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WASHINGTON, D. C.

AN
INAUGURAL DISSERTATION:
BEING AN
A T T E M P T
TO DISPROVE THE DOCTRINE OF THE
PUTREFACTION OF THE BLOOD
O F
L I V I N G A N I M A L S.

SUBMITTED TO THE EXAMINATION OF THE
REV. JOHN EWING, S. T. P. PROVOST;
THE TRUSTEES, AND MEDICAL PROFESSORS OF THE
UNIVERSITY OF PENNSYLVANIA,
FOR THE DEGREE OF DOCTOR OF MEDICINE;
On the 2th. day of May, A. D. 1793.

BY ADAM SEYBERT, OF PHILADELPHIA;
*Honorary Member of the Philadelphia, and Member of the
American, Medical Societies.*

“FOR THE LIFE OF THE FLESH IS IN THE BLOOD.”

Leviticus.

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M,DCC,XCIII.



For John Redman Coxe
with the Compliments
of the Author.

TO THE REVEREND

JUSTUS HENRY CHRISTIAN HELMUTH,
D. D.

PROFESSOR of the GERMAN LANGUAGE in the
UNIVERSITY of *PENNSYLVANIA*, &c. &c. &c.

AS a small tribute of gratitude for his care
and attention, in directing and superintending my stu-
dies, whilst under his tuition, at the University,

THIS DISSERTATION IS,

Very respectfully inscribed,

By his obliged friend,

A. SEYBERT.

To CASPAR WISTAR, M. D.

ADJUNCT PROFESSOR OF ANATOMY, SURGERY, AND MID-
WIFERY IN THE *UNIVERSITY OF PENNSYLVANIA.*

SIR,

IN addressing this Inaugural Dissertation to you, every body that knows you will readily admit the peculiar propriety of the tribute. Your justly acknowledged abilities leave no room to doubt that I have an interest in making this offering. But, whilst I candidly admit this, I feel and confess profound obligations of gratitude to a beloved master, for his very beneficial instructions, and his many courtesies and attentions. To whom can the following production, with more evident propriety, be inscribed, than to him who planted the Seed of Knowledge, and, with anxious sollicitude, superintended the growth?—To you, Sir, I am indebted for whatever progress I may have made in Medical Science; and I am ambitious, that the first fruits of my labors should be presented to the world under the adorning sanction of your patronage.

The cordiality of your friendship has naturally inspired me with sentiments of esteem and attachment, which it will ever be my pride, as it is my duty and interest, to cherish.

Believe me, Sir, nothing can ever obliterate from my mind the mingled sentiments of gratitude and esteem with which I remain

Your obliged friend and Pupil,

A. S E Y B E R T.

INTRODUCTION.

THE Opinion, that the blood became putrid in many Diseases, is of ancient date. It has been embraced, with various modifications by most of the sects in Medicine. It particularly engaged the attention and belief of the learned Boerhaave, who publicly taught it in the celebrated school of Leyden. Under the imposing authority of his name, and by means of his industry, it was diffused throughout almost the whole globe. It became the theme of the vulgar; and at this day has many great and illustrious names to support it.

There have been, however, at different times, a few Medical Philosophers, who, although they adopted it in some instances, seem to have had doubts respecting the truth of the doctrine, and to have rejected it in their explanations of many diseases. In this class may be reckoned the celebrated Hoffmann, who explained the phenomena of many disorders without any regard to the state of the blood: He was followed in this by the great Cullen, who, in his system, attends less to the state of the fluids in diseases than any of his predecessors. The doctrine has of late been wholly denied by the very ingenious Dr. Milman in his treatise on the Scurvy; and entirely rejected by the late sagacious Dr. Brown of Edinburgh, from his System of Medicine. Many physicians appear to have adopted the sentiments of these gentlemen; so that at present there

is a diversity of opinion among medical philosophers respecting the Putrefaction of the Blood of *Living Animals*.

On examining the subject, I found that it had never been put to the test of fair experiment; but, that speculative reasoning (too often delusive at best) and a few indecisive facts, were the chief supports of the argument on both sides. With these data, the result was neither satisfactory nor convincing. I then determined to contribute my mite towards investigating the matter by experiment. In the prosecution of this attempt, my attention has been directed to the advancement of Science; and, in the detailing of the Experiments which I have made with this view, truth shall be my polar star.

I have been induced from reflection and experiment to adopt a positive belief on this subject, and to deny the truth of the doctrine which I have just noticed. I am, therefore, necessarily led into an opposition to the opinions of many celebrated men. But, I trust, I have observed a decent regard and veneration for those from whom I dissent, without being at all depressed by the weight of their authority. Having viewed nature *attentively*, I shall endeavour to communicate *accurately* what I have observed. I will not sacrifice truth to the lustre of great names, but, with confidence adopt the sentiment of Cicero—"Mea fuit semper hæc in hac re voluntas et sententia, quemvis ut hoc mallet de iis, qui essent idonei, suscipere quam me: me, ut mallet quam neminem."

INAUGURAL DISSERTATION.

IT will be proper, before I enter upon the consideration of the Putrefaction of the Blood of living Animals, to take a brief view of *Putrefaction in general*; for, this is, indeed, the only method by which we can be prepared for an examination of the circumstances necessary to induce that state of the blood in living animals.

Though Chemistry is much improved, and numerous discoveries are made almost every day, by different philosophers, *Putrefaction* is, at this moment considered as the same difficult subject, that it was in the days of the celebrated *Lord Bacon*.

This last stage of Fermentation, in the days of Stahl, was supposed to be a mere consequence of the *vinous* and *acetous* stages; but, modern discoveries teach us the contrary; for we find, that while some substances undergo only the last stage, others suffer the three successive changes in a regular manner: thus mucilages, &c. become acid without undergoing the vinous fermentation, and the glutinous matter of vegetables will putrefy before it undergoes either of the other changes.

Observation, the grand parent of discovery, has taught us, that no substance is capable of undergoing a change by the putrefactive fermentation, except it be *animal* or *vegetable*; and that the numerous classes of the productions of nature, comprehended under the title of the Mineral Kingdom, are excluded. It is also an opinion, generally established, and proved by experiment, that the fluid and softer parts of those bodies, putrefy much sooner than the harder and more solid parts. It has likewise been observed, "That the flesh of younger animals is somewhat more prone to putridity, than that of older animals."*

Animal and vegetable matters cannot putrefy in every situation or condition in which they may exist; for it is necessary that a living animal or vegetable should undergo a considerable change, before it can be rendered capable thereof: It must even be deprived of life, or the vital principle. No one has ever seen an entire animal or vegetable putrefy whilst alive; and Beccher, on this subject, beautifully observes: "*Causa putrefactionis primaria defectus spiritus vitalis balsamini est.*" And, indeed, in all researches into those kingdoms which are the subjects of *fermentation*, it is of so great consequence to keep this universal actuating principle in view, that by neglecting it, we may commit great mistakes, and look to other causes than the true ones for its palpable effects; insomuch that the learned Chaptal, when regretting the imperfect success which Chemistry has met with, in the analysis of animal matters, cannot help observing it. "All (says he) have mistaken or overlooked that principle of life which incessantly acts upon the solids and fluids, modifies, without ceasing, the impression of external objects; impedes
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* Medical Commentaries, Vol. II. p. 142.

the degenerations which depend on the constitution itself; and presents to us phænomena which chemistry never could have known or predicted by attending to the invariable laws observed in inanimate bodies.”*

The presence of that invisible elastic fluid, which we term *vital air*, is so necessary to putrefaction, that a body cannot putrefy without being in contact with it; and may be preserved sound and pure for years if the communication between them be destroyed. It is a well known fact that a body will not putrefy *in vacuo*: This has been noticed by an ingenious author in the following words: “How much the air contributes to putrefaction, is evident hence, that bodies buried deep under the earth, or in water, out of the reach of air, shall remain for ages entire; which yet, being exposed to the open air, shall soon rot and moulder away.”†

It appears that too great a degree of moisture, or a total want of it, retards the process of putrefaction. In order, therefore, that a body may putrefy, it is necessary that it be only duly moistened. Thus it happens, that after an animal or vegetable substance has been made perfectly dry, it may be preserved, in that state, for many years after. It has been observed by the immortal Béchér, that too great a degree of moisture prevents putrefaction: These are his words: “*Nimia quoque humiditas a putrefactione impedit, prout nimius calor; nam corpora in aqua potius gradatim consumi quam putrescere, si nova semper affluens sit, experientia docet: unde longo tempore* *integra*”

* Chaptal's Chemistry, Vol. III. p. 280.

† Frewen's Physiology, p. 128.

integra interdum submersa prorsus a putrefactione immunia vidimus; adeo ut nobis aliquando speculatio occurreret, tractando tali modo cadavera anatomix subjicienda, quo diutius a foetore et putrefactione immunia forent.”*

That *all* enlivening principle, *heat*, which, in a certain degree, is necessary to life, is no less necessary to the bringing on of the dissolution of a body. Temperature has been found to have great influence in promoting and retarding putrefaction.

I have now mentioned the most essential circumstances necessary to promote the inception of putrefaction; to which we may subjoin *Rest*; for bodies do not putrefy while in continual motion. In proportion as these circumstances take place the process will advance with greater or lesser rapidity.

Certain substances, as well known to the vulgar as to the philosopher, by the name of Ferments, when added to a fermentable mass, are found to hasten the process in a manner truly astonishing; though both the peasant and the philosopher stand on an equal footing with regard to a knowledge of the principle by which their application produces a specific operation. “We are told indeed (says the ingenious Mr. Henry) that a vinous ferment induces the vinous, that a ferment of an acetous kind brings on the acetous fermentation, and a putrid one, that fermentation which ends in putrefaction. But we receive no more information, relative to the manner in which they produce those effects, than we do with regard to fermentation itself.”†

I will

* *Phyf. Sub. lib. 1. f. 5. cap. 1. p. 277.*

† *Manchester Memoirs, Vol. II. p. 259.*

I will now proceed to examine, whether, in becoming putrid, a body undergoes any considerable change; and, whether it be possible to remove putrescency after it has taken place.

The very meaning of the word *putrefaction* conveys the idea that an essential change must have taken place in any substance which has acquired a putrid state.

Putrefaction reduces both animals and vegetables to the same principles, for, it causes an entire and complete decomposition of them, insomuch that it is difficult, and indeed impossible, to distinguish between a putrefied mass of animal and one of vegetable matter. The former characteristics of each are at an end. Colour, texture, and every sensible quality of the body, are thereby destroyed. It reduces the animate part of the Creation to an indiscriminate level with inanimate matter. There is abundance of truth in the observation, that bodies, in this process, undergo a new combination, as well as separation of their constituent parts. Putrefaction causes the sweetest substances to become the most offensive and disagreeable to the smell; and, instead of a vegetable acid, at length produces a volatile alkali—bodies possessing very opposite principles.

Many philosophers have gone so far as to say, that, by certain chemical processes, they were able not only to render putrid substances sweet again, but restore them to the condition they were in, previously to their undergoing this peculiar change. This, according to them, is to be effected by surrounding the putrefied body with an atmosphere of fixed air; to the loss of which principle, an enlightened

enlightened philosopher wholly attributed the changes produced in a body by putrefaction ; though the opinion has been ingeniously refuted, and therefore needs not much consideration in this place*.

