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AN  
INAUGURAL DISSERTATION  
ON THE  
DISEASE PRODUCED  
BY THE  
BITE OF A MAD DOG,  
OR OTHER  
RABID ANIMAL:

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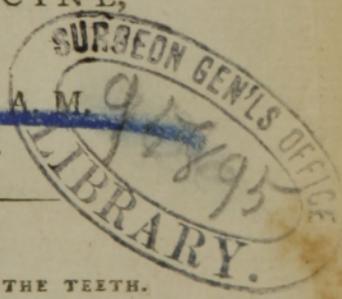
SUBMITTED TO THE EXAMINATION OF THE  
REV. JOHN EWING, S. T. P. PROVOST;

THE  
TRUSTEES AND MEDICAL FACULTY  
OF THE  
UNIVERSITY OF PENNSYLVANIA,

ON THE ELEVENTH DAY OF MAY, 1792,

FOR THE DEGREE OF  
DOCTOR OF MEDICINE,

By JAMES MEASE, A. M.  
OF PHILADELPHIA.



THE POISON KILLS,  
THE BITE CONVEYS IT, DEATH LURKS IN THE TEETH.

Lucan. Pharsal.

PHILADELPHIA:  
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TO  
BENJAMIN RUSH, M. D.  
PROFESSOR OF THE INSTITUTES,  
AND OF  
CLINICAL MEDICINE,  
IN THE  
UNIVERSITY OF PENNSYLVANIA.

*TO whom can the following pages be inscribed with  
so much propriety, as to you, honored sir, by whom  
my studies in medicine have been directed; and from  
whose publication, I received the first hints which led  
me to adopt the principles contained in this disserta-  
tion? Be pleased, therefore, to allow me to dedicate  
this essay to you, as a small mark of respect from,*

*much esteemed sir,*

*your affectionate and grateful pupil,*

**JAMES MEASE.**

*Philadelphia, May 7, 1792.*

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TO  
ANDREW MEASE, M. D.

OF STRABANE, IRELAND.

HONORED SIR,

*ALTHOUGH* related, yet personally unknown, I have taken the liberty to inscribe to you, likewise, the inaugural fruits of my studies in medicine: at the same time, I beg leave to express the high sense I entertain of the honour you have conferred, by your instructing and friendly correspondence, upon

your affectionate nephew,

JAMES MEASE.

Philadelphia, May 7, 1792.

TO  
MR. JAMES MEASE.

MY DEAR FRIEND,

*I Cannot consent to the publication of your ingenious dissertation, without requesting you to allow me room enough in your preface, to express the great pleasure I derived from reading it. It will be resorted to hereafter as a repository of facts and opinions upon the disease of which it treats.*

*I have only to add my best wishes for your success and usefulness in life; and to assure you, that I shall long retain an affectionate sense of the zeal and fidelity, with which you have discharged your duty to  
your friend and preceptor,*

BENJAMIN RUSH.

Philadelphia, May 8, 1792.





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P R E F A C E.

**E**VER since the institution of Universities and Colleges, the publication of a thesis has generally been the condition, by which the highest honor in medicine was obtained. Custom, and the state of learning, has hitherto made it usual to publish this specimen of the student's abilities, in the *Latin* language. But this has ceased to be the general medium of the communication of the learned to the world, and almost every author writes in his native tongue. As the *English* language is at present understood by as great a part of the globe as any other, the University of Pennsylvania have wisely resolved to leave it to the option of the candidate, to write either in the *Latin* or *English* language.

By delivering my sentiments in a language not generally intelligible, I might indeed be supposed to exhibit proofs of my learning; yet as a *few only* would be qualified of judging of its merit, I willingly dispense with the honor I would derive from my dissertation being read by those men, for the more humble wish of being *generally* useful to my countrymen. This can only be effected by publishing in my native language, and this alone would be a sufficient induce-

inducement to make it the medium of communicating my sentiments to the public.

FROM the first period of my studies in the science of medicine, no disease that I met with in books, engaged so much of my attention as that consequent on the effects of the poison of a mad dog, or other rabid animal, on the human body. I early deemed it an object worthy of enquiry; but the primary cause of my attention being immediately called to the complaint, was in consequence of a paragraph being inserted in almost all the newspapers in this city, in the month of January, 1790, taken partly from Boerhaave, in which the disease was described in the most erroneous and dreadful manner; and the Tonquin remedy recommended at the same time, as an infallible preservative and cure. The consideration of the very great uneasiness the piece alluded to, would excite in the minds of the people, together with the certain death that would ensue from an improper confidence being placed in the medicine, induced me to pay an immediate attention to the investigation of the disease. The very publication, however, would have been sufficient to take away any confidence that might have been put in the remedy it recommended, or any other whatever, if the assertions respecting the disease had been credited. The very numerous cases that I had met with in authors, of the failure of the Tonquin remedy, and a persuasion of its inefficacy, grounded from reasoning on the known qualities of the ingredients which enter into its composition, as suited to counteract the symptoms they were intended

induced me to determine at once as to the total inefficacy of this once famed remedy.

IN a short essay on the disease, which I drew up and inserted in the American Museum for August, 1790\*, I combated the many erroneous opinions with respect to it, and particularly attended to a comparison of the different methods of cure hitherto employed. On a contemplation of the whole of these, I was convinced of their total inefficacy, from their uniform failure, in every case where they had been used. None seemed more rational, of any that had hitherto been untried, than that hinted at by Dr. Rush, in an essay on the tetanus, contained in his volume of Medical Inquiries and Observations, published the preceding winter. In the appendix to that essay, he more particularly noticed the great similarity between tetanus, and the disease consequent on the bite of a rabid animal, and advised the same tonic remedies in the latter, which he had found so successful in the former. In the essay, above alluded to, I concluded with declaring my readiness to adopt the opinion of Dr. Rush, with regard to the propriety of the application of the same mode of treatment to both diseases, which, from reasoning on their causes and the phenomena they exhibited, I was fully convinced was founded in truth.

REPEATED reflection on the same subject, since that period, has served to strengthen me in the idea of the truth of the opinions I then delivered, and has induced me to take

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\* Vol. viii. p. 68.

a much more enlarged view of the subject than I first intended. How far I have succeeded, does not become me to say; the decision of this question rests with the public—I do not pretend to infallibility, and therefore declare, that if, from future observation, and maturer judgment, I shall find that a single opinion advanced in the following pages, is erroneous, I will readily retract it. I shall therefore, as cheerfully receive any objections offered to my opinions, as I shall be made happy by observations or remarks tending to confirm them: and whether they are offered in print, or I am privately informed of them, they shall be duly attended to, and answered in their respective modes of communication.

As to the language of my dissertation, I have endeavoured to be as clear and perspicuous as possible; and although I was fully sensible, how much elegance of style would influence the opinions of some, as to the merit of the work; yet this was not so much attended to as the matter. I also reflected, that however pleasing a well turned period may be to the ear at the time of its perusal; that finally it is sound argument which will stand the test of philosophical examination.

I shall conclude this preface by remarking, that notwithstanding we are indebted to accident for many of the most important discoveries in medicine, as well as in the sciences in general, it is nevertheless an humbling consideration to human pride, that it is seldom any truth is perfectly established, until all the errors relating to it, are first pointed

ed out. If, therefore, my labours have been attended with no *positive* good, but are merely *negative*, by shewing the fallacy of many supposed truths, concerning the subject of my dissertation, I shall think myself fully rewarded. I may, by these means induce others to extend their researches, and finally become the *indirect* instrument of stopping the ravages of a disease, hitherto the most *fatal*, and certainly the most *dreadful*, to which human nature is subject.



## INAUGURAL DISSERTATION.

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**T**HE fatal effects that have hitherto followed the action of the canine virus on the animal system, have, in every age, occasioned it to be justly viewed with horror. Indeed, whether we consider the peculiarity of the symptoms, or the total inefficacy of the medicines which have been used for its relief, no disease to which human nature is liable demands a more serious attention. The variety of opinions entertained by physicians respecting this disease, and their very great contradiction, is the surest proof of the little knowledge we possess concerning it.

IN the following dissertation I shall be under the necessity of opposing many of these opinions, but with what success, the sequel of this essay must discover: without, however, any further preface I shall enter on my subject.

## HISTORY OF THE DISEASE.

IN imitation of the practice, followed by almost every writer on diseases, it will be expected, that I should enter into the antiquity of the one that I have chosen for the subject of this dissertation.

ON this as well as on all other occasions which admit of doubt, or an opportunity for cavilling, there have been endless disputes: but as it rather affords matter of curious speculation, than a deduction of any practical utility, I shall decline entering fully into the discussion of the question, especially, as in my opinion, it can be very easily decided.

RESPECTING the first appearance of the disease, I deem it impossible to speak in a positive manner. The most probable opinion is, that as dogs have existed in all ages, this disease was of very ancient date. From the circumstance of its not being mentioned by Hippocrates, some authors, as Plutarch, and after him M. Le Clerc \*, have insisted on its origin at a later period; viz. in the time of Asclepiades.

\* Le Clerc, Hist. de la Med. part ii. p. 463.

Asclepiades, who was physician at Rome, in the 62<sup>d</sup> year of the Christian era.—But although it is not noticed by Hippocrates ; yet, as Van Sweiten\* observes, this “ amounts to no proof that the distemper was not in being in his time. It might perhaps be less frequent in the parts which were inhabited by Hippocrates ; since Aurelian † tells us, that this is a distemper, not alike common to all countries.” I shall hereafter mention also, that, in some places, the disease did not appear for a long time, and that others are entirely exempt from it, as far as we have any account, to this day. But although no particular time can be ascertained at which the disease appeared, yet we have the most positive proof of its having been known at a much earlier period than that of Asclepiades. Homer, in the ninth book of the Iliad ‡, introduces Ulysses, when on an embassy to Achilles, to request his return to the Grecian camp, comparing the fury of Hector to the rage of a mad dog. Achilles, it is well known, studied medicine under Chiron ; and therefore, as Dr. James justly observes, “ was the more capable of receiving an idea of the mischief Hector did to his countrymen by this metaphor.”

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“ phor.”

\* Comment. on Boer. aphor. 1129.

De Morb. Acut. lib. iii. p. 229.

‡ Line 237

“phor\*.” From this, it appears evident, that the disease is of very remote antiquity, as it certainly must have been known even before the time of Homer, although he is the first author from whom we have any account of it.

THIS disease is generally said by authors, to be peculiar as an original affection to the three species of the genus *canis*; viz. dogs, wolves, and foxes. No other animal, upon which any accurate observation has hitherto been made, has been known to be seized with it in a spontaneous manner, except those mentioned, although all are capable of becoming affected with it, in consequence of a bite, from any of the former†. Cats, indeed, are said to have

\* Philosoph. Transf. vol. XXXVIII, p. 249. In the 8th and 13th books of the Iliad, Hector is also compared to a mad dog, both by Teucer and Neptune.

† Throughout the whole scale of animated nature, we may observe a general law prevailing, whereby certain diseases belong to certain ages, conditions, and kinds of animals. Thus the present disease is peculiar to the canine genus, as an original affection, although man as well as other animals are liable to be affected therewith in consequence of a bite; on the contrary, there are some diseases to which mankind are peculiarly obnoxious, and which it is impossible to communicate to brutes. Thus, in repeated experiments, Mr. Hunter could never inoculate a dog, bitch, or an ass with the venereal disease. Treatise on the venereal disease, chap. i. sect. 6.

The same observation is likewise applicable to man: the Indians in Nantucket, many years ago, were carried off by diseases which never

have been seized with it spontaneously; and the excellent Morgagni remarks\*, that after dogs, he knows of no animals more liable to the disease than cats. If this be true, and these animals are seized with it originally, independent of a bite, an inquiry immediately offers itself, why other animals belonging to the same genus are not affected with it in a similar manner. † Aurelian mentions its appearing spontaneously in leopards: but I apprehend, from the same observation having never been made by any succeeding writer, and his not having sufficient opportunity to make the remark, that it is without foundation, and only taken for granted, as a probable circumstance; “as these animals, constantly dwell in the caves of great woods,  
“ remote

never affected the white inhabitants among them. Dr. Lining tells us, that the negroes were never afflicted with the yellow fever in South Carolina, although constantly around the sick; and these again have diseases peculiar to themselves, to which the whites are entire strangers.

\* Morgagni on the seats and causes of diseases. Letter LXI. art. 15.

† This inquiry was suggested to me by that excellent physician, and learned natural historian, Dr. Samuel L. Mitchell, of the state of New York, in a letter which I received from him, dated the 29th of January, 1792.

“ The genus *felis*,” says he, “ to which the domestic cat belongs, has a great number more of species than the *canis*, and it is not a little strange, that though it is reported that the house cat has been originally affected by Hydrophobia: yet, that wild-cats, lions, leopards, panthers, and other species of the genus should never have it, but in the derivative way.”

“ remote from human society, and if they happen  
 “ to fix their jaws upon men, it is commonly with  
 “ the fatal view of slaughtering them\*.” To enter  
 into an investigation of this inquiry, would lead  
 me too far from the main object of this disserta-  
 tion, and would, I fear, from the difficulty of ob-  
 taining a sufficient number of facts, be attended  
 with but little success: I shall, therefore, leave  
 it for those disposed to engage in the undertaking,  
 and pass on to another in which I am more inte-  
 rested: I mean an examination of the question,  
 whether the disease, which we are considering, ever  
 arises spontaneously in the human body, without  
 the bite of a rabid animal?

NOTWITHSTANDING, the relation of many cases,  
 by different authors, of spontaneous hydropho-  
 bia, I doubt much whether it ever appeared as an  
 original disease. In respect to some of them, it may  
 be with propriety questioned, whether the symp-  
 toms they described pertained to the actual dis-  
 ease. For, from a careful perusal, and attentive  
 examination of the many histories on record, the  
 dread of fluids appears to me to be no more than  
 a symptom of an original disease, which, from its  
 being particularly urgent, has been mistaken for  
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\* Van Swieten Comment. Aphorism 1132.

an idiopathic affection. Thus Dr. Innes \* has given an account of a hydrophobia attending an inflammation of the stomach; but the impropriety of the name will at once be evident in this case, as there must have been an equal dread of both solids and liquids, on account of their increasing the disease †; but as the great thirst under which the patient laboured, induced him only to call for drink, which from his sensations he knew he could not swallow, the aversion from it, was the cause of the disease being stamped with the name.

In other cases also, where the disease was said to have appeared without a bite, I am convinced that they were no more than cases of Tetanus, both from the causes that induced them, and from the known circumstance of an aversion from fluids taking place in that disease. Mr. Arthaud ‡, relates the history of a spontaneous Hydrophobia, which is an exact description of Tetanus.—It was brought on by the person being exposed to the alternations of violent heat in the day, and cold and damp at night, in the island of Hispaniola, which in that island, as well as others in the West-Indies,

\* Edinb. Med. Essays, Vol. I. p. 226.

† Gastritis,—pyrexia typhodes, anxietas in epigastrio, ardor et dolor, *ingestis, quibuslibet auctus*, &c. Cul. nosol. method. genus 15.

‡ Recherches sur la malad. Epizootique de Saint-Domingue, vol. I. p. 220.

dies, are among the most frequent causes of this disease. In other cases of this supposed disease, they may be with as much propriety termed fits of mania, or violent delirium in fevers. Thus Van Swieten relates the history of a man from Boerhaave, who, after being exposed to a scorching sun, and drinking at the same time a quantity of spirituous liquors, fell into a most *ardent fever*, and with great horror refused all liquors that were offered him \*. Sauvage † also mentions a case, where a malignant remitting fever was attended with a perpetual delirium, convulsions, and *dread of fluids*. Dr. Mead ‡ relates, that he saw palpitations of the heart, accompanied with a *dread of fluids*; but here, in my opinion, it was a mere symptom of hysteria, in which disease the same author also observed it to take place. The disposition to the marvellous, which is so fully shown by almost all the old writers in medicine, their extreme credulity, and the little attention generally paid towards inquiring, any further than the surface of things, have been another cause of their mistaking the real nature of this disease. Many of them likewise, as Dr. Vaughan || remarks, “ are so deficient

\* Van Swieten, Comment. 1130.

† Sauvage Nosolog: method. Vol. I. p. 354.

‡ Meads' Works, p. 81.

|| Vaughan's Cases, and Obs. on the Hydroph. p. 40.



aversion from drinking more remarkable, and more nearly to resemble the original disease depending on the virus.

BUT why should this disease be thought to arise spontaneously, any more than the small pox, venereal disease, or any other depending on a specific contagion? I will not deny that an *aversion* from *swallowing fluids* has occurred in no other disease, but that proceeding from the bite of a rabid animal. I have already mentioned its occurrence in tetanus, hysteria, and other diseases. All I contend for, is, that such cases as are called spontaneous instances of the disease, are not accompanied with the other symptoms which so characteristically designate the idiopathic affection. The same mistake has been made, in calling every emaciation of the body *a consumption*; when, accurately speaking, and according to the definition of the disease, no such emaciation deserves the name of consumption but that which is accompanied with a local, though not primary affection of the lungs, constituting the disease, and understood by physicians by the name of Phthisis Pulmonalis.

