

**Observations on the dropsy in the brain / by Robert Whytt ... ; to which are added his other treatises never hitherto published by themselves.**

**Contributors**

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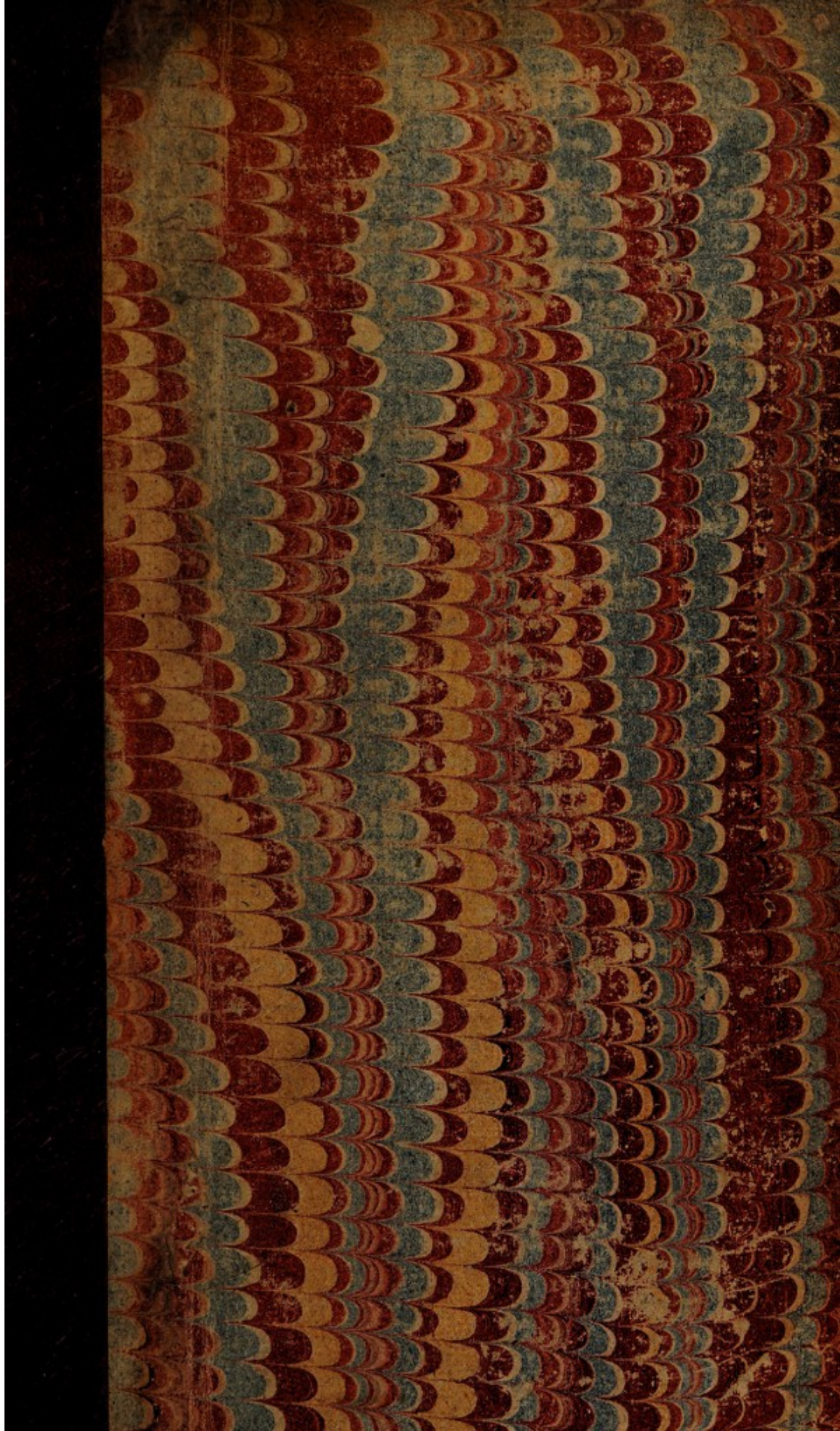
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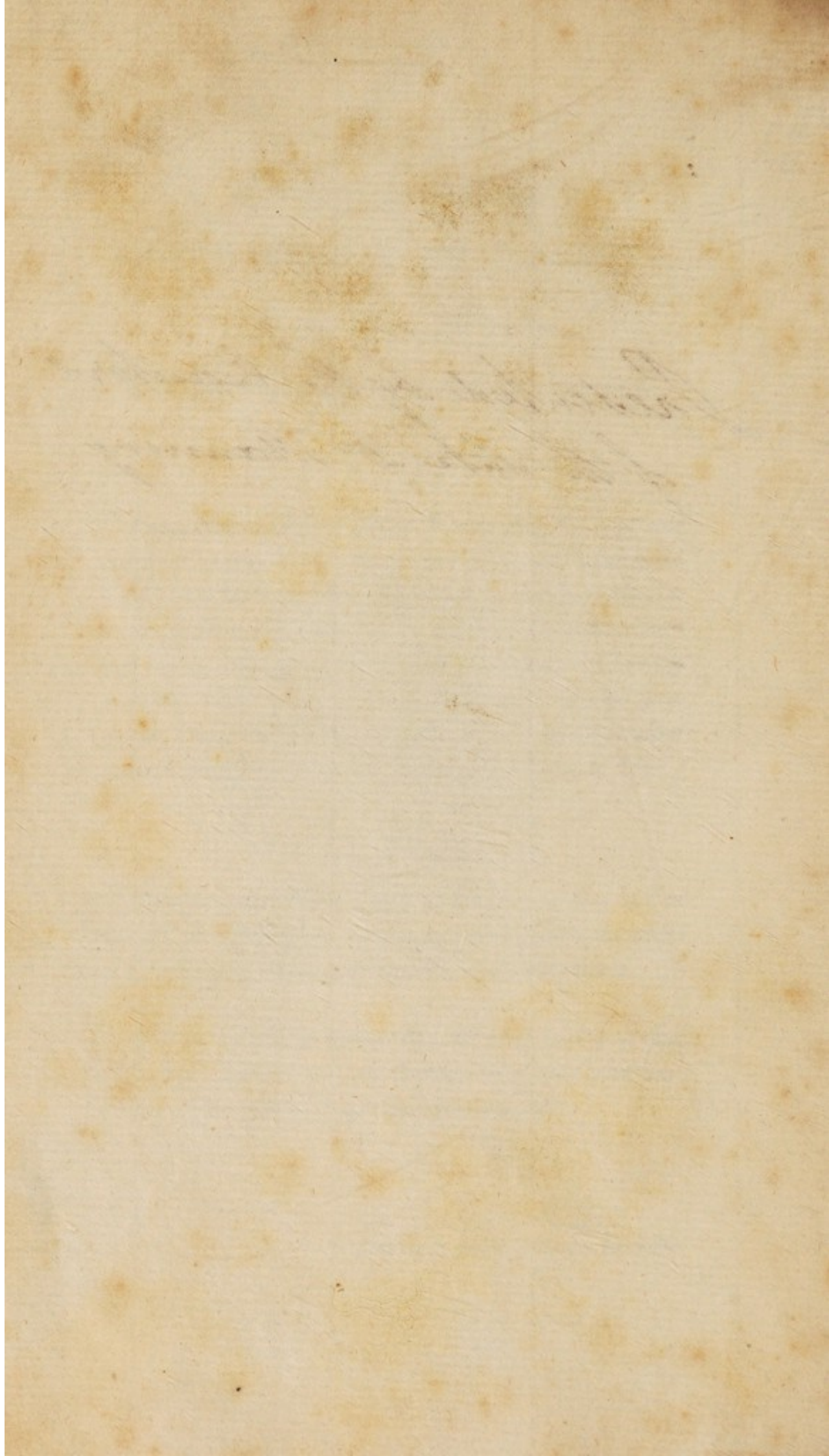
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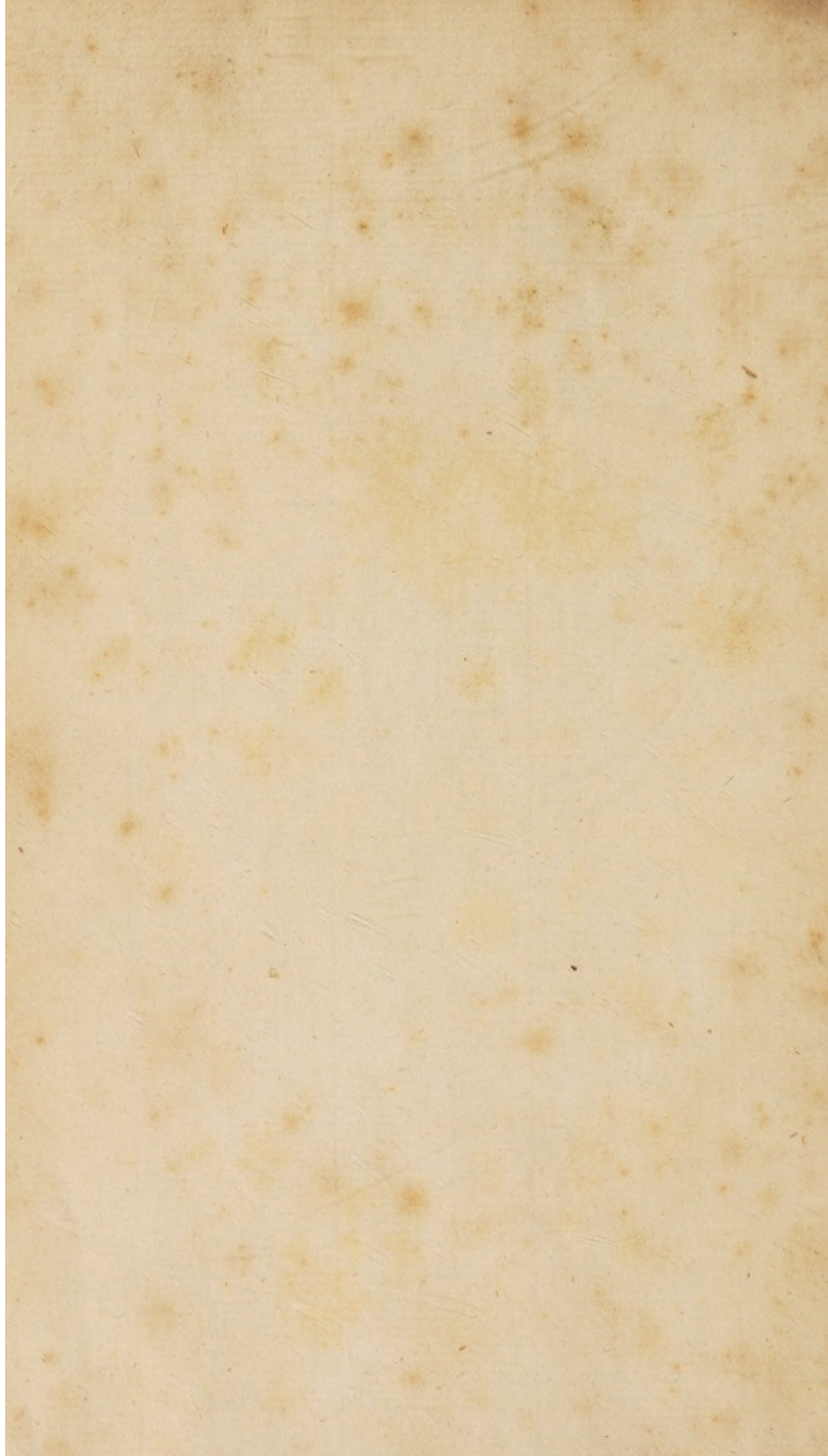




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OBSERVATIONS  
ON THE  
DROPSY in the BRAIN,

BY

ROBERT WHYTT, M. D.

Late PHYSICIAN to his MAJESTY,

President of the Royal College of Physicians, Professor of  
Medicine in the University of Edinburgh, and F. R. S.

TO WHICH ARE ADDED

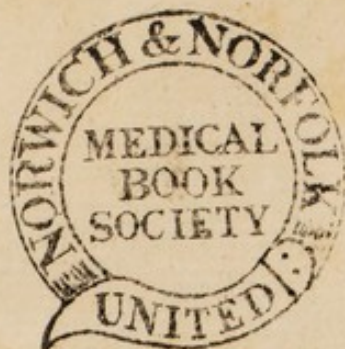
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# O B S E R V A T I O N S

ON THE

Most frequent Species of HYDROCEPHALUS INTERNUS,

V I Z.

The DROPSY of the VENTRICLES  
of the BRAIN.

THE *hydrocephalus*, or dropfy of the head, is either external or internal. The former has its feat in the cellular substance, between the skin and the *pericranium*, or between this membrane and the skull. In the internal *hydrocephalus*, the water is sometimes collected between the *cranium* and *dura mater*, or between this last and the *pia mater*; but most commonly is found in the ventricles of the brain, immediately below the *corpus callosum*: And this is not only the most frequent



#### 4 OBSERVATIONS ON THE

and fatal species of the *hydrocephalus*, but also that with which medical writers seem to have been least acquainted.

HIPPOCRATES, in his second book *de morbis*, has enumerated the signs of water in the brain, as his words have been rendered by all the translators. But *ἐπὶ τῷ ἐγκεφαλῷ* more properly signifies *upon* than *in* or *within* within the brain; and that Hippocrates only speaks here of water lodged between the *dura mater* and brain can scarcely be doubted, since he proposes to evacuate it, by making a perforation in the upper part of the *cranium*, *πρὸς τὸν ἐγκεφαλόν*; which operation could have been of no use, had the water been contained within the brain itself.

CELSUS has only mentioned briefly the *hydrocephalus externus*, or dropsy of the teguments of the head\*. Aetius and Paulus Ægineta go a little farther; for when they treat of this disease, they observe that  
water

\* De medicina, lib 4. cap. 2.



## DROPSY IN THE BRAIN. 5

water is sometimes found between the skull and the membranes of the brain.

HIERONYMUS MERCURIALIS, who flourished in the beginning of the sixteenth century, mentions the collection of water in the ventricles of the brain as a thing that may possibly happen; but adds, that in such a case an apoplexy must be the consequence\*.

WEPFER has collected several cases from different authors, in which water was found in the cavities of the brain †; and the celebrated Boerhaave mentions such a disorder as one species of the *hydrocephalus* ‡. But none of these authors, nor indeed any other that I have met with, who wrote before them, have favoured us with the signs by which we may distinguish a dropsy of the ventricles of the brain from other diseases affecting that organ.

M. PETIT,

\* Opuscula aurea, lib. de morb. puerorum.

† Hist. apoplecticorum.

‡ Boerhaave Aphorism. § 1218.



## 6 OBSERVATIONS ON THE

M. PETIT, in a short paper on the *hydrocephalus*, published in the Memoirs of the academy of sciences for the year 1718, observes, that in all the bodies which he had opened, he never found water any where within the *cranium*, but in the ventricles of the brain; and therefore supposes the other species of internal *hydrocephali* to be very rare.

THE symptoms of a dropfy in the cavities of the brain, according to that justly esteemed author, are, in the beginning, slight convulsions of the mouth and eyelids, biting of the lips, grinding of the teeth, and picking of the nose, as in the case of worms. The patients are either costive or have a purging, and sometimes a vomiting. They are more or less drowsy, according to the quantity of water within the brain. They grow languid, feeble, sad, and pale; the eyes look dull, the pupil dilates, the sutures of the skull open, and its bones become soft. The forehead rises, the eyes seem to be protruded



## DROPSY IN THE BRAIN. 7

truded out of their orbits, the head swells so as sometimes to burst, and the patient dies soon after.

ALTHO' this account of the symptoms of the *hydrocephalus internus* be much more just than what is to be met with in any author before M. Petit; yet still it is so far incompleat, that I may venture to say, that it will not be found sufficient to distinguish a dropsy within the brain, unless when it is attended with a swelling in the head.

M. PETIT mentions slight convulsions of the mouth and eye-lids in the beginning; whereas I have never seen any convulsions till towards the end. He says, the patients are always more or less drowsy; but I, on the contrary, have often observed them more watchful at first, altho' in the advanced state they not only become drowsy but comatose. He informs us, that he never saw the water collected any where, but in the ventricles of the brain. Now, were this the case, it  
is



### 8 OBSERVATIONS ON THE

is certain that the opening of the futures and swelling of the head could not happen but to the youngest infants, who, by the bye, are not so subject to this kind of *hydrocephalus* as children of two years old and upwards ; for, of about twenty patients whom I have seen die of this distemper, one only was under half a year old, the rest between two and sixteen ; who all went off without any swelling of the head, opening of the futures, or protrusion of the eyes.

LASTLY, M. Petit has taken no notice of the aversion to light, squinting, the variations of the pulse, and the degree of feverish heat, which, as we shall afterwards see, are the surest diagnostics of the disease.