It is true, that by surrounding a putrefied body with fixed air, we shall prevent the advancement of the process ; yet the fixed air has no specific operation in this respect, for other substances possess a similar property. After I had suffered several pieces of highly putrid beef to remain completely covered with fresh pump water for several hours, and then washed them frequently in water several times renewed, I found, that by this operation the water gained a highly offensive and putrid smell, and that the meat had lost a great degree of its own ; but its solidity was not in the least restored. It is a common and well-known fact in domestic œconomy, that meat, which has become tainted, is very frequently washed in fresh water before it is cooked ; and the reason assigned for this process is, that the meat is thereby rendered sweeter. Thus do house-keepers reason from a knowledge of the fact without any theory to bias them.

Hence it appears very clear, that to explain this fact no specific operation is necessary to be recurred to, since water, free from the combination of fixed air, had effects similar to those produced by fixed air itself. Mr. Chap- tal, in my opinion, explains the operation of this, and of the other substances which are said to have the like effect, upon very simple and philosophical principles. When speaking of the necessity of the presence of vital air to
putrefaction,

* Medical Commentaries, Vol. II. p. 150.

putrefaction, he says, " We shall observe, on this subject, that the effects observed in flesh exposed to the Carbonic acid, Nitrogene gas, &c. are referable to a similar cause; and it appears to me that it is without sufficient proof that a conclusion has been drawn, that these same gases, internally taken, ought to be considered as antiseptic; because, in the cases we have mentioned, they act only by defending the bodies they surround from the contact of vital air, which is the principle of putrefaction *."

To me it appears plain, that, so great is the chemical change and decomposition which a putrid body has undergone, that no means can remedy it. On this point Fontana truly observes, " We do not know any power, nature herself does not disclose any, that can recompose an organ that is destroyed, and entirely decomposed by putrefaction, or by the concussions of external bodies. This is what has never yet either been accomplished or seen. We have, therefore, every possible reason not only to believe an animal that is reduced to this state dead, but likewise to believe it dead for ever †."

From the above consideration of putrefaction, as going on out of the body, it appears, that a certain degree of heat, the presence of vital air, a certain degree of moisture, together with rest, are indispensably necessary to putrefaction; and that without their presence no body can putrefy. We also know, that by adding certain substances to bodies which are to undergo the change, the process is hastened in a manner surprising to every one who has ever had occasion to notice it.

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* Chaptal's Chemistry, Vol. III. p. 398.

† Fontana on Poisons, Vol. I. p. 406.

Having considered the circumstances in which fermentation, in general, takes place, as it is conceivable that if the above circumstances be present in the living animal, the process might readily go on in it—let us examine if these necessary requisites be present, so as to act in a due and proper manner upon that vital fluid, *the blood*.

Several questions occur here.—Is there any difference between dead and living matter?—Is there not in living animals a positive power of resisting putrefaction?—Whether the nice and inexplicable operation which is employed in converting the dull mass of the motionless creation into the peculiar condition of organic sensibility, acts to no purpose?—In fact, whether the principal characteristic of death, has not been determined by physiologists to be the beginning of putrefaction in the body?

The principle we denominate *vital*, with whose effects we begin to be acquainted, though its nature and origin will perhaps ever be hidden from us, is found to be the chief impediment to the putrefaction of a living animal. As soon as the animal is deprived of it, putrefaction is the inevitable consequence, except the above precautions be well observed. This may be fairly demonstrated. All the circumstances necessary to putrefaction take place with respect to the external surface of our bodies, yet they do not putrefy while alive; but if life be by any means destroyed, they will as readily undergo the process as other inanimate matter.

It is to be observed, that by the living principle is understood that power which in an animal actuates its whole system, or from which is derived sensation, moti-

on, and the other qualities of life. It is the cause of the preservation of the body from dissolution, and is capable of existing, for sometime, under a suspension of all its actions*.

We will now proceed to consider the presence of *heat*, pure *air*, *moisture*, and *rest*, with regard to the blood; in order to determine whether we might expect a putrefaction of that fluid if the principle of life, were not alone sufficient to obviate it.

1st. Of Heat. Though different degrees of heat are found sufficient to maintain life, in different animals, yet no animal while alive has ever been found devoid of a considerable quantity of it; indeed, so careful was nature in this respect, that she has endowed man, and the inferior animals, with a power, whereby they are capable of generating heat;—a process the investigation of which has of late much engaged the attention of philosophers, and upon which considerable light has been thrown. It is probable that the blood of every living animal contains a degree of heat sufficient to support the process of putrefaction.

2dly. Pure Air. That element so necessary to our existence, and which we receive into our bodies, by means of those vital viscera, the lungs, is no less necessary to the maintaining of animal life, than to the process of putrefaction. A man will no more live, than a dead body will purefy, in vacuo †.

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* Gardiner's Animal Economy, p. 3.

† That air is a very active and powerful agent in putrefaction, is evident from the following fact, viz. Whilst pus remains shut up
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It was for a long time supposed that elastic air existed in the blood-vessels of living animals ; but, the experiments of the ingenious Darwin clearly prove the contrary ; the following is one of them. “ Part of the jugular vein of a sheep, full of blood, was included between two tight ligatures, and cut out while the animal was yet alive. It was immediately put into a glass of warm water, and placed in the receiver of an air pump. It sunk at first to the bottom of the water, and did not rise again, although the air was carefully exhausted. After this, it was wiped dry, and laid on the brass floor of the receiver. The air was again exhausted, but there was not the least visible expansion of the vein or its contents *.”

By the experiments above referred to it is reduced to a certainty, that no air exists, formally in the blood, while enclosed in the blood-vessels. And it is evident, that it was without sufficient grounds, that philosophers inferred, that air existed in the blood, while enclosed in the blood-vessels ; because they perceived it in blood drawn from a vein, and placed in the receiver of an air pump ; for during its exposure it must have had time to absorb air from the atmosphere.

The celebrated Huxham was of opinion, that, “ elastic air is probably generated in the arterial and venous systems,

in a perfectly close cavity it will keep sweet and inodorous, but on exposure to the atmosphere, it contracts in a very short time a putrid smell ; the same circumstance takes place with regard to extravasated blood.

* Medical Commentaries, Vol. VI. p. 35.

tems, in putrid fevers *," though he has no experiments to prove it. Let us then examine, whether air can exist in the blood-vessels of living animals.

With this view the ingenious Luzuriaga tried many experiments on living dogs. He injected several different kinds of air into the blood-vessels, and in every instance the animals were killed, in a very short time. I will only mention that he twice injected inflammable air; once phlogisticated air; once fixed air; once nitrous air. I will state the particular circumstances that happened in the instance, when dephlogisticated or pure air, which is the chief agent of putrefaction, was injected.—It was forced into the jugular vein of a dog, and in three minutes he died. On dissection, the blood appeared of a lively red colour, and frothy, but not grumous nor coagulated †.

Thus it appears, that air does not, and cannot exist, formally, in the blood-vessels of a living animal; a circumstance which refutes the opinion, that the blood is capable of undergoing the process of putrefaction, during the life of the animal.

3dly. Moisture. No person in his senses dares to deny the presence of this third circumstance, as essential to putrefaction, and therefore it needs no further consideration.

4thly. We have mentioned that it was necessary for a body, which is to undergo the putrefactive process, to be at rest. The continual motion of the blood of living animals,

* Medical Observations, Vol. III. p. 36.

† Luzuriaga's Inaug. Dissertat. Eden. 1786, p. 26.

animals, must certainly be a great impediment to the process; and indeed motion has been found to be a very effectual means of preventing it; even a brisk wind has been known to retard it*.

We have found that the life of the animal, the want of air in the blood-vessels, and its circulatory motion, were great impediments to the putrefaction of the blood; we shall now proceed to consider, whether the principle of animal life is contained in the blood.

The situation of that principle, to which we owe our existence as living beings, is at present much disputed. While some Physiologists will have it to exist wholly in the nervous system, in the form of a subtle fluid, whose presence the best microscopes have not been able to discover; there are others, whose authority is by no means inferior, who strenuously oppose the doctrine, and persist that blood partakes of it also. In this latter class we may rank the celebrated Hunter and Fontana.

That the life of the animal exists in the blood, is an opinion of as ancient a date as Holy Writ itself: It was the favourite sentiment of many ancient philosophers; and the great Harvey, to whom we are so much indebted, says, "the blood is the *primum vivens*, and the *ultimum moriens*, of the animal."

This hypothesis was for sometime sunk into oblivion, but was again revived and placed on a firm basis by the able Mr. John Hunter of London; who supports the doctrine
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* Medical Commentaries, Vol. II. p. 146.

by reasoning truly philosophical, and by experiments, in my opinion, incontrovertibly convincing. It would be needless for me to repeat all the ingenious arguments which are made use of by him on this occasion, as they may be easily seen, by referring to the Encyclopædia, Vol. III. P. 313.

His 5th argument alone would almost suffice to convince me of the truth of his opinion. “The blood (says he) preserves life in the different parts of the body. When the nerves going to a part are tied or cut, the part becomes paralytic, and loses all power of motion; but it does not mortify; if the artery be cut, the part dies, and mortification ensues. What keeps it alive in the first place? Mr. Hunter believes it is the living principle which alone can keep it alive; and he thinks that this phenomenon is inexplicable on any other supposition, than that life is supported by the blood”*.

This doctrine is every day gaining ground; and appears to be further supported by the opinions and experiments, of the celebrated Fontana; who observes, “What may lead one to suspect, that a very active and volatile principle does really exist in the blood, is, that the vipers venom prevents its coagulation when it is drawn from the vessels, and on the contrary, produces it in the vessels themselves. One would suppose (says he) in the first case, that something had flown off from the blood, which exists in it when it is enclosed by the vessels” †.

“My experiments on animals (continues this last mentioned enlightened philosopher) in which the nerves were bit
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* Encyclopædia Loc. citat.

† Fontana on Poisons, Vol. II. p. 135.

by vipers, shew that the venom is a substance perfectly innocent to these organs, that it does not occasion in them any sensible change, and that they are not even a means or vehicle of conveying it to the animal. In a word, it appears that the nervous system does not concur more to the production of the diseases of the venom, than does the tendon, or any other insensible part of the animal: on the other hand, all the experiments on the blood, the injection of venom into the vessels, and so soon constantly evince that the action of the venom of the viper is on the blood itself. This fluid is alone changed by the venom, and this fluid conveys the venom to the animal, and distributes it to its whole body. The action of the venom, and its effects on the blood, are almost instantaneous.”*

His experiments, in my opinion, allow this inference, that since life was not destroyed by the immediate application of the poison to the bare nerves, and that the loss of it was almost instantaneous when the poison was applied to the blood; there exists some quality in the blood, that does not exist in the nerves; and as death can only be produced by destroying the vital principle, this principle must consequently exist in the blood, and in a state different from that in which it exists in the nervous system.

I will now proceed to give a connected view of the reasons, which together with the experiments hereafter to be mentioned, induce me to deny the putrefaction of the blood to have ever taken place, and which confirm me in believing, that no such change can happen in the blood of living animals.

1st. We

* Fontana on Poisons, Vol. I. p. 396.

1st. We have seen that the process of putrefaction cannot take place in the animal or vegetable kingdoms, until after the animal or plant be deprived of life. I think we have given some proof of the blood's containing the principle of Animal Life, of which (whether it be convincing or not) the candid will judge. When life is once perfectly destroyed, there are no means, at our command, by which it can be again restored: therefore, we must infer, that all putrid cases are necessarily mortal, or a putrefaction of the blood cannot take place.

2dly. From viewing the process of mortification, as going on in the solid parts of animals—for the death of the part *invariably* precedes the symptoms of putrescency, as is evidenced by the want of sensibility in the part.

3dly. There is no vital air in the blood-vessels, and we know putrefaction cannot go on without it.

