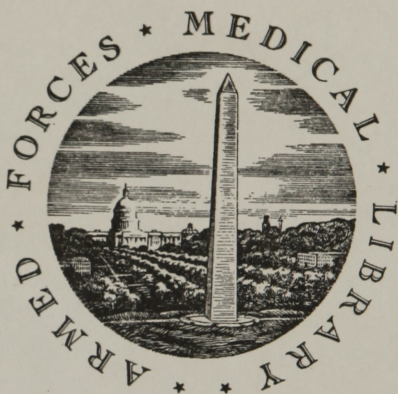


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OBSERVATIONS
ON THE
Arguments of Professor Rush,
IN FAVOUR OF THE
INFLAMMATORY NATURE
OF THE
DISEASE PRODUCED
BY THE
Bite of a Mad Dog

BY JAMES MEASE, M. D.

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Professor Rush,

Respected Friend,

SINCE the publication of my Inaugural Essay, upon the disease which is the subject of the following pages, reflection and investigation have served to inforce upon my mind, the truth of the doctrine you first unfolded.

You will perceive that the arguments which have induced you to adopt the system previously destroyed by yourself, have had no influence upon my mind. I have fully detailed my reasons in the

DEDICATION.

following pages, but I declare, if a single fair case of the disease be hereafter cured by the depleting plan, I will wave all theory on the subject, and adopt the practice.

THAT you may be enabled, by the preservation of your health, and activity of mind, to prosecute your studies, and to benefit science, is the wish of

Your Sincere Friend,

THE AUTHOR.

PHILADELPHIA, }
March 4, 1801. }

OBSERVATIONS,

PL. PL. PL.

IT is a mortifying reflection to the pride of Physicians, that, notwithstanding the disease produced by the bite of a rabid animal was very early described by medical writers, so little progress should have been made toward the discovery of its nature, or in establishing a successful mode of cure. The history of this singular disease has indeed been accurately detailed by the antients, but the numerous remedies and plans of treatment prescribed by them, are marked by ignorance, superstition and want of success. During the long period preceding the time of the great Boerhaave, we find little on the complaint, but a collection of vague theories, and unsupported conjectures; repeated from author to author: that la-

borious investigator however attempted to give a true theory of the disease, and to establish thereon, a successful method of treatment, which it is evident was grounded upon the degree of apparent vigour exhibited by the sufferer, and caused him to pronounce the complaint "*summe inflammatorius.*" The method of cure was accordingly logically deduced to consist in *copious bleeding*. The pupils devoted to the doctrine of their respected preceptor, calmly acquiesced in the adoption of his theory, and anxious to be the instrument of proving the truth of his system by putting it to the test of experiment, boldly pushed the depleting plan to an astonishing extent. But it was found much easier to theorise upon diseases in the closet, than to cure them when they actually occurred, and that however agreeable to the nature of the complaint, the plan of treatment appeared to be; its want of success was full proof of the erroneous principles upon which it was founded. Physicians were still at a loss to determine what substitute to adopt. Every one whose lot it was to be called to a case, followed the treatment which his own reflection pointed out as the most likely to succeed: but every new case was an addition to the former

melancholy accounts of the feeble efforts of medicine in the disease. At length, the striking analogy subsisting between the tetanus, and the disease in question, induced Dr. Rush to apply to the latter, the principles he had adopted and successfully practised in the former; and the investigation finished with a conclusion the reverse of the Leyden professor, viz. that the disease was not founded in vigour, but in relaxation. From a conviction of the truth of this opinion, I supported it in my inaugural essay published in Philadelphia, in May 1792; but since that period, the professor has deserted his first theory, and now concludes that both tetanus and the disease under consideration, depend upon excess of action, and that they are to be cured by debilitating remedies. While I admire the condour and meritorious example, exhibited in renouncing opinions deemed erroneous; yet I am forced to declare, that the arguments he has brought forward have not operated with a force requisite to change my mind, and as I consider it of importance to ascertain truth upon this question, I shall proceed to examine it, and I hope with impartiality: if, in the result it shall be determined that Dr. Rush's present opinions

are true, then I will grant that the support I formerly derived from the analogy drawn by the professor himself as existing between the two diseases will fail of effect, but if, on the contrary, his recent sentiment shall appear to be erroneous, then I will recur to the original ground and derive all the support which I first expected from its adoption.

THE theory at present entertained by Dr. Rush of the disease produced by the bite of a mad dog, is, that it "is a malignant state of fever." His reasons for this opinion are as follow.

I. "THE disease in all rabid animals is a fever. This is obvious in dogs, who are mostly subject to it. It is induced in them by the usual causes of fever, such as scanty or putrid aliment, extreme cold, and the sudden action of heat upon their bodies. The animal matters which are rendered morbid by the action of the above causes upon them, are determined to the saliva, in which a change seems to be induced, similar to that which takes place in the perspirable matter of the human species, from the operation of similar causes upon it. This matter, it is well known, is the remote cause of the jail

fever and the plague. No wonder the saliva of a dog should produce a disease of the same kind after being vitiated by the same causes, and thereby disposed to produce the same effects."

THE causes here stated, are certainly those from which the disease in dogs very commonly proceeds: They are likewise the occasional causes of fever in the human race. I am willing to allow all the weight which could be wished from this argument to prove the truth of the theory respecting the disease, but I may hint by the way, that, considering the frequent spontaneous origin of the disease in dogs, and that a complete assemblage of the symptoms peculiar to the idiopathic affection, never arise in the human species, without the assistance of the canine virus; a doubt is suggested respecting the propriety of employing the fact of the existence of a fever in dogs labouring under the disease, and of its inadmissibility as a collateral argument in favour of the doctrine advocated, as to the human race.

I AM at a loss to conceive with what propriety it is supposed, that the morbid animal

matters producing the disease are determined to the salivary glands ; or whence arises the necessity of this supposition to the explanation of the phænomena of the disease. It is apparently grounded upon the admission of the supposition that the affection of the salivary glands is the original disease, whereas it appears to be merely the result of a general diseased action produced by a cause operating in the first instance upon the system at large. It would indeed be difficult to account for the particular violence with which the throat is affected in this disease, but the same difficulty occurs in tetanus where no foreign morbid matters exist, and this fact fully shews the total insufficiency of the theory of the elective determination of the morbid matters to the throat, to account for the symptoms. The analogy of the perspirable matter of the plague, it is conceived, is not applicable to the saliva of a rabid animal. In the former disease, a fluid is accumulated, and probably undergoes a putrefactive fermentation which produces a set of symptoms very different from those that result from the canine virus ; in the latter, the same causes do not operate, for the saliva, tho' accumulated, is not retained, but discharged in greater quantities than usual.

THE second proof adduced of the disease in question being a malignant fever, is, “that it prevails occasionally among dogs at those times in which malignant fevers are epidemic.”

