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To Dr. BRADLEY.

DEAR SIR,

HAVE the goodness to permit an old friend to say a few words in his own defence, when attacked on all sides, and in all manner of ways. It is not many months since I was taken severely to task for being candid to Mr. Goldson. This I bore with becoming patience, not thinking it a crime to be civil. But only a month ago I have been accused of being uncivil to this same gentleman, or, what is still worse, of mistaking his meaning. Now, having for many years past been careful not to quarrel with any one, I little expected that those who should be the guardians of medical literature would have taken pains to foment discord.

"We apprehend," says a writer in one of the medical periodical publications, "Dr. A. has mistaken Mr. Goldson, when he makes him *say, in an unqualified manner*, that vaccination in the hand may be relied on, though it cannot in the arm. On turning to the passage in Mr. G's book, we find only that he conjectures the pustule in the hand, from having a closer resemblance to the casual vaccine pustule, may possibly be more effectual in preventing small-pox."

It is impossible not to approve of the Reviewer's industry in turning to the passage in Mr. Goldson's book, but it seems to have required much more industry to overlook the passage in my book, which, it is to be presumed, was open before him whilst *under review*. Perhaps, however, if he overlooked it, on that occasion, he may meet with it here, as his work makes occasional reference to the Medical and Physical Journal. My words are as follow:

"However, the most respectable among those who maintain this opinion, is now *led to believe* that if a person is vaccinated in the hand, he may be secure from the small-pox for life; but if in the arm, only for a time."

Mr. Goldson's words are, "*I was induced to infer.*"
My words, "*led to believe.*"
The Reviewer's words, "*to say in an unqualified manner.*"

Having thus renewed my correspondence with you, let me trouble you with a few notes, as they were collected in the Small-pox Hospital. Though I am preparing them in a better form, yet the important questions now agitated, render it the duty of every one who has facts to produce, to be early in communicating them.

I shall begin with reminding you of an aphorism delivered by the first person who taught us to discriminate these subjects with accuracy, viz. that *every morbid poison has its distinguishing character;* and that however this may be modified on the first appearance of the local action, by any peculiarity of the constitution in the subject or atmosphere, still, in the progress of the disease, these adventitious circumstances will cease, and the true character of the poison show itself.

That keeping these objects in view, our business should be, in all chronic cases, to rest till we can ascertain the disease by its true character; and in acute cases to regulate our practice by the symptoms which show themselves. When these subside spontaneously, or by our remedies, the disease will assume its true character, and should be treated accordingly.

I have been led to these reflections by two most remarkable cases, which have lately occurred at the Small-pox Hospital. The first was a young woman about twenty-five years old, who had gone through small pox twenty years before, and was severely enough marked with it. She applied without any introduction to the hospital, on accoun of fever, and an almost universal eruption, which she conceived to be small-pox. It was soon discovered that the progress of the disease was different from that of small-pox; but as no urgent symptoms occurred, she remained more than a week without the use of any powerful remedy. By degrees it was seen, that though some of the eruptions appeared purulent, yet the early ones became dry, the cuticle falling off, and discovering a circumscribed red spot underneath it. The fever assumed the true hectic form, and the spots, particularly about the joint of the humerus and fore arm, assumed the true venereal character. A regular course of mercurial friction removed all the symptoms, and the patient is now convalescent.

The second case was of a woman who twenty years before had been inoculated by a gentleman celebrated for his

his success in that branch of the profession. Her situation was soon reported to the hospital, and the above gentleman, on the following day, applying to us for vaccine matter, was informed of the event. He was so good as to visit his patient, and examine his register. It does not appear that he made any objection to the reality of the disease, but discovered by his register, that the subject had been inoculated by his assistant, which rendered it uncertain whether the progress of the disease had been regular. The symptoms not being very urgent, and the progress of the eruption somewhat irregular, no active remedies were applied. In a very few days the true venereal character showed itself much more strikingly than in the first instance; the eruptions being fewer, somewhat larger, in solitary clusters, and situated in those parts where there was most reason to expect them.

It is most likely that at another time these cases would not have found their way to St. Pancras; but the occasional occurrence of small-pox after cow-pox, has brought forward a number of second attacks of small-pox, which tho' well authenticated, might otherwise have been concealed, or left unrecorded. This of course has taught the public to expect still more. Happily for mankind, the instances are rare after either vaccination or inoculation; but it is truly surprising that men, who are daily witnessing the various deviations from the usual course of Nature in organic structure, in the common actions of health and disease, should refuse their assent to facts so well established, and so much within the line of probability. I know it has been said, that these accounts of second small-pox have never been mentioned till the occurrence of the disease after vaccination; but this is so far from the truth, that Diemerbrock, who wrote in the middle of the seventeenth century, very circumstantially relates the history of four persons, (brothers and sisters) who had all of them the small-pox severely a second time. In this account he could not well be mistaken, as the family were inmates of his own house. His words are, "Nam erant nostri domestici, in quos singulis fere horis occuli convertebantur." His son, in his annotations on the cases, speaks of them as rare instances, but without expressing the smallest doubt of their reality.

Forestus, who wrote much earlier, admits the possibility of a second recurrence.

However, instances in our own days are more to the purpose, because we have it in our power to examine their validity. These are now become so numerous, and well

authenticated, that I was surprised in your last number to find the case of Miss Price, which I had before related, brought as a fresh communication. However, the little addition at the close, which afforded the opportunity of a second time introducing *Mr. Travers's firebrands*, explained the cause of the insertion of the case.

If I had not had the pleasure of Mr. Travers's acquaintance more than thirty years, if I had not for that time witnessed his good intentions and the strength of his understanding, I should still have required no proofs that he was actuated by the purest motives in his eloquent Philippic. Mr. Travers could not be aware that the small-pox is so constantly epidemic in London, that no person who has not had the disease thinks himself safe in passing a night in the metropolis. That this has been the case not only since inoculation has been introduced; but that as far back as we can trace any correct accounts, pest houses have been erected in the neighbourhood of other cities and large towns, to which those who were seized with small-pox were sent, but that even a separate hospital for small-pox in London, is of modern date. Nor was it to be expected that Mr. Travers should have made himself acquainted with the writings of the great master in this disease, of whom Boerhaave says, that every physician should read his Treatise on the Small-pox thirteen times over. Every medical man will know I am speaking of Sydenham, and also that Sydenham died before inoculation was known in Europe. Yet we find inoculation was not necessary to spread the disease *universally* through London, under certain constitutions of the atmosphere. His words, as translated by Dr. Swan, are,

"The small-pox in those years it is epidemic, when it is mild and regular, usually begins about the vernal equinox; but in those years when it is not only epidemic, but likewise irregular and of a more dangerous tendency, it sometimes appears sooner, viz. in the month of January, seizing whole families, and sparing none, of what age soever, unless they have already had it."

Now, Sir, knowing Mr. Travers well enough to be satisfied of his love of liberty, of the genuine sentiments of affection with which his heart is stored, I can have no doubt how he would conduct himself if, he saw a lovely little infant whose parents are too obstinate, if you please to use such an expression, to permit it to be vaccinated. He would, by all the eloquence he is master of, endeavour to remove their prejudices; but if he was unsuccessful, would

would he coolly condemn the innocent infant to the horrors of so dreadful a disease? To this it will be answered, that by the tenderness shown to this infant, a disease may be spread over a whole neighbourhood. This answer is admissable from any but a medical practitioner of London. They know that the town is never free from the disease, and that the only preservative is an early vaccination; which it is every ones duty to inculcate.

There is one argument against inoculation for small-pox, which deserves the fairest investigation, not only on account of the source from which it is derived, but on account of the facts by which, like every thing from that source, it is supported. This is, that the comparative number of deaths by small-pox have been greater in London since the introduction of inoculation. There are, however, two reasons for this. First, that the number of children reared is greater; and it is well known that London children rarely take the small-pox at a very early age. Next, that since the introduction of inoculation, the terrors of the small-pox have gradually lessened. Before that important discovery, few were willing to trust themselves in London without having gone through the disease, and families were always on their guard not to hire servants who were liable to it. 'Tis much to be regretted that this caution is so much lessened. Almost all the females in the Small-Pox Hospital are servants from the country. Many of the men are of the same description, or labourers. It is still more surprising that we have had several soldiers from the Guards. Though I have been particular in my inquiries, I do not recollect an instance of a grown person, a native of London, who has applied for admission. The evil therefore may be remedied without difficulty. Women who migrate to London are so anxious to be settled in service, that few, if any, will refuse to be vaccinated, on condition of being hired; and this may be done with little or no interruption to their common employment.

Since the invaluable Jennerian discovery, there can be no objection to certain legal restraints on inoculation for the small-pox. It has been known from the time of Sydenham, that there are certain constitutions of the air more favourable to the extension of that disease than others. When they occur, whether inoculation is permitted or not, no caution will secure such as are still liable to the disease, and remain in town. It does not appear that such a constitution has occurred since the in-

troduction of vaccination till the present season; and even now, the mortality is considerably less than might be expected. This can only be attributed to the very large proportion of vaccinated children. The mildness of the remedy, and the laudable zeal of the friends to the new discovery, have extended the practice much further than inoculation for small-pox ever attained; and that it will become universal in a few years can scarcely be doubted. But I need not remind every medical man, that our present knowledge will by no means enable us to ascertain how long the sources of small-pox infection may remain in London after the disease has disappeared. Dr. Jenner relates the case of a boy, who was infected by remaining near the spot where the grave was opened of a person who twelve years before had died of the small-pox. Before therefore we can be secure from the disease without vaccination, it will be necessary to shut up for ever all our present church yards and burying grounds. The furniture of all our hospitals and workhouses should be destroyed, and the walls fresh plastered. The same should take place in every house, and, perhaps, every alley, in which the disease may, at any time, re-appear. These are only a few of the difficulties to be encountered; however there are none but what may be overcome, and ought to be attempted; but till this is seriously begun, the only security for an inhabitant of London must be early vaccination.

This digression has led me from the subject I began with. Since the late alarms concerning small-pox after vaccination, we have had applications of all kinds: some to be inoculated, who had been vaccinated. All these have stood the test without a single exception. Others with suspicious eruptions; but none of them have added to the catalogue of unsuccessful vaccination: and I cannot help thinking, when we reflect on the numbers vaccinated, that the very few instances of consequent small-pox, are not, if fairly estimated, greater than would have occurred after the same number of casual or inoculated small-pox.

That kind of eruption which Dr. Willan calls *favus*, and which is one of the eruptions that Dr. Jenner considers as infectious herpes, has been very common. It has affected several whole families; even the elder branches have not entirely escaped in their fingers which come in contact with their children. The principal peculiarity of it, is, that it seems to preserve no certain periods

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in its progress, continuing to break out afresh repeatedly as it dries, and even in the neighbourhood of the parts last affected. It dries also with a foveolous, somewhat resembling the cell of a honeycomb in the regularity of its figure; but the cavity is extremely superficial, and never leaves a pitting. Many of these cases were called, or suspected of being, small-pox after vaccination.

The following history is remarkable for the number of persons who had previously gone through small-pox, and were inoculated by nursing the same child. It also furnishes another instance of the manner in which vaccine and variolous virus infect under different stages of either.

Charles Pigott, an infant, had the small-pox. He had two sisters; the eldest of which was removed from the house as soon as the disease was ascertained. The youngest remained at home.

They were both vaccinated fourteen days after Charles began to sicken. Nothing particular occurred in the eldest.

The insertion in the youngest appearing backward, vaccination was repeated. Before the second insertion could produce any effect, the first came forward and continued its progress. On the eleventh day from the insertion the areola was formed. Three days afterwards some variolous pustules appeared. These dried in a few days, and the vaccine pustule somewhat later than in the elder sister.

Whilst Charles was ill, he was occasionally nursed by his father, mother, a maid servant and her mother, all of whom had gone through the small-pox. The father and mother had pustules in the face, with fever and swoln glands at the neck. The maid had pustules on her arms and fever, and the maid's mother had pustules in the face.

These are only a few of the facts with which the hospital has furnished me, and which, as I find leisure, I shall occasionally offer you.

P. S. We have just received into the Hospital a woman, brought us by Mr. Hodges, whose children were vaccinated in Fulwood's Rents. She is said to have received the small-pox, after a regular vaccination at Wooburn, six years past. On enquiry it appears that there was no regularity in the progress of her arm, which was never examined by the surgeon who performed the operation. In your next you shall have more minute particulars.

I am, &c.

Berner's Street,
August 20, 1805.

JOSEPH ADAMS.

To Dr. BATTY.

Sir,

THE enclosed extract of a letter from Thomas Ross, Esq. Surgeon to the Forces at Gibraltar, gives some further account of the fever which raged so fatally there. I only regret that the account is so short from Mr. Ross, who is so well qualified to have given a longer and more complete one. Mr. Ross, from twenty years experience of fever in the East and West Indies, China, Egypt, and Gibraltar, could have given some valuable remarks and comparison of fever as appearing in these various regions.

Your's, respectfully,

London, Aug. 10, 1805. JAMES M'GRICOR.

Symptoms of the Fever which prevailed at Gibraltar in September, October, November, and December, 1804.

It commences with the usual symptoms of fever; sometimes vomiting with the cold fit, but not always; when the hot fit comes on there is a violent pain in the head, chiefly across the forehead, throbbings of the temporal and carotid arteries; the eyes inflamed and watery; from the distention of their small vessels with blood, they have the sensation of being swollen, and give pain when moved; there is violent pain in the loins and calves of the legs; the face is flushed; the skin hot and dry, and when felt leaves a burning sensation at the fingers-end for several seconds; there is great prostration of strength, and generally anxiety and depression of spirits; but in some instances there is the greatest unconcern and indifference as to their situation, from which circumstance the patients were frequently lost, by not reporting themselves in time to be benefited by medicine. The tongue in general was white and moist, sometimes perfectly clean, sometimes the centre was white, the point and sides clean, red, and shining; the thirst in some cases was very great, while some had no thirst; the urine was high coloured; the bowels in general costive, but when they were not so, or when purgatives were given, the stools were uncommonly fetid; little reliance could be put on the pulse as to strength or quickness; in some it exceeded 130 in a minute, and in others it did not exceed 60 during the whole course of the disease. The eye was by much the best index to be guided by.

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These were the symptoms of the first stage of the disease, which, if not relieved by Nature (which was very seldom the case) or by medicine, in twenty-four hours, the eyes became more suffused and dull, the heat of the skin continued, and all the other symptoms increased till about the end of the second day, when a dull heavy pain was felt at the pit of the stomach, which was soon followed by an effort to vomit; the dullness of the eyes continuing to increase and assuming a dirty yellow glassy appearance, which peculiar appearance was always a fatal symptom. The patient now becomes very restless and delirious; the skin puts on a slight yellow tinge, beginning about the neck, and which in a few hours changes to a dull yellow livid colour; the irritability of the stomach continues, and every thing is rejected; the vomiting becomes constant; what is brought up is a dirty brownish coloured liquor like the washings of Port wine bottles; it gets darker and thicker, and has now more the appearance of coffee-grounds; the desire for cold water now comes on, the restlessness increases, and frequent attempts are made to get out of bed; the delirium is generally expressed in mutterings, but sometimes the convulsions are violent and the ravings frantick; the urine is secreted in small quantity, and sometimes all secretion is suspended; invariably a fatal symptom, and which in some instances occurs early in the fever. Cases have occurred in which not a drop of urine has been passed during the whole course of the disease, yet these patients rarely complained of any inconvenience in consequence. Petechiæ now appear about the breast and arm-pits, and then spread over the whole body; the vomiting continues, attended with hiccup, and a cold, greasy, clammy moisture on the skin, of a peculiar and cadaverous smell; the yellow colour of the countenance acquires a darker hue, and it is strongly marked by the expression of horror; sometimes there are tremors and convulsive motions of the eyes and muscles of the face or limbs; constant muttering; partial intervals of recollection, during which the patient seems sensible of his state, and approaching dissolution, although unable to express himself in words; the extremities are cold; the pulse sinks; and on the third, fifth, or seventh day he dies; in some instances the whole train of symptoms were run through and terminated by death in thirty-six hours from the first attack.

This disease frequently ran quickly on to the putrescent state, when the tongue was covered with a dark brown crust

crust; no thirst; early delirum; the skin soft, of a dirty yellow colour, and in point of temperature below the natural standard; profuse cold clammy sweats; very fœtid stools; tension of the abdomen; hæmorrhages of a very dark colour from the nose, mouth, stomach, and anus; livid petechial eruption, black vomiting, hiccup, coma, death.

Sometimes the black vomiting and all the other symptoms subsided, the secretions and pulse appeared natural, the stomach became perfectly retentive both of medicines and nourishment, the senses perfectly collected, the spirits revived, and the patient appeared confident of recovery, and continued so for one or even two days; and when there was every reason to think all danger past, he suddenly, and when least expected, sunk, and expired without a groan or struggle. There is one thing to be observed, that when these illusive appearances took place, the eye still retained its inflamed and glassy appearance.

On the SYMPTOMS *and* TREATMENT *of the* FEVER *at* GIBRALTAR; *communicated by* ROBERT THOMAS, M. D. *of* Guilford.

THE disease is the Typhus gravior, or jail, or malignant fever.

Great preceding heats, a numerous population in confined situations, and small houses, inattention to proper cleanliness, and a free ventilation, a neglect of separating the infected from the healthy, and of cutting off all unnecessary communication between them, and no efficacious means having been early resorted to for subduing or eradicating the contagion, this fever has now acquired a degree of virulence and malignity equal to the plague itself.

This disorder is highly contagious; as all fevers of the typhoid kind are universally admitted to be capable of being propagated from one individual to another, either by contact or by inhaling the effluvia arising from the body of a diseased person, or from linen clothes and other articles, strongly impregnated with their miasma.

Universal weariness, faintness, great depression of strength, severe pains in the back and head, particularly in the forehead and sockets of the eyes; rigors succeeded by universal heat; thirst, a parched tongue, somewhat covered with a brown fur; nausea, with now and then a vomiting of bilious matter; costiveness, laborious respiration, accompanied

accompanied with deep sighing; a small but quick pulse, of from 100 to 140 in a minute; slight wanderings, incoherency and coma, are the most usual symptoms attendant on this fever; but in its very advanced state, those of putrescency are observable. In some cases, the patient will die on the fourth day.

By the following mode of treatment, I have never lost a patient, where my advice has been applied for early.

If the person is incommoded by nausea or vomiting, on his seizure with this fever, I would recommend the contents of the stomach to be evacuated, by directing him to drink a few cups of a strong infusion of camomile flowers, in preference to his taking an emetic, either of ipecacuanha or tartarized antimony; which seem inadviseable, from the great irritability of this organ, which is apt spontaneously to ensue. If nausea does not prevail, then the first step to be adopted, should be to clear the bowels of all feculent matter, by a sufficient dose of calomel; the operation of which may be rendered more certain and quick, by an addition of a few grains of coloquintida.

The bowels being cleansed, I advise an immediate use of the muriatic acid. To adults, we may give ten or twelve drops at first, which dose should be repeated every four hours, guarded by four or five drops of the tincture of opium, to prevent the acid from acting unpleasantly on the stomach and intestines. By degrees, we may increase the quantity of the acid in each dose to eighteen or twenty drops, employing as a vehicle, about an ounce and a half of a strong infusion of columbo: the effect of mineral acids, but more particularly the muriatic, in all fevers of a malignant nature, is truly great; and from employing it in all such cases, my practice has been marked with the most decided success. To increase the antiseptic effect of this medicine, as well as to obviate debility, I always recommend a quantity of wine, proportionable to the age of the patient, and the exigency of the case, to be administered at the same time. Bleeding should never be used; as the Spanish physicians, at Malaga, sacrificed most of their patients by it.

At a very early period of the disease, when the rigors have ceased, and the skin is become dry and hot, I strongly advise a general affusion of the whole body with cold water, as recommended by Dr. Currie, of Liverpool; and since practised by myself, and many other physicians, in innumerable instances, with the happiest effect. This remedy may be repeated twice or thrice in the course of the

the twenty-four hours; but it should be at those periods when there is no great sense of chilliness present, when the heat of the body is steadily above the natural, and when there is no general or profuse perspiration. Where either the delicacy of the system, the apprehensions of the patient, or the prejudices of the bye-standers, prevent the employment of cold affusion, we must be content to substitute tepid affusion for the more powerful remedy. The earlier in the disease that cold affusion is employed, the more likely will it be to bring it to a favourable termination, should it fail in arresting its progress wholly.

