no effect whatever upon the stomach.† The *flesh* of *animals* that have died of pestilential and contagious diseases may be eaten, without reproducing the disease in those animals that feed upon their carcases.‡

That the *viperine venom*, and the *poison* of *all serpents*, are perfectly innocuous in the stomach, is a fact which has been noticed in the earliest times, and which has been established by subsequent observation.§

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* Hunter on the Venereal. Bell on Lues Venerea.

† Dr. Bancroft's History of Guiana.

‡ Mead's Works. Discourse on the Plague, p. 161. Rush, vol. 2. p. 326.

§ Noxia serpentum est admisto sanguine pestis
Morsu virus habent, et fatum dente minantur:
Pocula morte carent. *Lucan. Pharsal.* lib. ix. v. 614.

Non gustu, sed in vulnere nocent. *Celsus, Medicin.* lib. v. c. 27.

Venenum viperinum ventriculo ingestum *impune fertur.* Plenck, Toxicolog. p. 10.

Vid. et Boerhaave de morb. nervor. Tom. 1. p. 207. Rhedi, Observations on Vipers, &c. p. 17.

It may perhaps be alleged that an organ so sensible as the stomach, and holding such extensive and intimate communion with every part of the system, would be violently acted upon by poisons of so virulent a nature. But may not its very exquisite organization and vitality, enable it to resist the activity of these substances? For it is a law of the animal economy, that parts which possess most vitality best resist the action of morbific causes. Hence even arsenic is less violent in its effects upon the system, when administered by the stomach, than when applied to a wound or any broken surface.*

But independently of this *vital* preservative action of the stomach, are there not *chemical properties* in the juices which it secretes, that may neutralize these poisons, and render them inert? This is neither difficult to conceive nor to prove.

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* A fact noticed by Mr. J. Hunter and Sir E. Home, and confirmed by the experiments of Mr. Brodie.

The experiments of Dr. VALLI,* that lamented victim of his zeal for the advancement of physical knowledge, have incontestably established the power of the gastric juice to *correct the venom* in the slaver of *mad dogs*, and in the poison of *vipers*. This ingenious and intrepid experimentalist found, that even the *variolous poison*, and the *pestilential virus* were rendered perfectly innocent by the gastric juice; or that in those cases in which their morbific property was not completely annihilated, the disease which was produced by the inoculation of this compound matter, was so mild as not even to endanger life.

In these cases, doubtless, the proportion of gastric juice was not *sufficient to neutralize* the poison. Thus FONTANA found, that the poison of the viper is innocent to the stomach in *small* doses, but that "it is both *hurtful* and *mortal* taken in *large*

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* Sulla peste di Constantinopoli de MDCCCIII. giornale del Dottore Eusebio Valli, &c., a faithful translation and abridgment of which, by Dr. S. L. Mitchill, may be seen in the Medical Repository, vol. 3. New Series, p. 362.

quantity,"* to which the vital energy of the organ cannot make adequate resistance, and which the gastric juice cannot wholly neutralize. And hence, also, we find that small-pox *may* be communicated to the system by the introduction of variolous matter into the stomach.†

Analogies in support of this opinion present themselves on every side: thus the mineral poisons are neutralized by alkalies, vegetable infusions, &c.;‡ corrosive sublimate is counteracted by the albumen of eggs;§ acids destroy the power of vegetable poisons, and the pestilential effluvia of infectious diseases.

When we reflect upon the wonderful powers of the gastric liquor; when we see the sensible and chemical properties of the substances ex-

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* Vol. 2. p. 323.

† Medical Repository, vol. 1. p. 258.

‡ MS. Notes on Dr. M'Neven's Lectures on Materia Medica.

§ Orfila, Toxicolog. p. 84.

posed to its action, so completely altered as scarcely to be recognised, when ejected from the stomach but a few minutes after being swallowed,* we can readily conceive, how it may possess the power of destroying the noxious principle of these poisons; and with these facts before us, which so perfectly accord with this idea, and which admit of no other explication, it amounts almost to a demonstration. How feeble then is this objection to the theory against which it has been advanced. Further to refute it, would be to attach to it a degree of importance which it does not merit.