THE falsity of the idea of an actual and idiopathic Hydrophobia arising in the human body, without

out the action of contagion for its production, I hope is now evident, and I shall therefore proceed to deliver the

## HISTORY OF THE SYMPTOMS.

AT various, and indeterminate periods of time, after the introduction of the poison, by a wound in any part of the body ; it most commonly begins to shew itself, by sharp pains in the place where it was received, shooting in every direction to the neighbouring parts, and frequently to the stomach, throat, and bowels. At the same time, there is a sense of lassitude and languor, and an aversion from motion, shewing evident marks of a general debility, such as usually precede the attack of a febrile affection. The flying pains continue to increase, and seizing the urinary organs, create a difficulty and heat in discharging the urine. A pain is also felt at the *scrobiculus cordis*, or pit of the stomach ; and a sense of constriction about the fauces and throat, occasioning a difficulty of swallowing. These symptoms are accompanied, or immediately succeeded, by restlessness, and anxiety about the *precordia* ; occasioning a frequent change of posture, and sighing—and the most exquisite sensibility prevails over the whole body ; particularly

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in the organs of sense ; light becomes painful to the eyes, a sudden noise occasions involuntary startings, and the air, although to persons in health it be of an agreeable temperature, feels cold and disagreeable. If at this time, or before, the person incline to sleep, it is short, disturbed with frightful dreams, and on awakening he is apt to fall in to slight convulsions. The eyes also are fierce and penetrating\*, the countenance exhibits a peculiar anxiety†, and a sense of despair; and frequently changes its aspect ‡: a copious secretion of saliva, now takes place; but notwithstanding the sick are troubled with a very great thirst, the difficulty in swallowing, which before this time could be easily overcome by a firm resolution §, now becomes so great, that an attempt to swallow, especially liquids of any kind, whether by compulsion, or with a view of mitigating their thirst, never fails to excite the most disagreeable sensations; they are seized with violent agitations; a ball as it were rises from the stomach up to the throat, and seems to threaten a suffocation; the muscles of the face become variously contorted, and the exertion is frequently succeeded by convulsions. Vomitings at this time frequently take place; and the con-

striction

\* Vaughan's Cases and Obs. on Hydroph. p. 6, 24.

† Med. Transact. vol. II. p. 46.

‡ Fothergill's Works by Lettsom, 4to. p. 353.

§ Med. Comment. Vol. II. p. 304.

striction about the breast, and the difficulty of breathing become so extreme, that if a blast of air blows on them, they are seized with the greatest distress, cover their mouths, and seem ready to expire, as if struggling for breath. A fever sometimes occurs, preceded by slight shiverings, but it is in general very mild, and does not often appear.

THE pain in the bitten part, at this period of the disease, has generally vanished; and a numbness\*, or paralytic affection of † it succeeds, with an increase of the violence of every symptom already mentioned. A recumbent or horizontal posture aggravating their complaint ‡: the sick commonly either sit up, or walk about; the flow of saliva becomes extremely troublesome from its viscosity, and the inability to expel it, the breathing is very laborious and difficult, and being quickly repeated, and attended with a constant and peculiar kind of hawking, in order to evacuate the saliva, has been thought to imitate the barking of dogs. The irritability of the system has by this this time arrived to such a morbid degree, as occasions it to be thrown into convulsions by the slightest causes, but by none in so ready

\* Edin. Med. Essays Vol. V. part ii, p. 97. Philosoph. Transact. abr. by Lowthorp. Vol. III. p. 288.

† Mead's Works, p. 661.

‡ Hamilton's Remarks, p. 196. Mem. Med. Society Lond. Vol. I. p. 243. Vaughan's Cases, p. 23

ready a manner, as an attempt to swallow fluids ; nay, the bare mentioning of an action so recent in their minds, and severely painful in its consequences, excites a return of their symptoms, and occasions the greatest distress \*. Even the sight of water or other fluid, or of any thing having the least resemblance to it, such as a looking glass, or any white substance, whereby an occasion will be given for the renewal of the idea of their former pain, will have the same effect. At this period also, solids become equally difficult to swallow, and the bare mention of even them has excited the same convulsions as that of fluids †. In this advanced stage of the disease, when the general disturbance has prevailed for such a length of time ; slight fits of delirium occasionally occur ; the patients frequently forget their friends and relations. This delirium is attended with a constant talking ; but even at this time, they have the power of summoning themselves ; and soon becoming collected, return rational answers to the questions proposed. At other times, for the chief part, they have the most perfect enjoyment of

\* Hoc tempore quoque in hominibus priasmus incidit, cum scroti contractione, & feminis involuntaria frequente emissione.

† Edinb. Med. Commen. Vol. I. p. 304. Mem. Med. Soc. Vol. I. p. 260.

of their senses to the last. The pulse, which during the former period of the disease, was but little altered from the healthy standard, except being less strong, now becomes evidently weak, quick, and intermitting. False vision, dullness of sight\*, together with a dilatation of the pupil†, and sometimes actual blindness ‡, now appear; quantities of saliva are collected in and about the mouth, and being mixed with air taken into the lungs, put on a frothy appearance, which the patient is constantly endeavouring to get rid of, by wiping it with a handkerchief, or spitting it about with great force. The voice becomes very hoarse, and at the same time the convulsions increase in frequency and force over the whole body. Spasmodic affections take place in the muscles of the face, occasioning violent contortions, and the most horrid assemblage of features; and in the muscles appropriated to moving the lower jaw, inducing involuntary quashing, and a grinding of the teeth, which some have construed into a desire of biting.

THE strength now fails—the extremities become cold, and death in a short time relieves the miserable

\* Med. Communications, Vol. I. p. 215.

† Meads' Works, p. 660—Hamilton's Remarks on bite of a mad dog, Lond. 1785, p. 199.

‡ Med. Obs. and Inq. Lond. Vol. III. p. 362.

miserable patient from his sufferings; sometimes expiring with convulsions, as if from a suffocation, and other times in a calm and placid manner.

The above, is the common progress and order of the symptoms, produced by the canine poison on the system. But a variation from them often takes place in different persons, which it is proper I should mention, previously to proceeding any further on the subject †.

NOTWITHSTANDING a pain in the bitten part, generally is the first symptom of the action of the poison, yet in some cases it has been entirely absent, and where the person has been ignorant of the cause

† A circumstance that I have omitted mentioning in the History of the symptoms, was the terror the patients are said to be struck with at the sight of a dog; because it is only mentioned in those cases where it is said the persons were acquainted with the cause of their disease, and by therefore dwelling on the idea of their sufferings, may very readily have conceived in their delirium that a dog was present, which they have accordingly desired might be taken away. I can readily assent, however, to another thing commonly taken notice of, viz. the convulsions excited by the barking of a dog; but this arises solely from the very great sensibility of the sick, by which this, or any other sudden noise, as shutting a door, a blast of wind, &c. produce the same effects.

cause of his symptoms it has never been mentioned, through the whole course of the complaint. This was the case with Mr. Bellamy, as related by Dr. Fothergill\* ; also with the person whose history is related by Dr. Lister †, and with many others whose cases are on record in books of medicine.

A Considerable variety, is also observed in the first symptom of the general affection, even when this is not shewn by the pain in the bitten part. In some, the urinary organs are first affected ‡, while in one of Dr. Vaughan's § patients as well as Wilbraham's ||, and others, the stomach, bowels, and throat, were the parts on which the disease first shewed its effects.

THE disposition which has hitherto almost universally prevailed among authors, to divide diseases into stages, has also extended itself to the one whose history I have now delivered. Each of these stages is described as having a peculiar and appropriated set of symptoms, never occurring in

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\* Fothergill's Works 4to, p. 353.—Med. Commun. Vol. I. p. 214.

† Philosop. Transact. Vol. III. p. 277.

‡ Fothergill's Works, p. 353.

§ Vaughan's Cases, p. 4.

|| Philosop. Transact. abridged, Vol. XLVII. p. 413.

any other stage, and which uniformly remained the same in all cases. But had these descriptions been drawn up from the many accurate relations of cases of the disease on record, rather than copied from preceding systematic writers, it would have been found, that a difference of circumstances, as those of constitution, and age, would have made a material difference in the progress of the symptoms, in different persons. This variation of the symptoms in the present disease, has already been pointed out, and is particularly observable in the pain at the bitten part, which although generally a symptom of the first stage, frequently does not appear through the whole course of the disease.

DR. Duncan\* has divided the disease into stages, though in a somewhat different manner from other authors. But although I have the highest respect for the authority of this excellent physician, from whose industry and labour, I have derived much information, yet I cannot assent to his division, as I do not find it supported by the cases on record. The affections of the *vital functions*, which are said by him to mark the commencement of the *second stage*, are very generally among the first symptoms of the

\* Heads of Lectures on the Theory and Practice of Med. Edinb. 1785, p. 227.

the disease: for as Dr. Seleg observes, “ It begins  
 “ with the disorder before the dread of water takes  
 “ place, increases with it, and attends to its last  
 “ stage, till at last the patient expires under it.\*”

The affection of the *mental* faculties, which is said to mark the third stage, is an equally uncertain sign whereby to designate any particular period. For while a delirium sometimes takes place very early in the complaint, at other times, it never occurs during its whole course, the sick having the most perfect enjoyment of their senses †; and most commonly the mental faculties are in a superior state of excitement. This is shewn by their quick perception, amazing acuteness of understanding, and the rapidity with which they answer questions. Thus, Dr. Howman ‖ gives us a case of a patient, whose “ reason was all along very good, and as “ some observed, better than in health.” Dr. Vaughan likewise remarks in the history of the third person whose case he relates, that “ His intellectual faculties seemed quickened by disease.” For this reason, therefore, that the divisions hitherto attempted by authors, were in a great

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measure

\* Medical Museum, vol. II, p. 110, Lond. 1781.

† London Med. Obs. and Enq. vol. III. p. 356.—Hamilton’s remarks, p. 196.—Vaughan, p. 29.

‖ Philos. Transact. vol. III. p. 281.

measure arbitrary, not being bounded by any particular set of symptoms, as well as from a conviction, that if it were possible to establish them, no useful deduction could be inferred from them in the *removal* of the *disease*, I determined to take no notice of them, but to mention the symptoms, in the order they appear most generally to follow.

FROM a consideration of the variety of the symptoms, and the strong manner in which they are marked, it would, on a first view, seem very probable that the greatest advantage or information would be derived respecting the pathology of the disease, from the dissection of the bodies of those dying of it. Accordingly, we have very numerous histories of the appearances after death, related by many authors; but on account of their extreme variety and want of uniformity, and the circumstance of their all being the consequence, and noways connected with the cause of the disease, it is impossible to draw any conclusions from them. The excellent Morgagni, although he relates the dissections of many cases, does not pretend to draw any conclusions from them, but observes, that as “there are many things in which  
“these patients, while living, differ one from another, so there are not fewer, but even more,  
“in

“ in which their bodies differ after death \*.” The violent convulsions that are observed to take place in the muscles of the throat when an attempt is made to drink, constituting one of the most remarkable symptoms of the disease, naturally turned the attention of physicians towards the examination of that part after death. A slight degree of inflammation, or rather redness in the superior parts of the trachea or windpipe, has sometimes appeared, but in others no marks of any such redness has been found, and where it occurs, as Van Swieten observes, “ it rather seems to be an effect or consequence of the distemper, than a productive cause of it,” arising from the frequent convulsion of the parts. The epiglottis has also been observed to be crisp and dry in some cases: but a more general circumstance attending after death, is the quick tendency of the bodies to putrefaction.

I shall now proceed to the rationale of the symptoms, or investigation of the disease.

FIRST. The *extreme sensibility* of the whole system to the external air, and the disposition to be thrown into convulsions by slight stimuli, is one of the

\* Morgagni, letter VIII, art. 30.

the most remarkable symptoms attending this disease. To attempt a full explanation of it, would be a difficult, and I fear, an impracticable undertaking: but although the particular charge induced on the nerves, cannot be ascertained; yet so far we know, that the poison by its action on those organs, deprives them of the power of performing their proper functions by which the above morbid sensibility is induced\*.

SECOND. The theory of convulsions being so very generally delivered by every pathological writer, needs no explanation in this place.

THIRD. The singular *oppression* and *distressing anxiety* at the præcordia, or round about the breast, which occur so early in the disease, and continue to oppress the patient, merit a particular explanation.

DR. Heysham † resolves them into an unequal determination of the blood from the vessels of the superficies to the larger ones near the heart and lungs; but we have no proof of this undue determination

\* This state of extreme sensibility to the air, has been remarked as far back by physicians as the time of Aurelian and constantly noticed by every writer on the disease: hence the patients were said to labour under ærephobia.

† Dissertat. inaug. chap. xi.

mination; and I am rather disposed to ascribe them to the irritation of the nerves of the lungs, and especially those of the bronchia, whereby their cavity is contracted, and a morbid sensibility induced; hence the application of the air to their surface proves highly painful. A full distension of the lungs, therefore, cannot take place, and this occasions a more frequent respiration, which then becomes in some measure voluntary, and of course very tiresome: anxiety will then follow, together with the difficulty of breathing, so much complained of.

THE palpitations, ascribed by Dr. Heysham to the collection of blood, or to its unusual determination to the heart, are not owing to this cause, but merely to the nerves of that organ partaking of the general irritation pervading the system, by which they excite the heart to more frequent contractions. The occurrence of the same symptoms in hysteria, and other nervous disorders, and from the same cause, is a further proof of this opinion. No such symptom, as a palpitation, takes place in those diseases, where there are the most unequivocal proofs of the heart being oppressed. In this case, a violent increased action of the heart and arteries, which is shewn by a full and hard pulse, is the only consequence.

FOURTH. The violent pain experienced at the *serobiculus cordis*, or pit of the stomach, appears to arise from the irritation of the nerves supplying the diaphragm, whereby it is thrown into inordinate contractions. That this muscle is affected, is rendered also probable by the hiccoughing which sometimes occurs. How far the diaphragm is concerned in the affection of the throat, I will not pretend to say. Dr. Vaughan, indeed, asserts he was led to believe, that in “this disease a new *sympathy*” “was established between the *fauces* and *diaphragm*,” “and that the latter was drawn into a most severe” “spasm, as often as any offending cause operated” “upon the former\*.” The connection, however, between these parts is certainly great, and was long since observed. Heister informs us, that in a case of inflammation of the diaphragm, he observed the power of swallowing destroyed, and a difficulty of deglutition from a contraction of the diaphragm, we know takes place in a fit of hysteria †.

FIFTH. One of the most remarkable symptoms attending this disease, which from the early notice taken of it, and the circumstance of its general predominance, has given a name to the disease, is

\* Vaughan's cases and obs. p. 47.

† Haller opus. de musc. diaph. No. 36.

is the intolerable aversion, shewn for the most part, by those who labour under this disease, from water and fluids of all kinds. This has never as yet been satisfactorily accounted for; for which reason, and because its solution will tend to render the complaint of a much more simple nature, than it hath hitherto been considered, I shall spend some time in investigating its cause.

THE idea generally entertained by authors respecting this symptom, is, that it is owing to some change induced in the system, from the action of the poison on it, whereby a *specific dread* of fluids is induced, independent of every other cause. But I hope I shall clearly prove, that it is entirely owing to an affection of the throat, from whose morbid sensibility, and the inability of swallowing, together with the pain excited, this symptom seems wholly to originate.

SALIUS DIVERSUS, indeed, was the first who disbelieved this doctrine, and came nearer the truth than any of his predecessors, by referring the aversion from drinking to the circumstance of the patients finding themselves worse after taking any fluid\*. Dr. Whytt † likewise entertained a similar opinion: for he observes, that “ the hydrophobia is only a  
D “ violent

\* Salius diversus de venenis, p. 349.—Van Swieten comment. aph. 1138.

† Whytt's works, p. 680, 4to.

“ violent convulsion of the gullet and stomach, arising from the disagreeable sensation excited by the liquid touching the fauces.” But neither of these explanations is satisfactory: the former does not inform us of the ultimate cause of the sick being rendered worse by swallowing liquids; and the latter, by the above quotation, and in other parts of his work, resolves it into the specific stimulus of water on the throat.

THE explanation, therefore, that I would propose of this symptom is as follows. In consequence of the action of the poison, on the nerves of the body as before mentioned, a morbid and excessive degree of sensibility is induced; whereby the action of the slightest stimuli produce the most disagreeable effects. The fauces also, particularly the muscles employed in deglutition, partake of this general morbid state; as soon therefore, as any liquid touches them, they are seized with spasmodic affections which consequently excite pain; in the very irritable state of the parts, this pain becomes extreme; on a second attempt, therefore, to drink, or a mere mention being made of it, the idea of the patient's former sufferings will be immediately excited, and consequently he will refuse it with disgust.

BUT

BUT even this pain may be excited by the irritation of the saliva, on the very irritable fauces; whereby an attempt will be made to swallow it, and this gives the first idea of disgust to fluids, before any exertion has been made to drink. Accordingly the patient will endeavour to avoid a repetition of an act which excited so much pain; and any liquor will be refused afterwards, or the mere sight of the water, renewing the idea of his pain, will produce the same effect. This explains the cause of the terror shewn by some persons in the first stage of this disease, before any attempt has been made to drink; and which has seemed to establish the common idea that the aversion from fluids was not owing to a difficulty of swallowing, but to a specific dread of them. Thus Sauvage † relates the case of a butler, who was unable to support the sight or touch of water, though he had not as yet made any attempt to drink: and Mr Babbington ‡ also mentions, that the boy, whose history he describes,

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shewed

\* “ To ask one in this condition to drink, is to desire to choak himself; and when he has found this to be so, he dreads the sight of liquors offered to him, as much as he would a knife presented to his throat, and strives to keep them from his mouth.” Mead’s Works, p. 83.

† Sauvage sur la rage, p. 12. Van Swieten, Com. Apher. 1138.

‡ Med. Com. Vol. I. p. 214.

“ shewed evident agitation at the sight of a cup of  
 “ mint water poured out for him to drink.” But  
 the disease had at this time subsisted for several  
 hours ; and therefore his age, and probably his  
 constitution, favoured the increase of that morbid  
 sensibility which it is the nature of the poison to  
 induce on the system. The affection of the throat,  
 then, contrary to the opinion of Van Swieten and Dr.  
 Heysham, precedes the aversion shewn from drink-  
 ing ; for the former are very frequently with the sto-  
 mach the first parts attacked, as mentioned before,  
 and occurred in a remarkable manner, in the cases re-  
 lated by Drs. Lister \*, Wilbraham †, Vaughan ‡, and  
 Mead § ; in these therefore the aversion from liquids  
 appeared as a primary symptom ; but in other cases,  
 where from a variety of circumstances this affection  
 of the throat or stomach did not come on for some-  
 time, water, and other fluids, were taken with the  
*greatest ease and composure*, until the commence-  
 ment of the affection of those parts, soon made  
 them disgusted at the sight of it. Thus Dr. Lister  
 remarks, that it was not until the fourth day  
 after

\* Philosoph. Transf. Vol. III. p. 276.

