AN

INAUGURAL DISSERTATION

ON THE

DISEASE PRODUCED

BY THE

BITE OF A MAD DOG,

OR OTHER

RABID ANIMAL:

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:
PRINTED BY THOMAS DOBSON, AT THE STONE-HOUSE,
SECOND-STREET, M,DCC,XCII.

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 diſſertation? 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.

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.

PREFACE.

EVER 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 phœnomena 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 chearfully 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 point-

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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.

THE 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.

A HISTORY

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 62d 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 diſ-"temper 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 meta-

 "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. Tranſ. 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 ani-
" mals, 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 diſſertation, and would, I fear, from the difficulty of obtaining 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 intereſted: 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 hydrophobia, 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 ſymptoms they deſcribed pertained to the actual diſeaſe. 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 Tétanus.—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. noſo. 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 atttended 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 litttle 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 defi-

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* 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.

" cient in point of accuracy, as to leave the reader " in ſuſpenſe as to the fact for which he conſults " them," and therefore deſerve any other name, as well as that of Hydrophobia. From the ignorance of the patients alſo, I am inclined to imagine, that many caſes, ſaid to be ſpontaneous, have actually proceeded from the bite, which having been either ſlight, or received at a conſiderable diſtance of time, has been overlooked, and the true cauſe conſequently neglected. In other inſtances, a difficulty of deglutition, or ſpaſmodic affection of the muſcles of the throat ſimply, has been magnified into a hydrophobia: this appears to me to be the explanation of the caſe related by Marcellus Donatus †. A violent ſurfeit in a boy, from eating beach nuts, cauſed a fever, delirium, ſpaſmodic affections, &c. is alſo ſaid to have been accompanied with a dread of water ‡. But this appears to be no more a caſe of the diſeaſe in queſtion, than the diſlike to water from eating an unripe perſimmon, or any other ſimilar aſtringent vegetable, deſerves the name of Hydrophobia. The diſagreeable effect of theſe fruits on the mouth and throat is well known, and the beach nuts appear to have induced a ſimilar ſtate;—a delirium accompanying the latter caſe, made the

* Marcellus Donatus, lib. iv. chap. i.

† Med. Muſeum, Vol. I. p. 98.

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 in

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 conconſtriction

* 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 viſcidity, 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 homnibus priaſmus incidit, cum ſcroti contractione, & ſeminis 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. 368.

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

 any

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

† Philoſoph. Tranſact. Vol. III. p. 277.

‡ Fothergill's Works, p. 353.

§ Vaughan's Caſes, p. 4.

|| Philoſoph. 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 in-"tellectual faculties ſeemed quickened by diſeaſe." For this reaſon, therefore, that the diviſions hitherto attempted by authors, were in a great

 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 an- "other, 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 con-" sequence 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 occurence 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 *ferobiculus 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

 "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, ari-
" ſing 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 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,

 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 drinking; for the former are very frequently with the ſtomach the firſt parts attacked, as mentioned before, and occurred in a remarkable manner, in the caſes related 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 ſometime, water, and other fluids, were taken with the *greateſt eaſe and compoſure*, until the commencement 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. Tranſ. 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 "Duſkatapoſis*." 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.

caſe, 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 con-

* Philoſoph. Tranſ. abr. by Martin, Vol. XI. p. 225.
† Ibid. No. 445.
‡ Mead's Works, p: 659,
‖ Morgagni Letter, viii, art: 19,

conſtriction and narrowneſs of their fauces, or *gula*, as Salius, teſtifies, and Aromatarius confirms.

In another caſe *, the perſon expreſsly declared, "he could not ſwallow for ſomething in his "throat, that interrupted the paſſage;" and made exertions to ſuppreſs a vomiting, with which he was repeatedly threatened, for fear of increaſing the obſtruction in his throat.

