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To the Editors of the Medical and Physical Journal.

GENTLEMEN,

AS it is a matter of the very highest importance, that medical improvements should be made as public as possible, as well for the credit and benefit of the profession, as the good of the community; I am happy in embracing the earliest opportunity of transmitting, for the information of your readers, the following Extract of a Letter, addressed by me to the Transport Board.

" HONOURABLE SIRS,

" In my last, I did myself the honour of communicating to you a new method which I had adopted, of refrigerating patients, ill of the ardent fever of Tropical Climes; I have now the pleasure to acquaint you, that I have improved upon the plan, and can in the space of a few minutes, so effectually cool a sick person, that he shall complain of an icy coldness, instead of a burning heat.—Therefore, the grand desideratum of reducing the temperature of the body to its natural and healthy standard being so easily effected, I have every reason to think, from the great experience I have had, that the fevers of this country will be brought more under the controul of medicine than they have ever *yet been*; for every medical Tyro knows that violent vascular action, and an increased degree of caloric, or matter of heat, soon destroy the principium vitæ, or what has been termed excitability, and kill the patient.

"The mode of refrigeration is as follows: I order the sick to be either sprinkled or sponged with ardent spirit; and though common rum of the country is what I have used, the higher the proof the more expeditiously will its effects be produced. I hope the simplicity of the thing will not, in the estimation of the Board, detract from its merits, for merit it certainly has, and will be the means, under the

Divine Blessing, of saving the lives of thousands of those brave men, on whom England places such great dependance. Had I time, and were I placed in a situation to do it, I would transmit the minutiæ of my practice; but what I have already pointed out, is the grand and leading principle of it, the basis on which every other indication rests. I have been very successful in allaying sickness and vomiting; blistering the epigastrium, opium, aq. menthæ pip. with the saline mixture in the act of effervescence, are remedies I use; the alkaline solution sweetened, I have put up in one bottle, and fresh lime juice in another; they are mixed almost in contact with the *lips* of the patient, and given instantly, before the carbonic acid gas has time to escape. It is not to be expected that every patient can be saved by any method, however improved it may be; yet, if one in ten cases can be preserved who otherwise would have died, the matter becomes a national object, and demands the serious and attentive consideration of the chief officers of the medical department, and of others whose talents and situations enable them to judge and appreciate, as well as to represent and make public what they conceive will benefit the service. I found the effect of the cold affusion to be so very transitory, that there was a necessity of continuing it every ten minutes, and after all, the fever would run on for days, continually wearing out the patient, losing time and himself. The refrigerating plan, with wet sheets, mentioned in my last, had a more expeditious and salutary effect. The method I have now had the honour of submitting to your consideration, I conceive to have arrived at the very acme of perfection. One pint of spirit is sufficient for one refrigeration; and three, will generally lower the pulse, and abstract redundant heat."

H. M. Naval Hospital, Antigua, June 7, 1807.

"I have great pleasure in stating to the Board, that the happiest effects continue to result from the application of rum; all the surgeons who have been made acquainted with this mode of refrigeration, are delighted with it; and such as have had an opportunity of trying it, declare that they have cured men in a few hours, who might otherwise have died, or remained ill for a long time; there is no doubt of the cold affusion being entirely superseded by its use, nor of the practice being generally adopted here, and elsewhere.

"As every thing is to be done on the first attack, and the surgeons are armed with so powerful an antidote, it is to be hoped

hoped that the mortality hitherto so prevalent, will at length be very considerably arrested in its progress.

" Before concluding, I beg leave to mention a remedy which I have discovered against Ulcus Gangrenosum, which might hold a place in a Dispensatory, under the name of Pulvis Antiphagedænicus, vel Pulvis Cinchona Nitratus Opiatus.

" R. Pulv. Cinchona ℥i. Pulv. Sal. Nitri. Pulv. Opii aa ʒi. M. ft. Pulvis inspergendus super ulcere quoties opus fuerit. The decoctum cinchona with some of the compound tincture is to be taken at the same time, and continued until the ulcer be healed.

" I am, &c.

RALPH CUMING, M. D."

To the Editors of the Medical and Physical Journal.

GENTLEMEN,

I Wish to make known, by means of your Journal, a Styptic Powder against external bleeding from *small* blood-vessels. It is a good application to stop the bleeding from leeches, when necessary; it has been found useful in various bleedings from small vessels, and particularly in an alarming bleeding from ulceration of the corpora cavernosa. Its composer designed it as a solid form of the ingredients of the tinct. antiphthisica.

R. Ferri vitriolati (sal martis.) Cerusæ acetatæ pondera æqualia, in mortario marmoreo misce, liquescentem materiam modico calore sicca, & in pulverem satis subtilem tere.

In very slight cases, a small quantity of the powder, with a bit of lint and sticking plaster, will suffice. When the bleeding is more profuse, the powder must be applied pretty thickly, (the more so the better,) lint spread with unguentum resinæ, or something of that kind, must be put over it, and secured by sticking plaster, and a proper pressure must be made. The powder forms with the effused blood a firm body, capable of resisting any farther effusion from small vessels; but it is necessary that the blood should be prevented from flowing through the powder at first by some ointment spread on lint, and pressure.

I make no doubt that many persons will suppose that

lint and pressure would be equally effectual without the powder, but on trial it will be found otherwise.

There is one inconvenience attending the use of this powder, that if the wound is wiped quite clean, the skin close to the edges will be tinged of a reddish colour, which will remain a considerable time, perhaps a year; but this is easily avoided. I am, &c.

July 26, 1807. ROBERT JOHN OKES.

Cases of Fractured Sternum, with Observations; by M. SABATIER, Surgeon to the Hotel des Invalides, at Paris.

WHEN the situation of the Sternum, and the thinness of the teguments which cover it, are considered, it might naturally be supposed, that this bone would be very liable to fractures, more especially as its substance, not being extremely compact, can oppose little resistance to external injuries. This, however, is a case of rare occurrence, in comparison with similar accidents sustained by other bones. Few writers on Surgery, not even Hippocrates himself, have treated of fractures of the sternum; while by others it has only been slightly noticed, and compared with those of the scapula, and ossa ilium.

I. L. Petit and Duverney afford an exception to this remark. The former of these gentlemen, in particular, to whom we are indebted for many improvements in the practice of surgery, has greatly contributed to elucidate this subject. According to him, fractures of the sternum bear much analogy to those of the cranium, and frequently require similar operations. He observes, that when a portion of the bone is displaced, in such a manner as to produce a dangerous pressure on the organs contained in the chest, as well as in cases apparently less formidable, wherein the osseous parts are not depressed; this accident has been attended with effusions, the consequences of which are equally to be dreaded. Martiniére has adopted these principles, in a Paper inserted in the fourth volume of the Memoirs of the Academy of Surgery, where he shews the propriety of applying the trepan in cases of fractured sternum, whether produced by falls, gun-shot wounds, &c.

This operation is fortunately, however, not always necessary, as the two following cases, which fell under my own observation, fully evince.

A middle-aged man, of a robust and sanguine temperment, was run over by a heavy loaded carriage, the wheel of

of which passed over his chest from the right side to the left, in consequence of which the sternum, and two of the true ribs, on the left side, were fractured. He instantly vomited a large quantity of blood, and became so weak, that his life was despaired of; a crepitation was heard within the chest, which evidently proceeded from the friction of the fractured portions of the sternum against each other. The inferior portion was pushed forward at each inspiration, and carried backwards during every expiration, while the superior portion remained relatively immovable. I vainly endeavoured to retain them in a proper position by means of pressure, with the hands, to the inferior and anterior parts of the chest. The agony of the patient was, however, augmented, and the difficulty of respiration became so great, that it could not be persevered in above a few minutes. I was therefore obliged to content myself with applying a loose bandage to confine the compresses, which were dipped in a mixture of *oleum hyperici* and ardent spirits, and placed over the whole extent of the contusion. As the pulse was extremely feeble, and the extremities cold, I ordered the patient to be covered up with warm blankets, which very soon recalled the natural heat of the body, and permitted me to employ the lancet. The bleeding was repeated seven times in the course of three days, at the end of which he breathed with so much greater freedom, that he could continue in bed. It may be proper, however, to mention that he remained in some measure in a sitting posture, and so much bent forward, that it was necessary to support him by means of a chair placed flat on the bed and covered with pillows. This was indeed the only position which he could endure for a moment, during the first eight days, through the whole course of which he was restricted to water gruel and mild mucilaginous drinks. By this treatment, the dyspnœa and the pains in the chest became gradually relieved; he occasionally obtained some short intervals of repose; the expectoration was less mixed with blood; the crepitation diminished, and in a short time could no longer be heard. At the termination of three weeks from the accident, the situation of the patient became much improved, but his health was not fully re-established till two or three months afterwards. He continued, however, to experience a slight difficulty of breathing, though on inspection it was scarcely possible to discover any vestiges of the fracture.

From the extensive nature of the injury, I experienced great uneasiness respecting the event. At first I dreaded

lest the lungs, the pericardium, and the heart had been bruised, or lacerated, as happened in two or three of the cases related by M. Duverney, and that the life of my patient would terminate in a few hours; and even after such immediate danger was no longer to be apprehended, I conceived that an accumulation of blood or pus might take place opposite the sternum, or in the cells of the mediastinum, and that it would be necessary to lay bare a portion of the bone, to afford a free exit to the effused matter, in case it did not escape through the interstices of the fractured parts, or to have recourse to the employment of the trepan, if these openings were not sufficient. A short time, however, happily proved this conjecture was also unfounded; but the motion of the inferior portion of the bone, and the crepitation, which continued sufficiently audible, rendered me uneasy, lest the fractured bones might not unite, and that a greater difficulty of breathing would remain than that which the patient had yet experienced. It is well known, that in order to facilitate the union of fractured bones, absolute rest is necessary; and, that if this condition be not fulfilled, the cure is not only greatly retarded, but sometimes wholly prevented. Nevertheless, Nature in the present instance surmounted circumstances the most unfavourable; and if a complete callus was not produced, as is most frequently the case, at least the osseous parts became consolidated in such a manner, that no farther motion was perceivable, and the functions of the thoraic viscera were performed as usual.

Since witnessing the above case, which occurred some years ago, I have had occasion to observe the efforts of Nature in an instance of fractured ribs, which, though very different from the one just related, affords an equal proof of her power in circumstances which preclude the interference of art.

A young robust coachman having fallen from his seat, one of the wheels run over his chest from left to right, by which accident the last two true ribs, on the left side, were fractured; the undermost of them in two places, so that the intermediate portion of bone was completely detached. Its motion was very considerable, and it was evidently carried towards the inside of the chest in the act of inspiration, and again thrown outwards during expiration. The consequences produced by this singular fracture were not, however, very formidable, and the patient was completely cured in the space of a short time.

A second case of fractured sternum, which lately occurred in my practice, did not prove equally fortunate in its

its termination as the first. A man upwards of 60 years of age, after having been violently bruised and beaten with the fist, was thrown by the individuals who assaulted him into a ditch thirty feet in depth. He fell on his back, and the shock was so great, as wholly to deprive him of the power of motion, and he remained in the same position from eight o'clock in the evening until the same hour on the following morning. He was removed with much difficulty from Vincennes, where the accident happened, to the Hôtel des Invalides. On being brought there, he was extremely weak; his respiration was convulsive; he expectorated a considerable quantity of blood, and complained of pain and soreness over all his body, but particularly in the region of the sternum. One of my pupils, who first saw him, perceived on examination that the sternum was fractured transversely, at the place where the first bone forms a junction with the second, and that this had been forced beneath the other. He endeavoured by the employment of lateral pressure to restore the bones to their natural situation; but not succeeding in his attempts, he bled the patient, and applied over the chest, and particularly to the lower part of the sternum, thick compresses, previously soaked in an embrocation, and supported by means of a roller passed round the body, and drawn somewhat tight at the lower part. Things were in this state, when I arrived. His respiration was performed in so singular a manner, and his pulse so extremely languid and feeble, that I conceived it impossible to afford him any assistance. Contrary to my expectations, however, I found him still living on the following morning; his pulse had become somewhat stronger, his respiration was not quite so laborious, and he had expectorated a considerable quantity of mucous matter. He still, however, appeared in very imminent danger. Besides a renewal of the embrocation, I prescribed for him a balsamic and pectoral decoction. For a few days, the symptoms continued nearly stationary, except that he appeared to suffer less, and to expectorate more easily, so that I began to entertain hopes of his recovery, and that he would acquire a sufficient degree of strength to allow us to put in practice some of the means recommended for raising up the depressed portion of the sternum, when he died on the eighth day from the accident.

On dissection, I found the sternum fractured at the place already mentioned, and the inferior part of the bone not only depressed, but forced in a little way beneath the superior portion. There was a considerable extravasa-

tion of blood beneath the integuments, as well as in the substance of the right lobe of the lungs, which every where adhered to the pleura; but this adhesion, it was obvious, had long existed. On the left side, neither the lungs, pericardium, nor heart, had suffered any injury, and there appeared no effusion of blood, which could be attributed to the rupture of the mammary arteries. This rupture is, however, one of the sequelæ most to be dreaded in fractures of the sternum, though even the organs contained in the chest should have suffered no material injury. The most remarkable circumstance in the present case, was the relative situation of the fractured bones. So far as I am acquainted, no writer on this subject has noticed it; because the bones of the sternum seldom overlap each other, even when the fracture is complete and accompanied with depression. The action of the intercostal muscles, which tends to diminish the interval between the ribs, and bring them together, should seem, however, capable of frequently producing this effect. It is easy to conceive that in such a case, the crepitation, which is one of the most certain signs of fractured sternum, and which, in my first patient, was distinctly audible on the least motion of the chest, without any pressure being employed to the injured parts, could not occur; and that if the external teguments were so much tumefied, that we had no other method of determining the nature of the injury but by the touch, we might readily fall into error, or form a false judgment respecting the real state of the part. I am also of opinion, that the processes generally recommended to restore the depressed bone to its natural situation are wholly unavailing. For how in fact can lateral pressure on the chest succeed in replacing it, though even the spine be bent backwards, with the intention of forcibly separating the superior from the inferior portion of the sternum? Neither is the method more available which some recommend, of exposing the fractured part, and of raising the depressed bone by means of an elevator, &c.

In cases wherein the overlapping is not so great as in the present instance, it might perhaps be possible, by this or similar means, to bring the divided portions of the bones into contact, or juxta position, with each other, but it would be wholly impracticable to retain them in that situation; and it is much to be feared, that the cause originally productive of the derangement, which we have pointed out, might displace them anew, especially if the fracture was in a very oblique direction.

The conclusion which results from these observations, is, it must be confessed, extremely depressing, since it is evident, that certain fractures of the sternum are beyond the power of art to remedy, and that they even sometimes prove mortal in cases wherein the internal viscera have sustained no very material injury. Instances of this last kind, must, however, have occurred more frequently, previous to the period at which Martiniere published his reflections on the application of the trepan to the sternum; as in fact, internal effusions, which require this operation, often occur without any displacement of the bones, and when they have merely sustained contusion.

It may be here proper to remark, that the case above related, exhibits a singular and incontestible instance of repercussion in the chest. This frequently happens in injuries of the head, and the cause is too well known and too intimately connected with the changes that spherical bodies undergo when they are struck, to render it necessary to enter into any reasoning on its possibility. It is well known that instances of a similar kind occur in other parts of the body; and that the injuries occasioned by strokes or falls frequently appear at a great distance from the parts on which the blow had been inflicted. In this last case a continuity of motion, or concussion, is communicated to the soft parts contained in the large cavities of the body. But in the present it is very different, and is produced in the same manner as fractures of the cranium frequently occur in the side opposite to that which received the stroke, and which are not an effect of continuity of motion, but proceed from that change of form which is the necessary result of repercussion on spherical bodies, but which is only evident when this re percussion is extremely violent, and attended with consequences equally marked as those of the case under consideration.

If any doubts remain on this subject, they may be easily resolved by comparing my observations with those in the fourth volume of the *Prix de l'Académie de Chirurgie,* in a memoir by Basile, respecting re-percussion in fractures in other parts of the body than the head, and in which he examines whether a fracture can occur in the sternum in consequence of a fall on the back. In the instance to which he alludes, the patient had fallen from a height of fifty feet, and there was no external mark indicating that he had pitched on the forepart of the chest. On the contrary, it was affirmed, that his back had struck a projecting portion of a scaffold before he reached the ground.

From

From this circumstance, the author takes occasion to suggest a curious explanation of the fact; he conceives the sternum must have been ruptured by the simultaneous action of the muscles which are inserted into the superior and inferior portion of this bone, and which had contracted with such force as to prevent the body bending backwards at the moment of receiving the shock. Be this however as it may, a species of re-percussion must still have been produced, though differing as to the cause from that which it appeared to have in the case I have narrated.

On the whole it must therefore appear equally necessary to extend our examination to parts at a distance from those which have been struck, in cases of blows or falls on the chest, as in those of the cranium, since similar injuries, and probably from the same cause, may occur in both instances.

To the Editors of the Medical and Physical Journal.

GENTLEMEN,

YOUR facetious Correspondent, designated by the letters T. Y. has, in a late number of your Journal, indulged, at no small length, in sportive comments on the heating and cooling methods of treating Burns and Scalds. Well timed *wit* must ever have its due effect; unenviable indeed would be the insensibility that could be *dead* to its pleasing influence, when seasonably employed. It would be difficult, however, to specify on what occasion *wit* could be either *well-timed* or admissible on a subject of medical investigation. Your Correspondent, therefore, has not been strictly judicious in adopting the ludicrous style of discussion displayed in the Paper referred to; nor has he been more correct in instituting a scheme of experiment that necessarily must be nugatory, from the incongruous circumstances in which it was conducted.

It is not compatible with any known law of motive power obtaining in the animal economy, for any part of the system so far to admit of the agency of contrary powers, as to produce a distinctness of effect, accurately corresponding to the action of the different causes. The idea is indeed a paradox, and the operation of such opposing influence must go either wholly to annul, or greatly to diminish the respective efficiencies of either agent. Heat and cold are not the same states; they are in fact, different conditions of the same thing, and necessarily destructive of each other.

Your

Your Correspondent might surely have an opportunity of gratifying his anxiety for solving the question respecting the comparative advantages of the heating and cooling treatment of burns and scalds, by making his experiments on different subjects. Occasions for trying the effects of remedies in such cases are not rare; they present in ample abundance, and will, no doubt, hereafter, enable your correspondent satisfactorily to ascertain whether injuries from fire are least advantageously cured by heating or cooling applications. This comparative trial has been already made in some degree, and has been extended to another instance, in the number of cases here subjoined, of the benefits resulting from topical refrigeration in burns and scalds.

Dr. Evans, whose situation at Ketley affords him superior practical opportunities for putting the subject in question to the test of experiment, has furnished me with the following additional examples of the salutary efficacy of the cooling treatment. Many similar proofs of its curative power have been before transmitted to me by the same respectable author, which I have already communicated to the public through the medium of your Journal. The further accession of intelligence contained in the subsequent cases, will of course enhance the value of the preceding, and collectively afford a mass of evidence on the subject, sufficiently important to be highly deserving of public attention.

It is worthy of remark, that Dr. Evans, in his several communications to me on the subject in discussion, has not mentioned a case in which the cooling treatment did not succeed to his wishes. If any had occurred, his regard for truth, his ingenousness, and the claims of impartial inquiry, must have irresistably disposed him to have made it known. What therefore has hitherto obtained so uniformly, may be justly expected to continue, as a natural order of things, not to be controverted by mere declamation, whether *lightly* or gravely conducted.

I am very solicitous that the pretensions of the *terebinthinate* remedy should be examined with the utmost practical correctness, and have been desirous of adequate occasions for putting it to the proof of actual experiment; but as yet, my own experience of its powers does not enable me to deliver a decisive opinion on its real merits. Time, the infallible arbiter of doubt, will eventually settle the question at issue between the opposite modes of treatment. It is erroneous to endeavour to accelerate the probation in a manner inconsistent with the slow attainment of truth. The real state of things in ambiguous circumstances

cumstances cannot be speedily discovered; it is only by cautious, patient, and unremitted research, that such information can be ultimately acquired.

In the fullest confidence that persevering investigation will sooner or later indisputably confirm the superior practical value of the cooling treatment of burns and scalds, its advocates need not be discouraged by any unphilosophical objections that may be made against it, whether proceeding from the *comic* levity of your correspondent T. Y. or from the more *decorous* opposition of those who conceive it ought to be superseded by *terebinthinate* efficacy.

I am, &c.

Taunton, August 6, 1807. ROBERT KINGLAKE.

Additional Cases of the beneficial Effects of the Cooling Treatment of Burns and Scalds; communicated in a Letter to DR. KINGLAKE, *by* DR. EVANS.

"DEAR SIR,

"IN compliance with your request some months since, I now transmit you a farther account of the good effects of the cooling treatment of Burns and Scalds; which, with the numerous instances of similar efficacy already before the public, will, I trust, be sufficient to induce every unbiassed practitioner to give it a fair trial. The cold application should be continued as long as any sensation of pain remains. The result, in many cases, will be the formation of a new surface, with little or no ulceration on the removal of the old one. I generally puncture the blisters with the point of a needle, which diminishes the pain by lessening the tension of the skin.

"If this communication affords any information that you think worthy of notice, you are at liberty to make such use of it as you may judge proper.

"I am, dear Sir,

"Your's respectfully,

Ketley, July 29, 1807. "J. EVANS.

"June 16th, 1804.—William Dudlick and John Jones, two boys, about 14 years of age each, had their faces and hands severely burnt by the explosion of gunpowder, and were both cured in a week by the cooling treatment, without any other application.

"Oct. 10th, 1806.—John Turner, aged 24, had the whole of his face burnt by the explosion of gunpowder; considerable tumefaction with numerous vesications took place; the cooling plan of treatment procured immediate ease, and the patient was cured in a week without ulceration.

"Oct

" Oct. 26th, 1806.—Samuel James, aged 40, had his face, hands, and back most severely burnt by the explosion of hydrogen gas in a coal mine. The cold application was used to the face and hands, and warm oil of turpentine, according to Dr. Kentish's plan, (originally recommended by Heister) was applied to the back, and dressed afterwards with the unguent. resinæ flav. softened down with the same, in order to try which mode of treatment afforded the most immediate ease to the patient, as well as the mosst expeditious cure. According to the patient's own account, the pain of the hands and face was immediately relieved by the cold application, but he complained of the oil of turpentine occasioning a smarting sensation on the back for five or six hours. This mode of dressing was continued for the space of two days; but observing a considerable degree of inflammation remaining from the terebinthinate application, that dressing was changed for the neutralized cerate, which the patient did not observe (his eyes being closed by the great tumefaction of the face), but he expressed the utmost satisfaction from the superior comfort he felt in that dressing, compared with the former. The next day the back appeared much less inflamed, continued gradually getting better, and was cured in three weeks. I am confident the back would have gotten well sooner under the cooling plan of treatment, for the patient constantly complained of great heat in the part during the application of the oil of turpentine.*

" William Cox, aged 55, had his face, ears, and hands severely burnt at the same time. Immediate ease was afforded by the cooling treatment. The face was cured in a week, notwithstanding there were several large vesications ; and the hands, after a slight ulceration, were cured in a fortnight by the neutralized cerate.

" Dec. 11th, 1806.—Joseph Trenter, had his face and arms, with part of his back, burnt in a coal mine, by the explosion of hydrogen gas. He experienced the same good

* Although (as before observed) making a comparative trial of the heating and cooling modes of treatment in the same person is not physiologically correct, and that, therefore, the result of such procedure must be too much vitiated to be conclusive; yet, the case here recited, clearly demonstrates the beneficial influence of the cold application in alleviating pain, and the noxious effects of the heating quality of turpentine in augmenting it. Heating and cooling agents have necessarily a contrary operation, so that their respective action must be rather confounded than distinctly explained by being conjointly employed in the same patient. The interchange of effect reciprocated by such constrasted agency is undoubtedly

good effects from the use of cold application, as the above mentioned patients, and was cured in sixteen days.