M. LE DRAN, who wrote after M. Petit, has described the *hydrocephalus internus* in such a manner as would make one believe he had never seen the distemper, except when it happened to be joined to a  
collection



## DROPSY IN THE BRAIN. 9

collection of water between the *cranium* and brain \*.

DR DONALD MONRO, in his treatise of the dropfy, has well enumerated the several kinds of the *hydrocephalus*: But by the symptoms he mentions, of the internal kind, we shall be hardly able to distinguish it from several other disorders of the brain, as he himself has very justly remarked.

It may seem strange, that a dropfy of the ventricles of the brain, which in our days so frequently occurs, should have been altogether unknown to the ancients, and so little attended to by most of the moderns. The reason may be, that those patients who were carried off by this disease have been generally supposed to die of a fever ending in a *coma*; and in such cases the head is seldom opened.

ALTHO' a dropfy of the ventricles of the brain does very rarely occasion any  
B opening

\* See his Operations in surgery, article of the Dropfy.



## 10 OBSERVATIONS ON THE

opening of the futures, or swelling of the head \* ; yet in most cases it may be easily distinguished from every other disorder, by the following symptoms, which with the greatest care I have collected, in attending about twenty patients in this disease.

An

\* VESALIUS gives an account of a child of two years old, whose head was greatly enlarged, and in the ventricles of whose brain he found nine pounds of water : But this is an extraordinary case ; and it is probable the water began to be collected soon after the child's birth, and before the futures of the skull could offer any considerable resistance to its pressure. I shall only add here, that I have not only never observed any increase of the size of the head in the species of *hydrocephalus* of which I now treat, but that it is an error, though a common one, to imagine, that those children who have big heads are most liable to this disease ; for of all those whom I have attended, few or none were remarkable for the largeness of their head, but several had been very sprightly, and of a delicate make.



An Account of the SYMPTOMS in the  
DROPSY of the Ventricles of the  
BRAIN.

FIRST STAGE.

**C**HILDREN who have water in the ventricles of the brain begin to have many of the following symptoms, four, five, or six weeks, and in some cases much longer, before their death.

AT first they lose their appetite and spirits; they look pale, and fall away in flesh; they have always a quick pulse, and some degree of fever. In some cases I have seen a *hydrocephalus* attended with a considerable degree of fever, which had frequent remissions, but without any order or regularity: In other cases the paroxysms came on pretty regularly in the evening, and then the disease was taken for a slow irregular nervous fever, or for



## 12 OBSERVATIONS ON THE

one occasioned by worms. At this time, in children of five years and upwards, I have found the pulse at a hundred and ten, in others at a hundred and twenty, and in a few cases at a hundred and thirty, or even at a hundred and forty strokes in a minute; but rarely ever so full as to indicate bleeding.

IN others the quickness of the pulse and heat of the skin were not so considerable; but I do not remember to have seen any patient who had not some degree of fever in this, which I call the first stage of the disease.

WHILE the feverishness continues or increases, they lose their appetite more and more; their tongue is often white, sometimes it is remarkably clean, and towards the end of the disease acquires an aphthous redness. They are thirsty, and frequently vomit once or twice in a day, or once in two days. They complain of a pain in the crown of their head, or in the forehead above their eyes. They  
are



## DROPSY IN THE BRAIN. 13

are commonly costive, tho' sometimes they have returns of a looseness. When bound, they are not easily moved by a purge; sometimes they are troubled with gripes. Their spirits being low, they incline most-ly to lie in bed, altho' they are often more disposed to watching than to sleep. They cannot easily bear the light, and complain when a candle is brought before their eyes. They are observed to pick their nose, and in their sleep to grind with their teeth, as in the case of worms.

THESE are the symptoms of the first stage, during which it is very hard to distinguish this dropsy of the brain from a slow irregular fever occasioned by worms, by some other disorder in the bowels, or by some other cause. In the second stage, the symptoms enable us, with some certainty, to discover the nature of the ailment. But before I proceed to enumerate them, I shall just observe, that I never had but two patients who had not the vomiting during either the first or second stage.

One



## 14 OBSERVATIONS ON THE

One of these was a girl of eight years of age, who, tho' she had an aversion to food, yet never threw it up but once, and that was on the third day before her death; nor did she ever complain of a headach till twelve or fourteen days before she died; whereas this last symptom, for the most part, begins three or four weeks, and in some cases several months, before the end of the disease: She also could bear the light better than any I have seen. The other, who had no vomiting, was a boy of eleven years; he had little headach, altho' he lay much in bed, and did not like to be moved. But in general, the vomiting once or twice a-day, or once in two or three days, the headach\*, and the aversion to light, are the symptoms which in the first stage of this kind of *hydrocephalus* characterize it most,

Symp-

\* The headach not only in this, but the succeeding stages, is in some moderate, in others severe; in which last case, it is always easiest in the morning and worst at night; and these patients have commonly a great aversion to food.



## Symptoms of the SECOND STAGE.

I date the beginning of the second stage from the time the pulse, from being quick but regular, becomes slow and irregular. This sometimes happens about three weeks, often a fortnight or less, before the death of the patient.

IN this stage the pulse is commonly not only much slower than it was before, but often more so than in health. In a girl of thirteen, the pulse, which for a fortnight beat above a hundred times in a minute, about nine days before she died, fell to eighty-four, next day to seventy, and the day after to sixty, becoming always the more irregular the slower it was. In a youth of sixteen the pulse, which for several weeks had been feverish, on the fifteenth day before his death, beat only sixty-eight in a minute; two days after, it fell under sixty, and once to fifty.



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A boy of nine years of age, fifteen days before he died, had a pulse from seventy to seventy-five in a minute, and irregular. In another of four years, the pulse fell to eighty-eight on the ninth day before his end. In a girl of seven years old, on the fifteenth or sixteenth day before her death, the pulse beat a hundred and fifty times in a minute; next day, it became slower than natural and irregular; for five or six days after this, it was from eighty to eighty-six in a minute.

In two other children, who were less feverish in this stage, the pulse from a hundred fell below eighty. I have never seen a patient with water in the ventricles of the brain, whose pulse did not come down to its natural state, or very near it, except one. This was a girl of about seven, whose pulse, after being for several weeks about a hundred and thirty in the forenoon, and a hundred and forty in the evening, a fortnight before her death, fell



fell two or three strokes under a hundred; yet neither her heat nor thirst, nor other complaints abated, altho' her pulse had fallen above thirty in a minute.

IN this distemper it is observable, that when the pulse is nearly as slow, or slower than natural, it is always irregular or unequal, both as to the strength and the interval of the strokes. When it grows quicker, the irregularity lessens; and when it becomes very quick, it is then most equal and regular. Farther, it deserves notice, that, altho' in the second stage the pulse becomes much slower than it was before, the heat of the skin continues much the same, and sometimes seems rather to increase.

I have insisted the longer on the state of the pulse in this period, as from thence we can learn the surest *diagnostic*.

DURING the second stage, most of the symptoms mentioned in the first continue. The sick are then unable to sit up, tho' generally they sleep little, till towards

C

the



## 18 OBSERVATIONS ON THE

the end of this period, when they begin to grow drowsy. They moan heavily, yet cannot tell what ails them. Their eyes are often turned towards their nose, or they squint outwards, and sometimes they complain of seeing objects double. Some, towards the end of this stage, grow delirious, and cry out in a wild manner, as if they were much frightened: About this time also, or later, they frequently void either real worms, or some substance like worms in a dissolved state; yet this discharge gives no relief to the patient, and only helps to deceive the less experienced practitioner with regard to the nature of the disease.

THE urine in this, as well as in the other stages, varies; it has often a large sediment, sometimes none at all; but most commonly it deposits one of a light consistence and a white colour. In several I have observed the urine have a large furfuraceous sediment, till within a few  
days



## DROPSY IN THE BRAIN. 19

days of their death, when it had no separation.

THE breath has now, but especially in the last stage, such a sickish and offensive smell, as I do not remember to have observed in any other distemper. During the second as well as the first stage, the patients are often, for some days, or parts of days, much easier than at other times.

### Symptoms of the THIRD STAGE.

WHEN the pulse (which for some time was nearly as slow or slower than in a healthful state) rises again to a feverish quickness, and becomes regular, the third and last stage may be said to begin.

THIS change in the pulse is observed five, six, or seven days before death. In two patients only the pulse did not become more frequent till two days before they died; and in two others it began  
to



to grow quicker nine or ten days before that event.

As the time of this change in the pulse is different in different patients, so is the degree of its quickness. In some it rises gradually from below seventy, eighty, or ninety in a minute, to a hundred and twenty, a hundred and forty, a hundred and seventy, and sometimes above two hundred, before they expire. In others the pulse gets up more suddenly, in one day perhaps from a hundred to a hundred and fifty. In the last stage, after the pulse grows quicker, it does not keep constantly to the same measure, but will be often a good deal slower for part of a day, and quicker all the rest. The pulse beats generally faster on the day they die than at any time before. In one of those whom I attended, it beat above two hundred and ten times in a minute. I never knew any go off in this disease whose pulse did not rise to near a hundred and thirty strokes in that time.



## DROPSY IN THE BRAIN. 21

IN the third stage, the patient, who before was little disposed to sleep, becomes then drowsy and comatose. When roused, he utters only a few incoherent words, and appears to be insensible. The beginning of the *coma* is uncertain; it is often about the end of the second stage before the pulse grows quicker for the second time; but in a few cases I have known this quickness of the pulse come on before the patients become comatose.

FREQUENTLY one eye-lid loses its motion, and afterwards the other becomes also paralytic. About this time, or rather sooner, the pupil of one or both eyes ceases to contract, and remains dilated in the greatest light. But the time of this symptom varies much: In some it happens five, six, or seven days, in others only two or three days, before they die. Three or four days before the death of a boy of five years old, I was surprised to find the pupils, which had been much dilated before, no larger than natural. At first I  
flat-



## 22 OBSERVATIONS ON THE

flattered myself, that the distemper had taken some favourable turn ; but was soon undeceived ; for, upon giving the child a spoonful of weak cinnamon water, with some drops of *spiritus volatilis oleosus*, the pupils became as wide as they had been the day before. In less than half an hour after, they contracted again ; but immediately dilated upon holding some spirit of *sal. ammoniacus* to his nose. I have since observed the same interchanges in the pupils of a boy of four years old, on the third day before he died. In this case the pupils not only were enlarged, by giving him a spoonful of wine, or holding volatile spirits to his nose, but also by so small a *stimulus* as my lifting up his eye-lids, which had lost all their motion, and had fallen so far down as to cover near the half of the eye. Before they are seized with the *coma*, they sometimes complain of seeing strange and frightful objects. A day or two before death, the *tunica conjunctiva* of one or  
both



both eyes frequently becomes inflamed; but they generally continue to hear for some days after they are blind.

IN this stage, the patients are sometimes observed to be constantly raising one of their hands to their head; and are generally troubled with convulsions of the muscles of the arms, legs, or face, as well as with a *subfultus tendinum*. In a girl of thirteen, the day before she died, the hands were strongly bent inwards by a fixed spasm of their muscles. A youth of sixteen, who when in health had been liable to spasms, about the end of the second stage began to be affected once or twice a-day with a cramp in one of his arms, which ascended to his throat, and often prevented his speaking for some minutes. One of the cheeks will twice or thrice in a day grow hot and red, while the other, with the lips, remains pale and cold. These flushings generally appear two, three, or four days before death. In a boy of five years old, one side of both  
his



## 24 OBSERVATIONS ON THE

his arms became frequently red, while the other side never changed its colour. After death, the arms and breasts have been seen of a deep purple colour.

I had one patient who, four days before he died, bled once and again at the nose.

THOSE who have been costive before, often become loose in the third stage, and complain of gripes. A day or two before death, the patient either swallows with difficulty, or not at all. Lastly, the respiration grows more frequent and laborious; and in some there is a considerable pause after every expiration. This kind of breathing I have also observed in those who have died of an apoplexy, arising from a suppression of urine.

UPON opening the heads of ten of those patients from whom I have collected the symptoms above mentioned, I found in all of them a clear thin fluid in the anterior ventricles of the brain, immediately below the *corpus callosum*.

There



## DROPSY IN THE BRAIN. 25

There was frequently the same kind of liquor in the third and fourth ventricles ; but whether this is always the case, I cannot say, as I had not attended sufficiently to this circumstance. I never met with water between the *dura mater* and the brain, between the hemispheres of the brain, or immediately above the *corpus callosum*. Altho' there seems to be a communication between the two anterior ventricles ; yet, in two cases, I found one of them much distended, while the other contained but little water.

THE quantity of water contained in the ventricles of the brain was generally from two ounces to five ; but I have been told of one case in which it amounted to near eight ounces. This fluid does not coagulate with heat, like the *serum* of the blood, or the lymph that is found in the *pericardium*, or what is taken from the abdomen by tapping in a dropsy ; and this difference seems to be owing to the exha-

D

ling



## 26 OBSERVATIONS ON THE

ling arteries of the brain being much smaller than those of the other parts.

### The DIAGNOSTIC SIGNS of a Dropfy within the Brain.

HAVING given an account of all the various fymptoms commonly attending a collection of water in the brain, I fhall now recapitulate fuch of them as are the fureft figns by which we may diftinguifh this diforder from others, which fo much refemble it as fometimes to deceive an experienced phyfician: And this will be the more neceffary, as the ancients were altogether ignorant of the difeafe, and as the few of the moderns who treat of it feem to have defcribed it more from theory than obfervation.

WHILE moft of the later writers have confounded the figns of a dropfy in the ventricles of the brain with thofe of the  
*hydro-*



*hydrocephalus externus*, a few have more reasonably assigned to this species of drop-  
 sy such symptoms as commonly attend a  
 compression of the brain, but without  
 giving such a distinct account of the first  
 appearance and progress of this disorder  
 as could enable a physician to distinguish  
 it from others of the head, from worms,  
 from a foulness in the stomach and  
 bowels, or from a slow fever ending in  
 a *coma*.

I have already observed, that in the first  
 stage it is hard to discover this internal  
*hydrocephalus*. But when we meet with  
 a patient under fifteen or sixteen years of  
 age, seized with a slow fever of no certain  
 type, and irregular in its accessions and  
 remissions; when in that fever the patients  
 vomit once a day, or once in two or  
 three days; when they shun the light,  
 and complain of a pain in the crown of  
 their head, or over their eyes, after the  
 fever has continued for some time, or of



## 28 OBSERVATIONS ON THE

a pain thereabouts, that in some does not abate like the headach in ordinary fevers: When these complaints neither yield much to repeated vomits, gentle purges, nor blisters, I say there is reason to suspect water in the ventricles of the brain. But as worms, and other disorders of the stomach and intestines, are sometimes attended with most of these, as well as other symptoms that accompany the internal *hydrocephalus* in its first stage, we are often at a loss to find out this disease, till it arrives at its second period, when the pulse begins to grow nearly as slow, or even slower than natural, but irregular; for this change of the pulse, added to the symptoms of the first stage, is, as I have observed, almost an infallible sign of water in the brain, if at the same time the patient is not relieved, and if the feverish heat does not abate with the quickness of the pulse \*.

WHEN

\* If we are to judge of the heat of the body in this disease, by feeling the hands and wrists, we shall be often deceived;



## DROPSY IN THE BRAIN. 29

WHEN the glands of the mesentery become scirrhus, the patients are liable to a slow fever; their pulse is quick and sometimes irregular, but is never so slow as in health. In the case of worms in the stomach and intestines, altho' the pulse be generally quick, yet sometimes it is slower than natural, and irregular; but when this happens, the skin is cool, and there is no fever. But in the dropsy of the brain, when the pulse becomes slow and irregular, neither the heat of the skin, nor any other of the feverish symptoms are sensibly abated: For in this case the motion of the heart is not accelerated in proportion to the degree of heat and fever.

WE often find a slow irregular pulse, in persons of a delicate habit, when labouring under cramps of the stomach, spasmodic colics, and violent nervous headaches,

deceived; for when these are exposed to the air, they become rather cold, while such parts as are well covered have a feverish heat.



### 30 OBSERVATIONS ON THE

achs, (as they are commonly called); but it is observable, that in such cases this kind of pulse is always attended with a cool skin.

WHEN therefore, with a slow and irregular pulse we meet with thirst and a feverish heat, watching, a *strabismus*, or double sight, a *delirium*, and screaming, succeeding the symptoms mentioned in the first stage, we may strongly suspect water in the ventricles of the brain. But this is still more evident, when soon after the patient grows comatose, the pupil dilates and loses its motion, the pulse becomes quick, the cheeks are flushed, the tendons start, and convulsions follow.