So far from being afraid of drinking water, the ſick lament with the greateſt anxiety, their inability to relieve the thirſt which afflicts them, and by various contrivances, endeavour eagerly to drink†. Mr. Bellamy ‡ expreſſed no fear of water, but only complained of the difficulty of ſwallowing; others again expreſs a deſire to go into the bath‖."

The conſideration, however, that the ſame averſion from drinking, happens in other nervous diſeaſes, where the ſame ſpaſmodic affection of the the muſcles of deglutition and the ſame morbid

 ſenſibility

* Edinb. Med. Com. Vol. III. p. 290.
† Philoſoph. Tranſ. abr. Vol III. p. 277.
‡ Fothergill's Works, p. 353.
‖ Vaughan's Caſes and obſ. p. 34.

ſenſibility occur, will alſo tend to prove the cauſe here aſſigned, and the total inutility of having any recourſe to the poiſon, in order to account for this ſingular ſymptom. It was mentioned, before that Dr. Mead * obſerved it in hyſteria, Drs. Percival †, and Ruſh ‡, have recorded caſes of its occuring in Tetanus.

But it may be aſked, if there be no ſpecific dread of fluids, why are ſolids ſwallowed with leſs difficulty than liquids, contrary to what is obſerved of all other affections of the throat? To this I would reply, that a very material difference exiſts between an affection of the muſcles of deglutition, proceeding from a ſtate of inflammation, and a diſtenſion of the part, and that affection proceeding from the diſeaſe at preſent under conſideration. In the former, liquids are ſwallowed with greater eaſe, as requiring leſs exertion of the muſcles than ſolids, which create great pain by increaſing the preternatural diſtenſion already exiſting. In the latter, liquids are ſwallowed with greater difficulty for the ſame reaſon, viz. requiring more exertion of the muſcles, of which the patient has entirely loſt the commmand; but ſolids are enabled to deſcend with greater

* Meads Works, p. 83.

† Eſſays Med. Philoſoph and Exp. Vol. II. p. 366.

‡ Med. Inq. and Obſ. 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

" 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 ſurface, which is endowed with this morbid ſenſibility, 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 ſufficient 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 accompanying 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 aſſerted 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. 818.

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

‡ Sauvage, diſſertat. ſur la rage. p. 12.—Van Sweiten 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. Tranſ. 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. Griffits, 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

DEFINITION.

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-

 ever,

* Potionis cujuſlibet, utpote convulſionem pharyngis dolentem cientis, 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 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 inspec-"tion, 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 meaſnre 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

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.

I

* 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. Diſ. 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 interved 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,

* 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 periſtalic 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 contrary, 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 weather, 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 Madraſs, he ſaw a boy brought into the fort, who died in the evening of a bite he received the ſame day.

SYMPTOMS IN DOGS.

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 Petapſco 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 morſu 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.

REMOTE

* 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.

1. 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 charac-"ter, 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 diſ-"eaſe; 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 madneſs 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 circumſtance

* Diſeaſes of Tropical Climates, p. 32.
† Ulloa's Voyage, vol. I. p. 296.
‡ Biblioth. raiſone, 1750, Avril, May, Juin, p. 422.
‖ Van Swieten's Commentat. on Boerh. Aphor. 1129.
§ Hiſtoire de Malad. de St. Domingue. Moſely, 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 exemption 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 effects of heat, in producing the diſeaſe. It is actually 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 attended with the production of the ſame general debility 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 violenceof Winter and Summer, was equally exceſſive‡. During ſeveral hard Winters within my

* Moſely Diſ. 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;

* 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 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

* Diſſertat. inaug. Edin. 1778.—Webſteri prax. med. ſyſt. vol. ii, p. 261.

† Mead's works, p. 80.

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 membraneous 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 remaks, 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 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 ope- " ration*." 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.

* 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, can-
" not be diſcovered by diſeaſed lymphatics, be-
" tween 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 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 Diverſus, de feb. peſt. p. 58.
Sauvage ſur la rage, p. 37
Vaughan's Caſes, p. 29.