" Nov. 30th, 1806.—Elizabeth Richards, a young woman, servant in a gentleman's family in this place, had her foot and part of her leg scalded, by accidentally stepping into a bucket half full of boiling water, which had been carelesly left at the bottom of some steps leading from one room to another, and being in the dusk of the evening, she was not aware of her danger. The injured foot and leg were immediately plunged into a bucket full of cold water, and retained therein until bed time, which afforded perfect ease. Linen rags were constantly applied wet with cold water during the night, and renewed, when required by any sensation of pain, during nine days. The patient was cured in a fortnight, with little ulceration; none indeed would have taken place, had it not been for the hastiness of a fellow servant in stripping off the stocking immediately after the accident, to which some of the skin adhered.

" June 23d, 1807.—Ann Parish, a corpulent woman, aged 60, in carrying a pail full of boiling water upon her head, struck her foot against a rail that lay on the ground, in consequence of which she fell and scalded her face, arm and breast. The application of cold water gave her immediate ease. It was continued as long as any inflammation or pain remained. The face was cured without the least ulceration. The arm in the fall, coming forcibly in contact with the ground, suffered a loss of skin from the wrist to the elbow, notwithstanding it was cured in a fortnight. The breast was sometime longer in cure, being more deeply ulcerated. J. Evans."

To the Editors of the Medical and Physical Journal.

Gentlemen,

In the second Volume of Fox's Work on the Teeth, recently published, mention is made of *a new method of stopping teeth* by means of " the fusible," (or Newton's) " metal," which had been suggested to Mr. F. by " some chemical

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doubtedly very considerable, but much too indefinitive to admit of being estimated with any degree of practical precision. The testimony, however, here adduced, brings into question the justness of T. Y's report, that fewer inconveniences arose from the use of *turpentine* than from that of *cold water*. Dr. K.

mical gentlemen," (in another place attributed to Mr. Pepys,) and which he thinks "promises to be very successful" in certain cases. Now it happened that the same idea occurred to *me* nearly three years ago, though I confess with much more moderate expectations of its success; but as I have since been induced to abandon all hopes of its practical utility, I trust I may be allowed to state my reasons.

In several teeth which I filled up in this manner, *(ex capite)* the degree of heat, though very small, compared with that requisite for the fusion of metals in general, was yet so considerable to my feelings, and so permanent, as to render it almost impossible for me, in filling a large cavity, to retain the tooth between my fingers until it became cool; and of consequence, I never considered myself warranted in making an experiment of the kind on a patient, when the probable result would have been such a degree of pain and inflammation, as would have completely frustrated my purpose. Another objection which I conceive to be fatal to it is, that in common with all other metals, in passing from the state of fusion to that of congelation, it contracts in bulk, and consequently cannot, when poured into a tooth, prove that "most perfect mode of filling up" that Mr. F. supposes. There are also several *minor* difficulties attending it, as the morbid sensibility that is frequently present in carious teeth; the decayed cavity opening on the side of a tooth, or between two teeth, &c.; and in the bulk of the most favourable cases in the *upper teeth*, it would be wholly inadmissible without the awkward circumstance of *inverting* the patient.

I will avail myself of this opportunity of mentioning an addition to the key instrument which I have been for some time in the habit of using; and though a very simple one, I have found it highly useful on many occasions. It consists merely in having *an ordinary claw made with a spreading point*, so as to be about one-third of an inch broad, (or the breadth of a common molaris) at its termination. By it, the *utmost possible hold* is obtained of the fangs of a tooth, when the crown is perhaps almost wholly gone, and their removal *at once* effected; thus, saving the patient, the pain and the operator the discredit of a second application of the instrument, where the ordinary narrow-pointed claw could at most have removed but *one* of the fangs at once, and that perhaps at the risk of not getting a firm hold of either. The discretion of the Practitioner must guide him as to the cases in which it is applicable; but in the course of between two and three years frequent

use

usage of it, I have on many occasions experienced in it a most decided advantage over the common claw

CHARLES N. WAWN.

Newcastle, Tyne, Aug. 10, 1807.

To the Editors of the Medical and Physical Journal.

GENTLEMEN,

TWO cases have lately occurred within the circle of my own observation, attended with the usual symptoms of "Hydrocephalus Internus." The subjects were both under two years of age, and as the disease assumed the same appearance in both, I shall not encroach on your valuable pages by a tedious description of each individually, but lay before you the treatment adopted in both instances.

They felt an acute pain in the head, nape of the neck, and across the forehead; respiration became difficult and irregular; pulse slow, and fluttering; the optic nerve paralytic, and the iris immovable. As the disease originates in a deficiency of the absorption of the fluid in the ventricles of the brain, whatever tends to restore the necessary absorption must of course undermine the disease, and be conducive to the relief of the patient. With this intention, powders composed of calom. ppt. and pulv. antimon. were given every four or five hours; and ℥j of ungt. hydrarg. fort. directed to be rubbed into each arm or thigh every night. A blister was applied to the nape of the neck, and mercurial purges given every second or third morning. By strictly adhering to this plan they gradually began to amend; the pulse and respiration became more natural, the pains by degrees subsided, the optic nerve performed its usual functions, and the iris regained its former motion.

Dr. Cullen, to whose indefatigable researches the medical world are particularly indebted, places the internal "hydrocephalus" as a species of apoplexy—"Apoplexia Hydrocephalica;" and the late Dr. Whytt strenuously insisted, that in cases of hydrocephalus the fluid was contained on the outside of the brain; and once went so far (to satisfy his mind with respect to the propriety or fallacy of his theory) as to have a puncture made; when having cut through the dura mater, a probe was introduced without any discharge of water; which is, in my opinion, a sufficient proof of the fluid having been contained in the ventricles of the brain.

August 7, 1807. MEDICUS.

Reply to Dr. PATTERSON *of Londonderry, on the Subject of* CHOREA SANCTI VITI. By JOHN REDMAN COXE, M. D.

IN the 15th volume of the London Medical and Physical Journal, Dr. Patterson of Londonderry, has published an answer to a communication of mine in a preceding volume of that work, extracted from the 8th volume of the New-York Medical Repository; entitled, "Observations on Chorea Sancti Viti, with a new Theory of the Disease;" in which the Doctor accuses me of having censured not only some of his reasoning, but also part of his practice, "in a manner at once abrupt, rough, and magisterial." Conscious of having never intended to give the slightest offence to Dr. P. in that essay, (of the truth of which I request every gentleman to convince himself by a candid perusal of the piece) I should not now have answered his illiberal and unjust aspersions, had he not called upon me "for refutal or concession."

It seems extraordinary, that such harsh epithets should be applied to me, because I ventured to differ in opinion from my learned opponent, by saying, "it appears to me, that Dr. Patterson has greatly erred, in supposing chorea the primary disease; of which each must judge for himself." I see nothing "abrupt, rough, or magisterial," in thus allowing every one to form his own opinion of the subject in dispute, after comparing the arguments on both sides. Surely I have the same right to my sentiments that the Doctor has to his: and he will find it difficult to persuade mankind that *his ipse dixit* is to carry conviction to the breast of every reader; or, that an individual may not be allowed the privilege of dissenting from him, without being presumed to act from unworthy motives!

The Doctor supposes I have censured his practice, by saying, (in referring to the case he relates in his letters concerning the internal dropsy of the brain,) "the operation of the emetics probably hastened her dissolution, by determining too forcibly the circulation to the brain," (an expression he seems to sneer at,) "and hence exciting an acute state of hydrocephalus."

Whilst I still adhere to the same opinion, justice impels me to absolve myself from any idea of imputing to the medical attendant any blame, from the administration of the emetics. I had not read the Doctor's remarks with so little attention, as not to have noticed that their exhibition was not his fault. The manner of expressing myself,

might to a candid mind have confirmed this; for (although Dr. P. appears to be unacquainted with that urbanity which should characterize medical men in their opposition to each other's sentiments, yet, knowing how liable we all are to mistakes,) had the Doctor been a principal on that occasion, I should have been peculiarly careful of speaking in a manner to wound either his feelings or his reputation. I should not, so virulently as he has done, have exclaimed,

"Quot Themison ægros autumno occiderit uno."

Considered, however, in a practical point of view, I must oppose, as far as an individual can, the use of emetics in those complaints, especially when increased action of the vessels exists; the practice may be sanctioned by "eminent professional characters."

"Nullius jurare in verba magistri."

Even several of the gentlemen he has named, are guarded in their expressions, by the terms, *gentle* and *mildest*, in recommending the use of emetics.*

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* That the Doctor does not himself approve of the use of emetics, I think we may fairly deduce from his own words, although he has ventured in his answer to quote the authority of several medical men in their favour. "If Dr. Quin" (says Dr. P, p. 6. of his letters,) "had felt even a faint conception of the present theory, would he in this case have prescribed, early in the disease," and if not early, why in its most advanced state? a remarkably strong vomit of ipecacuanha and antimonial wine?"—And in speaking of errhines, (p. 52.) he says, that before using them, "precautionary considerations are very necessary; our subjects are tender, irritable, and affected with plethora of the head, the particular part on which these medicines operate, and to which they give such a degree of commotion, as would, *no doubt*, prove highly pernicious in the incipient stage of the disease. Nay, I am not clear, that they are perfectly safe in the more advanced periods, when vascular repletion may be diminished, and serous effusion on the approach, or in any degree existing. For I apprehend, that the shock of sneezing, as well as the *agitation* of *vomiting*, if *by chance* eventually *harmless*, would not promote absorption in the advanced torpid state, and would be a vexatious experiment to the patient, to whom the smallest motion of the head is generally so great an annoyance."

In speaking of the administration of cathartics, he says, (p. 34.) "For a stimulating purgative must either be in such quantity, or of such quality, as would in many cases, especially where the stomach is so frequently disordered, prove actually emetic; an effect which you, (Dr. Quin) *properly observe*, would *hasten considerably the progress of the disease*, and I would add, an effect which must be highly pernicious at its commencement." He afterwards says, (p. 35.) "Touching vomits we may here remark, that several years ago, it was started as a question, whether they be, or be not, safe medicines in febrile disorders, *caput petentes?* Applying

The Doctor thinks it behoved me, before pronouncing that he had greatly erred, &c. to refute the reasoning on which his conclusion is founded; at which, however, he says, I have not made the smallest attempt, "the performance of which is so necessary to vindicate my dogmatism."—Whilst, however, a doubt remains on my mind, whether the Doctor comprehends his own reasoning, if such it can be termed; I should recommend his looking at home for a plentiful crop of dogmas dispersed throughout all his writings. His "innate nervous stimulus," and "principle of irritability," which he so confidently tells us, "is not confined to the fibre, or simple solid; but is also possessed by the fluids of the body, particularly by the blood;" I conceive to belong to this class; unless indeed, the Doctor has better reasons to confirm them, than mere assertion. His assertion, that the heart "*must be* the centre of the principle," I regard as equally unqualified and devoid of proof. Of its "influence on the contents of the cranium," I desire no other proof than the effect of the emetics in the case alluded to.

The Dr. says, "As in those ages, in which this constitution prevails, convulsions likewise prevail, the increased action which they produce will augment the impetus of the circulation, especially in the vessels of the head; which we see is actually the case, since a considerable determination to that part is observed in those disorders. This determination increases the action of the blood-vessels, and inflammation is the consequence. This consequence is forwarded by the plethora, particularly of the head, which exists in the early and advancing stages of life; and the whole train of symptoms constituting *febricula hydrocephalica*, (the name which I appropriate to the disease) is then brought into view.*

plying the question to hydrocephalus internus, which I reckon of a febrile nature, and most certainly *caput petens;* I would answer, that here emetics *are in my opinion injudicious remedies*, especially in strong doses at the beginning of the malady. And I ask, would you give them at this period, or indeed *at any other*, upon the principle of promoting absorption, by the general concussions arising from their operation?"

* It may here be observed, that in the history which Dr. Patterson gives us of this disorder, (hydrocephalus) after enumerating the symptoms of he first and second stage, he ends those of the third stage, (p. 11) by "impaired deglutition, and *convulsions*, which *form the catastrophe*." I certainly might with as much reason say, that these final convulsions of life, were the cause of the effusion of water, which dissection shews in the

I perfectly agree with him in the effect which is produced by the determination to the head, of inflammation, and of the whole train of symptoms, constituting febricula hydrocephalica, (as Dr. P. expresses it;) but I view the convulsions, which he supposes the *cause* of the increased action, as the *effect* of the plethora, or effusion induced; for that hydrocephalus, induced by inflammation of the brain, should, by pressure on the nerves, (or their origin,) induce convulsions, (especially as it is in a part incapable of distention) is surely a more rational view of the subject, than that the convulsions should excite the determination to the brain, (which he only asks to be conceded as the petitio principii,) and the consequent febricula hydrocephalica. I would only ask, which is the most simple of these doctrines, or whether all the Doctor's reasoning on the subject amounts even to a presumptive proof of it? The Doctor assigns not even a probable cause for the convulsions; but I have, I think, rationally proved the legitimacy of my deduction; and might add, that every case of injury to the brain, from blows, falls, and the like, tends to establish it; inasmuch, as in those instances where death takes place after convulsions, dissection has evinced the cause to be matter formed, or water effused, or great engorgement of the vessels of the brain.

"But how are we to explain (says the Doctor) the occurrence of effusion, where the accumulation of the irritable principle is not so vigorous, and yet the action of stimuli is assiduous? In these cases *I would conjecture*, that the nervous stimulus, acting, *as I suppose*, on a mass of

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the ventricles, as the Dr. has for asserting chorea, (which he afterwards admits to be merely a *species* of convulsions,) to be its origin. I think too, it may not be irrelative to state, that in the view of the different kinds of inflammation which the Doctor has taken to aid him in the investigation of the subject; he adds, p. 19, "No douht there is a propriety, and even an advantage in distinguishing with accuracy, the organ immediately affected by inflammation; yet it is equally certain, that, in all such cases, the *great diversity* in the symptoms is *more imputable to a difference in the function of the part engaged*, than to any specific variation in the nature of the inflammation." I think, although this is applied to the effect of inflammation, it is not asking too much of the Doctor, to impute the same diversity of symptoms, (that is, the various forms of convulsions,) to a difference in the function of the part engaged, by the greater or less pressure of a fluid, or turgid vessels in the brain, on one part more than on another, at different times. For we cannot but admit that pressure from the same source will produce a different effect on one part of the brain, from what it does on another.

of irritability less moveable than in convulsive habits, excites a pa sion, which, if violent, suddenly destroys irritability, or the vital principle, and death ensues; or which, if moderate, engages only a part of the irritability, and the natural state of temper is regained for a time." And he illustrates this (reasoning!) with an account of the dissection of Mirabeau, who was found to have "un petit épanchement dans *le cerveau*;"—adding, does not this instance shew, that an irritable temper, almost constantly exercised, may be a cause of effusions in different cavities, and of a watery effusion in the brain; which conditions, *in all probability*, were preceded by more or less inflammation in the diaphanous membranes of those parts? And must we not admit an irregularity of temper in those persons peculiarly subject to hydrocephalus internus?"

I am sorry to say, I see nothing like reasoning in the above conjectures and suppositions. What does the example of Mirabeau prove? What, if an irritable temper almost constantly exercised, is a cause of effusion in different cavities; and even if preceded by inflammation in the diaphanous membranes of those parts; it does not prove convulsions, (unless convulsion and irritable temper are synonimous,) to have excited it. Mirabeau was not subject to these; although, it is probable, had he lived, the increasing effusion might have induced them. Neither can I see the necessity of admitting an irritability of temper, in those persons peculiarly subject to hydrocephalus internus, more than in any other species of dropsy. I would, however, recommend it to the Doctor, (if his theory is just,) to guard against that temperament so conspicuous in his writings, lest it should be followed by the same fatal effect as in Mirabeau.*

I thought I had been sufficiently explicit in attempting to explain, how a small additional effusion, in a case of chronic hydrocephalus, might render it similar to a case of acute; for it is certain, that if the brain is capable of containing by slow increments, lbj of fluid, without a fatal issue; a small addition to this, suddenly made, though

* It might not have been amiss for the Doctor to have added the small residue at the end of his *reasoning*, as it contains more sense than all the preceding. "To promote this end, namely, the extension of practical knowledge, an accumulation of facts is, undoubtedly the aptest means." If the Doctor could have viewed my cases, as a small addition to the facts elsewhere enumerated, he might have spared his ungenerous remarks.

only to the amount of a tea-spoonful, might nevertheless induce all the symptoms of an acute attack, and even death itself; for the brain being incapable of distention, must have time afforded it to accommodate itself to the effect of pressure, by absorption of a part. There is in this nothing incomprehensible, nothing obscure; and I must still declare my firm belief, that the Doctor's patient fell a victim to an attack of acute hydrocephalus, induced by the operation of the emetics. At least, if a greater effusion of water did not take place suddenly, the dissection shews such an increased determination to the blood vessels of the brain as was fully adequate to the final catastrophe.

The Doctor proceeds to say, that "the influence of convulsive affections in causing accumulations of the fluids in the brain is not a new point in pathology; it has occupied the attention of physicians during some years past. A child was indisposed about 2 months with frequent head-ach, which was supposed to proceed from worms, but anthelmintic medicines afforded no relief, and he died in a convulsive fit. On opening the head, the vessels of the brain were observed to be uncommonly turgid, and in the ventricles was found more than double the ordinary quantity of serum." 'In this case, (says the late Dr. Percival, of Manchester,) I apprehend the turgescence of the vessels was the *effect*, and not the *cause* of the convulsions; for the reflux of the blood from the head to the heart being obstructed during the fit, in which I believe the patient expired, the vascular distention must have been permament. The redness and even the blackness of the face, which takes place in convulsions, affords sufficient proof of sanguineous accumulation;' "hence it appears, that the same speculative opinion, which Dr. Coxe styles a *new theory*, with respect to convulsive motions in hydrocephalus, was conceived at least 15 years before Dr. Coxe wrote, and had obtained so much credit as to induce the learned Dr. Percival to counteract its extension."

Although the attention of physicians during some years, has been occupied by reviewing "the influence of convulsive affections in causing accumulations of the fluids in the brain;" it by no means follows that they were correct in their conjectures. On the contrary, I think every fact tends to shew that, (as Dr. P. has done) they have mistaken the cause for the effect; a circumstance by no means uncommon.

The case of the child mentioned above, is clearly in point. The head-ach for two months might have led to a more

more rational deduction than that it proceeded from worms; and the uncommon turgescence of the vessels of the brain, after such a long continued head-ach, might have, I apprehend, led Dr. Percival to regard the convulsive fit of which he died, as the effect of such turgescence; and I doubt not, *to be* in no wise singular in this opinion, although it is opposed to the sentiments of that truly eminent physician. I must say I think Dr. Patterson strives hard to give my opinions to another, when he asserts that Dr. P. had thought it necessary to counteract its extension at least 18 years before I wrote; I see no author of that time, quoted as bringing forward the doctrine; nor any credit which it had then obtained, to induce Dr. P. to counteract its extension. Had the Doctor really conceived it 18 years before; or had it acquired any credit, the extension of which he might think it necessary to counteract; is it probable he would have said so little upon it? And is it not still less probable that Dr. Patterson in his express treatise on the subject, and with this case before him, should have passed it in silence, and not till now, when he thinks it will answer his purpose, detect a theory in Dr. Percival's writings, which any man of common candour will allow to have no pretensions to it? If even I was to admit the fact as such, Dr. Percival is stating a case of convulsions, to which he had not approximated chorea, (the disease I have particularly treated of, as symptomatic of the affection of the brain; and, until Dr. Patterson shews that Dr. P. intended his ideas to extend to chorea, *then* viewed as a primary disease, he must excuse me for still upholding my claim to the new theory, as he (contemptuously) says I style it.

"If, as Dr. Coxe supposes," says Dr. Patterson, "chorea be not an idiopathic disease, but arises *solely* as a symptom of the *chronic* hydrocephalus, the characteristic phenomena of the latter should always precede, or accompany, the appearance of the former. Whereas, upon a careful examination, we shall find, that the symptoms of chorea exist without any token of hydrocephalus, as strikingly exemplified in the case of my patient, already alluded to; in whom its essential features prevailed, long before it wrought the change which terminated in the fatal effusion into the ventricles of the brain." The case above alluded to, examined attentively, will, I think, most satisfactorily prove that hydrocephalus is a cause, not an effect, of convulsions. The brain by long and slow effusion, (as was the case with his patient) is enabled to ac-

commodate itself to a pressure, which produced more rapidly, would induce death; witness the numerous cases related by authors It is strange that Dr. P. should be so attached to the doctrine he holds, as to overlook the objections against it, and to bring forward cases strongly opposing it! I should be disposed to rest the merit of the question on this case, and doubt not that it will convey conviction to the minds of many, who perhaps at present, receive, without reflection, the Doctor's assertions for proof.

"Had the cases of chorea, (says Dr. Patterson) which came under the notice of Sydenham, been accompanied with hydrocephalic symptoms, so careful an observer certainly would have mentioned such striking occurrences, and would not have found as he did, those cases liable to periodical returns, which rarely, or never, happen in any species of hydrocephalus."

Had Sydenham been acquainted with the *acute* state of hydrocephalus, I think there is little doubt, that so sagacious an observer would have drawn an accurate contrast between it and the chronic state; and might also, from the knowledge it would have impressed upon his mind, have been led to investigate more thoroughly the sources of chorea, on which he has so slightly treated. The reason is evident why Dr. Sydenham did not detect the symtoms of hydrocephalus; because in the chronic state, many are wanting (owing to the effect produced by habit, as I have before attempted to explain) which are almost essential to its acute attack. The wonder is less, that Sydenham should have failed in demonstrating the cause of a disease he regarded as primary; when Dr. Patterson, with the advantages he possesses over that illustrous character, and with cases and dissections so strong in point, came no nearer than putting cause for effect.

"Non omnia possumus omnes."

And here I must advert to Dr. P's note, in which I am charged with oversight, in respect to Sydenham's theory of chorea, because I say, he "scarcely adverts to any thing but its curious gesticulations." I confess I did not think it necessary to attempt a confutation of his theory, which ascribes it to an humour thrown on the nerves, &c. I had not overlooked it; but I must leave it for Dr. P. to combat, unless indeed he means to uphold it; which mentioning it as he does, seems to imply. It is indeed, rather surprising, that Dr. P. has not detected my theory

theory in Dr. Sydenham's; as mine depends upon the pressure of a fluid on the brain and nerves.

As to my being aware of the difficulty on this point, (Sydenham's, &c. above,) and putting the question, "If hydrocephalus is the cause of chorea, it may be asked, as the former is not an unfrequent disease, why the latter does not more often occur?" I confessed myself ignorant of the reason, but it by no means follows, as the Doctor so confidently affirms, "that it would *always* be produced, if it possessed the affinity of a symptom." I will only ask, if any disorder exists, in which any individual symptom of it, which may be named, is not occasionally absent? Does not the Doctor himself allow, as well as others, that one of the most leading symptoms of hydrocephalus, *viz.* strabismus, is not always present? Cold applied to various parts, will produce a diversity of complaints, attended with their peculiar symptoms: in like manner it may be imagined that pressure on different portions of the brain, may induce a diversity of symptoms. The objection of the Doctor to a thing devoid of certainty, does not prove, either that he is right, or that I am wrong. I must however, be permitted to enjoy my belief, that although hydrocephalus is not always, or even frequently, accompanied by chorea, yet wherever chorea does exist, there hydrocephalus, or its preceding state of inflammation and turgescence of the vessels, (which I presume may also be chronic) also exists as the cause. It is not necessary I apprehend to its existence, that a gallon of water should be effused; for the disease may be as complete with but a tea-spoonful as with a larger quantity; although it may be less dangerous, inasmuch as the brain will sooner accommodate itself to the pressure.

The Doctor asserts, that in his second case of chorea, not a symptom of hydrocephalus can be traced, and adds, that my second case is of the same description. To this I may observe, that the assertion by no means proves that water did not exist in the brain. The Doctor, however, gives reason to suppose, that some undue effusion, or determination of blood in the brain existed, when he tells us, that his patient "although addicted to much talking in his health, yet his loquacity is now greatly increased." And few will dispute the probability of the same in the case I have detailed, from the fatuity and idiotic appearance conspicuous in the patient.