WHEN any particular circumstance is adduced as an argument upon any occasion ; it necessarily implies the uniform presence of that circumstance, otherwise very little weight will be attached to it. It is acknowledged that the disease appears only “*occasionally*” during the prevalence of malignant fevers, and this seems to be merely an accidental occurrence, for it sometimes appears among dogs at periods of general health, and does not occur upon other occasions during the prevalence of a malignant epidemic. The state of the air producing both diseases must therefore be different. During our revolutionary war, the malignant yellow fever ravaged the West Indies, and yet madness among dogs, did not appear until the close of the contest : and Dr. Moseley asserts that it had been a stranger to the Islands for fifty years before 1783. Don Ulloa and Des Portes say that it is equally unknown in South America, and in St. Domingo ; and yet both these

authors describe very malignant fevers which they witnessed. In the island of Antigua, though great heat, (one of the usual causes of the disease) exists in common with all the West India Islands, and another, viz. a deficiency of water, is known to be peculiarly great, yet we find from Dr. Athill* that the disease is unknown. My own reflection will furnish sufficient proofs to corroborate the facts of the absence of the canine madness during the late prevalence of the American pestilence in Philadelphia. An unusual mortality was indeed observed among the cats in several places previous to the appearance of the yellow fever, but the disease of which they died was not madness.

THE third argument to prove the existence of fever in dogs labouring under the disease is, "that they exhibit the usual symptoms of fever, such as want of appetite, great heat, a dull fierce red or watery eye, indisposition to motion, sleepiness, delirium, and madness." To this I reply that the question before us, and the one I am anxious to determine, is, not what is the nature of the

* Parry diss. inaug. Edinb. 1778.

disease in dogs, but what is the state of the system produced by the action of the canine virus on it. I grant that the disease either when originating spontaneously in dogs, or from a bite, may appear with symptoms similar to those that mark the presence of fever in man, and that they do prove the existence of fever in dogs; but I conceive no support can be derived therefrom in favour of the existence of the same state in the human race; for we well know that the symptoms in man are directly the reverse of those that occur when he labours under fever. The appetite so far from being diminished, is sometimes unimpaired; the eye is lively and piercing, and endowed with unusual sensibility; relief is obtained by walking or running, wakefulness commonly attends, the sleep is short and uneasy, and in nineteen cases out of twenty no delirium is present, but death strikes the sufferer in the moment of conversing freely with his friends. Of these facts I have abundant proofs.

“ 4 A dissection of a dog by Dr. Cooper that died with this fever, exhibited all the usual marks of inflammation and effusion which take place in common malignant fevers.”

It is not said under what circumstances, the dog was dissected; if, after running many hours in hot weather and exposed to the persecution of a host of pursuers, the appearances noted are such as might be expected; but they would also appear in any other dog dissected after a similar exertion of body. At any rate, the analogy derived from symptoms in the canine genus occurring during life, or from appearances after death, is not transferable to the human race. We must reason from symptoms as they appear in man.

“ II. THE disease produced in the human species by the bite of a rabid animal is a malignant fever.” This it is said appears from the symptoms, and it must be acknowledged that some of those that occur in the disease, likewise appear in febrile affections, but it must also be recollected that the symptoms resulting from the action of the canine virus upon the human system are in many respects common to nervous affections, where no fever is present. It may be asserted with great safety, that the states of the skin, of the pulse, and of the eye, are very different from those which take place in the commencement of a malignant fever, and from which the indica-

tions of cure of the proposed treatment, it is expected, would be taken. The pulse is small, quick, irregular, or natural; the skin is cool, the eye clear, face pale and shrunk: the full bounding tense pulse, the burning skin, red eye, and flushed face, which are so characteristic of the malignant fevers of our country, never appear in the supposed fever arising from the action of the canine virus on the human system. It is presumed, that the symptoms of the latter complaint, indicate a state of the system directly the reverse of inflammatory action.

It is further remarked that "there are cases on record in which there is said to be a total absence of fever. The same thing has been said of the plague. In both cases the supposed absence of fever is the effect of stimulus acting upon the blood vessels with so much force as to suspend morbid action in them. By abstracting a part of this stimulus a fever is excited which discovers itself in the pulse, and on the skin, and frequently in pains in every part of the body." To this explanation three objections may be offered. 1. The absence of fever in this disease so far from being an occasional

occurrence, is almost uniformly observed.* Salius Diversus long since observed this fact. The pulse as already remarked, is either natural, small or irregular, and never hard or tense: the *occasional quickness* evidently arises from the rapid contraction of the arteries occasioned by the violent irritation of the nerves of the heart. Certainly, therefore, the absence of febrile symptoms in almost every case, implies that they are not essential to the disease: and even supposing that the stimulus of the virus may in some sudden and violent attacks go beyond the point of fever, yet this cannot be imagined to happen in nearly all the cases recorded; some of which came on gradually and would have been accompanied by fever, if it were the nature of the virus to stimulate. But the fact is, that whether the disease makes

* In every case I have consulted, the absence of fever is noted, except two. One was Dr. Lister's patient, Phil. Trans. ab. vol. 3. in whom a "*violent fever*" came on the 5th day, with a "*very quick pulse.*" The same occurrence frequently takes place in hysteria. The other was in Master Rowley, as recorded by Dr. Hamilton. In this case, the fulness, and tension of the pulse and head-ach, evidently proceeded from the mercury, with which his system had been gradually and fully impregnated, to the day of his attack, with a preventative view. The blood like the blood of persons using mercury, was "*somewhat sizy.*"

its appearance in a formidable and sudden manner, so as to leave no doubt as to its nature, or comes on in an insidious way, resembling a common cold, yet a true fever has never yet appeared. The presence of heat in a single case, or even in two or three, (and more will not be found) cannot prove that it arises from an increased force of the heart and arteries; for the heat of the human body is by no means solely dependant upon the circulation, but on the contrary, is very intimately connected with the state of the nerves. That careful observer, Sydenham, long since noticed the death-like coldness of the body in a fit of hysteria, while the pulse was regular; and we every day see the same thing in women labouring under a disordered state of the nerves, who will be alternately attacked by flushes of heat and chills, without any sensible change in the pulse: De Haen* gives a similar fact of the extreme coldness of the hands, though the pulse was said to be "*robustum in carpo.*" The same author† relates a fact of a part being unusually warm, "*sine sanguinis arteriosi perceptibili transfluxu.*"

* Ratio medendi, p. 198. Ed. Lond. 1761.

† p. 201.

2. THIS absence of fever, cannot be said to proceed from the excess of stimulus acting upon the blood vessels, for, from numerous cases, it appears that the pulse does not increase in frequency or force as in other diseases, (where the same circumstance takes place,) by abstracting part of this supposed excess. On the contrary, the pulse gradually sinks and alarming symptoms have sometimes followed the first bleeding. In others, no alleviation has been produced by the operation. The pulse did not rise with the *copious blood-letting*, prescribed by Dr. Rutherford, nor after the moderate quantities by any other physician, who has recorded his practice in the disease.