† ibid. Vol. XLVII. p. 413.

‡ Vaughan's Cases, p. 4.

§ Mead's Works, p. 660.

after his patient first complained, that any aversion from drinking appeared; for, on the day preceding, "he called for burnt brandy and drank it." And it was not until the next day, when he perceived a strong rising in his stomach, that he had "an impotence to drink." Dr. Howman also says, that no aversion from water, took place until the seventh day of the attack, and that preceding the death of the person, when the spasmodic affections became severe. Mr. Bathie's † patient shewed no disgust to fluids, until the difficulty in swallowing came on; and he remarks, that when this occurred, "and the fluid touched the fauces, it seemed at the peril of his life." Dr. Vaughan ‡, in the history of his second patient, mentions, that he was attacked on Tuesday, yet he drank all that and the succeeding day, until the evening, and next morning; when the occurrence of a vomiting evidently shewed an affection of the stomach. In the case related by Dr. Gray §, it was not until the fifth day, that any aversion from fluids was shewn. Morgagni || remarks also, that there are some who will drink water itself without difficulty, when the first

† Edinb. Med. Com. Vol. III. p. 290.

‡ Vaughan's Cases, p. 22.

§ Med. Comment. Vol. XI. p. 304.

|| Letter 8th, Art. 30.

first trouble of swallowing is overcome; and quotes two cases from the Ephemerides of the curious, in proof of the assertion: and so fully convinced was Dr. Mead of the aversion from fluids, depending on a difficulty of swallowing, that he said the name of the disease ought to be changed—and that instead of “hydrophobia,” it should be called “Dyskataposis\*.” But further, although these cases render it probable, that the dread of water, as it is called, succeeds, or at least appears at the same time, with the affection of the throat, yet that the former depends entirely upon the latter, is fully proved by this fact, viz. that in those persons where the throat was entirely free from any affection during the whole course of the disease, or where the violence of the symptoms had abated, water and other fluids were taken with the greatest composure. “A learned physician,” says Dr. Mead, “has assured me, that in Shropshire he “saw three patients in one year, yet none of them “during the melancholy scene, had any difficulty “of swallowing, or shewed any signs of a dread “of liquids.” Dr. Houlston has also in the London Medical Journal†, published a letter from a physician, where it is mentioned, that during an interval of sixteen hours, which took place in this disease,

\* A difficulty in swallowing. Meads' Works, p. 84.

† Vol. V. No. 4. for 1784.

ease, liquids of all kinds were swallowed freely. The third person, whose case is related by Dr. Dickson \*, also drank several cups of tea in the latter end of the disease. The public papers † last year, gave us an account of a servant in one of the public inn's near London, who died of this disease; and who, “to the astonishment of the attending medical faculty,” drank in the progress of his complaint, freely and without the least discomposure, great quantities of liquids.

IF, therefore, the aversion from drinking, most commonly shewn by those persons who labour under the effects of the canine poison, were owing to the poison simply, and some specific change wrought on the system, the absence of the affection of the throat, entirely in some cases, and its abatement in others, ought not to make the least alteration in this generally supposed pathognomonic symptom; for the poison being still in the system, its effects should invariably and constantly continue. But the direct contrary is observed to take place. How then can this specific dread be accounted an universal cause?

But

\* Med. Obs. and Inq. Vol. III. p. 368.

† London Paper; Times Novemb. 30, 1790. Dunlap's American Advertiser, January 1791.

BUT further, another proof of the truth of the explanation I here have given respecting the aversion shewn from fluids by persons labouring under the effects of this disease, is derived from the declaration of the patients themselves, who, as was said before, are most commonly possessed of their senses, and are capable of returning rational collected answers to questions proposed to them. These constantly refer the whole cause of their disgust to fluids, to the difficulty in swallowing. Thus, in the case related by Dr. Hartley\*, it is remarked, on being asked, “whether his  
 “aversion from drinking proceed from any pain in  
 “*swallowing, or something else?*” he replied, “to  
 “a pain in swallowing.” Mr. Nourse’s † boy being asked, why he had not taken any nourishment as desired, gave as a reason, that “he could not swallow.” In the first case related by Dr. Mead‡, the patient declared twice on attempting to drink, *that it hurt him to swallow*, and threw the fluid out with violence. Morgagni|| likewise takes notice that the sick, “when asked why they did not drink?” have answered, that they could not by reason of the great  
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\* Philosoph. Transf. abr. by Martin, Vol. XI. p. 225.

† Ibid. No. 445.

‡ Mead’s Works, p: 659,

|| Morgagni Letter, viii, art: 19,



fenfibility occur, will alfo tend to prove the caufe here affigned, and the total inutility of having any recourse to the poifon, in order to account for this fingular fymptom. It was mentioned, before that Dr. Mead \* obferved it in hifteria, Drs. Percival †, and Ruff ‡, have recorded cafes of its occurring in Tetanus.

BUT it may be asked, if there be no fpecific dread of fluids, why are folids fwallowed with lefs difficulty than liquids, contrary to what is obferved of all other affections of the throat? To this I would reply, that a very material difference exifts between an affection of the mufcles of deglutition, proceeding from a ftate of inflammation, and a diftenfion of the part, and that affection proceeding from the difeafe at prefent under confideration. In the former, liquids are fwallowed with greater eafe, as requiring lefs exertion of the mufcles than folids, which create great pain by increafing the preternatural diftenfion already exifting. In the latter, liquids are fwallowed with greater difficulty for the fame reafon, viz. requiring more exertion of the mufcles, of which the patient has entirely loft the command; but folids are enabled to defcend with  
greater

\* Meads Works, p. 83.

† Effays Med. Philofoph and Exp. Vol. II. p. 366.

‡ Med. Inq. and Obf. p 178.

greater ease, as by their bulk they do not require such a forcible contraction of the muscles in order to force them down. It must be also recollected, that in performing the act of swallowing, the tongue is drawn backwards, and at the same time pressed against the upper and back part of the palate, extended over the roof of the mouth, whereby the substance is pressed against the epiglottis, which by its own elasticity is constantly at other times erect, and thereby effectually and completely covers the windpipe, directing the passage of the food immediately into its proper place or gullet; the extension and continuation of the soft palate at the same time preventing its regurgitation up through the nose. When, therefore, these parts are affected with a morbid sensibility, and the healthy action is taken away, as in the present disease, a *fluid* is no sooner applied to them, than a spasmodic affection is excited in the part, and they not being able to overcome this, it terminates in a violent convulsion: but *solids*, by their distension, overcome the stricture and resistance made to their progress by the convulsion of the parts, and thus they descend into the œsophagus with greater ease: they are also enabled to press down the epiglottis, which *liquids*, by their want of this distending power, are rendered incapable of doing. Fluids likewise, as Dr. Seleg observes, “penetrate the sides of the

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mouth,

“ mouth, the tongue, and the throat much more,  
 “ and produce therewith a greater irritation or  
 “ commotion than the solid food can have upon  
 “ these parts \*,” in consequence of a greater sur-  
 face, which is endowed with this morbid sensi-  
 bility, being exposed to the stimulus of the fluid.  
 In a state of health, when the muscles of the  
 throat can be commanded at pleasure, and the  
 nerves which supply them are not affected with a  
 morbid sensibility, the action of deglutition is suf-  
 ficient to press down the epiglottis, assisted by the  
 slight gravitating influence of the fluid itself; but  
 in the present disease, this healthy action of the parts  
 being destroyed, there remains nothing but the  
 mere mechanical force of the fluid to effect what  
 was done by the united force of both before, which  
 being unable to accomplish, a violent irritation in the  
 part ensues, with great pain, and an immediate  
 rejection of the liquids.

THIS explanation is greatly strengthened by the  
 consideration of this circumstance, that in other  
 diseases where there is the same want of command  
 of the muscles of deglutition, solids are swallowed  
 with greater ease than liquids; but from the  
 same morbid sensibility of the parts not accompany-  
 ing the disease, the latter do not excite so much  
 pain, as in the disease consequent on the bite of a  
 mad

\* Med. Museum, vol. ii. p. 228.

mad animal. Thus Van Swieten \* relates that he attended a woman, who, as he says, had “ a palsy  
 “ of the muscles of deglutition, in whom the  
 “ swallowing continued to be hindered in such a  
 “ manner, that she could not get liquids down at  
 “ all, but was only able to swallow large mouthfuls  
 “ of solid food.” The same is further confirmed  
 by this, that patients in this disease can frequently  
 swallow liquids, but only when taken in large quantities  
 at a time, or in successive draughts †, as by  
 their bulk they prove superior to the spasmodic  
 constriction which takes place from the irritation  
 created by them. And although Sauvage’s ‡ patient  
 could not swallow water, yet he could *broth*,  
 which proved much less disagreeable to him, as it  
 approached nearer to the nature of a solid, and  
 consequently would act more by gravity in pressing  
 down the epiglottis, and overcome the stricture  
 raised by the irritation on the fauces.

IF this aversion from fluids depended on a power  
 of irritation possessed by all kinds of liquids, as as-  
 serted by Dr. Heysham, then solids should never be  
 refused, but swallowed with as much ease as in  
 health. The contrary, however, is the case; and  
 I apprehend one great reason, why the notion  
 has

\* Comment. on Boerh. aph. 318.

† Memoirs Lond. Med. Soc. vol. i. p. 249.

‡ Sauvage, dissertat. sur la rage. p. 12.—Van Swieten aph. 1158

has so universally prevailed, of a specific dread of fluids being peculiar to the disease, is this: From the circumstance of the thirst, that distresses the patient, he is induced to ask for drink, which he finds it difficult and painful to swallow. Hunger, although it sometimes takes place, is by no means so uniform a concomitant of the disease as thirst, and it has seldom occurred to try whether the same difficulty prevailed equally with regard to both *solids* and *fluids*. In the few cases, however, where the experiment has been made, the same difficulty was experienced in swallowing *both* the former and the latter, and the convulsions have been equally excited by a mere sight of either.

DR. Lister \* relates, that his patient found great difficulty in swallowing food. The boy, whose case is recorded by Dr. Dickson †, declined eating some meat that was offered him, and when pressed, he begged that it might be cut small, in order that he might have as little trouble as possible. Dr. Johnston ‡ says, that any attempt to swallow some bread, occasioned the greatest agonies. Dr. Gray likewise takes notice, that after the disease had subsisted some days, his patient equally abhorred

\* Philos. Transf. vol. iii, p. 277.

† Lond. Med. Obs. and Inq. vol. iii, p. 364.

‡ Memoirs Med. Soc. Lond. vol. i. p. 260.

red *solids* as well as *fluids*, and when importuned to eat, he was thrown into convulsions\*. Notwithstanding Mr. Babbington † mentions the dread the boy, whose history he records, had of fluids, he takes no notice of the least difficulty in swallowing solids: but our professor, Dr. Griffiths, informed me, that he attended the hospital at the time the boy was there, and that the aversion from swallowing equally respected solids as well as fluids, and refused either to drink or eat; giving as a reason, that “it tore his stomach.” In the case recorded by Mr. Bathie, the patient objected to eating some food, saying, “its passage at the throat would be interrupted as had hitherto been the case with drink.”

AFTER this discussion of the apparently simple question, respecting the cause of the aversion from fluids, I expect no doubt will remain, as to the propriety of referring it to the affection of the throat. I have shewn, that in those cases where this did not occur, fluids were swallowed with the same ease as in health, and also explained the reason why for the most part solids excite less pain. From the actual declaration of the patients themselves, it was likewise rendered clear, that there was no specific dread

\* Edin. Med. Comment. vol. xi, p. 304.

† Med. Commun. vol. i. p. 215.

dread of fluids, but that the sole cause of the horror expressed at the sight of them, originated from their renewing the idea of their former pain. But lastly, the same aversion to fluids happening in other diseases where a similar spasmodic affection of the muscles of the throat, and a similar excessive sensibility takes place, prove the falsity of the opinion, which supposes the aversion to depend on the poison.

THERE are several other symptoms of this disease which merit an explanation: but were I to attempt these, it would extend this dissertation beyond all bounds, and I must therefore decline their consideration, and proceed to the

#### D E F I N I T I O N.

I deferred this part of my subject until this place, because there were several circumstances required to be explained, before the definition could be given, and which tended to contradict the opinions of authors on which the definition of the disease was founded.

DR. Cullen defines the disease to consist in “ a  
 “ disgust and dread of any fluid to be drunk, as  
 “ exciting a painful convulsion of the *pharynx* or  
 “ gullet, for the most part from the bite of a mad  
 “ ani-

“ animal \*.” By this definition, the illustrious professor has constituted the aversion from fluids an essential symptom of the disease, and has supposed it to precede the affection of the throat: but I have shewn, that it is not a constant symptom of the disease, and that solids as well as fluids frequently are equally difficult to swallow. It has been also rendered clear, that this aversion from swallowing, depends entirely upon the recollection of the difficulty and pain experienced in a former attempt.

DR. Cullen likewise supposes the disease to arise spontaneously in some cases, which I have before rendered probable, never happens. I would, therefore, say, that the disease of which we are treating, consisted in “ violent convulsions of the  
“ whole body, particularly the throat, creating a  
“ difficulty of swallowing, proceeding from the  
“ bite of a mad animal.”

THE length of time that elapses between the infliction of the poison, and the appearance of the disease, is very various; I am by no means, howe-

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ever,

\* Potionis cujuslibet, utpote convulsionem pharyngis dolentem eientis, fastidium et horror. Cul. nosol. method. genus lxiv.

ever, disposed to give credit to stories related by various authors of years elapsing before the person became affected. Thus Morgagni \* refers to the *Ephem. naturæ Curios.* for a case where twenty years intervened between the bite and the appearance of the disease; and also to another author, who says forty years elapsed; but I think these authorities may justly be suspected. And even Van Swieten, who, on other occasions, appears to have been sufficiently ready, to give credit to things related on slight authority, entirely rejects the above; and very properly mentions the necessity of avoiding to inculcate such stories, on account of the bad effect it might have on the minds of weak people †. But whatever doubt may be entertained of the credibility of the case referred to by Morgagni, “there can be none,” says Dr. Percival, “of the case which the same author ‡ relates, of a boy under his own inspection, in whom the symptoms came on five months after a bite§.” Dr. Vaughan|| has given us a case, where nine months elapsed between the bite and the commencement of the disease and instances at nearly

\* Letter 8, Art. 21.

† Comment. on Boerh. aph. 1137.

‡ Letter 8, Art. 22.

§ Percival's Essays, Vol. II. p. 370.

|| Cases and Obs. on Hyd. p. 22.

nearly similar distances of time, are related by authors. Dr. Tilton has recorded a supposed case \*, where nineteen years elapsed between the infliction of the bite and the attack of the disease. But I cannot think with that gentleman, that it proceeded from the poison having remained so long latent, without affecting the system. It is said that the wound would frequently break out, and discharge freely. Now, we have no instance of the real disease, where the same circumstance is mentioned; even on the attack, although a pain is generally felt at the bitten part, yet it seldom happens that the wound breaks out afresh. If this case, proceeded from the poison, it certainly would have operated on the system the first time of the wound's breaking out; which, moreover, was not the case in the present instance. Another reason for not supposing it to proceed from the poison, is, that there is no instance on record of a real case of the disease being cured, by the same mode of treatment, viz. large and copious bleeding, and other debilitating means; which though constantly recommended by writers on the disease, yet they have adduced no proof of the efficacy of the treatment. It is true, this woman had a most violent dread of water; but this, as I have already proved, takes place in the hysteria, which disease, in fact, I imagine the one described by Dr. Tilton to be. In this disease the same

\* Med. Comment. Vol. VI.

morbid sensibility of the nerves of the whole body, and particularly the fauces, prevails, into which I have endeavoured in some measure to resolve the aversion from fluids. It is remarked also that this woman was of a very irritable habit, and such we know, are most subject to hysteria. A very violent attack of this disease, frequently borders on mania, and requires very copious blood-letting for its cure; and such indeed does the case related by Dr. Tilton appear to have been. The only case to depended on of the greatest interval of time occurring, is that mentioned by Mr. Nourse, where nineteen months intervened between the bite and the appearance of the disease. On the contrary, the shortest space recorded, is that related by Dr. Gray, to have happened in the East Indies, where death followed the evening of the same day in which the bite was received. At different periods between these two last mentioned, the disease has frequently appeared; but the most common time may be included between three and six weeks.

HERE it would be an useful inquiry, to investigate the cause of the variety, in the time of the appearance of the disease. This has been attempted by many authors; but, in my opinion, it has not as yet been accounted for in a satisfactory manner. The following

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ing are the principal causes which have been assigned, as influencing the late or early attack.

1. THE part of the body bitten.
2. THE stage of the animals disease, at the time of inflicting the bite.
3. THE difference of the original virulence of the poison.
4. THE quantity of it inserted into the wound.

To enter fully into a discussion of each of these assertions, although highly useful, would extend my dissertation to too great a length; I therefore shall be as brief as possible.

1. THE part of the body bitten, is universally admitted by authors, as having a very considerable influence on the period of attack. From the erroneous idea entertained of the specific tendency of the virus, to unite with the salivary secretion, it was said, that the nearer to the head, the bite was received the sooner would the disease appear. But for this opinion, there is not the least foundation. In repeated instances, where the bite has been received in the head and parts adjacent, and where consequently the poison might be supposed to have a very ready communication with the saliva, the disease has not appeared sooner than in other cases where  
the

the wound has been received in the most distant part of the body\*.