Throughout the whole course of this malignant disorder, the bowels ought to be kept open, by administering, as occasion may require, the laxative of calomel, before recommended; but I do not think that mercury should be given in sufficient doses to excite any degree of salivation, as has been practised in the yellow fever. Should further experience, however, sanction this mode of proceeding, the process will not interfere with the employment of tepid affusion at the same time, although it might with that of the more powerful remedy.

As I am induced to suppose, that a high degree of delirium does not often attend on the fever in question; and that the patient is rather incommoded by muttering and slight wanderings, I do not hesitate to advise the giving a dose of opium, proportioned to the age of the person, every evening at an early hour; which practice I generally adopt in all fevers of the typhoid class, and with very good effect.

Having pointed out the means which I think most likely to procure a favourable termination of the disease, I beg leave to add a few monitory cautions for suppressing its further propagation, and destroying its contagion. To answer the first of these intentions, it will be necessary to keep the mind cheerful, and as free from all apprehensions and anxiety as possible; and carefully to avoid intemperance, sensuality, great fatigue, profuse evacuations, a poor vapid diet, or whatever else may tend to produce debility. By strengthening the bodies of men, it is supposed they will thereby be enabled to resist contagion the better; I would, therefore, recommend cold bathing every morning, with two or three doses daily of some tonic medicine; such as the Peruvian bark. If wine is used at all, it should be sparingly; the less perhaps the better.

Nurses, and medical attendants, who are immediately exposed to the contagion, should be careful to come into immediate

immediate contact with the diseased as seldom as possible; and they ought never to inhale the breath of the sick, nor place themselves in such a direction, as that a stream of air can waft the miasms or effluvia towards them. Possibly, it might be advantageous to anoint the hands with sweet oil, as has been practised, it is said, in the plague, with much advantage.

For the purpose of destroying the contagion, the sick should be removed to Lazarettos; and these must be guarded, so as to cut off all unnecessary communications with those in health. The atmosphere surrounding the infected, should be purified as much as possible, by a strict attention to cleanliness, a free ventilation, and frequent fumigations with the nitrous or muriatic acid, in the form of gas. All substances capable of being impregnated with the effluvia, and of vitiating the atmosphere, should be speedily removed from the apartments of the sick, to situations where the healthy eannot suffer by them, and where they will be made to undergo proper purification.

It does not signify which of the acids we employ, as they are both equally efficacious in destroying every species of contagion. If a preference be given to the muriatic, place a saucer, or any other earthen vessel, containing about half a pound of common salt, in the apartment of the sick, and pour over it, from time to time, a sufficient quantity of vitriolic acid, to moisten the whole of the salt. If the nitrous is preferred, put half an ounce of vitriolic acid into a cup, saucer, or glass, and add, from time to time, some nitre reduced to powder. In rooms from fifteen to twenty feet in dimensions, one vessel will be sufficient; but in larger ones, two or more will be requisite; and when the air is foul, and peculiarly offensive, it will be adviseable to apply a slight degree of heat under the vessels, in order to extricate a larger quantity of vapour.

Europeans of a full plethoric habit of body, who may be obliged to go out to Gibraltar during the prevalence of this malignant fever, will act prudently in taking now and then, during the passage, some cooling laxative medicine; and in undergoing a slight mercurial course, so as to produce an alterative effect. The same plan was found to be a good preventive against any attack of the yellow fever.

July 13, 1805.

Observations on a Case of Strangulated Hernia; *communicated in a Letter from* Dr. Chatard, *of Baltimore, to* Dr. Miller.

[From the New York Medical Repository.]

Norbere Belair, surnamed the Picard, a sailor who had deserted from the French frigate Poursuivante, requested me to visit him on the 20th of last February, at five o'clock in the afternoon, on account of violent illness. I found him in bed, with high fever, occasioned by the strangulation of an inguinal hernia, which had taken place five hours before, from his exertions in loading a cart, while unprovided with his truss, which he had generally worn for the two preceding years. As this man was of a robust habit, I ordered him to be bled to twenty-five ounces, and then attempted to reduce the hernia by the *taxis;* but in this I was unable to succeed. I directed him that night to go twice into the warm bath, and to remain in it two hours at each time, and, in the intervals of the bath, to apply a bread poultice. On the 21st, at eight o'clock in the morning, he continued in the same situation, and his fever being still high, I determined on a second blood-letting, nearly as copious as the first. The baths, the poultices, the injections, were used, one after another, for twenty-four hours, with great exactness; and, in addition, I ordered a grain of opium to be given every sixth hour, to relieve the violent pain, the hiccup, and the vomiting. Every thing was unavailing; and at the beginning of the third day no change had taken place in the condition of the unhappy patient. I thought it my duty to urge the operation as the last resource; but he obstinately opposed it, and declared he would rather die than submit to it. Having much confidence in the treatment used in this case, as I had often seen it succeed under the direction of M. Desault, after persevering in it many days, I insisted on the employment of the baths, the injections, the poultices, and the opium, as before, during the six first days, but they were not used with perfect exactness; and still every day I continued to urge the operation, which was firmly and steadily rejected. At the end of the time just mentioned, the patient, who could take nothing without rejecting it from his stomach, became

came extremely weak, and appeared to me to be in a desperate situation. Still, however, I continued to visit him as frequently as I could, as well as to encourage the assiduities of those to whose care he was committed, and who were beginning to grow weary of their duty, as to observe the progress and termination of a case which seemed to assume a singular character. It was not till near the close of the fifteenth day of the disease that the patient breathed his last. On the twelfth day of the disease he still had strength enough to walk from the cellar in which he lay to a kitchen situated above, and at the distance of some paces from it; where he ate eggs, meat, bread, and drank tea, with very great appetite, and retained these things on his stomach for several hours. During the whole course of the disease he only discharged by the anus the water of the injections, after those first administered had emptied the large intestines of the excremental matters contained in that portion of them which was below the strangulated part.

About the sixth day the hernial tumour seemed, by imperceptible degrees, to grow smaller, and on the twelfth I observed a fluctuation at the bottom of it, which led me to apprehend that possibly an opening might take place there. But this apprehension was rendered doubtful by the skin having undergone no change of colour; and it continued to show the same appearance several hours after the death of the patient, when I performed the operation, in order to satisfy myself as to the nature of the disease. The hernial tumour was then of the size of a turkey's egg, of an oblong figure, descending into the scrotum, and from four to five inches in length. I found the peritonæum thickened to one-fourth or one-third of an inch, adhering to the skin and to the omentum, which formed a large proportion of the hernia. The omentum was so condensed and indurated that it was difficult at first to distinguish it; and the testicle, exceedingly diminished in size, was enveloped in it. I observed no appearance of gangrene in the omentum, except at the lower part of it, which was black for about the extent of an inch, and reduced to a sort of ill-conditioned matter, rendered thin by the serous fluid, which, as I observed before, began to be formed about the twelfth day of the disease. So far I had been unable to discover any intestine, although it was plain that some part of that canal held an important share in the disease. I determined to divide the omentum to within

within two inches of the abdominal ring, and I there found that it enveloped the intestine in its superior part as it did the testicle in its inferior part. This portion of intestine was strangulated by the ring for about an inch, but the appearance of it had undergone little alteration. This made me regret that the patient had not consented to undergo the operation even in some of the last days of the disease; as there is reason to believe that, even then, his life might have been saved by it.

Although this case does not afford the satisfaction of having been successful, it seems to prove that the treatment employed sufficed to support the life of the patient during a principal part of the time of the disease, and that it would finally have succeeded if his obstinacy had not prevented it.

I can also state, in favour of this treatment, another case more fortunate, in which I was consulted by my partner, Dr. Dunan. This practitioner was called to visit Mr. V. who had been attacked with the same disease. After having fruitlessly used attempts to reduce the hernia, and having tried the application of ice, he invited my assistance in the case. My opinion was, that it would be advisable to use the remedies employed in the former case in the fullest extent. Accordingly the patient was twice bled, he received injections, took three grains of opium, and was kept immersed in the warm bath for forty-two hours, at the end of which time the hernia was reduced of itself, without any external aid.

I know of no practitioner, before myself, who has undertaken to prolong the continuance in the warm bath so far; and M. Desault, although strongly attached to the remedy, only ordered it for a few hours, with the direction to repeat it afterwards. But I have supposed that no other limits ought to be placed to the use of the remedy but the weakness of the patient, who, if well nursed, and vigilantly watched, may be withdrawn from the bath on the least appearance of syncope.

Memoir intended to show that the Cause of the Entry and Motion of the Sap in Plants, ought to be attributed to Vacuities created in their Vessels by Transpiration. *By* I. B. Bizet, *of Amiens.*

[Extracted from the Journal de Physique, tom. lix. p. 43.]

It is known, and perhaps the knowledge would stand high in the calendar of human acquirements, if its triviality does not prevent its admission among them, that vegetables receive their nourishment, at least the most substantial part of it, such as their sap, by means of the roots; but it does not appear that we have yet attained to the discovery of the cause of its entrance into their interior. Animals have in themselves the faculty of choosing their food, and of conveying it into organs, where it is converted into their substance; the vegetable has no other faculty but that of receiving the juices it meets with in the spots of earth and water to which its roots extend. It is true that these parts of a vegetable are all disposed for the reception of such juices; they are covered with an infinity of mouths destined and ready to serve as entrances to the canals designed to distribute them through the whole of their interior. But what introduces them into these canals? What is the cause of their entrance into, and circulation in them?

Grew, a celebrated English philosopher, was of opinion that the sap must be greatly rarified, and in some measure reduced to vapour before it could enter into plants, and that it was only raised in their vessels through the levity it acquired in this state.

To this it was answered, by M. Duhamel Dumonceau, that sap was never found in plants, except in a liquid form.

If he had replied that this was because the vapours were condensed in their roots, it might have been asked, what refrigerants of sufficient activity could these vapours have encountered there, to be condensed so early as to be found only in a liquid state, while the roots necessarily participate in the same degree of temperature as that of the earth or water with which they are surrounded; a temperature which must equally have opposed the formation of these vapours?

It has been attempted to explain the cause of the introduction and motion of the sap in plants by the dilatation

and condensation of the air they contain. It has been said, that when the air included in the trachea of their roots become rarefied, it pressed upon the vessels filled with the sap, and by this impulse forced it towards the upper part of the vessels; and that when the volume of air in these trachea was diminished by condensation, a vacuum was formed in the sap vessels of these parts, which produced a suction, and attracted new sap thither.

But the air in the trachea of the roots of plants can only be dilated by heat; now when the heat dilated this air, it must also have dilated, at the same time, and more powerfully, that of the trachea of all its other parts, since they are more exposed to it; the impulse of the pressure must therefore be stronger towards the inferior than towards the superior parts of vegetables. The dilatation of the air in plants, produced by this cause, would consequently be more disposed to drive the sap from the upper parts of them, and to cause it to pass downwards to the roots, than to occasion such vacuities in them as would produce suction.

Others have been satisfied to assimilate the first entrance of the sap into plants, to that of water into spongy bodies or into capillary tubes, where nevertheless they could only, by either of these methods, cause it to ascend some fractions of a metre; and still less could they thus convey the sap to the height of the cedars of Lebanon, or even to that of the oaks of our forests.

The inadequacy of these causes assigned for the entrance and motion of the sap in plants, has occasioned other observers to be contented to attribute them to heat, or, which is the same thing, to the influence of the sun. It is true that while a plant vegetates, the sap is always most abundant in the warmest weather; but it appears to me that to attribute its entrance and its motions to this cause, is to assign the general cause of vegetation, or the universal cause of all its phenomena, to an individual phenomenon; and as each of these, in addition to the general, must have a peculiar cause, the motion of the sap must also have the same, the discovery of which is the object of the present memoir. Besides, how is it possible to suppose that heat is the particular cause of the entry of the sap, when it is this which occasions it to exude so abundantly in transpiration? Effects so opposite imply different causes. It may also be asked of those who support such an opinion, how heat can introduce these nutritive juices into

into such plants as push their roots to a depth in the earth at which the influence of the sun is never sensible.

Hence M. Duhamel, enlightened by the knowledge of the philosophers who had preceded him, and assisted by that of his contemporaries, after having weighed their opinions, repeated and examined their experiments, extended their researches, and greatly multiplied his own, was compelled to acknowledge, that *all he had said on the cause which determined the ascent of the sap in plants, must only be considered as simple conjectures.* These are his own words in his Physique des Arbres, tom. ii. p. 264.

But if M. Duhamel could only offer conjectures on the cause of the motion of sap in plants, can I flatter myself with having discovered its true cause? I am, at least, authorized to believe so, since the theory on which I rest my proofs, is established on facts, and these facts are confirmed by numerous and constant observations, which leave no room for the smallest doubt.

It is known, as I have already observed, that the juices which nourish plants are introduced into them by their roots; it is also known that these juices rise to the extremities of their stems and their branches, whence it is probable that they pass between their internal parts and their bark, where they continually form new layers, which progressively increase the bulk of them, and consequently that of the roots, to which they descend, and also reach to their extremities.

The nature of the vessel which conduct these juices in plants is not yet known; it has only been discovered from multiplied experiments, that such of these vessels as receive them at their entrance into the roots, follow their fibrous or ligneous parts through all their extent. Are these vessels of a nature to be compared to the arteries and veins of animals; or, which is less probable, are they only cellular or spongy textures, which by communicating with each other, distribute the juices into all the parts of the plant? On this subject there is nothing but uncertainty; but it may be considered as an established fact, that those conductors of sap in vegetables, whatsoever they may be, retain it in such a manner that no part of it can escape except by transpiration, and only by the passages destined for that purpose; and that what does escape is only the constituent parts of the sap; the lymphatic parts reduced to vapour; the oxygen, hydrogen, and azotic gases, which plants continually emit into the atmosphere; but it is never sap. A proof of this is, that the extravasation

of sap is a mortal disease to plants, and always, or nearly always, at least while they vegetate, is the cause of a state of debility and languor. Hence arises the maxim in gardening, which directs the covering and taking care of the wounds made in trees when they are pruned, as well as those arising from any other cause.

If the sap cannot escape from the vessels which distribute it through the plant, these may for that reason be compared to tubes as far as regards the sap, and their properties in this respect are the same; now, it is universally known that if a vacuum is made in a tube, the end of which is plunged into a liquid, the vacuum attracts it, and causes it to ascend in the tube. I shall not, in describing the cause of this phenomenon, use the language of the learned of former days, and say that it is because *Nature abhors a vacuum.* Torricelli and Pascal opened the paths which led to the discovery of this cause, by enabling us to discover the properties of a vacuum. The same cause produces the same effect in Torricelli's tubes, and in the vessels of plants; it is also this cause which is the principle of the power of suction, which M. Duhamel and several of his contemporaries suspected in their roots. To arrive at this cause, it would only have been necessary for them to have discovered that of the suction, which is also the subject of this inquiry.

To perceive the reality, and all the efficacy of this cause, it is sufficient to consider a vegetating plant as being what in reality it is, a compound of vessels, all the capacity of which is filled with the juices or sap it contains, and the lower extremities of which are enveloped in humid earth, from which it draws its nourishment. Were it necessary to bring proofs of this assertion, they would be readily found in the state of humidity and verdure of all its parts.

These vessels are successively emptied, more or less, of their juices by the effects of the transpiration they experience, which necessarily creates vacuities in them, and these produce the same effects in the vessels as those occasioned by the piston in the Torricellian tubes, or in air pumps; that is to say, they attract the juices from the lower parts of the vessels until they are filled.

The parts of the vegetable in which these vacua are first formed, are those most exposed to heat or to the action of the sun. These exposed parts, by gradually attracting the sap from those less exposed, give rise to similar vacua there, which, in their turn, act on the sap of the roots, and

and consequently produce that power of suction suspected by authors; this attracts the water, or humidity of the earth, which, by this mechanism, becomes the nourishment of the vegetable.

But Torricelli and Pascal have shown, that it was the weight of the atmosphere on the liquid which caused it to rise in the tubes in their experiments; now, it may be said, how can this weight reach the fluid which enters into the roots of vegetables, and press on them to force their entrance, while the roots are frequently at considerable depths in the earth, which, itself bears all the weight, and consequently relieves the liquid from it? Besides, it may be added, in the experiments of Torricelli and Pascal, the tubes made use of were of a capacity which offered no obstacle to the entrance and motion of the liquid they contained; and those which receive the sap in plants are very capillary tubes and almost imperceptible, in which the liquids introduced meet greater obstructions in proportion to the friction they experience. By what means can the power of the weight of the atmosphere, at least weakened at such depths, overcome them?

The weight of the atmosphere which bears on the surface of the earth and compresses it, also compresses all the waters it meets with there. One of the principal effects of this pressure on the waters is to prevent them from being carried off and dissipated in vapour; and this force of compression is so powerful that it raises a column of mercury equal in weight to a column of water thirty-two feet in height, as is shown by the Torricellian experiments; there is nothing in action except caloric, which has the power of reducing a part of these waters, proportionate to its intensity, to vapour, and carrying it into the region of clouds. But what remains in the earth does not cease to be compressed by the atmospheric weight; this power alone does and can cause it to enter and be dispersed in its bosom, since without that, the water, instead of descending and penetrating into it, would necessarily be elevated as in a vacuum.

The waters spread through the earth are therefore in a state of compression in it; they must, consequently, enter into the vacuities they meet with in the roots, and rise in the vessels of the plants, for the same reason as they ascend and fill the tubes in the experiments of Torricelli and Pascal.

We shall however presently see that this cause of the

entrance and circulation of the sap in the vessels of plants is not absolutely necessary.

It is true that these vessels, as well as their entrances in the roots, offer only very capillary passages to the water or the sap; but, 1st, this circumstance is so far from being a hindrance to the introduction of it into the roots, that, on the contrary, it appears to be very favourable to it; it is known that water enters and rises in capillary tubes without a vacuum being necessary; the water or sap must therefore enter more readily into the roots of plants where there are vacuities which attract it.

2d. I do not mean to be understood that the sap meets with no obstacles to its ascent in plants, or that its motion in their capillary vessels are as free and as rapid as that of the water in the tubes in the experiments mentioned above; but these obstacles, without injuring the efficacy of its circulation, only render it slower, and serve to explain some phenomena of vegetation, which, otherwise, perhaps, it would be difficult to understand.

There are few who have not observed that in the great heats of summer, many plants, particularly those most exposed to the sun, the extremity of whose branches, and also other parts which have not acquired sufficient solidity to be supported by the strength of their fibres, become relaxed and hang almost perpendicularly towards the earth; this gives them an appearance of languor, which may be said to be almost momentary, for as soon as the sun has disappeared, these drooping plants regain the vigour they had before.

The cause of this phenomenon can only be sought in the great quantity of sap which these parts of the plants lose by transpiration, while they are heated by the sun; they were only supported before because they were filled and distended by the sap. The transpiration they afterwards experience occasions great vacuities in them, and the parts which were strengthened by being filled, cease to be so, which causes them to bend towards the earth, and they remain drooping because they are filled slowly, on account of the obstacles which the new sap meets with in the capillary vessels of the plants to stop or retard its passage from the roots.

Another objection may be offered, which at the first blush appears more plausible. It may be said, if the cause which occasions the elevation of the sap in plants be the same as that which makes the water ascend in tubes, in the experiments of Torricelli and Pascal, this can only raise

raise the water in them to the height of thirty-two feet, how then can it carry the sap to the tops of the tallest trees, which are seventy or eighty feet and upwards?

This objection only appears plausible because we have not yet determined in a precise manner the cause which makes the water rise in the tubes in the above experiments. At first, this cause appears to be only the pressure of the atmosphere; this pressure, however, is evidently but a remote cause; the true, the immediate cause which occasions the ascent of the water in these experiments, must be sought in the vacua which are made in them; it is evident, that the water only rises as they are made and in proportion to them. Torricelli and Pascal stood in need of the pressure of the atmosphere to be able to make a vacuum, and hence they were unable to produce them beyond a height of thirty-two feet; but the Ruler of Nature has created a profusion whi h are independant of this pressure, in the economy of vegetation.

To the Editors of the Medical and Physical Journal.