3. THE diseases in which this excess of stimulus occurs, differ essentially in their modes of attack from that now under consideration. In the former as the apoplexy, epilepsy, and in some cases of the American pestilence, the sufferer is sometimes arrested before he is aware that the seeds of disease are implanted in his system, and the functions of life are performed with vigour the moment before the electrical stroke from the disease is recei-

ved,* but that produced by the action of the canine virus, comes on in most cases slowly, and exhibits the gradual diminution of the powers of life, by symptoms highly distressing, and which never occur in inflammatory diseases. The arterial system indeed appears to be no way necessarily affected, either, in tetanus or the disease under consideration. In the former, we have the fullest proof of the concentration of the morbid action in the nerves, and of the entire freedom from affection in the blood vessels.†

IT is observed further “ that the hydrophobia partakes of the character of a malignant

* IN a case of violent pneumonia, produced in a negro, by exposure to cold in the night, while thinly clad, I have seen the blood vessels so paralyzed, or affected, as that very little blood would flow, altho' I was sure I had cut the veins in the arm. Death took place in six hours after I saw him, and in thirty-six from his attack :

† DR. MUDIE relates that the catamenia appeared in a lady ill with the tetanus, and continued the usual time. Med. Comment. vol. 3d. The tetanus and the disease under consideration, are so nearly allied in their nature, and symptoms, that any observation made on one, may be applied to the other, and I have no doubt the event just mentioned would also take place in the disease produced by the canine virus, if it should happen to be excited into action a little before the usual return of the discharge.

fever in appearing at different intervals from the time in which the infection is received into the body. These intervals are from one day to five or six months."

To this I may reply, that more or less interval must necessarily take place between the application of every cause, and the production of its effect; and the great difference in the intervals of attack from the application of the remote causes in the disease produced by the canine virus, and in malignant fevers, may even be urged against the supposed analogy; for the disease has appeared in all the intermediate periods between ten days, and 19 months after the reception of the bite.*

* Phil. trans. N^o. 445.

Dr. Hamilton vol. 1. p. 112, rejects the idea which I admitted in my inaug. diss. and founded upon hearsay, of the action of the virus taking place a few hours after a bite in the East Indies: he is followed by Dr. Gidlestone. The reasoning of the latter has so weakened my faith, that I am disposed to suspend my belief, and to consent to reject the account I formerly quoted. Dr. H. after a very laborious research, fixes the *tenth* day as the earliest period at which the disease has appeared, and 19 months, as the latest:

BETWEEN these periods the times of attack were very various.

3. "BLOOD, it is said, drawn in the hydrophobia, exhibits the same appearances which have been remarked in malignant fevers."

To this I answer, that the appearances exhibited by blood drawn, are so various in

OF 131 cases, 17 were seized before the 30th day,

63	between	30 & 59	} days inclusive.
23	from	60 to 90	
9		90 to 120	
2	at	5 months.	
1		5 months and 11 days.	
1		6 mo.	
1		7 mo.	
2		8 mo.	
1	between	8 and 9 months.	
2	at	9 mo.	
1		11 mo.	
1		14 mo.	
2		18 mo.	
1		19 mo.	

I BELIEVE I first denied the truth of the opinion universally received since Aurelian, that the disease commenced its attack sooner or later in proportion to the nearness of the bite to, or its distance from the head. I find this erroneous position lately delivered as an axiom by Dr. Hunter, (Trans. of soc. for promoting med. and chirurg. knowledge, 1793.) To the authorities referred to in my inaug. diss., I may add those collected by Dr. Hamilton, (vol. 2d.) which clearly disprove the opinion. Of twenty nine cases there are twenty one of persons attacked from one to four months, of whom twelve were wounded in the cheek, and four in the lip, yet the patients were not attacked sooner, and in some cases not so soon as others bitten in the hands and legs.

the same disease, and depend upon so many causes; that physicians have generally ceased to draw any conclusions respecting the nature of a complaint from them. With respect to the size, it appears to be formed merely in consequence of the greater fluidity of the blood, whence the coagulable lymph suffers the red particles to precipitate before it coagulates. But the assertion that the same appearances are exhibited by the blood drawn in malignant fevers, and in the disease under consideration, appears to be weakly supported. Indeed the almost uniform accounts I met with, of the little appearance of disease in the blood, struck me at an early period of my researches, and I used the fact upon a former occasion* to prove another point; I merely however, referred to three cases,† though twenty more were at my hand. In Mr. Belamy's case related by Dr. Fothergill, the "slight traces of size," must have been accidental. They were probably produced by

* Inaug. dissert. 1792. p. 72.

† Phil. transact. abd. vol. 3d. Dr. Lister's case.

Do. do. vol. 47. p. 413. Dr. Wilbraham's case.
Morgagni, letter 8th art. 30th.

the circumstances attending blood-letting, which are well known to favour the appearance of size. Even granting that this appearance was indicative of inflammation, it must necessarily be supposed to have been moderate, from the remark itself made in the case ; and if any weight will attach to the sizy appearance of the blood of Mr. Oakely's boy, noticed by Dr. Rush, it must be counterbalanced by the numerous and well attested histories of cases, in which none or very little difference has been discovered in the blood, from that of persons in health. For proofs I must refer to the three authorities already quoted, and to the numerous medical publications of England, in which the fact is fully substantiated.

4. THE fourth argument used to prove the disease a malignant fever, is, that it " exactly accords with malignant fevers in its duration. It generally terminates in death, according to its violence, and the habit of the patient, on the first, second, third, fourth, or fifth day, from the time of its attack, and with the same symptoms which attend the last stage of malignant fevers."

THOUGH the duration of the disease in question, does agree with what takes place in malignant fevers ; yet it is conceived that little weight will be derived from the fact, to support the analogy between them. All that can be concluded from it is, that the virus operates with great violence, and quickly destroys life ; but death frequently takes place, in other diseases, in periods equally short, and where no malignancy is suspected, as in the cold fit of an intermittent fever, in apoplexy, and epilepsy. But I am forced to deny the accuracy of the statement respecting the similarity of all the symptoms in the diseases whose analogy it is attempted to trace. The symptoms exhibited in the close of most violent acute affections certainly do resemble each other in part, and it is true, that some few appearances in the disease produced by the bite of a mad animal, occur in malignant fevers ; but I contend that they are not peculiar, and that they are far outweighed in their influence by the circumstance of the absence of several striking symptoms in the one, which occur in the other. A moment's reflection will suggest my allusion to the different modes of attack

of the two diseases; the bilious suffusion of the surface, the bilious evacuations, violent vomiting, black vomiting, purple eruptions, and the spontaneous hemorrhages from the eyes, nose, and alimentary canal, all of which occur more or less, in our malignant yellow fevers, and which never have been known in the disease produced by the canine virus.

5. "THE body, it is said, after death from the hydrophobia, putrifies with the same rapidity that it does after death from a malignant fever, in which no depletion has been used."