ſervation 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. Tranſ. 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 occuring 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. Trans. Vol. II. p. 46.

† Hunter's Vreatise 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. Tranſ. 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 .*

* Morgagni ſeems to entertain the ſame idea, with reſpect to the action of the poiſon on the nerves; he remarks, "from the pro- "greſſion 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 perform- "ed) but by the nerves up to their origins. Letter VIII. art. 32. Dr. Percival alſo aſcribes the diſeaſe entirely to "nervous irrita- tion." 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 excrutiating pain, and long reſiſted the "remedies commonly employed, with much more "ſpeedy ſucceſs, in that diſorder." In a caſe of

 obſti-

* 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 reſcuſcitated 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 ſurpriſing, 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.

PROX-

* 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. Tranſ. 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 proselytes 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 phœnomena 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 accute 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

 aſſertion

* Meads's works, p. 661.
Philoſoph. Tranſ. 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

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.

1. 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.

(1.) 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 *tenanus* 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 ſurpriſe, 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

 mad

* 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

 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 hasbeen, for the moſt part, in thoſecaſes where theſenſibility of theſyſtem, either from age or *idioſyncracy*, 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 uſualy melancholy.

Morgagni † relates the hiſtory of an old man, who

* Med. Tranſ. 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 indiſpotion 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 ſimilarity 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 labouriug 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.

* 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*

 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.

METHOD OF CURE.

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 ar-"rives; the ſufferer may long reſiſt all ſolicitati-"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 application of the cautery there are ſtill more valid objections. The intenſity of the pain attending the operation would be ſuch, as to prevent numbers from ſubmitting to it; and the idea of this would operate 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 powerfully, 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 Admiral 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. Tranſ.

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 enlargement of it with a scalpel or lancet, will prove useful, 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 preventing 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

 they

* Galen de ſectis, 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 equally 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 totally uſeleſs: it is nevertheleſs abſurdly retained at this day in ſome European pharmacopœias.

Another remedy more deſerving of a particular 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 to any virtue in the medicine taken, but to other circumſtances.

One great cauſe of the celebrity of the Ormſkirk, 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 or 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 Ormſkirk 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 exhibited of their efficacy.

These facts, while they ſerve to ſhew in a decided manner, the fallacy of the experience ſuppoſed 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. Fo- "thergill, and other eminent practitioners, uſed it "with freedom. It is alſo powerfully antiſpaſmo- "dic; but it is at the ſame time powerfully debili- "tating. 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 caſe where it was employed, as from a ſtab in the heart with a ſmall ſword. I defy a ſingle inſtance of the real diſeaſe to be produced, where either a ſymptom was relieved, or a cure effected by it. Caſes are indeed related by ſeveral authors, where it was uſed with *other remedies*, and the patient has recovered. But when a number of means are employed, is it rational to aſcribe the ſucceſs derived from their uſe to any *one*; eſpecially when the operation of them is directly oppoſed to each other, as in the preſent inſtance? Thus bleeding, muſk, opium, the warm bath, and ſometimes mercury, have all been employed in the treatment of the ſame caſe.

But let the circumſtances of theſe ſuppoſed ſucceſsful caſes be examined. The firſt is that related by Dr. Nugent; he uſed the three firſt remedies: but I have before rendered it probable, that this caſe was the effect of an hyſteric paroxyſm. Dr. Tilton's ſuppoſed caſe of this diſeaſe, which was alſo cured by very copious bleeding, was ſhewn to be a violent hyſteria bordering on mania. Mr. Wrightſon* alſo mentions a ſucceſsful termination of a ſuppoſed caſe of this diſeaſe, by the uſe

 of

* Med. Tranſ. 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 nnd 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. Tranſ. 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 aview 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 ſimiliarity 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 COLD BATH, 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 both, 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

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.

ſary 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 however

* 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 conſi-"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 Petersburgh, Virginia, 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

 " 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."

THE END.

ERRATA.

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. Tranſ. 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.*