Objection III. With regard to the objection, which has so strongly been urged against our theory—that the *venom of the viper exerts no influence upon the nerves, when immediately applied to them*—it is only necessary to remark, that the manner in which these experiments have been

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* Richerand's Physiology, p. 116. For further on the subject refer to Spallanzani's Natural History, Stevens on Digestion, Fordyce on Digestion.

conducted, renders them wholly invalid. For what inference can be justly deduced from the application of such substances to the *trunks* of nerves, which are not only invested by a thick membranous sheath, but which possess little or no sensibility, but that all such experiments are wholly inconclusive, and are of no importance whatever in determining the question? For it is a fact, that the sentient principle resides principally, if not exclusively, in the fibrillar extremities of nerves.

But Fontana found that *all* the animals, upon which he performed these experiments, *died* sooner or later; and although he ascribes their death to the mechanical irritation of the teeth, with which he applied the venom, it is certain, that they died *many hours sooner* when the poison was applied than when the wound was inflicted with a clean tooth;* and, in the former cases, spots were found in the lungs, in the distant

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* Vid. vol. 1. p. 352.

muscles, and in the course of the nerves, which were not observed in the latter.

But it is indeed strange, that after FONTANA had "confessed that circumstances were *sufficient to induce one to believe* that the venom of the viper *has* in effect a *strong action upon the nerves*,"* "*he could find nothing* that could give him the smallest suspicion of the *nerves* being a means of communicating the venom of the viper to an animal;"† and that after boldly asserting, as a "*demonstrated* truth," that "*the venom of the viper is altogether innocent to the nerves*,"‡ he should, in the very succeeding paragraph, confess "that the experiments which he had just related," and upon which he founded his opinion, "were *not yet sufficient to satisfy and convince him;*" and that "*every thing concurred to a belief of the contrary.*"§

II. We come now to consider those poisons, the action of which cannot be explained upon the

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* Vol. 1. p. 340. † p. 351. ‡ p. 353. § p. 357.

principle laid down; and to investigate those laws by which they are governed. These are those poisons which are the *result of diseased action* in the animal body, and which retain the property of *reproducing the identical disease in other animals* exposed to their influence. They are hence designated by the term *Specific Morbid Poisons.*

The peculiarity in their action, which constitutes them an *exception* to the law, that we have seen universally applicable to those poisons which have heretofore been considered, is, that *in order to affect the system, they must necessarily be absorbed and conveyed into the circulation.*

The proofs of their absorption are numerous and conclusive. Thus, when the matter of small-pox, or of measles, the poison of hydrophobia,*

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* Mr. Hunter and Mr. Cruickshank have remarked painful enlargements of the axillary and inguinal glands occurring after the bite of a mad dog, and have traced the inflamed absorbents to the wound.

or of syphilis, or the pestilential virus, is in any manner inserted beneath the cuticle, the absorbents of the limb shortly become inflamed throughout their course, and the lymphatic glands enlarged and painful. This phenomenon is actual demonstration that absorption has taken place.* And whatever sophistry may be employed to prove the fastidious nicety of sense, which the mouths of the absorbents possess, so that they reject every thing which is acrid in its nature, or hostile to the body, or which has not undergone the process of assimilation, the fact is certain, that the most irritating and stimulating substances are readily taken up by them.†

It is almost unnecessary to adduce facts to establish this assertion, the truth of which is

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* I am aware that it will be objected that this is no proof of absorption, for the wound of a pin, a needle, or scissor will produce the same appearance. But it is the result of *punctured* wounds only; in which cases the inflammation is not confined to the absorbents. However, it is a fact that the same phenomena will be observed when the poison is applied in any other way than inoculation, as by friction or simple contact.