"Instead (says Dr. P.) of being solely a symptom of chronic hydrocephalus, chorea sometimes proceeds from obstructed

obstructed menses, sometimes from exposure to rigorous weather, but generally from irritation in the first passages." I would ask, if the former of these supposed causes may not be itself reasonably supposed dependant on the disease of the brain? or if it be the original source of the disease, whether it may not act by the determination of blood, inducing effusion, &c. in the brain? In the same way may rigorous weather, and irritation of the first passages, promote chorea, by first inducing hydrocephalus.

As for convulsive agitations excited by particular irritations of the stomach; or choreatic gesticulations being attended by a spasmodic affection of the organs of deglutition; it would require more proof than the Doctor has brought, to convince me, that poisonous and other substances taken into the stomach, cannot excite determination to the brain, (as well as they can excite disorders of the skin) and thereby be a remote cause of convulsions; or, that the spasmodic affection of the organs of deglutition and the accompanying choreatic gesticulations, did not both depend on one common cause, viz. pressure on the brain.

The cases of Mr. Alexander, to which Dr. P. refers, are strongly corroborative of my sentiments respecting this disease, although Dr. P. has laboured hard to obviate the impression, which a review of the symptoms must necessarily create. In the first case, the symptom of strabismus occured, "a symptom (the Doctor says) of cerebral affection, yet hydrocephalus (he adds) shall not be the consequence." No indeed! it shall not be the consequence but was apparently the cause, both of it and of the chorea. The Doctor asserts that "the strabismus can be with reason ascribed only to the efforts of the brain to *recruit its impaired energy*, (risum teneatis!) but cannot, on good grounds, be imputed to hydrocephalus;" thus does theory oppose reason! The diminished tone of the alimentary canal, which the Doctor considers as the occasional cause of the convulsive motions, is certainly more rationally deduced from the pressure on the brain, to which it might then add additional force by the pressure of the contents of the alimentary canal on the descending aorta. "But surely, (adds the Doctor triumphantly) no theory, even the *newest*, can make us believe, whilst we are in our senses, (Q. E. D.) that every repetition of the strabismus was occasioned by the effusion of a fluid into some cavity of the brain, and that each cessation of the ocular

cular distortion was the effect of the absorption of that fluid." I will venture to ask if there is any thing remarkably difficult in this belief? Do we not see fluids effused and partially re-absorbed, and again recurring, in other cavities? And why is the brain alone to be an exception? If this belief is even not correct, it is not unreasonable to believe that the strabismus ceased, when the brain had become habituated to the pressure excited by the fluid, and continued absent, until by a fresh addition of fluid, an inordinate degree of pressure was again excited.

The "morbid condition of the alimentary canal" in Mr. Alexander's second case, in my mind, serves to strengthen the conviction, that the pressure on the brain was the cause of both it and the chorea: not, as the Doctor supposes, that this morbid condition "in a constitution of considerable mobility," (an expression conveying no precise meaning) is the general cause of the disease in question.

The third case, I view as equally conclusive on my side, (although Dr. P. gives it against me,) more especially as it serves to evince the propriety of a different treatment in different states of the disease. For that "the tonic and antispasmodic system of prescription" is *invariably* just, will I think be advocated but by few. Sydenham bled and purged repeatedly in this disease; and the medicines employed by Mr. Alexander, were best adapted to the stage at which they were employed, though they probably might not have proved equally serviceable in the commencement. I presume Dr. P. himself will acquiesce in the justice of this observation, and that he does not mean to say, that tonic and antispasmodic remedies are invariably proper.

The Doctor I observe, had some slight disposition to allow the doctrine to be just, (that chorea is a symptomatic disease,) when he adds, at the close of his remarks on Mr. Alexander's cases, that "they (the cases) all likewise tend to evince, that chorea is not a symptomatic ailment, *at least not naturally* symptomatic of hydrocephalus."—I must therefore believe that it has made more impression on the Doctor than he is willing to admit, or than his endeavours to overturn it, by his copious references, would seem to show.* The

* Both the symptoms and cure alike prove chorea to be *symptomatic* of pressure on the brain, and that it never is, nor never can be, an idiopathic disease. We might as well call the convulsive movements of hysteria, epilepsy, &c. idiopathic, without a reference to their primary source of which those movements are merely symptomatic!

The case brought forward from the Medical Repository is one which depends on a cause, originally so different from any other, that it is truly difficult to reason upon it. I cannot however perceive that it adds any strength to the Doctor's ideas respecting chorea, nor that it precludes the possibility of the existence of water in the brain, either previously existing, or excited by the primary source of the complaint itself.

If Dr. Whytt has shewn, as Dr. P. asserts, "that a delicate state of the first passages, or a depraved sensibility of their nerves, not only disposes people to many complaints in those parts, but that the whole nervous system is thereby rendered more moveable, and liable to be affected by the slightest exciting causes;" may it not be equally inferred, that what is capable of exciting such morbid action in the nervous, is equally capable of producing similar results in the arterial system, and thereby of promoting effusion in various cavities of the body; in which the brain would be likely to be a principal sufferer?

Is it possible the case of Dr. Macbride, adverted to by Dr. P. could not awaken in his mind one idea, that in a disease of twelve years standing, the brain was enabled so to accommodate itself to the changes induced in it, that no symptoms of disease should be apparent, except of symptomatic chorea? Where is the Doctor's morbid condition of the alimentary canal, &c. &c. for so long a period? And why could not tonics and antispasmodics effect a cure? The answer is plain; because the chorea was not idiopathic, and the remedies were not adapted to the original disease.

The cases adduced from the Bristol Infirmary, are I think, strongly in my favour. The boy of 17, who received a blow on the head several years before, requiring the use of the trepan, although well till within three months of the attack of chorea, was apparently during that period, slowly receiving the impression from watery effusion, although this chronic state of hydrocephalus did not produce the symptom of chorea till so long after. The brain by slow effusion, conformed to the increasing pressure; as is surprisingly evident in every part of the body: and could the cases enumerated have been dissected, no one can dispute the great probability of finding the ventricles largely distended with water. In these instances, it can scarcely be called congestion; because the absorption of the brain would correspond with the slow deposition of fluid.

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The girl who died of hydrocephalus, after being cured by zinc, shews a case similar to mine related. Here, for a time, by the use of zinc, chorea is suspended or cured; yet from some cause, an increased determination to the brain, (produced more suddenly than it had been accustomed to) converts the chronic hydrocephalus into its acute form, and with a fatal issue.

The other girl is cured by bark and the cold bath; remedies, apparently highly proper at that period, to excite the absorption of the existing fluid. Some analogy might here be drawn from the conversion of chronic rheumatism into acute, if it was necessary; but every one can apply the arguments which might be used.

As for the case of Dr. Hales, in the note, of a woman receiving a severe blow on the head, &c succeeded by chorea, during the existence of which, no symptoms of hydrocephalus appeared, and in which the cure was effected by stimulants principally; I would ask, if such a blow was not likely to lay the foundation for effusion as the cause of chorea; although the brain may have accommodated itself to the slow deposition, and thereby have been uninfluenced by symptoms, which its more sudden effusion must have induced; especially as the Doctor allows that in a great majority of injuries to the head, no convulsions arise? And why? Because in general, in injuries of the head, venesection, the best preventive of morbid effusion from arterial action, is timely had recourse to.

The five cases collected by Mr. Mullen, in the Edinburgh Medical and Surgical Journal, which were under the care of that able and judicious physician Dr. Hamilton, I regard as highly instructive; inasmuch as they afford sufficient evidence of the great importance of constant purging in the complaint, which, so far from debilitating, added strength to the system, by probably exciting absorption in the brain; the influence of pressure on which, has a strong tendency to produce that apparently weakened but overburthened state of the system, (or what Dr. Brown has called indirect debility, and which is to be removed, not by tonics and antispasmodics, but by depletion. It is a great pity Dr. Patterson has not stated what Mr. M'Mullen has said of Dr. Brown, as it is very applicable to Dr. P. himself. "Dr. Brown," says Mr. M'Mullen, does not mention this disease particularly, but we can be at no loss for his sentiments with regard to it. With *his usual dogmatism*, he asserts that all spasmodic diseases are diseases of debility, and yield readily to tonics and antispasmodics. The first part of this assertion is hypothteical, and the second

cond is erroneous." Here then we see Dr. P. has little to aid him, either in theory or practice, from the opinions of Mr. M'Mullen. And although I cannot agree with Mr. M'Mullen as to the cause of the complaint, I am truly rejoiced at the speedy triumph of the practice. And this the more so, because I see in its effects, the powerful, yet well known influence of evacuants, in promoting absorption of fluids in distant parts; and I see no one reason, on a careful perusal of the cases, to change my sentiments of the nature of the disease. Whatever may have been the cause of the black and fetid stools, there can be little doubt of the influence, which either their acrimony, or quantity, produced on the disease; for we find, that if at any time "the cathartics did not produce an evacuation, the involuntary motions recurred, and all the symptoms were aggravated." Now, that the suspension of the alvine discharge should augment the disease by checking the absorption of the fluid in the brain, or by giving an opportunity to the exhalants to throw out an additional quantity, is not surprising; and rather adds weight to the theory I have advanced. Be this as it may, I think I may confidently affirm, that if these cases give no sanction to my sentiments, they afford still less to those of Dr. Patterson.

The cases adduced by Mr. M'Mullen, of chorea produced by teething, and on which so great stress is laid by Dr. Patterson, do not I apprehend militate against me. It is true I may not be able satisfactorily to explain the effect this irritation might have induced on the brain, disposed, it is possible, (nay probable, as we so seldom see a like effect from the cause assigned,) to hydrocephalus: yet if Dr. P. will admit of analogy, I might mention the numerous cases of tetanic convulsions excited by a slight puncture in a very distant part of the body; and if such an effect may arise from a cause, apparently so trifling, it cannot be a matter of astonishment, that irritation of the gums may excite preternatural action in the vessels of the brain, and by causing effusion, induce a disposition to chorea; which a removal of the cause, (by preventing the increase or continuance of the effect, viz. effusion) would immediately obviate: One thing, however, is proved from the cases, viz. that however this irritation might act, the chorea induced was symptomatic, and not a primary disease: and this is confirmed by the case mentioned by Mr. M'Mullen from Dr. Gregory; in which two successive attacks of chorea were removed, by taking out the first set of teeth, by the side of which the second set were pushing up.

up. At the age of 15, this boy was again attacked with chorea, when *no cause* of irritation was discovered *in his mouth*, and he was now cured with extract of cinchona and sulphate of iron. The important changes, which at the age of puberty take place in the system, may, I conceive, be a sufficient cause of increased action in the vessels of the brain, disposed, by former attacks on that organ, to effusion; which was removed by the tone and vigour given to the system generally, enabling the vessels of the brain in particular to resist the continuance of undue determination to them, or the absorbents to receive with equal facility, any fluid which the exhalants might pour into the ventricles.

That worms are very unjustly accused of exciting this disease by their irritation in the alimentary canal, I have endeavoured to shew in the essay which has called forth Dr. Patterson's remarks. That they may, when existing in a person affected with chorea, aggravate the disease, I have little doubt; but this must be by a primary effect induced on the vascular system, tending to augment the already overloaded vessels, or ventricles of the brain. The influence of purgatives in the cure of the cases related by Mr. M'Mullen, evince sufficiently, that when worms are in any case evacuated by purgative medicines, it is the operation of the cathartic, and not their mere expulsion, that benefits the patient; although, no doubt, much good may result from obviating a source of irritation, whether arising from worms or from a constipated state of the bowels.

Dr. P. has related the cases recorded by Sir George Baker, of certain poor children, "who by breathing vitiated air, were thrown into excessive tormina of the alimentary canal, which were attended with convulsions and delirium." Surely, without referring this to the indirect irritation on the *primæ viæ*, a nearer road might have led to the truth, when the *immediate* action of this vitiated air upon the lungs is taken into view; which, by not supplying sufficient oxygen to sanguify, (if I may use the expression without the lash of Dr. P's criticism) the blood, would produce such remora in the vessels of the brain, as may reasonably account for convulsions and delirium. Whether the tormina were produced by the vitiated air, or by (as is probable) unwholesome food; it appears evident, that both they and the convulsions were only symptomatic; and that the convulsions were less likely to be induced by the tormina, than by the cause I have assigned.

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The three cases of chronic convulsions occurring in one family, as related by Dr. Armstrong in the ninth volume of the Edinburgh Medical Commentaries, and quoted by Dr. P. are so very extraordinary, as to leave little room for reasoning upon them. I see, however, small reason to connect them with chorea; and as I have undertaken to say, that *all* convulsive diseases depend on hydrocephalus, so I shall pass them over, without any further remark, than, that although no hydrocephalic symptom was apparent, it does not conclusively follow that that disease did not exist. Dr. Armstrong considered them as truly epileptic, and I see no reason for dissent.

As for what Dr. P. says, respecting general convulsions attending an herpetic eruption, or epileptic fits from the stimulus of bile, in rheumatic fever, &c. I have nothing to remark, as his observations are irrelevant to the subject in dispute; inasmuch as he is speaking of convulsions generally; and I am only contending for *one form of them*; and it would too far extend the limits of my paper, to wander into another field for argument.

"In genuine hydrocephalus internus, (says Dr. P.) convulsive affections do not occur, *until the disease be far advanced*, and the *choreatic species of those affections is the rarest stage of it*." I think, was I disposed to attack the Doctor unfairly, I might say, this is an approximation to my opinion. I will however say, that this is the very thing I contend for. In the acute stage, the symptoms are rapid, and readily distinguished; in the chronic, *gradatim* and habit prevent the changes being so apparent.

"From a review, (adds Dr. P.) of 32 cases of genuine hydrocephalus internus, I find, that in 15 of them no convulsions took place; and that in any of the other 17 cases, spasms or convulsive motions did not occur, *until* the hydrocephalus had subsisted *several days*, nay in some, not until it had continued *several weeks*." "Besides, from a review of a few cases of spurious hydrocephalus, wherein the head was considerably enlarged, it appears that, in some of these, the convulsions *did not* attack *at the beginning*, and that, in others, even in those ascribed to external injuries, no convulsions appeared."

The last part of this paragraph, I add to prevent any idea of withholding evidence; though I have no where said, that either convulsions or chorea must inevitably follow the existence of hydrocephalus, but have distinctly asserted the contrary, without pretending to say, why it should so happen. As to the former part, I think it strongly

strongly conclusive in my favour; as the acute hydrocephalus, strictly speaking, is rarely a disease of weeks; and the Doctor allows that the convulsions did not come on, till *long after* the presence of hydrocephalus! What more could I desire than this confession? for if the Doctor admits this, surely he might admit the same of chorea; which he above regards as a species of convulsions.

The cases adduced from Mr. Pott, to shew that in a majority of cases of wounds of the head, convulsive affections have not occurred, are entirely unconnected with our subject; for until the Doctor shews, that by convulsive affections he particularly means chorea, I cannot but think he is unnecessarily enlarging the original point of controversy. Yet even here I might safely meet him; for when particular information is given of the appearances on dissection of those who died, after convulsions had ensued, I think, without a reference to the cases, that suppuration, &c. producing pressure on the brain, was evident; evincing the convulsions to be the effect, (and consequently symptomatic) unless the Doctor can afford *stronger* grounds for belief that the convulsions caused the suppuration, than he has that chorea produced the effusion, evinced in the dissection of his and my cases.

As for Dr. P's assertion, that my styled "*new theory* is nothing more than that pathological doctrine, known since the days of Hippocrates, and termed *conversions* of *diseases*;" I can only say, I am sorry to see Dr. P. descend to subterfuges, to invalidate my claim to what I consider a just view of the doctrine of chorea. I think it needless to follow the Doctor in this part of the dispute; or to deny that any conversion of disease takes place in the complaint which is the subject of my remarks.* I considered the Doctor as a more open champion for his cause than I find him to be; and should therefore regard any triumph over him, (if the medical world should sanction my remarks) as less considerable, than I should be otherwise disposed to consider it. I shall, however, observe, that the Doctor appears to have brought cases even here, to support my theory; as, when he says "hydrocephalus internus has been transmuted into palsy, and palsy into hydrocephalus internus;" and furnishes us with, what he says, is an apposite instance of the latter, from Dr. Ferriar. "*Eight*

* If it be as old as Hippocrates, why has Dr. P. endeavoured to give it to Dr. Percival.

months after the appearance of the paralytic symptoms, the patient complained of severe head-ach; vision became indistinct, and at length was entirely lost. Epileptic fits *then* came on, and he died comatose. When the head was opened, the ventricles of the brain were found full of water, and several tumours, which in the prevailing medical language might be called scrophulous, were observed in different parts of the brain." Here it seems to me, cause and effect are very evident, without recurring to transmutation. The scrophulous tumours (as the patient laboured under them elsewhere) seem the original disease; and probably by their pressure induced the palsy. That they tended to excite the hydrocephalus, I have no doubt; nor, that this hydrocephalus had been long advancing to its fatal termination; not as a chronic disease, but by some adventitious cause, evinced by the head-ach, &c. (the brain having accommodated itself by habit to its previously diseased and deranged state) which induced a sudden effusion, in addition, by which the symptoms of acute hydrocephalus became evident, and epileptic fits ensued. This also shews, that the epilepsy here was a symptomatic affection and not a primary one. It proves the connexion of those convulsive symptoms; and I will add, it strengthens the probable justice of my observations.

Of the conversions of disease, I have no doubt; but I must be permitted to question it, in the present instance. I would add the same of the case related from Dr. Percival, of pulmonary consumption converted into hydrocephalus, by the violent succussions of coughing. That the coughing induced the effusion of water in the brain, I doubt not; but I can only consider the absence of the cough, &c. &c. after the symptoms of hydrocephalus ensued, as a suspension of the one by the more violent action of the other. Still, however, if I was to admit all the Doctor's fallacious reasoning from the various cases he adduces, I do not think it would add one particle of strength to the weak fabric he has attempted to raise.

The case of Dr. P's patient, (neither "mutilated nor misconstrued by Dr. Coxe," as Dr. P. is pleased to affirm; but in which sentiment I trust no person will join him, after a candid perusal of the Doctor's own statement, and of the short abstract I have given of the case) is not, I venture to affirm, "a clear case of the conversion of chorea into hydrocephalus;" but in my opinion, demonstrative of the truth of the theory I have ventured to bring forward. In detailing

detailing what I did of the case, it was not my intenton to criticise either the Doctor's opinions or practice, but merely to use those parts of it, which I imagined favourable to my sentiment. I shall here, however, analize more closely this case, in hopes of rendering those sentiments more plausible than in my former Essay;* and I think I shall make

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* In this case (of a girl between eight and nine years of age) the patient had for some years "been liable to *frequent* tormenting head-achs, and *severe* stomach sickness; which were always removed at the time by an *emetic*, whose operation pumped up some bile." A few weeks before Dr. Patterson's visit, (Ap. 3d, 1790) "an unsteadiness of her limbs' was first observed, with other symptoms of chorea. "She had a catarrhal complaint attended with a *severe cough*, before the convulsive gesticulations were noticed." The Doctor found her pulse *slow* and small; and the eyes regular and sound. Bowels tardy, their habitual disposition. Head free of pain, and disturbance, &c.: a blister was applied between the shoulders; a flannel shift ordered; and the most acceptable diet to her palate.

I would here ask, whether the long continued head-ach to which she was subject, may not be imputable (if not to the deposition of water, at least) to an augmented determination of blood to the brain, at those periods of the accession of head-ach? and whether the severe stomach sickness, and use of emetics, might not have been continually increasing the disposition to effusion, by still further adding to the determination of blood, in a part apparently in a weakened state? I would ask likewise, whether the severe cough before the convulsive gesticulations were observed might not have produced the same effect, as is allowed to have occurred, not however by conversion of disease, in Dr. Percival's case, as noticed in Dr. Patterson's observations, with the view of shewing the influence of coughing upon the brain? Here then we have some room, at the very commencement of the history of the case, to apprehend the existence of the effusion, which Dr. Patterson supposes was the *effect* of the chorea; but which appears to me, more properly to be regarded as the effect of pressure, then existing.

It is by no means requisite to notice the daily prescriptions, which were chiefly of the tonic and antispasmodic kind, accompanied with occasional purgatives, exercise, the warm and cold bath, electricity, &c. I shall therefore observe, that on the 10th, after apparently mending, "an alarming faintishness came on, succeeded by languor, oppression, and much agitation," for which ether and laudanum were given. On the 28d. a little head-ach, for the fourth time since her present illness. She vomited twice on the 22d. On the second of May, she began with zinc and columbo. On the fourth, her hair was cut short, and her head dipped in the cold bath: her appetite diminished, yet her strength was as good as before.

15th. Sickness and vomiting, &c. 21st. Removed into the country; in every respect seemed gaining ground. From this date to the 21st June, she was not seen by Dr. P. but regular accounts were transmitted to him.

24th. Fatigued by ride. Very sick, retched, vomited, and threw up phlegm. 26th. Agitations morning and evening.

27th. So much agitated that the antispasmodic medicine was given along with the tonic. She has some little use of the *left*, but not any of the *right*, hand.

make it appear, that it was truly a case of symptomatic chorea, arising from the pressure of water in the brain, which

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28th. Very sick. Little or no use or power of the right hand; the left she can do almost any thing with.

I shall here beg permission to inquire on what could depend the total loss of power of the *right*, whilst the *left* hand remained capable of doing almost any thing? Surely we shall not be told that the chorea was the cause! I will ask, what more reasonable explanation can be given, than that pressure, and that partial, on the brain, produced this effect? and if pressure, what more likely than that it arose from water, already existing in the ventricles; which the dissection but a short time subsequently, proved to amount to three ounces? It has been observed, that injuries sustained on one side of the head, produce a paralysis of the opposite. Now, as the dissection shews that two ounces of fluid existed in the *left* ventricle, and only half that quantity in the *right*, may we not imagine it to induce this paralysis most particularly on the right-side?

June 8th. Sick again. I must observe that my reasons for mentioning the periods of her sickness at stomach, which was accompanied with vomitting, is, that I consider the straining induced thereby, as adding greatly to the determination of blood to the brain, and consequently rendering the disposition to effusions: still greater.

It is subsequently observed, that "notwithstanding these two sick days she had got strong and steady in the muscular powers, so much so that she was able to run about, to use her left hand nearly as well as ever, and to command her right hand in a visibly progressive degree of improvement." This I should apprehend, depended on some degree of absorption of the effused fluid, by which the pressure on the brain was diminished. This state of improvement was however but short, for on the 13th she had much head-ach and sickness of stomach, which continued without relief three days: and on the fourth, by domestic advice, she got two grains of tartarized antimony, with camomile infusion, which excited a watery evacuation by vomiting; her night's sleep was worse than usual; appetite much impaired; involuntary motion of the right hand; and what she looked at appeared clouded, or rather striped with red and purple. Do not these symptoms indicate increased determination to the brain? And what so likely to increase it as the emetic?

18th. Head and sickness so bad that a second emetic was given, with the discharge of much bile and phlegm; and seemingly with relief; but the head-ach next morning was equally bad. The bath was used, and being as ill as ever, she begged for more tartarized antimony, and went to bed very feverish. About nine that evening three grains of James's powder were given, with no visible effect. She passed a tolerable night, but soon after waking, her head was as bad as previously.

20th. Four grains of the fever powder at eight in the morning, which proved *emetic*. Slept a good deal in the course of the day,* yet complained much

* In speaking of a case attended by Dr. Quin in 1771, (at p. 5, 6,) Dr. Patterson says, "for we do not find the idea held by the parents, namely, that of the disease being a common fever, was contradicted by the Doctor, who accordingly permitted the administration of *James's* powder; the consequence of which was, *as might be expected*, that the stupor very soon came on." Now, can we suppose, that in Dr. Patterson's case, the use both of James's powder and emetics, would be less likely to produce a similar result?

which the present explanation of Dr. P. serves but to confirm, although he affirms it to come under the second subdivision,

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much of her head; powder repeated at five and ten in the evening; a gentle perspiration of some hours. Head-ach, quick pulse and heat undiminished; the heart beat remarkably rapid; much flushed. Could not water be yet suspected?

21st. Very little sleep; very hot, pulse quick, but not so strong as yesterday; sighs frequently; purgatives produced no effect.