IT is true indeed, that some of these very symptoms are observed towards the end of common fevers, in which, from the brain being much affected, the patient falls into a *coma* before his death. But a fever from water in the brain is easily distinguished from others, by attending to the whole course of the disease,  
and



## DROPSY IN THE BRAIN. 31

and particularly to the pulse, which, after having been at first quick, becomes slow and irregular; and lastly acquires a greater frequency than ever. Besides, the screaming, squinting, and dilatation of the pupil, rarely occur in other fevers.

THE symptoms of no distemper resemble these of water in the brain so much as those which arise from worms in the stomach; for with a slow fever there is a want of appetite, vomiting, pain in the head, raving, and convulsions; but when worms in the stomach or intestines occasion a slow and irregular pulse, the patients have not that feverish heat so observable in the internal *hydrocephalus*.

### Of the CAUSES of a DROPSY in the Ventricles of the Brain.

THE immediate cause of this disease, and indeed of every kind of dropsy, is always the same, *viz.* such a state of the parts



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parts as makes the exhalant arteries throw out a greater quantity of fluids than the absorbent veins can take up.

THIS may be owing to several causes :

1. THERE may be an original laxity, or weakness in the brain, whereby the small exhalant arteries of the ventricles will throw out the lymph faster than the absorbent veins can imbibe it.

IN children under a year old, I have frequently met with a *hydrocele*, or collection of water between the *tunica vaginalis* and the testicle, from such a cause : And this disease I have cured by small doses of rhubarb, by applying linen cloths dipt in brandy, or impregnated with the fumes of *myrrh*, *olibanum*, and *succinum*, to the *scrotum*, and by supporting the testicles with a bandage or truss. If in young children we could discover the dropfy of the brain as early as we do that of the testicles, and could apply our remedies

as



as near to the part, we should probably often succeed in the cure: Tho' a dropfy in the brain would always be more unfavourable, as the circulation there is slower and more languid than in any other part.

2. ALTHO' there has been no original weakness in the brain, yet it may have suffered so much in the time of birth, by the compression of the skull, as afterwards to give rise to a collection of water in its cavities.

3. A scirrhus tumour of the *glandula pituitaria*, or in any part contiguous to the ventricles of the brain, by compressing the neighbouring trunks of the absorbent veins, will prevent the due absorption of that fluid which the small arteries constantly exhale, and occasion a dropfy in the brain; in like manner as a scirrhus liver, spleen, or *pancreas*, are often the cause of an *ascites*. As a proof of this,

E

we



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we may observe, that M. Petit often found the *glandula pituitaria* scirrhus in those who died of a dropfy of the ventricles of the brain.

IN one case I met with a hard tumour within the right *thalamus nervorum optico-rum*: It was almost as large as a small hen's egg, of a yellowish colour within, and of a firm consistence.

4. ALTHO' there may be no obstruction in any part of the brain, a dropfy may be formed in it, merely from a too thin or watery state of the blood. When the blood is too thin, the exhalent arteries will pour forth their fluids in greater quantity than usual; while the bibulous veins will absorb them more sparingly; and from this cause the water will be apt to accumulate, either in the *abdomen*, *thorax*, or brain, according as one or other of these parts is the weakest. I have known an instance of a dropfy in the cavity of the *abdomen*, where there were no obstructed *viscera* to be seen  
after



after death, and where the cause of the disease seemed to be no other than a dissolved state of the blood joined to an uncommon relaxation of the vessels.

ABOUT fifteen years ago, I had a patient who died of the *hydrocephalus*, probably owing to this cause; for this child, about a year before his death, and after the measles, falling into a bad state of health, the blood taken from his arm was observed to be preternaturally thin. From this time he never recovered his looks or strength; and, about ten months after, the symptoms of the *hydrocephalus* appeared. In this case I thought it probable, that the water began to be collected in the brain soon after the measles, which first broke the health of the child, and then the blood became too watery.

5. A suppression, or a diminished secretion of urine, may also give rise to this disease. Thus grown people, who die of an *ischuria*, have often water in the ventricles



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tricles of the brain, and become comatose before their death ; but such patients generally die before any considerable quantity of water is collected in these cavities.

6. LASTLY, in tedious chronic diseases, water is often collected in the ventricles of the brain, as well as in the cavity of the *pericardium*, but not in such quantity as to occasion the symptoms of a dropfy within the brain.

AN ATTEMPT to account for some of the most remarkable SYMPTOMS attending a Dropfy in the Brain.

IN general, the whole symptoms of this disease proceed from different degrees of the same cause, *viz.* the pressure or distension of the parts of the brain, occasioned by the water contained in its ventricles.

I. THE



1. *THE loss of appetite and inclination to vomit*, are owing to the disordered state of the brain, between which and the stomach there is so great a sympathy, that in wounds of the head, where the brain is hurt, a vomiting is almost a constant symptom.

2. *THE aversion to light, in the first and second stage of the disease*, proceeds from an increased sensibility of the *retina*; and this is probably owing to the irritation of the *thalami nervorum opticorum*, in consequence of the water accumulated in the anterior ventricles of the brain.

3. *THE slow irregular pulse in the second stage.*

THE motion of the heart is owing to the irritation of the returning venous blood poured into its ventricles. This irritation, however, could have no effect upon the heart, were it not for its sensibility, which depends intirely on its nerves.

Wherefore,



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Wherefore, in a *hydrocephalus*, when the water is collected within the brain in such quantity as to press, with a considerable force, on the medullary substance, the nerves proceeding from it will in some degree lose their powers, and consequently the heart will be less sensible. And hence the pulse becomes often as slow, and sometimes slower than in a natural state, altho' there be a real fever in the body; which fever, were it not for this pressure on the origin of the nerves, would occasion a quick pulse.

WHEN, in this disease the pulse is slow, it is always more or less irregular; and this may also be owing to the nerves of the heart being, in some measure, deprived of their usual power, by which means that organ cannot move with its wonted steadiness and regularity.

### 4. *THE quick pulse in the third stage.*

OF all the symptoms that attend a dropsy in the brain, there is none so hard  
to



to be accounted for as the quick pulse towards the end. For if the pressure of the water occasioned the slow pulse in the second stage, one would imagine that in the third, when this pressure is increased, the sensibility of the heart should be still more impaired; and that therefore its motion should be slower, instead of being quicker. However, we find in fact, that the pulse is remarkably quicker towards the end, when the pressure of the water must be greatest; let us therefore inquire what may probably be the reason of this symptom.

WHEN, in the second stage, the pressure on the sides of the ventricles of the brain occasions the slow irregular pulse, it seems to produce this effect, by lessening the sensibility and other powers of the cardiac nerves. When in the third stage the water increases, this pressure must be greater; and therefore it might be natural to think, that these nerves should be rendered still more unfit for performing their function.

But



But we must consider, that when the sides of the ventricles are stretched by the water beyond a certain pitch, the violence done to the medullary fibres of the brain causes such an uncommon irritation as must quicken the pulse: For in animals newly dead (where we must suppose the nerves to be still more insensible and unfit for action, than in the third stage of the *hydrocephalus*) an irritation of the *medulla oblongata* restores the motion of the heart; and if, as I have observed above, the volatile salts held to the nose, or cinnamon-water taken into the mouth, by their *stimulus*, though for a short time, give new vigour to the nerves of the *uvea*, (which towards the end of this disease begin to lose their powers), why may not the irritation of the medullary part of the brain, occasioned by the immoderate distension of its ventricles, so affect the nerves of the heart as to accelerate its motion?

IN



IN an apoplexy, the pulse, tho' at first slow, becomes very quick towards the end ; and indeed, in almost every disease, the pulse is uncommonly quick before death, not because the nerves of the heart are then more sensible, or fitter for performing their office, than they were before, but because at that time there is an uncommon struggle in the body, and all its powers are excited into action by the great irritation of the brain and nervous system. The same seems to be the case in those who are dying of a dropsy in the brain ; for how much soever the medullary part of the brain may be compressed, yet the convulsions which happen in the last stage show that the brain and nerves are sensible of irritation, and still retain their power of putting the muscles in motion.

5. *THE dilatation of the pupil.*

THE contraction of the pupil is owing to the uneasy sensation excited in the re-

F

tina



*tina* by too much light; and hence it is, that in a dark place, or when the *retina* becomes insensible of the *stimulus* of light, the pupil is always observed to be wide. In the *hydrocephalus*, when the water in the ventricles presses so much on the *thalamus nervorum opticorum* as to render the optic nerves in a great measure insensible, the *retina* will no longer feel the impression of light; and therefore the pupil will remain dilated.

IN the account of the symptoms of the third stage, I mentioned an instance of a boy of five years of age, whose pupils were much dilated on the fifth day before he died; but we observed them next day to be as much contracted as is usual in a person in health placed in a moderate light. At this time, having endeavoured to rouse the patient, by holding a volatile spirit to his nose, and making him swallow some cinnamon-water, the pupil instantly became as wide as it had been the night before. In about half an hour after,



ter, I found the pupils again contracted; but they were presently enlarged as before, upon holding the spirit of *sal. ammoniacus* to his nose. This experiment I repeated four times in two days, and always with the same success.

IN this case the dilatation of the pupil was at first owing to the compression of the *thalami nervorum opticorum* by the water contained in the anterior ventricles of the brain. But soon after, the origin of those nerves which serve the *uvea* being also considerably compressed by the increased quantity of water, the longitudinal fibres of this membrane (which by their natural contractility dilate the pupil) become paralytic and flaccid, as happens in the bodies some time after death; wherefore the edges of the pupil being less drawn outward, of course it would become smaller.

THE volatile spirits applied to the nose, by irritating its nerves, so affected the brain as to give some vigour for a short



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time to the nerves of the *uvea*, by which means its longitudinal fibres, regaining their power of contraction, immediately dilated the pupil; but as soon as the effect of this *stimulus* ceased, the fibres of the *uvea* being again deprived of their contractility, the pupil returned to its former dimensions.

##### 6. *THE slow respiration towards the end of the disease.*

IN this kind of breathing (which I have also observed in patients who died of an apoplexy and an *ischuria*) there is a considerable pause after every expiration before a new inspiration succeeds. This pause is ordinarily for a few seconds; but I have sometimes observed it longer, and in one apoplectic case it continued above half a minute. Now the brain being greatly compressed, the uneasy sensation arising from the difficulty the blood finds in passing through the lungs will be much less felt than usual: Hence, after  
expiration



expiration (which is performed by the power the cartilages of the ribs have to restore themselves) a long pause intervenes before a new inspiration takes place; because the mind is not excited to put in motion the muscles concerned in inspiration, till the sense of suffocation in the breast becomes so great as to rouse, as it were, the sentient principle from its lethargic state.

#### Of the CURE of a DROPSY in the BRAIN.

IF this disease could be known early, and before any considerable quantity of water has been collected, it might probably be sometimes cured by purgatives, diuretics, blisters, frictions, exercise, and diet. But as it never discovers itself till so much water is accumulated as, by its pressure on the sides of the ventricles, to disturb the action of the brain, we have  
little



little to hope from any medicine. An *ascites* indeed has been often cured by diuretics, or purgatives. But if we consider the distance between the brain and the *abdomen*, (where these medicines by their *stimulus* increase, in a particular manner, the action of the absorbents, at the same time that they evacuate the watery part of the blood), the extremely slow motion of the fluids in the small vessels of the brain, and the pressure of the water on the sides of its ventricles, which must render the absorption of that fluid still more difficult, we shall see the reason why diuretics and cathartics should be so inefficacious here.

IN an *ascites* the patient is generally relieved, and sometimes cured by tapping; but in a dropsey of the ventricles of the brain, any such attempt to draw off the water, could have no other effect than to hasten death.

I freely own that I have never been so lucky as to cure one patient who had  
those



## DROPSY IN THE BRAIN. 47.

those symptoms which with certainty denote this disease \*; and I suspect that those who imagine they have been more successful, have mistaken another distemper for this. I remember several years ago, that an able and experienced physician being called to a child of a year old, in a fever attended with convulsions and a *coma*, was of opinion, that the disorder proceeded from water in the head; on which account, besides blisters which had been applied before, he ordered a purge of jalap and calomel, which had a very good effect; for in two or three days the *coma* and convulsions ceased, and the patient soon recovered; which, I am persuaded, could not have been the case, had he

\* The medicines I chiefly used were repeated purges of rhubarb or jalap, with calomel and blisters; by which last I have seen the patients somewhat relieved for a short time in the second stage. I have also ordered the powder of *asarum* to be drawn up into the nostrils, with a view to make a discharge of a watery humour from the vessels of the head.



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he laboured under a dropfy of the brain. Farther, this child was not only fuddenly feized with the fever, (as commonly happens when it takes to the head), but at no time of his illnefs had he either an irregular or a flow pulfe, or indeed any number of the other fymptoms which I confider as effential for diftinguifhing the *hydrocephalus internus* from another difeafe.

T H E E N D.



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A C C O U N T  
O F

Some EXPERIMENTS made with  
O P I U M on Living and Dying  
ANIMALS.

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THE TREATMENT OF

THE DISEASES OF

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A N  
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Some EXPERIMENTS made with OPIUM  
on Living and Dying ANIMALS \*.

THE ancient physicians imagined, that  
*opium* extinguished the flame of life  
in animals by its excessive cold; and in  
later times, there have not been wanting  
those who deduced its effects from a quite  
opposite quality, whereby it was thought  
to rarefy the blood, and to compress the  
brain or origin of the nerves. These false  
notions, however, of the nature and action  
of *opium* have been refuted by several  
of the moderns, whose writings have  
thrown considerable light upon this sub-  
ject.

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\* August 7. 1755.



## 52 EXPERIMENTS MADE WITH

THE following experiments were made with a view still further to illustrate the manner in which this wonderful drug produces its effects, and particularly to shew its influence upon the motion of the heart.

1. HAVING injected a solution of *opium* in water, into the stomach and guts of a frog, I observed, that in little more than half an hour it seemed to have lost all power of motion, as well as feeling; for there was no contraction produced in the muscles of its limbs and trunk by irritating them. I opened the *thorax* an hour after the injection, and found the heart, instead of between 60 and 70, making only 17 pulsations in a minute. The auricle, which was much distended with blood, always contracted first, and after it the ventricle.

2. A frog continued to move its limbs, and leap about for above an hour after



I had cut out its heart, and was not quite dead after two hours and a half.

FIVE minutes after taking out the heart of another frog, I injected a solution of *opium* into its stomach and guts. In less than half an hour, it seemed to be quite dead; for neither pricking nor tearing its muscles produced any contraction in them, or any motion in the members to which they belonged. After cutting off its head, a probe pushed into the spinal marrow made its fore-legs contract feebly.

3. EIGHTEEN minutes past four in the afternoon, I injected a stronger turbid solution of *opium* in water than that used in the preceeding experiments\*, into the stomach and guts of a frog; and as it squirted out most of the solution  
injected

\* *Viz.* half an ounce of *opium* dissolved in eight ounces of water; which was also made use of in all the following experiments. The heat of the solution was nearly the same in all the experiments, *viz.* about 60 degrees of Farenheit's thermometer.



## 54 EXPERIMENTS MADE WITH

injected by the *anus*, I threw in some more in its place. At twenty four minutes past five, I opened this frog, and observed the heart with its auricle greatly distended with blood, and beating very slowly, not above seven times in a minute. When the heart was touched with the point of a pair of scissars, its motion was rendered quicker for two or three pulsations: after which it became as slow as before.

4. IMMEDIATELY after decollating a frog, I destroyed its spinal marrow, by pushing a small probe down through its spine, which occasioned strong convulsions of all the muscles, especially those of the inferior extremities. Ten minutes after this, I opened the *thorax*, and found the heart beating at the rate of 45 times in a minute. Sixteen minutes after decollation, it moved 40 times in a minute. After half an hour it made 36, and after fifty minutes, only 30 pulsations in the minute;



minute; which were now also become very small and feeble.

*N. B.* WHEN I opened the *thorax* of another frog immediately after decollation, and destroying its spinal marrow, I observed its heart beating at the rate of 60 in a minute, which is four or five pulsations less than I have generally seen the hearts of frogs make in that time, when their *thorax* was opened without decollation.

5. AT nine minutes past eleven in the forenoon, immediately after decollating another frog, I destroyed its spinal marrow with a red hot wire, which produced terrible convulsions in all the muscles, as in the last experiment. I opened the *thorax* of this frog thirty-five minutes after decollation, and observed its heart beating 30 times in a minute. The contraction of the auricle regularly preceded that of the heart. The auricle was not near so much distended with blood, nor the heart so much swelled as in those frogs which had a solution



## 56 EXPERIMENTS MADE WITH

lution of *opium* injected into their stomach and guts \*. At one o'clock (*viz.* an hour and fifty-one minutes after decollation) the heart of this frog made 20 pulsations in a minute. At half an hour past two, when the room was become warmer by the shining of the sun, it beat 25 times in a minute; and when placed in the sun-beams, it performed 31 contractions in that time. After this, I removed the frog to an east window, where it was exposed to a cool breeze; upon which the motion of its heart became slower, so that in a short time it only made 25 pulses in a minute. I then exposed it a-new to the sun-beams, by which its motion was soon quickened, so that it beat 30 times in a minute.