PUTREFACTION in the bodies of persons dying of the first mentioned disease, is by no means an universal occurrence. In the modern records of cases it is not mentioned, except in three instances by Morgagni;* two of which occurred in the winter: but to shew how little dependance ought to be placed upon the circumstance I may mention, that the body of one of the persons who

* Letter 8. Art. 31. 32.

died in the latter end of July, when the speedy progress of putrefaction ought to have been expected, was the least offensive. But granting the general occurrence of the fact, little weight ought to be attached thereto, for we know that “all animals which die suddenly, and without loss of blood, are disposed to speedy putrefaction. This has long been remarked in animals that have been killed after a *chase*, or by lightning. The poisonous air called *Samniel*, produces, when it destroys life, instant putrefaction. The bodies of men, who die of violent passions, or after strong convulsions, or even after great muscular exertion, putrify in a few hours after death. Hence the practice of fishermen of breaking the heads of their fish, as soon as they are taken out of the water. The sudden extinction of life in the fish, prevents those convulsive or violent motions which induce sudden disorganization in their bodies. Putrefaction takes place most speedily after death from the yellow fever, where the commotions of the system are not relieved by evacuations.”†

† Rush's Works, vol. 3. p. 74.

HERE then we find the same event produced equally by the affection, or violent excitement of three distinct agents in the human body; viz. the *muscular*, *nervous*, and *arterial systems*. The explanation I do not pretend to. But the fact satisfies me as to the weakness of the argument in favour of the analogy between the disease under consideration, and malignant fevers.

6th. It is asserted that “dissections of bodies which have died of the hydrophobia, exhibit the same appearances which are observed in the bodies of persons who have perished of malignant fevers. These appearances according to Morgagni and Sauvry are “marks of inflammation in the throat, œsophagus, brain, stomach, liver and bowels. Effusions of water, and congestions of blood in the brain, large quantities of dark coloured or black bile in the gall bladder and stomach, mortifications in the bowels and bladder, livid spots on the surface of the body, and above all the arteries filled with fluid blood, and the veins nearly empty.” To these appearances I may add ano-

ther, first mentioned by Boerhaave, and lately urged by Dr. Ferriar, viz. "congestions in the lungs."

It is true that a superficial redness has occasionally appeared in the trachea œsophagus or stomach, but dissections shew it is much oftener absent than present, and that "is far different from that kind of inflammation which has been properly called active. It is of the erysipelatous kind, and is always accompanied with marked debility of the system."* If the redness found in the throat was of the kind we meet with in inflammatory angina, or the inflammation of the larynx, we would find it more extensively diffused and deeper; whereas the fact is, that very often no discoloration is perceived, and in others, it is very slight; where this redness does occur in the throat, it may in part proceed from the severe convulsive motions of the muscles producing some slight increased action in the vessels of the part, and by interrupting the regularity of the circulation, cause an *error loci*, and force

* Hamilton, vol. i. p. 85.

red blood into serous vessels, V. Swieten gives a similar explanation of the occasional redness found in the throat. He observes, "that it seems rather an effect of the distemper than a productive cause of it; for it is no wonder if these parts become inflamed when they are convulsed at every sight of, or attempt to swallow, liquids." It is generally proportioned to the duration of the disease and to the violence of the convulsive motions. Hence the same author* observes, that in a boy taken off very soon by the disorder, his throat was free from redness, but in a young man who lay longer, some appeared.

THE supposed inflammation, as red spots and streaked lines noticed by some authors, in the gullet and stomach, are occasioned by the weakened force of the heart and arteries, and by the consequent irregularity of the determination of the blood throughout the body, and partly by the irritation of the nerves of the parts, which by being communicated to

* Comment. § 1140.

the arteries, may increase the rapidity of their contractions, and hence create the superficial redness taken for a true active inflammation. That the redness is merely on the surface, and very different from the true inflammation of the stomach, we know from a remark made by Dr. Ferriar, who “upon cutting into the muscular coat, found that it appeared quite sound.”

WE frequently see similar supposed marks of inflammation in those who die of typhus fevers, or some dysenteries, which are known to be accompanied by a deficiency of general action; the proof of which is, that when these diseases are early attended to, and due care is taken to prevent the progress of the general debility by invigorating remedies, these appearances do not occur.

THIS inflammatory appearance may also be occasioned, as Dr. Hamilton hints, by the acrid saliva passing undiluted into the empty stomach; and in Dr. Ferriar's last patient its effects were increased by his intemperate habits.

THE congestions of blood in the brain, and lungs upon which so much stress is laid, admit of an easy explanation without having recourse to inflammation. I must first however remark, that this appearance is more frequently absent than present, and therefore cannot be of that importance which is attached to it, especially by Dr. Ferriar. I have already remarked that the nerves of the heart partake of the general irritation pervading the system. This is evinced by the palpitations and the great irregularity of the contractions of that muscle during life, and by the morbid appearances it exhibits after death. The blood will be consequently impeded and retarded in its passage through the lungs, and irregular distribution and congestion must follow.* Hence the paleness of the surface, its painful sensibility to the air, and the sense of suffocation that distresses the patient in this disease. A similar accumulation of blood in the lungs, attended by precisely similar symptoms, takes place in the cold fit of an intermittent, by reason of the determination of the fluids from the surface to the internal parts,

* Hamilton vol. 2d append. xxii.

arising from the inability of the heart to propel its contents with its usual energy. In the latter case we see these symptoms vanish upon the restoration of regularity to the circulation, by running,* by warmth, or hot drinks, and the same event would likewise happen in the disease under consideration, if it were possible to increase the weakened force of the heart, but the difficulty of exhibiting any medicine prevents the exertions of the physician to overcome the cause, and hence the morbid determinations and the general derangement of both arterial and nervous systems go on increasing, from the commencement to the close of the short, but severe period of the patient's sufferings.

THE case of the late Mr. John Hunter affords a striking confirmation of the truth of the pathology I have laid down. It is

* FROM running, patients have been frequently observed to find momentary ease; it must be produced by the temporary restoration of regularity to the circulation as in the intermittent, which relieves the lungs from the blood accumulated in them. And by the way I may remark with Dr. Hamilton that this fact adds another proof of the existence of debility in the disease, for in what inflammatory complaint do we see the same relief obtained by exercise: would it not produce death in pneumonia?

well known, that for a long time he laboured under an organic affection of his heart, and that he suddenly died, yet the coats of his stomach and intestines were found upon dissection unusually loaded with blood, and the vessels of the pia mater covering the two hemispheres of the brain, were also turgid. Any one who had seen Mr. Hunter's bowels and head, and who was ignorant of the true cause of his death, would have concluded without hesitation that he had died of some violent inflammatory disease, in which no bleeding had been used. Mr. Hunter was affected moreover, during life, with symptoms that occur in the disease produced by the bite of a mad dog, particularly false perceptions, alteration in vision, and increased sensibility of the organs of sense, and all occasioned as Dr. Hamilton justly remarks, by partial and irregular distribution of the blood.