4thly. The blood in the living animal is in continual motion—a small degree of motion has been known to impede the process.

5thly. The doctrine of ferments. For as long as a drop of this putrid blood remained in the vessels, no particle could exist in them, without being changed by it; therefore it is a necessary consequence, that in every case, where putrefaction is supposed to have taken place in the blood, every drop of that fluid should be drawn from the vessels, and the vessels should undergo a perfect cleansing before healthy blood could exist in them; consequently, if the vessels are not cleansed, the process must necessarily
continue

continue for ever, and in attempting to cleanse them, would not death be the inevitable consequence? *

6thly. We are as yet unacquainted with any means by which we are able to recover a body, when once putrid; for, according to a chemical axiom, it has undergone a perfect decomposition in the process; and, certainly, without obviating the effects of putrefaction on the blood, and reducing it to its former state, the animal cannot exist for any length of time.

7thly. In every case of putrefaction a great quantity of elastic air is evolved, and no author (as far as I know) has made mention of the presence of air in the blood-vessels of those who have died of diseases, which were supposed to belong to the putrid class, and whose bodies were dissected immediately after death. It has been fairly proved that air, on being injected into the vessels, kills in a very short time †.

8thly. According to the ideas of the supporters of the putrefaction of the blood in diseases, the process must be

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different

* The difficulty of restraining putrefaction after it has taken place, and of destroying the putrid ferment in vessels that have contained putrid substances, is well known to the brewers. When their liquor becomes putrid, or as they term it, "foxed," the vessels which contained it become so contaminated, that they will excite the same fermentation in any fresh liquor that may be put into them, even after they are washed. It is only by white-washing or scouring them with lime that they can be thoroughly cleansed. I have heard of a brewer, who was so much embarrassed by a putrid ferment in his vessels, that he was about giving up his business, when he fortunately learned the use of lime.

† Vide ante Page 19.

different in Typhus, &c. from what it is in scurvy; since, in the former case, bark and wine, together with other stimuli, are the only true remedies; whereas, according to them, these remedies are found to be of little or no service in the latter, and fresh vegetables are its only remedies. For the blood, they say, is putrid in both instances; and since putrefaction going on in the same substance would appear to be always the same, we might, with propriety suppose, that, what retards and corrects it in the one instance, would do it in the other. Who has thought of exhibiting ~~such~~ *fresh* vegetables, in typhus, yellow fever, &c. and neglecting those powerful remedies, bark and wine?

gthly. It is incompatible with sound reasoning to suppose that putrefaction of the blood takes place in the plague, &c. since we find that persons sometimes fall down dead, immediately on being exposed to the effluvia, that have been said to give origin to this and other diseases of the same class, as the porters, who opened bales of goods in the lazarettos of Marseilles. Mead relates, that, "Upon opening one of the bales of wool in a field, two Turks employed in the work were immediately killed, and some birds which happened to fly over the place, dropped down dead*." How is it possible that the mass of blood, or any part of it, could become in the least degree putrid in those instances? For the space of time, before death was produced, was too short to allow of it. Since the poison kills so quickly, can we with reason suppose that an animal can live while the fountain of life is impregnated with it? If a small portion of such effluvia produces disease, when its action

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may, exclusively, be supposed to be merely on the external surface of the body, Is there not strong ground for believing that instant death would follow if the blood were in the least tainted by it ?

10thly. Experiments seem to prove, that the blood is peculiarly qualified to excite motion in the heart and arteries, and that no other fluid will answer the same purpose.—That even milk, and other bland liquors, when injected into the vessels of living animals, kill. We know it is an established truth, that by putrefaction bodies undergo a complete decomposition and dissolution of their of their constituent parts, and are rendered totally different from what they were before—How then can we imagine that life can be kept up whilst the blood is in a putrid state in the vessels ?

For these reasons I concluded that a putrefaction of the blood could not take place in the living body : but the importance of the subject induced me to engage in a series of Experiments which might enable me, with more indisputable certainty, to decide the question, Whether the blood of living animals can be rendered putrid ?

And this I suppose, if possible, would be ascertained by the following circumstances, viz.

I. By Starvation.

II. By a putrid Diet.

And, III. by injecting putrid and other substances into blood-vessels.

1st. Of Starvation. When we consider the many and various actions performed by man and other animals, we plainly see why a certain quantity of aliment is necessary to be taken by them. Physiology teaches, that by every action man suffers a loss of the solids, and that by every secretion the blood becomes less in quantity.

The blood being the fountain from whence all the solid parts derive their support, and the fluids their origin, it must naturally have been supposed to be affected by starvation, as it is immediately prepared from the aliment we take in.

If the case be as above stated, is it not surprising that man can live for a length of time without taking any nourishment at all? There are instances related where men have lived for six, twelve, and even fourteen days, or longer, without receiving any aliment*.

Drink is found to be no less necessary to the maintaining of animal life than the solid matters of our food. The necessity of this article was supposed to be merely in recruiting, blunting, and correcting the acrimony of the blood which would take place if we did not use them. And it has been found by experiment, that life may be supported a much longer time by water than it could without it. Rhedi, who made experiments to ascertain how much longer life might be supported by the use of drink, without the use of any other article of diet, than when the animal was totally deprived of all food, found, that fowls to whom he gave no drink lived until the ninth day ;
whereas

* Manchester Memoirs, Vol. II. p. 467. and seq.

whereas one who was allowed water, lived more than twenty days*.

Though man may live for a considerable length of time, without taking in any nourishment, yet his situation is not agreeable; for, he not only suffers pain, but all the functions, as well bodily as mental, are in consequence affected; his sensations are strange, his ideas confused, his sight deceptive and deranged, his countenance becomes pale and fallow, his weight decreases considerably, though the excretions are sparing or none at all, the respiration suffers, the action of the heart decreases in proportion, till at last no pulsation can be felt, debility by degrees overpowering life, death approaches and puts an end to his sufferings.

If after having fasted a length of time, the person recover, he becomes melancholy, hysterical, effeminate, stupid, &c. †

The functions performed by the fluids and solids of an animal, being so nearly connected the one with the other, have occasioned this question, Does starvation operate by inducing disease in the solids or fluids? Most physiologists are in favour of an opinion, that the fluids are particularly affected, and that by starvation the animal suffers from the quality, rather than from the deficient quantity of the blood. They suppose it to become acrid and even putrid.

To determine whether it was the blood that suffered, I performed the 2d. and 3d. following Experiments, and also

* Manchester Memoirs, Vol. II. p. 575.

† Haller's Elementa Physiologicæ.

also examined the phænomena presented by the blood of an healthy dog, the better to judge of the state of that fluid in all the succeeding Experiments. And let me here observe, that the Experiments I shall have occasion to relate, were made in the presence, and with the assistance of *my* ~~many~~ worthy friends, Messrs. Jesse Thomson, John Andrews my fellow graduate, and Jacob Wagner, junior. They were witnesses to all the phænomena; and I take this public method of acknowledging my obligations to them for their kind assistance.

EXPERIMENT I.

September 6th. 1792. An ounce of blood was drawn from one of the crural veins of an healthy dog, at 15 minutes after 2 o'clock, P.M. it was coagulated at 20 minutes after 2; separation into serum and crassamentum was evident at 34 minutes after 2. Serum did not change the colour of a piece of paper stained with a vegetable blue.*

EXPERIMENT II.

A young dog was kept for the purpose of starvation, and the phænomena were as I shall now relate.

I there-

* The vegetable blue was made use of as a test, with the view of ascertaining whether the serum of an healthy animal contained any alkaline salt in a formal state, as it is a well established fact that the volatile alkali is produced by putrefaction; and the production of it is one of the characteristic circumstances of that process; the same test was made use of in the subsequent experiments, to ascertain whether serum gained any alkaline properties by the different treatment which the several animals suffered.

I thought it not unnecessary to weigh him, at different times. I shall therefore mention the weight, as observed at those several periods.

July the 8th. he weighed $5\frac{3}{4}$ lb. July 29th. $7\frac{1}{2}$ lb.

Food was given him the last time, in the afternoon of the 29th. of July.

August 3d. he weighed $6\frac{1}{2}$ lb.

August 12th. he weighed 5 lb. At 2 minutes before 12 o'clock an ounce of blood was drawn from one of the crural vessels. The blood flowed freely, but by misfortune an artery was opened instead of a vein. The colour and smell of the blood were quite natural, being highly florid like arterial blood. It was coagulated at 3 minutes after 12—separation into serum and crassamentum was evident at 13 minutes after 12. The serum and crassamentum appeared perfectly natural. A piece of the blue stained paper was dipped into the serum, and no change of colour was produced. To prevent a further hæmorrhage, as he was much weakened by the bleeding, the wound was well closed, and a piece of sponge bound tightly over it. The leg became paralytic.

August 13th. the leg was much swollen below the ligature, and when handled, did not appear to give him any pain. The ligature was somewhat loosened.

August 14th. weighed $4\frac{1}{2}$ lb.

August 15th. This morning at 8 o'clock I found him dead.

During

During the above management the dog had very few evacuations by stool or urine, till the two last days of his life, when the urine dropped continually from the urethra.

His eyes were several times examined, and no alteration in them was evident.

He did not appear to suffer pain till the 3d. of August, at which time he cried very much, gaped frequently, and appeared very weak; weakness continuing daily to increase till his death.

I did not perceive that respiration was much affected; but the action of the heart became so feeble, that I could scarcely perceive its beating for some days previous to his death, though I applied my hand to the part where its stroke is generally felt.

The abdomen was much contracted, and the fore-part of it was drawn up to the spine.

The body was examined immediately on finding him dead, when appearances were as follow :

The stomach contained a considerable quantity of a whitish fluid, its texture was perfectly natural. The intestines were of a greenish cast. The other viscera appeared in a sound and natural state.

At 39 minutes after 8 o'clock, blood was caught in a tumbler from an opening made into the heart; colour and smell natural. It was coagulated at 45 minutes after 8. It began to separate into serum and crassamentum at 51 minutes after 8. Coagulum and serum perfectly natural. Serum did not change the paper stained blue.

E X P E R I M E N T H I I.

April 10th 1793, a dog was kept for the purpose of *starvation*.

April 20th. Blood was drawn from him at 15 minutes past 3 o'clock. The colour and smell were natural—it coagulated at 20 minutes after 3—and separated into serum and crassamentum at 35 minutes after 3—The serum was not in the least acrid to the taste.

April 26th. Blood was drawn at 33 minutes past 3 o'clock—The colour and smell were natural—It coagulated at 38 minutes past 3—and separated into serum and crassamentum at 53 minutes past 3—The coagulum and serum were natural—The serum was not in the least acrid to the taste.

2dly. *Of Putrid Diet.* The necessity of our taking aliment being fully established, Nature, with her usual wisdom, has given man and the other animals an appetite for it. She has also provided different kinds of aliment in the different climates, in a proper quantity, so that each might have what was most suitable and agreeable to his particular condition.

To this purpose the appetites of the inhabitants of the different regions vary; while some prefer a vegetable, others according to the situation, with more pleasure and benefit to their health, derive their chief support from the same class of beings to which they themselves belong, though of different genera and species.