† Cruikshank, Anat. of Absorb. p. 123.

proved by daily experience; but consistently with our intention we must "*prove all things.*"

Dr. Musgrave* found that *indigo* and *blue-stone* would colour the chyle of dogs, when mixed with their food; and, in conjunction with my friend, Professor Francis, I have made a similar observation. Dr. Fordyce obtained the same results by experiments upon sheep.† That not only the blood, but even the secretions derived from it, will exhibit the peculiar qualities of certain substances which have been taken into the stomach, the following facts show: the *milk* may be tinged by madder, indigo, saffron,‡ &c., and will afford the peculiar odours of garlick, turpentine, pepper-grass, juniper-berries, balsam-copaibæ, &c. Baudelocque§ has detected mercury in the *liquor amnii;* and Haller tells us, that he has seen it tinged by the use of

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* Philosophical Transactions.

† Fordyce on Digestion.

‡ Haller, Element. Physiolog. tom. 2.

§ Midwifery, vol. 1.

saffron.* The *urine* will assume a bloody hue from the use of logwood;† and it is rendered alkaline by the use of alkalis.‡ The test of iron will exhibit the presence of this article in the urine of those who are under a course of chalybeates.§

But, says a late ingenious writer,‖ "that these articles have made their way into the blood, will not be denied. But they entered it, not in a crude and formal state, but as a digested and assimilated portion of chyle. Hence they became a component part of the blood itself, perfectly homogeneous with its other parts." But to account for the presence of their peculiar qualities in the

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* Haller. Element. Physiolog. tom. 3.

† Percival, Essays.

‡ Home, Clinical Experiments and Histories. Huxham on Fevers, p. 49. 181. Haller. Element. Physiolog. tom. 2. p. 90. Percival's Essays, Medical and Experimental, vol. 3. 316. Trotter's Medicin. Naut. tom. 2.

§ Chaptal's Chemistry, part 3. c. x.

‖ Charles Caldwell, M. D., in a late edition of Cullen's First Lines. Philadelphia, vol. 1. p. 114.

various secretions, he supposes that they are "*revived*," and "*recomposed*," by some wonderful process; frustrating, by this means, the very intention which he supposes nature has in view, that nothing crude or unassimilated should elude the vigilance of the absorbents. Now, as a necessary consequence, he has denied the existence of these articles in the *blood:* i. e. as exhibiting their peculiar properties. But we are assured, on no less authority than MEAD, BOERHAAVE, and HALLER, that globules of mercury have been seen to flow out with the blood in the operation of phlebotomy. Mr. MENGHINI found that the blood of persons under the use of chalybeates, abounds with iron.*

The yellow tinge of the skin, and adnata in jaundice, proves that bile exists as bile in the serum of the blood, exhibiting, at least, one property which is not "perfectly homogeneous," with the blood.

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* Chaptal's Chemistry.

Other facts, which will hereafter be adduced, will tend to show that foreign, unassimilated matter may be absorbed into the circulation without losing or changing its peculiar qualities.

Dr. Caldwell has repeated an argument, that has very frequently been urged, that no crude, *unassimilated* materials can be introduced into the blood-vessels, without immediate and great suffering, if not actual death. Now, this argument has really very little weight; for, as we have seen before, the transfusion of *blood* from the system of one animal into that of another is productive of the same effects. It certainly will not be denied that blood is perfectly assimilated; and the manner in which transfusion was performed precludes the supposition that its properties might have been changed during the operation. Nay, even chyle itself cannot thus be introduced without fatal consequences. Now, is it not as easy to conceive that these acrid substances, by being mixed and diluted with the lymph or chyle, may become less active, and thus gain admission through the absorbent mouths, as that carbonic

acid gas, which, when inspired alone, will spasmodically close the glottis so as to prevent its entrance, will easily find admission when mixed and diluted with atmospheric air?