GENTLEMEN,

IN my Observations on Mr. Mace's case is remarked, "that my sentiments on the nature and cure of gout did not essentially differ from those of your ingenious correspondent Mr. Mantal;*" that opinion was solely founded on his comments on Mr. Edlin's "account" of Mr. Baker's case. The previous communication of Mr. Mantal, published in No. 70 of your Journal, had at that time escaped my perusal, nor were his farther thoughts on the subject, inserted in No. 75, at all anticipated by me. It is necessary to premise these facts, to justify the irreconcilable difference which subsists between Mr. Mantal's notions of the nature and cure of gout, and those of my own.

Mr. Mantal has favoured the public with three distinct communications on the subject of gout in the course of five months, and which he wishes to have considered as supplemental to, and explanatory of, each other. These communications have gained by delay the practical value

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* Medical and Physical Journal, No. 74, p. 360.

of being in part illustrated by the occurrence of what the author deems demonstrative cases of the validity of the opinion which he espouses. Liberal discussion is the torch of truth, and cannot fail to elucidate the obscure speculations of physiological conjecture. Mr. Mantal has been largely indulged in expatiating on my theory and practice relating to gout, you will consequently feel no unwillingness in acceding to my right of ample reply. The subject is indeed important, but it shall be discussed by me with becoming brevity.

Mr. Mantal imagines insuperable objections to admitting that the genuine seat of gouty inflammation is in the ligamentous and tendinous structure, and affirms the theory that insists on this opinion to be a "*gratuitous assumption.*" But how does he prove it to be gratuitous? Not by exhibiting a view of the phenomena of the disease incompatible with such theory, but by roundly asserting it is gratuitous.

Permit me to ask Mr. Mantal, if every feeling and every external appearance in gouty inflammation do not concur to prove that the affection originates and chiefly continues on the ligaments and tendons? These parts are the principal instruments of loco-motion, they naturally occupy an intermediate office between muscle and bone; and to fit them for their purpose, they possess less excitability than the former, and more than the latter. They are susceptible of inflammatory affection, slow indeed in its approach, but becoming at length exquisitely severe. The structure of these parts is too dense easily to admit of suppurative inflammation, hence the period of pain and tension is in general indefinitely prolonged. The tardy accession of inflammatory disease on the ligamentous and tendinous fabric often insidiously disturbs the general excitability of the system, and occasionally induces rigor, followed by increased heat, indigestion, loss of appetite, bilious tinge of the secreted fluids, head-ach, and the usual oppressive sensations of symptomatic fever. As the local pain augments, the circulating fluids become more rapidly and equally distributed over the frame, and at length are more particularly determined to the cutaneous surface, where, by the consequent event of perspiration, much superabundant heat is dissipated and temporary relief obtained. This is a general view of the accession, progress, and termination of a gouty paroxysm. The short period of diminished excitement, which succeeds the incipient and more active stage of a gouty fit, has delusively induced

duced the popular belief that the attack has been salutary, and that the affection is Nature's mode of expelling from the system either morbid matter or motion!

Mr. Mantal may remain incredulous as to the disease originating in the ligamentous and tendinous structure, but his want of credence does not alter the real state and character of the distemper, which every unprejudiced reasoner, and every gouty patient, will admit to be derived from the fabric insisted on. The analogy of common sprain to gouty inflammation is another argument which raises my claim to correctness, higher than that of a mere "*gratuitous assumption.*" When a ligament or tendon is mechanically forced beyond its healthful dimensions, what happens? Neither immediate inflammation nor inability to move the affected part. These effects usually occur in the course of a few hours after the accident, and mark the slow but eventually severe accession of inflammatory disease. Rigor also, and more or less of systematic ailment, often arise between the time of inflicting the external violence and that of the full production of its inflammatory consequence. During this period the latent growth of stimulant disease at the sprained part imperceptibly generates sympathetic commotion throughout the system, which, in the instance of gout, is erroneously supposed to have occasioned the local inflammation, because this appearance does not conspicuously attain its formal character before the systematic effects of its earlier stage are somewhat subsided. Thus a strong resemblance may be intelligibly made out between the inflammatory affection of the ligaments and tendons from mechanical violence, and that which spontaneously arises from an unnatural susceptibility in the motive power of these parts for being morbidly impressed, whether induced by the debilitating influence of immoderate loco-motive exertion, or proceeding from a distempered state of systematic excitability, preponderating on the ligamentous and tendinous structure.

"*Assumptions,*" however "*gratuitous,*" are not proved to be so by mere affirmation; that which is destitute of support should appear in its untenable deficiency, and not be rejected by declamatory refusal. When a theory is impugned, the public have an anxious interest at stake; it is at once hoped that the evils of an erroneous opinion will be prevented, and the advantages of superior correctness realised. But how has Mr. Mantal discharged this important obligation? He has objected to the ligaments and tendons

tendons being the seat of gouty inflammation, to suggest his opinion that it is rather a "disease of the membranes that cover the joints." What are these membranes? My knowledge of anatomy reminds me of no membranous covering on the joints excepting the cellular texture which forms the interstitial or connecting fabric of the surrounding parts, but no where distinctly envelopes the joints. The bones constituting the joints, are variously tied and bound together by round and capsular ligaments, and more or less overspread by tendinous insertion or adhesion.

But Mr. Mantal removes all obscurity respecting his opinion of the nature of the *membrane*, on which he would have gout to originate, by likening it to the "membranous parts of the stomach and liver;" and conceives that, by analogy of structure, the community of gouty affections subsisting between the visceral and articular parts is clearly explicable. Mr. Mantal then, with seeming triumphant complacency, remarks, "perhaps Dr. Kinglake would find it difficult to assign a reason, why, when gouty inflammation attacks the ligaments and tendons of the great toe, of the ankle, and knee, in succession, or at the same time, the intermediate parts of that structure should escape without injury; but there seems no room for that objection on the view of the disease which I have presented." No more difficulty occurs in assigning a reason why an inflamed joint from gout should excite correspondent disease in a distant joint, than why extreme pain in the head or stomach should be occasionally reciprocated between these several organs; than why sprain should ever induce locked-jaw; than why external blistering should relieve visceral ailment; in short, than why the most common instances of remote sympathy should obtain.

It will not be denied that similar structure is actuated by kindred motive powers, and that diseased conditions may be either simultaneously or successively excited in distinct portions of it, through the medium of the connecting fabric, without necessarily involving the transmitting or intermediate parts in disease; an inflammatory action in the dense fabric of a ligament or tendon, may be propelled through the comparatively lax texture of muscular and cellular substance, and produce no diseased impression until it may reach the analogous structure of other joints. In like manner, remote sympathies obtain between the sufferings of other similar parts, independantly of contiguous influence.

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It is not a little strange that Mr. Mantal should raise a difficulty against my theory, to make way for one of his own, which he has founded on an *ideal membrane* existing only in his own imagination. The joints have no surrounding membrane resembling either the internal mucous lining, or the external peritoneal covering of the stomach, liver, or any other viscus. Why then will Mr. Mantal invite a dispute concerning a non-entity? Mr. Mantal first creates a *membrane*, distributes its extensions, and determines its sympathies; he then, not so *originally*, but quite as obscurely, assumes *torpor* on the vital organs, however resulting, as the cause of *inflammatory* gout.

The laws of Nature or (if not synonymous) those of Darwin, are variously pressed into the service of generating inflammation. At one time Mr. Mantal is obliged to suppose that *strength*, by a law of organic life, is the offspring of *weakness*; at another, that the protecting ordinance of Nature has made the place of *vital langnor*, the post of *motive energy*. Visceral or constitutional *torpor*, Mr. Mantal asserts, is the cause of inflammatory gout on the joints. How is it conceivable, that while parts actuated by the higher energies of vital power should be from torpor (which implies comparative inaction if it signifies any thing) incapable of inflammatory irritation on a visceral part, but that it should be able to excite that disease on a remote joint? No reason appears to common sense intelligence, why this seemingly incompatible effect should happen; but Mr. Mantal has supplied this want of explanation by considering the process as under the provident guidance of the "*vires medicatrices naturæ*," which cannot err, and which Mr. Mantal affirms, will defend the vital parts to the *last extremity*, by generating an inflammatory action on the joints, the salutary effects of which, are shewn in the removal of the morbid *torpor* of the system.

Has this theory a feature of the captivating simplicity, and dexterous sufficiency of Nature? Is it not toilsomely incumbered by the crooked and incompetent contrivances of art? Does it not bespeak a TORPID and fallacious attempt at a satisfactory explanation?

But how does the idea of *torpor* comport with the usually existing state of the system of those who are most afflicted with gout? Does not an attack commonly ensue a series of intemperance in eating and drinking, or some other stimulant excess? Does it arise after a course of abstemiousness, the occurrence of mental distress, or the influence of any other debilitating power? Is it not manifoldly

nifoldly proved, that the *torpor* (if Mr. Mantal will have it) of extreme sobriety and temperance very generally prevents the periodical recurrence of gout?

Mr. Mantal has not done Dr. Darwin full justice in employing *torpor* as a cause of inflammatory excitement, and not resorting to his ingenious device for rendering its operation at all explicable, namely the accumulated excitability during the prevalence of *torpor*, by which the usual portion of vital power not being expended, additional susceptibility is supposed to be induced for being impressed by stimulant agents. This explanation is indeed more specious than real; it imagines the vital process to be undiminished in a state of *torpor*, and that its energy is accumulating in circumstances of *inaction!* Neither the one nor the other can intelligibly happen. If a part be torpid, it must be deficient in the necessary energy to generate an excess of vital power, and when vital power abounds, it irresistibly dissipates itself in the irregular actions of violent disease. Torpor therefore is too *negative* a state to be compatible with the *positively* active quality of inflammation, and much too barren ever to generate an exuberant portion of vital power.

Mr. Mantal, in dismissing his theory of gout, observes, that, "with respect to the practice of Dr. Kinglake, my only difference with him is, that I think *caution* is necessary in the application of the remedy." My advice in directing the treatment is replete with *caution*, on my principle of the nature of the affection. It is held by me to be solely a disease of excessive temperature; and when either threatened by uneasiness, or characteristically shewn by violent inflammation and pain, that it ought to be resisted by the topical application of cold water, and that the practice should be unremittedly prosecuted as long as a painful degree of heat might continue to prevail on the affected part; that when it shall cease, the remedy should be suspended, and resorted to as often as the recurrence of inflammatory excitement might require, attending to any ailment which might either accidentally or sympathetically arise in the system, according to its seeming nature. If tremor and coldness should be felt in the region of the stomach, relief should be attempted by stimulant agents; if heat and pain, by small draughts of cold aqueous liquids at short intervals. Under this obvious caution, at once suggested by common sense, and capable of being precisely regulated by the sure indications of natura

tural feeling, no fear need be entertained respecting the perfect safety and eventual efficacy of the treatment.

Mr. Mantal thinks the disease should never be prevented; but that "when the inflammation is completely formed on the extremities, and not at the first feeling of painful stiffness of the affected joint, and when visceral uneasiness arising from torpor (which he believes always precedes the inflammaton of the extremities) has entirely subsided, the cooling remedy might be safely resorted to." This may be a judicious caution, if the theory on which it is founded be just; but surely that is too factitious to be natural, too much composed of "*gratuitous assumptions*," of *torpor* and *action*, of native providence, and of hypothetical laws of organic life, to pretend to an indisputable claim to intelligent assent. It is farther opposed by the evidence of facts. The forming stage of the disease has, in numerous instances, been counteracted, and in spite of the powerful stimulus of *visceral torpor* aided by this law, or that, and farther backed by the "*vires medicatrices naturæ*," has not again advanced, and all his seeming repulsive mischief has manifestly rather relieved than disordered the general health.

The precarious connection subsisting on Mr. Mantal's theory of *inflammatory torpor* between the torpid viscus and the inflamed joint, and the importance which is made of relieving the former by promoting the latter, rather justifies than condemns the stimulant treatment of gout, by rendering the safety of the refrigerant practice in every instance extremely problematical. In the doubtful calculation, whether or not the inflamed joint (the offspring of *visceral torpor)* has endured long enough to have sufficiently energised the system, too much well founded anxiety must ever be felt to admit of a confident and successful use of the remedy. With such theoretical shackles, therefore, it is scarcely conceivable that any beneficial reform could be effected in the old mode of treating gouty inflammation.

Mr. Mantal's view of gout is embarrassed with much too large a portion of *contingent* mischief to obtain many proselytes to his *cautionary* treatment. When acted upon, it must necessarily lead to a timid, feeble, undecided, and often inert practice. The delicate balance of *visceral torpor* and *inflamed extremities* must ever tremble in the patient's fearful imagination, and resolutely lead him back to *patience* and *flannel*, rather than to purchase relief at such speculative risk.

Mr.

Mr. Mantal then refers to a case of sprain, treated by cold applications, inducing sympathetic gout in the stomach, and terminating in death. This is egregiously stretching an attempt alternately to destroy by sympathetic *torpor* and sympathetic *inflammation*. After affirming that *visceral torpor* always precedes gouty inflammation on the joints, and that the latter removes the former, it is hardly appropriate to his argument that a sprained, and of course inflamed ankle, had been too speedily relieved of its salutary pain by the influence of topical cold, and had given occasion to sympathetic gout, or a state of fatal torpor in the stomach. Here a very arbitrary association is made to arise; it did not result from habit. The chain of irritability said to subsist between the stomach and ankle, was not as usual first disturbed by *torpor* at the stomachic link, but inflamed at that of the ankle; and though Mr. Mantal asserts that this chain runs through the *same membranous structure*, yet, by a mysterious but preserving law of life, its *visceral end*, when suffering from gouty excitement, is in a state of torpor, whilst its *articular extremity* is tortured by inflammatory action! Such inconsistent arguments may be convenient to hypothetical reasoning, but will not satisfy the just demands of scientific inquiry.

Mr. Mantal's theory, in my opinion, does not bear him out in explaining the phenomena of the disease, as he appears to imagine; but its supposed validity is not refuted by my considering it as groundless. It must be tried by the evidence of facts, and farther experience will furnish a true award of its merits. It is utterly beyond the reach of my conception, and at variance with my uniform observation, that visceral excitement when at all connected with articular or gouty inflammation, should be founded in torpor. The viscera are highly susceptible of original as well as sympathetic inflammation, why therefore should gouty disease, said to be diffused throughout the system, especially attack the dense structure of the joints, and fail to produce inflammatory effects on the vital organs?

My theory, be it good or bad, (and it cannot be justly contended that my "*vanity*," as Mr. Mantal questions, has led me to suppose it perfect) accounts for every possible diversity of systematic disorder sympathetically arising from the severe local irritation which prevails, by attributing it to the existing circumstances of vital power in the organs which may become affected. The sympathetic effect of acute and protracted pain may be shewn in either

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an inflammatory or a nervous form, and will be respectively cognizable by the re-active symptoms that prevail.

The ground of the different symptoms will be laid in the comparative temperature and energy of vital motion. Inflammatory tone and action will mark the one, whilst tremulous debility and chilliness will distinguish the other; the precise degree of either, will be governed by the temperamental conditions of motive power.

Mr. Mantal exhibits, in his second communication on Mr. Baker's case, a lucid and instructive view of the refrigerant treatment of gout. By the removal of visceral torpor, and the establishment of systematic tone through the efficiency of articular inflammation, he arrives at practical rules, nearly similar to those which are derived from the changeful course of temperature on the principles of my doctrine. In my mode of reasoning, it is vindicable to counteract the full formation of gout by topical cold; this Mr. Mantal disapproves, but he justifies the unremitted use of the remedy after it has fully obtained. We shall therefore practically agree in the vast majority of instances, nor will any disinclination or "*reaitancy*," (as Mr. Mantal terms it) be shewn by me in admitting the due import of whatever further experience may prove to be adverse to my present view of the subject.

Mr. Mantal, in his third communication, commenting on the cases adduced by Mr. O'Neal and Messrs. Scott and Taynton, discovers a precipitancy and warmth of expression unworthy of his usual correctness and moderation. His philanthropy has probably misled him to hasty and severe remonstrance. It were illiberal to suspect him of a worse motive. He conceives my theory of gout to be deeply involved in the cases detailed by Mr. O'Neal, and Messrs. Scott and Taynton, and thinks it should be revised and corrected, to prevent the practice founded on it, from falling into disrepute and consequent disuse. It would indeed be gratifying to me to clear the question of every difficulty in the way of solid truth. No undue tenaciousness of opinion on my part shall ever obstruct such a desirable elucidation; but Mr. Mantal will have the candour to exempt me from censure, in not allowing my acquiescence to precede my conviction.

The case of Mr. Baker has not in my judgment received any confirmation from the others on which Mr. Mantal comments, as affording corroborative evidence. Mr. O'Neal's case cannot, with either common consistency or ingenuousness, be dragged into the discussion of the merits of the cooling

cooling treatment of gout, the practice pursued on that unhappy occasion was unprecedented, supported by no authority, and vindicable only by the author's peculiar knowledge of circumstances. It was instituted for an ideal disease as it respected the visceral affection, and perhaps no conditions of either health or distemper could escape the destructive influence of such deadly coldness.

Mr. Macer's case, as reported by Messrs. Scott and Taynton, does by no means warrant Mr. Mantal in inferring any thing to the disadvantage of my theory of gout in support of his own; yet he has with too much apparent captiousness availed himself of it, as disreputable to the indiscriminate use of topical cold in gouty inflammation. This case has already been remarked on by me in No. 74 of your Journal, but probably in a way not satisfactory to Mr. Mantal; but how can it be impartially considered as bearing at all unfavourably on my want of sufficient caution, when its unbiassed authors remark as follows? "We forbear making any comments on the above facts, but it appears worthy of remark, that this gentleman, in former fits of the gout, had suffered as much constitutional indisposition when the parts affected were wrapped up in flannel as under the present (cooling) mode of treatment." This avowal, it may be presumed, shifts a heavier *onus probandi* on Mr. Mantal, in establishing his assertion that the injury resulted from the unseasonable use of topical cold, than he appears to imagine; and the entire silence with which this important fact is passed over, presents a regretful instance of Mr. Mantal discovering more solicitude to confirm his own speculation than fairly to solve the question at issue. Nor does Mr. Mantal's *playful* recurrence to the several terms, "*accidental*," "*casual*," and "*co-incidencies*," at all contribute to the philosophic value of his discussion.

It surely will not be gravely contended that these terms have no admissible significancy in philosophical inquiries, and that they are, as Mr. Mantal would insist on, "*declarative of ignorance.*" It is evident to the most superficial observer, that the regular progress of fever is often disturbed by the accidental occurrence of inflammatory irritation; that the variolous and vaccine diseases may be deranged in their course; in short, that no ailment is exempt from the disconcerting influence of *accident, casualty,* and *co-incidence.* But to be more pointed, may not the bursting of a blood-vessel in a vital organ, the occurrence of an habitual epilepsy, an occasional apoplexy, the first

first impression of febrile contagion, or that of various other adventitious evils to which human life is incident, pending a paroxysm of gout, prove mortal without incurring any just imputation on the credit of a mode of cure adopted for the disease?

These occurrences are too rare to be liable to mislead; they are also the effects of certain causes, and might be totally irrelevant either to the definite action of a given disease, or to the operation of remedies instituted for its cure. Can Mr. Mantal imagine a fact in either the moral or physical world that is not liable to have its accustomed efficiency or character diversified by contingent influence? or will he aspire to a severity of theory that shall explain every anomaly, systematise every irregularity, and present one unvaried picture of uniform agency? If he cannot, why will he contend that Mr. O'Neal's case could not have suffered from the *co-incident* effects of an unauthorised treatment; and that Mr. Mace's does not present a *casual* instance of inveterate association, which neither protracted nor curtailed gout could destroy? Why does he attempt to raise on forced and loose analogies a theory which cannot endure but on a basis of more correct inquiry and more just conclusions? These are the errors of intemperate zeal, they are the crude offspring of visionary physiology, and are chiefly objectionable for the premature attempt they discover to controvert a different opinion on equivocal and insufficient data.

No harsh epithet Mr. Mantal has thought proper to bestow on me, shall be retorted on him; he has my implicit credit for integrity and honest intention, nor do his strictures on my theory at all displease me, they serve to invite farther inquiry, and will probably conduce additionally to elucidate a subject of momentous and arduous investigation. Experience has been hitherto my sole guide in this research, no other source of instruction can so safely and usefully advance my knowledge on the subject; and to no other authority, however specious, will my persuasion of the correctness of my opinion, respecting the refrigerant treatment of gout, ever yield.