AT twenty-five minutes past five in the evening, (*viz.* six hours and sixteen minutes after decollation and the destruction

\* See No. 3. above, and Essay on the vital and other involuntary motions of animals, p. 371 & 372.



tion of its spinal marrow) the auricle of this frog's heart, which was still filled with blood, contracted 12 times in a minute; but the heart itself lay without motion, was swelled and very red: However, when pricked with a pin, it performed two or three pulsations, and then remained at rest, till roused by a new *stimulus*. At thirty-five minutes past five, the heart seemed to be quite dead, but the auricle continued its motion; nay, at half an hour past eight, near three hours after the heart had been without motion, the auricle, which was very near as much filled with blood as when I first opened this frog, beat 11 or 12 times in the minute: Its pulsations, however, were not now so regular as to time, as they had been before.

Is it not probable, that the auricle of this frog's heart beat longer than usual, because it continued, to the last, to be filled with blood; whereas generally the auricles of frogs hearts, which are open-

H

ed



## 58 EXPERIMENTS MADE WITH

ed after decollation and the destruction of their spinal marrow, expell after some time the blood which they contain, and acquire the appearance of a small pellucid bladder filled with air?

6. I laid bare the abdominal muscles and *thorax* of a frog, by dissecting off the skin, and at twenty minutes before nine in the morning, I immersed the whole body of the frog in a turbid solution of *opium* in water, in a small basin, which I covered to prevent the frog from leaping out of it. Thirty-five minutes after immersion, I took it out of the solution, and opened the *thorax* and *pericardium*. The heart's auricle, which was much distended with blood, beat 15 times in a minute, but the heart itself only 6 times. Forty minutes past nine (*viz.* twenty-five minutes after the frog was taken out of the solution of *opium*) the heart seemed to have recovered more life; for it performed eight pulsations in a minute: The contractions of the auricle now became feebler,



feebler, and were scarce more numerous than those of the heart, but always preceded them some little time. Six minutes before ten this heart moved only six times in a minute. Twenty-four minutes past ten it made only five pulsations in sixty-five seconds, the first, third, and fifth of which pulsations were after an interval of fifteen seconds, and the second and fourth after a pause of ten seconds. Seventeen minutes before twelve, and two hours and twenty-eight minutes after the frog was taken out of the solution of *opium*, its heart moved only thrice in seventy-five seconds, and performed its *systole* very slowly. Before two o'clock afternoon the heart was quite dead; but how long, I cannot say, not having had leisure to observe it from a quarter before twelve till near two.

7. AFTER cutting off a frog's head, and destroying its spinal marrow with a red hot wire, I laid bare the abdominal muscles and *thorax*, as in the last experiment,



## 60 EXPERIMENTS MADE WITH

ment, and immersed the whole body of the frog in a turbid solution of *opium*, at half an hour past nine in the morning. Thirty-six minutes after immersion I took it out of the solution, and opened its *thorax* and *pericardium*. The heart and its auricle beat, each, twenty-six times in a minute, and the pulsations of the auricle preceded those of the heart regularly. The heart did not appear to be more swelled or redder than in a natural state, and the auricle was not near so full of blood as in Exp. 6. Twelve minutes past ten, *viz.* six minutes after this frog was taken out of the solution of *opium*, its heart beat twenty-seven times in a minute. At eleven o'clock it performed eighteen vibrations in that time; and sixteen at a quarter before twelve. At two o'clock after noon, the auricle, which, having expelled all its blood, was now only filled with air, continued its motions; but the heart lay at rest. Ten minutes past four, *i. e.* five hours and forty-four minutes after the  
the



the frog was taken out of the solution, the auricle of its heart beat nine times in sixty-four seconds.

8. I laid bare the abdominal muscles and *thorax* of another frog, and at fourteen minutes past eight in the morning, immersed it as above in a turbid solution of *opium*. Fourteen minutes past nine, I took it out of the solution, and laid open its *thorax* and *pericardium*; after which the heart began to beat at the rate of nine times in a minute: But the auricle, which was greatly distended with blood, made no motion, except in so far as it was agitated a little by the pulsation of the heart: Nor were the muscles of the legs or thighs brought into contraction by cutting or tearing their fibres. At half an hour past nine the heart beat only seven times in a minute; and the auricle, which was now pretty empty of blood, and, in place of it, filled with air, had a pulsation as well as the heart. Thirteen minutes before ten, *i. e.* thirty-three minutes

minutes



## 62 EXPERIMENTS MADE WITH

minutes after the frog was taken out of the solution, the auricle shewed, at considerable intervals, a very faint pulsation; but the heart lay without any motion.

9. THE same day, after cutting off the head and destroying the spinal marrow of another frog, I laid bare its abdominal muscles and *thorax*; and, at eighteen minutes past ten, immersed it in a solution of *opium*, as above. Eighteen minutes past eleven, I took it out of the solution and opened its *thorax* and *pericardium*, after which the heart began to move at the rate of eight times in a minute. Twenty-five minutes past eleven, the heart beat 15 times in a minute; and at twelve o'clock it performed between 13 and 14 vibrations in the same time. At two o'clock, (*viz.* two hours and forty-two minutes after the frog was taken out of the solution), the auricle, which was now filled with air, continued to vibrate weakly about 11 times in the minute; but the heart itself was without motion. At ten minutes



minutes before four in the afternoon, the auricle still continued to move, but more feebly than the auricle of N<sup>o</sup> 5.

10. I laid open the whole *abdomen* of a larger frog than any of the former ; and, at twenty-two minutes past ten in the morning, immersed it in a solution of *opium*, as above. Thirty-five minutes after immersion, I took it out of the solution, and opened its *thorax* and *pericardium*. The heart was vastly red and much swelled, and its auricle greatly distended with blood ; but both were without any motion : After two minutes, however, the heart began to vibrate at great leisure, scarcely performing nine pulsations in a minute ; but the overstretched auricle made not the smallest motion. During every *systole*, the heart was remarkable paler, and in the time of its relaxation became much redder ; which appearance I observed likewise in all the frogs hearts in the above experiments, but more remarkably in those frogs who had been exposed



## 64 EXPERIMENTS MADE WITH

exposed to the action of *opium*. Another thing, which I remarked in all these experiments, was, that the heart during its *systole*, became manifestly shorter, and was lengthened in the time of its relaxation. But to return; at six minutes past twelve, (*i. e.* an hour and nine minutes after the frog was taken out of the solution), its heart made only six pulsations in the minute; and at eleven minutes past twelve, observing it without motion, I pricked it with a pin, and breathed upon it, in order to renew its pulsation; but to no purpose.

II. TWENTY eight minutes past seven in the evening, I laid open the whole *abdomen* and *thorax* of a frog, and immediately after immersed it in a solution of *opium* as above. Thirty-eight minutes past seven, when I pricked its legs with the point of a penknife, it made very little motion. Two minutes after this, I turned it to its back, and observed its heart moving only between ten and eleven times



times in a minute. Having laid the frog again on its belly, that it might be more exposed to the action of the *opium*; at forty-eight minutes past seven, *i. e.* twenty minutes from the first immersion, I turned it again to its back, and observing the heart without motion, I opened the *pericardium*; which producing no effect, I cut the heart out of the body, and laid it on a plate, when it gave two or three pulses, and never after moved, though it was pricked once and again with a pin.

No motion was produced in any of the other muscles of this frog, by irritating them.

12. I cut off a frog's head and destroyed the spinal marrow with a hot wire, then laid open its *thorax* and *abdomen*, and immersed it in a solution of *opium* at nineteen minutes past eleven. Eight minutes before twelve, *i. e.* thirty-three minutes after immersion, I observed its heart beating very slowly: But two minutes before twelve, when I took it out of the solution



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of *opium*, it had no motion. After this, I opened the *pericardium*, and irritated the heart two or three times with the point of a *scalpel*, which always produced a few pulsations. I then put the frog in the solution for five minutes more, and, upon taking it out, found its heart quite dead.

13. AFTER cutting off a frog's head and destroying its spinal marrow, I laid open its whole *abdomen*, and immersed it in a solution of *opium*, twenty-three minutes before one. After it had lain sixteen minutes, I cut up its *thorax* and *pericardium*; and observing the heart beating very regularly and pretty strongly twenty-one times in the minute, I immersed it again in the solution, which had now immediate access to the heart. After five minutes, I took it out of the solution; and finding the heart without motion, I pricked it with the point of a knife; upon which it began to beat at the rate of fourteen times in a minute, and continued



nued its motions very languidly, and not without some interruption, for about a quarter of an hour.

14. I cut out the heart of a frog, and put it in fountain-water at ten minutes past ten; immediately after immersion, it beat about twenty-eight times in the minute. Eighteen minutes past ten, it made six pulsations in thirty seconds. Twenty minutes after ten, I took it out of the water and laid it on a table, and observed, that as often as it was gently touched with any thing, it made one full and strong contraction, and no more: However, in four or five minutes, it began to beat of its own accord, and, at twenty-eight minutes after ten, performed nineteen pulsations in a minute. Thirty-five minutes past ten, it beat twelve times in a minute.

15. TWENTY three minutes past twelve, I cut out the heart of another frog, and put it in fountain-water. After twelve minutes immersion, I took it out



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out of the water, when it beat above twenty times in a minute. Having put it in the water for five minutes more, it ceased from motion, and when taken out, did not move except when pricked, and then only performed one pulsation.

16. EIGHT minutes past eleven, I cut out the heart of a third frog, and put it into fountain-water. Eleven minutes after immersion its heart beat eight times in a minute, and four minutes after this it vibrated eleven times in thirty seconds; but the motion was confined to about one third part of the heart next its *apex*. Twenty minutes after immersion, it continued to move much in the same way; but in two minutes more, observing no motion in it, I took it out of the water, and laid it on a table, where it remained at rest, unless when touched. Soon after this, however, it began to move; and at twenty-five minutes after immersion, it made nine pulses in sixty-three seconds. Four minutes after this, it moved only  
thrice



thrice in fifty seconds, and then ceased altogether; unless that, when pricked with the point of a knife, it gave one very faint pulsation. At forty-seven minutes past eleven, it was quite dead.

17. I cut out the heart of a fourth frog, and at thirty minutes past ten immersed it in a turbid solution of *opium* in water of the same degree of heat with the fountain-water used in the three last experiments \*. After this heart had been immersed ten minutes, I took it out of the solution, and laid it on a table, but it made not the smallest motion; and when pricked with the point of a knife, though it quickly recovered its shape, yet it was not excited into a proper contraction, as the heart of N<sup>o</sup> 14. I continued to observe this heart from time to time for more than half an hour; but it never made the least motion.

18. I cut out the heart of a fifth frog, and put it into a solution of *opium* in water

\* *Viz.* Nearly sixty degrees of Farenheit's thermometer.



## 70 EXPERIMENTS MADE WITH

ter five minutes before eight. After seven minutes immersion, I took it out, and laid it on a plate, where it remained at rest. When pricked with a knife, it did not perform a full pulsation like N<sup>o</sup> 14. but seemed to feel a little, by a very faint kind of motion which was excited in some of its fibres.

19. At thirteen minutes before twelve, I cut out the heart of a sixth frog, and immersed it in a solution of *opium*. Six minutes after immersion, it had no motion; but when pricked made one pulsation. After lying five minutes more in the solution, it was quite dead.

20. I cut out the heart of a seventh frog, and at thirty-seven minutes past nine in the morning, immersed it in a solution of *opium* as above. Forty-two minutes after nine, when I took it out of the solution, it was without motion; but when touched with the point of a knife or probe, it performed one contraction, but with less vigour and more slowly than



than the heart of N<sup>o</sup> 14. Forty-seven minutes past nine, it began to beat of its own accord. Two minutes after this, it moved six times in a minute, but much more feebly than N<sup>o</sup> 14. Six minutes before ten, it beat only four times in a minute: after this, it began to beat much faster; but its motions soon returned to their former slowness. At ten, after having lain near a minute without motion, it began again, of its own accord, to beat at the rate of seventeen times in a minute, and continued for eight or ten minutes after this to beat very feebly, and in an irregular manner as to time.

21. MR ROBERT RAMSAY student of medicine in this place, having dissolved two scruples of *opium* in an ounce of water and a dram of liquid *laudanum*, injected it blood-warm into the *intestinum rectum* of a very small dog near six months old. In less than a minute after the injection was made, the dog could not stand on his hinder-legs; and in  
three



## 72 EXPERIMENTS MADE WITH

three or four minutes he had lost the use of them so much, that when they were strongly pinched, he neither moved them, nor seemed in the least degree sensible of pain. He could, however, still scramble about with his fore-legs; and when they or his ears were pinched, he howled remarkably, and seemed to feel considerable pain. Ten minutes after the injection, he lay as if he had been quite stupid; only when a noise was made by beating on the ground, he opened his eyes a little and howled, but presently after fell into a profound sleep. In a few minutes after this, he began to be convulsed; upon which Mr Ramsay injected a strong solution of sea salt in water into his guts, which purged him severely, and occasioned a *prolapsus ani*; soon after this, he awakened from his sleep, and gradually recovered the use of his hinder-legs; so that in less than an hour he could run about the room, though he often fell down, his legs bending under him. After three or four hours,



hours, he seemed to be quite well in every respect; but altho' the experiment was made at mid-day, he could taste no meat till late at night. When he was in the most stupid state, he could make use of his fore-legs, and complained when his ears were pinched.

22. THE same young gentleman, at my desire, made the following experiment. On the 9th of April 1755, after making an opening into the cavity of the *abdomen* of the dog on which the last experiment was made, he injected by the wound a dram of *opium* dissolved in two ounces and a half of water; but before he could stitch up the wound, about an ounce of the solution escaped. The dog lost the power of his hinder limbs almost instantaneously. Two minutes after the injection was made, he began to be convulsed; and, in two minutes more, after having raised himself upon his fore-legs, he fell down senseless. At this time Mr Ramfay laid bare the *thorax*, by dissecting off the

K teguments,



## 74 EXPERIMENTS MADE WITH

teguments, which did not seem to give the dog any pain, and could plainly feel the motion of his heart thro' the *pleura*: it beat seventy-six times in a minute, but became gradually slower \*. Immediately after counting the pulse, Mr Ramsay cut the ribs on each side of the *sternum*, which he laid back in the usual way. The heart, which was thus brought in view, appeared quite turgid, and continued in motion about five minutes; during which time it performed only between sixty and sixty-five weak vibrations, for they were not compleat contractions. While the heart was thus moving, warm *saliva* was first applied to it, then cold water, and last of all oil of vitriol; which shrivelled the parts it touched almost in the same manner as a hot iron would have done; but none of them accelerated the heart's vibrations, which became gradually slower, till they ceased altogether.

THE

\* The dog's heart in a natural state, and before the injection of the solution of *opium*, beat 150 in a minute.



THE fibres of some of the intercostal muscles on the right side of the *sternum* continued to be agitated with a weak tremulous motion near half an hour after the injection was made into the *abdomen*; but the intercostal muscles attached to the ribs on the sides of the *thorax* were not observed to move, nor did the diaphragm make any motion when its fibres were pricked or cut.

NOTHING remarkable was seen in the *abdomen*; only, although it was opened ten minutes after making the injection, the intestines had no motion; whereas, in another young dog, which had got no *opium*, Mr Ramfay observed the peristaltic motion continue half an hour after laying open the *thorax*.

THE dog lost little or no blood in making the wound into his *abdomen*, nor were any of his bowels hurt by it.

23. A small dog into whose stomach the late celebrated Dr Mead had forced, at four different times, a solution of two  
drams



## 76 EXPERIMENTS MADE WITH

drams of *opium* in water, lived above an hour and three quarters after getting the first dose. *Vid.* Treatise on poisons, essay 4.

24. IT may not be improper to add here an experiment related by DR ALSTON, in his learned dissertation on *opium* \*. Into the crural vein of an old dog forty-two pounds weight, he caused be injected, at three different times, half an ounce of *opium* dissolved in four ounces of water, filtrated, and of the same warmth with the blood of the animal. The first time, about fifteen drams were thrown in, and very slowly. It had no observable effect. About an hour after, eight drams more were injected slowly, and immediately the dog was seized with strong convulsions: The pulse was frequent and small, and after some time he foamed at the mouth. But there appearing no signs of immediate death, after waiting an hour  
more,

\* Edinburgh Med. Essays, vol. v. p. 1. art. xii.



more, the last nine drams were thrown in quickly; upon which the pulse became full and flow, and in a minute or so the dog expired.