To proceed, then, with the proofs that these poisons are absorbed. If, as after the bite of a rabid animal, a ligature be applied to the limb, or as in bubo, the enlarged gland be extirpated, so as to arrest the further progress of the poison, the constitutional affection is prevented: another evidence that these poisons find their way into the system by absorption, and that the general disease depends wholly upon this process. But it is more particularly manifested by those successive changes which we shall now endeavour to trace.

The poison, being thus deposited in the blood, is conveyed, by means of the circulation, to every part of the animal machine; being mixed and diffused in each successive portion of the circulating fluid with which it comes in contact, and imparting to each its peculiar properties,

these become new ferments,* or centres of infection, until, at length, the *whole mass of blood* is contaminated, and *assimilated* to the specific nature of the virus introduced.†

According to the nature of the poison, the predisposition of the system, and other circumstances to us imperceptible, will be the time required for the assimilation of the mass, and the production of the symptoms consequent upon this general vitiation.

Thus, the symptoms of *hydrophobia* never come

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* The process of fermentation being a chemical one, cannot be precisely similar to the process of assimilation in the living body, which is exclusively a vital action; but it conveys so good an idea of it, that the analogy may be admitted with the limitations with which we would be understood to restrict it.

† This doctrine of assimilation is ably illustrated in Walker on Small pox, Hosack on Contagion, Dyckman's Pathology of the Human Fluids, and in the American Medical and Philosophical Register, by J. W. Francis, M. D. vol. 4. p. 484. Cruickshank, Anatomy of the Absorbing Vessels, Bell on Gonor. Virulent. and Lues Ven. vol. 2. p. 164.

on until at least *six* or *eight* days after the bite of a rabid animal. They sometimes do not appear until *six* or *seven weeks* have elapsed;* and the virus sometimes remains in the system *months*, without producing any sensible operation. Dr. MEAD tells us, that the disease frequently does not appear until the eleventh month.† Galen observes, that it is sometimes deferred until the expiration of a whole year. It is, however, generally developed in fifteen or sixteen days,‡ or in thirty or forty at furthest.

The peroid during which the poison of *small-pox* remains latent, varies from the *third*, *fourth* or *fifth day*, to the *sixteenth*, the *seventeenth*, or *twenty-third* day.‖

The constitutional symptoms of *lues venerea*

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* Thomas's Practice, p. 357.

† See also Boyer's Surgery, vol. 1. p. 210. Americ. Edit.

‡ Mead. Thomas. Boyer, &c.

‖ Cullen's First Lines. Haygarth's Sketch of a plan to exterminate the casual small-pox from Great Britain. Thomas's Practice.

manifest themselves in *six* or *eight weeks*, though sometimes not for *months*.*

This humoral vitiation, I am aware, has been denied by some, who, blinded by the prejudices which necessarily result from their fondness for preconceived opinions, have not been able to appreciate the force of the innumerable and conclusive facts upon which the doctrine is supported; or, led away by that intemperate zeal which is the inseparable companion of error, have dogmatically denounced as designed misrepresentations, or dangerous delusions, militating against every principle of sound physiology, all the phenomena which their own contracted views of the animal œconomy cannot embrace. Such a spirit is unworthy of a science that has nature for its guide and truth for its object!

Let us enter into a brief examination of the facts upon which the humoral pathology is erect-

* Thomas, ib. p. 530.

ed, and by which the doctrine of assimilation particularly is confirmed.

The symptoms of the malignant or typhous state of fever, are such as evince a putrescency of the humours. The blood recently drawn at this period of diseases exhales a putrid odour, and exhibits evident marks of a dissolution of its crasis.* The cadaverous exhalations from the morbid body, the fœtor of the excretions, the evolution of air in those cavities in which particularly fluids abound, and the speedy decomposition of the body after death,† prove, beyond a

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* Schwenke, Hæmatol, p. 90—129. Morton, Pyretol. p. 26. Huxham, de aere et morb. epidem. tom 1. p. 103. Huxham, Essay on Fevers, p. 42—51. Milman, Inquiry, p. 54. Pringle on Jail and Hospital Fever, p. 337. MS. Notes, Prof. Hosack's Lectures.