2. THE stage of the animal's disease, at the time of inflicting the bite.

FROM the virulence of the poison, which has hitherto been generally supposed proportional to the continuance of the disease in the animal, a bite received in the latter stage, has been imagined to be much more dangerous than one inflicted when the dog was not long mad. But experience has shewn, that a bite received in the first stage, has proved equally and as suddenly fatal with one received after the disease had subsisted sometime †.

3. THE difference of the original activity of the virus is supported on hypothesis alone. We have no proof of this difference; and the idea is opposed by the analogy of other contagions particularly the small-pox ‡, and venereal disease || in which it is well known the kind of matter has no influence over the disease it produces.

\* Med. Commun. Vol. I. p. 214.

Morgagni Letter viii, Art. 29.

Ibid. Letter viii, Art. 22.

Med. Obs. and Inq. Vol. III. p. 359.

Memoirs Lond. Med. Soc. part i. p. 255.

† Hamilton's Remarks, p. 201.

Med. Commun. Vol. I. p. 216.

Med. Comment. Vol. XI. p. 304.

‡ Cullen's First Lines, Vol. II. p. 140.

|| Hunter's Treatise on the Ven. Dif. Chap. I. Sect. vi.

I should imagine likewise, that as the disease cannot be excited, unless the poison has arrived at a certain degree of perfect formation, so, if this has taken place, I cannot see the propriety in supposing any additional activity, as all it can effect is to excite the disease. The proportional violence of the symptoms, must then depend on the difference of constitution.

4. THE quantity of the poison inserted in a wound, is equally void of influence respecting the appearance of the disease. In many cases, the bite has been so trifling, as scarcely to be noticed, which has produced the disease as soon and certainly as where it has been very extensive. It would be unnecessary to quote instances, as they may be found in every author.

HAVING, as I hope, shewn the fallacy of the many causes assigned by authors, for the comparative, early, or late attack of the disease in different persons; I shall now proceed to lay down that which appears to me to be the true cause of the variety.

THIS, in my opinion, depends on the influence the greater or less sensibility of the system, as depending on

1. ORIGINAL CONFIRMATION ; and,
2. CLIMATE.

WHEN

1. WHEN speaking of the action of the poison in the production of the disease, I shall shew that it first acts for the most part, on the nerves of the place where it was inserted, and afterwards brings those of the whole system into sympathy. The influence of the greater or less sensibility of the nervous system will, therefore, be readily perceived to be considerable, in favouring, or retarding, the appearance of the disease, inasmuch as it favours the increase of that morbid state, which I have already mentioned to be the peculiar property of the poison to induce. This has been found to be the case in the present instance; for those persons, who either by habit, or other circumstances, were of an irritable nature, have been observed to be attacked much sooner than in those who possessed less sensibility of the nerves. Thus, in women and children, who, for the most part, have their nervous systems very easily moved, a much shorter period has intervened between the bite, and the commencement of the symptoms, than in men, who from possessing less delicacy or sensibility of their nerves, have remained longer free from the disease. Thus, Sauvage\* relates the case of a woman, in whom the disease came on in three days after the reception of the bite. In a boy†, a  
 period

\* Sauvage, sur la rage, p. 4.

† Med. Commun. vol. i, p. 214.

period of only seven days elapsed between the bite and the appearance of the symptoms: while a “laborious farmer \*,” who was bitten in September, felt no inconvenience until the June following.

THIS reasoning is confirmed by what we see respecting other nervous diseases, where a greater or less degree of sensibility, has a most important influence in their production, by rendering the system more liable to the impression of stimuli. Thus a sudden fright, will throw some women into convulsions, while others will be not in the least affected by it, except a slight momentary agitation. The noise of a person chipping bricks †, or the sound of a bell ‡, have occasioned fainting and convulsions. The Turks, from the immoderate use of opium, are affected with the greatest sensibility of system; and to such a degree does it prevail, that the slightest noise, such as the sudden shutting of a door, the falling of a sonorous body on the floor, will occasion involuntary startings and tremors, similar to what we observe in women who are highly hysterical ||. On the contrary,

G

when

\* Vaughan's cases, p. 22.

† Kirkland's enquiry present state med. surg. vol. i, p. 199.

‡ Boyle's usef. of experiment. philosoph. part. ii, p. 248.

|| This fact I deliver on the respectable authority of Alexander Ross, M. D. of this city, who, from several years residence at Constantinople, had frequent opportunities of witnessing the fact.

“ when either the *whole* nerves, or a *part* of  
 “ them, are deprived of a proper degree of  
 “ sensibility, the body in general will then be less  
 “ apt to be affected by the above causes ; and the  
 “ action of those parts will be imperfect. Thus,  
 “ when the nerves of the intestines are less disposed  
 “ than usual to be affected by their natural *stimuli*,  
 “ the irritation of the aliments, air, and bile, will be  
 “ be only able to raise a languid peristaltic motion,  
 “ and therefore the person will be costive : when  
 “ the sensibility of the retina is impaired, objects  
 “ are seen less distinctly \*.”

2. THE second cause mentioned as favouring this  
 early appearance of the disease, by producing a  
 greater sensibility of body ; was the *Influence of  
 Climate*.

THE effects of excessive heat, in producing a great  
 degree of sensibility of body ; and on the contra-  
 ry, the tendency of cold, to diminish it, are well  
 known. Hence in warm countries the greater  
 frequency of nervous diseases, particularly Tetanus,  
 which is accounted endemial to hot climates and  
 seasons. In this city also, during the warm wea-  
 ther, it frequently appears : but the observation of  
 physicians respecting the present disease, puts  
 it

\* Whytt's works, 4to. p. 527.

it beyond a doubt, that in warm climates, the disease comes on much sooner after the infliction of the bite than in those that are cold. In the East Indies, it is said to be in particular very rapid in its progress, and Dr. Gray \* relates, on the authority of Mr. Murray, formerly an officer in the Nabob of Arcot's service, that when at Madras, he saw a boy brought into the fort, who died in the evening of a bite he received the same day.

#### S Y M P T O M S I N D O G S.

IN order to dispel any fears, in case a bite should be received by a dog, supposed to be mad, or to stimulate others to apply for early relief, it may be necessary to briefly to mention the symptoms whereby a dog may be known to be affected. These, after the disease has subsisted for some time, must be evident to any one, but in its very commencement, it is an important consideration to be able to ascertain the fact in a clear and decided manner; as I have shewn that the poison, though contrary to the universal opinion of all writers, is equally capable of producing its fatal effects in the first attack of the disease, as in its latter stage.—

1. A Loathing of food, which is generally accounted by authors as a certain sign of the disease,  
is

\* Med. Commen. vol. xi. p. 304.

is by no means worthy of being trusted to, as dogs in the first stage after heartily eating, have given a bite, which has caused the disease. In the case of Admiral Rowley's son \* "the animal turned from its  
 "meat, and bit him on the right side of the lower  
 "lip."—Mr. Bathie also tells us, that the dog,  
 "far from exhibiting any appearance of madness,  
 "deceived the boy by fawning on him, and *without*  
 "*reluctance eat bread* which he threw down to  
 "him †." Both these persons, however, were af-  
 "terwards affected and fell victims to the disease."

2. AN aversion to water, though likewise usually mentioned as a symptom of the disease in these animals, does not always appear. Mr. Andrew Ellicot ‡, informed me, that he saw a dog in the height of the disease, swim across the river Petapasco near Baltimore. Dr. Hamilton likewise mentions two instances where dogs lapped water but a few hours before they died §.

THE only symptoms in the case of Mr. Rowley's dog, as related by Dr. Hamilton, was, that he looked poor and thin; this, however, in my opinion, is not so certain a sign as those which occurred

\* Hamilton's Remarks, p. 202.

† Med. Comment. Vol. III. p. 290.

‡ Geographer General to the United States,

§ Hamilton, p. 262.

curred in the dog that bit Mr. Bathie's boy, viz. a dulness, and inflammation of his eyes, and being avoided by the dogs that came near\*. This instinctive principle of self-preservation, wisely implanted in those animals, is sufficient to distinguish the actual presence of the disease, although it may not appear by any symptom whatever.

IN case, however, as it very frequently happens, that the dog which gave the bite is killed, and the unfortunate person wishes to know whether he was actually mad, we have an experiment related by Mr. Petit†, whereby our doubts may be fully ascertained. If a piece of meat be rubbed round the teeth and gums of a dog, that has been killed, and supposed to be infected, and given to another dog, he will eat it if the dog was free from infection, but reject it, if the disease existed in him. From the known fact of all dogs flying from an infected animal, I am disposed to think that Mr. Petit's observation is well founded, and therefore in such a case of doubt, it certainly deserves a fair trial.

Too

\* This observation generally attributed to Dr. James, was mentioned by Palmarius, de morfu canis rabidi, lib. cap. i.

† Acad. de Sciences, 1723. p. 39.

Too much, however, cannot be said against the practice of killing every dog supposed to be mad: Van Swieten\* indeed strongly advises it, and common opinion appears fully to concur with him. But I agree with Dr. Hamilton† in reprobating a practice, by which the truth can never be ascertained, while the person may be daily under the apprehension of an impending terrible disease. On the contrary, the dog supposed to be mad, should not be suffered to be at large, but immediately confined. This caution is more absolutely necessary, if a bite has been received, as a patient will thereby be enabled to judge from the termination of the dog's disease, whether he labours under any risk, and the propriety of undergoing a troublesome treatment.

## R E M O T E

\* Van Swieten Comment. Aph 1135

† Hamilton's Remarks, p. 153.

## REMOTE CAUSES IN DOGS.

THE Remote Causes generally laid down by authors, as producing a predisposition to this in dogs, and other brute animals, are,

- I. GREAT HEAT, *or* COLD.
- II. PUTRID ALIMENT.
- III. DEFICIENCY *of* WATER.
- IV. WANT *of* PERSPIRATION.
- V. WORM *under the* TONGUE.

I. OF all the Remote Causes enumerated, to none has more influence been attributed than *heat*; hence the disease is generally said to be most prevalent in warm countries: and Dr. Hilary says, “It is so frequently seen in the most hot countries, and especially in the West-Indies, that it may be said to be endemial\*.” Dr. Mosely, however, in opposition to this asserts, that it is “So far from being true, that if Hillary, who treats of it, and relates several cases that were under care, had not been a man of good character, I should have doubted whether he had ever seen a mad dog in the West-Indies. During my residence there, I never heard of the disease; and from the inquiries I have made, I am  
“ certain

\* Diseases of Barbadoes, p. 245.

“ certain that there has been no canine madness  
 “ in many of the islands for fifty years before the  
 “ year 1783\*.” In other countries equally warm  
 with the West Indies, the disease has never been  
 known. In South-America, Don Ulloa says, “ The  
 “ people there express their astonishment when on  
 “ European relates the melancholly effects of it. †”  
 The same fact is given on another authority ‡, and  
 noticed by Van Swieten ||. Mr. Desportes § also,  
 who practised physic in Hispaniola, from 1732 to  
 1748, relates that the disease was a stranger  
 there during that period. Mr. Volney ¶ likewise  
 informs us, that in Egypt and Syria, canine mad-  
 ness is unknown, and that Prosper Alpinus has  
 also made the same remark in his treatise on the  
 physic of the Egyptians.

ALTHOUGH the facts here stated may seem,  
 at first view, to militate against the common idea  
 of the influence of heat in the production of  
 the disease; yet I apprehend that the circum-  
 stance

\* Diseases of Tropical Climates, p. 32.

† Ulloa's Voyage, vol. I. p. 296.

‡ Biblioth. raisonne, 1750, Avril, May, Juin, p. 422.

|| Van Swieten's Commentat. on Boerh. Aphor. 1129.

§ Histoire de Malad. de St. Domingue. Mofely, p. 32.

¶ Travels, vol. I. p. 149.---Dub. 1791.

stance of its not prevailing in the above places, may be fully explained, and will be found not in the least to detract from the general influence of the cause. To account, therefore, for the exemption of those countries abovementioned from the disease, I should imagine that there was, and may be, some local cause operating, tending to prevent the appearance of it, by counteracting the effects of the heat. Thus, though Desportes did not see it, yet it broke out in the Spring of 1783 in Hispaniola, and in the month of June in Jamaica, where it raged until March, 1784. In other countries, also, the disease does not always prevail, although the heat of the weather be extreme, while at other times, under the same circumstances, it is universal. Thus although dogs do not go mad at present in Syria and Egypt; yet Mr. Volney tells us further, that the “ name of the malady is to be found in the “ Arabic language, and is not borrowed from any “ foreign tongue,” which plainly shews hat it was once known there. Dr. Mosely likewise informs us, that during the late war, in the West-Indies, “ many dogs were seized with the disease, “ which had no communication with each other; “ and some dogs that were brought from Europe “ and North-America, and that were not on shore

“ went mad on their arrival in the harbours of the  
 “ islands\*.” I am therefore disposed to embrace  
 Dr. Mosely’s idea of the cause of the exemp-  
 tion of the above places, and with him think  
 it is owing to some influence of the air.

BUT as I mentioned cold as a remote cause, this  
 may also appear to oppose the opinion of the ef-  
 fects of heat, in producing the disease. It is ac-  
 tually admitted for this purpose by Dr. Heysham†.  
 This, however, only proves that the same end  
 can be produced by two opposite causes. Heat  
 and cold, when applied in in a moderate degree,  
 produce the most different effects; but are attend-  
 ed with the production of the same general debi-  
 lity when applied in a violent manner. This  
 state I am disposed to believe, has a considerable  
 share in exciting the disease in dogs, as I shall  
 hereafter shew it has in men. The influence  
 of excessive cold or heat as equally favouring  
 the production of the disease was known at a very  
 early period of time, Ætius informs us, that it  
 was common in those countries where the vio-  
 lence of Winter and Summer, was equally ex-  
 cessive‡. During several hard Winters within

my

\* Mosely *Dif. of Trop. Climates*, p. 33.

† *Dissert. de Rabie*, cap. vi.

‡ Ætius, lib. vi. cap. 24.

Van Swieten *Aph.* 1134.

my remembrance in this city, dogs very commonly went mad. This was particularly the case in that of the year 1779-80, when more of those animals perished by the disease than for a long time before. Throughout Maryland, I am informed on very good authority, it was still more general. That dogs are capable of, and do actually labour under *debility* in the beginning of this complaint, is fully proved by their being affected with the same symptoms, which so clearly characterises the existence of that state in men; as aversion from motion, love of solitude, down-cast look, tendency to sleeping, &c.

SECOND. *Putrid aliment* is generally supposed to favour the production of the disease among brute animals, but this opinion is opposed both by Drs. Parry and Heysham, on the principle of hounds not being more liable to the disease, who are fed on carrion, than other animals, \* and because such food is agreeable to them †. But, although I do not suppose, that a dog by feeding on such aliment alone, would become mad, yet I must deny the position of those gentlemen, when they assert that such dogs are not more liable to the disease than others;

H 2

for

\* Parry dissert. inaug. de rabie contag. Edin. 1778.—Websteri prax. med. syst. tom. ii. p. 261.

† Heysham dissert. inaug. de rabie can. Edin. 1777. chap. vi.

for not only such aliment, but too high feeding also, favours the production of it. We had a remarkable proof of the influence of carrion eaten by dogs, in setting them mad some years since in this city. At the conclusion of the late war, and before that period, all the horses and other animals that died in the city, were carried out to the commons, and suffered to putrify there; and it is well known, that at this period, madness was a most common disease among the dogs, who used constantly to devour those carrion; but of late it more rarely occurs among them, since the former practice is not any longer suffered.

THIRD. *A deficiency of water*, has also been universally accounted one of the most common causes of this disease. Hence it has been said to prevail most in dry seasons, and countries; and so powerfully does the idea of its influence operate, that in some countries, it attracted the attention of government, and measures are accordingly taken to prevent the disease, by having the animals duly supplied with that article\*. But there are some circumstances

\* Dr. Mosely informs that in Venice they suppose it is often brought on by thirst; for which reason, barbers, shoemakers, &c. have a small tub of water always before their doors, that the dogs running about the streets may drink when they want, as there are no places in that city where they can otherwise supply themselves with fresh water. *Diseases of Tropical Climates*, p. 33.

stances to prove that there is but little connection between the production of the disease and the deficiency of water. For in the island of Antigua, where there are no springs, but all the water used is brought from the neighbouring islands, or caught when the rain falls, Dr. Parry \* asserts, on the authority of Dr. Samuel Athill of the above place, the disease is unknown.

FOURTH. *A want of perspiration*, has likewise been one of the causes to which the most powerful influence has been attributed in the appearance of the disease among dogs, and other animals: “The rabies or madness,” says Dr. Mead, “in a dog, is the effect of a violent fever: no dog ever sweats, from whence it follows, that when his blood is in a ferment, it cannot, as in other creatures, discharge itself upon the surface of the body; and must, therefore, of necessity, throw out a great many saline and active particles upon those parts where there is the most constant and easy secretion; and such, next to the miliary in the skin in us, are the salival glands †.”

I will

\* Differtat. inaug. Edin. 1778.—Websteri prax. med. 1yst. vol. ii, p. 261.

† Mead's works, p. 30.

I shall not stop to refute the erroneous opinions contained in this paragraph, as their fallacy will be readily seen, by any one acquainted with the improved state of physiology, at the present day, but will only observe, that the assertion of there being no perspiration in dogs is a mere hypothesis. The peculiar structure of their skin, together with the circumstance of its being covered with dirt, or dust, prevents the appearance of actual sweat; yet that do they perspire, and in a copious manner, is fully proved by the strong smell, that every one perceives on approaching them; and “by one of those animals being able to trace another by the scent of his footsteps, which could not happen if a large quantity of perspirable matter was not constantly going off\*.”