Animals are styled herbivorous, carnivorous, and omnivorous, from the particular kind of aliment they make use of: Thus sheep live wholly on a vegetable diet, while the wolf and other animals of the same species are found to support themselves best by a diet which is entirely animal; and man, from the peculiar structure of his teeth, appears to be destined to take in all kinds of alimentary matter, and thus he is found to live best on a diet, composed of animal and vegetable substances. Experiments would seem to prove that an animal, who is naturally herbivorous, may be made to live entirely on flesh, although not so conveniently. A mixed diet, as already mentioned, best suits the appetite and peculiar state of man, but there are instances where he entirely lives on vegetables; and on the contrary, he has, in other situations, been found to live wholly on the inferior animals. While the mixed diet renders him placid and fit for every purpose, for which nature formed him, one entirely animal renders him ferocious as the brute, and that altogether vegetable renders him weak and feeble.

Among civilized nations, aliment, before it is eaten, always undergoes some kind of preparation, whereby it is rendered more or less fit to be subject of digestion.

Aliment, after being received into the stomach, remains there for some time, and undergoes a considerable change, before it quits that viscus; for it thereby becomes fitted for the forming of the chyle, from which the blood itself is immediately prepared.

The state of the chyle and consequently that of the blood, is by many supposed to be much influenced by the aliment

we take; thus a putrid and bad diet, is supposed to produce vitiated and putrescent chyle, and consequently in their opinion the blood is tainted in the same manner. This opinion probably took its rise from the flesh of certain animals tasting similar to the food they eat; thus sea-fowls, we know, have a fishy taste; pigeons who have fed on poke-berries for sometime have their flesh tinged with the colouring matter of that vegetable; and the pheasant who has lived upon laurel-berries is capable of communicating the deadly effects of that active poison to the human system. These, and perhaps other like circumstances, were the means of giving rise to the opinion, that the blood of animals must be affected differently by different articles of diet, and that corrupted or putrid food would be the means of producing a putrefaction of the blood. As long since as the days of Hippocrates, physicians have been of opinion that water and corrupted meat were the true causes of putrid diseases. In this manner was the plague, scurvy, &c. thought to have been produced; but the sentiment of many moderns is, "That scurvy arises from the want of a due quantity of alimentary matter in the food of those who are afflicted with the disease." This opinion is supported by the two cases of scurvy related in the Medical Transactions by Dr. Milman, and also by Dr. Stark's Dietetic Experiments*.

To convince myself whether, the state of the blood was affected by a putrid diet, the following experiments were performed.

EXPE-

* Vide Blane on Seamen's Diseases---Medical Transactions---and Stark's Works.

EXPERIMENT IV.

July 29th 1792. A dog was put upon a diet of putrid meat and putrid water, and continued to feed thereon till the 27th day of August following. The meat was beef, and never given to him till it was highly putrid: the drink was clear pump water, rendered putrid by suspending a piece of putrid beef in it, and exposing it to the action of the sun.

August the 12th his eyes were examined, and the pupils of both appeared much contracted. They were watry. At 20 minutes past 12 o'clock an ounce of blood was drawn from one of the crural veins. Its colour and smell were perfectly natural—it was coagulated at 25 minutes after 12—separation into serum and crassamentum was evident at 30 minutes after 12; the coagulum and serum were perfectly natural—the serum did not change the colour of a piece of paper, stained with a vegetable blue.

August 19th. he appears weak and very sick. For these few days past, he has had a disrelish for the putrid meat. His eyes appear sore and inflamed. I washed them with cool pump water. The action of the heart was regular though feeble. Blood was drawn at 4 minutes before 11 o'clock—the colour and smell were perfectly natural—it was coagulated at 11—separation into serum and crassamentum was evident, at 6 minutes after 11. The serum and coagulum were perfectly natural—The serum did not change the colour of the paper stained blue.

August

August 20th his eyes appear much better—washing them with cold water continued. He appears weak.

August 22d. his eyes appear quite well. Strength somewhat recovered. Appetite appears also to be encreased.

August 24th blood was drawn at 20 minutes after 5 o'clock. Its colour and smell were perfectly natural. It was coagulated at 24 minutes after 5—separation into serum and crassamentum was evident at 31 minutes after 5. The coagulum and serum were natural. The serum did not change the colour of the paper stained with the vegetable.

He in general ate and drank a sufficient quantity; but ate more in the beginning than towards the latter end of the experiment. He was reduced by the diet. The excretions were not evidently affected. The action of the heart was regular as has been mentioned, and respiration appeared to be performed in a natural and easy manner.

EXPERIMENT V.

A dog was kept fasting from September the 10th 1792, till the 14th of the same month.

During his fasting he did not appear uneasy till the evening of the 13th, when he cried. He continued in this state till the morning of the 14th, when at 15 minutes after 9 o'clock highly putrid broth was given him, he swallowed it greedily, belched several times afterwards, but did not vomit. The action of the heart which before was frequent and feeble, now became slower and fuller; he appeared

peared very lively and full of play. At 3 o'clock P. M. he was fed again with putrid broth.

September 15th. At 2 o'clock, he received some more of the putrid broth. The action of the heart in every respect natural, except that it was little feeble.

September 16th. At three o'clock he was fed again with putrid broth which he took in heartily.

September 17th. At 33 minutes after 9 o'clock blood was drawn from one of the crural vessels. Its colour and smell were natural. It was coagulated at 39 minutes after 9. It began to separate into serum and crassamentum at 50 minutes after 9. The serum and coagulum were perfectly natural. The serum had no effect in changing the colour of the paper stained blue.

EXPERIMENT VI.

A bitch was kept under the same circumstances as the dog last mentioned, and the phenomena were alike in both instances.

September 17th. At 12 minutes before 10 o'clock, blood was drawn. Its colour and smell were natural. It was coagulated at 6 minutes before 10. It began to separate into serum crassamentum at 3 minutes after 10. The serum and coagulum were perfectly natural. The serum did not change the colour of the paper stained blue.

From these experiments, it is evident, that the sensible qualities of the blood are not, in the least, affected by a putrid diet. The question then must certainly be, *How does*

does a putrid diet operate upon the system? I answer, by inducing debility from the little nourishment it contains; and that from hence the solids suffer. This I hope to prove by an experiment performed on the dog, the subject of the 19th experiment, which will be related under the head of injections of putrid matters into the blood-vessels.

The process of *digestion* is one of the most curious operations of nature. It acts wonderfully and powerfully upon our aliment, changes the properties of animal and vegetable matters, reduces them both to a substance possessing like properties, and operates upon the hardest as well as the most fluid substances — All this is said to be performed by a fluid we call the gastric juice.

If such are the effects of digestion on our aliment, is it not reasonable to suppose, that the properties of putrid matters taken into the stomach may be also changed? Let us examine whether putrid food undergoes a change of properties in the stomach.

That great Italian Philosopher, Spallanzani, was I believe, the first who made experiments to this purpose. He performed them on birds, cats, dogs, and even went so far as to swallow putrid meat himself. He found that the meat, in every instance, lost its putrid smell*.

I repeated his experiments on dogs, with the like success, and shall now relate them.

EXPERIMENT VII.

September 17th 1792. Three ounces of highly putrid beef were given to a bitch. She retained it. Three hours
and

* Spallanzani's Dissertations, Vol. I. p. 284 and seq.

and a half after, the contents of the stomach were examined. The meat was found surrounded by the gastric-fluid; its putrid smell was entirely destroyed, and its colour appeared more natural than it was before the animal had swallowed it.

EXPERIMENT VIII.

September the 17th. 1792. Highly putrid broth was given to a dog. The stomach retained it: and three hours after, its contents were examined, some of the broth was remaining together with some of the solid matters that were mixed with it. The putrid smell was found to be abundantly diminished.

I shall now proceed to the consideration of my 3d. head, which has for its object of enquiry, Whether the blood be materially changed and rendered putrid, by the injection of putrid and other substances into the blood-vessels?

Many centuries have elapsed since physicians first began to be afraid of the access of the least particle of contagious matter into the blood-vessels, for, that it excited a fermentation therein, contaminated the whole mass of blood, and caused it to partake of its contagious properties.

If (say they) by adding a small quantity of a ferment to a mass of flour, or other fermentable matter, we are able to bring on a fermentation in it, and convert it into the nature of the ferment, why should not the blood, in like manner, be affected, if by chance or otherwise a putrid

or other ferment should get into the vessels and mix with it? Thus they supposed the small-pox, measles, and other eruptive diseases to be produced.

These men always overlooked the vital principle, which exists in the one substance, and not in the other; and here in my opinion the material difference lies; for we know that if the other requisite circumstances be duly observed, it is only necessary to destroy life, in order to bring on a putrefaction in the animal. I think, from what I have stated, there is ground for believing that the blood cannot putrefy in the living animal; but the deductions of reason, however just and true, the inferences are not to be solely relied upon, when the better evidence of substantial facts—and of facts more immediately in point than the preceding ones, can be had.

To ascertain beyond a doubt whether the blood could be excited to a putrid fermentation, by injecting putrid matters into the blood-vessels, became an important and essential desideratum. With this view I made the following Experiments, which were performed *on healthy Dogs*.

The operation of injection was always executed on one or the other of the extremities of the animal. An incision was made, and the vein was laid bare, which was for some distance dissected free from connection with the adjacent parts. An opening was then made into its cavity, sufficiently large to admit a curved tube, which was retained in it by means of a ligature passed round the vessel. The tube was made so as to screw on the mouth of a common pewter syringe.

E X P E R I M E N T IX.

Was performed July 29th. 1792, on a Bitch, two months old in good health.

About 5 drachms of putrid serum were injected into one of her femoral veins. The serum was obtained from the blood of an healthy dog; and exposed in an open phial, for one week; and had a smell similar to that of rotten eggs.

Some difficulty attended the introduction of the pipe, by which the matter was injected, owing to its large size, and the smallness of the vein—The vein, when laid bare and touched with any instrument, contracted considerably, its diameter being thereby much diminished—During the operation, she lost but a very inconsiderable quantity of blood, yet she seemed very weak and languid, probably owing to the pain she endured—Whilst I was injecting the serum into the vein, she had two convulsive paroxysms, and appeared to be in great pain, which she expressed by several loud cries.—The muscles of her whole body seemed affected with these convulsive motions; that they were not owing to the irritation produced by introducing the pipe into the vein is certain, because no such symptoms appeared before the injection of the serum, though it was introduced twice or thrice.—After the injection was finished, she was placed on the floor, and attempted to walk, but was so weak that she fell down. The action of the heart was very frequent, though weak and feeble; respiration was anxious and difficult. She seemed not at all

inclined to move, and was quiet, except that at different intervals, she groaned and sighed, and afterwards vomited some of the food she ate at noon, in an indigested state, together with about two ounces of a yellowish green coloured fluid.—Continuing in this situation, she was frequently affected with convulsive motions of the abdominal muscles and lower jaw, she became weaker and weaker. At 15 minutes past 7 o'clock, about an hour and an half after the experiment was performed, all her muscles were in a relaxed state, and she appeared motionless. At half past 7 she was dead.

Dissection. I did not examine the body till the 30th. at half past 8 o'clock; so that she was 13 hours dead, when the examination took place.

The abdomen was much distended; upon cutting into its cavity, a quantity of putrid air, of a smell like that of rotten eggs, rushed out. There was an effusion of a lymphid fluid. The intestines were distended with air, and contained but a small quantity of liquid feces. The stomach contained little else, but a large quantity of the before-mentioned flatus, and a fluid of a yellowish colour. At its lower orifice it appeared rather preternaturally red. All the other viscera were in a sound and natural state. The veins appeared much distended; and air, intermixed with with blood, of a dark venous colour, appeared through their coats. When I cut into the vessels, air came out in bubbles, together with the blood. The blood did not appear to be very firmly coagulated. The heart was much distended with blood, particularly the right auricle, which contained some air also.—In the heart the coagulation was more perfect than it was in the veins. The blood did not smell in the least putrid.