FROM the preceeding experiments, we may, I think, fairly draw the following conclusions.

(a) *Opium* applied to the stomach, guts, cavity of the *abdomen* and *thorax*, and abdominal muscles, soon lessens, and after some time intirely destroys, all feeling and power of motion, not only in the parts to which it is applied, but through the whole body, N<sup>o</sup> 1. 2. 3. 8. 11. & 22.

(b) *Opium* produces these effects much more quickly in animals which are soon killed by want of food and air, than in those which can live long without them, and the parts of whose bodies preserve a power of motion and appearances of life, for a considerable time after they are separated from each other, N<sup>o</sup> 1. 3. &c. compared with N<sup>o</sup> 21. 22. & 23.

(c) SINCE



## 78 EXPERIMENTS MADE WITH

(c) SINCE a solution of *opium* injected into the stomach and guts destroys the sensibility and moving power of frogs, fully as soon when they are deprived of their heart, as when this organ remains untouched ; it follows, that *opium* applied to these parts does not produce its effects by entering the blood, and being by its means conveyed to the brain, as some have imagined, but by its immediate action on the organs and parts which it touches ; N<sup>o</sup> 1. compared with N<sup>o</sup> 2. See also Edinburgh Medical Essays, edit. 3. vol. 5. part 1. p. 140.

(d) SINCE after decollation and the destruction of the spinal marrow, *opium* operates much more slowly in destroying the heart's motion in frogs, than it does when the animals are intire (N<sup>o</sup> 6. compared with N<sup>o</sup> 7.) ; it follows, that it must produce its effects chiefly, if not wholly, by its action on the brain, spinal marrow, and nervous system. The heart of the frog N<sup>o</sup> 7. whose brain and spinal  
marrow



marrow had been destroyed, beat 27 times in a minute, after the animal had lain thirty-six minutes in a solution of *opium*; which was only three pulsations less than the heart of the frog N<sup>o</sup> 5. performed thirty-five minutes after the destruction of its brain and spinal marrow, although it was not exposed to the action of *opium*.

(e) WHEN *opium* injected into the veins, and thus mixed with the blood, lessens or destroys the sensibility and moving power of animals much in the same way as when it is applied to their stomach, guts, or cavity of the *abdomen*, (N<sup>o</sup> 24.); is it not probable, that it produces these effects by its action on the extremities of the nerves which terminate upon the internal surface of the heart and whole vascular system; and perhaps also by affecting immediately the *medulla cerebri* itself? And when a solution of *opium* applied to the bare abdominal muscles of a frog deprived of its brain and spinal marrow,

does



## 80 EXPERIMENTS MADE WITH

does after a long time considerably impair the heart's motion; is it not reasonable to think, that this is owing to the finer parts of the *opium* being absorbed by the bibulous veins and carried to the heart, and thus brought into contact with the nerves of this organ? N° 7. compared with N° 9.

(*f*) SINCE *opium*, without entering the blood, or being carried to the several parts of the body, destroys the power of feeling in animals merely by acting on the nerves to which it is applied (*c*) (*d*), it follows, that the nerves are the instruments of sensation, or at least necessary to it. Nor is it sufficient to destroy this conclusion, that there have been instances of animals endowed with feeling whose brains were so greatly diseased, as to seem incapable of performing their functions. It is far from being safe to build theories in physic upon a few monstrous appearances in nature.

(*g*) It appears from N° 4. and 5. compared



pared with No 3. 6. 8. 10. and 11. that decollation and the destruction of the spinal marrow does not weaken or destroy the heart's motion in frogs, near so soon as *opium* injected into their stomach and guts, or applied to the muscles and bowels of the lower belly and *thorax*.

(*h*) ALTHO' a solution of *opium* applied to the opened *thorax* and *abdomen* of a frog, after decollation and the destruction of its spinal marrow, soon weakens or destroys the motion of the heart; yet it does not produce these effects so speedily as when the brain and spinal marrow are intire, No 11. and 12. In the former case, the *opium* can only affect the heart by its topical influence; in the latter, it not only acts this way, but also exerts its powers upon the brain, spinal marrow, and whole nervous system; and therefore must produce more sudden effects.

(*i*) IT appears beyond doubt, from the preceeding experiments, that the heart is not exempted from the power of *opium*,

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as



## 82 EXPERIMENTS MADE WITH

as the learned Dr Haller has affirmed \*, but has its motion destroyed by it, as well as the other muscles, only not so soon. See No 4. and 5. compared with No 3. 6. 8. & 10. and No 14. 15. & 16. compared with No 17. 18. 19. & 20.

'Tis true, that the fibres of the intercostals on the right side of the *sternum* of the dog No 22. continued to be agitated with a tremulous motion considerably longer than the heart, and when the intercostal muscles attached to the ribs were quite dead. But did not this happen because, after separating the *sternum* from the ribs, and thus cutting off all communication between it and the spinal marrow, the muscles attached to it could be no more affected by the *opium*, which had been injected into the cavity of the *abdomen*; while the heart and other muscles whose communication, by means of the nerves, with the brain and spinal marrow was intire,

\* Aët. Gotting, vol. 2. p. 147. & 154.



tire, continued to be exposed to its action?

(k) As Dr Langrish has observed, that the distilled water of laurel-leaves injected into the cavity of the *abdomen*, kills dogs sooner than when it is taken into the stomach \*; so No 21. and 23. compared with No 22. shew, that *opium* injected into the stomach and great guts of dogs, does not produce either such speedy or powerful effects as when thrown into the cavity of the *abdomen*. And No 6. compared with No 10. shews, that a solution of *opium* applied to the abdominal muscles, does not kill frogs so soon as when all the *viscera* of the lower belly are exposed to its action.

(l) ALTHO' it seems probable, from No 22. compared with No 24. that a solution of *opium* injected into the veins of dogs, does not kill them so soon as when thrown into the cavity of the *abdomen*; yet this cannot be certainly concluded  
since

\* Physical experiments on brutes, p. 64.



## 84 EXPERIMENTS MADE WITH

since the dog of No 24. was much older, and above ten times heavier than the other.

(*m*) It appears, that a solution of *opium* injected into the great guts of a dog, affects the inferior part of the spinal marrow much more remarkably than its superior part, or the brain; since the dogs of No 21. and 22. not only lost the power of motion sooner in their hinder legs than in their fore ones, but also were insensible of any pain in them, and yet howl'd strongly when their ears were pinched.

(*n*) A solution of *opium* injected into the cavity of the *abdomen* or great guts of dogs, does not destroy the feeling and power of motion of their hinder limbs, by sending any *effluvia* to their muscles; otherways it could not produce these effects so instantaneously, (No 21. & 22.). Besides, since *opium* thrown into the stomach and guts of a frog, after being deprived of its heart, destroys the sensibility and moving power of its muscles equally soon

as



as if the animal had been intire (No 2.) ; 'tis plain, that these effects cannot be owing to the finer parts of the *opium* being received into the blood, and by its means carried to the several muscles and organs.

(o) Nor does a solution of *opium* injected into the great guts, or cavity of the *abdomen* in dogs, produce its effects by transmitting through the nerves any subtile *effluvia* to the spinal marrow ; otherwise its operation could not have been so instantaneous, (No 21. and 22.) ; nor could the spinal marrow and its nerves have recovered their functions so soon, after the *opium* was evacuated by a purgative clyster, No 21.

(p) It remains therefore that *opium*, by affecting the extremities of the nerves of the parts to which it is applied, does, by means of their connection and sympathy with the brain and spinal marrow, destroy or prevent through the whole nervous system, the operation of that power upon which depends sensation and motion in the bodies of animals.

(q) SINCE



## 86 EXPERIMENTS MADE WITH

(*q*) SINCE *opium* applied to the abdominal muscles of a frog deprived of its brain and spinal marrow does not destroy the motion of the heart so soon as when it is applied to the abdominal muscles of a frog, whose brain and spinal marrow are intire, (No 6. and 7.), it follows, that the brain and spinal marrow, and consequently the nerves derived from them, have a greater influence than any other part of the animal system upon the motion of the heart.

(*r*) OPIUM does not only destroy the moving power of the muscles of animals, by intercepting the influence of the brain and spinal marrow, but also by unfitting the muscular fibres themselves, or the nervous power lodged in them, for performing its office: Otherways a solution of *opium*, when applied to the abdominal muscles or *viscera* of a frog, would not put a stop to the heart's motion sooner, or indeed so soon, as decollation and the destruction of its spinal marrow, (No 4. and 5. compared with No 8. and 10.).

*Opium*



*Opium* therefore does not produce its effects solely by putting a stop to the function of the brain and spinal marrow; but its influence reaches to the fibres of the muscles themselves, or to the extremities of the nervous filaments which terminate in them.

WHEN I say the influence of *opium* reaches to the nervous filaments which terminate in the muscular fibres, it is not meant, that any *effluvia* or subtile parts of the *opium* are transmitted to them, (See (*n*) and (*o*) above), but that it destroys their powers by means of that sympathy which they have, through the brain or spinal marrow, with the nerves to which the *opium* is immediately applied.

(*f*) FROM the above experiments we may infer, that not only the power of voluntary motion in the muscles, but also their irritability or power of motion when stimulated, proceeds from the nerves, or is at least immediately dependent on their influence; since *opium*, which produces its effects



## 88 EXPERIMENTS MADE WITH

effects solely by affecting the nervous system, (*m*, *n*, and *o*), destroys those powers so suddenly. I know it has been lately argued by a celebrated author, that the irritability of the muscles must be independent of the nerves, because the muscles of animals preserve a power of moving when irritated for some time after the communication between them and the brain, by means of the nerves, is cut off\*. But since a solution of *opium* applied to the abdominal muscles of frogs, merely by its action on the nerves, puts a stop to the irritability or moving power of the heart much sooner than the destruction of the brain and spinal marrow (*g*); is it not reasonable to conclude, that the tremulous motions of irritated muscles, after their nerves are tied, proceed from the integrity of the nervous filaments below the ligature, and the nervous power still remaining in them or in the muscular fibres themselves?

THE

\* Aët. Gotting. vol. 2. p. 134. &c.



THE tying or cutting of a nerve, only prevents the derivation of any new influence from the brain to the parts to which it belongs; but does not immediately destroy the power or influence remaining in the nerve itself. *Opium* applied in sufficient quantity to the sensible parts of animals, not only quickly puts a stop to the function of the brain and spinal marrow, and thus produces in the muscles all the effects of a ligature on their nerves, but also destroys the power of every nervous filament in the body (*r*); and therefore puts a stop to the motion of the heart in frogs sooner than the destruction of the brain and spinal marrow.

(*t*) THE almost instantaneous palsy brought on the hinder legs of a dog, by injecting a solution of *opium* into the cavity of its *abdomen*, (No 22.), and the effects of the same solution injected into the stomach and guts of a frog deprived of its heart, (No 2.), where no part of the *opium* could be conveyed to the muscles,

M

nor



nor be conceived to alter the nature of their *gluten*, shew, that the irritability of the muscles has not its seat in this *glue*, as some have lately imagined \*. But if the motions of irritated muscles be owing to a disagreeable sensation excited in them or their nerves, as we have elsewhere endeavoured to shew †, it is easy to see that *opium* must, by destroying the sensibility of the muscles, of consequence also destroy their irritability.

(u) IN animals which have got a large dose of *opium*, the veins, especially those of the membranes of the brain, are observed to be much swelled; whence it has been thought, that *opium* produces its effects in the bodies of animals, partly at least, by rarefying the blood and compressing the brain: but this distension of the veins seems to be no more than a consequence of the very slow motion of the  
blood

\* Aët. Gotting. vol. 2. p. 152.

† Essay on the vital and other involuntary motions of animals, sect. ix.; and Physiological Essays, p. 188. &c.



blood through the heart, on account of the insensibility with which this organ is affected \*.

(v) SINCE *opium* soon puts a stop to the vital motions of animals, which yet continue in time of sleep with little or no diminution of their vigour; since it often eases pain without bringing on sleep; and since, by its topical action on the heart, it destroys the motion of this organ after all communication between it and the origin of the nerves is cut off †; it follows, that the effects of *opium* are not owing, as some have thought, to its producing sleep: On the contrary, the sleep which it occasions seems to be only a consequence of its impairing the sensibility of the whole nervous system.

THE

\* In frogs, into whose stomach and guts I had injected a solution of *opium*, I not only found the heart's auricle, but also the great veins leading to it, much distended with blood. Vid. Essay on vital motions, &c. p. 371 & 372.

† Vid. No 12. 13. 17. 18. 19. and 20. &c. above.



## 92 EXPERIMENTS MADE WITH

THE other effects of *opium* may be also deduced from the same cause, particularly its restraining all evacuations that are owing to an unusual irritation of the parts of the body, and at the same time promoting those natural secretions which have been diminished or stopt by spasmodic strictures of the vessels, from some uncommon *stimulus* affecting them.

(w) LASTLY, does not *opium* kill animals by rendering their several organs wholly insensible of the *stimuli* which are destined by nature to excite them into action; whence not only a stop is put to the peristaltic motion of the guts, and to the propulsion of the chyle \*, but the fluids

\* In a small dog, which Dr Kauu Boerhaave opened, after having given him three grains of *opium*, he observed scarce any peristaltic motion in the guts: the stomach was much distended; the *pylorus* was shut; and the bread and milk, which the dog had taken with the *opium* about ten hours before, was indigested. There was nothing like chyle in the *duodenum*, nor any lacteal vessels to be seen in the mesentery. The bladder of urine and great guts were



fluids also begin to stagnate first in the smaller and afterwards in the larger vessels \*; while the heart becoming gradually less sensible of the *stimulus* of the blood with which it is distended, contracts more feebly and at greater intervals, till at last it ceases from motion altogether?

AN

were much filled, nor had the animal evacuated either urine or *feces* from the time he swallowed the *opium*; *Impetum faciens Hippocrati dictum*, p. 402. & 403. The learned Dr Haller has also observed, that *opium* puts a stop to the peristaltic motion of the guts in frogs and other animals; Act. Gotting. vol. 2. p. 154.

\* This my worthy colleague Dr Alston observed with a microscope in frogs into whose stomach he had conveyed a few drops of a solution of *opium* in water. *Vid.* Medical Essays, vol. 5. part 1. art. 12. And indeed the great distension of the heart and its auricle in frogs killed with *opium* (No 5. compared with No 3. 6. and 10. above) indicates a more than ordinary resistance to the blood's motion in the arteries, as well as a less degree of irritability in the heart. Further, is not the slow full pulse, and dry parched mouth, in those who have got an over-dose of *opium*, owing partly to the slower motion of the fluids in the small arteries and secretory vessels of the glands? Though it must be confessed, that the dryness of the mouth may be in some measure owing to the perspiration being greatly increased by the *opium*.



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VARIOUS STRENGTH of DIF-  
FERENT LIME-WATERS.

First published

In the Edinburgh ESSAYS PHYSICAL and LITERARY,



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AN  
ESSAY

ON THE

Various Strength of different LIM E-  
WATERS.

**T**H E Reverend and ingenious Dr  
Stephen Hales having informed  
me, in a letter dated May 1751, that he  
had found the strength of lime-water  
much increased, by powering it a second  
time on quick-lime, fresh from the fire;  
I thought it might be worth while to  
make a few experiments, in order to de-  
termine, with some degree of certainty,  
the different strength of different lime-  
waters: From these experiments it appear-  
ed, that lime-water acquired a considera-  
ble addition of strength by being power-  
ed on quick-lime newly taken from the  
N fire;



fire; and that the first water got off quick-lime was sensibly stronger than the fourth and succeeding ones \*.

ON the other hand, my worthy friend and colleague Dr Alston, having observed, several years since, that quick-lime continued to communicate its virtues to water much longer than any one before had imagined, tells us, that he found afterwards, by experiments, that half a dram of stone quick-lime yielded forty ounces of lime-water; and that, after a pound of the same quick-lime had afforded five hundred pounds of lime-water, the water procured from it was as strong of the lime as ever †. Hence he imagines, that as water can only be impregnated to a certain degree by quick-lime, so this will happen equally, whether the quick-lime be fresh from the fire, or has had five hundred

\* Essay on the virtues of lime-water, &c. p. 38. 39.

† Philosoph. Transact. vol. 47. p. 266.; and Dissertation on quick-lime, &c. p. 4. 5. & 6. where the same thing is affirmed of oyster-shell lime.



hundred times its weight of water powdered on it before, provided the water be allowed time enough to extract the virtues of the lime\*. And further affirms, that the strength of lime-water cannot be increased by flacking new-made lime in it, because the water can take up no more of the lime than it had before †.