BUT what shall be said to the "mortifications in the bowels and bladder and livid spots on the surface of the body." Do not these shew the malignant state of the fluids? I answer, that I am much disposed to

doubt the occurrence of these appearances in the actual disease. No modern physician has noticed them in his account of any dissection. Morgagni does not enumerate them in either of the two cases which were dissected for him by his friend Mediavi. He does indeed quote them from ancient authors, but little attention is due to them, for we well know that the mere occurrence of an accidental disgust to water in the delirium of a malignant inflammatory fever, or in mania, was sufficient to mark the disease with the name of "*hydrophobia*." On dissection, the appearances found were attributed to the action of the canine virus when in reality, they were occasioned by very different causes. The mortifications I therefore refer to a febrile cause, and not to the canine virus. The livid marks occasionally seen about the breast and neck, may be readily produced by the obstruction to the blood, from the convulsion of the throat.

THE emptiness of the veins, and the fulness of the arteries with fluid blood, upon which so much stress is laid, to prove the malignant nature of the disease, do not

occur so uniformly, as to authorise its use as an argument upon the present occasion. The industrious Morgagni* in summing up the various appearances observed by him in the blood vessels says “ as to what relates to the blood in general, the arteries of one were very full of blood, and the veins almost empty; in another the vena sine pare was almost empty, but the iliacs greatly distended therewith, and the corresponding arteries empty: the internal jugular veins and carotids were as empty as both arteries and veins within the cranium were full. In one the blood was not concreted in any part, but the whole was fluid and did not coagulate in the open air though cold, but in another, it was rather concreted than dissolved. Nor indeed have I found less variety in other dissections published by learned men: there is one who affirms that in thirteen bodies the veins were filled with fluid blood, but that the arteries were empty.” Thus then it appears, that this supposed important circumstance is not essential to the disease, and consequently no argument can with propriety

* Letter 8. Art. 30. 31.

be drawn from it to prove the truth of the theory, nor can it authorize the deduction as to practice which its admission would lead to. The emptiness of the veins, and fullness of the arteries, was moreover, discovered in the late Mr. Hunter, in whom no previous symptoms of inflammation existed.

It is acknowledged that “two cases of death from hydrophobia are related by Dr. Vaughan, in which no appearance of disease was discovered by dissection in any part of the body.” The cause is said to be the “violence of the morbid action,” which “prevents inflammation.”

But the absence of morbid appearances in the bodies of those who die of the disease, is so commonly observed; that it must be occasioned by a cause intimately connected with the nature of the complaint. Upon my theory of the exclusive action of the virus on the *nerves*, a solution is readily offered. Though death takes place merely from the action of a morbid cause upon them, they do not exhibit marks of disorganization. Their influence upon our systems, and the

intimate connection between the preservation of their healthy state, and the regular discharge of all our functions, is one of the mysteries of our nature. But,

1. I HAVE already shewn, that the progress of the symptoms is essentially different from those that occur in other diseases, in which this arrestation of the powers of the blood vessels takes place.

2. WHERE this morbid action occurs, to a great degree, the blood is dissolved, and it will not recover its usual consistence, much less exhibit an inflammatory buff, until after the second or third bleeding. Every one whose lot has been to attend cases of the American pestilence which has ravaged various towns in the United States, and who has followed the successful practice of Dr. Rush, has seen this occurrence. But do we see any thing in the disease produced by the canine virus to induce a belief, that this intensity of action, takes place? Certainly not;.....In no case where blood has been taken away to a great extent, has it been observed to be dissolved, nor has the buff afterwards appeared; Nay, in the very bodies in which no marks

of disease were found after death, the blood previously drawn, has resembled that of healthy persons. It must be understood, that by this general assertion, I allude to those *real cases* of the disease recorded, and resulting unequivocally from the action of the canine virus. To this assertion there is but one *apparent* exception, and that occurred in Master Rowley, whose blood was remarked to be sizzly. But this appearance must without doubt be ascribed to the effects of the mercurial stimulus on his blood vessels, under which he had sometime laboured, as a preventative mean. Whenever the appearance of the blood is mentioned in other cases, it is noticed to be no way different from that of healthy persons. Of the accuracy of this fact, numberless cases might be quoted.

3. It is natural to expect, that in those cases where the supposed excess of stimulus was in part removed by bleeding, the vessels would act, and produce inflammation, agreeably to the theory delivered before. But in Dr. Rutherford's case although 66 oz. were taken away, yet no appearances were discovered on dissection, different from what might

be found in those who die of asthma. This case of itself, I consider as decisive in its authority.

I APPREHEND therefore, that but little support will be derived to the theory of the inflammatory nature of the disease in question, from the appearances of the bodies of those who died of it. I early remarked the little light which was to be derived from dissections, and I may again repeat with Morgagni “that much as the afflicted differ while living, they differ more after their death.” Succeeding observations have fully confirmed this truth.

I SHALL now proceed to examine the validity of the proofs to the theory, derived from the cure of the disease by depleting remedies.

THE first case adduced by Dr. Rush, is that related by Dr. Nugent, who is said to have cured a woman by the loss of 47 oz. blood, in three bleedings. I agree fully with Dr. Hamilton in thinking that the canine virus had no concern in this case, and ascribe with him to “imagination and appre-

hension of danger, the chief symptoms which are attributed to hydrophobia." This objection I have in another place, had occasion to urge, against the admission of Dr. Nugent's as a true case of the disease in question, and I may add further; notwithstanding the patient "suffered great anxiety from the bite, yet she remained perfectly well, until her fears were in some measure rendered probable by the death of a dog bitten by the same animal as herself." After taking a variety of remedies, which from their direct opposition to each other, could have had no share in the temporary abatement of the disease, "she nearly recovered, when the whole of the symptoms were a second time renewed, by the officious interference of a person who told her, that her recovery was *impossible*. At this as might be readily expected she was greatly moved, and instantly thrown into violent hysterics, and no doubt had both a dread of fluids, and difficulty of swallowing, symptoms which this protiform disease has actually put on.*"

* Hamilton on Hyd. 1st Edit. p. 126.

MR. Wrightson's supposed case of the disease,† was certainly a temporary phrenzy. Dr. Hamilton is also of this opinion.....Two reasons offer to support it: first, the symptoms appeared in three days after the bite, which is a period much earlier than has ever been known, except in warm climates, and 2dly, because the remedies were not carried to that extent which the disease would have required, had it been inflammatory, and actually had proceeded from the canine virus. Twelve ounces of blood only were taken away: now in a disease said to be the most inflammatory to which the human body is subject, the loss of this quantity of blood could profit nothing. The absurd combination of musk and the two cinnabars was also taken internally: thirty drops of laudanum were also given when the patient was first visited, and one grain and a half of opium every three hours after; this remedy appears to have had a sensible effect in abating the spasmodic affections; but judging from the general inefficacy of that medicine in the real disease, when given in much larger doses, it is evident, we must refer the complaint to ano-

† Med. Trans. vol. 2.

ther source than the virus. Thus Dr. Vaughan gave fifty seven grains of opium in the course of fourteen hours ; used half an ounce of laudanum applied by way of injection, and an anodyne plaister to the throat. In the last case, he relates, “ opium was given with no sparing hand,” to a child eight years old, and yet it is observed, that not even a short relief was obtained. Dr. Wavell* also gave fifty grains in 24 hours, besides opiate clysters, without any benefit : and Dr. Babbington,* apparently determined fairly and fully to try the effects of the remedy, gave 180 grains in twelve hours, without inducing sleep, or producing the smallest alleviation of the spasms.