22d. Dr. Patterson saw her; and found her complaining *grievously* of the *head-ach*; fits of incoherent muttering, but in general sensible; face *flushed*, eyes composed, and regularly affected by light, &c.; pulse 90, hard and full.

From this particular attention to the state of the eyes, then, it is probable the Doctor now began to regard it as a case of hydrocephalus, but not finding the symptoms he expected, he cannot yet view it as such; and we must conclude, his mind did not embrace the idea of water in the ventricles, until the coma and strabismus came on, on the 25th.

"Convulsive agitations vanished; and is possessed of as full power of her hands, &c. as consistent with her lately diminished strength." Blisters to the head, effervescing mixture and infusion of columbo freely given; emollient injections; two grains of calomel every night; and a few drops of the baic tincture were added to two doses of the neutral mixture, to compose the anxiety and restlessness.

23d. Rested tolerably; blister operated fully; much relief of head; pulse and other circumstances as yesterday; *vomited* soon after dressing the blister, &c. The Doctor returned home, and was informed by letter the next day, that soon after leaving her, "she was seized with a violent delirium, raving incessantly, &c." An emollient injection produced an evacuation but no relief; she *lost her speech suddenly*, and groaned continually. &c. In some time a copious sweat broke out. It appeared she had suffered a paralytic stroke, as evinced by a distortion of the mouth, and a subsequent privation of her right hand. *Quere?* What did the previous privation of her right hand evince?

Here, I may observe, that the *chronic* hydrocephalus appears to be converted into the *acute* state, by some additional effusion, more rapidly made than the brain could conform to; which I attribute to the action of the repeated emetics; for now, 25th, the Doctor found her in a *comatose* state; low delirium; *strabismus* towards the left side; *pupils dilated* and *insensible* to light: flushed; breathing quick; suspiria; partial perspirations; convulsively affected in the *left* side; *torpid* in the *right* side; deglutition much impaired; pulse 110, and unequal; costive.

26th. Quiet during the night; but coma, &c. continued; *left* arm and hand more convulsed; *right* arm continues motionless.

This shews the affinity of the paralysis, and the convulsions; and that both were symptomatic, arising probably from pressure on different portions of the brain, and differing in degree.

27th. General convulsions last night, between 11 and 12, recurring at different intervals, and of various strength until the last; death closing the scene about two the ensuing morning.

division, third head of conversions, thus laid down by D Ferriar. " If the original be a chronic disorder, such a state of the habit (favourable to the production of another disease), may take place during its continuance, and the accessory disease may be simply super-added, or it may vary the form, or affect the duration of the former."

The case I have first related, " which (says Dr. P.) inspired him (Dr. C.) with the rudiments of the *new theory*, was a case of similar conversion," I can only say that this conversion (or perversion) exists only in the Doctor's imagination. It was this case, and the dissection, which awakened

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DISSECTION.

Blood vessels of dura mater *turgid* and reddish, exhibiting strong marks of a preceding inflamed state. About two oz. of blood flowed from the longitudinal sinus by wounding it with the saw. Cortical and medullary substance of brain shewed no other morbid appearance than a kind of *serous diffusion* over the circumvolutions of the lobes: the left lateral portion of the plexus choroides seemed inflamed. From the left ventricle two ounces of watery fluid issued, and half that amount from the right. No other signs of disease observed.

I have thus, without giving the minute detail of each day, stated the various circumstances which appear to lead inevitably to the conclusion I have drawn; and I believe it is unnecessary to enlarge my remarks to a greater extent, as the whole of the Essay will give my explanation of the various symptoms. After reading this case, I think few can doubt that the convulsions and the paralysis, were here the symptomatic effects of pressure from the water; for it is not to be credited, that so much water could have been evacuated in so short a time after the symptoms of hydrocephalus enumerated on the 25th, and the decease of the patient on the 27th. I must therefore still believe, that every circumstance upholds the truth of the opinion I have advanced, that the chorea, &c. were induced by the water which had been gradually accumulating, probably from the original commencement of her head-ach, many years before; and that the effects of the emetics on the 17th and 18th of the month, were, by inducing too great a determination to the brain, to induce the inflammation apparent, and turgescence of the vessels, and thereby, also, probably to cause an additional effusion, inducing coma, delirium, strabismus, and dilated pupils, &c.

I shall not follow the Doctor in the observations which the case has led him into; except to say, that I think his case proves, that the ancients and most of the moderns" were right in considering chorea under the general head of convulsions; " although it be attended with several peculiar circumstances." I would ask, what but these " peculiar circumstances," could discriminate it from other species? Cullen then, has not corrected an error, but has fallen into one, when he acknowledges, in the later editions of his Synopsis, that he was mistaken in supposing it to be merely a variety of these maladies. His stumbling from truth into an error, is greatly to be regretted, since even the errors of so great a man are so readily embraced.

awakened in me a desire of investigating the subject, which till then had not been particularly the object of my attention. Will the Doctor affirm, that the chorea was converted into hydrocephalus, when chorea existed to the last; or that so large an accumulation of water as 12 oz. could have taken place suddenly, without producing instant death, when the slightest attention to the circumstances of the dissection must evince, that it must have been slowly accumulating? The brain, incapable of yielding, must have gradually accommodated itself by its absorption. I can now readily see the reason why I did not then suspect hydrocephalus; for I had no idea that the chronic hydrocephalus could progress, with so few characteristics of its acute form. I was, indeed, then in the dark, and should truly have taken in "good part" the Doctor's endeavours (however feeble) to furnish me "with some *light* upon the subject." His case I thank him for, as it has done more than his arguments; but to be led by him, I should regard (if I am wrong) as the case of the blind leading the blind; and the event which would follow, the Doctor well knows; I hope, therefore, he will "take in good part" my leaving him to grope by himself, unless these remarks may aid him; as I conceive his doctrines require much further elucidation than he has thrown upon them.

If the Doctor had read with candour my explanation, I think it impossible he would have thus expressed himself. "Very much in the dark, indeed, must he have been, and labouring under great perplexity, when we find him expressing his ideas in the following confused and contradictory terms." 'The *water* must have been accumulated (*accumulating*, I apprehend it must have been in the original) a *considerable time*, as evinced by the quantity, and the very enlarged state of the foramen ovale. Its *sudden effusion* must have produced apoplexy, but the *slow progress* of the effusion permitted the brain to accommodate itself to the pressure.' 'The *water in the brain* was not, I believe, the immediate cause of death. Some *sudden effusion* or congestion, was the source of the fatal issue, by producing apoplexy.'

Few persons will consider the first quotation of mine, given by the Doctor, as confused or contradictory, and I apprehend the deduction at the end will be opposed by yet fewer; a due attention to the text and context might have shewn the Doctor, that the last quotation does not contradict the former; for, I apprehend, it will be allowed, if the brain had accommodated itself to this great

pressure, it could not be the immediate cause of death in itself; and yet that some sudden effusion or congestion in addition, above its capability of accommodation, might very probably induce apoplexy and death.

I shall not say much on the illiberal and ungentlemanly attack on the "*medical treatment,*" as it would savour too strongly of the *disposition* which led Dr. P. to employ his pen so *unworthily* upon a supposed intention to offend him. I shall merely say, that I candidly stated it (whether right or wrong); as I considered it proper in investigating the subject, to withhold no circumstance, even if militating against me. I think, however, that whoever reviews the immense proportion of fat in all parts (as shewn by the dissection), the inordinate appetite, and the enormous accumulation of blood in the vessels of the head, will see reason to dissent from the idea of Dr. P. of the patient being in a "reduced condition," although he had lost above 100 ounces of blood (in above four months, a quantity by no means uncommon in as many days, on this side the Atlantic, and which might probably have *really* benefited the patient under consideration, if he had lost it in the same space of time). If the Doctor has no more sense of the propriety of conduct due from one gentleman to another, and especially of that decorum, so essential in members of the same professson; I would at least advise him to review his own practice, and see if the unwarrantable application of the line addressed to me, may not reverberate upon himself.

(To be continued.)

To the Editors of the Medical and Physical Journal.

GENTLEMEN,

READING in your Journal for May last, the Address to the Professors of Physic and Surgery, for investigating the Cause, Symptoms, &c. for the Cure of Hydrophobia, and wishing to be useful to mankind, I beg leave to relate a case in the fewest and plainest words possible. A dog, not supposed to be mad, came into the yard of Mr. J. Stoll, farrier, at Charlestown, South Carolina; he bit several hogs, a cat, a woman, and a boy, about fifteen or sixteen,

sixteen. The hogs were first seized with the rabies and died raving mad, as well as the cat. The boy was next attacked with the hydrophobia; I was directly sent for, and found him in a very dreadful situation, endeavouring to bite every thing. Upon the sight of a glass of tea, he was seized with the most violent convulsions I ever beheld. He was copiously bled, and the intestinal canal emptied with cathartic injections. As I had not seen many cases of rabies, I requested a consultation with Dr. John Delapowe, one of the most learned, skilful, and ingenious physicians I ever had the honor of knowing. He advised the part to be scarified and cauterized with a red hot iron, and dressed with blistering ointment; which was immediately done. We retired into a private room, where Dr. Delapowe made the following observations. The hydrophobia is a spasm, not only of the pharynx but of the œsophagus also, and the dread of choking, when shown any liquid, is mental. Therefore I propose that we should endeavour to change his ideas, by taking him in a boat, out of his depth in the sea, and detaining his head under water, until he is *all but dead.* I undertook to superintend this business myself, and kept him under water until he was totally deprived of his senses, and indeed I feared, *not a little,* that he could not be restored to life. However, with frictions, &c. in an hour, or thereabouts, he opened his eyes, and sometime afterwards spoke. He of course must have swallowed some sea water; but his ideas of the dread of liquids were removed. He drank a little grog; rested tolerable that night. The next day he ate and drank moderately, and slept very easy the following night. The third day he appeared to be perfectly well, and his sleep was natural, without any symptoms of the disease; we now congratulated ourselves that we had discovered a method of cure after the commencement of the dread of water. Alas! the fourth day, about noon, he was seized with a violent vomiting, which resisted the power of medicine. He expired quite exhausted, on the seventh day, without the least symptom of the hydrophobia.

Q. What did this vomiting proceed from? The bowels were open: He took broth, gruel, &c. &c. but could not retain any thing on his stomach.

The woman went through a mercurial course and escaped. She was kept in a gentle ptyalism three weeks. This happened in the year 1774.

I am, &c.

Milford, S. W. July 4, 1807. J. HARRISON. M. D.

P. S. I have vaccinated (gratis) more than 150, in defiance of one of the most illiberal, malicious combinations, of country practitioners, that any Gentleman ever met with. I have also to observe, that several of the vaccinated children lay in the same beds with other children infected with the small-pox, without taking the disease.

To the Editors of the Medical and Physical Journal.

GENTLEMEN,

THERE is no accident or disease, concerning the proper treatment of which there are more opinions, than in that of burns and scalds. Turpentine, cold water, chalk and vinegar, lime-water and oil, scraped potatoes, pounded onions, &c. &c. have been recommended by different practitioners, and every one seems to place as much confidence in the efficacy of his own remedy, as Mr. Perkins does in his metallic tractors; and, in my opinion, when any one remedy is used indiscriminately to every accident from fire, they have just as much reason to expect success.

If Drs. Kinglake and Kentish could be prevailed upon to join their plan of treatment, I am confident they would find their patients would be sooner relieved and cured, than by restricting themselves with empirical obstinacy to one favourite application.

Burns and scalds may, perhaps with propriety, be divided into three kinds, differing indeed only in extent.

The first division may include those cases, in which though the cuticle is raised in blisters, it still remains to cover the cutis.

The second those, in which the cuticle is stripped off, and the cutis exposed.

And the third, those in which the parts are so completely destroyed as to render a separation necessary.

In all the cases of the first description which I have met with, the application of cold water has been so universally successful, that I have never thought it necessary to seek for a more efficacious one. If an extremity has suffered, it may be plunged into a vessel of water, and kept there for any length of time with safety and advantage;

age; indeed, so great is the mitigation of the pain, that the patient could not well be persuaded to remove it. If the injury has been inflicted upon the body, cloths wet with cold water, to which a small quantity of shrub has been added to increase the evaporation, will be found the best application; some caution will however be necessary, as inflammation of the viscera has sometimes been produced.

After a few days if the serum from the blister is not absorbed, it may be let out by *small* punctures, and the sores, if any exist, dressed with saturnine or calamine cerate.

The second division of cases are by far the most painful, and the most frequently fatal; few cases, if the injury is extreme, particularly in children, will do well under any mode of treatment. Symptoms of irritation come on immediately, and the patient frequently sinks in less than eight and forty hours. As death seems to be occasioned by the shock being greater than the constitution can recover, the indications of cure can never be to extinguish the small remains of life by the sedative effects of cold. And the cases of this kind, which I have seen, tend to support the opinion.

I have seen a child brought into a hospital, under the care of one of these extinguishing practitioners, and cooled to death's door, when the House Surgeon thinking the child would die, removed the cold applications, and gave the child some warm negus; it became much better and continued so till morning, when the cold was again applied, and with the same effect; till it was recovered by the use of cordials and warmth.

The plan I should recommend, would be to sheath the inflamed cutis as much as possible, and then lessen irritability, and support the patient with small quantities of wine. The first intention would be best answered by oil and lime water, cream, or milk and lime water. The second indication would be sought for in opium, which might be given in doses according to the pain, &c.

It is only in these cases, where there is destruction of parts, that I should ever think of using spirit of turpentine; in the first mentioned cases, it is unnecessary, and in the second it is, in my opinion, *highly improper*. Who would think of dressing a common blister, after the cuticle had been removed, with turpentine? What would be the consequence? Any man not biassed by prejudice, must know, that the most excruciating pain would be the actual consequence; and practice, (as far as my experience goes) confirms

confirms the supposition. There may, perhaps, be some cases, in which, at the same time that the cuticle is repaired, the cutis may be so far destroyed, as to be immoveable to the stimulus of turpentine. In cases, where there are sloughs to be cast off, turpentine may hasten the separation, and it may lessen the fœtor from a large wound; this, in my opinion, is all a reasonable man can expect from it.

It may happen, that each of these cases may occur at the same time in the same patients. I should not hesitate to combine the methods of treatment I have mentioned, according as circumstances might require. I should never fear offending the constitution by different applications at one and the same time.

I am, &c.

A MEMBER OF THE COLLEGE.

July 9, 1807.

To the Editors of the Medical and Phyſical Journal.

GENTLEMEN,

YOUR correspondent Mr. Pope, in the last Number of your Journal, makes a profession of being solely actuated by a motive of benevolence in calling the public attention to what he considers as evidence of the "bad effects of the cooling treatment of gout." So confident does Mr. P. appear to be of the great importance of his communication, that he seems to anticipate no small share of influence from his narrative of the alleged injury "in inducing me to renounce a practice often fraught with the most mischievous and dreadful consequences."

That Mr. P. really believes what he asserts does not become me to question, but that the speculations he founds on the statement of facts he has given, are entitled to admission, is a point of considerable doubt. Mr. P. enters on his subject with a declaration of "having had three instances of the bad effects of the cooling treatment of gout," but for want of having taken notes of two of them, he has not felt competent to detailing them to the public. The case stated, therefore, was daily *noted*, and consequently assumes the authenticity of a medical report. This *notable* case then, it should seem, originated from a "trifling

fling sprain of the ankle," which had occurred "three days before an agonising pain of the stomach, attended with nausea, vertigo, and a pulse not exceeding 54 strokes in a minute" presented. The local effects of the sprain at length proceeded to "violent pain, and considerable inflammation," yet no abatement is said to have happened to the constitutional symptoms. On the sixth day, however, "rags wetted in cold water, mixed with a small quantity of Goulard's extract, were applied to the *(sprained)* ankle and foot," which afforded almost immediate relief: but it is observed, the relief was "short lived," the aforementioned symptoms recurring in the evening. Our author, however, does not trouble himself with any speculation on the reason why the "short lived" relief was not protracted to a longer period. It is, indeed, not very likely that an *effect* will long survive its *cause*. The cooling application is allowed to have procured the relief that was experienced, but its employ would appear to have been insufficiently continued, which probably led to the "short lived" relief afforded by it.

The impartial reader must perceive an unaccountable strangeness in Mr. P. not resorting to the remedy that had proved advantageous; on the contrary, in his preferring to excite by warm water and other internal stimulants, parts but recently, and for a short time only, relieved from the inflammatory effects of a sprain. Mr. P. closes the detail of his *notable* case by observing, what should have been the first object of notice, that the lady who was the subject of it, had "paternally inherited the gout," and had been periodically attacked with the disease during twenty years. This circumstance warranted our author in concluding, that what by his own confession was primarily an affair of *sprain*, eventually became gout. This persuasion was sufficient in his estimation, (whether "reasonably" or not, as he assumes, is not quite clear,) to authorise an assiduous endeavour to drive the gouty enemy from the system, by a stimulating mode of treatment. Mr. P. indeed asserts, that the difficulties of his patient were not to be surmounted, until what he terms the "morbific action" had run its career: whether this said action was on the extremities, the stomach, or the brain, is not stated.

These, if I mistake not, are the leading merits of a case which formed *one* out of *three* deserving to be *noticed*, or rather, of which *notes* were actually taken. If the case thus honoured by public *notice*, be superior in its claim to attention

attention to the *other two*, nothing surely will have been lost in not *noticing* them; whilst injury might be done to the reputation of a treatment intended to be opposed, by the *insidious* intimation given of their existence.

In return, I can truly aver that I have no personal "disrespect" to Mr. P.; but it behoves me likewise to affirm, that it is not in my power to feel the smallest *respect* for the kind of case with which he has thought proper to censure the cooling treatment of gout, nor for the apparently hopeless prejudice that renders him so *unthinkingly* averse to the practice. I am open to conviction on all questionable subjects, but am, and ever must remain, inaccessible by any such authority as Mr. P's case furnishes.

I am not disposed to trespass on your book by a lengthened explanation of the support which my opinion of the nature and appropriate treatment of gouty inflammation, derives from the real merits of Mr. P's case. It will be sufficient for that purpose to observe, that sprained ligaments and tendons, often induce distressing constitutional symptoms; amongst others, *locked jaw*, and even general convulsions, are not very rare occurrences. "Agonising pain at the stomach, nausea, and vertigo;" these may well arise from such a cause, and that they did originate from it in the case under consideration, is evinced by the fact that those symptoms *followed*, and did not *precede* the infliction of local violence.

In the course of a few weeks, I shall submit to the public in a work now in considerable forwardness in the press, a large *additional* stock of facts on the cooling treatment of gout, in the number of which, it is possible there may be arguments sufficiently cogent in favour of the practice, to overcome Mr. P's prejudice against it, and to convince him, (if open to conviction) that crude and common place notions on a medical subject, are "fraught with more mischievous and dreadful consequences" than are justly imputable to the cooling treatment of gout.

I am, &c.

ROBERT KINGLAKE.

Taunton, July 9, 1807.

To

To the Editors of the Medical and Physical Journal.

GENTLEMEN,

IN the communication I made to you in a late number of your useful work, page 460, on the improved state of the flexible metallic bougies and catheters, I mentioned I had carried them to every size which was considered by the faculty useful to their success in practice.

The great recommendation of the caustic, held out by its favourers, has been the quickness of the cure, or decrease of the stricture; but it is acknowledged, that this also is a matter of uncertainty, and it has required to be continued longer than the parts could bear, and even failed then completely of a cure, nay, that in many cases after submitting to much pain, only a partial cure has been effected, and that in a few weeks a reproduction of disease has occurred. Taking these facts as they stand, will any practitioner say, that the caustic either admits a *general application* in the cure of strictures, that it promises relief with certainty, or that its application can be trusted in any case as safe? On the contrary, it appears to be, in almost every instance, severe, painful, and incomplete in its operation. But to judge of it more fully, let us state next the progress of the Flexible Metallic Bougie.

In the construction of this instrument, No. 1, is formed so small, that there is hardly a stricture of the urethra so complete which it will not with proper caution and perseverance pass; experience is no doubt necessary here, as in all other operations, that the first attempts may be successful; and I can with confidence say, that by attending minutely to the state of the stricture, and adopting the size of the instrument to the degree of occlusion in the passage, I have never failed in introducing the bougie, and thus in the end of removing the disease. When the bougie is past, it is generally my rule to allow it to remain for half an hour, and what is remarkable and peculiar to the metallic bougie is, that during this time the patient suffers no uneasiness from is retention, which is so much the case with other bougies. Hence, I conceive that it really possesses a soothing or anodyne quality, connected certainly, as I have formerly stated, with galvanic principles. On the second application of the bougie, I begin with No. 1, as on the preceding day, to judge whether the advantage first gained over the stricture

ture continues, and finding it pass readily I then introduce No. 2, which is retained for an equal length of time as the instrument at first, and by this regular daily succession of applications, and gradual enlargement of the size of the instrument, the disease comes to be completely removed in the space of two or three weeks at farthest, without any inconvenience to the patient, or any symptons arising to prevent his ordinary pursuits, and with his acquiring rather better health while under the influence of the cure, as the discharge of the bladder goes on more regularly.

Some surgeons, while they decry the use of the caustic in the form employed by Mr. Hunter, have still not abandoned the principle, but have expected to get the better of its hurtful consequences by substituting another article of the same description, or the *kali purum*. This, instead of being an improvement, I should conceive, is still more to be dreaded than the lunar caustic, for in its action it is less circumscribed, and more apt to affect the surrounding parts, which is in fact, the leading objection to the use of caustics at all. What I contend against, is the general principle of cure, whatever the form of the caustic may be that is used; and since a method exists, as I have proved to the satisfaction of the most celebrated of the faculty, that is *easy, certain*, and attended with none of the dangerous effects which universally arise from the application of caustics of any description on such a delicate and irritable structure as the urethra; the employment of caustics, I flatter myself, will appear both a *useless* as well as *pernicious* practice. It is even the more so, since its greatest favourers admit, that in order to perfect any cure, the use of a bougie is still necessary. If, therefore, it is necessary in the progress, the same means will equally avail in the first instance, and be much more agreeable to the patient.

In closing my Remarks, I cannot here omit the paragraph of a letter from a patient in Edinburgh, who has just now applied to me, and had formerly submitted to the caustic for a cure, where it not only failed, but my correspondent observes, "that from the time he had *suffered* the operation of the caustic, the urethra had been constantly ulcerated;" a proof of the justice of my observations. I am, &c.

WM. SMYTH.

Tavistock Street, 15 June, 1807.

To

Some Account of a new Theory of GALVANIC ELECTRICITY, *founded on Experiments.* By M. J. A. HEIDMAN, Physician in Vienna.

M. HEIDMAN sets out with giving a succinct account of Galvanic Electricity, relatively to the discovery of the Voltaic pile. He includes under the same head, the Observations of Galvani on animal electricity; the Experiments of Valli; the Essays of Volta, in opposition to the advocates of a particular animal electricity; the Dissertations of Fontana, and of Professor Reil; Du Creve's Essay on the irritability excited in a vacuum; the Inquiries of Fowler, Pfaff, Humboldt and Ritter; and lastly, he notices the experiments which led Volta to the discovery of the electrical pile, which M. Heidman designates by the appellation of *Galvanic Battery.* He next proceeds to give a description of this apparatus, of the several parts of which it is composed, and of their different dispositions. In this part of the work he treats successively of the simple galvanic chain; of the nature of conductors; of their mass and extent; of the properties of solid or liquid conductors; and of the qualities attributed to solid and moistened conductors.

M. Heidman next considers the experiments and opinions of the various authors who have written on this subject: after which he proceeds to explain his own theory, which he supports by appropriate observations.

Two experiments which he made with a view of determining the influence of humid bodies in the galvanic chain are related. The first consists in placing in contact two homogeneous metals with the nerves and muscles of a frog, and establishing a communication between the two metals by means of zinc and silver; in this case no contraction is perceived, either in closing or opening the chain, which is not in conformity with the opinion of Volta; there appears then to be wanting the essential condition, viz. *humidity,* between the two heterogeneous metals.

In the second experiment he plunges the metallic plate into vessels containing salt water, and the electricity becomes augmented on increasing the quantity of the liquid, so as to bring it in contact with a greater surface of the metals. The author here refers to similar experiments by Messrs. Desormes and Van Marum, who also concluded that the size of the moist surfaces greatly contributed to the strength of the pile.

