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 facutly 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.

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TO

ANDREW MEASE, M. D.

OF STRABANE, IRELAND.

HONORED SIR,

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

your affectionate nephew,

JAMES MEASE.

Philadelphia, May 7, 1792.

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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-

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

 a much

* Vol. viii. p. 68.

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

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

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.

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.

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,

" 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.

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.

" 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, however,

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

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

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.

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.

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.

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.

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

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.

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.

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.

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

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.

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.

"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

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.

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.

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.

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.

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

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

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 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.

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.*