As these experiments and conclusions appeared inconsistent with what I had advanced, the Doctor has endeavoured further to weaken the credit of my experiments, by some arguments drawn chiefly from the imperfection of the hydrostatical balance, and from the nature of quick lime and its water. In order therefore to know whether I might not have been mistaken in what I had said concerning the strength of different lime-waters, I thought it necessary to make some new experiments; an account of which I beg leave to lay before the society.

I. (a)

\* Differt. on quick lime, p. 11. & 53.

† Ibid. p. 11.



I. (a) HAVING got from my ingenious friend Mr James Gray, a cylindrical copper-vessel ending in a narrow neck, which contained exactly 100 cubical inches; I filled it with the fountain-water of this city, and by means of a very nice balance, found it weighed 25320 Troy grains \*, besides the weight of the vessel itself, which amounted to 13055 grains.

(b) I powdered upon 90 grains of calcined oyster-shells, newly taken from the fire, and reduced to a powder, 96 ounces, or five hundred and twelve times their weight of boiling water. After 92 hours, during which time the infusion was frequently

\* According to Mr Gray's experiments, the water which this vessel contains, only weighs 25318 grains, that is, two grains less than we have made it. This difference may have arisen from our having put a few more drops of water into the vessel than Mr Gray did. But although, in weighing fluids with this vessel, one might err six times more than this, yet it would not affect the point we have in view, which is not to determine with the greatest accuracy the different specific gravities of different lime-waters, but only to show that they are *different*.



## DIFFERENT LIME-WATERS. 101

quently stirred and shaken, I decanted off the clear water, and filtered it through a piece of a very thick linen-cloth doubled; by which means it was rendered free of any crusts, and equally pellucid with fountain-water. With this lime-water I filled the above vessel, and found its weight to be 25356 grains \*.

(c) MONDAY, at seven in the evening, I poured upon a pound of calcined oyster-shells, fresh from the fire, ten times their weight of water. Next morning at ten I decanted off the clear lime-water, and having filtered it, as above, filled the vessel with it; it weighed 25397 grains.

(d) TUESDAY at mid-day, I poured seven pounds of the single lime-water (c) upon one pound of calcined oyster-shells, newly taken from the fire, stirring them well for some time after; at three quarters past six in the evening, I decanted off,

\* The oyster shells made use of in this, and the following experiments, were got from among the rubbish on the south side of the castle of Edinburgh, and were quite free of any sea-salt.



off, and filtered as above, the clear lime-water; and having filled the vessel with it, found its weight to be 25457 grains.

HENCE it appears, that 100 cubical inches of the lime-water (*b*) exceeds in weight that quantity of fountain-water by 36 grains, (*c*) exceeds it by 77 grains, and (*d*) by 137 grains.

THE specific gravity therefore of the weak lime-water (*b*) is to that of fountain-water, nearly as 704 to 703; the specific gravity of the single lime-water (*c*) is to that of common water nearly as 329 to 328; and the double lime-water (*d*) is in specific gravity to water nearly, as 186 to 185.

IT is observable, that the specific gravities of the single and double lime-waters (*c*) and (*d*) are considerably less than the specific gravities of the single and double lime-waters (*a*), and A. and B. mentioned *p.* 39. and 40. of my essay on the virtues of lime-water, &c. But if it be considered, that, in making the latter, a much less proportion of water was added to the quick-



quick-lime, than in making the former; it will appear that this difference of their specific gravities does not infer any thing against the accuracy of the hydrostatic balance; but clearly shews, that the strength of lime-water varies according to the quantity of water poured on the quick-lime.

It may be worth while to observe, that the specific gravities of the lime-waters, (*b*), (*c*), and (*d*) did not differ more than their tastes. The first was weakest and least disagreeable; the second was stronger; and the third still stronger and somewhat pungent. Further, while the double lime-water (*d*) gave, in a few minutes, a copper-colour to silver, the weak lime-water (*b*) produced no sensible change upon it.

II. HAVING formerly found, that lime-water and claret wine, mixed together in a certain proportion, acquired a colour like that of gun-powder\*, I thought, that  
by

\* Essay on the virtues of lime-water, p. 47.



by mixing claret with different lime-waters, one might judge whether they were all equally strong of the lime or not. The result of the experiments was, that one tea-spoonful of claret required four tea-spoonfuls of the lime-water (*b*); two and about one third of (*c*); and one and a half of (*d*), to give it the full gun-powder colour. These experiments, tho' not so accurate as those made with the balance, yet clearly demonstrate a remarkable difference of strength betwixt the above lime-waters.

III. TWENTY grains of salt of tartar being mixed with eight ounces and two drams of the weak lime-water (*b*), after it had stood five days on the lime, the mixture became immediately white and turbid, and soon precipitated a white powder; which, being separated from the water by filtration, and dried, weighed  $2\frac{2}{3}$  gr.

THE same quantity of salt of tartar, mixed with eight ounces and two drams of



## DIFFERENT LIME-WATERS. 105

of double lime-water, that had stood eight days on the lime, became considerably thicker and whiter than the former; and afforded rather more than seven grains of white powder.

THE same quantity of salt of tartar being mixed with eight ounces and two drams of the double lime-water (*d*), which had stood 24 hours on the lime, gave eight grains of a white powder.

IT was observable, that these three lime-waters retained the taste of the lime, after being mixed with the salt of tartar, and this equally after precipitation as before it.

SINCE the earthy powder precipitated by these different lime-waters proceeds wholly, or almost wholly, from the waters, and not from the fixed alkaline salt; \* these experiments shew beyond doubt,  
O that

\* What proves this is, that the calcarious matter, precipitated by mixing salt of tartar with lime-water, is greater or less, in proportion to the strength and quantity of the  
lime-



that double lime-water may contain thrice as much lime, as lime-water made by pouring on quick-lime 512 times its weight of water.

IV. 1. MONDAY 24th December, at eight in the evening, I poured upon a dram of fresh calcined oyster-shells, reduced to a powder, 520 drams of boiling water.

2. AT the same time, I poured upon a pound of the same calcined shells, eight pounds of boiling water.

3. TUESDAY, at eleven before noon, I poured fifty ounces of the lime-water N<sup>o</sup> 2. on nine ounces fresh calcined oyster-shells; and, at eight in the evening, I filtered through brown paper these three waters, and put sixteen ounces of each of them into a basin by itself; and, having placed the basins in a closet, where they might  
be

lime-water; but not in proportion to the quantity of the salt. Thus 12 grains of salt of tartar, mixed with four ounces of strong lime-water, yielded as much of this matter as the same quantity of this lime-water mixed with 18 grains of the salt.



be pretty free from dust, I let them stand 19 days. After this, I filtered the several waters through brown paper, and having collected the earthy crusts, and dried them them well, I found, that No 1. afforded very near four grains, No 2. near 12 grains, and No 3. rather more than 13 grains.

ALTHO' these three lime-waters had, at the time they were filtrated, quite lost their taste; yet, observing that No 2. and 3. became turbid when mixed with salt of tartar, I added eight grains of this salt to twelve ounces of these two waters; and the white powder which was precipitated, when dried, weighed just one grain and a half.

HAVING filtered the lime-waters of No 2. and 3. into the same bottle, before I suspected that any thing of the lime remained in them, it became impossible to know which of them afforded most of the calcarious powder precipitated by the alkaline salt, or whether it did not proceed



ceed wholly from No 3. ; in which case sixteen ounces of it must have contained 17 gr. of the earthy part of the lime, and No 2. only 12 gr.

SINCE No 2. and 3. were not quite free of the lime, although they had stood exposed to the open air 19 days, and had lost above  $\frac{1}{4}$  by evaporation; it follows, that the surest way of knowing the quantity of calcarious earth contained in lime-water, is to evaporate it, as Dr Langrish did \* : And if it be objected to this, that all water affords some earth when evaporated, the quantity of this may be determined by experiment; though in many waters it may well be neglected, on account of its smallness.

It has been argued, that quick-lime must, after many repeated affusions of water, yield as strong lime-water as at first; because, as long as there remains any virtue in the lime, the water will extract it, and continue to do so till it has  
taken

\* Physical Experiments on brutes, p. 11.



taken up as much of the lime as it can bear. But to this we cannot agree: For, though there is undoubtedly a certain degree of strength which lime-water can never exceed; yet, in order to communicate to water this degree of strength, slacked lime may not only be insufficient, but repeated additions of quick-lime may be necessary; unless perhaps a very small proportion of water is poured upon it. Quick-lime, fresh from the fire, yields its virtues more easily than when weakened by long exposition to the air, or by many affusions of water: The water must extract the virtues of the latter, while the former, by a sort of explosive force of its own, quickly impregnates the water. Nor is it to be wondered at, that quick-lime, fresh from the fire, should, at first, impregnate water more strongly with its virtues than it does afterwards. This is as easily conceived, as that boiling water should extract more of the virtues of tea or coffee than cold water. The only difference is,  
that



that the *menstruum* in the latter case acts more powerfully, while in the former the substance to be extracted affords its finer parts more readily, and in greater abundance.

UPON comparing the experiments No I, with those of No III. and IV. it appears, that the difference between the specific gravities of different lime-waters and common water, is much more than the weight of the calcarious matter contained in these lime-waters : There must, therefore, be something else besides this earthy matter which quick-lime communicates to water, by which its weight is increased\*. Perhaps

\* As lime-water, after its earthy part has been precipitated by an alkaline salt, continues to taste strongly of the lime, it follows, that, besides this earth, it contains some more active and subtile part, to which its taste and virtues are chiefly owing : For we know that the calcarious matter of lime-water is perfectly insipid and void of any other virtue than what all absorbent earths possess. This active and more subtile part of lime-water seems to be separated from its earth by the alkaline salt, which strongly attracts and embraces it. And hence lime-water mixed with salt  
of



## DIFFERENT LIME-WATERS. 111

haps quick-lime may also, in some other way unknown to us, alter the specific gravity of water. But whatever may be in this, it is evidently unreasonable to deny, that lime-water is as much specifically heavier than common water, as the hydrostatic balance or other accurate experiments shew, because we cannot account for this excess of gravity from any thing we know of the contents of lime-water. This is no less unphilosophical, than if one was to doubt of universal gravity, because philosophers have hitherto attempted in vain to account for it. If we mistake not the matter much, the contrary has always been the opinion of mankind, *viz.* that every well attested fact is to be believed, although we are ignorant

of tartar does not lose its taste of the lime, by being exposed to the open air. Does not a solution of a fixed alkaline salt in water, poured on quick-lime, separate this subtile active matter of quick-lime from its earthy part, by strongly attracting it? And do not soap-leys consist of water and fixed alkaline salt united with this active part of quick-lime, without any, or almost any, of its earthy part?



ignorant of its cause, or cannot shew the particular way in which it happens.

ENOUGH, it may perhaps be thought more than enough, has been said, to shew, that the strength of lime-water is very different, according to the different quantities of water poured on quick lime. However, I must be allowed to say, that this point, which has been disputed by my good friend, is of that consequence as to deserve to be fully cleared up; since to such as drink lime-water, with a view to the cure of the stone, it is of no small importance to know, how it may be prepared so as to have the surest and speediest effects. And as lime-water, injected into the bladder will undoubtedly dissolve a stone lodged there; it is evident, that, after the bladder has been accustomed to the weaker lime-waters, or to these even softened with a little sweet milk, the dissolution of the stone may be much hastened, by injecting such as are more strongly impregnated with the virtues of the lime.

WITH



WITH regard to the lithontriptic powers of oyster-shell and stone lime-water, I shall only say, that, as in a variety of experiments made during the course of ten years, I had always observed the superior efficacy of the oyster-lime water, I thought it to no purpose to make a new trial: Any one who doubts on which side the truth is, may easily satisfy himself. But, in making the experiment, the *calculi* should either be immersed in a large quantity of lime-water, or else it should be renewed upon them every three or four days.







A  
L E T T E R  
T O T H E

REV. THOMAS BIRCH, D.D. Secret. R. S.  
from JOHN PRINGLE, M. D. F. R. S.  
inclosing two papers communicated to  
him by ROBERT WHYTT, M. D. F. R. S.

*Pallmall-Court, St James's, Dec. 10. 1757.*

SIR \*,

ABOUT three weeks ago I put into  
your hands an extract of a letter I  
had then received from Dr Whytt, con-  
taining a postscript to his observations on  
Lord Walpole's case, and slightly men-  
tioning some doubts he had then about  
the justness of Dr Springsfeld's experi-  
ments

\* First published in the Phil. Transf. and read Decem-  
ber 15. 1757.



ments with lime-water, from some trials he himself had made, upon reading that gentleman's curious treatise on the extraordinary lithontriptic quality of the waters at Carlsbad in Bohemia. Within these few days, Dr Whytt having favoured me with a full account of those experiments, I have herewith sent you his paper, in order, if you please, to lay it before the society; which the author desires may be done, in case these observations should be judged useful.

THE other paper inclosed was sent me by the same hand, to be likewise presented to the society, as a well-attested instance of the electrical power in the cure of a palsy. To the other testimonies I have subjoined what Dr Whytt says in his letter to me, by way of strengthening the evidence. I shall only add, that since Mr Brydone, the author of this account, has omitted telling how long the patient has continued in perfect health since the operation, it appears she must have been well



well for some months before the date of his paper; because, before the end of last summer, Dr Whytt transmitted the same case to me, which I then returned, in order to have it drawn up in a fuller manner, and with other vouchers besides the gentleman who performed the cure. The Doctor has been so good as to comply with my request, having procured a more ample account of the circumstances from Mr Brydone, and the attestation of two ministers, besides that of the patient herself\*. My difficulties being thus removed,

\* After this paper was read at the society, Dr Pringle having acquainted Dr Whytt, that Mr Patrick Brydone had omitted, in his account, the name of the parish where the woman lived, the time when she was cured, and also that he had not fully dated his paper; Dr Whytt some time after wrote to Dr Pringle, that having desired Mr Brydon to furnish him with these particulars, he had received for answer, “ That the woman, on  
 “ whom the cure was performed, had lived all her life  
 “ in the parish of Coldingham, and for the last twelve  
 “ years in that town: That her father had died of the  
 “ palsy seven years ago, after having been subject to  
 “ that



ved, I believe I may now with freedom offer this very curious case to the attention of the society. I am,

S I R,

Your most obedient humble servant,

JOHN PRINGLE.

“ that distemper for several years: That the cure was  
“ performed in his father’s house as Coldingham, on  
“ the 4th, 5th, 6th, and 11th days of April 1757, a  
“ circumstance he had noted down: That as to the date  
“ of his paper presented to the Royal society, he only  
“ recollects it was written some day in the beginning of  
“ November last: But as the woman still continued well,  
“ he hoped the precise day of the month was no material omission.” This letter to Dr Whytt is dated, Coldingham, January 9. 1758.

POST.



P O S T S C R I P T

T O

Dr WHYTT's observations on Lord WAL-  
POLE'S CASE\*.

“ I Do not know, if it be worth while to  
“ observe, that lately, in making some  
“ experiments with different *calculi*, there  
“ was one almost as white as chalk, but  
“ of a less hard substance than the others ;  
“ and which was not in the least degree  
“ dissolved or softened by being infused  
“ twenty days in oyster-shell lime-water,  
“ but yielded somewhat to a solution of  
“ Spanish soap in common water.

“ FROM this experiment one may con-  
“ clude, that it is better to prescribe both  
“ soap

\* Read December 8. 1757.



“ soap and lime-water for the stone, than  
 “ any one of them alone; and that, if  
 “ one of these remedies has failed of gi-  
 “ ving relief, the other ought to be tried:  
 “ for as the above white *calculus*, which  
 “ yielded a little to the solution of soap,  
 “ resisted lime-water; so there may  
 “ perhaps be others that are readily dis-  
 “ solved by lime-water, but little affected  
 “ by soap.

“ DR SPRINGSFELD'S experiments  
 “ with lime-water are somehow not just;  
 “ for in several *calculi* I have found the  
 “ dissolving power of oyster-shell lime-wa-  
 “ ter above eight times greater than he  
 “ makes it.”

SOME



S O M E

O B S E R V A T I O N S

On the Lithontriptic Virtue of the Carlshad waters, lime-water, and soap: In a letter to Dr JOHN PRINGLE, F. R. S. from Dr ROBERT WHYTT, F. R. S. and Professor of medicine in the university of Edinburgh.

S I R \*,

FROM the experiments related in Dr Springsfeld's *Commentatio de prerogativa thermarum Carolinarum, &c.* which you were so good as to send me some time ago, it appears, that these waters are not only possessed of a very extraordinary power of dissolving the stone, but that in

Q this

\* Read, December 15. 1757.



this respect they greatly exceed lime-water.