EXPERIMENT X.

Was also performed July 29th. 1792, on a bitch in good health. About a drachm of pus, diluted with a small quantity of clear pump water, was injected into one of her femoral veins. The pus was obtained on the 27th. inst. from an abscess of the intercostal muscles. This day it possesses a putrid smell.

During the injection of the matter, she had two very violent fits of convulsion, the last of which continued for some time, and she appeared to be in great pain; respiration was quick and irregular; the heart beat frequently, though with some force. The pulsations were small and irregular, accompanied with frequent intermissions, and to all appearance she was dying. When she was placed in a cool situation, the respiration became less difficult, and the action of the heart more regular. She continued in this state and lay quiet for some time, when a mercurial thermometer was applied to the axilla, and the mercury rose to 100° , as it did when it was applied before she underwent the experiment. After lying about twenty minutes in a cool place, she got up and attempted to walk, but was very weak, and did not go far before she fell down; after which she seemed again to recover. At 30 minutes past 7 o'clock she was seized with twitchings of the muscles of her abdomen and lower jaw; the weakness increased; at length she became motionless; and at 15 minutes before 8 o'clock, about an hour and an half after the injection, she was dead.

Dissection.

Dissection.—The body was not examined till Monday morning the 30th. about 14 hours and an half after she died. The abdomen was considerably distended, I made an incision into it, but found that no air escaped—about 2 oz. of a lymphid fluid was found effused in the cavity. The intestines were much distended, and when an incision was made into them, air and fæces come out. The stomach was likewise much distended with air and half digested food. The same preternatural redness about the lower orifice, which occurred in the former case, appeared also in this. The other abdominal viscera appeared perfectly natural and sound. The heart was much distended with blood, but no air was found in it or the blood-vessels. The blood was more firmly coagulated than that mentioned in the last Experiment, and it had no unnatural smell.

EXPERIMENT XI.

August 6th. 1792. A drachm of fluid matter, produced by highly putrid beef, was diluted with four drachms of putrid pump water, and injected into one of the femoral veins of an healthy dog.

The matter was injected at half an hour after 11 o'clock. During the injection he was much convulsed, and appeared to be in great pain. The pupils of the eyes were somewhat dilated. The beating of the heart was now very frequent, though feeble, as it was during the injection of the matter. At 15 minutes before 12 o'clock the breathing became very frequent, accompanied with great difficulty and sighing. At this time he vomited

ed a quantity of half digested food, and appeared to be greatly relieved by it; at 12 o'clock he vomited again, which relieved him so that he rose, but in one or two minutes laid down again. At 4 minutes past 12 he had an evacuation of fæces—at 7 minutes after 12 he attempted to lay down, but fell, and laid in an unnatural posture—he appeared to be very weak—his breathing became more difficult and laborious—his eyes appeared watry and sunk in the orbits. At 10 minutes past 12 he had another evacuation of fæces. At 15 minutes after 12 he was raised up on his feet—he stood, though with difficulty, and his left hind leg became paralytic*. In a short time all his hind parts appeared to be more or less affected in this manner. At 40 minutes after 12 he became very restless, turning and twisting his body every way. At 45 minutes after 12 he was again raised upon his feet, but could not stand, for all his muscles appeared greatly relaxed. At 10 o'clock the pupils of his eyes were much dilated, and his sight seemed to be greatly diminished—he had an evacuation of urine. He began to groan, and the abdominal muscles were convulsed. The convulsions, after continuing some time in those parts, extended to the muscles of his head and neck. Respiration, and the action of the heart ceasing, he died at 12 minutes past 10 o'clock.

Dissection. The body was examined immediately after death, and nothing unusual was observed. The blood was quite natural in every respect.

E X P E-

* The incision in this Experiment was made on the *right* thigh.

E X P E R I M E N T XII.

August 10th. 1792. Twelve grains of putrid blood, diluted with a drachm of clear pump water, were injected into one of the femoral veins of an healthy bitch, at thirty-five minutes past 10 o'clock. During the injection she seemed uneasy, and had an evacuation of urine. The action of the heart became much slower, and very feeble. Respiration was somewhat difficult. She was then placed on the floor, and continued standing—12 minutes after she had a natural motion from the rectum, and then laid down. At 11 o'clock her eyes were examined, and they did not appear any way preternatural. She seemed dull, heavy, and much inclined to sleep. At 12 o'clock I perceived that she had had another evacuation of urine. The eyes were now examined again, and no alteration appeared to have taken place in them. She laid quiet till half past 1 o'clock, when violent efforts to vomit came on, and she brought up a quantity of the food she had eaten previously to the performing of the experiment. The heart beat very frequent and feeble—respiration was not greatly affected, but she continued dull and heavy. At 2 o'clock meat and drink were offered her—she would not even bear the smell of meat, but rose and drank some water. She laid down. At half past 2 o'clock she got up again, walked about, and evacuated urine; then laid down again, and was dull and heavy as before. At 5 minutes before 5 o'clock she had another evacuation of urine. At 3 minutes before 5 she had a very copious evacuation of extremely fluid fæces, of the colour of coffee-grounds, and of a very putrid smell. She now appeared weaker than she

she was before the evacuation, though not quite so dull. At 6 o'clock she evacuated urine again. I left her at 25 minutes after 6, when circumstances did not appear much altered. At 8 o'clock I saw her again, meat and drink were offered her—she drank, but would not eat. The heart beat very frequent and feeble—in other respects as before.

August 11th. This morning, at 8 o'clock, I found her dead. A disagreeable odour arose from her body. The abdomen appeared somewhat distended. I perceived she had had another loose evacuation of the description above mentioned, though not so copious as the former one. There was a great deal of saliva about the mouth, and the tongue protruded through the teeth. On

Dissection, the following appearances were observed. Upon cutting into the cavity of the abdomen, no air escaped, but I experienced a very disagreeable foetid smell. The superior parts of the intestines appeared in several places of a dark green colour, spotted with small white specks, while the lower portions appeared natural. When an incision was made into the intestines, a quantity of putrid air rushed out, together with liquid dark green coloured fæces. The stomach appeared to be rather small—I made an incision into it when some of the like liquor flowed out. The liver was in many places of a preternatural colour, and adhered to almost all the other abdominal viscera, particularly to the stomach and right kidney. The gall-bladder was much distended with a light green coloured bile; the lungs were collapsed; the right lobes were of a blackish colour, intermixed with red; the left lobes appeared natural. The red appearance

ance at the lower orifice of the stomach, mentioned in the other cases, was not apparent in this. The veins and heart were much distended with blood, which was not very firmly coagulated, but its smell was perfectly natural.

EXPERIMENT XIII.

August 14th. 1792. At 40 minutes after 10 o'clock six grains of putrid blood, mixed with a drachm and an half of clear pump water, were injected into one of the femoral veins of a bitch. During the injection she appeared very uneasy—the action of the heart was slow and feeble—respiration slow, and performed with difficulty. She was placed on the floor, appeared dull, and laid down. At 10 minutes before 11 o'clock her abdominal muscles were violently convulsed. At 7 minutes before 11 she was seized with violent retchings and efforts to vomit, but did not evacuate. At 15 minutes before 3 o'clock meat and drink were offered her—she ate, but would not drink. At 23 minutes before 7 she had a copious evacuation of urine. At 20 minutes before 7 she ate and drank. I left her at 15 minutes before 7, when she seemed easy. I saw her again at 8 o'clock and no alteration was evident.

August 15th. This morning, at 8 o'clock, I saw her—she appeared perfectly easy—action of the heart nearly natural, though rather frequent. She was now fed, and ate as before. I perceived that she had had a natural evacuation of fæces. At 21 minutes after 5 blood was drawn—its colour and smell were natural—it was coagulated at 24 minutes after 5—the separation into serum and

I

crassamentum

crassamentum was evident at 30 minutes after 5—a piece of the paper stained blue was dipped into the serum, and no change of colour took place—the serum and coagulum were quite natural. I saw her again at 8 o'clock. She appeared as she did when in health.

August 16th. She was perfectly well.

EXPERIMENT XIV.

August 17th. 1792. At 15 minutes after 12 o'clock twelve grains of putrid blood, mixed with a drachm and an half of clear pump water, were injected into one of the humeral veins of the bitch last mentioned. During the injection, she cried violently, and appeared to be in great pain. Respiration became very frequent—action of the heart frequent and feeble. The eyes were examined, and the pupils were found to be much contracted. She was placed on the floor, walked a few steps, leaned against the wall, in a standing posture, and appeared very sick. At 26 minutes after 12 she had an evacuation of fæces, which was rather loose. At half past 12 she laid down, appeared dull, and much inclined to sleep. At half past 2 the abdominal muscles were affected with convulsive contractions—they did not continue long. The eyes were again examined, and the pupils appeared natural. At 3 minutes before 5 she had a copious evacuation of urine. At 10 minutes after 6 I left her eating, and to appearance easy, though dull and languid. I saw her again at 8 o'clock, when I perceived she had had another loose evacuation of fæces, which was copious. Pulsation of the heart frequent and feeble—she appeared easy.

August 18th. Blood was drawn at 5 minutes before 6 o'clock P. M.—its colour and smell were natural—it was coagulated at 6—at 5 five minutes after 6 it separated into serum and crassamentum—coagulum and serum were quite natural—Serum did not change the paper stained blue. At 8 she appeared perfectly well.

August 19th. She was perfectly well. The blood, which was drawn yesterday, had a natural odour, when it was examined this morning at 11 o'clock.

EXPERIMENT XV.

August 20th. 1792. At 4 minutes before 11 o'clock, half a drachm of putrid blood and a drachm of clear pump-water, were injected into one of the humoral veins of the last mentioned bitch during the injection, she was very uneasy and gave several loud shrieks. The heart beat very frequent and feeble. Respiration became very difficult. She was placed on the floor, and immediately after had a copious evacuation from the stomach. She appeared very weak, and leaned against the wall. At 7 minutes after 11, she had retchings and violent efforts to vomit, but no evacuation ensued. At 32 minutes after 11, she began to groan and sigh. At five minutes after 12, she had efforts to vomit, but nothing was thrown up. I left her at 15 minutes before 1; she drank, and appeared easy, though she was very weak. I saw her again at 2 o'clock, the heart beat very frequent and feeble. I perceived she had had an evacuation of fæces, while I was absent—it was rather loose. Meat and drink were offered her; she drank, but did not eat. She appeared very sick.

I saw her again at 8'clock, when she appeared just as she was at 2 o'clock.

August 21st. Blood was drawn at 2 minutes before 5 o'clock P. M. its colour and smell were natural—it was coagulated at 3 minutes after 5—separation into serum and crassamentum was evident at 9 minutes after 5—the coagulum and serum were perfectly natural—the serum did not change the color of the paper stained blue. At 8 o'clock she appeared very well.

August 22d. Serum of the blood drawn yesterday, did not change the colour of the paper stained blue.

EXPERIMENT XVI.

August 22d. 1792. At 11 o'clock, a drachm of putrid blood mixed with half a drachm of clear pump water, was injected into one of the crural veins of the bitch last mentioned. During the injection, the action of the heart became very frequent and feeble. Respiration very laborious. She vomited. After this she was placed on the floor, and appeared very weak, yet stood for some minutes, and then fell down gently. At 8 minutes after 11, she had an evacuation of urine and of fæces. At 14 minutes after 11, the eyes were examined; the pupils were found to be very much contracted, and her sight much diminished. At 16 minutes after 11, she had another evacuation of urine. At 19 minutes after 11, she began to cry violently, and appeared to be in very great pain. The respiration became very laborious, and the action of the heart increased in frequency and feebleness. At 23 minutes after

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ter 11, she became convulsed. At 25 minutes after 11, a finger was drawn over the eye, and no contraction of the eye-lids took place. At 28 minutes after 11, she was dead.