M. Heidman afterwards endeavours to ascertain the power of the different fluid conductors, according to the degree of their chemical action. For example, having placed two prepared frogs in such a position, that their nerves were inserted in watch glasses containing different liquids, such as a solution of alkaline sulphuret on one side and water on the other, and the communication of the two fluids being established by means of a metallic wire, he observed, that at the instant of the formation of the chain it was the muscle on the side of the first of these liquors that became contracted, and that immediately on the rupture of the chain, the muscle on the side of the water alone suffered the contraction.

In order that the stimulant qualities peculiar to some liquors, and particularly to the acids, might not lead him into error, he interposed between the nerve and the liquor, a piece of flesh well soaked in water.

In this manner he submitted to experiment all the different liquids already known as conductors of ordinary electricity, and he has arranged them in the following order, relative to their chemical power, and their galvanic action.

Acids—oxymuriatic.
——— muriatic, impregnated with azote.
——— muriatic.
Solution of oxymuriate of pot-ash.
Acids—sulphuric.
——— fluoric.
Sulphurets—alkaline.
——— earthy,
Acids—phosphoric.
——— arsenic.
——— oxalic.
——— boracic.
——— molybdic.
——— acetic.
——— benzoic.
——— gallic.
Solution of ammonia.
——— pot-ash.
——— soda.
——— phosphate of soda.
——— ammoniacal acetite
——— ammoniacal muriate.
Solution of martial muriate.
——— ammoniacal nitrate
——— ammoniacal sulphate.
——— muriate of pot-ash.
——— nitrate of pot-ash.
——— sulphate of pot-ash.
——— phosphate of pot-ash.
——— acetite of pot ash.
——— muriate of soda.
——— nitrate of soda.
——— sulphate of soda.
——— phosphate of soda.
——— acetite of soda.
——— muriate of barytes.
——— muriate of lime.
——— muriate of magnesia.
——— sulphate of alumine
——— nitrate of silver.
——— sulphate of copper.
——— sulphate of iron.
——— sulphate of zinc.

Solution

Solution of acetite of lead.
Citron juice.
Solution of acidulated tartrite of pot-ash.
——— tartrite of pot ash.
——— emetic tartar.
Water saturated with carbonic acid gas.
Solution of ammoniacal carbonate.
——— carbonate of pot-ash.
——— carbonate of soda.
Lime water.
Recent urine.
Serum of the blood.
Blood.
White of eggs.
Fresh muscles.
Nerves, while yet moist.
Water.
Sweetened water.
Saliva.
Juice of fresh plants.
Milk.
Wine.
Unrectified alcohol.
Rectified alcohol does not act as a conductor.

The author draws the following conclusions from his experiments on the above liquids.

The oxydating liquid, first in the order, possesses the greatest chemical power, and determines the oxygen pole with the greatest number of the solid conductors.

The liquid of an inferior rank is therefore only a simple conductor, which in the apparatus for the decomposition of water, indicates the hydrogen pole. The oxydating, or irritating liquid, and the liquid conductor, ought therefore to be distinguished from each other. He observes, however, that the order he has assigned to these liquids, is not constant and uniform, unless they be employed at an equal degree of concentration.

M. Heidman afterwards examines the action exercised by these liquids upon the various solid conductors, which he regards as depending upon chemical affinity, and which is manifested by the displacement of the poles, in the same manner as with zinc; for instance, the acids precede the sulphurets and determine the oxygen pole, as with lead, tin, &c.; while with gold and platina, the alkaline and earthy sulphurets have a superiority even over the acids, and leave to them the hydrogen pole only.

In treating of the galvanic chain, the author makes two distinct classes.

The first he represents by the following series:

Metal more oxydizable — water — metal less oxydizable than the body communicating by immediate or intermediate contact with the two solid conductors. In this chain the oxygen pole is always according to the direction in which the two solid heterogeneous bodies touch the water.

The second is represented by this series, *oxydizing liquid—solid oxydizable conductor—liquid simply a conductor*; then follows the communication of the two heterogeneous liquids.

Thus in a chain formed of *silver—water—zinc—silver—water—zinc*; the first plate of silver and the last of zinc are, according to this author, superfluous conductors, and ought not to be considered as essential parts of the battery, since by these two plates the chain is only compleated, and the two chains of six members are changed into a battery of two. Consequently, when the pile is formed with plates of copper and zinc soldered, it should be terminated at the two extremities by simple plates, in order to avoid superfluous conductors, since it is not by placing the copper or zinc at one of these extremities which decides the hydrogen and the oxygen pole, but only the respective disposition of the two heterogeneous metals, and their contact with water; whence he concludes, that the plates by which the pile is terminated, are superfluous members of the battery, in opposition to the opinion of Volta, Carlisle, and all those who assert that the galvanic action is produced only by the contact of two heterogenous conductors, whether liquid or solid.

In the third division of the work, a detail is given of the phenomena exhibited spontaneously, and without any foreign aid, by the battery of Volta, when in a state of the greatest activity: the author enumerates seven principal ones.

1st. The peculiar odour perceived after a short time, differing a little according to the nature of the conductors, which are for the most part metallic.

2d. The slight noise or crackling produced by a strong battery, whether in the pile or in the dish apparatus, and which proceeds from the hydrogen gas, liberated sometimes even with a sort of white froth.

3d. The changes which the moistened conductors undergo in both kinds of apparatus, such as the decomposition of water, the saline efflorescences, the presence of a free alkali, when neutral salts are employed, &c. &c. A more accurate investigation into the nature of these changes is regarded by M. Heidman as opening a vast field for new experiments.

4th. The oxydation of the metals, which the present author regards, in common with many others, as essential to galvanic action. It occurs, he observes, when the chain is formed, not only in atmospheric air, in nitrous and

and oxygen gases, in oxyde of azote, in acid gases, but also, though in a less degree, in the vacuum of the air pump, and in those gases which contain no oxygen, such as hydrogen and azotic gas. He discovers in these circumstances what principally constitutes the difference between the battery of Volta and the common electrical machine, in which no electricity is produced without the presence of oxygen. In the galvanic battery, oxygen is furnished by the water, by the liquors in which it is contained, and by the acids, and its action is powerful in proportion to the abundance of this principle.

5th. The **absorption** of oxygen. One of the most remarkable experiments is, that in which Professor Schaul observed the decomposition of common air under the bell-glass; the cessation of action when the oxygen becomes exhausted, the renewal of the action even with commotion, on furnishing a supply of fresh air, are phenomena which prove, in the opinion of M. Heidman, that the presence of oxygen is necessary to produce this action, and that it depends on a chemical operation.

6th. The immediate action of the Voltaic battery upon Bennet's electrometer. When this instrument is brought near to the hydrogen pole, the divergency of the gold leaves is stronger than when it is brought near the oxygen pole. Immediate contact is not necessary to produce this effect; it is sufficient that the base of the electrometer communicates with the earth, to establish a conductor so as to produce a divergency; which is, however, in this case more feeble. The author was not able to ascertain two opposite states of electricity in the hydrogen and oxygen poles. He employed with this intention a very strong Voltaic battery, and an electrometer formed of a very narrow and sensible glass cylinder; on establishing, by means of a conductor, the communication between the hydrogen pole and the top of the instrument, the divergency nearly carried the gold leaves to the sides; and they remained in the same situation when he established the communication with the oxygen pole, which would be impossible in the case of two contrary electricities. If a communication be formed by means of a good metallic conductor, between the Voltaic battery and a prepared frog, which does not communicate with the ground, no movement whatever takes place; which affords a convincing proof, that in every galvanic action there must be in reality, a discharge, or partition of the galvanic electricity, which did not occur in the present experiment.

7th. This article has for its object the spark of the Voltaic battery, as observed in the dark by Nicholson. Here the author also relates the observations of Pfaff, Hebebrandt, Biot, and Halle, by which it appears, that independently of the closing of the chain by the two extremities of the battery, a light often appears on the pile itself, that is, on the sides of the metallic plates. M. Heidman thinks himself warranted in doubting this phenomenon, the observation of which appears to him not to have been accurate, or to be reconcilable with received principles. He supposes, that in the dark we may close, by means of the conductor, only a part of the battery from which the spark has been elicited.

From the above analysis, it must be evident, that M. Heidman frequently differs from those whose opinions he details, and that he builds his own doctrines on experiments peculiar to himself.

To the Editors of the Medical and Physical Journal.

GENTLEMEN,

I Take the liberty of transmitting you a small Collection of "Facts, determining the efficacy of Fowler's Mineral Solution in Chronic Rheumatism," which was circulated about a year and a half ago amongt the Physicians, Surgeons, and Apothecaries of Manchester and its neighbourhood. The first of those facts occurred nearly four years since, and was not long afterwards published in your Journal;* it forms, I presume, the basis upon which the present use of arsenic in that disease rests, whoever may or may not chuse ingenuously to acknowledge the source from which the hint was derived. When I consider the characters of the gentlemen, in different parts of the kingdom, of whose attention to the subject I have had the pleasure to be informed, I entertain great hopes that the remedy in question will soon be more generally adopted; as the weight of their recommendation, if

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* See my letter, Vol. II. p. 492.

if it should prove successful in their hands, will make up at once for the lightness of my own.

Dr. Bardsley has lately published cases, which will go far to bring it into notice; for though I have not yet had the pleasure of perusing his book, I know he has reported favourably of it from an extract or two which I have seen; and I know him too well to doubt either the candour or the success of *his* communications. I have nothing to add to the testimonies already adduced upon the subject, my own opportunities of prosecuting the inquiry having ceased for some time; but I would throw out a hint or two before I conclude. A long perseverance in a moderate dose of the solution, without augmentation, would seem to promise every advantage likely to be derived from its use, without exciting those inconveniencies which, with a full and increasing dose, are occasionally sufficient to forbid a continuance of the plan. Catarrhous symptoms, rather more troublesome than usual, are easily brought on in some persons by *full* doses of this medicine; a circumstance which may very rationally beget much caution, as to the exhibition of it in patients of a phthisical constitution. There are also irritabilities about the stomach, in which it may be requisite to watch its effects with care, and to suspend its use altogether, without delay, in case of material disturbance; but it is not *all* weak or disordered stomachs that oppose its use, for I have witnessed *good* effects from it in several dyspeptic cases, which were almost unmanageable upon other plans. Nevertheless, it may be required in other patients to resort to a fuller dose, gradually carried as high as can be borne; but in my own practice I should *now* be inclined to restrict *this* employment of the medicine to cases wherein the gentler mode had previously failed of doing good. In young persons, with full habits particularly, low diet, *small doses*, and occasional cathartics, of the saline kind, are necessary to ward off heat and other febrile symptoms, in the beginning of the plan at least; and in general it may be observed, that elderly persons, or others much debilitated, bear the medicine best, and of course most frequently profit by it. Former remedies have effected very little indeed towards affording relief in "nodosities of the joints," of the painful or rheumatic sort; and if it should appear that we possess in arsenic the means of benefiting any considerable proportion of the unhappy sufferers from that most tormenting disease, the fact itself will supercede the necessity of other eulogies. I should suppose it would be

sufficient, for the purposes of truth, to refer in some part of your Journal to the letters which compose the collection I have sent you a copy of, as, with the exception of the last, they were taken from your pages; but if you should think farther notice called for by any circumstances within *your* observation, you are at liberty of course to act upon your own opinion.*

Oxford, June 8, 1807.

I Have lately had the gratificatien to learn from several quarters of the highest respectability, that Fowler's Mineral Solution has been found essentially servicable in tic doloureux, where the pain has had either regular or irregular remissions of some duration; it should be observed, that this remedy seems to require such cessations to ensure its efficacy, and that the more periodical these cessations are, the more successful it is, not only in tic doloureux, but for the most part in the other complaints which it relieves. It was originally in my contemplation to name tic dolonreux as one of the disorders in which it would be right to put arsenic prepared thus, or in some other manner,† to the test of experience, after I had ascertained its utility in chronic rheumatism; and I was only deterred from doing so by the general hopelessness of those inveterate cases. Its similarity to rheumatic affections, however, very easily and naturally pointed out tic doloureux, as requiring such a trial of the powers of the mineral solution, and I am extremely happy to learn how favourable the issue of it has proved. I hope, that the gentlemen who have ascertained the fact of its *efficacy* in that disorder will favour the public with some account of their success: the names of those to whom I allude, are such as would establish the remedy in the public opinion most satisfactorily. At this period, when the efficacy of this medicine, in the worst rheumatic cases, has been so fully proved,‡ I scarcely know any idiopathic *external* pain not connected with inflammation, and not absolutely *incessant*, in which I should not expect some *alleviation*, at least from its use, if more common remedies demanded the experiment; and perhaps it is not too much to say, that its various properties entitle it to *trial* even in other asthenic

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* Vid. Med. & Phys. Journ. Vol. xiii. p. 16, and 525.

† The Kali Arsenicatum of St. Bartholomew's Hospital Pharmacopœia is essentially the same, and a very convenient form for watery solutions.

‡ Particularly by Dr. Bardsley in his recent volume.

asthenic diseases, whether of a spasmodic nature or not, for which we are at present unprovided with efficient remedies.

Under such circumstances, little benefit would more than recompense our attention; and how is physic to be improved if new articles are not to be cautiously tried after the failure of all the old ones, whatever may be the chances against them *before hand?* Of course, I would except such diseases as originally affect the thoracic and abdominal viscera, and the lungs particularly. I have seen cancerous cases, in which it has proved the best anodyne internally employed; and athritic ones, in which it has afforded complete relief *indirectly*, by converting lingering and irregular attacks into regular paroxysms.

I am, &c.

JOHN JENKINSON.

Oxford, July 8, 1807.

To Dr. BATTY.

SIR,

MEN of liberal education, I perceive, are ready either to blame or lament every little variety of action or mode of expression, that is not exactly conformable to the settled conduct of their own professional circles; perhaps in essential points, the feeling is proper; yet where this variety does not arise from the *study* to be different, from the *affectation* of eccentricity, these *petty tumours* in society, these little *warts and pimples* in manners are worthy to be as little noticed by the public, as by the individual to whom they belong. But if the individual once begins to be proud of his tumors, and fancies his warts and pimples are beautiful, or the means of acquiring the smallest notoriety, he is then fair game for the Critic, and his *affectation* is just as contemptible as any other species of this ridiculous genus. Now, my dear Doctor, I am not petulant enough to suppose, that your recommendation of seriousness in style, nor the friendly and gentleman-like letter of Dr. Smith, (which deserves my sincere thanks) were levelled at any habitual and inveterate disorder in my manner of expression, but merely to tell me I should observe Hamlet's directions, "suit the words to the action;" that diseases should be detailed in serious language; that physic is a grave

grave profession, and its practitioners "sad men." As I am sure these were your motives, I take them in good part, and though I do not conceive it necessary to array a short letter, containing a case, in studied technical phraseology, or in cautious well rounded periods, or pompous verbosity, (these being I should suppose more adapted for essays or orations) yet a letter may occasionally (I speak with submission to your better judgment) be colloquial; it may be as nearly resembling the spontaneous conversation of the person as possible. However, be this as it may, I shall observe especial care to relate the first case that occurs, which I may think worthy the public and your notice, *with all the seriousness the subject demands.*

I know it is imagined by some, that since the canes, ruffles, cloaks, monstrous perriwigs, and other paraphernalia of the Faculty, have given place to a dress and deportment more conformable to the times, that physic and physicians have lost some of their respect from the great world, and nearly the whole of their former estimation and consequence with the vulgar. In my opinion, although I have no wish to "strip medicine of its plumes," this is a *very* vulgar error. If we revert to times when these ornaments were not in fashion, we find the Bishops of former days were not less reverenced in their simple garb, nor the physicians less valued, in dresses far from ostentatious. In after times, the dress, affected deportment, and phraseology of the faculty, arose to such a height of absurdity, that nothing less than the keen satire of MOLIERE could have subdued the monster; and it is certain, more good, more reformation arose in the appearance, education, and conduct of the faculty from the sharpness of his wit, from the ridiculous light in which he displayed their ignorance, follies, and affectation, than from any book of medical ethicks ever published. It must be an agreeable reflection to the faculty to remark, that at present, medical learning and professional skill are justly valued, and obtain their appropriate rank amongst the other learned professions, and are as highly esteemed by the fashionable world, although they are ushered in without the solemn and ridiculous appendages, so essentially necessary seventy years ago.—Not but I think I have observed among some of the faculty of these days, attempts to entrap the world by *affectations* of one sort or other.

The catalogues of the Royal Colleges of Physicians and Surgeons, and the Worshipful Company of Apothecaries, contain no small collection of antiques and curiosities. We

We may see the importance attached to some Gentlemens' wigs; with what solemnity they tap their snuff boxes, and examine their stop-watches. You need not be so ready to start! and tell me there is no harm in this. I say there is harm, they are tricks *ad captandum*. The same remark applies to those who are so pertinaciously technical; these gentlemen discover such *vast constriction of the cuticular capillaries*, and order *deobstruents to promote a diaphoresis, without deranging the catenated associations, &c. &c.* Others affect extreme interest for a patient at the *first interview*; nay, such interest, that undoubtedly they must be the most charming men in the world. No less silly and affected are certain chemists, who blow their filthy *hydrogen* in the face of all companies, and sometimes succeed by their noise and bluster in taking a *weak* part of the town by storm. Now, Sir, these may be all "honourable men," and I am persuaded would be natural men, if they did not conceive they *had parts to act;* they know that the *cullibility* of human nature is infinite, and they wickedly pamper the glutton, instead of endeavouring to restore the constitution of the public, and enlighten the world they live in. As I despise *affectation* in whatever dress it appears, I am as much, nay, more disgusted, with the slovenly, unshaven, uncombed, vulgarity of character certain *Gentlemen* assume: I know it is assumed, their education and writings being in direct opposition to their manners. These men glory in their sneers at all good breeding and gentility, and are almost as offensive as the pretended sanctity and humility observable in some others. When the vulgar detect these tricks, that men of education stoop to practice, I fear it will furnish the strongest arguments for inferior understandings to dispense with believing or practising any thing. I could give you more examples of *affectation*, as combined with some feats of hypocritical dexterity, not at all honourable to some of the profession, but I have no wish to encroach on the pages of your excellent Journal with subjects of this nature. I am more delighted with contemplating the fair side of the picture, and it gives me infinite pleasure in knowing that medical men have now almost universally good educations. For my own part, I reverence the profession, and (with all their foibles) the professors. I believe them, as a *body*, the most learned, and best portion of the public. I know that more private charity is done by them, than by any other class whatever. I know that, in order to gain a livelihood, they have to struggle with more difficulties than in any other profession

sion or occupation. I also know they are worse rewarded, and that fewer fortunes are made by them than in any common trade or merchandize.

Having said this in their behalf, I trust you will believe me incapable "of setting down aught in malice;" and as you have been kind enough not to apply *caustic* to my *warts*, I am civil enough to send you no *drastic* in return.

I am, &c.

T. Y.

To the Editors of the Medical and Phyſical Journal.

GENTLEMEN,

WE were given to understand, that after an appeal to the Royal College of Physicians, for their opinion on Vaccination, private communications on that subject would be for the most part suspended; but as several pages of the last Number of your Journal were occupied in treading the beaten path of animadversions on the Anti-vaccinists, you will allow me to make an observation or two on medical controversies in general, and this in particular.

When new Theories in medicine are discovered, however ingenious in themselves, they will be received by experienced practitioners with caution, and examined with candor; nor should the discoverers be too sanguine in their expectations of a general adoption of their systems, till a sufficient period of time has elapsed to confirm the success of the practice.

It is possible that an opposite mode of treatment of the same disease may prove successful with patients of a different temperature. Hence the application of cold water, in burns and scalds as a powerful sedative, proves an appropriate remedy for a patient of a robust and vigorous constitution, when a more stimulant application, (such as the terebinthinate) is equally necessary to another, where inanition or the want of vital energy prevails. These circumstances considered, why should practitioners so furiously contend for the infallibility of the one, or the futility of the other treatment in the same case?

The same observation perhaps may be admitted respecting the cold ablution in gouty inflammation. It has been administered

administered with success (doubtless) in one case, whilst a practitioner would be deterred, by a reverse of temperature in the patient, from adopting it in another; and as both the cold and warm treatment have both proved efficacious in this disease, (for facts are stubborn things) I humbly apprehend, the success has chiefly depended on the constitutional variety of their respective patients.

As to the Vaccine Contest, there has been hardly any cessation of arms since hostilities commenced; and though I am no enemy to vaccination, believing it to be a general, though, perhaps, not an universal defence, against variolous infection; yet, I was sorry to find a zealous Correspondent in the last Number of the Journal raking the ashes of the dead, to support the almost thread-bare controversy. I think when the antagonist has quitted the field, that well known sentiment should be adopted, "*Nil, nisi bonum de mortuis.*" And as he has appeared before a higher tribunal, his hatchment should be allowed the accustomed motto, "*Requiescat in pace.*" Therefore, since the practice has been sanctioned by general approbation, throughout the greatest part of the habitable globe, it should seem needless to "*Ring*" another peal in the ears of the medical world on that long debated point.

It is, Sir, to be hoped, that your valuable miscellany will be furnished more with *practical* and less with *controversial** subjects in future. Your readers would be much better satisfied to find it a vehicle for useful information, than a field for endless contention; and as there is an ample scale for improvement in the "medical art," (for few will allow it has arrived to its "*Ne plus ultra*," every correspondent should aim at the accomplishment of this grand and essential point.—Should these cursory hints have the least tendency to promote the purposes of general utility in a deservedly esteemed work, the writer will be amply gratified.

I am, &c.

H. DAVIES.

Piccadilly, Aug. 3, 1807.

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* Mr. D. seems to forget, that without a stimulus all vital action is suspended. This stimulus, in Writers, is Controversy, or a discovery of new facts. The consequence is obvious. Ed.

To the Editors of the Medical and Physical Journal.

GENTLEMEN,

BEING convinced in my own opinion, that William Miles's Case, which you did me the favour to insert in your Medical and Physical Journal, No. 97, page 272, proceeded from the bite of a dog, and from the symptoms believing it to be genuine hydrophobia, together with the singular way in which he was relieved by the application of a caustic,* I felt it my duty to communicate it to the public; and notwithstanding the very ingenious remarks of Mr. Ward on the same case, in your subsequent Number, I still continue in the same opinion, and should have made an earlier reply, had not the very numerous engagements I have met with, kept me from it till within these few days. From the great haste in which Mr. Ward made those remarks, I am inclined to suppose he has misconceived me, not only in the length of time Wm. Miles took the medicine, it being five days and a half instead of seven, but in many other instances; and was he to reconsider the case, perhaps he might find it less inaccurate than he at first imagined; particularly, if he takes into the account the great variety of appearances which take place in nervous affections, and in none more than in hydrophobia.

The Edinburgh Practice of Physic, vol. 2, page 444, observes, "In this disease the symptoms are so various, that they cannot be enumerated, for we seldom read of two cases of hydrophobia, which do not differ very remarkably in this respect."