† Shebbeare, Practice of Physic, vol. 2 p. 169. Fordyce, Third Dissertation on Fever, p. 92. Walker on Small-pox, p. 109. Cleghorn, Diseases of Minorca, p. 95. Hunter, Diseases of Jamaica, p. 171. Percival's Essays, Med. & Experim. vol. 2. Rush, Medical Inquiries and Observations, p. 159. Hosack's Lectures. Dyckman, Inaugural Dissertation, p. 133.

doubt, that a strong *tendency* to putrefaction, and even a degree of *actual putridity* may, and does exist, even before the extinction of the vital principle.

It is objected to this doctrine, that as *rest* is a circumstance essential to putrefaction, it is impossible that this process can go on in the blood, which is continually in circulation. But it is undeniable that *excessive motion*, as well as rest, favours putrefaction. Nimia agitatio longe adhuc celerius putredinem inducit.* The observation is confirmed by Haller; and he adduces, in illustration, the well known fact, that the flesh of animals killed in the chase soon runs into decomposition.† By this excessive arterial action we may account for the speedy putrefaction produced by lightning, electricity, poisons, &c., exhibiting itself in hæmorrhages, purple blotches, yel-

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* Van Swieten, Commentar s. 84.

† Element. Physiolog. tom. 2. p. 84.

lowness of the skin,* tympanites, &c.; dissolving the blood, and preventing its coagulation.†

But it seems so absurd to deny that the blood, which, from its very nature is most liable to decomposition,‡ is wholly incapable of undergoing any change; and to exempt from the influence of disease any part of the living system, and particularly that in which the vital principle especially resides;§ that even were the facts on the subject

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* The putrefactive process changes the colour of the serum in the cavities of the body, and in the capillary vessels of the skin. This is observed in dropsical effusions. Sir John Pringle, Diseases of the army. Append. Exp. XLV.

† As observed by Mead, Fontana, Orfila, Wepfer, and other toxicologists.

‡ The tendency to decomposition is in direct ratio to the complication of the elements of bodies. Richerand, Elem. Physiol. p. 462. Thomson, System of Chemistry, vol. 5. p. 770. Alexander's Experiments, &c.

§ I am aware that this is a subject of dispute, but I think that, independently of the positive declarations of the holy scriptures, that "*in the blood is the life*," and the demonstration of its vitality which Mr. J. Hunter's experiments afford, it is perfectly absurd to deny the existence of a living principle in that which constitutes a part of a living system, and upon which every vital action immediately depends.

less numerous and forcible, we would be justified by the principles of sound pathology in considering the blood as being frequently the seat of disease.

This constitutional vitiation, and the assimilation of the blood to a peculiar poison, are manifested by the *constant reproduction of the same disease*, and this too *whatever secretion may be the vehicle of infection.*

Thus the venereal virus always reproduces syphilis, and no other form of disease: the matter of measles will not produce small-pox; nor the poison of small-pox generate the plague: the identical character of the disease is invariably preserved; for the poison although multiplied, is not changed.

And *whatever be the vehicle* of the poison—whether it be pus or lymph—whether the bile or the blood—whether the saliva or the perspiration —whether the pulmonary exhalation, or the invisible miasmata which emanate from the morbid

body, and taint and assimilate the atmosphere—the same law obtains.

That plague may be produced by inoculation with the *matter* of a pestilential bubo, is a fact established by evidence which cannot be resisted:* and that the *bile* and the *blood*† will also communicate the disease, rests on testimony equally sure. Dr. Desgenettes has distinctly and unequivocally declared, in a personal interview with Dr. Francis, his conviction of the contagious nature of the plague: and Sir Gilbert Blane and Sir James Mc Gregor have assured him, that the accuracy of the statement of Dr.