Dissection. The examination of the body took place at 8 minutes after 12 o'clock. All the viscera were found in a sound and natural state, except the lungs---there a bloody effusion was discovered, particularly in the inferior parts of the left lobes. At 20 minutes after 12, blood was obtained by opening one of the large veins; the colour and smell were natural. When I left the blood, it was not so perfectly coagulated, as in the other instances, owing to its having been by accident much agitated. I returned in the afternoon, and found the coagulation complete---the coagulum and serum were natural---the serum did not change a piece of paper stained blue.

EXPERIMENT XVII.

August 14th. 1792. At 16 minutes after 3 o'clock, six grains of putrid blood, diluted with a drachm of clear pump water, were injected into one of the femoral veins of an healthy dog. During the injection, he was very uneasy and gave several loud shrieks. The action of the heart became so feeble, that it could scarcely be felt, and was also frequent. He was placed on the floor, when he lay down. In a short time he rose up, stood sometime, and appeared very weak. Respiration became laborious, and he lay down again. At 15 minutes before 4 o'clock, he was seized with twitchings about the bottom of, and across the thorax, in a great degree resembling an hiccup.

At

At 4 minutes after 4, meat and drink were offered him; he would not drink, and seemed as if desirous to eat; but when he approached near the meat, he drew his head from it, as if the smell of it were offensive to him, though the meat was fresh killed this morning. At 2 minutes before 5, he had an evacuation of urine, and rather a loose evacuation from the rectum. At 4 minutes after 5 he had a copious evacuation from the stomach, when the action of the heart became perceptible to the touch. At 20 minutes after 5, he had violent efforts to vomit, and brought up a quantity of greenish coloured fluid. At 34 minutes after 5, they recurred again, with the like effect. They attacked him a third time at 3 minutes before 6; the consequence was the same as in both the former instances. I left him at 15 minutes before 7 o'clock, when he was eating and drinking. He seemed easy.

I saw him again at 8 o'clock, when he appeared heavy. I perceived that since I had left him, he had had a sparing evacuation of faeces, rather loose.

August 15th. This morning at 8 o'clock, he appeared dull, heavy and weak. The action of the heart was frequent and feeble. He was now fed. At 2 o'clock, I saw him again; he was as described in the morning. At 5 o'clock, blood was drawn—the colour and smell were natural—it coagulated at 4 minutes after 5—separation into serum and crassamentum took place, at 1½ minutes after 5—the colour of the paper stained blue was not changed by the serum—The coagulium and serum were perfectly

perfectly natural---I saw him again at 8 o'clock, and he appeared perfectly well.

August 16th. He was perfectly well to-day.

EXPERIMENT XVIII.

August 17th. 1792. At 15 minutes after 4 o'clock, ten grains of putrid blood, mixed with a drachm of clear pump water, were injected into one of the femoral veins of the dog last mentioned. During the injection, he appeared to be in much pain, and had an evacuation of urine; the action of the heart became frequent and feeble, but respiration was not much altered. At half past 4, the abdominal muscles were convulsed; the convulsions did not last any length of time. At 20 minutes before 5, he was seized with tremors over his whole body; they lasted about two minutes, and then went off. At 16 minutes before 5, he had an evacuation of urine. At 15 minutes after 6, I left him eating, when he appeared pretty easy. I saw him again at 8 o'clock, and perceived he had had a motion since I left him. The action of the heart was frequent and feeble. While I was with him he had an evacuation of urine, and appeared easy.

August 18th. Blood was drawn at 10 minutes after 6 o'clock this morning—its colour and smell were natural—it was coagulated at 14 minutes after 6—separation into serum and crassamentum began to take place at 23 minutes after 6. The coagulum and serum were natural. The serum did not change the colour of the paper stained blue. At 8 he appeared to be perfectly well.

August

August 19th. He was perfectly well to-day. The blood, that was drawn yesterday, was examined this morning, and possessed no unnatural smell.

EXPERIMENT XIX.

August 27th. 1792. At 40 minutes before 10 o'clock, a drachm of putrid blood, mixed with half a drachm of clear pump-water was injected into one of the femoral veins of the dog, the subject of the 4th. Experiment. During the injection, he was very uneasy, and evacuated urine. The action of the heart became very frequent and feeble. He was placed on the floor, and immediately after vomited. At 3 minutes before 10, he had an evacuation of natural fæces. At 2 minutes before 10, he vomited again. At 10 his breathing became very laborious, and he had a loose and small evacuation of natural coloured fæces. At 3 minutes after 10, he lay down. The heart beat so feeble, that it could scarcely be felt. At 10 minutes after 10, he rose, vomited again, and then lay down. At 15 minutes after 10, the eyes were examined and no alteration in them was apparent. At 21 minutes after 10, he rose again, walked about the room, stood for sometime, and then again lay down. The action of the heart became more evident. At 11 o'clock, he groaned very much. At 6 minutes after 11 the action of the heart became more frequent and feeble. The eyes were now again examined, but presented no unnatural appearance. At 20 minutes after 11, he rose and walked a few steps, had a sparing evacuation of chocolate-coloured, liquid fæces, then fell down, and appeared as if stimulated to evacuate again; he rose and had an evacuation of urine, and again fell down. At 18 minutes before 12, his abdominal

dominal muscles became convulsed. At 4 minutes before 12, he vomited again. At 28 minutes after 12, he rose, walked a few steps, had an evacuation of very thin chocolate-coloured fæces, and appeared to be very weak; after this he walked a few steps again, and then lay down. I left him at 10 minutes before 1 o'clock, when he appeared easy, though very weak and sick. When I returned, at 12 minutes before 3, I perceived, that during my absence, he had had two or three evacuations of urine. The heart beat frequently and feebly. At 10 minutes before 3, he had an evacuation of urine, and afterwards vomited. At 8 minutes before three, he had an evacuation of very liquid fæces, intermixed with mucus. At 5 minutes before 3, he had retchings and efforts to vomit, but did not evacuate. At 16 minutes before three, he rose, and had another evacuation of fæces similar to the one last mentioned. At 10 minutes after 4, he had an evacuation from the rectum, of mucus intermixed with blood. At 25 minutes after 4, putrid meat and putrid water were offered him, he drank plentifully, but did not eat. I left him at half past 4, and saw him again at half past 7, when I perceived he had had a very copious evacuation of urine, but had not eaten. The heart beat frequently, though not so feebly as before. Respiration was pretty free. He seemed easy, and appeared much better than when I left him the last time.

August 28th. When I saw him this morning at 8 o'clock, marks of an evacuation of fæces and urine were evident. I perceived he had eaten nothing. Putrid water was given him and he drank of it. The heart beat frequently and feebly. He appeared very weak; and the
wound

wound put on a bad appearance. At 2 o'clock I saw him again, when I perceived that he had had two or three evacuations of urine, but that he had not eaten. I now offered him some fresh meat, he held it in his mouth, but did not swallow any of it, and let it drop. Putrid water was again offered to him and he drank it. Respiration did not appear to be much affected. The action of the heart was frequent, and so feeble as scarcely to be felt. In my presence he had an evacuation of urine; I caught some of it in an earthen vessel, dipped a piece of the blue-coloured paper in it, but no change of the colour was evident. The wound appeared in a gangrenous state. He seemed very weak, and it was with difficulty that he stood. At 8 minutes before 5, blood was drawn; the colour and smell were natural. It coagulated at 4 minutes before 5; separation into serum and crassamentum was evident, at 7 minutes after 5; the serum and crassamentum were perfectly natural; the serum did not change the colour of the paper stained blue. The wound appeared much worse. In every other case, yet mentioned, it invariably put on a good appearance, and healed readily. After bleeding he appeared exceedingly weak. At 35 minutes after 5, I left him very uneasy. I saw him again at 8 o'clock, and he appeared nearly in the same condition as when I left him last.

August 29th. This morning at 8 o'clock, I found him dead. The blood drawn yesterday, was now examined. No unnatural smell was evident. The serum did not change the colour of the paper stained blue. The coagulum was so firm, that when thrown out of the tumbler on the floor, it did not break. A very disagreeable and foetid odour arose from the body.

Dissection. The body was examined at half past 9 o'clock, when I cut through the skin covering the lower ribs, the flesh below appeared gangrenous. The liver in some places was of rather a lighter colour than natural. The inferior part of the small intestines appeared inflamed. The stomach was perfectly natural, both internally and externally; it contained mucus and a whitish fluid. A bloody effusion was found in the lungs, particularly in the right lobes. The bladder was quite natural. The heart and veins were much distended with blood, which was firmly coagulated in both; the colour and smell of it were perfectly natural.

As yeast is well known to be a powerful ferment, and the volatile alkali a great chemical agent, and a dissolver of the blood when out of the body, the following Experiments were made, to ascertain what effect they would have on the blood, when injected into the blood-vessels.

EXPERIMENT XX.

August 14th. 1792. At 20 minutes after 11 o'clock three drachms of stock-yeast were injected into one of the femoral veins of a dog. During the injection he appeared somewhat uneasy. Respiration became very difficult and laborious; the action of the heart irregular, intermitting, and somewhat encreased in force. He was placed on the floor but was not able to stand. At 30 minutes after 11, he had an evacuation of urine; at 33 minutes after 11, he had an evacuation of natural fæces. The eyes being examined, did not appear evidently altered. The muscles of his whole body now seemed to be in a relaxed

laxed state. At 40 minutes after 11, his eye-sight appeared to be much diminished; at 5 minutes before 12, he was dead.

Dissection. The body was examined immediately after death. Nothing preternatural was observed, except a bloody effusion in the lungs. The blood was in every respect natural.

EXPERIMENT XXI.

August 7th. 1792. At 20 minutes before 11 o'clock, 15 grains of mild volatile alcali dissolved in two drachms of clear pump water, were injected into one of the femoral veins of an healthy bitch. During the injection, she gave three or four loud cries, and seemed to be in great pain. She was placed on the floor, walked three or four steps, and then lay down. The action of the heart was increased in frequency, and respiration was performed with difficulty. In other respects she seemed easy and quiet. At 11 o'clock, meat and drink were offered to her; she would not take of either. Respiration now seemed to be performed with no great difficulty. The action of the heart was as before mentioned. At 35 minutes past 4, the eyes were examined, and they appeared no ways altered. At 42 minutes past 11, she began to be restless. The action of the heart seemed to have recovered some degree of quickness; the frequency of it still continued. Respiration appeared pretty easy, though short. At 40 minutes past 12, her whole body was seized with a tremulous motion, when a thermometer was applied to the axilla, and the temperature was as when in health. These tremors came on in paroxysms, each of which lasted but

a little time, and they recurred frequently. The heart, during a paroxysm of trembling, beat frequently and feebly. The tremors encreased in violence, and in frequency of recurrence. At 14 minutes past 1, she was affected with several violent convulsive contractions across her abdomen. At half past 1, the eyes were again examined, and the pupils were observed alternately to dilate and contract frequently and considerably. At 40 minutes after 1, she rose, looked about; meat and drink were again offered her, but she refused both, and lay down again. At 10 minutes before 2, the tremors attacked her again, her breathing became more difficult and laborious, the tremors continued more or less violent for 5 minutes, then went off, and she appeared easy. At 15 minutes after 2, the tremors and laborious breathing came on again; they were of short duration; when they went off she appeared easy, and continued so till 5 minutes after 4, when they occurred again. At 10 minutes after 4, she gave three or four loud cries, as if affected with much pain, then was easy again, and remained so till 40 minutes after 5, when I left her. At 8 o'clock I saw her again; she seemed perfectly easy, the action of the heart was frequent and feeble.