Mr. Mantal, and all others who may laudably occupy their attention on the subject, must proceed on experimental ground, examine with scrupulous fidelity the facts which may occur, and neither raise any constructions, nor draw any inferences from them, which do not legitimately flow from their undisguised and true import. To inquiries thus chastened, the public may confidently look for ster-

ling truth; and whether the result should be propitious or adverse to my opinion, it shall have my unprejudiced and cordial assent. I am, &c.

Taunton, May 20, 1805. ROBERT KINGLAKE.

To the Editors of the Medical and Physical Journal.

GENTLEMEN,

IF the few following Observations on the Use of the cold Affusion in the Cure of the late Influenza, as it appeared in the Orkney Islands, are deemed worthy of a place in your excellent Journal, they are much at your service.

The exact period when this epidemic made its first appearance in those islands I am not able to state; but it certainly was not very prevalent there, till a considerable time after it had appeared in Paris, London, and even Edinburgh. In the course of the following summer I visited Edinburgh, and during a conversation with my friend Dr. Wright, I learned that he had treated influenza as a fever of debility, and had successfully employed the cold affusion; I mentioned my having employed the same remedy in this disease; and concluding that many other physicians had done the same, I abandoned the idea of transmitting an account of my practice to some of the Medical Journals. My attention to the subject was however roused by a careful perusal of the last edition of Dr. Currie's invaluable Reports on the Effects of cold Water in Febrile Diseases, where he mentions having employed the *tepid* affusion on himself while labouring under influenza, but says he did not employ the *cold* affusion, on account of the pulmonary symptoms and cough attending this disease. From this I conceive that the cold affusion, as a remedy in influenza, has been generally deemed a dangerous practice, and therefore a narrative of its successful employment may not be uninteresting.

The first case I was called to, was a woman aged 68 or 69, who complained of the usual languor, uneasiness, &c. that accompany febrile diseases, to which were superadded cough, difficulty of breathing, and considerable pain in the chest. The heat of the body was not very great; the pulse frequent, but not full; her belly costive. Before I was called, she was, at her own desire, bled to

eight

eight ounces, but without any abatement of the symptoms. Never having seen the disease, I perhaps might have prescribed bleeding, had not this been recently done without success, and had I not been previously warned by my correspondents, of the fatal consequences that too often followed this practice in influenza.

At Paris, as a friend informed me, the great mortality arose chiefly from venesection, to which the French are in many diseases too much addicted; and both at London, and at Edinburgh, it was said, bleeding had been attended with the worst consequences. Conceiving this to be a case of influenza, I determined not to have recourse to the lancet, but ordered a blister to be applied to the sternum, an enema to be administered, and small doses of Dover's powders to be given, both to promote perspiration, and by the opium it contained, to support the vis vitæ, and to relieve the cough. *Second morning.*—Blister rose well; pain of breast rather less urgent; pulse very frequent, and more feeble. Slight perspiration during the night. Had some sleep; enema operated once, and she had one natural stool this morning. Had I been aware that the moderate use of wine had been recommended by Dr. Wright, I should certainly have employed it; but I was intimidated from its use by the pulmonary complaints. Saw her again at night; breathing and pain of chest as in the morning; debility extreme. She expired easily towards morning.

The age of this patient no doubt was much against her recovery; but taking the rapid sinking of her strength, and other circumstances into consideration, I was of opinion, that the bleeding at least hastened her death; and I resolved not to employ the lancet in similar cases, should they occur.

A short time after this, the influenza became common in the town of Kirkwall, and spread with astonishing rapidity through the islands; but in rather a milder form than it assumed in France, London, or Edinburgh. It proved fatal however to several individuals, especially those advanced in life. A great many cases fell under my observation; and by prescribing, in the beginning of the disease, emetics of tartarized antimony, and afterwards by the use of gentle diaphoretics, calomel purges, and when the cough and pain of chest were urgent, blisters to the breast, I was so fortunate as to see all my patients recover; some of them indeed but very slowly, from the extraordinary debility which this singular disease induces.

The following accident first suggested to me a more

decisive mode of treatment, which I invariably found safe and advantageous.

From the smallness of many of the Orkney Islands, medical assistance can only be had by applying to the practitioners in Kirkwall or Stromness.

In an island four miles from Kirkwall, a boy of about thirteen years of age, was seized with symptoms of influenza; and to save the expence of calling out a medical man, his friends carried him by boat to town. When I was called to him, I found all the usual symptoms that characterize the disease, pretty strongly marked; and from his having been for a considerable time exposed to the weather in a cold, raw day, as well as from his having been during the passage accidentally wet with spray of the sea, I was apprehensive that the disease might terminate fatally; I therefore judged it expedient, both on my own account and theirs, to inform his parents, that my prognosis was unfavourable. But to my infinite surprize, he recovered more rapidly than any other patient in whom the symptoms were so violent. This led me immediately to bathe the arms, face, and hands of my other patients labouring under influenza, with cold water. This practice was to them highly grateful, and observing no bad consequences following, I thought myself justified, from what I had seen, to employ a more decisive application of cold to the surface of the body.

The first case I employed the cold affusion in was the following:

G. Mowat, aged sixteen, while attending some chemical processes, in which I was at that time engaged, complained of pain in his back, listlessness, vertigo, some pain in his breast, attended with fits of coughing, and such weakness that he was scarcely able to stand. His pulse was 110, and pretty strong; skin dry; tongue slightly furred; belly natural. Not having noted the degree of heat the thermometer indicated, I am unable to state it accurately; but it was somewhat above natural. These symptoms had not been so severe as to attract his attention, till on getting out of bed in the morning; but they were much increased since that time. (It was now one o'clock afternoon.) I ordered him to go home immediately, and prescribed gr. ijß. tart. antimonii, which produced copious vomiting. After the sweat caused by the emetic had entirely subsided, I directed two pails of cold water to be at the same instant dashed over his naked body. In two or three hours after the affusion I saw him again. The pain

of

chest was not increased, his pulse was reduced to 102; copious perspiration had come on, and he thought himself refreshed. Hab. haust. anodyn. cum gutt. xx. h. s.

Second Day. *In the morning*, his pulse 86; slept well; skin almost natural; tongue still a little white; repetat. affus. aqua frigid. As he had not had a stool, I prescribed gr. iv. of calomel. *In the evening*, the febrile symptoms almost gone; powder operated once; repetat. affus. et haust. h. s.

Next day he was able to go about; and the following one, insisted on assisting me in my experiments as usual.

Two or three days after his recovery, his younger brother, and a sister several years older, were both attacked with influenza; but their symptoms were more violent than his. A similar treatment was adopted in their cases; and so rapid was their recovery, that though it was not till early on Friday morning I was first sent for, when I came the following Sunday to enquire for my patients, I was surprized to learn that they had thought themselves so far recovered as to go to church; nor did this rashness induce a relapse.

The mother of the above patients, a short time after their illness, had the same complaint; but from peculiar circumstances that attended influenza in her case, I did not use the cold affusion. Her recovery was more slow than theirs had been. She had diaphoretic. mistur. which I never had occasion to prescribe where the affusion was employed; for perspiration soon followed its use, and the patient was relieved.

This plan of treatment I pursued in every other case of the disease that came under my care. The notes I took on two cases, one of a woman aged twenty-six, the other of a man aged forty-seven, which from the great violence of the pulmonary complaints that attended, were interesting, and in which I successfully employed the cold affusion, I have not at present by me. But in the extensive practice I then had in this complaint, I am enabled to state, that I never observed any bad effects arise from the employment of the cold affusion; and when used in the beginning of the attack, it was uniformly highly beneficial.

Some practitioners may contend, that this practice might answer in the comparatively mild form the influenza assumed in Orkney, but would have been improper in the virulent cases that occurred in many great towns; but from the experience of its safety by Dr. Wright, and the application of the tepid affusion by Dr. Currie, in his

own case (where his consitution was unfavourable to any practice that might tend to increase the inflammation of the lungs) and from the above cases, we may at least be tempted to try the effects of the cold affusion in influenza, if ever it should visit us again; a circumstance not unlikely, since about twenty-two years ago, it was, I believe, fully as prevalent as lately over Europe.

Your's, with respect,

Liverpool, July 10, 1805. T. S. TRAILL.

To the Editors of the Medical and Physical Journal.

GENTLEMEN,

PRESUMING on your indulgence and the honor you have done me by the insertion of my last, I now hasten to offer my sentiments respecting the propriety of establishing what I then proposed, a *Provincial Jennerian Circle.* Some of your correspondents resident in large populous manufacturing towns or cities may possibly differ with me as to the necessity of such an establishment, and be ready to observe, that a communication with the metropolis, per post, is open to every one, and that through this medium the faculty may, on the shortest notice, be expeditiously supplied. That these are blessings of considerable magnitude I am ready to allow, nay more, that I have repeatedly availed myself thereof; but let me not conceal what others, situated in similar remote situations, must be ready to acknowledge, viz. that disappointment from hidden causes already have, and may again hereafter occur; such, for instance, as the unavoidable exposure to the extreme variations of temperature from heat to cold; from the seal of office injudiciously applied; from mechanical injury done to the infecting medium, and from other equally obvious but unavoidable contingencies. It is not however to be understood, that the number of Public Institutes which are at present so happily established in various parts of the metropolis, ought to be lessened; on the contrary, it is much to be wished that those I am now alluding to, should, if possible, be considered as collateral branches issuing from, and connected with the *Royal Jennerian Society in London*, subject to similar rules and regulations, as professing to have one common object in view, the general

general diffusion of the *vaccine practice* and the total extermination of the *small-pox.*

In many small market towns similar to this, where population is extremely limited, it will not be in the power of any individual practitioner to keep a regular supply of *vaccine virus* for any considerable time together; but were five or six towns of this description adjoining each other to unite, so as to compose from their locality a kind of circle, it would be an easy matter for the respective individuals to keep up the vaccine inoculation for a limited time, and during such period, to furnish their professional brethren of the circle with a supply of recent vaccine fluid. The next member on the circle should in rotation likewise (for a period to be adjusted) keep up the vaccination, so as to complete the circle once annually. The medical few composing such a society, should have a president and secretary, to be appointed at their annual meeting; a collective list of patients vaccinated during the year should be published; vaccine topics only should be discussed, and such cases registered as the members might deem worthy of record. It would moreover be an easy matter under such an arrangment, when thought proper, for the practitioner undertaking to keep up the virus, pro tempore, to accommodate his right and left hand brother of the circle with a supply of cow-pox fluid in its most desirable state, viz. in that of its own *encircling cell* or *vesicle,* by meeting half way those vaccinated, and those wanting to partake of vaccination. These circles should be named from the district, wapontake, riding, or division, where placed, with the additional adjective *Jennerian,* both as expressive of their nature, and as a proper compliment due to the celebrated Discoverer. They should be under the controul of such rules and restrictions as the peculiar circumstances of the society might call for; and if the members composing such circles were to publish unitedly their intention of adopting the practice in their own provincial papers, there is reason to believe, that such a measure would greatly contribute to increase the public confidence. Some of your readers may smile at my proposal, and think it Utopian; I have conversed with a few of my neighbouring brethren however, who think otherwise; and whilst I am addressing and inviting them here to unite with me in carrying these proposals into effect, should the plan in your estimation seem calculated to meet a more public extension, you will do well to insert it.

This address might possibly be thought to have been subjected with more propriety to Dr. Jenner individually; I cannot however allow myself to doubt his readiness to acquiesce with any proposal that may contribute to disseminate a practice, the utility of which is incontrovertibly established, and which, from his own public example, he has uniformly promulgated and supported for a series of years with the happiest success. Before I conclude this letter, excuse me for observing, that unless the celebrated Discoverer had committed himself egregiously, in his nomenclature on vaccination, there can be no reason for endeavouring, by an assemblage of new coined terms, to supersede him in his technicals. *Vacciola, vaccæ vara, bounosus*, &c. are fanciful, unnecessary, and less descriptive of the subject than *variola vaccina*. *Vaccine matter*, a term unappropriate, and apt to mislead, since it is generally known and allowed, that a vesicle possessing its true characteristic, is distinguished by a clear transparent colourless fluid, and that in proportion as it acquires a degree of thickness and opacity, approximating to pus or matter, it should be avoided. *Vaccine fluid* is therefore more consonant and expressive. As well might any one indulge himself in new phrases as his fancy might dictate, and the term *equi-vaccine*, and its attendant compounds, might be rung upon in successive changes, ad infinitum.

Herschel has named the star he discovered *Georgium Sidus;* will any astronomer dispute the title, or fail to recognize the object discovered by the term employed?

Linnæus has given numerous titles to plants, founded on his sexual system, which a scientific botanist will scarcely depart from; and shall our Jenner have his *variolæ vaccinæ* trampled upon, to accommodate the visionary whims or conceptions of any one, merely, as it has been suggested, *Euphoniæ Gratia*. There is something sacred in the terms, as employed by the Original Discoverer, which even if barely intelligible, should not be exchanged; but when these are strictly proper, and explanatory, why should we quarrel with them, unless it be to set up an ostentatious and unmeaning display of our own classical acquirements.

I am, &c.

T. HARRISON.

Kirby-moor-side, Yorkshire, July 16, 1805.

Copy

Copy of a Letter from Dr. FRIESE *to* Mr. RING, *dated Breslaw, June the 9th,* 1805.

DEAR SIR,

THE unremitting zeal with which you have endeavoured to promote the Jennerian discovery in your country, and the interest you have so philanthropically shewn, on hearing of its first providential introduction into Silesia, will, I hope, excuse me, when I take the liberty to trouble you with some further account of the successful progress which that invaluable prophylactic has since made in this part of the Prussian dominions. Should the following Report be deemed acceptable to the Editors of the Medical and Physical Journal, I shall feel myself highly gratified, by adding, as a foreigner, some further proofs to the evidence, that vaccination, when properly managed, every where proves a permanent security against the small-pox

I could not but be astonished when I read over the pamphlets of Messrs. Goldson and Squirrel. I apprehend the alarm they excite, will come at too late a period for them to flatter themselves with much success. At any rate, I am convinced the new doctrine which they promulgate, will find but few proselytes in Germany; where both the government and the people are more and more sensible of the advantages of the new practice; and where similar equivocal arguments, advanced some years ago by the late Dr. Herz, Mr. Ehrman of Frankfort, and Dr. Mattreschka of Prague, have been silenced by time and experience.

You remember, perhaps, by my former letter, that there was also an adversary of some celebrity in Silesia, who rose up against the vaccine inoculation, at its first introduction into this country. His name is Mogallæ, a physician known in Germany by his very valuable writings on the several mineral waters, and bathing places of Silesia, and by some other works on the veterinary art; but I have the pleasure to inform you, that this respectable practitioner has been converted by reason and evidence into one of the warmest friends and promoters of vaccination. I must add, that it was particularly by his assistance, that we are now in possession of two public vaccine institutions at Breslaw and Glogaw; which are to be regarded as the centres from which the practice is spread, and continues to be spread, through every quarter of the province.

His Majesty has been graciously pleased to appoint me

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not only a counsellor of the medical department of Silesia, and a director of the royal institution at Breslaw; but he has also honoured me with the superintendancy of this new branch of the healing art, in the department of the royal chamber in this capital. The establishment of the vaccine institution has been so expeditious, that I found myself enabled to begin my operations on the 14th of April 1804, with equine matter, sent to me by my friend Dr. De Carro; from the very same source of which you have spoken in the Medical and Physical Journal for November 1804.

I have the pleasure to subjoin a statement of the number of persons inoculated at the Royal Institution of Breslaw, from its establishment till the present day; as well as a general abstract of vaccinations performed by different medical men in all the subordinate districts of the department of Breslaw during the year 1804, from the annual reports. I hope you will see by these lists, that the progress of the Jennerian inoculation, during the last year, has, by far, surpassed those of all preceding, since the year 1800. I do not yet know all the particulars of the results of vaccination in the second department of the Royal Chamber at Glogaw; but I am informed by private letters, that the number of persons vaccinated there is more than ten thousand; the total number of inoculations successfully performed in the last year, amounting to nearly thirty-four thousand; besides some thousands more, who have been vaccinated by surgeons of the army.

Government have pursued measures well calculated to promote vaccination. I have been charged with the commission of writing not only a popular publication on it, which is ordered to be printed, and distributed among all classes of people; but also a brief instruction for the physicians and surgeons of the province; wherein I have endeavoured to give an account of this new discovery; and also to acquaint them with the genuine and spurious pustule, and the best method of inoculating and treating the disease.

Another measure, not less favourable for promoting uninterrupted vaccination at the central institution of this city, is the grant of a sum of two hundred rix-dollars annually, destined for small premiums at a dollar each, to be distributed among children of the lower classes; who, for the sake of such a trifle, willingly comply with the rules and conditions of the inoculators. It must be particularly ascribed to this encouragement, that the institution

tion has been enabled constantly to provide, not only all Silesian inoculators, but also several of those of the adjacent countries, with fresh and genuine cow-pock matter, having disseminated during the last year 1312 armed ivory lancets of Dr. De Carro's invention.

Our mutual friend of Vienna has informed you, that several German clergymen have participated the labour of promulgating the new practice; and I have the pleasure to acquaint you, that there are many in this country likewise, who partake in this laudable design. I even venture to assert, that there are some country clergymen in our province, who are so well acquainted with vaccination, both in theory and practice, and have conducted it with so much care and skill, as justly to deserve the name of benefactors of their parishioners. In order to acknowledge the merits of these respectable divines, and at the same time to excite a laudable emulation among all the medical men of the province, government have granted to several of those who have distinguished themselves in this line, small premiums from thirty to fifty rix-dollars.

I find by the Medical and Physical Journal for March 1805, that Mr. Goldson is indefatigable in promulgating his cases of small-pox subsequent to vaccination; having just published a second Treatise on that subject. I have perused the last numbers of this before mentioned Journal, as well as several other reflections written on this occasion. I wonder that there are people who think the punishment you and Dr. Walker have inflicted on Mr. Goldson, in your answer to his productions, too severe; nay, that there are some anonymous writers, who pretend Mr. Goldson's pamphlet is entitled to the most serious attention of the faculty. For my own part I cannot find, that the evidence of cases related by him, in order to prove that vaccination affords no permanent security against the infection of the small-pox, is so clear and satisfactory as they pretend. I shall have an opportunity of communicating to you some similar cases, which happened in Silesia; but I assure you, that after due inquiry, this could not in the least degree alarm the public, who, on the contrary, have every day had the satisfaction of seeing, that the cow-pock is the only powerful and permanent preventive of that dreadful scourge of mankind, the small-pox; which, in the course of the last year, destroyed several thousands of the rising generation, while those who had regularly undergone the operation, remained secure from its malign influence and its dreadful contagion.

Few

Few mistakes indeed have lately been committed here, in the practice of vaccination. I shall mention one which occurred in the year 1802, at Brieg, a city, six German miles from this place. Mr. Taber, a surgeon, inoculated several children from the arm of a child, in whom the pustules were already approaching to the scabbing process. He confesses, he had at that time never seen the process of genuine pustule. This occasioned him to mistake the ulcers produced in the arms of these children, for the true kind; and to transfer from them a spurious and purulent matter to thirty-one persons, thinking they would all be perfectly secured by this operation. The small-pox, however, making its appearance at Brieg the next summer, three of the children were attacked by the disorder. At length perceiving his error, and having in the mean time acquired a sufficient knowledge of the true progress of vaccination, he inoculated the rest of his patients with genuine matter; in consequence of this, they all took the disease in the regular way, and resisted the small-pox; which then committed great ravages among those children, whose parents had refused the benefit of vaccination.

I cannot omit stating three other cases of supposed vaccination, in children of one family, in a village in this neighbourhood; which greatly resemble those in Fullwood's Rents, and at Kensington. One child, a girl of four years, was attacked four weeks after by the confluent small-pox. I saw her on the 11th day of the disease; and, as the physician who had performed the inoculation, and accompanied me, confessed he had not had an opportunity of observing its progress, I immediately tried a second vaccination on the two remaining boys, who had at this time large greenish and irregular scabs on their arms, remaining from the first operation. The inoculation succeeded, and on the 6th day two flat vesicles appeared, with the common depression in the centre; but on the 7th day, they both became feverish, and on the 9th a small crop of distinct variolous eruptions broke out. The father, though a common country labourer, reproached himself for having delayed a second vaccination of his children; who, by rubbing and scratching the pustules, had entirely destroyed the progress of the first.