I would also remark, that the salivary discharge, is the most unfit secretion, to furnish an outlet to fluids requiring to be evacuated. In man, the discharge which is vicarious to that of the skin, is by the kidneys, or bowels: hence the old adage, “Cutis laxitas est alvi densitas;” and it accordingly happens, that on the obstruction of the perspiration, either a *diarrhœa* follows, or copious discharge of urine, and *vice versa*, those who have a free flow of perspiration,

\* Note to Monro’s comparative anatomy, in the new system, vol. iii, p. 347.

perspiration, have the secretion of urine diminished, and are habitually costive \* ;” but this is not observed in dogs. What reason, therefore, can be given for this variation in the performance of the same function in different animals? Do not similar laws govern the œconomy of all animated nature, under similar circumstances?

FIFTH. The last cause mentioned of this disease, was *a worm under the tongue*. Pliny † was the first author who took notice of this. Various subsequent writers, and even at the present time, when ignorance and superstition are nearly banished from the science of medicine, and given way to truth and reason, there are not wanting some “ who “ have paid it implicit obedience, and given to it a “ stupid belief ‡.” The idea of a worm is utterly false,

\* The frequent inclination to, and discharge of urine in dogs, may also seem to favour the idea of the defect of perspiration in those animals, by which a greater flow is determined to the kidneys: but I hope I have fully proved that this position is groundless, and the cause of this frequent expulsion of urine is owing to the greater acrimony of the secretion, and more muscular make, and less capacity of their bladders, by which they are unable to retain the urine secreted, so long as other animals, whose bladders are of a more membranous structure, and of greater dimensions.

† There is a worm in the tongue of dogs, says he, which is called by the Greeks Lytta; and this being taken out, when they are young whelps, they neither become mad, nor feel any sickness or loathing. Nat. Hist. lib. 29, cap. v.

‡ Hamilton’s remarks, p. 135.

false, as no such thing exists. Dr. Brodie \* says he never could discover, on dissection, any worm : yet Dr. Heysham †, who furnishes me with this authority, admits the idea of this substance being a cause of the disease, and approves of the vulgar practice of extirpating it. Others, who deny the existence of this supposed worm, assert that it is a gland, and secretes the venom which produces the disease: but no secretory duct has been seen, which being essential to the nature of a gland, this idea must then be equally futile ; neither is it a nerve, as supposed by Dr. James, but a spiral substance between the nature of a *ligament* and *tendon*, as shewn by that excellent anatomist Morgagni ‡.

THE nature of this substance being determined, let us examine into the actual merit of it, in the production of canine madness.

IN the first place, Dr. James, who was one among the few opposed to the doctrine, asserts, that not the least security is afforded to any dog by the extirpation of this substance. The great experience of this gentleman in the diseases of the canine tribe is well known, being as Dr. Hamilton says,

\* Dissert. inaug. p. 8.

† Dissert. inaug. Edin. 1777, cap. vi.

‡ Letter viii. art. 35.

says, “ a professed dog doctor,” and his opinion, therefore, merits the utmost attention. “ Dogs,” says he, “ thus treated, run mad equally with those who have never suffered this absurd operation\*.” Dr. Berkenhaut likewise treats it with ridicule, as having no foundation in truth †.” But, Dr. Hamilton’s testimony would alone have been sufficient to disprove the notion. He has fully shewn, from the testimony of a person of credit, at Ipswich, who has wormed many hundred dogs, that it afforded no security.

## I REMOTE

\* Treatise on canine madness, p. 204.

† Essay on the bite of a mad dog, quoted by Hamilton.

## REMOTE CAUSE IN MAN.

HAVING already in the preceding pages, endeavoured to refute the common idea, of the spontaneous production of this disease in the human body; the only remote cause that I can allow, is *a poison of a peculiar nature in the saliva of a rabid animal, belonging to the genus canis.* A question here arises, which it is necessary to determine. In what manner, or by what means, is the virus which produces this disease, communicated to the system?

THE idea of the peculiar subtlety, and penetrating activity of this poison, which has hitherto been generally entertained by physicians, has occasioned them to suppose many ways by which it was capable of entering the system. Some of these appear to be founded in imagination alone. There are other opinions, however, respecting the mode of introduction of the virus, which deserve some attention; but which likewise appear to be erroneous. These shall accordingly be noticed, and the action in the virus in the production of the disease finally pointed out.

I SHALL confine my observations to the following

ing modes, by which it is said, the poison can be received into the system.

1. BY absorption.
2. BY the breath drawn into the lungs.
3. BY contact with the saliva.

1. THAT the virus enters the system by absorption, and thus produces its specific effects, is an opinion, which has been entertained long before the discovery of the lymphatics; and although, at first view, this may seem a probable way of accounting for the production of the disease, yet I apprehend, on a more minute investigation, the idea will be found totally void of foundation.

IF an actual absorption of the virus took place, we should uniformly find, that it would stop at the first lymphatic gland, which was situated between the place of absorption and the common receptacle of the thoracic duct, and there cause a swelling and inflammation, similar to what is constantly observed to take place, in the absorption of the poisons producing the small pox, venereal disease; or of pus of any kind. No such appearance, however, has ever been noticed by the writers of any of the

cases on record. Dr. Hamilton \*, indeed, speaking of the pain felt in the course of the lymphatics, and in the axilla, or groin of the inoculated arm or leg, observes, “the same may be said of the  
 “venereal disease; and the same remark has been  
 “noted in the absorption of the poison from rabid  
 “animals.” But in all the histories, which I have consulted, with a direct reference to this circumstance, I have never found it mentioned; and in the many cases which Dr. Hamilton has abridged from various authors, and subjoined to his treatise, this affection is not taken notice of in any one of them. This, he certainly would not have omitted, had he met with it in a single case, inasmuch as it tended, in so decisive a manner, to have confirmed his assertion respecting the absorption of the poison. A pain in the bitten part, as I have frequently mentioned, is for the most part the first symptom of the general attack, but no pain in any of the lymphatic glands is ever noticed. Nay, Mr. Babbington, expressly observes, that the boy whose case he relates “complained of a pain in his right arm, (the  
 “bitten part) which was attentively examined, but  
 “without any discovery of inflammation, or enlarge-  
 “ment of the glands of the axilla.” Dr. Vaughan † likewise observes, that “the progress of the virus,  
 “towards

\* Hamilton's Remarks, p. 13.

† Vaughan's Cases and Obs. p. 48.

“ towards an admission into the system, cannot be discovered by diseased lymphatics, between the wound, and the next conglobate gland, or what is more common in the gland itself.”

BUT, granting that the virus is absorbed and carried into the circulation, yet still a difficulty remains in accounting for the symptoms of the disease. For, if like the contagion of the small pox and venereal disease, the canine virus enters the circulation, it would affect the arterial system, and produce an inflammatory state of the whole body. The pulse, would then become full and hard, the heat increased, and these symptoms would be accompanied by others which are well known to occur in inflammatory diseases. None of these symptoms, however, are observed to appear in the present disease; and the histories of numerous cases inform us, that the pulse is weak, quick, and intermitting, and that a fever seldom or ever occurs\*. The blood also, when drawn from persons labouring under the small pox, or any other inflammatory disease, seldom fails to be covered with a *buffy* coat or *size*, but this has never appeared in any case of the present disease; for repeated observation

\* Salius Diverfus, de feb. pest. p. 58.

Sauvage sur la rage, p. 37

Vaughan's Cases, p. 29.

fervation has shewn that it is no ways different from that drawn from a person in health\*.

INDEPENDENTLY of the want of similiarity, in the symptoms of the disease produced by the canine virus, with those which originate with an absorbed poison, the very great difference in the periods, at which the present disease appears, militates strongly against the idea of absorption. In every case of the transmission of a poison into the system, through the medium of the lymphatics, the greatest uniformity is observed. The small pox and venereal disease, have each their particular, and determinate periods of attack, from which they rarely depart in any climate, or constitution; but the canine poison is greatly influenced by both those circumstances, and has been known to infect, in all the intermediate periods, between the first day of a bite †, and nineteen months afterwards ‡.

IF the absorption of the poison be rejected, the stories related by Palmarius, of the disease being communicated by kissing a patient ill with the disease, must be without foundation. Nay,  
Dr.

\* *Philos. Transact.* vol. III. p. 276.

*Ibid.* Vol. XLVII. p. 413.

Morgagni, letter VIII, art. 30.

† *Edinb. Med. Com.* Vol. XI. p. 304.

‡ *Philosoph. Transf.* No. 445.

Dr. Vaughan has proved by actual experiment, the freedom from a morbid affection, in the saliva of a human person. He inoculated a dog with some, which was taken from a patient in this disease, but without producing any effect. He also says, that a nurse who was constantly with the child, whose case he relates, often kissed it, and received its breath full in her face, without any bad consequences. A person also used to put his finger into the mouth of Dr. Munckley's \* patient, in order to extract the viscid saliva, and felt no ill effect from the practice. But when we see that other poisons, whose absorption no one doubts, are not propagated by the blood or its secretions, as the small pox, and venereal disease †, which have never been communicated by inoculation with the blood or any of its secretions, why should it be credited as occurring in this disease, where there are so many probable arguments against the absorption of the virus ‡ ?

2. As to the propagation of the disease by the air drawn into the lungs ; nothing at first sight seems  
more

\* Med. Transf. Vol. II. p. 46.

† Hunter's Treatise on Ven. Dis. chap. i. sect. i.

‡ This freedom from infection in the secretions extends also to brute animals. A whole family nigh Chester-Town, Maryland, drank the milk of a cow, and the negroes on a farm ate the flesh of several hogs which died of this disease, without experiencing any inconvenience. Dr. Rush's Lectures.

more improbable. If it were possible for the poison to assume an aeriform state, of which, however, we have no proof, and be carried into the lungs; it would be expelled again in expiration; or if, by mixing with the saliva, it were conveyed into the stomach and bowels, its activity would be immediately destroyed by undergoing the digestive process. Other poisons, in a solid form, have been swallowed without any injury; as that of the venereal disease\*, small pox†, plague‡, viper||, ticunas §.

THE supposition of the disease being produced by mere contact of the saliva, being founded on the idea of absorption, needs no particular refutation, as I hope I have proved that no absorption takes place.

IT now becomes necessary for me to determine the mode of the introduction of the poison, and its manner of action on the system.

IN the first place, I deem a wound absolutely necessary for its operation. From a consideration of

\* Hunter's Treatise on Ven. Dis. part vi. chap. i.

† Rush's Inquiries, append. p. 7.

‡ Philosoph. Transf. No. 370.

|| Mead's Works, p. 37.

§ Bancroft's Hist. of Guiana p. 300, Lond. 1769

of the symptoms and nature of the disease, I am disposed to embrace the idea of the operation of the poison on the nerves, and think it can be better supported, than that of any other theory, hitherto offered.

THE poison, as existing in the saliva, when inserted by a wound into a part of the body, lies dormant for some time, and at length in various periods, in different persons, begins to shew its effects on the system at large. This opinion of the action of the poison on the nerves, is supported by the striking analogy subsisting between the present, and other nervous diseases, particularly tetanus.\*

## K

## I. IN

\* Morgagni seems to entertain the same idea, with respect to the action of the poison on the nerves; he remarks, “from the progression of the pain upwards, and from what Salius observed it to terminate in, a certain confusion, unsteadiness, and weakness of mind, the virus does not seem to be carried through the veins (by which vessels in his time, it was thought absorption was performed) but by the nerves up to their origins. Letter VIII. art. §2. Dr. Percival also ascribes the disease entirely to “nervous irritation.” Essays, vol. ii. p. 369—and Dr. Vaughan says, “we must seek for the action of the poison solely in the nervous system.” Cases and Obs. on Hydrop. p. 51.

1. IN both tetanus, and the disease consequent on the action of the canine virus, we may observe, that the same affection of the throat takes place, and the same morbid sensibility over the whole body,

2. THE pain at the pit of the stomach, and the rigidity of the muscles of the *abdomen*, which are such constant symptoms in tetanus, likewise occur in the present disease.

3. IN both these complaints, we may observe the same affection of the urinary organs, the same freedom from disease of the arterial system, and lastly, the same tendency to putrefaction, in the bodies of those who die of the disease. Morgagni \* and Sauvage † make this remark on those who die from the bite of a rabid animal: and Dr. Rush mentioned in his lectures, a case communicated to him by Dr. Hahnbaum, of South Carolina, of a negro boy, who died of a tetanus, becoming so putrid, that it was thought unsafe to open him, a few hours after his death.

BUT the paralytic affections, ‡ and numbness, || which have seized the bitten limbs, and the dimness of

\* Letter viii, art. 23.

† Nosolog. method. vol. i. p. 354.

‡ Mead's works, p. 661.

|| Med. Essays, Edinb. vol. v. part ii. p. 27.

of sight \*, and sometimes total blindness †, without any visible fault in the eyes, which are well known symptoms of nervous diseases, || admit not the least room, or suspicion for doubt, as to the action of the canine virus on the nerves.

IT was asserted, that the poison remained long dormant in the part where it was first inserted, and afterwards brought the whole system into sympathy. This, I apprehend, can clearly be proved. We see the same thing every day in other cases where topical affections of nervous and other parts, remain long without affecting the whole system, until the application of some cause renders them manifest.

DR. Percival § relates the case of “ a lady, who  
 “ had received a bruise on the *os sacrum*, by a fall  
 “ when she was young : she soon recovered from  
 “ its effects ; but eighteen years afterwards, the  
 “ rheumatism fixed on the part, was attended with  
 “ unusually excruciating pain, and long resisted the  
 “ remedies commonly employed, with much more  
 “ speedy success, in that disorder.” In a case of

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\* Med. Commun. vol. i. p. 214.

† Lond. Med. Enq. and Obs. vol. iii. p. 368.

|| Whytt's works, 4to. p. 622.

§ Percival's essays, vol. ii. p. 370.

obstinate head-ach, on which Dr. Rush \* was consulted, it came on 18 months after the stroke which caused it had been received; and my kinsman, Dr. Andrew Mease, observed when the influenza prevailed in the place of his residence, “that affections of the abdominal viscera, which had long lain dormant, were resuscitated by the disease.” † Cases of a similar nature, are frequently met with in practice, and in which there subsists a morbid local affection of certain parts, which are afterwards rendered manifest on the application of particular causes.

THAT the virus in the present disease, remains local in the part where it was first inserted, until the symptoms are produced, is confirmed by this fact, that persons have undergone general diseases, and the operation of general remedies subsequent to the bite; and yet the virus has afterwards shewn its effects on the system. Thus, there are repeated instances of persons having taken mercury as a preventative of the disease, and notwithstanding they had their systems fully impregnated with that mineral, have afterwards been seized with the disease.

\* Rush's lectures.

† Med. Commun. vol. i. p. 23.

ease \*. Mr. Nourse † also informs us, that he cut a boy for the stone, several months after receiving a bite, and never saw a wound more disposed to heal than in that case: the boy was abroad in five weeks after the operation, and yet was afterwards affected by the disease. Van Swieten † also takes notice of the local nature of the virus, and says, “it seems very surprizing, that the most considerable changes that can be made in our humours, should be so often neither able to expel the infection, nor yet move it into action.” Those, also, who have the misfortune to be bitten, perform all their functions equally well, as when in the most perfect health; until the poison comes into action: there can, therefore, be no doubt but that it remains in the part where it was originally inserted, until the application of some cause favours its producing the disease.

## P R O X-

\* Med. Obs. and Enq. vol. v. appendix, p. 2.

† Ibid, vol. iii. p. 356.

Hamilton's remarks, p. 49.

Med. Comment. vol. iii. p. 290.

Philos. Transf. No. 453.

† Comment. on Boerh. aph. 1137.

## PROXIMATE CAUSE.

WHEN treating of the action of the poison, I asserted that its operation was on the nerves, and supported this opinion, by the consideration of the symptoms being similar to those which occur in other cases, where those organs of the body are the seat of diseases. I deferred speaking of the peculiar manner in which the virus produced its specific effects, until this place, and now propose to enquire in what manner, or by what operation on the nerves, the poison excites the symptoms of this disease?

To attempt to determine the principle communicated to the nerves, by which the poison produces this disease, would be impracticable; all that we know of it, is from its effects: I assert, then, that the virus induces a general debility of the nerves, and deprives them of their healthy tone, and the customary energy, which they had over the whole system.

BUT, it may be asked, In what manner does the poison act? Is it by a *direct* operation that it produces its effects? and on which the indications of cure are grounded: or, is it by *indirect* means, that it excites the disease?

THOSE

THOSE who acknowledge themselves profelytes of the ingenious Bruno, will at once determine the canine virus to be endowed with a stimulating quality, as that author has all other poisons and contagions \*; and if they agree with me in my proximate cause, will explain its mode of action, and the symptoms induced, on the principle of its inducing that *debility*, depending on the application of *excessive stimulus*, and therefore denominated *indirect*; in contra-distinction to that, proceeding from the *abstraction* of usual stimuli, termed *direct*. But, although I firmly assent to the idea of the stimulant action of those contagions and poisons, which enter the system, in consequence of absorption by the lymphatics, as the small pox and venereal disease; yet as I have fully proved the impossibility of accounting for the symptoms of the present disease, on that principle, and cannot find the idea of the stimulant power of the poison, to be supported by the phenomena exhibited by the disease, I shall offer such arguments, as in my opinion, tend to invalidate the general application of the assertion.

1. If the virus producing this disease acted as a stimulant, even granting that it induced indirect debility, it must be evident, on the same principle that accounts for this mode of action, that previously

\* Elements of Medicine, sect. xxi.

previously to the induction of this state, the virus must exert its stimulant effects on the system, which will be shown by the production of a general intermediate excitement. The symptoms consequently following, would be similar to those that accompany other diseases, where this preternatural excitement is observed to take place, and the functions of the nervous system would be performed with greater force and energy in consequence of the vigour induced in it by the stimulant operation of the virus. Thus, in maniacs, where from other causes than a poison, the nerves are under this preternatural tone, we observe a surprising increase of strength, great insensibility to cold, ferocity of disposition, and constant delirium: While, on the contrary, in the present disease, the most opposite set of symptoms are observed from the beginning; as great timidity\*, extreme sensibility to cold, or the least variation in the temperature of the air, great languor and prostration of strength, the

\* Hence patients in this disease were called pantaphobi.