August 8th. At 8 o'clock this morning she appeared easy, but was very weak; and her heart beat frequently and feebly. She now ate and drank; at 2 o'clock she appeared as in the morning: I also perceived that she had had a natural evacuation from the rectum since I saw her in the morning, which was the first that occurred since the operation was performed. At 7 o'clock I saw her again; no alteration was evident. I found that she had had a natural evacuation of fæces this afternoon; but I did not perceive any marks of a discharge of urine.

August

August 9th. She seemed quite well, but drank more than dogs usually do in health.

EXPERIMENT XXII.

August 10th. 1792. At 40 minutes after 11 o'clock, 25 grains of mild volatile alkali, dissolved in two drachms of clear pump-water were injected into the bitch last mentioned. During the injection, she was violently convulsed, and gave several loud cries; she also had an evacuation of urine and vomited. The heart beat very frequently, and breathing was laborious. She was placed on the floor, ran about the room, and then lay down. Her aspect was very wild. She did not lay long before she got up again; but in a few minutes lay down. At half past one she rose, and immediately afterwards lay down again. The breathing was very frequent. At 2, meat and drink were offered her, but she did not take of either. I left her at 25 minutes after 6, she continuing to be in the same condition. At 8 o'clock I saw her again; meat and drink were then offered her, she drank, but would not eat. Her heart beat very frequently, though weak; she seemed in other respects as she was before.

August 11th. This morning at 8 o'clock she appeared to be easy, was quiet, and ate and drank. Her heart beat frequently and feebly. I saw her again at 8 in the evening; she was in the same condition as in the morning. At 26 minutes after 3 blood was drawn—its colour and smell were quite natural—it coagulated at 32 minutes after 8 o'clock—separation into serum and crassamentum was evident at 38 minutes

minutes after 8 o'clock—the coagulum and serum were every way natural—the serum did not change the colour of the paper stained blue.

August 12th. To-day she appeared perfectly well.

EXPERIMENT XXIII.

August 13th. 1792. At 15 minutes before 11 o'clock 45 grains of mild volatile alkali dissolved in two drachms and an half of clear pump water, were injected into the same bitch. During the injection she appeared to be in great pain, and gave several loud cries. She was placed on the floor, ran about the room, then lay down, and was very restless. The action of the heart was extremely frequent, and her aspect was wild. After lying a few minutes she became easy. At 35 minutes after 2 meat and drink were offered her; she ate, but would not drink. At 30 minutes after 4 o'clock blood was drawn—its colour and smell were natural—it coagulated at 35 minutes after 4—separation into serum and crassamentum was evident at 39 minutes after 4—the crassamentum and serum were perfectly natural—the serum did not change the colour of the paper stained blue. At 39 minutes after 4 more blood was drawn from the same vein—its colour and smell were natural: As soon as it was drawn, I added 45 grains of mild volatile alkali dissolved in ʒij of clear pump water to it, when the colour became a very deep brown, nearly black. At 8 minutes before 5 o'clock it appeared to have a tendency to coagulate. At 10 minutes before 6 it was of the consistence of mucus. At 10 minutes after 6 she seemed perfectly easy, and was eating meat. At 8 o'clock she appeared in the same easy situation.

August

August 14th. At 8 o'clock this morning she appeared perfectly easy. The blood last mentioned was examined, and its consistence was much as before.

From the above Experiments the following inferences result :

1st. That, contrary to the generally received opinion, the blood is neither rendered alkaline, acrid or putrid by starvation. This I think is evinced by these circumstances—that the blood went through its spontaneous changes in the regular and usual manner—that the serum had no effect in changing the colour of my test—and that no sense of acrimony was perceptible to the taste.

2dly. That a Putrid Diet does not operate upon the blood so as to change its sensible properties. This is a material part of the enquiry, since physicians, from Hippocrates down to the present day, have supposed various and wonderful changes to be produced in the animal economy by such food. From the above Experiments, it appears very clear, that a putrid diet had no effect whatever in changing the qualities of the blood, although the animals were strictly confined to it. We are also led to conclude from them, that a putrid diet does not change the sensible qualities of the excretions; since, as I have related, the urine did not alter the colour of the vegetable blue. Here it may be objected to me, that I ought merely to infer from these Experiments, that the blood of dogs only is not affected by this treatment—I answer, that the inference may, probably, be extended to the whole animal creation; for, we cannot but suppose that nature is, in this respect, as beneficent to the one species as she is to the
 2. other;

other; and to substantiate this opinion, I avail myself of the result of the Experiments of the celebrated Spallanzani as above mentioned, who not only operated upon the brute creation, but went so far as to risk his own life for the benefit of science; and found, contrary to the received opinion, that vitiated food, previous to its leaving the stomach, is converted into a matter capable of furnishing good blood. "Men, such as the inhabitants about the mouth of the Orange river, in Africa, live always on animal food, such as whales, seals, limpets, and what fish they can catch; that many times their food has entered into a great degree of putrefaction, and there is no vegetable food whatever employed at the same time; probably most of them never tasted any vegetable substance in their lives, excepting aromatics for seasoning; yet they are perfectly healthy and free from all putrefaction in their fluids or solids, though they are not very careful of avoiding it in the exterior parts of the body. We see likewise maggots live in and upon putrid masses, while they themselves, and all their fluids, are perfectly sweet and free from all appearance of putrefaction*." There appears to be an intimate connection between the fluid and solid parts of our bodies, but how far this connection between them subsists, I will not undertake to assert. Perhaps I might, in truth, have said that a putrid diet acts upon the solids of our machine, by its not containing alimentary matter in a proper quantity, and therefore not affording a sufficiency of blood to support the animal. We know that the solids derive their nourishment from the blood; and, in my opinion, it is sufficiently evidenced, from the mortification of the muscular parts which ensued upon making an incision into the thigh, as

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* Fordyce on Digestion, p. 155.

has been already stated—That a putrid diet does operate upon the solids.

3dly. That although the blood, or other animal, matter may be excited into a putrid fermentation out of the body, by the addition of a putrid ferment, yet that such a process cannot be excited in the living body. We have found that many grains of putrid matter existed in the blood-vessels for some days without changing the blood. Is it probable that a fermentation can be induced in the blood when the ferment enters the system by absorption, and no such process take place by introducing a putrid ferment immediately into the blood-vessels?

4thly. That by the introduction of putrid matter into the blood-vessels, very violent symptoms were produced, although the quantity of the matter was small, and of the same nature as the blood, except that it had become putrid*. Is it probable, then, that the animal could exist with the whole mass of blood in a state of putrefaction, when such violent symptoms were the consequence of the introduction of so small a quantity of putrid matter as was injected in the foregoing Experiments?

And, 5thly. That though the volatile alkali may operate upon the blood in a wonderful manner out of the body, it does not evidently affect the blood when injected into the blood-vessels.

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* My reason for making use of putrid serum and putrid blood, is, that they are the same substance, only altered by putrefaction. We know, from Experiments of Transfusion, that blood may be passed from the vessels of one animal, into those of another, without any evident injury. From this circumstance we are led to infer, that it, in these instances, acted as putrid matter.

From these experiments it is also evident, that cathartic and emetic medicines, when injected into the blood-vessels, cannot operate in a specific manner; for in almost every instance evacuations were the consequence of the experiments when neither cathartics nor emetics were injected. Probably any matter capable of producing a sufficient irritation, will produce those effects, when thus thrown into the animal.

I attribute the presence of air in the vessels and heart, which occurred in one of the experiments, to the imperfect state of the syringe; for it consisted of one whole piece, when the two first experiments were performed.

Before I leave this part of the enquiry, I beg leave to state the following questions, viz.

Is there a power in the blood-vessels, or in the blood, capable of assimilating to the blood matters which are injected into the vessels? Do not the following circumstances make it probable? I could not by a minute examination distinguish between the blood and the matter injected. The serum of the blood, where the volatile alkali was injected did not possess the odour of that salt, neither did it change the colour of the paper stained with a vegetable blue. Does not the doctrine of secretion, which is now gaining ground, favour such an idea? I own that there are certain exceptions to this opinion.

Do not the loss of vision, the dilated and contracted pupil, together with the convulsions which happened, denote the nervous system to be affected by the contents of the blood-vessels?

I shall

I shall now proceed to enquire, whether there is any reason to believe that the blood becomes putrid in any disease ?

This is a question of considerable importance. Extensive experience is necessary to decide it ; and as my own has been very limited with regard to diseases supposed to be putrid, I must beg leave to refer the unprejudiced reader to books, which treat at large of them. Perhaps upon thorough consideration, he will believe the matter to be at least doubtful.

All I can do, will be to state some objections to the inferences that have been drawn from the symptoms and circumstances attending these diseases.

A symptom on which great stress has been laid, is, the appearance of Petechiæ, Vibices, or effusions of blood, which takes place in the last stage of Typhus, Yellow Fever, Scurvy &c. These appearances are generally supposed to be indicative of a dissolved state of the blood, in those diseases, though in my opinion without a sufficient reason. It is conceded by all that the system in those diseases is very much debilitated, and of consequence the system of blood-vessels, and these particularly at their ultimate terminations. The effect of this debility is a relaxation of the solids ; the fibres of the blood-vessels will not now be in as close contact as they were in health, and the mouths of the exhalents will not be as narrowly contracted. This particular state of the vessels at their terminations, will allow the blood to transfuse, and be effused in the cellular membrane ; or the exhalents, which in health pour out a lymphid fluid, may now be capable of forwarding the
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red blood itself. Analogy supports the opinion. Do not the vessels of the eye in ophthalmia, through which a colourless lymph is circulated in health, receive in this diseased state, red globules? Do we not find in dropsy, that the vessels allow of a greater exhalation than when in health? If in proportion as we become debilitated, we approach nearer to a state of death, Does not the transfusion of bile through the coats of the gall-bladder, in dead subjects, give additional strength to the idea? Again, is it not confirmed by the circumstance, that these appearances take place in the last stage of those diseases when the debility is greatest?

If Petechiæ were true symptoms of a dissolved and putrid state of the blood, we should certainly find that the bodies of those who die, marked with such symptoms, would certainly putrefy much sooner than the bodies of others who die without any such marks. The contrary of this is said to happen: And anatomists assert “that the bodies of those who die, of what are called putrid fevers, are longer before they undergo real putrefaction than of those who die of other diseases, or who die in perfect health by violence.” *

The dissections of the celebrated Lind and Rouppe, seem to prove that the blood in those diseases is not putrid. The former found it in the yellow-fever to be grumous and viscid, and covered with a yellow gluten, impenetrable to the finger unless cut by the nail †

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* Moore's Medical Sketches.

† Lind's first paper on Fevers and Infection, p. 13. & seq.

The same gentleman relates; "that in scurvy, red coagulated blood is found extravasated in almost all parts of the body, not only into the tela cellulosa, but into the bellies of the muscles, particularly of the legs and thighs, which often become quite stuffed, and even distorted, with large grumous masses*."

After stating that, "Some authors from a supposition of the great dissolution of the blood in petechial fevers, and from another supposition, that blisters encreased the dissolution of that fluid still more, have forbid their application in such fevers." He adds, "But the experience of the most eminent practitioners does not support those theoretical opinions †."