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* Sir Robert Wilson's History of the British Expedition to Egypt, p. 357. Mc Gregor's Medical Sketches. Sonnini's Travels, p. 497. Larrey's Memoirs of Military Surgery, p. 202. vol. 1. Désgenettes, Hist. Méd. de l'Armée de l'Orient. Mead's Works, p. 161. Thomas' Practice, p. 242. Hosack on the Laws of Contagion, p. 55. Also, MS. Notes on Hosack's Lectures.

† Dédier and Couzier's Experiments in Lowthrop's Abridgment of Lond. Philos. Trans. vol 3. p. 165. Also Dr. Valli's Journal before referred to.

WHYTE's inoculation and consequent death might confidently be depended upon.*

That *measles* may be produced by inoculating with the *blood* of morbillous patients, the experiments of Dr. HOME† have satisfactorily shown; that *small-pox*,‡ *lues venerea*,‖ &c., may be com-

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* Letter on Febrile Contagion, addressed to David Hosack, M. D. F. R. S., &c. by J. W. Francis, M. D., London, June 16, 1816. p. 23, &c.

† Medical Facts and Experiments. Professor Coleman, of the Veterinary College of London, assured Dr. Francis that he had communicated the *Glanders* from one horse to another by means of the *blood*.

‡ Mead, Medical Works, p. 252. Duncan, Commentaries, vol. 19. p. 249, &c. Jenner, in Medico-Chirurgical Transactions, vol. 1. Haygarth, Sketch, &c. on Small-pox. Thomas, Practice, p. 197. Memoirs of the London Medical Society, vol. 3. p. 364. Eclectic Repertory, vol. 1. Dr. Francis, in the American Med. and Philos. Register, vol. 4. p. 476. Foot on the Venereal, p. 468.

‖ Bell on the Venereal, vol. 2. Foot on the Venereal, p. 472. Swediaur on the Venereal, tom 2. p. 10. Thomas' Practice, p. 763. MS. Notes on Hosack's Lectures. American Med. and Philos. Register, vol. 4. 484. *Hydrophobia* has been communicated to the *fœtus* of a bitch, vid. N. Y. Med. Repository, vol.

municated to the *fœtus in utero*, and consequently through the medium of the *blood*, cases innumerable incontestably establish: even the *milk* and the *saliva* are capable of infection.*

Some have attempted to explain the constant reproduction of the same disease by supposing that the application of the poison to any part of the body will, by means of *sympathy*, excite and communicate a peculiar action throughout the system, whereby the peculiar virus is secreted.†

Were this the case, the appearance of the dis-

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12. p. 137. We are told that even *Dysentery* has been received in this manner. Zimmerman on Dysentery, p. 19.

* Underwood, Diseases of Children. Hosack's Lectures, &c. Facts of this nature abound in books of practice.

† Mr. J. Hunter first suggested this theory: it has been defended by many since his time. See Adams on Morbid Poisons. Swediaur on Venereal, tom. 11. Caldwell's edit. of Cullen, p. 92. Dr. Barthez claims this theory, vide, Nouveaux élémens de la Science de l'Homme. Montpellier, 1778.

ease in various and remote parts of the body would not be deferred for many days, weeks, or months. The operation of sympathy is more speedy; and it is *reciprocal:* so that when the venereal poison is applied to the throat, or mammæ, the genital organs would necessarily participate in the diseased action. But further, any counter-irritation by exciting a new action, would as effectually remove the disease as mercury, which is now well known to be the only remedy. For further on this subject I must refer the reader to Dr. J. W. Francis' paper in the American Medical and Philosophical Register, vol. iv.; also his Inaugural Dissertation on Mercury, in which this hypothesis is ably refuted.

When the fact is established, on evidence so clear and decisive, that *the fluids of the living body are capable of being assimilated to the specific nature of a poison introduced* into the system, what shall be said of those who deny the fact because they cannot comprehend the *manner* in which this change is effected? Their reasen we cannot convince, but we may subdue their presumption, by

reminding them that on every side there is a limit to man's understanding.