† Dr. Mead has related the case of a man, who, in a convulsive paroxysm of his disease, broke all the cords with which he was bound to the bed; but this is the only instance to be found of such apparent strength taking place; and even if it were a constant symptom, the action of the poison in producing debility, would not be invalidated, as the same increase of strength is observed in hysteric, and epileptic girls, who, although, when in health, are extremely weak, yet will require several strong men to hold them, when seized with an acute attack of those complaints.

paralytic affections \*, great difficulty of breathing, and a variety of other symptoms, which are well known to accompany diseases depending on a debility or relaxation of the nerves. If the poison acted by inducing indirect debility; during the state of excitement which must necessarily precede, debilitating remedies would be serviceable, and by preventing the progress to indirect debility, should cure the disease. But bleeding, and other evacuating remedies, have been used in every period of the complaint, and melancholy experience proves the injury sustained by their use.

It is therefore by a direct debilitating operation on the nervous system, that I suppose the virus to act in producing the disease. I well know it contradicts the theory of the above mentioned author, in whose opinion, all things in nature are stimulant †; and who also asserts that those causes which are of a debilitating nature, do not possess any positive power, but become hurtful by possessing a less degree of stimulus than is necessary to support animal life. This opinion, however, to me appears very erroneous; for there are many things capable of acting as direct sedatives on the human body, which do not contain a particle of stimulus. Of the truth of this

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assertion

\* Meads's works, p. 661.

Philosoph. Transf. Vol. III. p. 280.

Med. Essays, Edinb. Vol. V. part ii. p. 27

† Element. Med. Sect. XXXI.

assertion, nitre is a very remarkable instance. The universal use of this medicine, and the benefit derived from it, in inflammatory diseases, is a full proof of its direct sedative properties. If it produced the least stimulant effect, however small this may be, it must add the *proportion of that stimulus to the system*, and consequently increase the inflammatory diathesis already existing. After taking frequent doses of this medicine therefore, the disease, which it was intended to remove, would be increased. It should also prove useful by the same stimulant operation, in diseases of weakness, although only in a small degree; but the direct reverse of both these takes place, and from the moment it is taken into the stomach, and shews any operation, it does not increase the force or frequency of the pulse a single stroke, but produces a diminution of both. It creates at the same time, a sense of coldness in the stomach; and if its use be long continued, these symptoms are succeeded by the total destruction of the tone and vigour of that essential organ to our existence.

THERE are many other medicines which appear to possess a direct sedative power on the system, and whose effects from their first operation are followed by debility, without the least stimulant effect whatever.

EXCLUSIVE of the arguments, in favour of the disease depending on a relaxation, or want of customary energy in the nerves, derived from a consideration of the symptoms; other proofs shall be adduced of the truth of the same opinion, which I shall refer to the following heads.

I. Predisposing causes.

II. Analogy of the disease with Tetanus.

III. The injury of debilitating remedies.

I. THAT the disease originates from a general relaxation of the nervous system, I conclude from its production being favoured, and its power increased by the existence of debility, whether it be *natural*, and depends on peculiarity of organization, or *acquired* by the application of debilitating causes.

(I.) THE original debility of constitution, has a very considerable share of the influence in the production of this disease. When speaking of the causes of the comparative early, or late attack in different persons, I endeavoured to prove, that the variety in the time of the appearance of the symptoms, was proportioned to the *sensibility* of the nerves. This also, in my opinion, is in a direct ratio to the debility that prevails in those organs of the body.

I do not mean by this assertion to favour the *universality* of Dr. Brown's idea respecting the *excitability* of the system being always proportioned to the *direct debility* existing, and *vice versa*. For, although this principle is true, when applied to the *nerves*, and receives full confirmation from tetanus and the present disease; yet I am far from thinking it a general rule, as the idea, in a great number of the diseases of the *arterial system*, is contradicted by experience. In *typhus*, where there is the greatest direct debility, a very powerful stimulus is required to produce any sensible operation; while, on the contrary, a very slight stimulus will aggravate an inflammatory complaint\*.

WHEN I say, however, that the sensibility of the nerves is proportioned to their relaxation, or want of tone, I mean to confine myself, to their natural state; for when they are morbidly affected, the experience of *tetanus* shews, that however sensible the superficies of the body may be, to external stimuli;

\* It may be said, in opposition to this doctrine, that in palsies, where there is great want of tone in the nerves, there is also a defect of sensibility. But I would observe, that besides the want of tone, or morbid state of the nerves, they are also deprived of some principle, on which their power of communicating sensation depends, and of which we are altogether ignorant.

stimuli; yet that it requires a very powerful internal stimulus, to produce even a slight *impression*, and to counteract that, under which the nerves already labour.

THIS law of the system, is so universal, that I scarce know an exception to it. Women, and boys, both of whose constitutions are very generally much debilitated; and, as formerly remarked, have their nerves very easily excited, likewise are affected with this disease, at a much earlier period than men, in whom, from their possessing a greater degree of strength and less sensibility of nerves, the poison requires a much longer time to come into action. OLD people, also, whose nervous systems have lost their usual tone, possess a very great degree of sensibility, and are affected in a violent manner by certain stimuli, which in others, not so advanced in life, or in themselves some years before, would have produced no operation whatever. Hence the reason, why a few glasses of wine will inebriate a man, in the decline of life, who previously to passing his *acme*, would have borne a bottle without intoxication.

(2.) THE causes concerned in the production of  
*acquired*

*acquired* debility, may be divided into external and internal :

THE external, are violent heat and cold ; the internal, are, 1. debility from previous disease ; and, 2. depressing passions of the mind.

I formerly treated of the effects of heat in the production of a general debility, and consequent proportional sensibility of body, and therefore nothing need be adduced on that subject. Indeed, the fact is so well known, that it only requires to be mentioned, in order to be at once assented to. I shall therefore proceed to treat of the effects of cold in producing this debility, which proves a predisposing cause to the disease.

ALTHOUGH the effects of *heat* and *cold*, are known to be directly opposite, when applied in a moderate degree, yet it is no less certain, that they are attended with similar consequences, and produce the same debility when applied in an excessive degree. The operation of *cold*, however, in the production of a predisposition to this disease, only respects *brute* animals. For, the people who inhabit *cold* climates, by means of heated stove-rooms, the use of stimulating diet \*, and of fur

\* Such as frozen fish, fried in rancid whale oil, in which the greatest part of the diet of northern nations consists.

fur cloathing derived from the animals, with which Providence has kindly stocked their country, prevent the occurrence of that excessive debility, which the cold has a constant tendency to produce, and which, without the above precautions, would inevitably ensue. The effect of this debility, however, is seen in other animals, which from not being possessed of the necessary means to prevent the action of cold on their bodies, are as liable to the diseases as those of warm climates \*. Proofs of the influence of excessive cold, in producing the disease among dogs and other animals, having been formerly adduced, I shall proceed to treat of the *internal* causes concerned in the induction of the *acquired* debility mentioned, which was, first, by *Previous Disease*.

I HAVE already, as I hope, proved, that a general debility of the nervous system, when not *morbidly affected*, is always accompanied with a proportional excitability, or disposition to be acted on by external stimuli. It has also been frequently

\* Notwithstanding the means made use of by the inhabitants of northern climates, to prevent the *violent* effects of cold, yet they are not sufficient entirely to prevent their occurrence in a certain degree. These appear in the smallness of the stature of both man and beast, and the astonishing slowness in the contraction of the heart, which does not produce more than half the number of pulsations, that are perceived in an inhabitant of a more temperate climate.

quently mentioned, that in this disease, this disposition or principle of the body occurs in a remarkable manner. It will readily appear, therefore, how the debility which succeeds diseases in general, should possess such a considerable influence, in favouring the appearance of the disease, as it has actually been found to have. This will be rendered still more apparent, when it is considered, that this debility or relaxation, is universally of the nervous kind, and is also accompanied with the same morbid sensibility of the nerves. Hence the impressions from external objects, which in health would scarcely be noticed, produce the most disagreeable effects, as frequent startings, or slight convulsions.

MANY cases might be adduced, where this debility which succeeds diseases, has proved a predisposing cause to this and other nervous complaints; but a few only shall be mentioned.

ON the recovery from a severe illness, and after some slight irregularity of conduct, a tetanus has been frequently brought on, and in this case very commonly proved fatal. Of this Dr. Moseley\* gives us a remarkable instance. Many women have

\* Diseases of Tropical Climates, p. 485.

have dated the commencement of an involuntary disposition to faint, and an immediate attack of the hysteria, on the least sudden surprize, which has continued to afflict them the remainder of their lives; to a fright received during their convalescence after some disease, or in the very excitable state succeeding parturition. A melancholy case of a fatal locked-jaw, fell under my notice in this city, during the summer of 1787, in a lady, by being awakened out of her sleep, by the sudden arrival of her husband in the night. She was naturally possessed of an uncommon sensibility of constitution, and had been affected with symptoms of the puerperal fever for two days before the occurrence of the unhappy accident.

How the general debility, which arises, from previous disease, should favour the appearance of the present complaint, will be understood, when it is considered, that the nerves are then more liable to be affected by the irritation of the virus; which was prevented from coming into action before, by the healthy tone and vigour which they possessed. The celebrated Cocchi \* informs us, that he knew many who underwent the small pox subsequent to the reception of the bite of a  
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\* Bagni di Pisa, p. 319; Van Swieten's Comment. aphor. 1137.

mad dog, and died of its effects after their recovery from the former disease. The small pox, it is well known, leaves the whole system in a very debilitated and relaxed state; and in children, or those not arrived at maturity \*, a variety of diseases, depending on that cause, frequently follow, especially scrophulous swellings of the lymphatic glands, &c.

2. The second of the internal causes mentioned as producing debility, was depressing passions of the mind.

SUCH is the connexion subsisting between the *mind* and body and the influence they mutually possess over each other, that they have been very aptly compared, by a facetious author †, to a coat and its lining; if you rumple the one, you rumple the other. The history of medical cases likewise teaches us, that this observation is founded on experience. The reciprocal influence of those two component parts of our nature over each other is so very considerable, that a disease of the body, is affected in a most astonishing manner, by the state of the mind. The reverse of this remark is equally true. The plague affords a remarkable proof of

\* These it was remarked, are endowed naturally with a greater degree of sensibility, than adults.

† Sterne.

of the assertion. But in no instance, is the remark more strikingly verified than in those diseases which have their seat in the nerves. The propagation of these from the affections of the mind, is decisive in its authority, and tends also greatly to confirm the ideas advanced respecting the present disease. The case of the children in the poor-house at Haerlem, among whom the epilepsy spread, from a few others being admitted among them who were afflicted with that complaint, is well known. It yielded after some time to the great Boerhaave, by the judicious application of a remedy, suited to operate on their minds, after the failure of a host of medicines prescribed by other physicians, and intended to act on their bodies. Dr. Whytt \* also informs us, that frequently in the Edinburgh infirmary, women have been seized with hysteric fits from seeing others attacked with them. In the complaint, which is more particularly the subject of this dissertation, the influence of the the mind is no less remarkable. When formerly treating of the aversion from fluids, and the difficulty of swallowing them, I mentioned the power of the imagination in continuing that symptom; and the possibility of overcoming it, by an act of volition. It has likewise been found that those

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persons

\* Whytt's Works, p. 481. 4to.

persons who, from a knowledge of the effects consequent on the bite of a mad animal, have continued the apprehensions respecting their safety, or have been afflicted with grief, from any cause, were much sooner affected than others, who either from ignorance, or inattention, have never suffered the circumstance of the bite to dwell on their minds. In some persons, however, it must be acknowledged, that the disease has appeared in a short period of time after the bite, and who were entirely unconcerned about it; but this has been, for the most part, in those cases where the sensibility of the system, either from age or *idiosyncrasy*, has been adequate to the production of the same effects, as the debilitating operation of other persons continually reflecting on the probability of their being afflicted with the disease. In the case of Dr. Munckley's \* patient, the consequences of a settled dread and fear, in bringing on the symptoms, were very obvious. From the time of his being bit, until the period of the attack, he was afflicted with the greatest solicitude, and constantly laboured under the utmost anxiety of mind respecting his situation. A day or two previously to the appearance of the disease, he was observed to be more than usually melancholy.

Morgagni † relates the history of an old man, who

\* Med. Transf. Vol. II. p. 46.

† Letter viii, art. 27.

who had no symptoms of the disease, although, bitten four four months before, until after receiving some very ill usage. The boy, whose history is recorded by Dr. Dickson, perceived no indisposition until he heard that a person in the neighbourhood, who had been bit by the same dog as himself, died that day. A still greater proof of the effects of fear in bringing on this disease, is derived from a knowledge of the fact, that an actual *dread of fluids*, and convulsions at the mere sight of them, have come on by the influence of fear alone, and where the poison was not in the least concerned. Dr. Percival\* has given two remarkable cases where the operation of mental impression, from a bite being inflicted by a supposed mad dog, produced these symptoms: and an instance occurred in this city some years since, where the natural fears of a gentleman from receiving a bite, were increased to such a degree, by the improper suggestion of his physician, that *an actual dread of water* took place, and continued for several days. Finding, however, that without the use of any remedy, his apprehensions were groundless, his reason triumphed; and when he became convinced of his error, he laughed at his own credulity, and at the fright that was occasioned by the false prognostic of his physician.

FROM

\* *Essays Med. Philosoph. and Experiment*, Vol. II. p. 368.

“ FROM a careful perusal of Dr. Nugent’s case,  
 “ it may be discovered, that imagination, and an  
 “ apprehension of danger, formed the chief of the  
 “ symptoms which the Doctor attributed to real hy-  
 “ drophobia\*.”

II. The second general argument adduced to prove that the disease, at present under consideration, depends on a debility of the nervous system, was its analogy with tetanus.

I take it for granted that none will doubt the *nervous* nature of tetanus ; but it may appear necessary to prove that it also depends on debility, before I make use of its analogy with the present disease, in order to shew that the latter originates from the same cause. To attempt this, however, would be digressing too far from my subject. Indeed it would be unnecessary, as it has already been so amply demonstrated by Dr. Rush†, who both by reasoning, and what is still more decisive, the success of the tonic plan of treatment, has rendered the matter beyond all doubt. I shall therefore proceed to make use of the supposition of tetanus depending on debility as an established truth.

In

\* Hamilton’s Remarks, p. 225.

† Vide Medical Inquiries and Observations Philadelphia, 1789, p. 169  
 A clear and decided proof of the injurious treatment of the old practice, and the success of the tonic plan, may also be seen by referring to a case related by the late Dr. Hahnbaum, of Charleston, which

IN a former part of this dissertation, I noticed the similitude which prevailed between the symptoms of these diseases; and I shall now add a few, of many more particulars, in which they agree.

1. Both these diseases prevail for the most part in *warm climates* and *seasons*, and both are propagated in *different* periods of time, after the application of their *respective causes*, in proportion to the *greater* or *less* sensibility of the system.

2. Both are rendered more fatal, by the use of debilitating remedies. From the erroneous ideas entertained respecting the pathology of these diseases, the spasmodic affections and convulsions observed to occur, were attributed to an *excess* of strength. The most powerful debilitating remedies were accordingly made use of for their removal. But, instead of this apparent strength being the consequence of *too much* vigour, it is actually the effect of a *deficiency* of strength. It is well known, that “in the greatest debility, and even a short  
“time before death, spasms and convulsions are  
“wont

which together with some remarks I inserted in the American Museum, for August 1791. For proofs and cases of the success of the tonic plan of treatment in this disease, I would also refer to Mem. Lond. Med. Soc. vol. II. p. 108, 114.—Trans. Royal Acad. at Vienna, Vol. I. in each of which several cases of the efficacy of the invigorating mode of treatment are given.

“wont to occur.\*” Notwithstanding I am indebted to Dr. Boerhaave for this remark, the same author was led into an error, by the apparent strength shewn by persons labouring under this disease, when he says, that it should be considered as “*summe inflammatorius.*” These convulsions do not arise “because the force of the muscles in “*contracting* themselves is encreased, but because “the force of the antagonists is diminished.†” Hippocrates, likewise, very early has noticed the occurrence of convulsions after hæmorrhages, and their uniform fatality. Hoffman appears to have been of the same opinion, when he says, “*atonia* “*gignet spasmos.*” So far are these spasms and convulsions from depending on real excess of strength, that they are evidently morbid, and deserve as much to be accounted so, as the apparent debility which takes place in *pneumonia*, rheumatism, or other inflammatory complaints. In these, the patients can neither move hand nor foot; and forming our judgment from fallacious appearances, bark and wine might with the same propriety be prescribed to remove this debility; as bleeding and other evacuations to cure the apparent strength in the former diseases.

BUT

\* Boerh. inst. sect. 401.

† Morgagni Letter x. art. 20.

BUT that the spasms and convulsions in tetanus depend on debility, requires no other proof, than the *death* and *destruction* which have in every case followed the *sedative* mode of treatment, and the speedy return to health by the use of tonic or invigorating remedies.\*

FROM this view of the analogy subsisting between tetanus, and the disease produced by the action of the canine virus on the system, it must appear, that although they are essentially different in their *remote*, they are very nearly related to each other in their *proximate* cause. No doubt, the presence of the virus in the one case, is the cause of the greater permanency of the symptoms in the disease produced by it, and may occasion some peculiarity in the appearances, in addition to those which take place in tetanus. This, however, only shews that the *same effect* can be produced by *two*

N *different*

\* Although the injury of bleeding in the disease arising from the action of the canine virus, has been shewn by its uniform failure; yet the other part of the argument cannot be made use of to prove still further, that *debility* is its cause. I apprehend, however, that no other proof would be required; notwithstanding no case can be produced of the success of tonics in the *cure* of the disease, yet the probability of their utility will scarce be questioned after the fatality which has been shewn to attend an opposite mode of treatment, and their success in tetanus, whose affinity with the present disease I have already pointed out.