Having recorded what past prior to my seeing Miles, and that, as near as I could, in the way it was given me, may make it appear to some, not only inaccurate, but likewise to contain a great deal of extraneous matter; for instance, what passed in his mother's house, and followed immediately after he left it. "And, as she says, his throat projected out almost to his chin, he grated his teeth, and appeared incapable of swallowing his saliva." But had I omitted any part, the case would not have been fairly stated to the public, and I should have been guilty of

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* I hope I may be allowed the expression, having never read or heard of an hydrophobic patient being cured by the application of a caustic.

of a much greater fault. The only idea his mother's account gives me, is, that at that time the spasmodic affection of his throat was great, and though she expressed herself in such strong terms, (which is not unusual with the lower order of people,) I have not the least conception the projection was so great as she mentioned, though there must certainly have been a considerable fulness, or it would not have struck her so forcibly. It likewise appears, he then laboured under a strong excitement of the nervous system, from whence his mind became hurried and agitated, which occasioned the hasty reply he made when she asked him to drink some beer, "Drink, no! I have been bit by a mad dog." Had she asked him to have eaten, his reply in all probability would have been, "Eat, no! I have been bit by a mad dog." It seems his principal reason for calling on his mother, was to make her acquainted with his situation, who till then, knew nothing of the bite, he having resided at his master's. At that time, he had not the least aversion to water, though he swallowed with difficulty; at sometimes, more so than at others, as the spasms of the fauces were more or less violent. The spasmodic affection of the fauces was always relieved for the time by the antispasmodic mixture, which accounts for his taking it so regularly: indeed, upon the striking of the clock in the next cottage at the end of every two hours, he would ask for his mixture, which could not be from its being pleasant to his palate, but from the effect it had as a topical application on the fauces, though it did not appear to have the least on the system. He said, upon its being put into his mouth, his throat felt for a short time more open, which enabled him to swallow it with more ease than other fluids. However, the excitement on the nervous system might have acted on a weak imagination,* I cannot suppose the acts Miles committed

* Mr. Ward says, that the symptoms appear to have been much aggravated by the influence of fear or terror upon a weak imagination. Fear or terror, certainly, had not the least influence over Miles prior to the system being affected by the virus generated in the cicatrix, though from the distrest feelings he experienced afterwards, he became much agitated; which agitation, in a great measure, subsided on his being secured, when he became calm and collected in his mind. There still, however, remained a disposition to scratch; for on the third day of his confinement, the two men who had the care of him, took off his handcuffs and were in the act of unbinding him, when he began to scratch, and the force with which he struck his small clothes (which were of the thick plush kind, with his nails was

mitted to have the least resemblance to those of a maniac who is disposed to commit much greater acts of violence than scratching and biting. It is not uncommon for the maniac to shew the greatest inveteracy against his nearest relatives. Some years back, I met with an instance of a maniac, who making his escape from St. Luke's, the moment he entered his father's house, knocked him down, and had it not been for those present, would have killed him: whereas, the conduct† of Miles to his mother shewed the greatest love and affection towards her. However great Mr. Ward's experience may be, both from practical observation and extensive reading, I cannot agree with him, that "the short space of time (ten days) which intervened between the bite and the accession of the disease, is a circumstance so unprecedented in the history of hydrophobia, as to afford the most satisfactory evidence against the rabid, and in favour of the tetanic origin of the disorder." We are informed in the Edinburgh Practice of Physic, vol. 2, page 443, that, in some few instances, the disease has commenced in seven or eight days from the accident. Motherby says, the symptoms in some take place in two or three days after the bite, more frequently not till after as many weeks. Dr. Hamilton, of Ipswich, is of opinion the disease takes place in a few days after the bite. M. Sabatier in the five hydrophobic cases he gives, in the Medical and Physical Journal, Nos. 97 and 98, (which proved fatal) observes, that the disease appeared at different periods from the time of the injury. The first patient was occasionally attacked with vertigo from the very day on which he received the wound; in a short time, he complained of a weight in his head, and his ideas became confused; his features were much distorted, he past restless nights, and his sleep was continually interrupted by frightful dreams: every symptom

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was such as to cut holes through them; upon which the men secured him as before. Had he been a subject easily impressed with fear, his fears would have induced him to have applied for a remedy as a preventive. We have heard of some, who, not through fear only, but who have prudently judged it proper to have the bitten part removed by excision or caustic, and others, who (in all probability through fear) not being satisfied with the excision of the part only, have undergone amputation.

† It is not an uncommon thing with those who have the misfortune to be afflicted with this dreadful disease, to give the same caution to their friends, as Miles gave to his mother in the hovel. "Conscious of the approaching inclination to bite, he warns his friends of their danger, and advises them to keep at a distance." — Motherby.

tom indicated approaching disease. Dr. Houlston gives an account of a pamphlet published at Paris by order of Government, in which we are informed, out of fifteen persons bit by a mad wolf, on the 8th and 9th of December, 1775, four died in a few days raving mad; the eleven were put under the care of M. Blaise of Cluny.

One of the eleven, who had, for ten days exactly, followed the plan laid down (as preventive) became melancholy, was seized with a horror and dread of fluids, and died within forty-eight hours afterwards. Nor can I agree with Mr. Ward, that the exposure to cold and moisture* on Sunday, November the 30th, was sufficient, without

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* Cold and moisture certainly, in some parts of the globe, are not unfrequently the cause of tetanus; and those who are but little acquainted with the disease, from the following observation of Mr. Ward, may be led to suppose, that it frequently takes place in this Island from that cause. "It is well known by every person, who is at all conversant in medicine, that nothing is more common than for tetanus, with all its dreadful train of symptoms, (compared with which, those of Mr. Hicks's patient are scarce worth mentioning) to ensue from a much less degree of exposure to cold, than that which evidently took place in the present instance."

From whatever authority Mr. Ward may have advanced this assertion, I am inclined to think, that those of the most extensive practice, have met with very few tetanic cases originating from this cause. Dr. Lionel Chalmers, in the first volume of the Medical Observations, which is supposed to be equal, if not superior to any thing on the subject that has been offered to the public, says, "Happy it is for the inhabitants of the more temperate climates, that such diseases appear very rarely among them; but, in those countries which lie in the more southern and warmer latitudes, they are endemic, especially to Negro Slaves. In South Carolina they shew themselves at all seasons, but not so often in the winter, more frequently in the spring and autumn, and are most common in the summer, when the people work abroad and are alternately exposed to the scorching heat of the sun, and heavy showers, which often happen suddenly, and greatly alter the temperature of the air. Others are seized with opisthotonos, after sleeping without doors, that they may enjoy the deceitful refreshment of the cold night air when the weather is warm. He farther observes, that these diseases so rarely appear as originals in Europe, that a good description of them cannot be expected from the physicians who practice in that part of the world; nor, says he, has any thing like a full description of them been given by any ancient or modern author which I have seen. Hippocrates, indeed, takes notice of them in many places, and seems to regard them only as consequences of other diseases, or of wounds or ulcers of the nervous or tendinous parts."

Should Mr. Ward have met with a sufficient number of tetanic cases, orignating from cold and moisture only, and that in the winter, to confirm him in his opinion, that Miles's indisposition originated from that cause, those cases, without doubt, would be very interesting to the public; and as he has a sincere wish to increase to the utmost of his power, the

without supposing the dog to have been mad, to account for all (and much more than all) that followed. Whatever opinion Mr. Ward may entertain of Miles's case subsequent to Sunday, November the 30th, he most certainly must agree with me, that cold received on that day, could not produce the stiffness of the knee and thigh, which took place on the Saturday preceding, nor could it occasion the morbid irritability of the skin, which came on, on the Saturday night, nor the stiffness of his throat, and difficulty of swallowing, he experienced on the Sunday morning in eating his breakfast.

If Mr. Ward really takes the case to be tetanic, he might with much greater propriety suppose the tooth of the dog to have punctured a nerve, and that the system became affected thereby, and not by cold and moisture: From what I have seen of tetanus, I readily allow, that the puncture of a nerve by the tooth of a dog may bring it on; but it appears to me that if Miles's indisposition had originated from the puncture of a nerve only, and not from virus, the symptoms would have been different. In tetanus there are three varieties, which are classed according to the muscles that are first affected, and the posture into which the body is thrown, from the contraction of those muscles. The variety which is most common, begins with the contraction of the muscles of the back part of the neck, shoulders, jaws, and fauces, with pain of the lower end of the sternum; from the shoulders the contraction extends down the spine and bends the trunk backwards, from whence it is called opisthotonos. Then the muscles of the lower extremities become affected in the same way; and, lastly, the anterior muscles become equally contracted with the posterior, so that the whole body is rendered inflexible. When the contraction of the flexors of the head and trunk is equal to the contraction of the extensors, so as to make the body straight, and incapable of being bent any way, it is called tetanus: but should the contraction of the flexor muscles not act equally against the extensors, the spine will remain incurvated, and an arch will be formed from the heels to the head; in this

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resources of medicine and surgery, I flatter myself we shall soon be favoured with them.

To me it appears more likely, that Miles's indisposition proceeded from virus generated in the cicatrix, and that the action which generated the virus, was excited by the saliva of the dog introduced into the wound, or rather by the virus contained in the saliva.

this case it retains the name of opisthotonos. And when the flexor muscles of the head and trunk are first contracted, it is called emprosthotonos: This variety very rarely occurs; Dr. Cullen has omitted it, and added trismus, which appears to be a slight degree of opisthotonos, or, (where the disease goes on) an incipient tetanus.

In hydrophobia, the fauces, œsophagus* heart,† trachea, intercostal muscles, diaphragm, abdominal muscles, stomach, intestines, &c. are the parts affected with spasm; from whence we may conclude, that the parvagum and intercostal nerves, are the nerves principally affected, whereas in tetanus, the system becomes generally affected;

Hydrophobic patients have a disposition to bite, which, as far as I have seen, does not take place in tetanus.

The dread of water in tetanus appears to be produced in a different way to that which takes place in hydrophobia. In tetanus it originates in spasmodic affection of the muscles subservient to deglutition being increased on an attempt to swallow, which is attended with great pain, and increases the spasmodic affection of the whole system, the smallest quantity passes with the greatest difficulty into the stomach, the stricture being, from one end of the œsophagus to the other; whereas in hydrophobia, the sight of water, of a glass, or of any thing pellucid, will bring on horror immediately, with an increase of the spasmodic affection; and if the patient by dint of resolution can get any thing to pass the fauces into the œsophagus, it passes very readily into the stomach.

On whatever part of the body the wound has been inflicted, the hydrophobic virus seems to have a specific action on the parvagum and intercostal nerves; and from the nervous communication there is between the intercostal nerves, and the ophthalmic branch of the fifth pair of nerves and from thence to the retina, may arise that peculiar morbid irritability of the retina, which occasions terror to take place on the appearance of water, or any thing pellucid: as likewise, from the same nervous com-

 munication,

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* At the commencement of the disease, it appears, that the œsophagus is not much affected with spasm, (though it becomes so afterwards,) from fluids, after they have passed the fauces, having a ready transit into the stomach.

† The pulse continuing natural, like a sound healthy pulse, for sometime after the attack of the disease, shews, that the heart is not affected, till towards the close, when the pulse becomes quick, weak, and intermittent.

munication, may proceed the spasmodic affection of the fauces, &c. which becomes visibly increased on the appearance of water. Strangulation of the throat is felt, and the larynx becomes outwardly swollen.*

Why Mr. Ward should suppose the recurrent nerves to be the principal agents employed in producing the characteristic symptom of hydrophobia, a dread of water, I confess I cannot make out. The recurrent nerves are given off from the parvagum just as it enters the thorax, the right near the axillary artery, the left a little lower; they both ascend and are lost in the muscles of the larynx and pharynx. It is a known fact, in experiments on dogs, that on cutting the intercostal nerves, their eyes become dull, they lose their bright water, and have a large secretion of gum or gore, the pupils become contracted, the eye balls diminished, and the cartilaginous membrane at the internal canthus comes more over their eyes.

Near sixteen years ago I was called to James Tyler, a pauper of the parish of Weston, about 13 years of age, who a few weeks before, as he was bringing a ploughshare out of the field on his shoulder, with the palm of his left hand applied to the point, had the misfortune to kick his foot against the root of a tree, which threw him down: his left hand coming to the ground in this position, the point ran into the palm of his hand, and produced a wound, which, considering the manner in which it was inflicted, was but small. It was, however, in great pain for two or three days, during which time his mother poulticed it: she afterwards dressed it with cerate, and it appeared to be well. In consequence of which, the preceding evening to my being called to him, he applied to his master for work, and being informed he might come to work again the next morning, he returned home in high spirits. After eating his supper, he was taken suddenly, as his mother supposed, in a fit; he fell backward out of the chair on the ground; upon taking him up, his jaws were locked, his neck and shoulders stiff, and his head drawn backward, as in the first stage of opisthotonos. She still, however, supposed it was a fit, and all that was given him that night was a few drops of hartshorn in water, which was got into his mouth, through an opening in his teeth,

* This was strongly marked in Miles, for on water being brought to him on Friday night, when at the distance of two or three feet, his throat became much affected, and, as Mr. Baron, of Walkern, who was present observed, it was much enlarged.

teeth, (he having lost a front tooth) which he did not swallow. In the morning, his mother applied to the parish for medical assistance; I was sent for, and found him in this state: no part of his body would bend in the least, he had a small, fluttering pulse, with cold perspiration. He expired soon afterwards.

I was called to William Honour, of Snailsworth, on Thursday, October 29, 1801, and found him in a dying state from a tetanic affection. On inquiry, I was informed he had pricked his right leg with a spur on the Monday evening preceding, October 26; the puncture was very small, a mere speck, about four inches above the inner ankle; it appeared but little inflamed, which unfortunately led Honour and his friends to suppose no harm would ensue, and kept them from applying for assistance till it was too late.

January 23, 1802.—I was called in the evening to a blacksmith of this town, who, in shoeing an unruly horse about noon the same day, pricked his leg with a nail; it gave him great pain at the time, which was now become very violent; he complained of pain under the sternum, with some difficulty of breathing. I immediately applied a saturnine poultice to the part, with a large quantity of tincture of opium in it, and gave him a large dose of opium internally every four hours. In the morning I had the satisfaction to find the pain very much abated, and the spasmodic affection entirely removed. The same application was continued to his leg for that day, and on the following he was perfectly free from pain. In this case, had not the patient been relieved so soon by the opium, I have every reason to believe, a caustic applied to the part would have produced a happy effect. Some other tetanic cases I could mention, which have occurred in my practice, proceeding from punctures; one very lately in a respectable farmer, who in putting on his coat was pricked in the arm by a needle, which was left in the lining, (the lining having been lately mended.) As soon as he felt the needle prick him, he pulled off his coat; the needle came out at the same time: the puncture was scarcely visible. The next day, his arm was stiff and painful; the stiffness extended up his shoulder and neck, towards his jaw and cheek on that side. In this situation he sent for me; I gave him an embrocation consisting of tincture of opium, with a large quantity of camphor dissolved in it, and one fourth part of Spt. Ammon. C. directing him to embrocate the part affected with it, very often, and gave him a large

dose of opium every four hours; at the same time informing him, if he was not soon relieved, it would be necessary to apply a caustic to the part. By these means, however, I had the satisfaction to find the pain and stiffness soon removed.

Miles was bit on the 19th of November; the bite was very small, a mere scratch with a single tooth; it healed up apparently well, and continued to look well till Thursday, November 27, when it itched and appeared rather red and elevated. On Fridy the 28th, the redness increased, with slight prickling pains in it. Saturday the 29th, the prickling pains* increased; about noon his knee became stiff, which stiffness, in the course of the afternoon, extended up the inner part of the thigh; in the evening he was taken with coldness after he was in bed, very different to rigor proceeding from absorption; no increased heat followed, nor was the pulse increased in its action;† there appeared to be a morbid irritability of the skin, in consequence of which, it could not bear the stimulus of the external air, which occasioned him to cover himself over with the bed clothes, saying, he could not bear the cold wind to come to his face. Nov. 30, upon getting out of bed in the morning, the sensation of coldness increasing, he went to the fire; and in eating his breakfast, found his throat stiff, and swallowed with difficulty; soon after which he felt himself much agitated, and could not keep himself still, in consequence of which he went into the field:‡ after some time, a sudden depression of muscular strength coming on, he fell down;§ and upon getting up, was

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* Motherby says, "the disease comes on with slight pains in the wound, sometimes attended with itching, but always resembling a rheumatic pain; it extends also into the neighbouring parts, and at length from the extremities it passes into the viscera. This pain is considered as the primary invariable mark of a beginning hydrophobia."

The Edinburgh Practice of Physic, vol. 2nd, page 443, in giving the description of hydrophobia, observes, "the approach of the disease is known by the cicatrix of the wound becoming high, hard, and elevated, and by a peculiar sense of prickling in the part, pains shooting from it towards the throat."

† Edinburgh Practice of Physic, vol. 2nd, page 444. "Some complain of the coldness of the air frequently, when it is really warm."—Motherby. "In some an exquisite sensibility is induced, so that the air offends if it touches them."

‡ During the whole of his indisposition, he had a strong desire to go into the field, as if he wished to be solitary.

§ It does not appear to me he layed long on the ground. I attribute his falling

was taken with pain * and stiffness at the pit of his stomach, which extended up to his throat, as if he had been bound with strong cords. He now became more hurried, and knew not where he went, having, from his distressing feelings, a very imperfect idea of what past, till after he was secured, when his reason returned. He had a strong disposition to scratch and bite, as appears from his biting the two men, &c. Dec. 1st, 2nd, and 3rd, he continued much the same, excepting the spasmodic affection of the præcordia and fauces being gradually increased. On Friday, Dec. 5, I called upon him in the evening, and was informed by his attendants, that on the preceding day, Dec. 4, he became much agitated at the sight of water, and would not suffer it to approach his lips, saying "it terrified him," and that the spasmodic affection of his throat became much increased upon its being brought to him. During the whole of the time from the Thursday evening, the only fluid he swallowed, excepting his medicine, was a spoonful or two at most, of broth, with the greatest difficulty, and that at a great many times, though he made use of every effort. They said, he appeared very fond of apples,† and requested to have slices put into his mouth, which they frequently did, and he swallowed them with ease. I then asked for a cup of water, which was brought, and as soon as he saw it the spasmodic affection was visibly increased, so that it was noticed by those who were present; and he expressed himself much terrified.

I had him raised up upon his breech, and it was with the greatest difficulty the two men and myself, with the assistance of another person, could get the water to his mouth. Upon forcing some of it into his mouth, entreating him at the same time to swallow it, he could not; it returned again. From these appearances, I have every reason to conclude that Miles's indisposition was genuine hydrophobia, and that if he had not been relieved by the caustic

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falling to an increased nervous affection which took place at that time, and produced a sudden depression of muscular strength, which was only momentary, or continued but a short time; that he then experienced an increased nervous affection, is evident from the spasmodic affection of the præcordia, &c. which immediately followed.

* "The oppression of the præcordia is one of the primary and constant symptoms of this disease; it begins, increases, and ends only with it."—Motherby.

† Edinburgh Practice of Physic, vol. 2nd, page 444. "Sometimes they can swallow bread soaked in liquids, slices of oranges, or other fruits."

caustic that was applied that evening, he would have died; and though he had none of that spitting of saliva, which is very common towards the close of the disease, I believe it would have taken place, as there appeared to be a greater quantity of it about the fauces, on the Friday evening, than I had observed before.

I have been favoured with the following account of John Gillet, of this town, who died of hydrophobia on the 8th of May, 1794, from the bite of a mad dog about Christmas preceding, from the minutes that were taken by the gentleman who attended him. He was attacked on the 5th of May in the evening with pain about the præcordia, past a restless night, and on the following morning, May the 6th, was taken with a spasmodic affection of the fauces, and could not eat his breakfast, which was followed with a difficulty of breathing and a dread of water. These symptoms increased till he expired. Upon the appearance of a glass, or any thing pellucid, the spasmodic affection of the fauces, &c. was increased to a great degree, as well as the terror and distress he had about him. On the 6th and 7th, he took a few doses of medicine, which he swallowed with difficulty; he continued sensible till within two or three hours of his death, when delirium came on. From the account of his sister, who constantly attended him, he had not that flow of saliva which is common, and therefore not that spitting. He requested to have slices of apple given him, which he swallowed.

As the following case of Thomas Surry tends to shew the utility of excision and caustic, as preventive of hydrophobia, I have thought proper to annex it.

About the middle of July, 1799, a mad dog, in passing through Baldock, bit three or four dogs belonging to a gentleman of this town. The dogs were confined in the kennel. About ten or twelve days after, Thomas Surry, whose employment it was to look after the dogs, observing one of them look heavy and dull, with its eyes gummed up, very imprudently, (knowing the dog to have been bit by a mad dog) sponged them with water; in doing of which, the dog bit him on the second bone of the left thumb, and produced two lacerated wounds of the integuments, not sufficiently deep to injure the periostæum. Fortunately, one of the wounds was on the side next to the fore-finger; and the other, on the side opposite, so that the tendons of the flexor and extensor muscles escaped uninjured.

He

He chained the dog up, after which it refused to drink, and died much convulsed the following day, with its mouth full of frothy saliva. Soon after the bite, Surry applied to me; I removed the part by excision, and lest there should be any remains of virus, I applied a slight caustic to each wound. No other remedy was made use of. He has never had the least unpleasant symptom, and is at this time in good health.

However Mr. Ward may differ with me in many respects, I am fortunate in his agreeing with me, that the caustic had the principal share in effecting the cure, tho' at the same time, he begs leave to differ with me as to the modus operandi. He thinks the good effects of the caustic are to be attributed to its rendering the nerve or nerves situated immediately above, incapable of transmitting what he calls in hydrophobia, the hydrophobic aura to the muscles affected with spasm situated in the superior parts of the body, and recommends the caustic to be applied at the distance of from one to three or more inches above the bitten part, with the view of intercepting the aura in its passage; paying no attention to the necessity there is of putting a stop to the action that is going on in the cicatrix, which generates the aura. Allowing Mr. Ward's hypothesis to be correct, with regard to the generation of the aura in the cicatrix, and that it is transmitted up the nerves to the muscles affected with spasm, it appears to me, that though the nerves above the cicatrix are divided by the caustic in the manner he recommends, so long as the source is suffered to remain, the aura may be communicated by some collateral branch. But till the action of the nervous system is better understood, that is, the manner in which the nerves communicate their influence to the muscular fibres, whatever is offered in this way must appear to me to be mere conjecture. The action of the muscular fibres most certainly depends on the nervous energy; and when that goes on undisturbed, there is harmony throughout the whole muscular system; but when from any cause that is interrupted, a new train of action takes place, corresponding with the cause that produced it, and which may very properly be called the specific action of that cause. It appears to me, the caustic would have a much better effect applied to the cicatrix, as it will not only produce a solution of the extremities of the nerves acted upon by the virus, but decompose the virus, and arrest the morbid action altogether, by which means the nervous energy will

will be restored, and the spasmodic affection produced by such morbid action, cease, as in Miles's * case.

I am, &c.

GEORGE HICKS.

Baldock, Aug. 10, 1807.

Botanical Description of British Plants.

[Continued from Vol. xviii. page 169—174.]

7. COCHLEARIA. *C. officinalis.—C. hortensis. C. rotundifolia.*

Ang. Scurvy grass. Scrooby grass.

Gen. Desc. Pouch notched, turgid, rough, many seeded; valves bulging, blunt.

Spec. Desc. Root leaves heart-circular, entire, veined, on long leaf-st. fleshy. *Stem-l.* oblong, sitting, a little indented. *Stem* angular. *Pet.* fleshy, clear white claws, greenish. *Pouch* v. slightly notched, smooth. *Partition* double. *Seeds* rough.—*Sea shore.* BL. April, May.

Use. It has an unpleasant smell, and a warm acrid bitter taste. Its principal virtue resides in an essential oil, separable in very small quantity by distillation with water, and so ponderous as to sink in the aqueous fluid; but of great volatility, subtility, and penetration: one drop dissolved in spirit, or received on sugar, communicates to a quart of wine, or other liquor, the smell and taste of scurvy grass.—*Lewis.* Scurvy grass is *antiseptic, (See Pringle's Exp.) attenuant, aperient,* and *diuretic,* and is said to open the obstructions of the viscera and remoter glands, without heating or irritating the system: it has long been considered the most effectual of all the *antiscorbutic* plants, for which we have the testimony not only of physicians, but of the most celebrated navigators who have experienced its beneficial effects at sea. *(Anson, Linschoten, &c. &c.)*

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* We have inserted this communication of Mr. H. before its turn, on account of the calm and gentleman-like manner in which he combats the objections of his opponent. When writers have a weak cause to defend, they naturally endeavour to turn the attention of their readers from the real question at issue, by the introduction of collateral matter, by sneers, personalities, or rash assertions. M. H. on the contrary, supports his opinion, solely, by facts and quotations from the most respectable authors that he had an opportunity of consulting.—Ed.