Accept my best thanks for the honour you have done me, in the second Volume of your excellent work; and believe me to be, With great respect,

Dear Sir,

Your obedient humble Servant,

D. FRIESE.

To the Editors of the Medical and Physical Journal.

GENTLEMEN,

IF you judge the following case merits a place in your excellent Journal, you will have the goodness to insert it. On Friday, July the 12th ult. I was called to attend S. L. a remarkable, active, intelligent child, three years and a half old; who on Monday the 8th preceding was suddenly indisposed with pain in his head, attended with vomiting, and purging, of a green and bilious matter, which ceased at night, and was succeeded by obstinate costiveness; the pain in his head continued, accompanied with fever and delirium to an alarming degree, the two preceding nights and days prior to my being called in, which left him extremely dull and heavy, with interrupted sleep, and a continual melancholic whine, with alternate general and partial flushing. His pulses were quick, small, and intermitting. His urine very scanty, not a small tea cup full in twenty-four hours. He constantly laid in one position with his right arm over his eyes, and with his left he continually kept feeling on the centre of the left parietal bone. His thirst was considerable, but he refused every kind of drink except water. From the beginning to the end of his indisposition he had no appetite; and added to the above symptoms, he had a slight cough some days previous to his being indisposed, which became so violent as to threaten suffocation. Judging the case to be hydrocephalus internus, calomel was prescribed in sufficient doses to evacuate the bowels, with a fever mixture, and blisters to his head and neck for three days, without any relief. On the 10th, lifting up his arm, and examining his eyes, I found both the pupils very much dilated and unirritable. He could not bear to have his head lifted from the pillow, or to be turned in the bed; on which I concluded it a lost case, and informed the parents I considered it as such; but they, being extremely anxious for the child's life, made me determine to adopt some other mode of treatment, with a view if possible to accomplish a cure. I accordingly made trial of digitalis; (but for the hint I must acknowledge myself indebted to the writings of that eminent philosopher and physician, the late Dr. E. Darwin, whose saturated tincture I used, with the compound tincture of lavender, to cover in some measure its disagreeable taste);

taste); the following is the formula I tried: Tinct. lavend. comp. tinct. digitalis aa. ʒij. M. Wishing to have the constitution under the influence of the digitalis as soon as possible, ten drops were given three times a day, in a glass of water, increasing three or four drops each dose every day, until twenty-four were taken ter in die. But as the digitalis affected the bowels by producing too many motions; fearing it might debilitate too much, I thought proper to restrain the frequency of that operation; and for this purpose, prescribed the following drops: Tinct. opii camphorat. ʒiijſs. tinct. opii ʒß. M. cap. coch. j. parv. omni nocte, which had the desired effect. No visible alteration for the better was perceived till the 19th, (the fourth day of using the drops) when the pupils of the eyes were less dilated and became irritable; the cough very much abated; the urine more plentiful with a copious perspiration; the pulse slower and uninterrupted; sleep refreshing. My patient now became cheerful, called for his *play-things*, and toasted bread buttered, which he ate. The digitalis was continued till the 22d day, when every former symptom vanished; and as only debility remained, a bark mixture with the night drops were given four or five days and nights longer, which completed the cure.

This case I have faithfully described, and as accurately as I possibly could, with an anxious wish, that, for the benefit of the little sufferers who labour under this obstinate, and generally fatal disease, and for the comfort and happiness of their parents, a more extensive trial may be given to digitalis, with a cautious but spirited hand; as it seems likely to prove a very efficacious remedy in hydrocephalus ex et internus, when not arising from any mechanical obstruction of the brain. I think the modus operandi of digitalis bespeaks its efficacy, for as it powerfully retards the circulation of the blood, it thereby prevents the hydrocephalic fluid from increasing on the brain; and which also gives time for the absorption of what may be already accumulated; while the nausea, which it seldom fails to keep up in well regulated doses, oftentimes produces a copious diaphoresis; and it always causes alvine and urinary evacuations. It evidently acted in this case as a sudorific, cathartic, and diuretic, and in my opinion, by that means so effectually and speedily carried off the disease. It could not be the blisters, as they did not vesicate; nor the calomel, as its effects produced no alteration. My practice being considerably among children, I intend to make farther trials with the digitalis as cases may occur, the

the result of which, (if this be accepted) I will communicate for general good; fervently hoping it may stimulate others to do the same. I am, &c.

Mattishall, Aug. 7, 1805. LIONEL PALMER.

On the Use of Aqua Frigida in Scalds, in epidemic Ophthalmia, and the Effects of Cold on the human Body. By Ralph Cuming, M. D. *of His Majesty's Ship Pegasus.*

To the Editors of the Medical and Physical Journal.

Gentlemen,

IMPRESSED with an idea that time can never be better employed, than when devoted to alleviate the sufferings of our fellow creatures; I have to request, that you will be pleased to insert the following paper in your very useful Miscellany, which I hope may have some share in being conducive towards the attainment of so desirable an end.

On the treatment of scalds and burns, there seems to have been great diversity of opinion, if one may be allowed to judge, from the different modes of cure which have been adopted by medical practitioners. The oleum lini sine igne, was formerly in high repute, and generally speaking, had, and still is allowed to have, by many, the precedence as a remedy; in recent cases, some mix with it aq. litharg. acetata, aq. calcis, &c. Vinegar used to be in high estimation at Edinburgh. All of these, and many other applications, I have seen tried; but as far as my observation leads me, they are not nearly so beneficial as the plan which I have for some time accustomed myself to; and I may say, most successfully; if speedy relief from pain, and a quick and easy cure, are desiderata claiming the attention either of the patient or medical attendant. I have read and heard much about the efficacy and superior power of ol. terebinthinæ in those cases, but all that I know of it is from report, for I never could make up my mind to use it, conceiving that it was a harsh remedy, and such an one as was calculated to produce exquisite *torture;* on this ground alone have I rejected it, as it has the appearance of applying one species of fire to extract another.

Aqua

Aqua frigida is my favourite remedy; in the last ship I served in, the Malabar, one of the Serjeants of Marines, as well as many others, were scalded; but as this was the worst case, I mention it in preference; part of the leg, the whole of the ankle, and upper as well as lower part of the foot, were vesicated. He was ordered to place his foot and ankle in a bucket full of cold water, which immediately relieved him from the excruciating pain he complained of; and when it became necessary to renew the water, which was frequently done when it acquired a certain degree of heat, imparted to it from the inflamed limb, he was extremely anxious for a fresh supply, for the colder the water was, the more comfortable he said he felt the part affected.

At night, nurses were appointed to attend him, with directions to apply linen rags dipped in water, and to renew them whenever they became warm; and this mode of treatment was persevered in until the inflammation subsided. Some of the vesications were very large, and by way of experiment I cut one of them through its whole extent, but found this part more difficult to heal than the others, from which the serum was evacuated by small punctures.

But after the inflammation is repelled, or I would rather say, superabundant heat is extracted, it is evident that something still remains to be done, as loss of continuity, or destruction of organization, must always take place, in a ratio proportionate to the extent of the injury sustained; which, sometimes, is so slight, as only to affect the epidermis and rete mucosum; but when the cutis vera is affected, a suppurative process must take place before the cure can be completed, and I found it necessary in this instance to apply emollient and warm cataplasms for that purpose; after which the cerat. lapidis calaminaris completed the cure in a few days, which would in all probability have been protracted for several weeks, had not the inflammation been speedily subdued by the refrigerating method.

It will appear from analogy and observation derived from our knowledge of the cure of all inflammatory complaints, that the above modus curandi is not the chimerical product of theoretical disquisition, or fanciful hypothesis; no, quite the reverse, for it has the support of successful experience, deduced from the principles of natural philosophy. And by way of illustration I shall beg leave to mention, that, lately, whilst cruizing off the Dutch Coast,

several of our men were affected with ophthalmia epidemica, occasioned, as I suppose, from the cutting easterly winds, which at that time prevailed; their eyes were much blood-shot, and the tunica adnata so much distended in two or three instances, as to protrude beyond the edges of the tarsi, yet almost all of them recovered in the course of three or four days, without having recourse to any *general* treatment; aq. frigida incessantly applied, effected a cure tute, celeriter et jucunde.

The effect of cold water on the human body is a subject which, if rightly understood, would throw great light on therapeutics, and so illuminate this path of medical science, as to enable the physician to divest his mind of much anxiety and diffidence; but when things of importance, from a want of knowledge respecting their action, are not properly understood, every one is left to form his own opinion, and wander in the mazes of doubt and uncertainty; and that theory which is most plausible, supported by reasoning, deduced from observation and well attested facts, will be generally received. Hence arises that difference of opinion, which prevails among medical authors. Cullen says, "Cold is *always* more or less *directly* sedative; but it is equally manifest, that in *certain* circumstances cold proves a stimulant to the living body, and particularly to the sanguiferous system." This account of it is both inconsistent and unsatisfactory.

Brown's opinion is, that it always acts as a debilitating power, and observes, "Because cold is so efficacious a remedy in the small-pox, it clearly follows that the use of cold should be extended to the whole range of predisposition, the whole circle of diseases depending upon sthenic diathesis."

Cold, according to my opinion, may be said to have a twofold effect, for its action on the human body must depend upon the mode of its application, and will be modified agreeably to the temperature of the body. For instance, cold is always grateful to a person when oppressed with heat, giving vigour to the relaxed frame; it acts in a manner diametrically opposite, when the body is cold, and circulation languid; and is the source of a variety of diseases. So that to say cold is either sedative or stimulant, or immediately debilitating, does not appear to me to convey a correct notion of its real properties and effects. Let us suppose that the heat of the body in a state of good health is at 90°; if cold should be applied sufficient to reduce it to 60°, the constitution will suffer in proportion

to the time of its application, and susceptibility of the body. On the other hand, we shall imagine that the heat rises to 110°, and the application of cold reduces it to the standard above at 90°, it is evident that in the one instance it will produce debilitating, in the other tonic effects.

What I have said on this subject may be considered as being too concise to render it worthy of much attention, but facts will ever have a proper influence on the mind of the man, who, from the purest motives, is in the habit of contributing his mite of knowledge to the general stock; who, fearless of reproach, and buoyed up by conscious integrity,

> Can laugh at envy, nor foul censure fears,
> Nor dreads the fabric that vile Mendax rears.

Harwich, August, 4, 1805.

To the Editors of the Medical and Physical Journal.

GENTLEMEN,

I Have to acknowledge the very liberal and handsome manner in which Mr. Dawson has offered some "*conjectural remarks*" on the singular case of delivery, inserted in page 17 of your present volume; and you will permit me to observe, that if every controversy, of which your Journal has been made the vehicle, had been conducted with the same modesty and candour, some of your readers would not so often have been offended with the rancour and asperity which have distinguished, as I think, too many of the papers in your valuable miscellany.

It is often difficult, and frequently impossible, to describe on paper every *feeling* and *appearance* we notice, in cases which it may be proper to lay before the profession, either as curious for the physiologist or useful to the practitioner, so as to make them clear to every perception. To this source I must attribute the misconception of Mr. Dawson, and the erroneous conclusions he has deduced therefrom. His argument is founded in error, from not considering the exact point from which the examination was first made; he does not seem aware that when I described the tumour, "as covered by an unusual substance, which felt *soft* and *uneven* to the touch," that my

my finger was then, *unknowingly*, in the *rectum*; and had he proceeded a little further on in the page, he would have found an insurmountable difficulty to his *conjecture*; viz. "that the projection of the *os sacrum* was much more plainly perceived than in a common examination;" and still lower in the same page, he would have seen all the obscurities of this first examination, "*satisfactorily accounted for*;" and *why* the os uteri and arch of the pubis were not to be felt. If this conjecture of Mr. Dawson's could be for a moment allowed, it would not, in my opinion, afford any of the criteria of this examination; the tumour was hard, very hard, allowing me to press on it firmly, so as to be satisfied it was the head of a child; and I could, through the *soft* and *uneven* covering, indistinctly feel the yielding of the bones. Had this been "*an unnatural thickening and resisting state of the membranes investing the fœtus*," the perception to the finger must have been very different; this "unnatural thickening and resisting state," would have prevented my so readily feeling the head, and the membranes would then have felt *firn* and *smooth*, instead of *soft* and *uneven*. Again I urge, if it was this unnatural membrane I was pressing against, it must either have been high up, or low down; if the former, there would have been no difficulty in finding a pervious vagina; and if so low down as "completely to wedge up the vagina," how could the projection of the sacrum be so easily perceived, as to make it a prominent difficulty in ascertaining the nature of the case?

Now all this *conjectural cloud* vanishes by taking the case as it really stands described and explained in my last. The soft and uneven substance is there stated to be the *intestinum rectum et vagina*, and the difficulty in not being able to discover the *os uteri* and arch of the pubis is clearly accounted for. Mr. D. also *conjectures*, that if there had been an adhesion of the sides of the vagina, "there would, at least, have been half an inch in length of the sides of the *vagina* united;* this I am not disposed to controvert, but who will say, what alteration might have taken plase in this thickening, between Tuesday and Friday, during the whole of which period the

* It is more than probable that the projection mentioned, p. 18, as immediately under the meatus urinarius, might be this thickening and adhesion, and which is described as a "pushing out of a portion of the *vagina*."

woman was in strong labour, and for a great part of the time the head of the child was within the brim of the pelvis, and of course, the vagina greatly on the stretch? But there is still an insurmountable difficulty to Mr. Dawson's *conjectural hypothesis.* He does not take into consideration, that Mr. Cooper was with this woman before I saw her; indeed, he was there early in the beginning of labour, and it is stated (page 18) " that the same circumstance" (passing the finger into the rectum, and not being able to find the vagina) " had constantly happened to him during the whole time he had attended her." Now, how comes it to pass, that this " surprisingly thickened, unnatural, and resisting state of the membranes," should obliterate the vaginal aperture in the beginning of labour, and before they could so " *completely wedge it up as that the hand could not pass?*" Alas! Gentlemen, how may a little fanciful conjecturing bewilder the imagination!

Mr. Dawson also *conjectures,* that the woman seemed to have no suspicion of the circumstances attending her case. She certainly was all along aware of it, and it was to this alone that the mother alluded, when she replied to Mr. Cooper's enquiries, that her " *daughter was just as she was,*" in point of delivery.

Having thus noticed the principal points of Mr. Dawson's conjectural observations, permit me to encroach a few moments on your patience, while I retrace, as shortly as possible, the leading features of the case, and which will tend to show, that we could not be so egregiously mistaken, as we have been supposed by Mr. D.

When Mr. Cooper visited her, under a suppression of urine, and passed the catheter, he " *found*" the vagina impervious; Mr. Dawson has it, " he *fancied* the vagina to be impervious." Here, indeed, Mr. D's candour and fairness seem a little to have forsaken him. When again Mr. C. was called to attend her in labour, he went with his attention roused, as to how this curious business would terminate; and he then found, as he had heretofore repeatedly done, the vagina impervious, or rather obliterated as to any external opening. He, however, waited a considerable time, to see if the parts would so far dilate, by the natural efforts, as that he might find an aperture; and seeing no probability of her being relieved without assistance, he requested Mr. Tice's and my attendance; and had the precaution to let us form *our own* opinions of the case, from repeated examinations, (that we might be unbiassed) before he related to us what he had before known

and

and observed. Had we even rested here, and trusted to the *touch*, it might have been said we were deceived; but that nothing might be left undone to acquaint ourselves with the exact nature of the case, the parts were submitted to *inspection*, under the most favourable circumstances, and yet no opening found. To suppose that three professional men, thus situated, who were extremely cautious and circumspect, could *possibly* mistake "an unnatural thickening and resisting state of the membranes" for an imperforation* of the vagina, appears to me a very uncandid conjecture; and I must be free to say, had that been really the case, neither of them were fit to practice; for I cannot accept Mr. Dawson's apology, p. 155, that it was, "a misapprehension, which, under similar circumstances, many men might have fallen into," as, in my opinion, no person, possessing proper intelligence for practice, could possibly have made so palpable a blunder.

From this short recapitulation, and a re-perusal of the case in question, I think it will appear, that it was, as I have related it, an *imperforated vagina;* arising most probably from ulceration and adhesion after the former labour. Had the case indeed occurred to me in my private practice, and not witnessed by an other practitioner, I certainly should not have ventured to brave the scepticism of my brethren by a description of it; but, supported as I am, by the opinion and presence of two such respectable and intelligent coadjutors, I feel confident, whatever my individual deficiencies may be, that these Gentlemen were not likely to be easily deceived, and that too by such an appearance as Mr. Dawson conjectures and describes. Whatever deception we may have laboured under, it was assuredly not of the nature which Mr. D. has pointed out, as, independent of my relation of the case not being at all adapted to, or to be explained by, this conjecture of his, I have no hesitation in affirming, we could not thus have been deceived; for, in that case, as in all others of *protrusion*, whether of the membranes, the occiput, or the nates, do we not instantly perceive, "notwithstanding the vagina may be completely wedged up," the *margin* of the stretched os externum? yet, in this case, the external parts were scarcely altered, except by that small projec-

* I use the word imperforation, (though not strictly proper) as being concise and expressive.

tion mentioned in page 18, and alluded to in the first note of this paper.

Having trespassed so long on your time and attention, I shall refer Mr. Dawson to the conclusion of my former letter, respecting his observations on the escape of the *liquor amnii*, and the menstrual discharge; they are such as must strike every intelligent physiologist; they were what occurred forcibly to our minds at the first; and in drawing up the case, I carefully avoided "*wandering into the regions of* CONJECTURE, *or building a principal hypothesis on so uncertain a foundation.*" I shall pursue the same course at present, with this single observation, that it was *our conjecture* at the time, that there was, although we had not discovered it, some connection between the rectum and vagina; but as we had no opportunity of examination post mortem, I purposely avoided all *conjectural reasoning*, and presented such facts and appearances only, as were really observed, and for the accuracy and fidelity of which we could readily vouch.

With again acknowledging Mr. Dawson's candour, and assuring him that no offence can be taken at the manner in which he has brought it forward, I shall take a *final* leave of the Controversy, with requesting the Editor to correct a verbal inaccuracy in page 18, l. 15, where, for "a year before," read nine years before.

I am, &c.

W. O. CRIBB.

Bishops Stortford, Herts, Aug. 3, 1805.

The following Letter from Mr. Cooper, will throw some additional light on this curious Case.

"Dear Sir,

"I have carefully perused your answer to Mr. Dawson's *conjectures*, in the last Number of the Medical and Physical Journal; and have only to add to your very satisfactory Reply, that I had more than once an opportunity of examining the vagina, when the woman was not in a state of pregnancy, and could never pass my finger, although I repeatedly attempted it, and which could not, then, have been caused by any 'thickening or resisting state of the membranes;' and this I did by desire of the mother, who always when I saw her, or at least if her daughter ailed any thing, was very solicitous about her on account of this, her unnatural situation, as she always thought it. Neither was the woman herself, or the husband,

band, ignorant of it; and if it was consistent with *delicacy* and *propriety*, to publish what *is known* on this head, it would place the case beyond the possibility of doubt. When I first saw her in labour, I could find no aperture to the vagina; and the *readiness* with which the finger passed into the rectum, is not to be conceived but by those who had an opportunity of examining; at this period the head of the child was not to be felt, it had not passed within the brim of the pelvis; when it did, the examination *per anum* soon discovered it.

"I am sorry that Mr. Dawson's exuberant imagination should give you so much trouble, as I think your former paper on this subject sufficiently clear and explicit.

"I am, dear Sir,

"Your's, truly,

Much Hadham, Herts, Aug. 5, 1805. "G. COOPER."

"To Mr. CRIBB."

CASE OF DISEASED BRAIN; *by* Dr. JOSHUA E. WHITE, *of Savannah, in the State of Georgia.*

[Extracted from the New-York Medical Repository]

ON the 21st of May last I was requested to see a mulatto boy, between seven and eight years old, who had been long sick, and of whose case I received the following history, as nearly as could be recollected by the mother.