(A) THUS, Dr Springsfeld having infused, for 14 days, in a heat of 96 degrees of Fahrenheit's scale, three pieces of the same *calculus*, each weighing 30 grains, in eggshell-lime-water, the Carlsbad water, and in the urine of one who daily drank this last water, renewing these several menstruums every day, he found, on the 15th day, that the *calculus* in the lime-water had lost one grain, the *calculus* in the Carlsbad water six grains, and that in urine five grains.

(B) AGAIN, having divided another *calculus* into four parts, each of which was reduced to 80 grains, he put the first in oyster-shell lime-water, the second in Carlsbad water, and the third in the urine of a person who drank this water. After 20 days, during which time the menstruums were renewed every day, and kept in a heat of 96 degrees, the dried *calculi* had



had lost of their weight as follows: The first 3 grains, the second 18 grains, and the third 14 grains.

ALTHOUGH I make no doubt that Dr Springsfeld, who appears to be a man of candour, as well as learning, has faithfully related the event of the experiments which he made; yet either the lime-water he used must have been very weak, or some other mistake must have happened in his experiments: For in all the numerous trials I made, about fifteen years ago, of lime-water as a solvent for the stone, I always found its dissolving power much greater than it appears in Dr Springsfeld's experiments. And as in these trials different urinary stones were used, it can scarcely be imagined, that it was owing to the peculiar hardness of Dr Springsfeld's *calculi*, that the lime water made so little impression on them. However, to be still further satisfied of this matter, I made the following experiments.



1. I put a piece of a very hard *calculus*, which I shall call *x*, weighing 80 grains, in oyster-shell lime-water, renewing the lime-water every day, and keeping it in a heat between 90 and 106 degrees of Fahrenheit's scale. After 20 days, I took out the *calculus*; and having set it by for some days, till it was become quite dry, I brushed away all the rotten part of it, which was reduced to a kind of chalky powder, and found that the undissolved part of it weighed 57 grains.

2. AT the same time a piece of another *calculus*, *z*, weighing 15 grains, was, after a like infusion of 20 days in oyster-shell lime-water, reduced to 10 grains.

3. I put a piece of *z*, weighing 14 grains, in a solution of half an ounce of the internal part of Spanish soap in nine ounces of water, and every third day renewed the solution, which was kept in a heat of about 60 degrees. After 14 days, I found the undissolved part not to exceed 11 grains.

4. A



4. A piece of white chalky *calculus*, *y*, weighing 30 grains, had near 4 grains of its substance dissolved, by being 14 days infused as above in a solution of soap.

FROM N<sup>o</sup> 1. above, compared with Dr Springsfeld's exper. (B), it appears, that the dissolving power of oyster-shell lime-water is to that of the Carlsbad water as 23 to 18, supposing the *calculi* used in these experiments to have been equally easy to dissolve.

N<sup>o</sup> 3. compared with Dr Springsfeld's exper. (A), shews, that the dissolving power of a solution of the inner part of Spanish soap, in a heat of 60 degrees, is to that of the Carlsbad water, in a heat of 96 degrees, as 15 to 14.

FROM N<sup>o</sup> 4. compared with (A), the dissolving power of soap is to that of the Carlsbad water only as 4 to 6; but it is probable, that had the solution of soap been kept in a heat of 96 degrees, its dissolving power would, even in this experiment,



ment, have nearly equalled that of the Carlsbad water. It may, perhaps, be worth while to observe, that a piece of the white chalky *calculus* of N<sup>o</sup> 4. was not in the smallest degree dissolved by lying in lime-water 20 days.

5. IN exper. 19. of my Essay on the virtue of lime-water, a piece of *calculus*, *b*, weighing 31 grains, lost 7 grains by being infused 36 hours, in a heat of above 100 degrees, in very strong oyster-shell lime-water. And in the same water, of a moderate strength, another piece of *b* lost, in the same time, 5 grains.

IN this last experiment, the lithontriptic virtue of lime-water appears to be stronger than in N<sup>o</sup> 1. and 2. above; and greatly exceeds that of the Carlsbad water in Dr Springsfeld's exper. (A) and (B).

BUT although, from what has been said, it appears, not only that lime-water, but also a solution of soap, dissolves the  
stone



stone in close vessels as fast, nay faster than the *thermæ Carolinæ*; yet these last waters, when the *calculi* were so placed in open vessels, that the water from the fountain might constantly flow along them, effected a much quicker dissolution than lime-water or even soap-ley, or indeed any known *menstruum*, except, perhaps, strong spirit of nitre: For, in the first experiment made by Dr Springsfeld, a *calculus* of two ounces and a half was, in this manner, quite dissolved in six days. From this experiment, compared with that of Dr Springsfeld mentioned above (B), it will be found, upon calculation, that the dissolving power of the Carlsbad water, when it is allowed to flow constantly from the fountain along the stone, is nearly 39 times greater than when it is only poured fresh on the *calculus* once a-day\*. What may have been the reason of this surprising difference of the lithontriptic power of the Carlsbad water in these different

\* Vid. Essay on the virtues of lime-water, edit. 2. p. 176. 177.



different circumstances, I will not pretend to say. I think it can scarcely be accounted for from the gentle motion of the water along the surface of the *calculus*. Was it then owing to some very volatile active part, which the water quickly loses, after being taken from the fountain?

BUT how great soever the dissolving power of the Carlsbad waters may be, when they issue from the bowels of the earth; yet that they do not communicate a much greater dissolving power to the urine than lime-water, will appear from comparing the two following experiments.

IN Dr Springsfeld's exper. (A) above, the urine of a person who drank the Carlsbad waters, reduced, in 14 days, a piece of *calculus*, weighing 30 grains, to 25 grains. And in an experiment made by Dr Newcome, now Lord Bishop of Llandaffe, who drank four English pints of oyster-shell lime-water daily, his Lordship's urine reduced, in four months, a  
piece



piece of *calculus*, weighing 31 grains, to three small bits, weighing in all six grains \*. Whence it follows, that the dissolving power of his Lordship's urine must have been to the dissolving power of the urine of the person who drank the Carlsbad waters nearly as 35 to 65 †. But if we consider, that the *calculus* infused in the urine of the person who drank the Carlsbad waters was kept always in a heat of 96 degrees, while in Dr Newcome's experiment, which was made during part of the autumn and winter, no artificial heat was used, it will appear probable, that the dissolving power of his Lordship's urine was little inferior to that of the person who drank the Carlsbad waters; for lime-water, in a heat of 96 degrees, dissolves the *calculus* at least twice as fast as in the common heat of the air in winter. Further, if it be attended to, that the quantity of Carlsbad waters drank e-

R

very

\* Essay on lime-water, edit. 2. p. 208. &c.

† Ibid. p. 176. & 177.



very day before dinner is from fix to eight lib. while his Lordship only drank four lib. of lime-water in 24 hours, it will follow, that whatever the different dissolving powers of the lime-water and Carlsbad waters may be out of the body, yet the former seems, in proportion to the quantity drank, to communicate at least an equal dissolving power to the urine.

BUT without presuming to decide certainly as to the comparative virtue of the Carlsbad waters and lime-water, I shall conclude with observing, that though the Carlsbad waters are less disagreeable to the taste, and may be drank in larger quantity than lime-water; yet this last may be drank equally good in all places, and at all seasons of the year; which is not the case with the Carlsbad waters.

November 30. 1757.

A N



AN  
I N S T A N C E  
OF THE  
ELECTRICAL VIRTUE in the cure of a  
PALSY.

By Mr PATRICK BRYDONE\*.

ELIZABETH FOSTER, aged 33, in poor circumstances, unmarried, about 15 years ago, was seized with a violent nervous fever, accompanied with an *asthma*, and was so ill, that her life was despaired of. She recovered, however, from the violence of her distemper; but the sad effects of it remained. For, from this time, she continued in a weakly uncertain state of health, till the month of July

\* Read December 15. 1757.



July 1755, when she was again taken ill of the same kind of fever; and after it went off, she was troubled with worse nervous symptoms than ever, ending at last in a paralytic disorder, which sometimes affected the arm, sometimes the leg, of the left side, in such a manner as that these parts, though deprived of all motion for the time, yet still retained their sensibility. In this condition she remained till the spring 1756, when unexpectedly she grew much better; but not so far as to get quite rid of her paralytic complaints, which, in cold weather, seldom failed to manifest themselves by a numbness, trembling, sensation of cold, and a loss of motion in the left side.

THIS paralytic tendency made her apprehensive of a more violent attack; which accordingly soon happened: For, about the end of August, in the same year, her symptoms gradually increased, and, in a very short time, she lost all motion and sensation in her left side. In  
this



this state she continued throughout last winter, with the addition of some new complaints; for now her head shook constantly; her tongue faltered so much, when she attempted to speak, that she could not articulate a word; her left eye grew so dim that she could not distinguish colours with it; and she was often seized with such an universal coldness and insensibility, that those who saw her at such times scarce knew whether she was dead or alive.

WHILST the woman was in this miserable condition, observing that she had some intermissions, during which she could converse and use her right leg and arm, in one of these intervals I proposed trying to relieve her by the power of electricity. With this view, I got her supported in such a manner as to receive the shocks standing, holding the phial in her right hand, whilst the left was made to touch the gun-barrel. After receiving several very severe shocks, she found herself



self in better spirits than usual ; said she felt a heat, and a prickling pain in her left thigh and leg, which gradually spread over all that side ; and after undergoing the operation for a few minutes longer, she cried out, with great joy, that she felt her foot on the ground.

THE electrical machine producing such extraordinary effects, the action was continued ; and that day the woman patiently submitted to receive above 200 shocks from it. The consequence was, that the shaking of her head gradually decreased, till it entirely ceased ; that she was able at last to stand without any support ; and on leaving the room, quite forgot one of her crutches, and walked to the kitchen with very little assistance from the other. That night she continued to be well, and slept better than she had done for several months before, only about midnight she was seized with a faintishness, and took notice of a strong sulphureous taste in her mouth ; but both faintness and that taste  
went



went off, upon drinking a little water. Next day, being electrified as before, her strength sensibly increased during the operation, and when that was over, she walked easily with a stick, and could lift several pounds weight with her left hand, which had been so long paralytic before. The experiment was repeated on the third day; by which time she had received in all upwards of 600 severe shocks. She then telling us that she had as much power in the side that had been affected as in the other, we believed it unnecessary to proceed farther, as the electricity had already, to all appearance, produced a complete cure. And indeed the patient continued to be well till the Sunday following, *viz.* about three days after the last operation; but upon going that day to church, she probably caught cold; for upon Monday she complained of a numbness in her left hand and foot; but, upon being again electrified, every symptom



tom vanished, and she has been perfectly well ever since.

Coldingham,

Nov. 1757.

PATRICK BRYDONE.

THAT the above is a true and exact account of my case, and of the late wonderful cure wrought on me, is attested by

ELIZABETH FOSTER.

I was eye-witness to the electrical experiments made by my son on Elizabeth Foster, and saw with pleasure their happy effects. By the blessing of God accompanying them, from a weak, miserable, and at sometimes almost an insensible state, she was, in a very short time, restored to health and strength; of which the above is, in every respect, a true account.

ROBERT BRYDONE,

Minister of Coldingham.

EXTRACT



E X T R A C T

O F A

L E T T E R

F R O M

Dr WHYTT to Dr PRINGLE, relating to  
this account: Dated *Edinburgh*, 1st  
*December 1757.*

SOME days ago, I had transmitted to  
me Mr Brydone's account (inclosed)  
of the success of the electrical shocks in a  
paralytic patient, attested by the patient  
herself, and by Mr Brydone's father, who  
is minister at Coldingham, in the shire of  
Berwick. At the same time I had a let-  
ter from the Reverend Mr Allan, minister  
of Eyemouth, (in the neighbourhood), in-  
forming me, that he had examined the  
S patient



patient particularly, and found Mr Brydone's account to be perfectly true. He further informs me, that he never observed the electrical shock so strong from any machine as from Mr Brydone's. It seems, that gentleman has not only applied himself to the study of natural philosophy, but also of medicine.

ROBERT WHYTT.

CASES



C A S E S

OF THE

REMARKABLE EFFECTS  
of BLISTERS,

I N

Lessening the Quickness of the Pulse in  
Coughs attended with Infarction of the  
Lungs and Fever.

First Published

In the PHILOSOPHICAL TRANSACTIONS, Vol. 50. Part 2.







C A S E S

O F T H E

REMARKABLE EFFECTS  
of BLISTERS,

I N

Lessening the Quickness of the Pulse, in  
Coughs attended with Infarction of the  
Lungs and Fever \*.

**O**NE of the most natural effects of  
blistering plaisters, when applied to  
the human body, is to quicken the pulse,  
and increase the force of the circulation.  
This effect they produce, not only by  
means of the pain and inflammation they  
raise in the parts to which they are ap-  
plied, but also because the finer particles  
of

\* Read February 16. 1758.



of the *cantharides*, which enter the blood, render it more apt to stimulate the heart and vascular system.

THE apprehension, that blisters must in every case accelerate the motion of the blood, seems to have been the reason why some eminent physicians have been unwilling to use them in feverish and inflammatory disorders, till after the force of the disease was a good deal abated, and the pulse beginning to sink. However, an attentive observation of the effects which follow the application of blisters in those diseases, will shew, that instead of increasing, they often remarkably lessen the frequency of the pulse. This I had occasion formerly to take notice of\*, and shall now evince more fully by the following cases.

I. A widow lady, aged about fifty, was seized (December 1755) with a bad cough, oppression about her stomach and breast, and a pain in her right side, tho'

not

\* *Physiological essays*, p. 69.



not very acute. Her pulse being quick, and skin hot, some blood was taken away, which was a good deal fizy: Attenuating and expectorating medicines were also prescribed. But as her complaints did not yield to these remedies, I was called on December 26th, after she had been ill about ten days; at which time her pulse beat from 96 to 100 times in a minute, but was not fuller than natural. I ordered her to lose seven or eight ounces more of blood, which, like the former, was fizy; and next day, finding no abatement of her complaints, I advised a blister to be applied, in the evening, to that part of her right side which was pained. Next morning, when the blister was removed, the pain of her side was gone, and her pulse beat only 88 times in a minute, and in two days more it came down to 78. However, after the blistered part became dry, the pulse rose in in one day's time to 96, and continued between that number and 90 for four days;



days ; after which I ordered a large blister to be put between her shoulders. When this plaister was taken off, her pulse beat under 90 times in a minute ; and next day it fell to 76, and the day after to 72. The cough and other symptoms, which were relieved by the first blister, were quite cured by the second .

II. JOHN GRAHAM bookbinder in Edinburgh, aged thirty-seven, of a thin habit of body, formerly subject to coughs, and thought to be in danger of a *phthisis pulmonalis*, having exposed himself unwarily to cold in the night-time, was, about the end of January 1756, seized with a bad cough and feverishness ; for which he was blooded, and had a diaphoretic julep, a pectoral decoction, and a mixture with *gum. ammoniacum* and *acetum scilliticum*, given him by Mr James Ruffel, surgeon-apothecary in this place. On the 12th of February, after he had been ill above a fortnight, I was desired to visit him.



him. He seemed to be a good deal emaciated; his eyes were hollow, and cheeks fallen in: He was almost constantly in a sweat; coughed frequently, and spit up a great quantity of tough phlegm, somewhat resembling *pus*: His pulse beat from 112 to 116 times in a minute. In this condition I ordered immediately a blister to be applied between his shoulders, which lessened in some degree his cough and spitting, as well as the frequency of his pulse; but the blistered part no sooner began to heal, than he became as ill as before, and continued in this bad way nine or ten days, gradually wasting with continued sweats, and a great spitting of a thick *mucus*. During this time he used *tinctura rosarum*, and the mixture with *gum. ammon.* and *acet. scillit.* without any sensible benefit, and had six ounces of blood taken away, which was very watery, and the *crassamentum* was of a lax texture. In this almost desperate condition, another blister,

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larger



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larger than the former, was put between his shoulders, which remarkably lessened his cough and spitting, and in two or three days reduced his pulse to 96 strokes in a minute. After this he continued to recover slowly, without the assistance of any other medicine, except the *tinctura rosarum*, and the mixture with *gum. ammon.* and *acet. scillit.* and at present he enjoys good health.