MR. Falkner's† treatment of another supposed case of this disease by “ *copious* bleeding aided by mercury,” is also quoted. The quantity of blood taken away is not mentioned, but it is clear that the mercury was the active remedy ; for no abatement of the delirium, or other symptoms was perceived until after the system had been saturated with that min-

* Med. records and researches.

† Med. Trans. vol. 2.

eral by frequent frictions, and by the turpith bolus. After this, it is said, "the disorder became less and less ; a salivation came on, and during the first days of it, the woman recovered her senses perfectly."

The patient cured by M. Poupart by bleeding until fainting appears to have laboured under a common mania. I find upon reference to the case, that she is stated to have been *enragée*, and that "she was bound to her chair for a whole year, and confined to a diet of bread and water." Now we know that the disease arising from the canine virus never continues so long, and when an alienation of mind is produced, it is but momentary.

MR. Berger also mentions that several persons were bitten and cured by bleeding, and that two others who were not bled, died. These cases together with that of Mr. Poupart, are copied by Plenck,* from the trans. of the acad. of sciences of Paris for 1699,

* Bibliotheque choisie de Med. 12mo. tom. xv. p. 212.

but the particulars which are so essential to the formation of a just opinion of the disease and its treatment, are not given. It is impossible to say positively, whether the cases reported to have been cured were not like those of Dr. Nugent, Mr. Wrightson, and others, no way connected with the canine virus ; and in a question of so much importance as the present, no attachment to theory, or to particular modes of practice, ought to lessen our caution in the admission of any facts, but those free from every shadow of objection. I think therefore I am warranted in throwing aside the cases related by Berger as inadmissable. With respect to the bleeding moreover, I am at a loss to determine whether it was used in these last cases, as a preventative, or to cure the disease. If with the first intention, but weak conclusions can be drawn of its success ; for we know from the testimony of Mr. Hunter, Dr. Vaughan, and other authors, that not one of a great many persons who are bitten, are ever attacked by the disease. Nay, Dr. James says he knew a footman who had been bitten by “ dogs manifestly mad,” at three different times, and who remained well, though nothing was

ever done for him.* If the bleeding was used with the latter intention, I will oppose to the weight which the success of the remedy will carry with it, the authority of twenty in which it has failed, for every one which in a recovery has taken place after its use. The recovery of certain persons in the American pestilence or yellow fever, from the use of certain remedies formerly in vogue, might in like manner be supposed to prove their utility, and yet we now see them totally proscribed as injurious ; I need but mention the general use of bark and wine, which have so fortunately given way to mercury, and the lancet.

THE particulars of the case said to have been cured by Mr. Le Compt in France, “by copious and repeated bleedings,” ought to have been stated, in order that an opinion might be formed as to the actual nature of the symptoms removed by him. As they have been kept back, I must suspend my assent to the efficacy of the remedies, until I know that the disease was not mistaken for some other complaint, but that it actually proceeded from the operation of the

* TREATISE p. 40.

virus. Nay, even granting the reality of the case, it is very probable the patient owed his recovery to the strength of his constitution, by which he was enabled to withstand the effects of the repeated bleedings prescribed for his cure.

As to Dr. Tilton's patient cured by copious bleeding, I have formerly shewn it to have been a case of violent hysteria bordering on mania, a combination we sometimes meet with in practice. I shall not therefore now repeat the arguments in favour of that opinion.

DR. INNES' patient is the last quoted to prove the utility of blood letting. In this case a most violent inflammation of the stomach attended by as violent a disgust to water, was cured by the loss of 116 oz. of blood. This case has been frequently quoted by authors as an instance of the spontaneous origin of the hydrophobia, and I have already had occasion to notice it on that account, and to assert the impropriety of calling it by the same name with which the true disease produced by the canine virus is designated. We all know that the inflammation of the stomach is increased by swallowing any thing, and in the

definition it is said, as constituting a part of its peculiarity "*ingestis quibuslibet auctus.*" The case described by Dr. Innes therefore, with equal propriety might have been noted for a dread of solids as of fluids; and the reason why the disgust to fluids appeared so strong, was, that the absence of the sensation of hunger, and the great prevalence of thirst induced him to crave only drink, and the pain excited by swallowing it, produced after frequent attempts so powerful an association of ideas, that the same effects followed the mere sight of fluids, as are produced by the attempts to drink in the true disease.

DR. RUSH aware of the objections which may be offered to the arguments in favour of the inflammatory nature of the disease, derived from the occurrence of a disgust to water in cases where no virus was concerned, as in Tilton's and Innes's patients, observes; "that the morbid actions were exactly the same in both patients with those which are derived from the bite of a rabid animal. There is but one remote cause of disease and that is stimulus, and it is of no consequence in the disease now under considera-

tion, whether the dread of water be the effect of the saliva of a rabid animal acting upon the fauces, or of a morbid excitement determined to those parts by any other stimulus. The inflammation of the stomach depends upon the same kind of morbid action, whether it be produced by the contagion of the yellow fever, or the usual remote and exciting causes of the gout. An apoplexy is the same disease when it arises from a contusion by external violence, that it is, when it arises spontaneously from the congestion of blood or water in the brain. A dropsy from obstructions in the liver induced by strong drink, does not differ in its proximate cause from the dropsy brought on by the obstructions in the same viscus which are left by a neglected, or half cured bilious fever."

THE first of the above remarks I am under the necessity of denying. The only symptom in which these supposed spontaneous cases of the disease resemble the true disease, consists in the disgust to water. This trifling resemblance ought not to be mentioned as a ground for establishing any

principles or analogy; it is merely to be considered as an accidental circumstance coexistent and inseparably connected with the affection of the throat, the occurrence of which is of no importance except as an aggravating circumstance, and which gives rise to no indication in the cure: and granting that there is but *one* remote cause of disease, viz. stimulus, it surely cannot be contended that the great difference which we observe in the nature of the various stimuli, does not cause any difference in the nature of the effects produced. In reply to the illustration of the doctrine quoted, I may ask, does the inability of resting, and the difficulty of breathing in ascites and peripneumonia notha give rise to no difference in the nature of the two diseases, and to the remedies proper for the removal of these symptoms? Is not delirium very differently cured when it proceeds from the atonic state of the brain that occur in typhus, and when it proceeds from the high tone of the vessels in phrenitis or pneumonia?