"Sufficit si quid fiat intelligamus, etiam si *quomodo* quidque fiat *ignoremus*." CICERO.

THESES,

OR

PROPOSITIONS

DEFENDED IN

THIS DISSERTATION.

I. The action of Poisons is not *Mechanical.*

1. Because, mechanical principles are inapplicable to the operations of a living system.

2. Because the action of poisons is most speedy and violent when administered in circumstances the most unfavourable to mechanical action.

3. Because the action of poisons is not always the same in similar circumstances; whereas the laws of mechanics are invariable.

II. *Chemical* Principles cannot be admitted to explain the action of poisons.

1. Because their effects are not always the same, which they should be, affinities under similar circumstances being always the same.

2. Because the poisons are not necessarily decomposed in the *primæ viæ;* and they are frequently, upon inspection

after death, found in the same state and quantity in which they were swallowed.

3. Because, frequently no lesion of the organic texture is to be discovered as the result of their action.

4. Because the appearances that are referred to their chemical action, can more satisfactorily be explained on other principles.

III. The Mineral, vegetable, animal, and aerial poisons do *not act upon the blood*, but *primarily* and *exclusively* upon the *nerves*.

1. Because, their action is too rapid to admit the supposition that they are conveyed into the blood.

2. Because a ligature applied to a limb will prevent the action of poisons upon the system, although the circulation be not interrupted in the part.

3. Because, a ligature upon the blood-vessels will not prevent the action of poisons.

4. Because, although the absorbents be tied, so as completely to interrupt the passage of the lymph and chyle, the action of poisons is not prevented.

5. Because, a ligature on the nerves, or a ligature applied so as to interrupt the influx of nervous energy, will prevent the action of poisons.

6. Because, the action of poisons is in direct ratio to the irritability of animals, which depends altogether on the condition of the nervous system.

IV. No *Objections* can be offered subversive of the Nervous Theory. For,

1. Although, when poisons are injected into the blood-vessels, death is the result: it is a fact,

I. That any foreign substance whatsoever directly introduced into the circulation will be productive of the same effects.

2. It may be proved, that they act upon the nerves of the internal membrane of the blood vessels, or of the inner surface of the heart, &c. For,

3. That the inner membrane of the blood-vessels is furnished with nerves, is proved not only by certain effects referable only to their action, but by the very existence of the organs themselves; and it is irrational to deny their existence because they cannot be perceived.

II. The fact that many of the most active poisons are perfectly innocent, in the stomach, constitutes no objection: For,

1. It is a law of the animal œconomy that parts which possess most vitality best resist the action of morbific causes.

2. Independently of the vital preservative action of the stomach, there are chemical properties in the juices which it secretes, that neutralizes these poisons, and renders them inert.

III. The objection that poisons exert no influence upon the nerves when directly applied to them, has no weight.

1. Because the manner in which these experiments have been conducted, renders them wholly invalid.

2. Because, we have before seen that poisons do act upon the nerves.*

V. The *Specific Morbid Poisons are absorbed* into the circulation, and do *assimilate the blood* to their peculiar nature. Their absorption is proved,

1. By the inflammation of the absorbents, and the enlargement of the lymphatic glands, in consequence of their inoculation, or application to a limb.

2. By the readiness with which the absorbents take up every thing presented to their mouths; even the most acrid substances.

3. By the prevention of the disease, by arresting the further progress of the poison.

The *Assimilation* of the humours to the specific nature of the poison is manifested.

1. By the constant reproduction of the same specific disease.

2. And this too, whatever secretion be the vehicle of infection.

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* It may, perhaps, be said, that this is begging the question, and refuting an objection by the proposition itself, against which it has been advanced. But, after having proved the truth of a proposition, may we not, agreeably to the rules of mathematical demonstration, thence infer the falsity of a contrary proposition? See Simson's Euclid, in which the demonstration of almost every proposition will afford examples of this mode of reasoning.

FINIS.

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