*different causes*, a circumstance which very frequently takes place in other operations of nature. No alteration, therefore, in the treatment of the disease depending on the canine virus, is necessary from that which has been proved to be so successful in tetanus. The history of other poisons also shew that the same state can be produced by two different causes, and yet the same remedies have been found necessary. Thus, in those eruptive diseases, whose remote causes are certain specific contagions, an inflammatory *diathesis* is as certainly induced, as by exposure of the body to alternations of heat and cold. The small-pox and measles afford a striking proof of this assertion. In these diseases, no particular complexion in the treatment is derived from the presence of the contagion, different from the *synocha*, or simple inflammatory fever: Why, then, should the remedies of the disease produced by the canine virus vary from those used in tetanus?

THE only difference subsisting between the *two* diseases, originating from contagion and the other simple affections, is, that in the case of the *small pox*, a *less* degree of the same inflammatory state is induced than that which occurs in *synocha*; while, in the disease produced by the *canine virus*, the same state which occurs in *tetanus* is also brought on,

but

but in a *greater* degree. In the *small pox*, therefore, a *less* use is required of the same *antiphlogistic* means which are proper in the *simple* inflammatory fever; in the disease depending on the *canine virus*, a *more vigorous* and extensive exhibition is required of the *same* remedies which are used in tetanus.

III. The third and last argument advanced to prove that the disease depends on debility, was the injury of debilitating remedies. I have anticipated myself, however, on this head, by proving the truth of the assertion, when treating on the analogy of the present disease with tetanus. I shall, therefore, defer speaking any thing further on the subject at this time, especially as I shall have occasion to prove the fatality attending their use, when I come to treat of the remedies hitherto used for the cure of the disease.

## M E T H O D   O F   C U R E.

To establish a general system for the cure of this disease, two indications are immediately pointed out.

1. To prevent the poison from being communicated to the system.
2. To counteract, or overcome its effects, after they have began to appear.

IN order to answer the first indication, there have been a variety of external remedies made use of. The first to be mentioned is the excision of the bitten part. Where the wound happens to be so situated, that the part in which the bite was inflicted, can with propriety be cut out, every one will allow, that this operation must afford the greatest security: it ought therefore always to be preferred. But there are many circumstances which may concur to prevent its accomplishment. The wound is often inflicted deep in a muscular part, where the excision of so much flesh would be attended with great inconvenience.—  
 “ Much time may be lost before the surgeon arrives; the sufferer may long resist all sollicitations  
 “ ons

“ ons to submit to the knife; the wound may have  
 “ been inflicted on the face, or near some large  
 “ blood vessel; or there may be so little probabi-  
 “ bility of the madness of the dog, as to render  
 “ it unjustifiable to subject the patient to present  
 “ pain, or future deformity\*.” To the applica-  
 tion of the cautery there are still more valid objec-  
 tions. The intensity of the pain attending the ope-  
 ration would be such, as to prevent numbers from  
 submitting to it; and the idea of this would ope-  
 rate so forcibly with many, that they would rather  
 take the chance of escaping the disease, than suffer  
 the protracted tortures of a hot iron. The idea of  
 subsequent deformity, also, would operate power-  
 fully, and this alone would be an insuperable bar to  
 its employment.

THE application of the *caustic*, as advised by  
 many late writers, has failed in cases where it had  
 unequivocally the fairest trial, and therefore does  
 not seem intitled to our faith. In the case of Ad-  
 miral Rowley's son†, to which I have had frequent  
 occasion to refer in the course of this dissertation,  
 the *caustic* was applied to the part immediately after  
 the bite, and by the hand of the very judicious  
 Mr.

\* Percival's Essays, Vol. II. p. 375.

† Hamilton's Remarks, p. 221.

Mr. Hunter ; the disease nevertheless came on, and, as usual, proved fatal.

VARIOUS other applications to the bitten part have been recommended. It may not, therefore, be amiss to take notice of a few of the most noted, as it will serve to reconcile the prejudices in favour of particular remedies, and excite persons to the use of others, when that which they most approve, may not be near at hand, at the time it is required. The mercurial ointment is recommended by many, particularly Sauvage\*. Red precipitate and sublimate has also been used †. Common salt has long since been highly commended, and additional proofs of its efficacy, have within a short time, been presented to the public by Dr. Gale of Connecticut ‡. The solution of the common caustic in water, has likewise been greatly extolled. To determine the superiority of these applications, would be impossible, as it must be evident they all act on the same principle, by raising an inflammation and suppuration in the wound, and by preventing it from healing, causes a discharge of the virus with the pus from the bitten part.

THERE

\* Sauvage Nosolog. Method. tom ii. p. 236.

† Palmanus de morb. contag. p. 272.

‡ Newhaven, Connecticut Med Soc. Transf.

THERE is also another application yet to be noticed which is intended to create a discharge, but not by an inflammation, unless long continued: this is the use of a long continued stream of cold water, poured on the wound. from a considerable height, from the mouth of a tea kettle. This plan was first proposed by the benevolent Dr. Haygarth, of Chester, in England, and is strongly recommended by Dr. Percival\*; it has likewise received the sanction of the late Dr. John Morgan, the honourable Arthur Lee, esq. and Dr. Samuel L. Mitchel, who separately published recommendations of the practice in all the newspapers of this country. I am disposed likewise to entertain the most sanguine hopes from a proper use of this simple application, as none of the arguments mentioned against the use of the former applications can be applied to this; no situation of the wound or part of the body on which it is inflicted, can be urged as a reason for its omission. The poison also we know exists in a *watery form*, and therefore, we should reasonably expect that water would be its most proper solvent. “ The preference  
 “ given to cold water for the first ablution is judi-  
 “ cious, and accords with the idea above advanced,  
 “ that the nerves are the parts alone injured by  
 “ the

\* Percival's Essays, vol. II. 372—3.

“ the canine *virus*. They may thus perhaps be  
 “ rendered torpid, and the virus may be greatly di-  
 “ luted, or washed away, before they recover  
 “ such sensibility as to be capable of suffering  
 “ from its action. When this has been sufficiently  
 “ applied, warm water should be used, not only as  
 “ a better solvent, but to produce a flow of blood ;  
 “ which coming from numberless small vessels, may  
 “ tend to complete the cleansing of the wound\*.” If  
 the wound received be but small, and there remains  
 any doubt respecting the possibility of the water  
 coming sufficiently to all parts of it, a slight enlarge-  
 ment of it with a scalpel or lancet, will prove use-  
 ful, and this can be so done, as not to create de-  
 deformity ; the wound also might be suffered to  
 bleed, and a continued use of the water would then  
 afford perfect security, from the disease.

THE wound, however, ought by no means to be  
 suffered to be healed suddenly, but should be kept  
 open for some time as the surest means of prevent-  
 ing the constitutional affection. For it has been  
 remarked that persons bitten by dogs, or other  
 mad animals, who have had their wounds kept  
 open, either by design or accident, remained free  
 from the disease ; while others, whose wounds have  
 healed, became affected with it.

A re-

\* Percival's Essays, vol. ii. p. 372.

A remarkable instance of this, is related by Galen.\*—Two men were bitten by the same dog, one recovered, in consequence of his wound being prevented from healing; the other, from not taking this precaution, and suffering the wound to close, died of the disease. The cases recorded by Dr. Fothergill, afford a striking proof of the same observation: Mr. Bellamy was bitten by the same cat as his servant maid; the wound of the former closed in a short time; but that of the latter not only continued open, but baffled the skill of a surgeon, to whom she applied to have it healed †. Mr. Bellamy was attacked with the disease, and died of it, but the girl remained well.

EVEN in case the wound should heal, and several weeks elapse before any remedy had been made use of, I am of opinion, that it should be opened, and prevented from closing, as we know it takes very different times for the poison to shew its effects in different constitutions, and therefore the probability will be in favour of the person escaping the disease from this treatment.

MANY general remedies, intended to prevent the disease, have been recommended; but  
 O they

\* Galen de fectis, tom. ii, p. 293.

Van Swieten com. aph. 1143.

† Fothergill's works, p. 353.

they “ rather serve to shew the credulity of their  
 “ authors, than to furnish us with proper means  
 “ of combating the dreadful consequences which  
 “ follow \*” the action of the canine virus on the  
 system. The use of the cold bath has been extolled  
 from the earliest ages, but there is no case that can  
 be depended on where it prevented the disease,  
 and there are an hundred that can be produced to  
 the contrary. The Tonquin remedy has been equal-  
 ly unsuccessful. Master Rowley was attacked with  
 the disease, during the use of it ; and our books  
 of medicine abound with many other instances of its  
 failure. Near fifty years experience has proved,  
 that the *lichen cinerius terrestris* of Dr. Mead, is to-  
 tally useless : it is nevertheless absurdly retained  
 at this day in some European pharmacopœias.

ANOTHER remedy more deserving of a particu-  
 lar attention, on account of the praises which have  
 been bestowed on it, for its supposed success, in  
 preventing the disease, is the *Ormskirk* medicine.  
 But notwithstanding the eulogium pronounced on  
 it by Dr. Heysham, I cannot help ranking it with the  
 many others by which the public have been duped.  
 Repeated experience has shewn that it is equally  
 inert with any that has been mentioned. I have no  
 doubt,

\* Vaughan's cases and obs. p. 40.

doubt, however, that the credit which this as well as many other remedies have obtained, was founded on the supposed experience of their success, from persons taking the medicine, and who have remained free from the disease. But this freedom from infection is not owing to any virtue in the medicine taken, but to other circumstances.

ONE great cause of the celebrity of the Ormkirk, and many other remedies, has been the circumstance of not one dog in an hundred being actually mad, from which a bite is received. These animals, impelled by the principle of self-preservation, are frequently obliged to commit this violence by making use of the only means of defence with which Providence has furnished them, when attacked on all sides by an ignorant rabble, who are more mad than the dog they pursue. Dogs may, indeed, especially in summer, have some of the symptoms of canine madness, as frothing at the mouth, panting, lolling out the tongue; but these may arise from their violent exercise, in pursuing their lost masters, or, in returning home from a journey.

HUNDREDS of persons, after receiving bites from dogs in such a situation, have taken the Oermskirk medicine, and a variety of others, and by remaining free from disease, have supposed it to be owing to the medicine, when they would have been equally secure without them.

BUT exclusive of the fallacy of the experience, with respect to the supposed efficacy of this medicine, drawn from such cases as the above, there is another consideration which helps to account for the exemption of persons from the disease after taking it, and clearly shews the impropriety of ascribing it to that vaunted nostrum: for however fatal the effects of the poison have hitherto been, when these have occurred, it fortunately happens, that by far the greatest number of those who are bitten by dogs or other animals actually mad, are never seized with the disease. This observation has been frequently made, and admits of no suspicion as to its accuracy. Thus Cocchi \* relates, that among several persons bitten at the same time, and by the same dog, some died, notwithstanding the most noted methods of cure had been used, and that others again remained perfectly well, although they

\* Bagni di Pisa, p. 318.

Van Swieten comment. aph. 1137.

they underwent no manner of treatment. Dr. Vaughan informs us\*, that “ of between twenty  
 “ and thirty persons, who were bitten by the dog  
 “ which gave the fatal wound to the boy whose  
 “ case he records, not one felt the least ill effect but  
 “ himself.” “ I know, says Mr. Hunter †, where  
 “ there were twenty-one people bitten by one dog,  
 “ nothing was done for any of them, and only one  
 “ was taken ill: if they had all taken medicine, then  
 “ it would have been said, that they only lost *one* out  
 of “ *twenty-one.*” In a letter formerly referred to,  
 and published by Dr. Houlston ‡, it is said, that out  
 of *nine* persons bitten by the same dog, only *one*  
 was taken ill. If all the persons in the above cases  
 had taken any medicines, the most unequivocal  
 proofs would have been thought to have been ex-  
 hibited of their efficacy.

THESE facts, while they serve to shew in a deci-  
 ded manner, the fallacy of the experience sup-  
 posed efficacy of preventative remedies, at the  
 same time afford the most comfortable hope to  
 those who may have the misfortune to be bitten  
 by a mad animal.

MER-

\* Vaughan's cases and obs. p. 56.

† Letter to Dr. Hamilton—remarks, p. 213.

‡ Lond. Med. Journ. Vol. VI.

MERCURY, though it has been said to have had a fair trial, in my opinion, has never been properly exhibited. After a few days or weeks use, it has been omitted, and from the disease appearing afterwards an unfavourable opinion was formed of it. If the use of the mercury could be continued long enough, and until the period of the commencement of the action of the poison, I have no doubt but it would prove successful. But the distance of time between the infliction of the bite and the attack, has been shewn to be very different, and it is impossible to tell how long it may be necessary to give it; its use, therefore, as a preventative, ought certainly to be laid aside.

THE method I would recommend, therefore, in case the bitten part is healed, and no application has been made, is the following.

AFTER applying a caustic to the wound, it ought to be prevented from healing; whereby the poison will be evacuated; for until the time of its action, there is great reason to suppose that it lays in the part where it was originally inserted. The use of bark ought then to be begun, and continued, until the common period has passed, at which the symptoms generally commence. Preparations of iron, and particularly the prepared steel, may be  
 advan-

advantageously joined to the bark. By the use of these medicines, such a degree of vigour will be given to the system, as will prevent the action of the virus from taking place; or, if this should actually come on, it must be evident that they will be slight, and consequently greater hopes may be entertained, that the disease will be overcome, than if the system was not under the operation of so powerful a tonic.

HAVING thus treated of the various preventative means hitherto recommended by physicians for this disease, I shall now proceed to the second indication pointed out, viz. to counteract or overcome the effects of the poison when they have began to be exerted on the system. On a consideration of the means advised heretofore, and actually put into execution, it will be found that they are equally useless with those, that I have just been considering as advised for its prevention. From the erroneous ideas entertained concerning the cause of the convulsions which occur in this disease, and the apparent strength exhibited by those labouring under it, Boerhaave, and others, as formerly observed, have considered it as highly inflammatory; and of course ordered copious bleeding, smart purging, and the whole of the antiphlogistic process to be strictly observed. The uniform practice of physicians, has been agreeable to  
that

that absurd theory. So far has the ideas of the inflammatory nature of this disease carried authors, that bleeding has been directed to be performed, not with a sparing hand, but again and again even unto fainting. Although the *uniform* failure of the remedy, and the constant subsequent increase of the spasms, and diminution of the pulse pointed out the absurdity of the practice, yet the continuance of the symptoms was not attributed to the mode of treatment, but to the obstinacy of the disease.

“ How far bleeding is indicated, Dr. Hamilton\*  
 “ remarks, I dare not yet venture to say. Dr. Fothergill, and other eminent practitioners, used it  
 “ with freedom. It is also powerfully antispasmodic; but it is at the same time powerfully debilitating. Here, then seems to be as much against  
 “ it as for it: and the pro and con are so equally  
 “ poised, that we are at some loss which side to  
 “ espouse.” Without further reasoning, I would observe, that my opposition to the remedy is founded on its *want of success*. Let this be candidly examined, and then see on which side the scale will turn. Dr. Fothergill it is true, and almost all other practitioners, have employed it largely; but with *what success*? Have they cured the disease? The  
 numerous

\* Hamilton's Remarks, p. 117.

numerous cafes recorded cry out *No. Death and destruction* have followed as *surely* and *invariably* in every cafe where it was employed, as from a stab in the heart with a fmall fword. I defy a fingle inftance of the real difeafe to be produced, where either a fymptom was relieved, or a cure effected by it. Cafes are indeed related by feveral authors, where it was ufed with *other remedies*, and the patient has recovered. But when a number of means are employed, is it rational to afcribe the fuccefs derived from their ufe to any *one*; efpecially when the operation of them is directly oppofed to each other, as in the prefent inftance? Thus bleeding, mufk, opium, the warm bath, and fometimes mercury, have all been employed in the treatment of the fame cafe.

BUT let the circumftances of thefe fupposed fuccefsful cafes be examined. The firft is that related by Dr. Nugent; he ufed the three firft remedies: but I have before rendered it probable, that this cafe was the effect of an hysteric paroxyfm. Dr. Tilton's fupposed cafe of this difeafe, which was alfo cured by very copious bleeding, was fhewn to be a violent hysteria bordering on mania. Mr. Wrightfon\* alfo mentions a fuccefsful termination of a fupposed cafe of this difeafe, by the ufe

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of

\* Med. Tranf. vol. ii.

of the same remedies. But I am of opinion with Dr Hamilton, that this was only a temporary phrenzy, brought on, as in the former case, by the effects of fear ; for two reasons—first, because it came on in three days after the bite, which is a much earlier period than usually happens ; and secondly, because it terminated favourably after the use of the remedies which have failed in every case of the actual disease.

ANOTHER means more frequently employed of late in this disease, is the *warm bath*. From this, in a few cases, benefit appears to have been derived while the patient was in the bath, but it was only a temporary alleviation ; for it has been remarked, that whenever the water was the least ruffled, so as to touch a fresh surface\*, the convulsions were again excited ; by rendering the body also more irritable to the external air, it has finally increased the disease, by adding to a symptom the most distressing that occurs in the complaint. For these reasons, in my opinion, the warm bath ought never to be used.

I have

\* Vaughan's Cases and Obs. p. 33.

The warm bath, though generally used in tetanus also, has been attended with no better success than in the present disease. Dr. Cullen says, it has even occasioned death in some cases. First Lines, Vol III. p. 304.