LET any one stop here, and seriously reflect whether the answers which will be offered to him upon the proposition of these

questions, will not entirely overthrow the position respecting the universal identity of the remote causes of diseases. But if the occurrence in inflammatory complaints of an accidental symptom of the real disease, should finally be received as an admissible argument in favour of its inflammatory nature, I hope I shall be permitted to oppose to its influence the weight of the fact of the occurrence of this supposed important symptom in diseases decidedly depending upon great debility. Thus Mr. La Coste* the French translator of Dr. Mead's works, relates that he saw a hydrophobia appear in typhus. Schenkius also relates its occurrence in low putrid fevers, and I have long since remarked it as an attending symptom of hysteria and tetanus: but I forbear to derive any advantage from these facts unless it shall be generally agreed to admit the legality of those they are intended to oppose, and to which I think it will be granted, they are a full counterpoise. It may not be improper here to remark that the occurrence of the dread of water in typhus as mentioned by La

* Med. Comment. vol. 4. p. 369.

Coste, was one of the first facts I met with when considering the ~~subject for the~~ nature of the true disease, and in the commencement of my speculations upon it, I imagined that it afforded a powerful argument in favour of the principle of debility which I had adopted as its cause, but I soon perceived upon reflection that the dread of fluids, though it gave name to the disease was merely an accidental attendant thereon; that being an effect of a symptom viz. the affection of the throat, it gave rise to no indication in the cure, and consequently no argument derived from its occurrence in diseases of acknowledged debility, ought to authorize the same mode of treatment in the real disease, more than the occurrence of angina in any complaint, should point out the treatment for that disease when it appears as an idiopathic affection. I am as great an enemy to the servile dependance of physicians upon nomenclature as any one can be, and I sincerely rejoice in the prospect of the speedy termination of its reign, but in the rage for revolutionizing medicine, let us be cautious in drawing any conclusions as to the cure of an awful disease from the prevalence of one of its symptoms in other complaints, which are directly

opposed in every circumstance to the one in which the symptom most generally occurs.

DR. RUSH “grants that bleeding has been used in some instances in hydrophobia without effect, but in all such cases, he thinks it was probably used out of time, or in too sparing a manner; and he begs that it may not be tried in this disease, by any physician who has not adopted in their utmost extent the principles and practice of Botallus and Sydenham in the treatment of malignant fevers.”

IN answer to this I would observe, that the theory of the inflammatory nature of the disease was very early adopted by physicians, from the apparent vigour exhibited by those labouring under it, and the depleting system was in consequence deemed the only one by which a recovery was to be effected. Boerhaave was so firmly persuaded of this opinion, that he says “bleeding is to be used non *parca manu, sed iterum iterumque repetenda usque ad deliquium animi.*” The advice was enforced by his laborious commentator Van Swieten, and rigidly followed by the pupils of

the great systematic. But was the result uniform success? By no means. With one single exception, (and this I shall presently shew is not free from suspicions of its not having been the real disease,) death almost constantly followed. As it may tend to carry weight upon this subject I will just mention one case in which bleeding had a fair trial, without success. The case is related by Dr. James Johnson;* the subject a man grown, began the debilitating plan as a preventative, two days after the reception of the bite, by losing 16 oz. of blood; and when the disease actually commenced, he was again ordered bleeding gradually to the amount of 66 oz.: but though he became sick from the evacuation, and was purged both by medicines and injections, and the plan of treatment was assisted by a total abstinence from food, yet the disease continued to advance until death closed the scene. It is moreover remarked that the blood exhibited none of those marks which Dr. Rush mentions as appearing in the blood of persons in the disease; it was not sizy, the serum was abundant, and the crassamentum though florid

* MEM. Med. Soc. Lond. vol. I.

was loose. Dr. Rutherford who was at that time clinical professor at Edinburgh, in the lecture which he gave upon the case, declared his belief of the error of his master Boerhaave, and his firm determination to treat the disease upon principles directly opposed to those by which his practice had been guided. This case made a strong impression on my mind, when I began to consider this subject, and I noticed it in my first publication on the disease.* I still consider it decisive as to the injurious effects of the debilitating plan of treatment, were there no other proofs to assist with their authority.

THE person whose case is related by Dr. Ferriar,† had been bled for the cure of a pulmonic complaint, which came on twice, between the reception of the bite and the appearance of the disease. The second time it attacked him was immediately before the operation of the virus. It is merely stated that "his complaints were removed the first time by the usual remedies ;" it is there-

* Amer. Museum vol. 8. 1790.

† Med. Hist. and Reflex. vol. 1

fore probable that bleeding was used ; the last time it is expressly stated, that “ he was *twice* bled ;” the disease however proved fatal.

BUT further, Dr. Hamilton* has collected with great industry no less than twenty two cases of this disease, in which bleeding had been used to *various* degrees and without success. It is true the remedy was not carried to that extent, which its respectable advocate thinks necessary for its success, but it must be recollected that some of the patients were children, in whom the quantity taken away would have produced some effect in all other diseases however inflammatory, and that the degree of supposed inflammation could not, in the common course of things, have been the same in all the rest : it is therefore granting too much to suppose, that the quantity of blood taken away was not commensurate to the exigencies of some of the various constitutions in which the remedy was tried ; and yet we find that in every case not the least alleviation of the symptoms took

† Remarks on Hydrop. vol. 2. append. p. xx.

place, on the contrary, death followed equally certain, as in other cases where no bleeding was used, or where the disease was permitted to run its course,.....remaining even to this late day a melancholy example of the weakness of our boasted knowledge in medicine.

THE only case which can in the least support the doctrine that the disease depends upon excess of action, is that related by Dr. Hartley in the 40th vol. of the Phil. transactions. In this case, a man was cured of a disease attended with raving, and a dread of water, and which was supposed to have originated from a bite received two months before, by the loss of 120 oz. of blood in seven bleedings. But I think we may fairly suspect that the disease thus said to have been cured did not proceed from the canine virus. For, 1. it lasted during *seven* days, a period longer by three days than the usual duration of the real complaint. 2. The actual madness of the dog was not ascertained. 3. As some of the most distressing symptoms of the actual disease are not noticed, we have a right to conclude they did not appear, such as the weak pulse, the hurried breathing,

catching of the breath, and the sense of suffocation upon being exposed to a current of air. 4. The violent vertigo, sudden loss of speech and of memory, and sullen disposition with which he was affected, seldom if ever occur in the real disease. But even granting that this was a case of the true disease, I think the authority attempted to be derived from its use is outweighed by the numerous and well attested cases recorded of the injurious effects of the debilitating plan of cure.

I HAVE now gone through all the arguments adduced by Dr. Rush in support of the opinion that the disease produced by the bite of a mad dog, is a malignant fever. For other arguments in favour of the opinion that it originates from debility, I must refer to my former essay on the disease.