In the summer of 1802 he had the measles in a favourable way, previous to which he had generally enjoyed good health. From this time he frequently complained of head-ach, would leave school in consequence of it, and had so constant a disposition to sleep, that it was with much difficulty he could be prevented from yielding to it, or be prevailed on to enter into the common amusements of children.

He was listless, inactive, and stupid. These symptoms continuing, in about two months he was attacked with convulsions at irregular periods. Almost every day he had one or more fits; and if sitting in a chair, would not fall, or, if near a wall, would support himself while it lasted, which was seldom more than two or three minutes. They generally terminated in sleep, for one, two, or three hours. The drowsiness continued to increase, and it was remarked to be always greater in the afternoon and evening. He

would frequently cry without any apparent cause. His appetite, during this period, was irregular, sometimes voracious. About six months ago a swelling made its appearance about the junction of the right parietal with the occipital bone, which, gradually increasing, was, when I first saw him, about the size of two thirds of a billiard ball. It was soft, yielding, and elastic, and there was evidently a loss of substance in this part of the cranium. A smaller one was afterwards observed immediately behind the right ear, which had been noticed to increase slowly, while the other as gradually decreased, so as to induce a belief of a communication between the two. He was now observed to squint, the pupil of the right eye became dilated, and he gradually felt the motion of the right arm and leg to be diminished.

During the progress of these symptoms his head was noticed to grow much faster than any other part of his body; and a hat of the usual size for a boy of his age was much too small for him. It was, in fact, so remarkable, that among his play-mates he had the nick-name of big-head.

Upon my first visit, the most striking appearance which manifested itself was the unusual dimension of the head, particularly in a transverse direction. The pupil of the right eye constantly turned towards the nose, and was less sensible than the other to the rays of light. His skin was generally hot, pulse quick and somewhat tense, thirst considerable, and bowels very costive; appetite impaired, and he had a constant propensity to sleep. The right side was paralytic; the arm and leg of that side were much less than of the other; and he had almost constant convulsive twitches in the right hand.

From the duration of his disease, the emaciated state of his system, and the symptoms denoting an organic affection of the brain, I had no hopes of a recovery. He was blistered; calomel was given in small doses, so as to affect the salivary glands; and it likewise acted briskly on the bowels. The salivation was continued for a fortnight, without effecting any change in the principal symptoms of his disease. There was now a discharge of puriform matter from the right ear, which, the mother says, possessed the singular quality of proving instantly fatal to flies which accidently lodged on it, insomuch that several have been found dead at one time within the concha and helix. He continued to have a return of convulsions almost every day, and in three weeks from the time of my first seeing him he expired.

Permission being obtained, an examination was made of the head on the morning after his decease, with the assistance of Dr. John Grimes.

From the upper part of one ear to the other, in a transverse direction, his head measured seventeen inches, and from the lower edge of the occipital to the same part of the frontal bone, sixteen inches.* After sawing through the cranium we found a firm adhesion of the dura mater to almost the whole inside of it, but particularly to the left parietal bone. The two anterior lobes of the brain were in a healthy state, but the left middle lobe, and the right posterior one, presented a remarkable appearance, totally different from the other contained parts. Perhaps no better idea can be formed of the change which had been produced by disease, than by comparing them to firm cream-coloured cheese. Between the cranium and the right posterior lobe was found some well-formed pus, and the skull was here thin and spongy. The ventricles contained about half a gill of dirty-coloured serum. In the other parts of the cerebrum, and in the cerebellum, no unusual appearances or marks of disease were discovered.

REMARKS.

The diseased appearance of the two lobes which I have mentioned, evidently denoted them to be in a state of schirrosity; and this idea will gain additional weight by attending to the first symptoms and the progress of the disease. That the extraordinary induration was the effect of previous inflammation, I have not a doubt; and I am still more confirmed in the belief, from the very firm adhesion which was observed to have taken place between the cranium and dura mater, which we know can only arise from inflammation, and which we also know does not exist in health, except at the sutures and the basis of the scull.

It would seem, from the long continuance of the symptoms, that the inflammation partook of that kind which Dr. Cullen calls chronic, which by Dr. Brown has been called asthenic, and which has been named phrenicula by Dr. Rush.

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* A more correct idea may be formed of the preternatural size of the head, by comparing the dimensions of his with those of a twin sister. From ear to ear her head measured twelve inches, and from the upper part of the nose to the inferior edge of the occipital bone, fourteen inches. Thus it will be observed that, in the first way, his head measured most, whereas in her's it measured the least.

Rush. The researches of the last-mentioned gentleman into the nature and causes of the internal dropsy of the brain, have been so far successful as to place the matter beyond a doubt, that this formidable disease is always the consequence of a primary inflammation in the brain, whatever may have been the direct or indirect causes of this inflammation.* The indirect cause of the congestion in the brain, in the above case, I believe to have been the measles.† As has been already noticed, he complained of his head from the period of his recovery from them; and perhaps the reason why water was not effused within the ventricles, and hydrocephalus formed, was owing to the less active nature of the inflammation than we know to be necessary for the effusion of water. A similar fact is often witnessed in other glandular parts of the body, particularly the liver and spleen; and is it not probable that the swelling of the joints in chronic rheumatism is the effect of a like action in the arteries of the part? It has also been remarked, that some pus was found between the cranium and the right posterior lobe; and for the formation of this, it is a fact well established, from the knowledge which we possess of the actions and powers of an animated body, that a certain degree of inflammation is indispensably requisite.

If the morbid appearances of the brain, on dissection, in the case just related, tend to elucidate the primary cause, and if the deductions I have drawn be admitted as correct, we shall have advanced a step further in our knowledge of that disease which has been generally known by the name of the internal dropsy of the brain, but which has been more properly called, by Dr. Rush, "chronic apoplexy." They will also aid in establishing the theory, as suggested by that gentleman and Dr. Quin; and, by attending to it, will consequently lead to a more successful practice than unfortunately has hitherto been exhibited in that formidable malady.

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* Hence we see the impropriety of naming a disease hydrocephalus where no water is found on dissection.

† Several authors have mentioned instances of hydrocephalus consequent to eruptive diseases; and Dr. Rush says, he has seen a case in which it was evidently the consequence of the measles.

To the Editors of the Medical and Physical Journal.

GENTLEMEN,

THINKING the following account of the second appearance of small-pox in the same person, interesting; and that it may be usefully conveyed to the public, through the medium of your very useful and widely circulated Journal, I have sent it for insertion.

East Street, Manchester Square, August 18, 1805.

I am, &c.
EDW. LEESE.

Mrs. PIDGEON, now living as housekeeper with Mr. Jarnet, of David Street, the corner of East Street, Manchester Square, informs me, that her late husband, when about fifteen years of age, then residing as a gardener with Capt. Markall, of Honiton, in Devonshire, was inoculated for the small-pox by Mr. Buller, a surgeon of that place; that his arm inflamed very much; and that it was succeeded by a thick eruption of pustules all over his body; his feet were so full of them, that he was unable to stand. He was for some time confined; and so very ill, that Dr. Robinson, of Honiton, was requested to see him. Dr. Robinson and Mr. Buller continued to attend him till his recovery, without expressing any doubt of the nature of the disease.

Some years after, a lad in the same family had the small-pox; during the progress of which, he slept with him, and received no inconvenience.

When he was about twenty-eight years of age, his own children had the small-pox; of which one died. He lived and slept with them, and at length was taken so ill, as to be obliged to leave his work; complaining of sickness, head-ach, and severe pains in his back. The medical gentleman attending the children was sent to; who observed, that if he had not already had the small-pox, he should think he was then sickening for it. After an illness of two or three days, a full eruption of the confluent small-pox made its appearance; of which he died, on the eleventh day.

To Dr. BRADLEY.

Sir,

FEW complaints are more painful in themselves, or more alarming in their consequences, than suppression of urine. The bladder will, in some instances, bear to be distended to a prodigious size with impunity, as we daily see in persons accustomed to swallow large quantities of liquor; while in others, if the effort to evacuate its contents be resisted but for a short time, the consequences are alarming. The greatest quantity of urine, which I recollect to have heard contained in the bladder, is mentioned in the 47th volume of the Journal de Medicine, where it is said, that sixteen pints of urine were drawn off by the catheter, in the space of two hours. The patient recovered, and in the course of two months the bladder had regained its proper tone. In some cases, when the distension has proceeded to a certain degree, Nature relieves herself for a time, though without removing the complaint, by exciting a flow of urine which deceives the bye-standers, and sometimes misleads the physician. This insidious case has been well described by Mr. Hey of Leeds, in his valuable Observations in Surgery; and the following instance, which occurred to me a short time ago, shews how cautious we ought to be in receiving the report of a nurse or a friend.

April 7, 1805, I was desired to visit the daughter of Philip Miller, living in this neighbourhood, and about ten years of age. I found she had, about a fortnight before, been affected with measles, from the effects of which she had not recovered. The cough continued severe after the eruption had disappeared; and in consequence of imprudent exposure to a cold wind, she was seized with symptoms of pneumonia. At the time of my visit, the cough was very troublesome, attended with much difficulty of breathing and acute pain in the chest. Her face was much flushed; her skin hot and dry. The pulse vibrated between 130 and 140 strokes in a minute, and was feeble; the tongue was brown and parched, with great thirst and restlessness. As she was costive, a few grains of calomel were ordered; a large blister was applied to the chest, and a mixture with spt. æth. nitros. was ordered occasionally, with an anodyne at night.

On

On the following day I found her in every respect much worse, though it seemed as if the blister had relieved the pain and extreme difficulty of breathing. She lay in a comatose state, and had much the appearance of a person in the last stage of typhus. Her pulse was above 140, and very feeble. The calomel had operated, but the stools and urine were passed involuntarily, and the latter in large quantity. As I considered this to be a hopeless case, and she lived at some distance, I did not see her for several days; but was informed by frequent messages, that no material alteration had occurred in the symptoms.

On the 17th, I was desired to visit her again; and to my astonishment, found her perfectly sensible, which had been the case since the preceding afternoon. In other respects, the symptoms were nearly as before, except that the cough and pneumonic symptoms had left her. She was still hot and restless; her pulse 160, and small; and she complained of very great thirst. Her thighs and labia pudendi were marked with black suffusions, and she observed that her belly felt hard though not painful. Upon examination I found a hard tumour extending from the pubis to above the umbilicus, and introducing the catheter I drew off three pints of dark coloured urine resembling coffee. I now found upon inquiry that she had made no water from five P. M. of Saturday, the day before my first visit, until four P. M. of Monday, some time before my second visit; but as she had experienced no uneasiness, and made no complaint, this circumstance was not noticed; from that time, however, the urine had continued to flow in such quantities that they could scarcely keep the bed dry. The child had experienced no pain from the distension, and felt no particular relief from the evacuation. Before my departure I explained the nature of the complaint, and desired that I might be informed if she continued unable to void her urine.

I heard nothing until the 19th, when I was told that she had made *plenty of water* since my visit, and had made at once about half a gill into the pot; but that she had been seized in the morning with a pain above the pubis, so acute as to cause her to scream aloud. I therefore desired that she might be put immediately into a warm bath, and if not relieved, that it might be repeated every three or four hours; ten drops of tinct. opii were ordered to be given, and a large blister to be applied to the hypogastrium. In the evening I found the pain considerably abated

abated, but the bladder, notwithstanding the assertions of the parents, was distended nearly as much as before, and I drew off a full quart of dark coloured and extremely fœtid urine, containing a copious puriform sediment tinged with blood. Her pulse was still 160; she complained much of thirst, and had during the afternoon several fainting fits. The warm bath was ordered every night, and one of the following powders every three hours: R. Pulv. digital. gr. iij. pulv. uvæ ursi ʒj. ft. pulv. viij.

20th. In the evening I drew off with the catheter three large cups full and a half, amounting to about a pint, of dark coloured urine; the last half cup full appeared to be pure blood. In other respects, the symptoms were as before, except that she was perfectly free from pain. Her urine still flowed involuntarily.

21st. She passed a bad night, and experienced frequent attacks of severe pain above the pubis, with a painful desire to make water, which forced away several clots of blood, accompanied with gushes of bloody coloured urine. No tumour being perceptible in the hypogastric region, I did not introduce the catheter. Her pulse continued at 160; her features were much contracted, and the debility was so great as scarcely to bear removing into the warm bath, which was however ordered to be persisted in. The powders produced no nausea. For her thirst, which had somewhat abated, she took eagerly of milk, which had been her sole support during her illness.

22d. She appeared more composed this day, and had passed a better night; her pulse was about 140, and her skin felt cooler. The urine still continued to flow involuntarily, but about two ounces were passed at once of a pale red colour and offensive smell, but with somewhat less sediment.

24th. The urine has continued to flow involuntarily, but a few spoons full have been occasionally voided into the bed pan; it had no appearance of blood, and appeared nearly of the natural colour; the sediment also was much less. The warm bath was directed to be repeated every night, and the powders to be continued.

I did not see her until a week afterwards, when she had regained, in a considerable degree, the power of voiding her urine, which was perfectly natural in colour and had only a strong urinous smell. Her pulse had fallen to 100, and she appeared to have no other complaint than extreme debility and emaciation.

This

This child perfectly regained her health, though her recovery might, perhaps, rather be termed an escape. Nor do I feel myself perfectly free from censure, because I ought to have been more upon my guard; especially as I had long ago received a caution upon this subject from my late invaluable friend Dr. Clark,* of Newcastle, who some years ago read an account of two similar cases, accompanied with some valuable practical remarks, to the Medical Society in that town. These cases have unfortunately been lost or mislaid, but it is hoped they may yet be recovered, and probably appear, accompanied with some account of Dr. Clark, by a physician peculiarly fitted by former habits of intimacy to be his biographer.

I am, &c.

July 30, 1805. TYNICOLA.

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* Dr. Clark was one of the best practical physicians in this kingdom, and was indebted solely to his own merit, which had overcome many obstacles, for the rank which he held in his profession. He possessed a profound knowledge of medicine, which his own extensive experience, and the study of the best antient and modern authors, aided by a retentive memory, had enabled him to acquire. He was accurate and diligent in his observation of diseases; cautious in forming his plan, but firm and decided in the execution. He was not much inclined to speculative notions, but omitted no means of storing his mind with practical facts; and when his professional engagements prevented him from indulging in a studious course of life, he never lost sight of the modern improvements in Medicine, though he was not easily induced to step aside from the path which experience had pointed out to him. Few physicians were more deservedly popular, or possessed in a greater degree the confidence of their patients; and it redounds still more to Dr. Clark's honour, that this popularity was acquired by no degrading arts; he despised the tricks of the Charlatan. For several years past I had repeated opportunities of meeting Dr. Clark in consultation, and of observing his practice; and it affords me a melancholy pleasure, in giving my mite of applause, to declare, that I always found him punctual in his appointments; anxious for the welfare of his patients; candid in giving his own opinion; and liberal in receiving that of others.

It was Dr. Clark's constant practice to take notes of every important case that occurred; and these, I have reason to believe, from a conversation in which he lamented his want of time to arrange them, he intended for the press. This valuable collection of facts will, I hope, in due time, be presented to the public, and though necessarily in an imperfect state, it will be received as a legacy worthy of the donor. To the above slight sketch I cannot refrain from adding the testimony of an excellent and amiable physician, who was almost daily in the habit of meeting Dr. Clark. "One object, (he observes) engaged his undivided attention through life, unceasing ardour in extending the narrow boundaries of the profession, by accuracy of observation and the vigorous application of remedies."

Copy

Copy of a Letter from the EARL OF WESTMEATH *to* Dr. JENNER, *dated May* 23, 1805.

SIR,

UNDERSTANDING that a report has been circulated, which, if believed, would tend much to weaken that confidence, which is at present so generally and so justly entertained by the public, in your system of inoculation for the cow-pox, namely, that my youngest son had taken the natural small-pox, after having been vaccinated, I think it but justice to you to contradict the report; and to state, for your satisfaction, the real circumstances of the case, which are as follow:

When he was about two months old, he was inoculated for the small-pox, in the Suttonian method, by a physician in Ireland, who has been very generally successful in inoculation, and pronounced by him to be perfectly free from the risk of infection; notwithstanding which, he caught the infection, about a fortnight since, and is now recovering from the natural small-pox.

I beg to inform you, at the same time, that my youngest daughter, who was vaccinated by you, about four years since, has not only been frequently exposed to the danger of infection, but was actually inoculated for the *small-pox*, without taking it. I have considered it incumbent on me to bear testimony to the efficacy of the vaccine system, as I consider the report relative to my son, which originated in misrepresentation, to have been circulated for purposes obviously prejudicial to that most useful and fortunate discovery.

I request you will make any use of this communication, which you may think necessary.

I am, Sir,

Your obedient humble servant,

WESTMEATH.

To the Editors of the Medical and Physical Journal.

GENTLEMEN,

THE professional interest which I necessarily feel in every thing connected with the economy of the organs of voice and enunciation, has occasioned me to peruse, with particular

with particular satisfaction, the remarks of Dr. Denman, in the last number of your miscellany, on the rash and mischievous practice of cutting, during the earliest season of infancy, "what is called the bridle of the tongue." I was not, indeed, aware that consequences so fatal as those which gave rise to Dr. D's communication were likely to ensue; but facts enough had fallen under my observation to convince me—that the operation was generally unnecessary, and, so far as related to *the particular object of my enquiry*, more frequently injurious than beneficial; and (in my Lectures on the Causes, Prevention, and Cure of Impediments of Speech) I have, accordingly, not been sparing in my warnings and exhortations against the officiousness of ignorant nurses, in this particular.

Permit me, however, to observe that, as far as relates to the power of distinct utterance, Dr. D. perhaps, expresses himself rather too generally, when he suggests—"that the frænum *never* requires cutting." In early infancy, indeed, I can readily admit that it never does; for, if medical gentlemen will answer for the *fact* so far as relates to the operation of *sucking*, I shall not scruple to affirm—that, for *elocutionary purposes*, it can never be advisable to apply the scissars before the *twelfth* or *fourteenth* year;—that is to say, before sufficient time has been allowed for the experiment, whether the natural action of the organs, in the process of enunciative effort and cultivation, will not stretch so far the *constricted* or *protrusive* frænum "as to allow all the necessary and proper motions of the tongue." I wish it also to be understood, as my decisive opinion, that if, in the mean time, the organs of enunciation *were judiciously educated*, with a marked attention to the efforts for the formation of the *lingual* elements, the progressive elongation which would inevitably ensue, would be, almost universally, adequate to the object in contemplation. But I am necessitated to declare, that there are some instances in which the degrees of effort and attention *actually applied*, have not been sufficient to produce the elongation of frænum and liberty of motion which perfect enunciation requires.

In a single class of private pupils (from the select Academy of the Rev. Mr. Inchbald of Doncaster) were four different cases, every one of which may, in some way or other, be regarded as illustrative of our present subject.

One of these young gentlemen had, in reality, suffered by the officiousness of which Dr. D. and myself, alike complain; so that the tongue, not being sufficiently re-

stricted in its position, had a tendency to advance and coil up in the mouth; and, though not absolutely intractible to volition, required a much more guarded and precise attention to the regulation of its motions, than is requisite in ordinary cases. His brother (whose original conformation had, at least, been equally unfavourable,—and whose mouth exhibited such a protrusive duplication and reduplication of the frænum as I have seldom witnessed) had, fortunately, escaped the malady of a like redress; and the preternatural ligatures had so elongated themselves, as to leave him in possession of powers of utterance—much more likely to be envied for their impressiveness, than censured for imbecility.

The other two, who were brothers also, had not been equally successful. In the younger of these, a sort of lisp was produced, from the almost utter impossibility of so completely removing the entire edge of the tongue from contact with the lower teeth, as to admit the complete percussion of the sharper sounds; while, in the elder, the stricture was so obstinate as, not merely "to form a small indentation at the extremity," but even to divide the tongue, in its attempts to advance beyond the teeth, into two thick lobes, which interdicted the complete formation of the element *th*, and imparted a sort of thickness to the whole enunciation. In both these cases, after reiterating every experiment, which, in other instances, I had found efficacious in subduing similar impediments, I was obliged to invite the assistance of the surgeon; whose operation, most assuredly, was considerably assistant to the efficacy of my instructions.