III. MRS. —, aged upwards of forty, who had for several years been subject to a cough and spitting in the winter-months, was, in October 1756, seized with those complaints in a much greater degree than usual; to remove which she was blooded, and got some attenuating and pectoral medicines from Mr John Balfour, surgeon apothecary in Leith. I was called on November 11th, after she had been ill several weeks, and found her in a very unpromising condition. She had a frequent and severe cough, with  
great



great shortness of breath and wheezing; her lungs seemed to be quite stuffed with phlegm, of which she spit a vast quantity every day, and of such an appearance, that I was apprehensive it was, in part at least, truly purulent. When she sat up in a chair, her pulse beat above 130 times in a minute. She had a considerable thirst, and her tongue was of a deep red colour, with a beginning apthous crust on some parts of it. She was so weak, and her pulse so feeble, that there was no place for farther bleeding: A blister was therefore applied to her back, November 11th, which somewhat lowered her pulse, and lessened the shortness of breathing and quantity of phlegm in her lungs. November 16th, a second blister was laid to her side, which gave her still more sensible relief than the former, and reduced her pulse to 114 strokes in a minute. November 25th, a third blister was applied to her back, by which her cough and wheezing were rendered considerably



siderably easier, and the phlegm which she spit up, lost its purulent appearance, became thinner, more frothy, and was much less in quantity. Her pulse beat now only 104 times in a minute. After this her cough and spitting increasing again, she had, on the 20th of December, a fourth blister applied to her back, which, like the former, did her great service. Her stomach being extremely delicate, I scarce ordered any medicines for her all this time, except a cordial julep, with *spir. volat. oleos.* tincture of rhubarb as a laxative, and a julep of *aq. rosar. acet. vin. alb.* and *syr. balsam.* of which last she took two table-spoonfuls twice or thrice a-day in a quarter of a pint of lint-feed tea. After the fourth blister, she drank for some time a cup-full of *infusum amarum* twice a-day, and continued to recover slowly: And though during the remaining part of the winter she was, as usually, a good deal troubled with a cough, yet



yet in the spring she got free from it, and is now in her ordinary health.

IV. CHRISTIAN M'EWEN, aged twenty-one, had laboured under a cough, thick spitting, pain of her breast, and pains in her sides affecting her breathing, for about a twelve-month: And after getting, by proper remedies, in a good measure free from those complaints, her cough, from catching a fresh cold, increased to a greater degree than ever, became hard and dry, and was attended with a constant difficulty of breathing, pain in her left side, and headach. After having been seven or eight days in this condition, she was admitted into the Royal Infirmary, January 9th, 1757. As her pulse was small, though very quick, *viz.* beating 130 times in a minute, I thought it unnecessary to bleed her, as from former experience I did not doubt but that blistering alone would relieve her. I ordered, therefore, a large blister to be applied to her  
left



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left side, where she complained of pain, and prescribed for her the following julep :

R. Aq menth simp. spirit. Minderer. ana unc. iij.  
acet. scillit. unc. i. sacchar. alb. unc. ij. Misce; cap.  
coch. ij. ter. in die.

She was also desired to breathe frequently over the steam of hot water, and to drink lintseed-tea.

JANUARY 10th. Her pulse beat only 112 times in a minute, and was somewhat fuller than on the 9th. The blister was not removed till late in the evening, and made a plentiful discharge. The cough having been so severe last night as to keep her from sleep, I ordered her the following anodyne draught.

R. Spirit. Minderer. unc. ss. acet. scillit. drach. i. fyr.  
papav. alb. drach. vi. Misce; cap. hor. somni.

JAN. 11th. The cough easier last night; difficulty of breathing less; pulse 108 in a minute. Ordered the anodyne draught

to



to be repeated, and the use of the julep, with *acet. scillit.* to be continued.

JAN. 12th. Pulse slower; cough and pain of the side easier; but still complains of a headach.

JAN. 13th. Pulse 94 in a minute; cough continues easier in the night, but is troublesome in the day-time.

JAN. 14th. Every way better; pulse only 80 in a minute. As her cough is still bound, ordered her, besides the medicines above mentioned, a pectoral decoction of *rad. alth. &c.*

JAN. 15th. Cough and other complaints in a great measure removed: Pulse 65 in a minute.

FROM this time her cough gave her little trouble; but on the 18th she complained of a pain in the *epigastrium*, with sickness at stomach, want of appetite, and a giddiness in her head, which were considerably relieved by a vomit, *infusum amarum*, and stomachic purges; and were almost wholly cured by the return of  
her



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her *menfes* on the 5th of February, after an interval of eight weeks.

V. A girl of twenty-one months old, who had (December 1756) a great load of the small-pox, and not of a good kind, with a cough and obstructed breathing, was, on the seventh day from the eruption, blistered on the back; by which the pulse was lessened from 200 to 156 strokes in a minute. Next day her legs were also blistered, and the pulse thereby fell to 136. But the child's lungs being much oppressed, and her throat being so full of pustules that she could scarce swallow any thing, she died towards the end of the ninth day.

I could add several other cases of the remarkable effects of blisters in lessening the quickness of the pulse in coughs attended with fever, pain in the side, and pituitous infarction of the lungs: But those above may be sufficient to put



put this matter out of doubt, as well as to remove any prejudice that may still remain against the free use of so efficacious a remedy.

IN a true peripneumony, especially where the inflammation is great, repeated bleeding is the principal remedy, and blisters early applied are not so proper. But when the peripneumony is of a mixed kind; when the lungs are not so much inflamed as loaded with a pituitous matter; when bleeding gives but little relief; when the pulse, though quick, is small; when the patient is little able to bear evacuations, and the disease has continued for a considerable time: In all these cases blistering will produce remarkable good effects, and, far from increasing, will generally lessen the frequency of the pulse, and fever, more speedily than any other remedy.

ON the other hand, when the fever and frequency of the pulse proceed from a true inflammation of the lungs, from  
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large obstructions tending to suppuration, or from an open ulcer in them, blisters will be of less use, nay, sometimes will do harm, except in the last case, where they, as well as issues and setons, are often beneficial, though seldom able to compleat a cure. But as in pituitous infarctions of the lungs, with cough and fever, repeated blisters applied to the back and sides are far preferable to issues or setons, so these last seem most proper in an open ulcer of the lungs. The former make a greater and more sudden derivation, and are therefore adapted to acute cases; the latter act more slowly, but for a much longer time, and are therefore best suited to chronic diseases. Further, while blisters evacuate chiefly the ferous humours, issues and setons generally discharge true purulent matter, and on this account may be of greatest service in internal ulcers.

IN what manner blisters may lessen the fever and frequency of the pulse attending internal inflammations, I have elsewhere endeavoured



endeavoured to explain (\*); and shall only add here, that in the cases above recited, where the quick pulse and feverishness proceeded more from a pituitous infarction than a true inflammation of the lungs, blisters, by relieving this organ, in some measure, of the load of humours oppressing it, would render the circulation through its vessels freer, and consequently lessen the quickness of the pulse and other feverish symptoms.

It may not, however, be improper briefly to point out the reason why blisters, which have been observed to be remarkably efficacious, even when early applied, in pleurifies †, are less so in true peripneumonies. This difference, I imagine, may be accounted for from there being no immediate communication between the pulmonary vessels and those of the sides and back, to which the blisters  
are

\* *Physiological Essays*, p. 69.

† *Dr Pringle's Observations on the diseases of the army*, part 3, chap. 2.



are applied ; whereas the *pleura* and intercostal muscles are furnished with blood-vessels from the intercostal arteries, which also supply the teguments of the *thorax* : So that while a greater flow of serous humours, and also indeed of red blood, is derived into the vessels of the external parts, to which the vesicatories are applied, the force of the fluids in the vessels of the inflamed *pleura*, or intercostal muscles, must be considerably lessened. Further, as the intercostal muscles and *pleura* are, as well as the teguments of the *thorax*, supplied with nerves from the true intercostals, blisters applied to the back and sides may perhaps, on this account, also have a greater effect in relieving inflammations there than in the lungs, which have nerves from the eighth pair, and from the intercostals improperly so called.

Edinburgh, May 23.

1757.



E X T R A C T

O F A

L E T T E R

F R O M

Dr WHYTT, Professor of Medicine in  
the University of Edinburgh, and  
F. R. S. to Dr PRINGLE, F. R. S.

Edinburgh, 10th Nov. 1757.

WHAT you remark with regard to  
blifters being freely used by the phy-  
sicians at London, in the cases mentioned  
in the paper I last sent you, is very just,  
and indeed what I knew; but although  
their efficacy in such circumstances is  
now generally acknowledged both in  
England and Scotland, yet I do not re-  
member that their remarkable quality in  
lessening

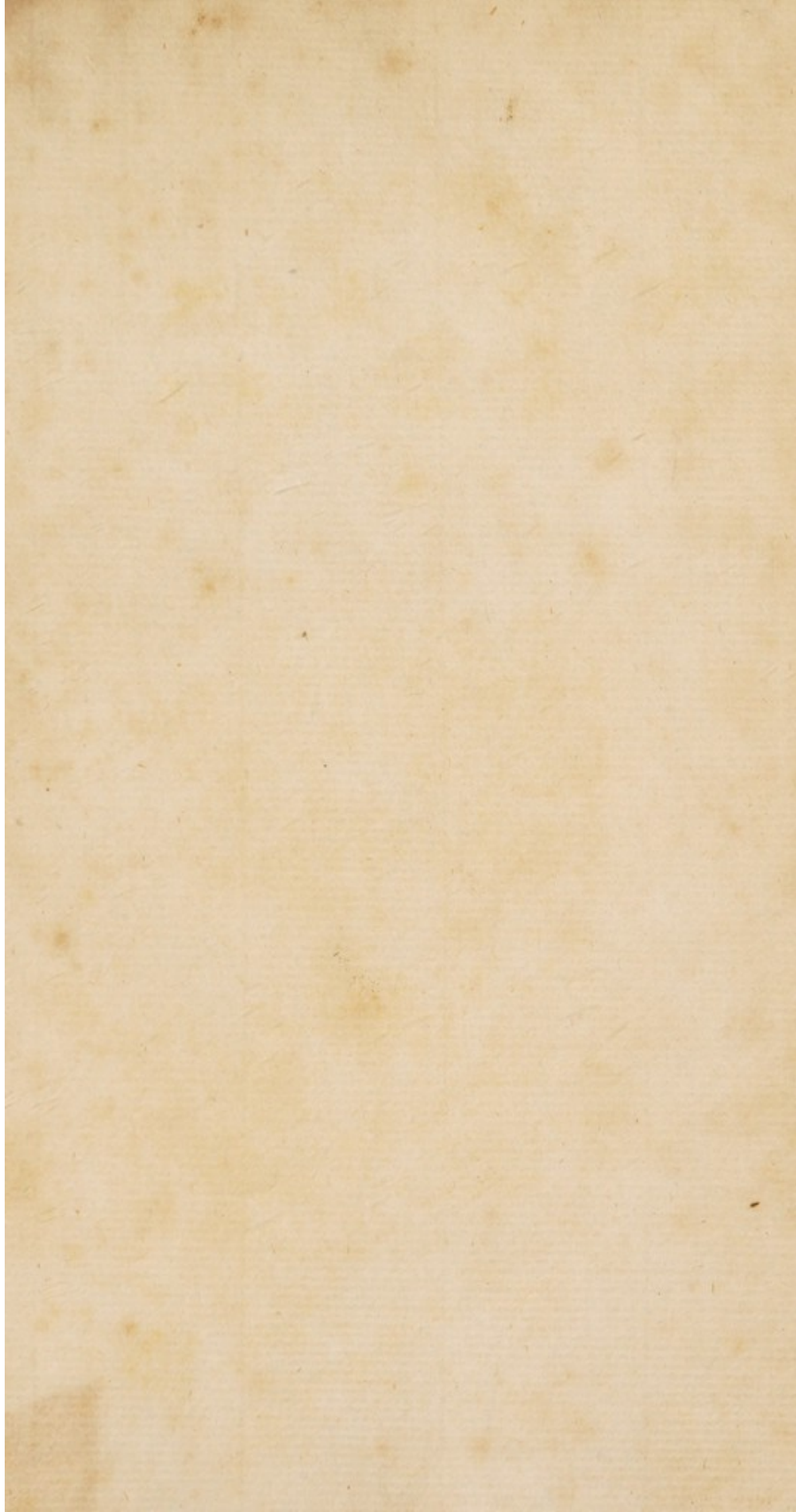


lessening the quickness of the pulse has been particularly attended to. This, therefore, I thought it might not be amiss to ascertain by a few careful observations.

I agree intirely with you, as to the use of blisters in fevers; being of opinion, that when there is no particular part obstructed or inflamed, they are of little service, and sometimes hurtful, unless perhaps towards the end, when the pulse begins to sink. Nay, in fevers, where the substance of the brain is affected, and not its membranes, I have never found any sensible benefit from blisters: And I always suspect the brain itself affected, when a fever and *delirium* come on without any preceeding headach, or redness in the *tunica albuginea* of the eyes. This kind of fever I have met with several times, and have observed it to be generally fatal.

T H E E N D.







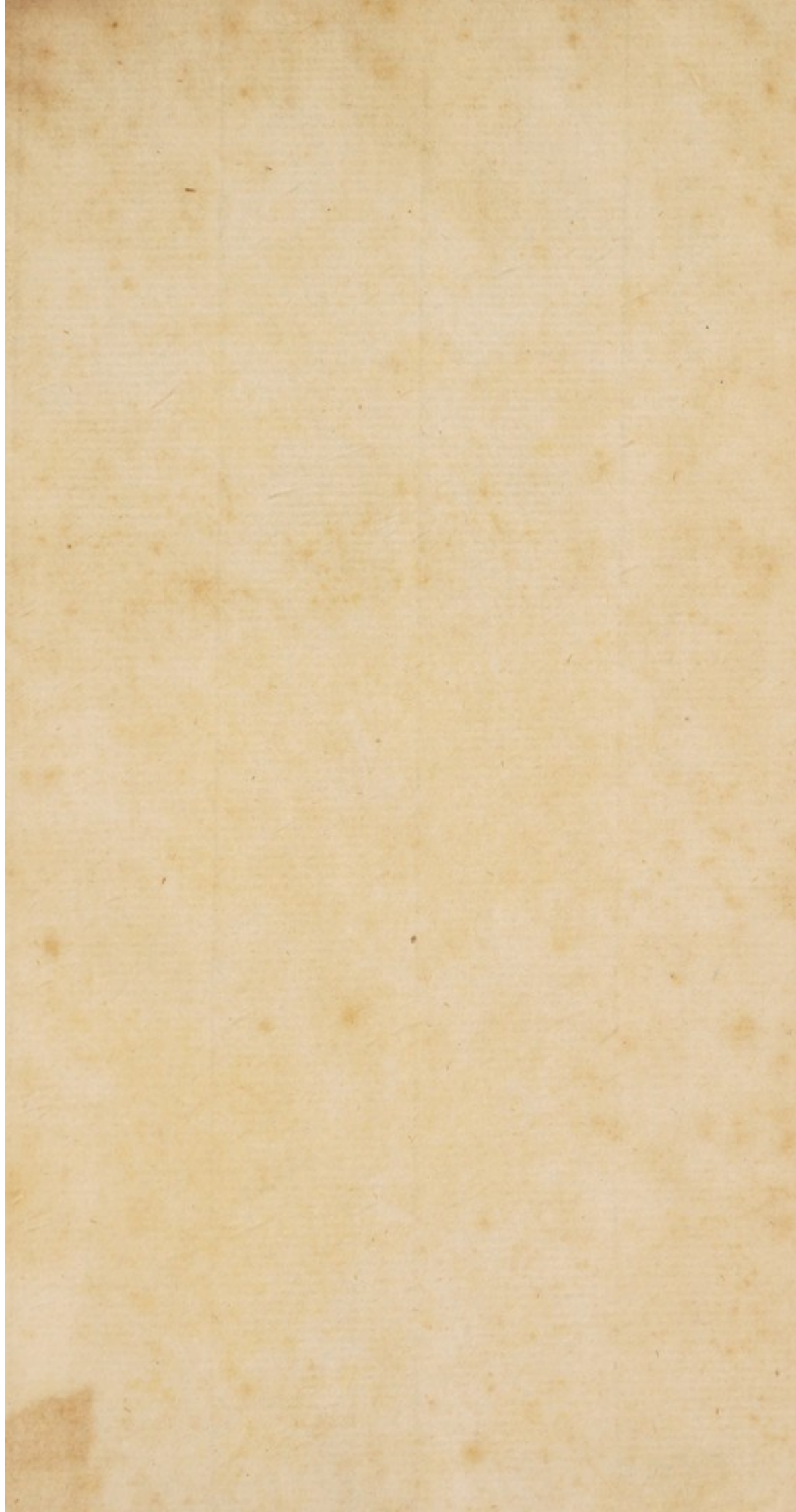
THE UNIVERSITY OF CHICAGO

PHILOSOPHY DEPARTMENT  
1155 EAST 58TH STREET  
CHICAGO, ILLINOIS 60637  
U.S.A.

Dear Sir,  
I have the pleasure to inform you that your application for admission to the Philosophy Department has been accepted. You will receive a letter from the Registrar's Office regarding the admission process and the required documents. Please contact the Registrar's Office at (773) 936-7200 for further information. We look forward to welcoming you to the University of Chicago.

Yours sincerely,  
The Registrar











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