IN my inaugural essay, I recommended the use of opium, on the principle of its antispasmodic virtue, but in much larger doses than it had ever been prescribed in the disease, because I perceived that the small quantities which had always been prescribed,

never in the least mitigated the symptoms. But from the examples before stated of its inefficacy, even when given in larger doses, than I thought the system could bear, I am now convinced, that it is loosing time to trust to it. In its place I would recommend the use of the powdered leaves of Stramonium† or their extract, in doses of two grains for an adult. By that quantity Dr. Cooper|| found the pulse “increased in frequency at first, and that it afterwards became full and quick, and produced giddines, *warm skin*, moist hands, and *sleepiness*.” A defect of due energy in the heart, wakefulness, and cold skin are symptoms that constantly attend the disease, and the two last are the sources of much distress. Hitherto no remedy has had the least effect in removing them. Their cure will greatly assist toward the removal of the whole complaint. This may be effected in my opinion by the *stramonium* if given *early* in the disease. It should be exhibited in such doses as will *powerfully affect the system*, and repeated as often as a

† Thorn apple, or James Town weed.

|| Inaug. Dissert. 1797.

previous dose has ceased to act. During the suspension of the symptoms, bark and wine ought to be given, and the dose gradually increased, so as to keep up a regular excitement and produce a permanent vigour in the system. The quantity of wine may be unlimited. Indeed the only rule that ought to be observed with respect to it, is, to *give it in as large quantities as the stomach will bear, and until it produces the desired effect.* For this disease exhibits a singular instance of the concentration of sensibility in certain parts of the body and of a great defect of it, nay almost a total exhaustion of it in another. We see the same thing in tetanus and other diseases. Thus while the eye cannot bear the sight of a looking glass, or a vivid colour; nor the ear the shutting of a door, nor the skin nor lungs the impression of the air, the stomach is so insensible to the impression of stimuli, that a bottle of wine will not produce as much effect on the pulse as a few glasses will in times of health. Dr. Currie of Liverpool cured a case of tetanus by 110 bottles of wine, and observes that “ebriety was not produced; it soothed the irritation of the nerves, and comforted the mind, and

without increasing the frequency of the pulse, it augmented it in strength.†”

IN case however the above remedies cannot be obtained or exhibited; I should have no hesitation in trying another plan which has several arguments to authorize the experiment, although at first view it may appear to be attended with danger. It is, *to excite a strangury by means of Cantharides.*

THE principle of the animal œconomy first unfolded by J. Hunter, of one irritation curing another is daily and amply confirmed in practice, and its application in the present disease seems highly probable. Without refering to the many instances afforded, in illustration, I may adduce one disease which is nearly allied to the present, viz.

† THE defect of sensibility exhibited by persons labouring under some diseases of debility is really astonishing. A delicate young lady of Philadelphia, a few years since, was recovered from the lowest state of a typhus or low fever, by the use of 127 bottles of old Madeira, which was given to her, at her own request, when so weak, that she could scarcely be heard to pronounce the single word “*wine.*” From one table spoonful she took at last, two bottles a day. I have also seen children in the *cholera*, or summer complaint, bear almost incredible doses of stimulants.

tetanus. When this is occasioned by the lesion of a nerve from a rusty nail, or other pointed instrument, we find it readily yields to an irritation of the wounded part, raised by scarification and the application of hot turpentine, marine salt, or cantharides ; and in the progress of the disease, or when it succeeds the exposure of the body to dews and night air after being heated in summer, an irritation of the salivary glands by mercury as readily proves effectual. A knowledge of these facts, and a conviction of the truth of the principle would have been sufficient to prevent my hesitating to try the plan I propose, but I am now confirmed in my opinion of its utility and perfect safety, in consequence of the cure by its use, of a desperate case of tetanus, by Dr. S. Brown of Lexington Kentucky, who lately communicated the history to Dr. Rush. The patient, a lady, was nearly exhausted by the disease, when her judicious physician gave her the tincture of cantharides, which by exciting a temporary inflammation in the stomach and bowels, and producing a stranguary, effected a cure. The most dangerous pleurisies have also been cured by the late Dr. Lieper of Maryland, after the common remedies had failed, by exciting a stranguary by means

of the same tincture mixed with camphorated spirit of wine;* and when combined with tincture of Peruv. bark, and given with the same view, it has been recommended by experience, in the hooping cough,†

THE recommendation of the remedy in this disease produced by the bite of a mad dog is not new. Morgagni mentions its general use for the cure of the disease in Germany; his remark is confirmed by a late author‡. A Silesian peasant also, acquired much reputation for the cure of the disease, and on the purchase of his secret by the King of Prussia in 1777, the basis was discovered to be the *meloë proscarabæus et majalis* || (oil beetle.) All the insects of the meloë tribe, possess a blistering quality. In a disease which has hitherto so generally proved superior to all the efforts of medicine, it is a duty to try every plan which promises the least success. The one I now urge, is supported by a just theory, a close analogy, and

* RUSH'S works, vol. 4. p. 35.

† Lettsom's Mem. Lond. dispensary.

‡ Diss. de Hyd. Auct. G. Uiberlacher, of Vienna.

|| See C. T. Schwartz de hyd., ejusque specifico meloë majalis, et proscarabæo, Halæ 1783. Tilloch's Phil Mag. vol. 6,

if we admit the German authority, I may add, is proved by experience. With respect to the safety of the measure, the bold practice of Dr. Brown leaves no doubt. But we every day see a stranguary produced by blisters in fevers, which sometimes continue for three or four days, without any injury following. An irritation of the neck of the bladder intentionally produced and continued for that period, would be sufficient in my opinion for a cure. The general morbid irritability of the system being subdued, would be followed by a restoration of the powers of swallowing, when tonics might be easily administered; and certainly ought to be given, in regular doses, so as to keep up the excitement. The association of ideas, producing the horror of water, being thus destroyed, the general or partial warm bath may be used to remove the stranguary, § if it should continue longer than necessary, and does not yield to the demulcent remedies

§ I have seen a stranguary of four days continuance in the close of a fever, when the patient was so reduced, that the necessary quantity of liquid could not be taken, during the application of several blisters, yield to the application of warm water poured from a bottle, on the lower part of the belly, as the patient lay in bed.

commonly used with success. If a soreness in the bowels should be troublesome, a mucilaginous and oily diet should be prescribed.

UPON the whole, although I have endeavoured to invalidate the arguments advanced by my respected friend and preceptor, in favour of the inflammatory nature of the disease in question, and though I am firmly convinced at present of the force of my objections; yet I have no hesitation in saying that if any one well authenticated case be adduced of the success of copious bleeding, I will give it a fair trial, and if I find the practice to succeed, I will reject my present theory. I earnestly recommend the subject to the attention of the American physicians, and request that they will either make trial of the remedies I have advised, or of the depleting plan of Dr. Rush; having reference however to the hints given of the practice of Botallus and Sydenham in the cure of fevers by its use, and to the cases related by Dr. Johnson and others, of the various quantities of blood taken away, without success.

— THE END. —

OBSERVATIONS
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Med. Hist.

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