There is, also, another observation in Dr. Denman's communication, which may, perhaps, be liable to some misapprehension; the statement, I mean, that "what is called speaking thick, pleno ore, is occasioned by cutting the frænum." In its present unqualified shape, this position is, certainly, much too general; for though the officiousness of nurses and gossips, in this particular, is *one* of the primary causes of such impediment, yet does not the defect, on the one hand, *irremediably* ensue, nor, on the other, does thickness of enunciation necessarily suppose any such peculiarity or injury in the physical state of the organs; two propositions sufficiently illustrated in the phenomena and successful treatment of the cases above described. In short, speaking thick does sometimes, undoubtedly, arise from too great *laxity*, and sometimes from too rigid restriction of the apex of the tongue; but, in general

general, like almost every other impediment, it will be found to originate in habitual sluggishness, or evil imitation; and the tongue of the thickest mumbler will frequently be found as perfect in its structure and capabilities as that of the most accomplished elocutionist.

While I was cultivating my little farm in Wales, before I had ever thought of taking up my present profession, an instance of this presented itself to my observance, which made a deep impression on my mind. Three children of Mr. Griffiths, a hatter in Brecknock, had contracted such a habit of coiling up the tongue, as rendered their speech almost unintelligible. Their parents had, accordingly, conceived that the boys had a natural impediment,—or, as they expressed it, "that their mouths were not made like other people's mouths." From this impression, it is probable that the lads would have been permitted to grow up in the habit of negligent utterance, till it had ripened into inveterate impediment, if the accident of my going into the shop to furnish myself with an article I wanted, had not brought me acquainted with the circumstance. Half an hour's attention, however, and the imposition of a very acceptable task (the reiterated pronunciation of a short, ridiculous sentence) enabled me to put them into a train of as intelligible utterance as any of the people by whom they were surrounded.

To conclude.—Let it not be supposed, upon one hand, that wherever there is a thickness of utterance, or indistinct enunciation, there must necessarily be any defect in the physical state of the organs; or, on the other,—that, even where the frænum has been injudiciously separated, or the tongue, from any other circumstance, is more loosely situated in the mouth than usual, that a mumbling fulness must inevitably exist. *Natural impediments*, (properly understood) are, indeed, exceedingly rare. Deafness and mental imbecility excepted,—they can only originate from extreme obstinacy of stricture in the frænum, from hare-lip, from malconformation of the jaw, or from fissure of the roof and obliteration of the uvula; and even of these, fortunately, there is not one which is beyond the reach and medicature of human art; or which may not yield to the co-operative influence of elocutionary and physiological science.

Your's, respectfully,

JOHN THELWALL.

Brownlow Hill, Liverpool, August 6, 1805.

CRITICAL ANALYSIS

OF THE

RECENT PUBLICATIONS

ON THE

DIFFERENT BRANCHES OF PHYSIC, SURGERY, AND MEDICAL PHILOSOPHY.

Salutary Cautions respecting the Gout, in which the Doctrines maintained in a recent Publication, by Dr. KINGLAKE, *are exposed and refuted, by* JOHN HUNT, *Author of Historical Surgery.*—8vo. pp. 94, London.

WHEN we were informed, that the author of "Historical Surgery" had engaged in this cold water controversy, we expected an accurate statement of the various periods at which this practice was recommended, from the first records we have of it in the writings of one of the *principes artis medicinæ* to its last renewal under Dr. Kinglake, and his disciples. If we were a little doubtful from the promises contained in the title page, we were not better satisfied after perusing the first paragraph of the preface.

"If (says our Author) apologies for imperfections are under any circumstances admissable, it must be in such cases where the *spur* of the occasion prevents that care and attention which are indispensably necessary for the complete discussion of a subject. But in the present instance the flames of enthusiasm have burst forth, and I fear that all the new invented powers of Refrigeration will not be sufficient to stop their destructive progress. In such alarming situation, delay might be productive of the most important consequences; and the elegancy or energy of an address would not make up for the loss of time, if the salutary caution should unfortunately come too late."

The latter part of this apology is perfectly admissible. Want of elegance or energy may be pardoned.—The first is altogether unnecessary, and we have not found our author at all deficient in the latter; but whilst Dr. Kinglake has so many other antagonists, we cannot see the necessity of so much *spurring*, as to "supercede that care and attention which are necessary for the complete discussion of a subject." As the apology is extended through the whole preface, it may be thought more candid to transcribe it entire. It continues thus:

"A general cry of fire resounds from all parts; but I am apprehensive that water is likely to become the most destructive element on the present occasion. At such a fatal crisis, to remain inactive, or to sit silent in obscurity, correcting an address *(nonumque prematur in annum,)* might be construed into an attempt to eclipse

eclipse the fame of Nero, who is said to have amused himself with his fiddle when the capitol was in flames.

"My first object is to sound the alarm, to warn the public of their danger, and protect the podagric multitude from the impending flood. But if the learned doctor, or any of his deluded followers, should presume to oppose this first attack, more circumspection may then be necessary; imperfections may be corrected; the opinions of other authors consulted, and such additional arguments brought forward as the exigency of the occasion may be supposed to require.

"Still I expect the work will not be thought complete; and have no objection to acknowledge, that I at first intended to add a plan of treatment more consistent with what may in some degree be considered the general principles of the present state of the practice of physic, and which I shall now reserve for a second publication. But the quantity of evidence which the learned Doctor has presented to our view has enevitably extended the limits of this part of the investigation; and if the whole of the learned Doctor's extensive correspondence were to have the same degree of attention, it would be impossible to anticipate the bounds. It is not with any intention of avoiding the discussion that so many cases are permitted to pass unnoticed, but it was impossible to do equal justice to the whole; yet if either the Doctor, or any of his fiery advocates, should be inclined to give the challenge, let me now beg leave to assure them, that they may with confidence depend on the most immediate and respectful attention."

The work begins with some pertinent remarks on the uncertainty of physic, and the danger of imputing too much to the fortunate administration of any new remedy, or of confounding the spontaneous operations of nature with the effects of our skill. This introduces some pointed expressions on Dr. Kinglake, whose language, it must be confessed, is, throughout his whole book, so open to sarcasm, that we cannot wonder if Mr. Hunt has availed himself of this advantage over his adversary. Some of his remarks, however, are not only pointed, but so severe, that we shall waive any transcript of them; and only hint to our author, that it is possible his manner of writing may *inflame*, instead of convincing *so fiery a* writer.

Finding a reference to an opinion concerning gouty metastasis, contained in "Historical Surgery," we thought it our duty to turn to the passage. We think it not less our duty to admit, that we were charmed with the bold and not less accurate reasoning with which the passage abounds. But in this, as in every other part, we could not help lamenting the petulence, shall we call it, or unfeeling wantonness, with which the author throws his random shot about him. Without entering into a disquisition of the theory, we shall in general remark, that what is usually called gouty metastatis, is considered as a new disease, which supercedes the gouty action; that this new disease may be treated conformably

to its nature, as we should treat it in any other subject; and that when we have subdued it, the gout will probably again resume its action. No one will doubt the ingenuity of this reasoning, which the author supports by his own experience, and by a variety of well authenticated facts, extracted from the writings of others, who have produced them without any view to the theory he has formed.

The foundation, or perhaps we should say, the illustrations of the doctrine, rests on the well known observation of Mr. Hunter, that two diseased actions cannot exist in a part, or in the constitution, at the same time. The analogy, though imperfect, is extremely well preserved; but here we must lament that Mr. Hunt, who always, to do him justice, wishes to pursue the noblest game, cannot introduce Mr. Hunter's name without a bye blow or two.

Mr. Hunter's doctrine of the incompatibility of two actions at the same time is traced as far back as Hippocrates, and again to Horace. Lastly, the illustration of the doctrine, in a case of small-pox and measles, is quoted from an early number of "Medical Commentaries." Yet Hippocrates only says, two pains existing at the same time, the greater will obscure the less. Horace only compares the waverings of the enthusiast with the succession of one disease to another; and Dr. Manget, in the "Commentaries," gives his case rather as a singular occurrence than as a general law. Mr. Hunter might therefore have fairly quoted it as a proof of his doctrine, instead of fearing the recital of it as a means of invalidating his claim to the discovery of the law. It is moreover highly probable that the case would have passed unnoticed, had not Mr. Hunter's opinions been by that time pretty generally diffused among all those who considered medicine as a branch of philosophy, as well as a means of acquiring money. Mr. Hunter began his regular course of lectures a year before the volume alluded to is dated, and a year after the date of the case: for ten years before, he had taught practical anatomy; and all who knew him are well aware of his fondness to communicate whatever he had observed, and his backwardness to publish.

Our readers will excuse this apparent review of one book whilst another is before us; but the reference seemed necessary, and we have explained the consequence.—To return then to the "Salutary Cautions," which we shall dismiss in very few words. We heartily recommend it to the perusal of our readers, and particularly to Dr. Kinglake.

We trust, if nine years be thought too long a silence, nine months may produce a more finished offspring. Of Mr. Hunt's extensive reading we can form no doubt; of his abilities, we are equally well satisfied: but whilst he is constantly on the watch for the errors and imperfections of others, we wish he would suspect a few in his own writings.

The

The Edinburgh Medical and Surgical Journal, exhibiting a concise View of the latest and most important Discoveries in Medicine, Surgery, and Pharmacy. No. 3. July, 1803.

THIS Number has reached us with more regularity than either of the former, yet we have no reason to complain that it is inferior in point of merit. As must always be the case in collections of this kind, the original communications are very unequal in value and importance, some however must atone for the deficiencies of others. The first will need no apology.

"*Essay on the Analysis of Animal Fluids, principally with the View of ascertaining their definite Characters.* By JOHN BOSTOCK, M. D. *of Liverpool.*

"The precision," says the author of this paper, "which the analysis of mineral and vegetable substances has obtained, does not appear to be yet extended to the products of the animal kingdom. This remark may be applied both to the solids and fluids which compose the animal body; but is the most applicable to the latter class of substances. The terms serous, mucilaginous, gelatinous, &c. are employed, even by the most esteemed medical and physiological writers, in a vague and indeterminate manner, without attending either to the original import of the words, or to the restricted meaning which it is necessary to impose upon popular expressions, when they are adopted in scientific researches. The object of the present paper is to ascertain a definite character for what I propose to call the primary animal fluids, and to discover accurate and delicate tests, by which their presence may be easily and certainly indicated. By primary fluids, I mean those into which the compound fluids existing in the animal body are capable of being resolved by the application of different reagents, without decomposing them into their ultimate elements."

The first of the fluids taken notice of is albumen. With the exception of water, no fluid seems to enter so largely into the composition of the animal body. It is said to form a very considerable portion of the blood, and to be found in greater or less quantities in all the secretions. Though it is generally supposed, by the most eminent chemists, that white of egg is composed entirely of this substance, yet by careful and repeated dilution and evaporation it was found to contain in 100 grains, 80 grains of water, 4.5 grains of uncoagulable matter, and 15.5 grains only of pure albumen.

The most distinguishing characteristic of albumen, Dr. Bostock found was the property of being coagulated by heat, which forms an easy test of its existence in any animal fluid. In order to ascertain how small a quantity of albumen could by this means be rendered visible, he added 13 grains of the white of egg to 87 grains of water, thus forming a solution, one grain of which contained $\frac{1}{10}$ of pure albumen. Five grains of this solution were then added to 95 grains of water, so that 100 grains of the fluid contained $\frac{1}{10}$ grain, or $\frac{1}{1000}$ of its weight of pure albumen. This exposed to the heat

of boiling water became perceptibly opake. After this the ingenious author tried the effects of oxymuriate of mercury—of nitro muriate of tin—of tan—of aqua lithargyri acetata—of nitrate of silver—of nitro muriate of gold—and of alum. All these experiments were conducted with the nicest accuracy, and varied according as the attraction of the different tests to any probable combination of the diluting water or surrounding air might produce any uncertainty. To show the manner in which the experiments were conducted, and how much our author was prepared for every possible objection, we shall transcribe his process with alum, and his general remarks on albumen.

"Albumen, in a concentrated state, is powerfully coagulated by alum: I found, however, that this reagent is not so accurate a test of its presence, when in a diluted state, as some of those which I had already employed. One-fifth of a grain of albumen, dissolved in one hundred grains of water, was indeed rendered slightly turbid by the addition of a few drops of a saturated solution of alum; but no precipitate was formed. Before I conclude my account of these experiments, I must observe, that the strength of the solution of albumen was, in all cases, rather less than my estimate. When I added the albumen to the water, a small portion of it always remained insoluble, and this was separated from the fluid by filtration, before the experiments were performed. This insoluble part I supposed to consist of the membranous matter, with which it is said that the white of the egg is intersected. The quantity was indeed almost too small to be appreciated; but where it is desirable to attain as much accuracy as possible, I think it necessary to mention every circumstance which may in the smallest degree affect the result.

"The experiments related above will, I conceive, indicate, with a sufficient degree of accuracy, the presence of albumen as a constituent in an animal fluid. The property of being coagulated by heat is a characteristic of this substance, which will always serve as a mark of discrimination; and we have found that this property is not destroyed by dilution with one thousand times its weight of water. This, therefore, may be considered as a test of its presence, minute enough for all practical purposes. We have also found that there are several reagents which possess the power of precipitating it from its solution in water, while existing only in the same proportion. It will, however, be necessary to observe their operation upon the other animal substances, before we can determine their use in the analysis of compound fluids."

The next subject examined is Jelly, the peculiar property of which, as our author observes, is that of concreting with cold and liquifying in a very gentle heat. It is found to make a small part of the composition of the blood, and a very considerable constituent of membranes, ligaments, cartilages, and tendons. Dr. Bostock chose isinglass for his subject, a substance supposed to contain jelly in its purest state. He found four grains of this dissolved in two hundred

hundred of water perfectly concrete by cooling. The temperature of the air is not mentioned, but probably by the expression the solution was somewhat above that degree. When this solution was reduced by double its weight of water, in which the proportion of water to jelly was as one to one hundred, still the mixture stiffened when cold; even with the addition of water so as to reduce the solution to one part of jelly in one hundred and fifty of water, the mixture became mucilaginous though it did not concrete.

The tanning principle proved so nice a test of the presence of jelly, that when to a solution containing only $\frac{1}{3000}$ of its weight in water, a few drops of solution of galls were added, a considerable precipitation followed.

Our author's experiments on mucus were confined to saliva diluted with water and filtered, and to water filtered after an oyster had been for some time agitated therein. Aqua lithargyri acetati in both instances produced a copious precipitation. The following is the result drawn from the whole.

"I apprehend that these experiments will be deemed sufficient to establish a decided and essential difference between mucus and jelly. Independant of the gelatinizing property of the latter, the effects produced upon them by the tanning principle, and by the aqua lithargyri acetati, are exactly opposite. Tan is a most delicate test of jelly, but does not in any degree affect mucus. Aqua lithargyri acetati is a delicate test of mucus, but does not in any degree affect jelly. The oxymuriate of mercury, on the contrary, which is one of the most accurate tests of albumen, does not appear to be affected either by jelly or by mucus.

"Albumen, jelly, and mucus, I am inclined to consider as the only primary fluids which are dispersed through the different parts of the body. Particular vessels or glands contain and secrete particular fluids, which cannot be resolved into other fluids without decomposition, as the fibrine of the blood, the resin of the bile, the urea of the kidney, &c.; but these are, in all instances, confined to their appropriate organs, and do not necessarily enter into the present investigation.

"From the above experiments, I think we may be enabled to lay down, with a considerable degree of accuracy, the leading characteristics of the three primary animal fluids, and to establish tests, by which their presence may be minutely ascertained. The most remarkable property of albumen is its becoming coagulated by heat; a property which it retains so far as to communicate a degree of opacity to its solution in water, when it forms only $\frac{1}{1000}$ part of its weight. A solution of the same strength has its albumen precipitated by the oxymuriate of mercury, and this test will indicate its presence, when composing no more than $\frac{1}{2000}$ of the mixture. The tanning principle, the aqua lithargyri acetati, the nitrate of silver, and the nitro-muriate of gold, are all tests of the presence of albumen, nearly as minute as the oxymuriate of mercury; but they are less valuable, because their effects are not confined to

albumen. The nitro-muriate of tin and alum are also precipitants of albumen, but they are less delicate in their operation than the reagents enumerated above.

" The peculiar characteristic of jelly is its property of becoming concrete by cold, and being again rendered fluid by a gentle heat; we have found that its solution in water retains this property when it composes $\frac{1}{100}$ part only of the weight of the fluid. Tan is a still more minute test of jelly than of albumen; but jelly is not in the least degree affected by the oxymuriate of mercury, and may thus, in all cases, be easily distinguished from it. No effect is produced in jelly by goulard, and scarcely any by the nitrate of silver, and the nitro-muriate of tin, when it is in a state of much dilution. By means of tan, jelly may be easily detected in a fluid of which it forms only $\frac{1}{5000}$ part.

" The properties of mucus are principally negative; it is not coagulable by heat, nor capable of becoming gelatinized; it is not precipitable either by the oxymuriate of mercury or by tan, but it may be detected with considerable minuteness by the aqua lithargyri acetati.

" It appears, therefore, that the oxymuriate of mercury, tan, and the aqua lithargyri acetati, are the three most valuable tests. The nitro-muriate of tin is a less delicate test of albumen than the oxymuriate of mercury, and is also in some degree affected by jelly. The nitrate of silver appears to be a very nice test of albumen; but it is objectionable, in consequence of its being decomposed by the muriate of soda, a salt which is supposed to exist in most of the animal fluids. The nitro-muriate of gold is a delicate test of albumen, but it likewise precipitates jelly.

" In the analysis of a fluid which is supposed to contain either albumen, jelly, or mucus, the first step is to observe the effect of the oxymuriate of mercury; if this produce no precipitate, we may be certain that the fluid in question contains no albumen. We must next employ the infusion of galls, and if this also cause no precipitate, we may conclude that the animal matter held in solution consists of mucus alone.

" I have before remarked, that the ideas which I have formed of the nature of jelly and mucus, and the relation which these substances bear to each other, differ considerably from those of Mr. Hatchett. It is not, indeed, without a degree of diffidence that I dissent from so distinguished a chemist; but I conceive that I am justified by the experiments related in this essay. Mr. Hatchett, in the valuable paper to which I have already referred, speaks of the white of the egg as consisting of pure albumen; but I believe that, in this particular, he will be found not perfectly accurate.

" There is a great resemblance between the physical properties of animal mucus and vegetable gum; and I found that they strongly resemble each other also in their chemical qualities. A solution of gum arabic, containing one grain of gum to two hundred

dred grains of water, was not affected either by the oxymuriate of mercury, or by tan. With the nitro-muriate of tin, and with the nitrate of silver, there was only a slight degree of opacity; but with the aqua lithargyri acetati there was a dense precipitate instantly formed."

Article 2.—"*A Memoir on the State of Health of the 88th Regiment, and of the Corps attached to it, from June 1, 1800, to May 31, 1801, as originally presented to the Medical Board, Bombay; by* JAMES M'GREGOR, A. M. M. D. Surgeon to H. M. 88th Regiment and Member of the Royal College of Surgeons, &c. &c."

This paper is very long, and we suppose is such as the Board, to which it was presented, requires of the army-surgeons in that country. It should however have been considerably altered for the collection to which it is here attached. That it contains useful facts and judicious remarks cannot be doubted; but fewer cases, well selected, and related with all their circumstances, detached from extraneous matter, would have been much more desirable. At present the materials are so peculiarly disposed that we have found it difficult to make any selection that could interest or improve our readers.

Article 3.—"*A Case of compound Fracture of the Humerus, from a Gun-Shot Wound, in which Amputation was performed at the Shoulder-Joint; by* R. WATSON ROBINSON, M. D. F. L. S."

This is a valuable case in every respect, and in its relation replete with useful observations. The whole does great credit to the ingenious writer.

Article 4.—"*History of a Case of Trismus, in which the Affusion of cold Water was successfully employed;* by WM. DALRYMPLE, *Surgeon, Norwich.*"

This was one of those cases of lock-jaw which will sometimes occur in very irritable habits. We sincerely wish the same remedy may succeed in other instances, but from our own experience cannot encourage the hope.

Article 5.—"*Remarkable Case of Tympanites; by* J. CHARLES COLLINS, Surgeon, Swansea, Member of the Royal College of Surgeons of London, and of the Medical Societies of Edinburgh and Guy's Hospital."