&c.) And it is worthy of remark, that it grows most plentifully in those high latitudes in either hemisphere, where the scurvy is most obnoxious. Its sensible qualities are sufficiently powerful to confirm this opinion. In *rheumatismus vagus*, called by Sydenham scorbutic rheumatism, consisting of wandering pains of long continuance, accompanied by fever, this plant, combined with arum and wood sorrel, is highly recommended both by Sydenham and Lewis. A remarkably volatile and pungent spirit, prepared from this herb, and known by the name of *Sp. antiscorbuticus, s. mixtura simplex antiscorbutica Drawizii*, was found by Werlof to be a useful remedy in *paralysis*, and other diseases requiring an active and powerful stimulant, given in the dose of thirty drops several times a-day. *(See Cullen M. M. v. ii.* 165.) But as an antiscorbutic, neither this nor the conserve promises so much benefit as the fresh plant eaten in the way of sallad, or the expressed juice, as directed in the Pharmacopœias. — *Woodville.* There are instances of whole ships' crews having been cured of the sea-scurvy by the use of this plant: and as it abounds with acid salts, there is no doubt that it resists putrefaction. The best mode of taking it is raw, in sallad. It is also *diuretic*, and useful in dropsies. The Highlanders esteem it a good stomachic.—*Lightfoot.* Its effects as an anti-scorbutic are universally known; and it is a powerful remedy in the *pituitous asthma*, and in what Sydenham calls the scorbutic rheumatism. It possesses a considerable degree of acrimony, which seems to reside in a very subtile essential oil: a distilled water and a conserve are prepared from the leaves, and its juice is prescribed along with that of oranges, by the name of antiscorbutic juices. It may be eaten in sallads. Notwithstanding that it is a native of the Sea-coast, it is cultivated in gardens without any sensible alteration of its properties. Cows eat it; horses, sheep, and goats refuse it.—*Withering.* In Iceland it is said that sheep eat it greedily, and fatten very greatly upon it, but their flesh acquires from it a nauseous flavor. *See Bergius M. M.* 557.

8. Cochlearia. *C. coronopus.*

Ang. Swine's cress. Scurvy grass.

Gen. Desc. As above.

Spec. Desc. Leaves wing-cleft. *Stem* depressed. *Root-leaves* prostrate; *leafits* cut along the fore edge, entire on the back edge. *Pouch* kidney-heart-shaped, furrowed and ridged. *Bunches* axillary. *Bloss.* white. *Cornfields, rubbish.* Bl. June, Aug.

Use.

Use. This plant was rendered famous some years ago, by its ashes being an ingredient in Mrs. Johanna Stephens's celebrated medicine for the stone and gravel: but unfortunately it has not supported its credit.—*Lightfoot.*

9. COCHLEARIA. *C. armoracia.—C. didyma. Raphanus rusticanus.*

Ang. Horse-radish.

Gen. Desc. As above.

Spec. Desc. Root-leaves spear-shaped, scolloped; *stem-leaves* snipt. *Flowers* white. *Var. Root-l. wing-cleft.*—Sides of ditches and rivers. BL. May.

Use. The root has long been received into the Mat. Med. and affects both the taste and smell with a quick penetrating pungency, but in certain vessels it contains a sweet juice, which sometimes exudes in little drops upon the surface; its pungent matter is of a very volatile kind, being wholly dissipated by drying.—*Lewis.* The root affords one of the most acrid substances of this order, and therefore proves a powerful stimulant, whether externally or internally employed. Externally, it readily inflames the skin and proves a *rubefacient*, that may be employed with advantage in *palsy* and *rheumatism*; and its application, if long continued, produces blisters. Internally it may be used with advantage for the cure of *hoarseness* which proceeds from an interrupted secretion of mucus: one drachm of the root, fresh scraped down, is enough for four ounces of water, to be infused in a close vessel for two hours, and made into a syrup with double its weight of sugar; of this syrup a tea-spoonful or two, swallowed leisurely, or at least repeated two or three times, has been found very suddenly effectual in relieving hoarseness.—This syrup is a good substitute for the juice of the *erisimum*, when the latter is not at hand, *(Cullen M. M. ii.* 167.*)* Received into the stomach, it stimulates, and promotes digestion; and therefore is a proper condiment for animal food. A portion of its infusion taken with a large draught of warm water readily proves *emetic*, and may be either employed as such by itself, or to assist the operation of other emetics. Infused in water and taken into the stomach, it proves *stimulant* to the nervous system, and is thereby useful in *palsy*: employed in large quantity it is heating to the whole body; and hereby is often useful in *chronic rheumatism*, whether arising from scurvy or other causes. Cut down, without bruising, into very small pieces, and swallowed without chewing, in a large quantity, to the amount of a table-spoonful every morning for a month together,

together, it is said to have been found extremely useful in *arthritic* cases, *(Berg. M. M. 559)*; but these were probably of the rheumatic kind: employed in this manner, it seems, like the unbruised mustard seed, to give out in the stomach its subtle volatile parts, that stimulate without inflaming. It is also a powerful *diuretic*, and therefore useful in *dropsy*: in this manner, by promoting urine and perspiration, it has long been known as one of the most powerful antiscorbutics. *Cullen M. M. v. ii. p.* 169.—The root scraped is in common use at our tables as a condiment for fish, roast beef, &c. and it is used for many other culinary purposes. In *paralytic* and *dropsical* cases it is an useful stimulant and diuretic: a strong infusion of it excites vomiting.—An infusion of it in cold milk, makes one of the safest and best *cosmetics*.—Horses, cows, sheep, goats, and swine refuse it.—*Withering*.

Tetradynamia. Siliquosa.

10. CARDAMINE. *C. pratensis.—Nasturtium pratense.*

Ang. Common lady's smock. Cuckow-flower.

Gen. Desc. Pod long, r. edged, opening with a jerk. Valves rolling back, parallel to the membranaceous partition. Summit a knob, entire. Calyx rather open.

Spec. Desc. Leafits of root leaves roundish; of *Stem leaves*, lower egg-shaped; higher spear-shaped; upper strap-shaped; entire. *Flowers* large, purplish red.—*Meadows*. BL. May.

Use. The flowers have a place in the Materia Medica of the British Pharmacopœias upon the authority of Sir George Baker, who recommends them as an *antispasmodic* remedy. *See Med. Trans. ii.* 442. He relates five cases, where these flowers were successfully employed; and in a P. S. to the 2d edit. he says, "Since the first edition, I have seen several instances of flores cardamines in convulsive disorders." In epilepsy, however, this remedy has generally been found unsuccessful. The dose of the powdered flowers is from half a drachm to two drachms.—*Woodville*. The virtue of these flowers in hysteric and epileptic cases was first mentioned by Ray; and since then by Dr. Baker, *(Med. Tr.)* The dose is from twenty to ninety grains twice a day. In the family of the Rev. Mr. Gregor, in Cornwall, the flowering tops of this plant have been used for some generations in the cure of epileptic fits with success; whereas our medical people use only the flowers. Can this account for the different success? Sheep and goats eat it; cows are not fond of it; horses and swine refuse it. *Withering*.

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11. CARDAMINE. *C. amara.*

Ang. Bitter cresses, or lady's smock.

Gen. Desc. As above.

Spec. Desc. *Leaves* winged; *suckers* from the bosom of the leaves, round, crooked. *Leafits* angular, sitting, thinly serrated. *Stem* angular, strong, almost woody. *Bunches* terminating, lateral. *Bloss.* white; *anthers* purple. *Near water.* BL. April, May.

Use. The leaves are pungent, bitter, and aromatic, in so great a degree as to promise very considerable medical uses.—*Withering.* The young leaves, though acrid and bitterish, do not taste amiss in sallads; *Lightfoot.* And in Lancashire they are much used for that purpose.—*Caley.* Sheep eat it; cows are not fond of it.—*Linn.*

12. SISYMBRIUM. *S. nasturtium. Nasturtium aquaticum.*

Ang. Water cresses.

Gen. Desc. Pod cylindrical, opening; valves straightish, about the length of the partition. Calyx and blossom expanding.

Spec. Desc. *Pods* declining, short. *Leaves* winged; *leafits* egg-shaped. *Bloss.* white, terminating. *Springs, brooks, rivulets.* BL. June, July.

Use. The leaves have a moderately pungent taste, and a quick penetrating smell. Their antiscorbutic qualities have been long generally acknowledged by Physicians. They are also supposed to purify the blood and humours, and to open visceral obstructions; they are nearly allied to scurvy grass, but more mild and pleasant; and therefore are frequently eaten as sallad. The juice of this plant is directed, in the Pharm. with that of scurvy grass and seville oranges.—*Woodville.*

This plant is an excellent antiscorbutic and stomachic, with less acrimony than the scurvy grass: it is an ingredient in the antiscorbutic juices.—It is very universally used as an early and wholesome spring sallad.—*Withering.* It is said to be useful in jaundice and other visceral obstructions—*Lightfoot.*

(To be continued.)

CRITICAL

CRITICAL ANALYSIS
OF THE
RECENT PUBLICATIONS
ON THE
DIFFERENT BRANCHES OF PHYSIC, SURGERY, AND MEDICAL PHILOSOPHY.

Medical Reports of Cases and Experiments, with Observations, chiefly derived from Hospital Practice. To which are added, An Inquiry into the Origin of Canine Madness; and Thoughts on a Plan for its Extirpation from the British Isles. By SAMUEL ARGENT BARDSLEY, M. D. M. R. S. Edin. and M.S. London, Physician to the Manchester Infirmary, &c.

NOTHING can be more desirable than the result of inquiries instituted with judgment, conducted with accuracy, and related with fidelity. If the Author is possessed of genius sufficient so to generalize the result of his facts, as to form practical conclusions, his works must for ever remain as so much added to that general stock of knowledge, which every student or writer may turn to his own use. Such appears to us the value of Dr. Bardsley's labours. It is not our intention to compliment the Author on any superior genius, the nature of the work before us would not admit of its display. It is sufficient, that all which was promised is accomplished; and without further preface, we shall proceed to show how much advantage the science may receive by such well-digested Reports.

Chronic rheumatism, the dreary companion of these humid regions, particularly in the north, and among sedentary manufacturers, is the first complaint of which our Author treats. The frequency of its occurrence has, it seems, induced the managers of the Infirmary, in compliance with the wishes of the medical gentlemen, to furnish the house with every means of curing or alleviating the sufferings of the patients. Warm and vapour baths, and well-constructed Galvanic and electrical apparatus, are always at hand, and have been judiciously applied.

"It would," says our author, "be trifling and useless to enter into any detail of the generality of the cases which have been submitted to my care. Indeed, the history of chronic rheumatism and its treatment, are both too well known to afford an expectation of the discovery of much novelty either of principles or facts; but still I am of opinion, that the communication of the general result of the treatment (under circumstances favorable both for practice and accurate observation) of a large number of cases, will be likely to prove useful, by establishing on a firmer basis, some of the various modes which have been generally recommend-

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ed for the cure of this disease. It will be proper to premise, that I mean to include under the term chronic rheumatism, such pasnful affections of the muscular fibres, membranes, and joints, as are unattended with fever, specific virus, or peculiar derangement of the stomach and bowels; and which are seldom accompanied with external tumour or inflammation, but are very liable to shift suddenly from one part to another, and are readily propagated along the course of the membranes and muscular fibres. This definition will include chronic lumbago, sciatica, and what has been considered a distinct disease,— nodosity of the joints.

"No doubt the violence and inveteracy of chronic rheumatism are modified by the peculiar structure of the part which it affects. When the large joints with their ligaments, such as the hip joint and lumbar vertebræ become affected, the contiguous nerves often suffer from the same cause, and a long train of aggravated symptoms arise, which are with great difficulty subdued.

"The practice therefore must be regulated according to these varying circumstances; but I think myself entitled to assert, from a large induction of facts, that the rationale of the practice in the cure of every species of chronic rheumatism is, in general, simple and uniform. It consists in removing passive inflammation, and restoring the debilitated vessels and muscular fibres to their due tone and action. These ends are chiefly to be accomplished by topical applications, although internal remedies are by no means to be neglected.

"I find from an inspection of my medical case book, that two hundred and sixty-nine persons, (exclusively of a considerable number which were of a mixed and doubtful nature) labouring under severe chronic rheumatism, have been admitted under my care as in-patients of the Infirmary.

"The majority of these cases were of long standing, and had been brought from distant parts of the country to the Infirmary, as the last resource, after the usual means had failed."

In considering the comparative value of hot, tepid, or vapour bathing, the preference is decidedly given to the last, applied topically or generally. The mode of application is very accurately described, and does great justice to the liberality of the institution and attention of the practitioners. Electricity, as in most faithful reports, was found a very uncertain thongh sometimes a most powerful remedy. Galvanism, as might be expected, stands on similar ground. From topical bleeding and issues, very essential benefits were derived, insomuch that the Author particularly recommends both, as the first step in young and tolerably healthy subjects; — even in obstinate sciatica, Doctor Bardsley found great advantages from issues. His plan sometimes was almost to surround the joint with these drains, and as the discharge and irritation were insufficient, or too great, to increase or dry them up. By this judicious and close attention, he found many very obstinate cases yield to these well conducted remedies; blisters

blisters and rubifacients were found highly serviceable in those pains which affected only the fascia. Under this head two very useful *formulæ* are given, the one of a stimulating epithema, the other of a liniment; the latter was found preferable, and very much to aid the use of vapour in cases of lumbago and sciatica.

The patient will perhaps hear, with less disappointment than the practitioner, that our author found so little effect from internal remedies. He divides them into sudorifics and stimulants. From neither of these was sufficient advantage derived to encourage a persevering use of them. But there are a few other remedies, whose *modus operandi*, to use the author's expression, is not so easily ascertained, and the advantages of which we cannot help wishing, had made a more considerable object in these inquiries. We refer to Peruvian bark and arsenic. That there is some correspondence in the actions excited by these remedies, however different they may seem in most of their known properties, is very certain by the effects each of them produces on intermittent fevers. The testimonies in their favour, in the various stages of rheumatism, are so powerful, that we are utterly at a loss to account for the few experiments we meet with, in a work so professedly designed to try the effects of various remedies. This is particularly to be regretted, because in the subsequent table, we have, doubtless, a very faithful report of the cases, the remedies, and, as it would seem, the comparative value of the latter. But if more cases have been submitted to one class of remedies than to another, the inference is obvious that we may see more cures under those articles, though the comparative value may be less. Bark indeed seems to have had more opportunities of fair trial; but after the recommendation of arsenic from a quarter so highly respected * by the Author, we were prepared to expect a more frequent trial of that important remedy, especially as some success was experienced in the few cases in which it was used. The following are the Author's concluding remarks on this subject.

"I am therefore disposed to infer (as far as my limited expeperience will permit) that it is only in the protracted chronic rheumatism, where the vital powers are much diminished, and the ends of the bones, periosteum, capsules or ligaments of the joints, are likewise partially affected, that the use of arsenic is likely to prove eminently successful. However, there can be no doubt, from its indisputable efficacy in such instances as have already been published, of the propriety of instituting a number of comparative trials of this remedy, with others of an approved and established kind, in the cure of every variety of so prevalent and disstressing a malady as chronic rheumatism. It is my intention to enter, at no distant period, upon this experimental inquiry. I feel more encouraged to this undertaking, from a conviction, the

* See Mr. Jenkinson's papers on this subject in our Journal.

result of frequent and attentive observation, that arsenic is a safe and harmless remedy, when *prudently* administered."

From turpentine less relief was derived than some writers have promised, but the cod-liver-oil was sometimes useful. A single case of nodosity of the joints, as described by Doctor Haygarth, was with much assiduity at last subdued by salivation, topical bleeding, and restorative remedies.

Two cases follow, of a *singular affection of the calves of the legs*, which we shall transcribe, as we do not remember to have met with such in our reading or practice.

"John Lindsay, weaver, aged 36, was admitted an In-patient of the Infirmary, April 3, 1796, with the following complaint. He was seized three weeks ago with a violent pain in the muscles of the calf of each leg, soon after his removal from a garret into a damp cellar, where he worked. The pain was almost incessant; it was not increased by pressure; but became so severe at night, as to deprive him of sleep. The disorder was entirely confined to the parts first attacked; and was unattended with discoloration, tumor, or any external appearance of disease, except that the muscles were unusually flaccid and shrunk. He was able to walk about the wards, and conceived this exercise afforded him a temporary respite from pain. The appetite was tolerably good, and the animal functions in general but little impaired. Pulse ninety-six, and rather feeble. His countenance exhibited symptoms of uncommon anxiety aed distress."

This patient died suddenly the third day after admission.

"James Mansel, joiner, aged 30, admitted an In-patient, February 6th, 1799. He first experienced, six weeks since, a soreness in the calf of each leg, which gradually increased, so as to prevent him following his employment. The pain was most severe in the night, but never entirely quitted him. He frequently has sat up the whole night in bed, endeavouring by pressure, and handling the muscles, to relieve his pain. He has not been affected with chronic rheumatism, and is totally unable to account for the origin of this disease. The pulse and most of the natural functions are regular; but the patient's countenance bespeaks great distress, and he is much reduced by pain and want of sleep. As I could have no doubt of the similarity of the present to the preceding unfortunate case, I entered upon its treatment with great caution. The parts were enveloped with flannel, and fomented with hot water. Leeches were also applied, and diaphoretics with opium prescribed internally. This plan was attended with no benefit. The pain increased to a distressing degree in the night, and seemed indeed, to be aggravated by opiates. Blisters were then ordered to each calf of the leg; and when the vesicated parts healed, the blisters were renewed. At the same time electrical sparks were drawn from the neighbourhood of the affected muscles; and slight shocks passed through them. Bark and Guaiacum were also exhibited. By this method the pains gradually subsided;

subsided; and in three weeks, the patient was discharged entirely cured."

A case of rheumatism from taking cold under the use of mercury, was cured by pushing that remedy to its utmost extent.—We sincerely wish, under this head of chronic rheumatism from mercury, that Dr. Bardsley had made two divisions. One is, where a patient is suddenly seized with cold during the use of that remedy; the other, where pains in the joints follow the repeated use of the remedy, without any improper exposure. The first may very probably be cured in the manner proposed by our Author, but we have always seen the latter exasperated in the end, though for a time somewhat relieved, by every repetition of mercury in any form.

The next subject considered is Diabetes Mellitus. On this we meet a number of truly valuable remarks, and eight cases related with an accuracy which will only weary those who are undeserving the labour bestowed on them by the Author. A variety of incidents show the advantage of confining the patient to animal food; but after all the diligence of Dr. Bardsley, we confess ourselves at a loss to determine whether the few cures that are established, are to be imputed to any of those remedies, or to those spontaneous changes which so often occur in chronic diseases. We are not, however, less thankful for the minute accuracy of the reports, and shall certainly avail ourselves of them as often as these dreary cases occur. We ought to mention, that some useful experiments are added to our chemical knowledge of diabetic urine.

The effects of Galvanism in paralysis are next mentioned. Of these, our limits will not admit us to give more than the Author's conclusions.

"It appears," says Dr. B. "sufficiently evident from the above recital of facts, that the Galvanic stimulus is an efficacious, though not certain remedy in paralytic affections. I have long been in the practice of employing Electricity in the treatment of Paralysis, and sometimes with the happiest result. But as far as my hitherto limited experience will permit me to institute a comparison between the effects of Galvanism and Electricity in Paralysis, I am inclined to prefer the employment of the former to the latter, in all cases which appear to originate solely from a diminished state of excitement in the sensorium. I have repeatedly had recourse to Electricity for the relief of such symptoms as are described in Mc Cartney's case, where the long continuance of the loss of speech, distortion of the muscles of the face, and depravation of the intellectual faculties, denoted a high degree of collapse, or impaired energy of the brain; but never with such speedy and decisive advantage as was obtained from the application of Galvanism. Indeed the superior efficacy of the Galvanic to the Electrical influence, was in Mc Cartney's case fairly put to the test. No perceptible advantage was derived from an assiduous application of electricity, for more than a week pre-

vious to the exhibition of Galvanism. It is certainly extremely difficult to point out any characteristic signs by which we may be enabled to discriminate between those affections of the brain, which depend merely upon diminished excitement, and those which originate from organic derangement of an incurable kind. Wherever Paralysis arises from tumors compressing the substance of the brain, or from a diseased alteration in its mass and structure, or from exstravastion of a fluid in such a state or degree, as will not admit of its absorption; it will be readily admitted, that no benefit is to be expected from the employment of Galvanism, or perhaps any other remedy yet discovered. But what are the diagnostic signs of such incurable conditions of the brain? I confess myself unable to point them out with any certainty. I may venture to state, however, from a few observations on this subject, that such affections of the brain are for the most part insiduous in their approach, gradual in their progress, and when once formed, they admit of little, if any, exacerbation or remission in their symptoms. But although no benefit can be rationally expected to accrue from Galvanism in such a calamitous state of the brain as has been just described; I am not aware that a *cautious* and *prudent* use of the remedy will prove certainly injurious. I am, however, enabled to affirm, that in those cases of Paralysis, where sense, motion, and intelligence, are greatly impaired, it is always imprudent to push the experiment beyond certain limits. If the patient's rest, appetite, and feeble sense of enjoyment, be evidently disturbed; if tremors, convulsive sobs, and tears, with other signs of increased irritability, should either immediately, or soon after the operation, supervene, it will be proper at least to suspend, if not altogether discontinue, the use of Galvanism. The necessity for caution is exemplified in the following brief statement of a fact which lately occurred. A young lady (the daughter of a late eminent physician) had laboured under a general Paralytic affliction (which most probably originated from an attack of Hydrocephalus internus, when an infant) for many years; and which had progressively deprived her of all muscular exertion and the power of speaking louder than a whisper. She had some years before, upon her father's consulting me on the subject, submitted to a seven weeks trial of electricity, under every form and degree which I considered most likely to be beneficial. No advantage was derived from the experiment. It was then my opinion, that the complaint depended on some internal organic derangement of the sensorium. She was, however, strongly recommended last year, when at Bath, to make a trial of Galvanism. The experiment, I have every reason to believe, was skilfully conducted. Its effects, however, were not only unproductive of any advantage, but very distressing to the patient's feelings, and injurious, for a time, to her general state of health.

"It may not be improper to remark here, by way of caution, that

that I have been more than once disappointed in communicating the Galvanic influence to a patient, notwithstanding the skin had been duly prepared by moistening it with a proper mixture of nitric acid and water, and covering it with metallic leaf, and although other patients, treated in a similar manner, had experienced at the same time, the Galvanic sensation in its due force. This failure I found to arise from some peculiar condition of the skin (perhaps increased thickness?) for upon the application of the wire conductors to another part of the same limb, which had been in like manner prepared, as in the first attempt, I was able to succeed with the greatest ease.

" *Result of the Effects of Galvanism in Paralysis, &c.*

" Five Cases of general Paralysis, of which,

Cured - - - - -	3
Relieved - - - -	1
Not Relieved - - -	1

" *Five Cases of Partial Paralysis.*

Ischuria - - -	Cured - - -	1
Hemiplegia - -	Cured - - -	1
Hemiplegia - -	Not Relieved -	1
Paraplegia - -	Cured - - -	1
Paraplegia - -	Much Relieved	1

Hip Joint Case, One Cured."

Dr. B. next considers the effects of oxyd of bismuth in pyrosis and other diseases of the stomach. Dr. Marcett's observations are amply confirmed. It is further remarked, that valuable as this remedy is found in the above mentioned complaints, it is less to be depended upon in the general class of *neuroses* than the other metallic oxyds.

The Work concludes with some valuable remarks on Rabies, and the means of exterminating this dreadful malady from the British isles. Whether this is practicable we pretend not to determine, but are ready to give every credit to our Author for his good intentions, and to entreat that he may meet with that assistance from us all, to which his labours entitle him, and which the importance of the subject demands.

Practical Observations on the Uterine Hæmorrhage: with Remarks on the Management of the Placenta. By JOHN BURNS, Lecturer on Midwifery, and Member of the Faculty of Physicians and Surgeons in Glasgow.

IT is somewhat surprising we should have been so long without any tract devoted exclusively to this important subject. Mr. Burns begins with a short description of the connection between the uterus and ovum, and the causes of hæmorrhage during gestation; the manner in which Nature endeavours to relieve them; the difficul-

culty which attends that attempt; and the necessary consequence of frequent returns of the complaint.