I have thus taken notice of the various preventative and curative means hitherto repeatedly tried in this disease, and have found, that the *further* use of *none* of these is warranted by any good effect derived from them; it becomes necessary, therefore, that I should point out the mode I would recommend in the treatment of the complaint, as any endeavours to destroy confidence, without giving grounds for fresh hopes, would be attended with little benefit to society. The establishment, then, of a mode of cure will be very readily done, if it can be granted that the effect of the remote cause is the production of the proximate. I have already shewn that the *only* remote cause of this disease is the *poison*; and that this acts on the *nerves* by a *debilitating* operation, whereby they are deprived of their healthy vigour and tone.

I supported this opinion by proving—that the predisposing causes of the disease were of a *highly debilitating nature*—by its analogy with *other diseases*, acknowledged to depend on the *same cause*, to which the *present* was referred—and lastly, proceeding according to the the strictest laws of philosophical induction, the truth of the opinion was established from the *injury of debilitating remedies*. I dwelt on the *similarity* of the *present* disease with *tetanus*; and the analogy then mentioned is

further strengthened by the fact, that *both* are prevented by the *same* remedies.

WHEN a puncture from a nail, or a wound in a nervous part, is received, a *locked jaw* and a general irritation of the nerves is prevented from taking place, by the *local irritation* of the nerves of the wounded part, and the tone given to them by stimulating applications. In the case of the bite of a mad animal, the *same local* applications also prevent the *general* effects of the *canine virus*, by inducing its discharge from the system. In both these cases, therefore, we see that the same remedies eventually obviate the occurrence of the *same proximate* cause, though on different principles with regard to the *remote* \*.

## OUR

\* The effect of an irritation raised in *one* part of the body removing that which already exists in *another* part, admits of very extensive application in medicine. This principle, which was first discovered by Mr. John Hunter, is frequently verified in the cure of diseases. A violent hiccoughing, which is known to arise from the contraction of the diaphragm, has yielded to the application of *vol. alkal.* to the nose. The stimulus arising from a pair of blisters to the thighs has restrained a vomiting in the bilious fever, which had resisted for two days all the remedies commonly employed with success in that case. Of this I have related a remarkable instance, in the American Museum, for Oct. 1790, in the observations on the weather and diseases of  
this

OUR views, then, in the cure of the constitutional disease should be,

1. To diminish the morbid sensibility of the system; and,
2. To restore that degree of vigour which it had lost in consequence of the action of the poison on it.

THE propriety of the first indication will be very readily perceived, when it is considered, that the effects of the poison on the system are to induce in it a morbid sensibility, and that the violence of the disease will be just in proportion to the degree of that existing. Whatever, therefore, will have a tendency to diminish this sensibility, or render the system less susceptible of the irritation of the poison, must be of very great importance in the cure.

ON the first attack, therefore, of the disease, such medicines as are known to possess this property of diminishing sensibility should be freely given: of these

this city, which I published, monthly, in that work, during the above and succeeding year. The utility of the application was first pointed out by Dr. Quier, of Jamaica, in his treatise on the bilious fever of the West-Indies.

these OPIUM is the most proper. But, from the violent irritation under which the nerves labour, the usual effects of the opium are not produced, unless taken in large quantities. For this reason, the first dose ought to consist of several grains, that a check may be at once given to the symptoms; and in order to derive any further benefit from its use, it should be gradually encreased to ten or fifteen grains, and occasionally repeated in double that quantity in the course of the disease, as often as a former dose has ceased to produce its effects on the system\*.

OPIUM, though it has been trusted to alone for the cure of this disease, yet experience proves that

\* The quantity of opium that a person in this disease can take with scarce any effect is really astonishing. Dr. Vaughan relates, that he gave 57 grains in the course of 14 hours, with scarce any advantage. The same observation is applicable to the tetanus, in which disease fifteen hundred grains, or three ounces and a drachm, were given in one case, in the course of 17 days, in the island of Antigua, with success. Amer. Philos. Transf. vol. i. p. 315. In the Mem. Lond. Med. Soc. vol. ii. there is also a case related, where several hundred drops of laudanum was taken in the course of 24 hours, combined with antimonial wine, which cured a spasmodic affection, proceeding from the puncture of a hair-pin in the thumb. In one day, particularly, near a thousand drops were taken.

that it is only equal to the palliation of the symptoms, of which advantage is to be taken by the exhibition of other remedies. Injected in a liquid form into the bowels it may exert its good effects on the alimentary canal, and the whole system in general, when it cannot from the difficulty of swallowing be taken by the mouth.

As a means of diminishing the morbid sensibility of the system, especially of the surface, frictions of the body with *oil*, appear to promise much benefit. Celsus,\* and other ancient authors,† mention the practice of immersing patients in a *bath* of oil in this disease, with a view of allaying the spasms, but it has long been neglected. Dr. Sims, of London, however, we have been lately informed, has renewed the practice; and by bathing the whole body of a patient in this disease for three days with oil, and also by a liberal use of it internally, it is said, effected a cure. I very readily subscribe to the promised utility of the practice, both from reflecting on the probable effects to be derived from it, and also from the very remarkable benefit that has attended its use in other spasmodic diseases and affections. In the tetanus, whose great familiarity with the present disease has been frequently

\* Celsus, lib. x. chap. ii.

† Arteus Cappadox de curat. morb. chap. vi.

quently mentioned, Dr. Blane\* informs us, on the authority of Dr. Warren, “that the uneasiness arising from the spasms was allayed by drawing a feather wetted with oil over the temples.” Morgagni† relates a case from another author, where a bath of warm oil had an evident good effect in quieting convulsions that proceeded from the vapours of a mineral poison. Every consideration argues much in its favour, and therefore further trials deserve to be made of it: additional efficacy will be derived by the oil being warm, as it will become more agreeable, and in allaying the extreme sensibility of the nerves on the superficies, which is one of the most troublesome and distressing symptoms attending the disease. The COLDBATH, though hitherto unsuccessful in the cure of the disease, I am disposed to think, if properly managed, might be used also with advantage. Instead of half drowning the patient by a forcible immersion, as generally advised, the water should be employed by way of *affusion*, and frictions made use of afterwards. The horror of water is no objection, as the same takes place in tetanus, and from the cure of that disorder by the cold bath, I am inclined to think benefit would be derived from it in the present disease.

2. To

\* Diseases of seamen, p. 491.

† Letter x. art. 21.

2. To restore the tone of the system, which it had lost in consequence of the action of the poison on it, the various medicines, called stimulants and tonics, must be used.

IN the commencement of the disease, when the power of swallowing may be as yet free, the bark should be exhibited in as great a quantity as the stomach can bear. One or two drachms may be given in the course of an hour in wine, until they begin to loose their effects on the system, when recourse should be had to some means to render them still more powerful; the wine, therefore, may be given hot. After the bark and wine have ceased to operate, they may be alternated with other powerful medicines of the same class. A constant state of excitement may by these means be kept up, and the bad effects arising from the system's sinking, from the omission of any, be avoided. In this manner therefore, the whole class of stimulants or tonics should be gone through, and after the use of all of them, the first that was exhibited may again be given with equal benefit as at first.

IN order to produce a more durable impression, and at the same time that nourishment is conveyed into the system, a considerable degree of stimulus may also be exerted; hot broths should be

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freely

freely given; they may be rendered more stimulating by the addition of some of the aromatic condiments, as pepper or allspice.

As the duration of the disease is but short, every possible advantage ought to be taken. By the omission of the medicines and nourishment for a few hours, the system may sink so far, as to put it out of our power to bring it up to the same point of vigour to which it had formerly arrived. It is only by keeping the system under the *uniform and powerful impression of these tonic medicines*, that I apprehend any good will be derived from them. By these means, the morbid state induced by the poison may be overcome; and if this be prevented from recurring again by a due continuance of the same remedies, I apprehend this dreadful disease may be cured. For this reason the medicines and nourishment should be exhibited during the night, and if a tendency to sleep be perceived, a large dose of opium may be given. To timid minds or those unacquainted with the nature of the complaint, and what a powerful stimulus it requires to make even a slight impression, danger may appear to attend the use of the quantities of medicine, which I recommend as absolutely necessary for effecting a cure. But they may rest assured that these fears are groundless. In the tetanus, although it has been  
fre-

frequently cured by bark and wine with similar medicines, yet I have known them objected to because they failed in cases where it was asserted they had a fair trial, and on enquiry, I have found, that half an ounce of the former and a half a pint of the latter were all that were given. That this quantity was useless, I believe will be readily perceived, when its known that the same quantity of both these medicines is very frequently unable to cure a simple intermittent. Several ounces of bark and a quart of wine, or more in a day, beside the intermediate use of other medicines intended to co-operate with the former, I should deem barely sufficient to counteract the impression made by the poison on the system, and to restore that tone which is essential to health. For although in health, a single glass of wine will produce the same effects in some that are observed from a bottle of wine in another; yet when the former labour under a *typhus*, where powerful stimuli are required, in consequence of the powers of life sinking, it is well known that the latter quantity may be drank in the course of a day with scarce any effect, when on the return of health, a single glass of it will be rejected.

MUSK may be given as an auxillary, but in doses not less than a drachm every hour; for I much doubt

the common opinion of its strong antispasmodic powers, as it appears to be one of those medicines, of which a false idea has been formed of its efficacy, from judging of its sensible qualities.

To act with the same intention, but in a much more powerful manner, ÆTHER certainly ought to be given. The wonderful property of this justly esteemed medicine, in calming spasmodic affections, and the suddenness of its operation, promises much benefit in this disease.

IN case the difficulty of swallowing should be so great, as entirely to prevent the use of these medicines by the mouth, they should be given by way of injection into the bowels, and combined with a large proportion of laudanum, at least half an ounce, in order to prevent them from running off; it will be perceived that, in this mode of exhibition, still larger quantities will be required, and at shorter intervals, in order to produce the same effect, than when given by the mouth. I apprehend that much advantage will also be derived from *mercurial ointment*, rubbed on the throat and neck. This has been hitherto generally used as a preventative; and discontinued whenever the mouth became affected with it. In the few cases in which

which it was used in the cure of the constitutional disease, two or three drachms rubbed into the bitten part, have been thought adequate to its removal; and because in these partial trials, it has failed of having the wished-for effect, it has been declared totally useless.

INSTEAD, therefore, of this partial and feeble use of mercurial ointment, I would advise half an ounce to be rubbed in the throat three times a day. By thus applying it to the parts more immediately affected, the benefit of the unctuous quality of the ointment will be obtained, and the specific effects of the mercury suspended in it, will also be exerted, and the morbid sensibility of the throat, thereby lessened. The good effects I have seen derived from its use in the tetanus,\* in relaxing the jaws, and lessening the difficulty in swallowing, induce us to expect the greatest benefit from its use in the present disease.

THE

\* In a case of tetanus, which occurred last winter in the Pennsylvania Hospital, during the attendance of Dr. Rush, half an ounce of mercurial ointment rubbed on the throat, was attended with the most beneficial effects in relaxing the jaws, which were so obstinately closed as to prevent the introduction of the least medicine or nourishment. In the course of twelve hours, by the use of the ointment, the patient could easily swallow: from the omission of the ointment, the symptoms again returned, and the cure was finally completed by large quantities of bark and wine.

THE influence of *depressing passions of the mind* in producing the disease, was formerly shewn to be very considerable; they will of course certainly assist in favouring its continuance. Every possible care ought, therefore, to be taken, to preserve the most equable and serene temper, and the utmost hope and confidence ought to be inspired. The idea of the propensity in the sick to *bite*, which may deter some from affording the requisite attendance, has no *foundation in truth*. *Systematic writers\**, indeed terrify us with apprehensions on this head: but the *unfettered* and *candid* historians of *real* cases of the disease assure us, that *no* such symptom *ever* occurs.

MELANCHOLY experience having so often taught us, that the effects of the poison are powerful; reason plainly points out, that in order to counteract them, active medicines should be used. The very large doses of those I have recommended, may seem alarming to some; and it may be apprehended, that the debility which I have constituted the proximate cause of the disease, may not only be removed, but a state of the body brought on, directly opposed to the former, and which will require contrary remedies. No apprehensions, however, need be suffered on this account, as it is the *ardor febrilis* which Boerhaave esteemed so necessary

\* Sauvage, Boerhaave and Van Swieten.

fary to the cure of the venereal disease, or that kind of *inflammatory diathesis*, which Dr. Rush \* has also deemed essential to overcome tetanus, that, in my opinion, is *alone sufficient* to counteract the effects of the canine virus on the system. Unless this be effected, no remedy will be successful. It is unfortunate that the enemy is obliged to be attacked on the most unfair grounds: for, by the difficulty in deglutition, we are deprived of the very means which in most other complaints remain free for overcoming the disease: were this obstruction not in the way, I entertain no more doubt of the efficacy of the *tonic* plan of treatment, than every one must do of the injury of a contrary practice. In the preceding pages it was shewn, that notwithstanding the difficulty patients labour under in commanding the requisite muscles to swallow, it can be in a *great measure* surmounted by a strong exertion, the patients should accordingly be advised to use their utmost endeavors to take freely of their medicines, as the only means of success.

BUT, as a successful practice can be the only test of the efficacy of any particular mode of treatment of a disease, it may be asked, where are there any proofs of its cure by the remedies here recommended? To this I would reply, that how-  
ever

\* Med. Inq. and Obs. p. 172.

ever impossible it may be to adduce cases of their success in the *cure*, as I have never had an opportunity of trying them ; yet I am happy in having it in my power to adduce *two*, in proof of the propriety of the method of prevention I have advised. The first, was communicated to me, and afterwards to the public \*, by Dr. William Weston, of the parish of St. Ann's Bay, Jamaica, in the number of the American Museum, subsequent to the one in which I had inserted some remarks on the disease, and had declared my opinion of the probable success of the tonic plan of treatment : the particulars are as follow :

“ IN January last, a negro boy was bitten in the  
 “ hand by a dog, to all appearance as mad as ever  
 “ I beheld one ; he also bit two sheep and was then  
 “ killed. Being called to the boy, a short time  
 “ after his receiving the bite, I immediately dilated  
 “ the wound, and filled it with strong mercurial  
 “ ointment, having in it a large proportion of com-  
 “ mon turpentine, which caused it to inflame confi-  
 “ derably, and discharge freely. I also gave him bark  
 “ in substance, with wine for eight days, gradually  
 “ increasing the dose, during which time, not the least  
 “ symptom of the disease appeared. The boy con-  
 “ tinued

\* American Museum, vol. viii. p. 100.—Sept. 1790.

“tinued perfectly well when I left the island, which  
 “was in July last. The two sheep, which were  
 “bitten nearly at the same time, died in ten days  
 “afterwards, raving mad.” The Doctor adds,  
 “Although the forming a general rule for the  
 “treatment of a disease from the successful termi-  
 “nation of a single case cannot be allowed; yet I  
 “shall be happy, if recording the above, shall in-  
 “duce a confidence in other practitioners, to give  
 “the same mode of treatment a fair trial, in this  
 “disease, which I have no doubt will prove equally  
 “successful in the *cure* with others, as it did in the  
 “prevention of it with me.”

THE other case alluded to, is one communicated  
 to me by John Shore, M. D. of Petersburg, Vir-  
 ginia, in a letter which I received from him, dated  
 the 17th October, 1791.

“On the first of February 1791, a negro girl,  
 “about 16 years of age, was bitten on the right  
 R “shoul-

\* The success in the above case was in all probability owing to  
 the external or local treatment made use of; as from the short time  
 that the bark and wine were given, it is impossible to ascribe any of  
 the good effects to them. In order to have acquired any pretensions  
 to the prevention of the disease, they should have been continued a  
 much longer time, and until the common period of the attack had  
 passed, as I have mentioned in the preceding pages.

“ shoulder in three different places, by a mad dog,  
 “ which at the same time was seen to bite another  
 “ dog and a cow. They both ran mad, the cow  
 “ on the 21st and the dog on the 28th of the same  
 “ month. I saw her the next day about 30 hours  
 “ after the accident, when I immediately directed  
 “ my whole attention to the wound, by making  
 “ upon it large and deep scarifications, after which  
 “ the lunar caustic was applied; the parts filled  
 “ with strong mercurial ointment, and the whole  
 “ covered with a blister, in order to excite in-  
 “ flammation in the wound and to keep it open  
 “ as long as possible, on which I conceived the  
 “ other, and still more important, part of the cure  
 “ depended. The mercurial friction was then direc-  
 “ ed in such quantities as to affect the mouth spee-  
 “ dily, with the occasional use of opium to procure  
 “ sleep and rest; after this, the tone of the system  
 “ was perfectly restored by continuing the use of  
 “ the bark and wine for some time. No symptom  
 “ has ever yet appeared, and I consider the girl as  
 “ quite secure.”

T H E E N D.

## E R R A T A.

Preface, page 2, bottom line, after the word "*intended*,"  
add *to relieve*.

Pages 12 and 14, in the notes; for "*Med. Com. vol. ii.*"  
read *vol. xi.*

Page 15, line 9, for "*put*," read *puts*.

Page 42, line 13, for "*it*," read *they*.

Page 77, the first dagger refers to the *Philos. Transf. No.*  
445; the second should be a double dagger, and refers  
to *Van Swieten's Com.*

Page 87, bottom line, erase "*external*."

Page 106, line 19, after "*experience*," add *respecting the*.

DISSERTATION

ON THE

INFELUENZA

SUBMITTED TO THE EXAMINATION OF THE

REV. JOHN EWING, S. T. P. Provost

of Teachers and Medical Professors of the University of Pennsylvania, in order to obtain the Degree of

DOCTOR OF MEDICINE

On the Eighth Day of May A. D. 1793

BY ROBERT JOHNSTON,

Philadelphia, Member of the American Medical Society.

The explaining, correcting and comparing the observations of our predecessors is more useful and as honorable as having their new discoveries; the truly learned will find but very few, whatever the ignorance may be.



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