Roupe, a physician who had great experience in the scurvy, found that the blood was always more or less coagulated in those instances, in which he had an opportunity of viewing it. He observes, "If we consider, we shall find that the blood of those who have a continual fever, or an inflammation, after they have lost a good deal by bleeding, is so dissolved, that no one can find blood of a thinner consistency even in the last stage of the scurvy. Who (he justly interrogates) has even pretended to say these diseases were owing to a dissolved or putrid state of the blood ‡?"

We may further observe, that a dissolved state of the blood is by no means a proof of its having become putrid, it merely indicates a lessened disposition to coagulate; and
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* Lind's 2d paper, p. 100.

† Lind's 2d paper, p. 87.

‡ Roupe on the Diseases incidental to Seamen, p. 200.

do we not find the blood in several other diseases besides the scurvy &c. to be in a more fluid state than it is in health? Certainly we do.

We are informed by Dr. Lind, that the blood of scorbutic persons did not impart the least sense of acrimony to the tongue, any more than the white of an egg; and that the blood of scorbutics does not become putrid sooner than other blood, which it certainly ought, *cæteris paribus*, if it had already begun to putrefy in the body. Nay we are moreover informed by this celebrated writer, that the serum of the blood of such patients, is not septic but antiseptic; and would it not be absurd and inconsistent with the facts of chemistry to say, that a portion of matter which had already begun to putrefy on being added to an unputrefied mass should retard the process?

The benefit derived from the use of fresh vegetables and their acids in curing scurvy, is, in my opinion, by no means to be admitted as proof of the blood's being in a putrid state.

We must in this place notice, that it is a well established opinion, that the powers of digestion are such, as to be capable of converting all matters into one and the same kind of chyle, whether animal or vegetable, though the one may not afford it in as great proportion as the other.

The above circumstance being admitted, and it cannot be denied, we at the same time must allow, that those matters undergo a considerable change in the stomach before they go on to the formation of the blood. Consequently, if they suffer a change, they possess no longer the properties of a vegetable, or a vegetable acid, and therefore a *tertium quid*

quid must be formed. What its nature and properties are I will not pretend to say, but leave the candid to judge for themselves; though we might equally well suppose it to be septic as antiseptic*.

There are many physicians, who supposed fixed air to be chiefly useful when injected into the intestines in those diseases suspected to be putrid, by acting as an antiseptic on the putrid blood; but when, thus applied, does it not rather act on the contents of the intestines, and destroy the bad effects produced by their offensive smell, &c. since it has been well observed, "that, any thing putrid is totally incompatible with the perfect well being of the animal?"

It is generally allowed, that putrid effluvia act as debilitating powers on the system. Indeed putrid matters, lying for a time in the intestines, would seem to be debilitating, as in the instance of the dysentery. It appears also that fixed air acts as a stimulant: Mr. Henry found that it inflamed an ulcer †. And Dr. Dobson says, that when received by the mouth into the stomach, in ten minutes, it raised the pulse from 71 to 77 strokes ‡.

It was objected to me, that the urine, breath, and other excretions of persons labouring under diseases of the putrid

* I think that the following circumstance justifies my assertion, that vegetable aliment after it has suffered the changes produced on food in the stomach, &c. when it is sent on to form the chyle, has more of a septic than antiseptic quality. Fordyce on Digestion, p. 164, mentions, that by distillation in a retort we obtain an empyreumatic oil, volatile alkali and water, and charcoal remains in the retort, whether the substance distilled be chyle, a piece of flesh, or other animal substance.

† Henry's Exper. p. 127.

‡ Dobson's Commentary on Fixed Air.

trid clafs were highly fœtid and obnoxious. We grant this may be the cafe ; though a question will then arife, whether thefe excretions contract this fœtor from the blood, or become fœtid after they are feperated from the general mafs ? I am of the latter opinion, and think it highly probable that they become fœtid in their refpective refervoirs, or in fome other manner not yet explained. We know that the excretions do not by any means poffefs the properties belonging to the blood, and we alfo know that they may be very different in difeafe, though the blood remain the fame as when in perfect health. To this purpofe, I will quote Dr. Home's obfervation—he mentions that the blood of diabetic perfons appeared perfectly natural ; that the ferum of the fame poffeffes no more fweetnefs than that of other blood ; though the urine of the fame patients tafte very fweet, and upon evaporation afforded a large quantity of faccharine matter.*

The cafe of a patient, who was lately under the care of my worthy preceptor Dr. Wiftar, may alfo be mentioned in confirmation of thefe fentiments. He was affected with pneumonia, and had all the ufual fymptoms ; but in addition to them, an odour proceeded from him fo putrid and offenfive that no one could remain long in his room without great inconvenience, it even extended its offenfive fmell into the rooms a ftory below him. His urine had alfo an intolerable fœtor. The fymptoms of pneumonia required bleeding, and this remedy was ufed with great caution ; but notwithstanding the above circumftances, the blood coagulated very firmly, and had fome inflammatory appearances on its furface.

* Homes's Clinical Experiments, p. 332.

The arguments taken from Dedier's, Couzier's, and Homes's Experiments, employed by Dr. Ferris to prove, that the bile and blood undergo a change in the plague and measles, are by no means conclusive. We know the plague and measles to be highly contagious diseases, and easily communicated to those who are in a state of predisposition to receive the infection. Dr. Ferris supposes, that as the blood or bile of one who died of the plague, when injected into the veins of a dog, produced symptoms of that disease; and as the measles ensue after inoculating with the blood of those who labour under it, the blood and bile must have undergone changes in those diseases. In my opinion these facts prove nothing that favours such a conclusion; and all that we can infer from them is, that the contagious or infectious matter was diffused through the blood, or adhered to it in those instances, as it does to old buildings, cloathing, &c. Facts prove that it does so with respect to these subjects, for months, nay even years, and then is as effectual as ever in its deadly operation. We are uninformed of the appearance or state of the blood in those instances. In the measles, the blood appears to be no more changed or altered than in other inflammatory diseases. How it is in the plague, I know not. Dedier and Couzier have taken no notice of any evident change—Gentlemen whose accuracy would not have allowed them to have neglected such a circumstance if it had occurred.

What confirms me in the opinion, that the contagious matter was only diffused through the blood, or adhered to it in the same manner as to old buildings, &c. is, that in the one instance the plague, in the other the measles,

was produced. If the contagious matter had mixed with the blood so as to produce a chemical change therein, neither the plague nor the measles would have been the result of the Experiments; as by this mixture, they would both have lost their former properties, and a new compound would have been produced by their union, not possessing the properties of the contagious matter or of the blood. The sixth law of the affinity of composition, reads thus: "Two or more bodies, united by the affinity of composition, form a substance, whose properties are very different from those of any one of the bodies before their combination."* Consequently a disease very different from the plague or measles must be produced, if a chemical union had been formed.†.

With respect to Dr. Home's Experiment, I beg leave to make one or two observations: He says, "I thought that I should get the blood more fully saturated with what I wanted, if it was taken from the cutaneous veins amongst the measles, than if I took it from a large vein, where there was a much greater proportion of blood from the more internal parts than from the skin. I therefore ordered a very superficial incision to be made amongst the
thickest

* Fourcroy's Chemistry, Vol. I. p. 64.

† We have already proved that the blood in the vessels of living animals was not subject to the laws of fermentation, and that no fermentation could be excited in it by the introduction of ferments; therefore if a change had been produced in the blood in the above instances, it must have been by a chemical union, and consequently must be subject to the laws of chemical affinity.

thickest of the measles, and the blood, which came slowly away, was received upon some cotton.”*

The Doctor appears to be of the opinion that a fermentation of the blood, produced by the introduction of contagious matter into the system, was not the cause of the disease; for, he says, the blood taken from the more internal parts was not as plentifully saturated with the morbillous matter as that flowing in the cutaneous vessels. Is not this inconsistent with the true and well understood course of the blood? For, is not the blood of the cutaneous vessels at one moment in one part, and at another in a very different part, of the system? Consequently the whole mass must have been equally affected. Again, if the motion of the blood were not itself sufficient to produce the change throughout the whole mass, this would have happened from the well known laws of fermentation; for we know that a very small portion of a ferment is sufficient to assimilate a very large mass of fermentable matter. Further, this experiment is not conclusive, since the blood was obtained by a very superficial incision made amongst the thickest of the measles, and the blood also flowed slowly. Here certainly in making the incision, the lancet or instrument used must have pierced some of the pustules from the situation in which it was made; and as the blood flowed slowly it had time to entangle or mix with a quantity of the matter contained in the pustules; so that this experiment is a very indecisive one. Moreover I have been told by a gentleman who sometime since attended the Lectures of the celebrated John Hunter, that

* Home's Medical Facts, &c. p. 268.

that Mr. Hunter informed his pupils, that he had made frequent attempts to inoculate with the blood of those who had the small-pox, and lues venerea, but never succeeded in imparting the infection.

Lind's observations on the blood in the yellow fever support the opinion, that substances different from the blood may be diffused through it without changing it. He relates that the serum was of a deep yellow tinge. A person by curiosity tasted it, and found it bitter.*

From these facts we learn, that the bile may exist in the blood-vessels, without producing a change in the blood, or suffering one itself, since its sensible properties were in these cases evident. Who will assert that the properties of the blood are changed in jaundice?

We might proceed to a much greater length in proving that certain substances may exist in the blood-vessels of living animals unchanged themselves, and without producing any change in the blood, as turpentine, &c. This, I believe, is a position at present generally allowed by physicians—I shall therefore pass it over in silence.

Eruptive diseases were, for ages, supposed to be owing to changes in the blood; but the following Experiment would seem to operate against the doctrine. D. Coxe, transfused between 14 and 16 ounces of blood from the jugular vein of an old dog, who laboured under an eruptive disease, into the jugular vein of another dog, who was in health. This being done, the diseased dog got well; and

* Lind's 1st. Paper, p. 13.

and to the other, who received the blood which might probably have been supposed to be affected, nothing amiss happened.*

This Experiment proves clearly that the blood did not suffer a change from the disease under which the dog at that time laboured. Eruptive diseases are, I believe, for the greatest part contagious; and as the blood did not, in this instance, communicate the disease, we cannot suppose that the contagious matter adhered to it; this, therefore, strengthens my opinion, and leaves room for the supposition, that contagion may adhere to different parts of the body in different subjects.

If contagious diseases are produced by the contagion operating upon the blood as a ferment, whence is it that the blood of brute animals is not susceptible of being excited into this fermentation, since, from Experiments, it appears to be much of the same nature with that of the human species?

Five months after the above sheets were written, I had the good fortune to obtain a reading of Dr. Milman's very ingenious "Enquiry into the source from whence the symptoms of the scurvy, and of putrid fevers, arise, &c." wherein the author displays a great deal of ingenious reasoning. I was happy to find that we agreed in sentiment respecting the general opinion, though we have treated of the subject in a very different manner. For particulars I refer the reader to the book itself.

Thus I have sought in nature the phænomena of my doctrine. In my own estimation, facts respond to theory,

† Etmulleri Opera, Tom. 3tio, p. 1619.

ory, and the inferences of my Experiments to speculation. When I first contemplated this subject, I was not particularly attached to any opinion respecting it; and, from this circumstance, I gained the advantage of an impartial and unprejudiced examination of facts. After collating and considering the Experiments I had made, I drew my conclusions with a deference to reason. If, in the event, I shall have contributed to advance the interests of Medicine, and furnished any principles to aid the labors of practice—to lessen the horrors of putrid diseases—and to arrest, for a moment, the dreadful arm of death, I shall be more than compensated for performing my duty, and paying this tribute to humanity.

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