The author next enters, with more precision, into the causes of uterine hæmorrhage under gestation. These he considers, as imputable to external violence, producing a separation of part of the ovum; to fatigue, or over exertion; which, by their effects on the parts, or on the general circulation, may produce such an effect; straining the abdominal muscles from any cause; a preternatural degree of action in the vessels going to the placenta, or decidua arising from general plethora, or some peculiarity in the state of the parts; a want of correspondence between the action of the uterus and ovum; spasmodic action of the os uteri; any cause which may interrupt the progress of gestation, or the formation of that jelly which ought to be secreted before the os uteri — in some cases, the author has observed a change in the structure of the placenta near the separated part; lastly, the insertion of the placenta over the os uteri. This last he considers with other practitioners, as the most common of all the causes producing uterine hæmorrhage during gestation.

The next object attended to, is the *effect* of Uterine Hæmorrhage, which leads to a consideration of the prognosis.

As the reputation of the practitioner depends often more on this last, than even on his success, we shall transcribe this part of the work, as a specimen of the Author's style.

"We may lay it down as a general observation, that few cases of profuse hæmorrhage, occurring in an advanced stage of gestation, can be cured without delivery or the expulsion of the child. For when the discharge is copious or obstinate, the placenta is generally separated, sometimes to a very considerable extent, and a re-union, without which the woman can never be secure against another attack, can rarely be expected. If the placenta present, the hæmorrhage, although suspended, will yet to a certainty return, and few will survive if the child be not delivered.

"But in those cases where only a portion of the decidua, or a little bit of the margin of the placenta, has been detached, and the communicating vessels opened, either by a state of over action in the vascular system, or by too much blood in the vessels, or by some mechanical exertion; then, if proper care be taken, the hemorrhage may be completely and permanently checked; or if it should return, it may be kept so much under, or may consist so much of watery discharge from the glands about the os uteri, as neither to interfere with gestation, nor injure the constitution; yet it is to be recollected, that even these cases of flooding may sometimes proceed to a dangerous degree, requiring very active and decided means to be used; and in no case can the patient be considered as safe, unless the utmost care and attention be paid to her conduct.

"It would thus appear, that some hemorrhages almost inevitably end either in the delivery of the child, or the death of the parent;

parent; whilst others may be checked or moderated without an operation. A precise diagnostic line, liable to no exceptions, cannot be drawn betwixt those cases; and, therefore, whilst we believe that rapid and profuse hemorrhagies, which indicate the rupture of large vessels, can seldom be permanently checked, we still, provided the placenta do not present, are not altogether without hopes of that termination which is more desirable for the mother, and safer for the child, than premature delivery. In slighter cases, our hope is joined with some degree of confidence.

" A second attack, especially if it follow soon after the first, and from a slight cause, greatly diminishes the hope of carrying the woman to a happy conclusion without manual interference.

" In forming our opinion respecting the immediate danger of the patient, we must consider her habit of body, and the previous state of her constitution. We must attend to the state of the pulse, connecting that in our mind with the quantity and rapidity of the discharge.

" A feeble pulse, with a hæmorrhage, moderate in regard to quantity and velocity, will, if the patient have been previously in good health, generally be found to depend on some cause, the continuance of which is only temporary.

" But when the weakness of the pulse proceeds from profuse or repeated hæmorrhage, then, although it may sometimes be rendered still more feeble by oppression, or feeling of sinking at the stomach; yet, when this is relieved, it does not become firm. It is easily compressed, and easily stopped by motion; or, sometimes, even by raising the head.

" If the paroxysm is to prove fatal, the debility increases—the pulse flutters—the whole body becomes cold and clammy—the breathing is performed with a sigh—and syncope closes the scene.

" If irritation be conjoined with hæmorrhage, then the pulse is sharper, and, although death be near, it is felt more distinctly than when irritation is absent.

" The termination in this case is often more sudden than a person, unacquainted with the effect of pain or irritation on the pulse, would suppose. For when the pulsation is distinct, and even apparently somewhat firm, a slight increase of the discharge, or sometimes an exertion without discharge, speedily stops it, the heat departs, and the patient never gets the better of the attack.

" We must likewise remember, that a discharge, which takes place gradually, can be better sustained than a smaller quantity, which flows more rapidly. For the vessels in the former case come to be accustomed to the change, and are able more easily to accommodate themselves to the decreased quantity. But when blood is lost rapidly, then very speedy and universal contraction is required in the vascular system, in order that it may adjust itself to its contents, and this is always a debilitating process. The difference too betwixt the former and the present condition of the body, is rapidly produced, and has the same bad effect as if we

were instantly to put a free liver upon a very low and abstemious diet.

" In all cases of flooding, we find that during the paroxysm, the pulse flags, and the person becomes faint. Complete syncope may even take place, but this in many cases is more dependent on sickness or oppression at the stomach, than on direct loss of blood. In delicate and irritable habits, the number of fainting fits may be great, but unless the patient be much exhausted, we generally find that the pulse returns, and the strength recruits. The prognosis here must depend greatly on the quantity and velocity of the discharge; for it may happen, that the first attack of hæmorrhage may produce a syncope, from which the patient is never to recover."

Having thus explained the causes, effects, general symptoms, and probable event of Uterine Hæmorrhage, Mr. Burns enters on the Treatment. In this, he is as minute as the nature of the subject requires, but not more diffuse. To enumerate therefore, all contingencies which the student is taught to expect, and all the remedies recommended under each, would be to transcribe the whole. Such means of relief, as depend on manual assistance, are reserved for the succeeding chapter, " On Delivery." This comprehends all the circumstances under which Delivery should be prematurely attempted; after which, the author describes the effects of the presentation of the placenta at the period of parturition, and the manner in which the practitioner should conduct himself under such an event.

A chapter follows of Hæmorrhage during Labour, and another on the same event after Delivery, and after the expulsion of the Placenta; and lastly, on the Management of the Placenta. These concluding chapters necessarily embrace every part of midwifery which is any way connected with the subject. All the directions given by the author are blended with those cautions, which cannot be too often impressed on young practitioners, and with that decision, which the nature of the case will always demand.

This work, though containing but little novelty, may be considered as a very valuable practical epitome of all that is required of the practitioner, for the relief of uterine hæmorrhage during gestation or parturition.

An Account of the Practice of one of the Physicians of the Westminster General Dispensary, and of the Western Dispensary, from the 20th of July, to the 20th of August, 1807.

ACUTE DISEASES.			
Synochus - - -	8	Inflammatory Sore Throat	3
Apthous Sore Throat	2	Measles - - -	2
		Cholera - - -	3
		Catarrh	

Catarrh	2	Dyspepsia	7
Acute Rheumatism	4	Gastrodynia	5
Erysipelas	1	Enterodynia	3
Tertian	1	Colica Pictonum	1
Acute Diseases of Infants	11	Bilious Vomiting	3
CHRONIC DISEASES.		Diarrhœa	10
Asthenia	18	Jaundice	1
Pulmonary Consumption	4	Colic	2
Scrophula	2	Ascites and Anasarca	4
Tabes Mesenterica	1	Cephalalgia and Vertigo	3
Cough and Dyspnœa	7	Hydrocephalus Internus	1
Asthma	2	Worms	3
Hæmoptoe	3	Cutaneous Diseases	6
Pleurodyne	3	Amenorrhœa	2
Chronic Rheumatism	9	Menorrhagia	3
Lumbago	6	Dismenorrhœa	2
Paralysis	2	Leucorrhæa	4

Since the last Report I have not seen a single case of typhus; synochus has frequently appeared, without any thing peculiar or extraordinary in its symptoms. The convalescence of one case, indeed, is attended with a remarkable and distressing prostration of strength; the faculties of the mind are impaired, and a general stupor prevails, more than the severity of the complaint gave reason to apprehend. The bowels were considerably purged during the continuance of the fever; several evacuations taking place daily without the operation of medicine. Change of air would probably restore the strength of the patient sooner than it could be effected by any other means. When, after acute diseases, the subsequent debility continues long, and resists the usual tonic remedies, it is to be feared that some other disease will soon destroy the broken constitution, and a journey into the country should be urged, as a likely means of promoting cheerfulness, by affording a succession of pleasing ideas, and at the same time effecting a gradual restoration of strength.

In acute diseases, from the urgency of the danger, and the dread of immediate dissolution, the patient is generally pretty tractable, and willing to submit to whatever his physician proposes for his relief; there is scarcely time to question his authority, or to doubt his practice. But in chronic cases we have often much difficulty to encounter. Every one dabbles a little in medicine, and knows something of his own case; we are called upon to account for every symptom, and to give a plausible theory of the disease; while, if we do not promise more for the effect of our medicines

dicines than experience authorises, it is very likely they will not be taken. Many physicians, standing on the vantage ground of age, skill, and reputation, boldly refuse to answer all interrogation; but this is not practicable in all cases, nor with all men; and I believe it will generally be found most advantageous, where the patient is rational, to explain what we are doing; and when he cannot hear reason, to tell him something which he will not comprehend. In complaints of long continuance it requires great skill in a medical practitioner not to be dismissed before his remedies have had any beneficial effect; the patient becomes weary, and doubts the success of the proposed plan; another practitioner is called in, and for similar reasons is equally unsuccessful; the complaint in the mean time goes on, begins to be regarded as incurable, and the regular professors of medicine to be contemned. A quack is next resorted to, and now and then has the luck to restore what has been erroneously considered a lost case; and thus the most impudent and imposing traffic in the credulity, misfortune, and ignorance of mankind, that can disgrace humanity, is affirmed and upheld.

The mischief resulting from the general diffusion of empirical nostrums through the country, is only surpassed by the pestiferous effects of the numerous gin-shops which disgrace every street in London. It is a severe libel on human nature, that government derives an immense revenue from three sources, of which the fatal and morbid influence upon society is incalculable; I need scarcely name them; patent medicine warehouses, gin-shops, and lottery-offices obtrude themselves on our notice, and excite the commiseration of the philanthropist in every part of the metropolis, whilst we are sending missionaries to enlighten the uncivilized Indians, who are much more happy and innocent than a great part of our population at home. When a man has swallowed a potent quack medicine, though he may say to any officious expostulator:

> " Quid enim? concurritur: horæ
> " Momento aut cita mors venit, aut victoria læta."

It were better to remember,

> " Improvisa lethi
> " Vis rapuit rapiet que gentes."

SAMUEL FOTHERGILL.

Southampton Street, Strand, August 25, 1807.

Account of Diseases in an Eastern District of London, from the 20th of July to the 20th of August 1807.

ACUTE DISEASES.			
Typhus	3	Dysuria	6
Dysenteria	2	Hysteria	5
Cholera	15	Herpes	4
Pneumonia	1	Scrophula	3
Rheumatismus Acutus	2	Cephalalgia	4
		Rheumatismus Chronicus	10
CHRONIC DISEASES.		PUERPERAL DISEASES.	
Tussis	4	Menorrhagia Lochialis	5
Dyspnœa	7	Ephemera	5
Hæmoptysis	3	Diarrhœa	6
Phthisis Pulmonalis	4	Mastodynia	5
Hepatitis Chronica	3	INFANTILE DISEASES.	
Diarrhœa	7	Rubeola	1
Gastrodynia	5	Diarrhœa	7
Menorrhagia	4	Aphthæ	3
Chlorosis	3	Ophthalmia Purulenta	2

Amongst the diseases which at present engage the attention of practitioners, cholera makes a distinguished figure. This has been divided by Nosologists into different species, in all of which a very large discharge of bilious and acrid matter from the bowels constitutes a principal symptom. These discharges frequently go on at the same time; and the quantity evacuated is truly surprising to those who are not acquainted with the nature of the disease. In some cases an uneasiness in the stomach and bowels is felt for some days previous to the principal attack; but in others this approaches more suddenly. Nausea accompanied by pain in the stomach, distention of the belly, and a considerable degree of flatulence, are observed. To these symptoms succeed violent vomiting and purging. What is first thrown up consists chiefly of food that has remained in the stomach in an undigested state; and the patient feeling some immediate relief, supposes that he shall soon be perfectly well; but this is soon succeeded by a discharge of a very different appearance: bilious matter mixt with mucus is thrown up, and there are very considerable evacuations from the intestines of a similar appearance. Spasmodic contractions of the muscles of the legs generally attend this complaint. In one of the instances referred to in the list, these were transferred to the abdomen, and formed a very troublesome symptom.

The general indications of cure in this disease are, plentiful dilution of the offensive matter in the stomach and intestines, and the occasional use of opiates to correct violent action and alleviate pain. INTELLI-

INTELLIGENCE.

It is well known that the exhalations from certain vegetables vitiate the air in which they are placed; and it seems probable that most of those which emit a strong odour, do this in a greater or less degree. Our Paris correspondent has favoured us with the following observations by M. Sage, Member of the Institute, on the soporific effects produced by the exhalations of saffron.

This plant, M. Sage informs us, is cultivated in great abundance in Gatinais, one of the former provinces of France, and is harvested during the Autumn.

The farmers, after carefully collecting the flowers of the saffron, carry them home, and spread them on large linen sheets in their dwelling houses. In the evening the females are employed in picking off the pistils, the odour of which, though not equally strong as after they are compleatly dried, nevertheless produces the most alarming effects on the nervous system. The disease, thus induced, is vernacularly termed the Soporific Fever; the inhabitants are never affected with it at any other period of the year except during the saffron harvest, which usually lasts one month.

The narcotic effect of this odorous emanation greatly resembles that produced by opium; it is capable of occasioning death, especially in feeble patients and children. Like the affection induced by opium, it is best remedied by the employment of vinegar, of which the following fact affords a confirmation.

Madame G———, being in the Gatinais, saw a child laid out for dead by the parents, but who was in reality only affected with the lethargic disease produced by saffron flowers; she had the good fortune to recal the infant to life by means of vinegar, gooseberry water, and the employment of friction, with flannel dipped in vinegar.

M. Sage himself once succeeded in relieving a person from a similar comatose state, who had been affected by remaining a long time in a garden abounding with poppies.

Through the medium of the same correspondent, we learn that the heat has been extremely intense during the summer, at Paris, and that tempestuous weather has occurred less frequently than usual. Among the complaints at present most prevalent in that capital are catarrhal affections; which, however, have been in general successfully combated by mild diluents, slightly acidulated, by the occasional application of vesicatories, and leeches to the pained parts of the chest, and similar means. Among young females, chlorotic affections, probably produced by the extreme heat of the season, have been also unusually frequent. In the treatment of this disease, medicine, instead of proving productive of benefit, has

has appeared rather to aggravate the complaint. The exercise of dancing, in a moderate degree, while care is at the same time taken not suddenly to check perspiration; the use of tepid and acescent drinks, &c. have been found more useful in alleviating it, than the prescriptions of the apothecary.

Owing to the state of the atmosphere, Phthisis Pulmonalis appears likewise to have proved very destructive. Besides the use of lowered air, the employment of fumigations with fir shoots, on the supposition that the tuberculous affection did not extend beyond the bronchiæ, has greatly tended to relieve the patients, and retard the progress of the malady. As this disease, our correspondent adds, has of late greatly increased in frequency, it will, he hopes, attract the particular attention of the Faculty.

It is known that various substances diffuse, under different circumstances, a phosphoric light more or less vivid and permanent. Such are the fluate of lime, when thrown in powder on heated bodies. The Bologna phosphorus, after being exposed to light, emits it again in the dark. Some sulphurets of zinc, when strongly rubbed with hard bodies, rotten wood, certain fishes, and other animal substances, when in a state of putrescence, display also similar phenomena. The physical and mathematical class of the French National Institute has proposed as the subject of a prize, to be adjudged on the first Monday of January, 1809, the following question. "To ascertain, by experiments, the relations which subsist between the different modes of phosphorescence, and the cause to which each species is owing, excluding from examination the phenomena of this kind which are observed in living animals." The prize is a gold medal of the value of three thousand francs. The memoirs must be transmitted to the secretary of the Institute, previous to the first of October, 1808.

Some experiments have been made at Hudson's Bay with frozen quicksilver. It has been reduced to sheets as thin as paper, by beating it upon an anvil with a hammer, at the same temperature as the quicksilver. On plunging a mass of frozen quicksilver into a glass of warm water, the latter was immediately frozen, the glass was shivered into a thousand pieces, and the quicksilver became fluid again.

There are upwards of two hundred warm springs in Portugal; and it deserves to be particularly remarked, that the greater number, and the hottest of them, issue from Granite.

University of Glasgow.

The Medical Lectures in the University of Glasgow will begin on Tuesday the 3d of November, at the following hours.

Dietetics, Materia Medica, and Pharmacy, by Dr. Millar, at ten o'clock in the forenoon. — Midwifery, by Mr. Towers, at eleven. — Theory and Practice of Physic, by Dr. Freer, at twelve. — Anatomy and Surgery, by Dr. Jeffray, at two o'clock afternoon. —Chemistry,

—Chemistry and Chemical Pharmacy, by Dr. Cleghorn, at seven.—Clinical Lectures on the cases of patients in the Royal Infirmary, on Thursday evening the 12th of November, at six.—— Dr. Brown will commence his Lectures on Botany about the beginning of May next.

The Lectures at the adjoining HOSPITALS of St. THOMAS and GUY, will commence as follows: ST. THOMAS'S.—Anatomy and the Operations of Surgery, by Mr. Cline and Mr. Astley Cooper, Thursday, October 1st, at two o'clock; Principles and Practice of Surgery, by Mr. Astley Cooper, Monday, October 5th, at eight in the evening.

GUY'S.—Practice of Medicine, by Dr. Babington and Dr. Curry, Friday, October 2d, at ten o'clock; Chemistry, by Dr. Babington, Dr. Marcet, and Mr. Allen, Saturday, October 3d, at ten o'clock; Midwifery, and Diseases peculiar to Women and Children, by Dr. Haighton, Monday, October 5th, at eight in the morning; Pathology, Therapeutics, and Materia Medica, by Dr. Curry and Dr. Cholmeby, Tuesday, October 6th, at eight in the evening; Physiology, or Laws of the Animal Economy, by Dr. Haighton, Wednesday, October 7th, at seven in the evening; Experimental Philosophy, by Mr. Allen, to begin in November; Clinical Lectures on Select Medical Cases, by Dr. Babington, Dr. Curry, and Dr. Marcet.—N. B. These several Lectures are so arranged as not to interfere in the hours of attendance; and the whole is calculated to form a complete Course of Medical and Surgical Instruction.—Terms and other particulars to be learnt from Mr. Stocker, Apothecary to Guy's Hospital, who is also employed to enter gentlemen as Pupils to such of the Lectures as are delivered at Guy's.

THEATRE, LONDON HOSPITAL.—The Autumnal Course of Lectures delivered at this Hospital, will commence on the 1st of October. Theory and Practice of Physic, by Dr. COOKE; Chemistry, by Dr. Yelloly; Theory and Practice of Midwifery, by Dr. Dennison; Occasional Clinical Lectures on Surgical Cases by Sir W. Blizard and Mr. Thomas Blizard; Anatomy, Physiology, and Operations of Surgery, by Mr. Headington and Mr. Frampton; Anatomical Demonstrations and Dissections, by Mr. Frampton; Principles of Surgery, by Mr. Headington; Materia Medica, by Dr. Buxton. Further Particulars may be known by applying to Mr. Price, Apothecary, at the Hospital.

THEATRE OF ANATOMY, Blenheim Street, Great Marlborough Street.—Mr. BROOKES, will commence his Autumnal Course of Lectures on Anatomy, Physiology, and Surgery, on Thursday the 1st of October, at two o'clock. The Dissecting Rooms will remain open from seven in the morning till two in the afternoon; where Mr. Brookes attends.

THEATRE

THEATRE OF ANATOMY, GREAT WINDMILL STREET.—Mr. WILSON will begin the Winter Course of his Lectures on Anatomy, Physiology, Pathology, and Surgery, on Thursday, October the 1st. at two o'clock in the afternoon, as usual.——The Rooms for Practical Anatomy will also be opened in the morning of the 10th of October, and demonstrations will be given and continued during the Winter, at one o'clock, by Mr. Wilson or Mr. Brodie.——Plans of the Course of Lectures may be had in Great Windmill Street.

Mr. ACCUM's Lectures on Operative Chemistry and Mineralogy, exhibiting a Summary Exposition of the Processes of Experimental Chemistry, and general practical rules to be observed in the performance of Chemical Experiments; with a Summary View of Analytical Mineralogy, exemplifying the Practical Analysis of Minerals, will commence on October the 1st.——For farther particulars, a prospectus may be had at the Laboratory, No. 11, Old Compton Street, Street, Soho.

Dr. BADHAM's Lectures on the Practice of Physic, Chemistry, and the Materia Medica, will be commenced on Thursday morning, 7th of October, at eight o'clock, and continued at the same hour.

Dr. BRADLEY will commence his Autumnal Course of Lectures on the Theory and Practice of Medicine, on Monday the 5th of October.

Mr. A. CARLISLE, Surgeon to the Westminster Hospital, will begin his Course of Lectures on the Art and Practice of Surgery, in all its Branches, on Tuesday, October 6, at eight o'clock, P. M. at his house in Soho Square.

Mr. CARPUE will commence his Lectures on Anatomy and Surgery, on Thursday, October the 1st. The Dissecting Room is open from seven in the morning till five o'clock in the afternoon. —Particulars may be known by applying to Mr. Carpue, No. 50, Dean Street, Soho.

Dr. CLARKE and Mr. CLARKE will begin their Lectures on Midwifery and the Diseases of Women and Children, on Monday, October the 5th. The lectures are read at the house of Mr. Clarke, No. 10, Upper John Street, Golden Square, every Morning, at a quarter past ten o'clock till a quarter past eleven, for the convenience of students attending the Hospitals.—For particulars apply to Dr. Clarke, No. 1, New Burlington Street; and to Mr. Clarke, No. 10, Upper John Street, Golden Square.

Drs. DENNISON and SQUIRE will, on Monday, the 5th of October, begin a Course of Lectures on the Theory and Practice of Midwifery, and the Diseases of Women and Children.

Dr. MARSHALL intends to deliver a Course of Lectures, in October next, on Midwifery, and the Diseases of Women and Children, at his house, No. 17, Holles Street.

Mr.

Mr. MOOR, Surgeon Dentist to Her Royal Highness the Duchess of York, will commence a Course of Lectures on the Structure and Diseases of the Teeth, on the 4th of November, in which will be explained the compleat practice of the Dentist. Further particulars may be known at his house, No. 6, Palsgrave Place, Temple.

Mr. THOMAS's Lectures on the Theory and Principles of Surgery, will commence as usual early in October. Particulars may be known at his house, Leicester-place; and at the Anatomical Theatre, Windmill Street.

The Report of the College of Physicians has been printed in a cheap form for general circulation, and published by Mr. HARDING in St. James's Street.

Dr. JOHN REID's Treatise on Consumption has lately been translated into German by Dr. Hilmershausen, of Neustadt in the Duchy of Saxe Coburg.

Dr. BEDDOES has a work ready for publication, entitled Researches, Anatomical and Practical, on Fever, as connected with Inflammation.

The Second Part of the Medical Observer, containing an Impartial Account of Quack Medicines, copies of the specifications entered at the Patent Office; with much interesting information relative to the practice of Quacks; and Observations on the present state of the Medical Profession in Great Britain, &c. will be published on the first of October next.

Dr. HALLIDAY of Halesworth, has in the press, Observations on the Causes and Consequences of Emphyzema, which will make its appearance early in September.

Mr. TURNBULL, Member of the Royal College of Surgeons, London, is preparing a work, to be comprised in three volumes octavo, entitled, "A System of British and French Surgery, Medical and Operative, &c. &c. with plates and original delineations."

Dr. CLUTTERBUCK (author of the Treatise on Fevers) is unanimously appointed Physician to the General Dispensary, Aldersgate Street.

To CORRESPONDENTS.

We have received Mr. Golding's Answer to Mr. Ring; but as Mr. G. as well as the Correspondent he quotes, have totally mistaken the question, we cannot publish it. They suppose the only doubt to be, whether Eliza had the small-pox; a doubt which, we apprehend, no one entertains. Mr. Ring, on the contrary, asserts that she never was *regularly vaccinated;* an opinion in which, we think, all intelligent Practitioners will agree.

The Observations of Tyro are too sarcastic for publication.

Mr. Marson's Answer to Mr. Ring is received, but we consider that account as closed for the present.