This case is very interesting, and in most respects new. It seems difficult to draw any practical inference from it; but we trust the author will not fail to acquaint the public with the result, let it prove what it may.

Article 6.—"*Case of Torpor from Cold, and some general Observations on the Effects of diminished Temperature upon the living System;* by GEORGE KELLIE, M. D. President of the Royal Medical Society, and Fellow of the Royal College of Surgeons, Edinburgh."

This case is worth recording, inasmuch as it may add to the register of those in which animation has been restored after appear-

ances

ances very unfavourable. It is however, as our readers will see, much less remarkable than many others which have been related.

"On the evening of the 26th of February, I was requested by Mr. Charles Cheyne, in the absence of my friend Dr. Cheyne, to visit William Dennis, a lad of 16 years, belonging to the Glasgow Packet, who had been found (as was supposed, dead) in a boat at the end of Leith Pier.

"He had, along with some companions, left the harbour about one o'clock, with the intention of taking a sail in the roads; and on attempting to regain the harbour, between four and five in the evening, the boat grounded on the flats a little to the east of the pier. His companions got ashore, leaving him in charge of the boat, and promising to return to his relief, so soon as they had refreshed themselves.

"Two seamen, who were walking the pier some time after, observed him stretched out in the stern of the boat, as they imagined, asleep; but, becoming alarmed, they went to his assistance about half past seven. He was found cold and insensible, and immediately transported to a neighbouring house, where we soon after saw him. When we arrived, he was stretched out, before the fire, on his back, with very little appearance of life; the whole body, with the exception of the face, which was well coloured, was of a deadly pale appearance, and very cold. The powers of sensation and of muscular motion, were completely suspended. The head and limbs, perfectly flexible, fell lifeless to the ground, from whatever position they were raised to; the mouth was half open, and the jaw, obedient only to the hand, could be moved upwards and downwards, but returned to the half closed position; the respiration was obscure and insensible; but the pulse was quite distinct even at the wrist, though irregular and slow. The organs of sense were equally inexcitable; a candle held close to the exposed eyes made no impression, the eye-balls remained fixed and motionless; the pupils, though dilated, contracted irregularly, while yet exposed to the light, in the way I have sometimes observed them to do in the recently dead."

It is hardly necessary to add any account of the means used for resuscitation, and still less of the author's reflections. We must remark however that he advises an *immediate* application of heat to the whole body, where the actions of the whole are so nearly suspended. This is contrary to the custom in colder climates, the inhabitants of which most frequently meet with such cases.

Article 7.—"*An Account of the Morbid Appearances observed in Two Cases of Diabetes Mellitus;* by DANIEL RUTHERFORD, M. D. Professor of Botany in the University of Edinburgh."

This paper is so valuable, and the cases expressed with so much brevity, compared with their minuteness, that we shall make no apology for transcribing the whole.

"*Dissection*

" Dissection of John Robinson."

" Upon opening the body, a much greater quantity of fat was found both under the skin, and in the cavities of the abdomen and thorax, than might have been expected from the apparent emaciation of the body; indeed, the skin was uncommonly thin.

" In the abdomen the blood-vessels all seemed unusually large, and very much distended with blood. The omentum contained a considerable quantity of fat, around large veins filled with black blood. The stomach and small intestines were of natural size and appearance, except that, on the coats of the intestines, the vessels were all very large, and distended with blood, even at their minutest ramifications, giving them exactly the appearance as if they had been well filled with a red injection. The coats of the smaller intestines seemed thicker, and more pulpy than usual. The colon was very large, and distended with air to the utmost; it formed a large doubling under the liver; and throughout its whole length, contained very many little balls of hardened fæces. The mesentery was considerably loaded with fat. The vessels were proportionally large and distended as those of the omentum. The lymphatic glands were uncommonly large, soft, and red; their surface too was painted with many red vessels, or with vessels filled with red blood.

" The liver, perhaps rather small, was otherwise in every respect in the most natural state, except that it lay far up under the edge of the ribs, pressed thither perhaps by the distended colon. The gall bladder rather large and flaccid, yet containing a considerable quantity of very deep yellow bile. The pancreas was perfectly sound and natural, and also the spleen, though perhaps rather large. The kidneys were much augmented in size, soft, with their surfaces painted with numerous vessels. The vessels were uncommonly dilated. The emulgent vein, *e. g.* of the right side, was not under three-fourths of an inch in diameter, as it appeared distended with blood. The ureters also were considerably dilated, the bladder seemed capacious, its coats thickened; and it was half filled with liquid.

" In the thorax there was a considerable quantity of fat in the mediastinum, and in the lower part especially, running along the diaphragm. The lungs were apparently perfectly sound; a few adhesions were observed betwixt them and the sides of the thorax, and also betwixt them and the pericardium. The lungs in texture were perfectly right, or even perhaps more spongy and pervious to air than common. The heart of moderate size, flaccid, and pale, though the coronary veins were greatly distended with black blood. The large cavities contained a considerable quantity of very dark blood, generally fluid, though some had coagulated slightly both in the right auricle and ventricle. In the anterior mediastinum, in the site of the thymus, was a cluster of glands altogether like those of the mesentery, in several of which were calculous concretions, one not much smaller than a small horse bean. The proper glands

glands of the lungs, those situated at the ramifications of the trachea, were unusually large, soft, and of a brilliant black colour.

The age of this man was between forty and fifty.

"*Dissection of Ann Laidlaw, aged nine or ten.*

"On external examination, every part of the body seemed to be much emaciated and reduced in bulk, except the abdomen, which was uncommonly distended with what was afterwards found to be air in the stomach and smaller intestines, and fæces collected in the larger ones.

"When the muscles covering the abdomen and thorax were thrown back, they seemed to be very slender, and of a rather more florid red colour than they usually are in the healthy subject. The fat was entirely abolished, nothing being left but the cells in which it was formerly contained. The lungs on both sides adhered slightly in some places to the pleura costales; but, on the right side, besides adhering every where very firmly to the pleura of the mediastinum, a quantity of coagulable lymph, of a yellowish colour, was found effused between the pleura of the inferior and middle lobes of the lungs and the mediastinum. On both sides, the lungs were rather of a pale colour externally, but they felt firmer to the touch than in the healthy state; and, on cutting into the substance of them, a puriform fluid was seen exuding from different places, although no distinct tubercles could be perceived, and there was little more than the common quantity of serum effused into either cavity of the thorax.

The pericardium contained about four ounces of a yellowish transparent serum. The heart was of the natural size, unusually free from fat, and rather of a pale colour. The right ventricle very flaccid, the left firm and contracted. The internal structure of the heart was natural; but, on opening the aorta, the inner substance of the anterior side of it was of a red colour, and the redness extended from the commencement of the aorta to that part where it passes through the diaphragm into the abdomen. The aorta below the diaphragm was, in every respect, in a healthy state.

"The pulmonary veins and vena cava did not appear to be any way diseased; several of the bronchial glands were considerably enlarged; in particular, one on the right side, which resembled a middling sized walnut, and contained a good deal of a brownish substance, of a consistence rather thinner than cheese. The trachea was perfectly natural.

"The stomach was much distended with air, but, in other respects, had a healthy appearance, both externally, and when it was laid open to examine the internal coat. The upper part of the intestinal canal was uncommonly distended with air, and, in some places, the blood-vessels near the juncture of the mesentery were filled with blood, giving somewhat of the appearance of inflammation. The caput cœcum, the colon, and rectum, were every where filled with hard fæces, by which they were distended to a

size

size far greater than could have been thought possible in so young a subject; in other respects they were healthy. The glands of the mesentery and mesocolon were enlarged; but, though very numerous, none of them had attained a size larger than a common bean somewhat flattened, and some of them nearly white. The omentum was small, contracted, and completely free from fat. The kidneys were somewhat enlarged, the colour natural; but, on cutting into them, the *tubuli uriniferi* were fully more distinct than usual, and the pelvis of each was also considerably enlarged; no other morbid change of structure was perceived. The ureters were much larger than common, in some places being nearly half an inch, in other places more, in diameter. The right lobe of the liver adhered firmly to the diaphragm in many places; but, in other respects, both as to external appearance and internal structure, it was quite sound.

"The gall bladder contained a small quantity of light-coloured yellowish bile. The bladder was nearly empty, but of a natural appearance. The spleen, pancreas, uterus, Fallopian tubes, and ovaria, did not seem to be any way diseased."

Article 8.—"*Case of Protrusion of the Tongue.* By W. R. Clanny, M. D. Bishop-Wearmouth."

This is a good practical illustration of a paper given in our Journal, vol. vi. p. 353.

Article 9.—"*Flatulent Colick, with Hernia, cured by Opiates alone, freely administered.* By James Ross, M.D. Montrose."

The subject of this article has a hernia for which he wears an elastic truss. He is also occasionally attacked with flatulent colic, at which time the whole abdomen swells, and a protrusion of the size of a hen's egg appears at the ring. The author of the paper tells us that the omentum, and part of the intestine form the contents of the tumour. Such may be the state of the parts, but we cannot help being surprized at the confidence with which Dr. R. assures us of a thing which would be guest at with some caution by men of the largest opportunities of examining such cases. The protrusion being only the temporary effect of general intumescence, without stricture or strangulation, it is not surprizing that it should so readily yield to opiates and aromatics. However, the narration may be useful in teaching the young practitioner, that under such circumstances evacuation of the bowels is not necessary before the patient is relieved from his immediate sufferings.

Article 10, presents us with a second paper from the Enquirer, "*On the Inexpediency of erecting Foundling Hospitals.*"

This is more properly a political than medical enquiry. However, it is connected with medicine, and the appropriation of a charitable fund towards the erection of such a hospital in Edinburgh, renders the enquiry peculiarly proper at this time. The author, after giving an account of the different institutions for Foundlings throughout the world, has no difficulty in shewing the inutility and danger of such establishments. Mr. Malthus has convinced

convinced many sceptics of the same. It is not difficult to show the inconveniences of any thing, and it is far from our intention to engage in so important a controversy: But to all questions there are two sides. We would ask, then, has not the severity of the Scotch kirk hitherto more than justified "*the pious and charitable* purposes of preventing child-murder, of receiving privately women big with child, assisting them in their delivery so as to conceal their shame, and of taking care of their children as foundlings?"

Such are all the original papers. There is indeed another, which we may perhaps hereafter transcribe from the New York Medical Repository, with our doubts and enquiries. It is not *our* intention to accuse Dr. Rush of deserting the sect of Contagionists and embracing an opinion contrary to European belief. For our own parts (perhaps from some unfortunate *mauvaise honte*) we should call the Professor's desertion, as it is here styled, an ingenuous acknowledgement of his former error; and in a disease (the yellow fever) if not altogether trans-atlantic, at least trans-marine, we should not presume to offer more than enquiries to those who have so much larger opportunities of judging than ourselves.

The Postscript contains a valuable communication from Prof. Pacchioni, of Pisa, on the composition of muriatic acid. The whole process is not yet sufficiently detailed, but the general result of his experiments go to prove that,

"1. *Muriatic acid is an oxyd of hydrogen*, and consequently composed of hydrogen and oxygen.

"2. *In the oxygenated muriatic acid, and therefore*, a fortiori, *in muriatic acid, there is much less proportion of oxygen than in water.*

"3. *Hydrogen is susceptable of very many and different degrees of oxydation*, contrary to what is universally believed by pneumatic chemists, who assert that hydrogen is susceptible of only one invariable degree of oxydation, that in which it forms water."

These discoveries were the effects of different attempts to decompose water by the Galvanic pile. Mr. Peel, of Cambridge, our readers must be aware, has been engaged in similar experiments; and we may consider the confirmation of these important facts as a new link to connect that chain of elementary decomposition, for which we are indebted to the honest industry of a Priestly, the perseverance of a Cavendish, and to the original genius of both.

This number contains an engraving to illustrate the valuable communication of Mr. Cooper, contained in the last.

A Clinical History of Diseases. Part First: being, 1. *A Clinical History of the Acute Rheumatism.* 2 *AClinical History of the Nodosity of the Joints. By* JOHN HAYGARTH, M. D. F. R. S. Edin. &c.

Of the value of faithful records of medical practice, given by men of mature judgment and large experience, and free from every bias of system, no one who is himself a practitioner can entertain

any

any doubt; and much it is to be regretted that the veterans of the profession are generally too fond of the ease they have purchased by their labours, and too indifferent to the instruction of their successors, to take the trouble of methodising the important facts which must have passed under their notice.

The late Dr. Heberden was a laudable exception to this charge, and merited the grateful acknowledgements of his brethren by his valuable legacy of observations.

Dr. Haygarth has too well established his character for benevolent ardour in promoting the welfare of society, and for assiduous attention to the improvement of his profession, to occasion any surprise that, while still engaged in active duties, he has followed such a respectable example. He has given us reason to hope, that the present publication is only the forerunner of a series of clinical histories, which will comprize every thing of importance that has occurred in an extensive practice of thirty-five years, carefully noted down at the time; and from the present specimen we cannot doubt that much benefit will be the result.

The history of the Acute Rheumatism is arranged in two sections, of which, the first contains a plain and popular account of the causes, symptoms, and mode of cure; the second consists of proofs and illustrations, referring particularly to actual cases in the author's own practice. Of these last there are 170, concisely disposed in a tabular form, under a variety of heads, which exhibit at one view the leading circumstances and the remedies employed. The practical point chiefly inculcated in this clinical history, is the early exhibition of the Peruvian bark; that is, without waiting for the subsidence of the fever or inflammation, and only premising evacuation of the primæ viæ by antimonials. This practice the author derived from the late Dr. Fothergill, who informed him of his having been instructed in it by Sir Edward Hulse; and from that physician, it is traced to Morton in his Treatise on Fevers. Dr. Haygarth gives a very exact statement of the time of the disease, and mode in which he has employed this remedy, and the results; and very fairly discusses the question, whether in the unfavourables cases it did harm. We cannot doubt that this testimony will be thought very conclusive on this head, and will go far to establish the practice. We shall copy two of the cases, which are narrated at length, as examples of the successful exhibition of the bark.

"No. 134. Mr. W. July 13, 1791, having caught cold, has been ill for five, especially for the last two weeks. The joints of his fingers, feet, shoulders, hands are swelled, red and painful. Profuse sweats, flatulence; much rumbling in his bowels; in a laxative state; shortness of breath; faintness; tears; pulse 96.

"He has been bled seven times from the arm; the last blood taken to-day has an inflammatory crust.

"At first x and soon xx grains of the powder of bark were taken in mint water every three hours.

" July 21. He has taken four pints of the mixture, which contained 5 $\frac{1}{6}$ ounces of bark in substance, in eight days. As soon as he began the medicine, there was an immediate abatement of the inflammation, flatulence, and languor. In five days his sweats ceased; in six days after he began to take the bark he was so well recovered as to ride on horse-back. No complaint remained, but some pain of one hand and one shoulder.

" In this case the efficacy of the bark was remarkable. Perhaps the frequent blood-letting, which had been previously employed might be conducive to this purpose. However, it is manifest that such copious evacuations from his veins did not cure the rheumatic inflammation, but reduced the patient to extreme languor, debility, and even tears.

" No. 170. Mrs. M. March 28, 1803, being the fourth day of a rheumatic fever, had suffered chills, burnings, profuse sweats, violent pain of the shoulders, back, elbows, feet, thigh, knee, hips; great thirst; very restless days and nights. She took the antimonial powder, which produced copious evacuations of her stomach and bowels, but without relief of the fever or inflammation. March 29. Bark in powder gr. x. was given every third hour. March 30. The pain of the shoulders remained, but it was diminished in the lower extremities; less sweat. The bark feels grateful. A cough. On account of this symptom. the bark was omitted, and the antimonial powders repeated. March 30, 31, April 1 and 2, without producing any abatement of the pains, swellings, or fever. I endeavoured repeatedly to persuade this patient to lose blood by the lancet, chiefly on account of her cough, but could not overcome her prejudices on this point. She was extremely averse to the operation, and her faint debilitated state gave her apprehensions that she could not bear such an evacuation.

" April 3. The bark was again taken in powder, gr. x. every fourth hour; April 4th, every third hour. April 6. A remarkable abatement of all the symptoms, except the inflammation of the left hand. The bark was increased to gr. xv. every third hour; April 8, to gr. xxii. every third hour. April 10. The pains, swellings, and sweats are much diminished. An ounce and half of the decoction and 20 grains of the powder of bark were then given every three hours. During this course, to alleviate the fever and watchful nights, effervescing draughts, and occasionally with anodynes were administered. April 12. Seldom coughs. Convalescent. Continue bark.

" In fourteen days (April 3 to 17) though the patient in that period thought her pains and fever several times aggravated by catching cold, she was restored to perfect recovery from rheumatism, which had reduced her to the wretched state above described. I never witnessed a more distressing case of this painful malady where the inflammation remained in its usual seat, the joints and muscles, and was not translated upon any of the vital

vital organs. By continuing this remedy, her appetite, strength, and sleep returned. She soon again enjoyed the blessings of good health."

The Clinical History of the Nodosity of the Joints, is a brief account of a painful affection which has usually been confounded with gout or rheumatism, but which appears to this writer to be a peculiar disease. It almost exclusively belongs to the female sex, and generally makes its appearance about the period of the cessation of the menses. It attacks a variety of joints, but especially those of the hands and fingers, and produces a gradual enlargement of the ends of the bones, the periosteum, and articular membranes and ligaments, with pain and impeded motion. The remedies found most effectual have been warm bathings, the douche, and leeches.

Dr. Haygarth has not given his cases of this disease in the same tabular form with those of rheumatism, (a mode, which, from the difficulty in drawing up and printing, he is inclined hereafter to disuse) but has formed general results from them, indicating the principal occurrences. It is scarcely necessary for us to recommend to our readers a volume so replete with practical instruction from so respectable a source.

Report of Diseases in the public and private Practice of one of the Physicians of the Finsbury Dispensary, from the 20th of July to the 20th of August.

Febris	2	Hypochondriasis	9
Cholera	1	Epilepsia	1
Diarrhœa	17	Amenorrhœa	14
Rheumatismus	3	Cephalalgia	1
Phthisis	8	Anasarca	5
Catarrhus	8	Morbi Cutanei	16
Dyspnœa et Tapis	13	Morbi Infantiles	19
Dyspepsia	12		

The last month has not been marked by the extraordinary predominance of any individual disease. Complaints of the bowels have indeed, as is usual about this period of the year, prevailed to a considerable extent; but decided cholera has been rare.

A remarkable instance has recently occurred, in which a fit of epilepsy almost immediately followed a paroxysm of anger. An attack of what are called nervous affections, in all their various and miscellaneous forms, not unfrequently originate from some agitation or impetuous move-

ment of the mind. The important influence of ill-managed passions is by no means sufficiently appreciated.

To a careless adjustment or an insufficient regulation of the mental, are to be attributed, much more frequently than it is in general imagined, the disorders and anomalous irregularities that occur in the corporeal department of our frame.

Pharmacy is but a small part of physic. In the successful treatment of disease, other and more powerful agents must often be employed than are to be found amidst the medicinal variety of the shops. The art of *healing* implies, in a metaphorical as well as a literal sense, a knowledge of the human heart—the anatomy of the mind as well as that of the body. Medical cannot be separated from moral science, without reciprocal and essential mutilation.

This remark applies more particularly to a proper knowledge and treatment of their complaints, whose rank and circumstances in life entitle them to the falsely envied privileges of luxury and leisure.

The diseases of the poor and the rich are not essentially different. Similar debility and disorders are produced in the one instance directly, and in the other indirectly, by a very full and high, or by a very low and meagre regimen.

The indigent wretch, whose scanty fare scarcely is sufficient to support the stamina of existence, and the no less wretched debauchee, whose intemperate indulgence daily accelerates the period of his destruction, may both with an equal propriety be said to *live hard*. The only important distinction that exists between the diseases of the vulgar and of the more fashionable world, arises from the former being so entirely engrossed in supplying the necessities of life, and in suffering from its physical inconveniencies, as not to be at sufficient liberty to feel and contemplate those infinitely more dreadful calamities that grow out of the soil of a pampered and consequently distempered imagination. A person must be idle in order to be perfectly miserable. No evil is worse than that intolerable sense of vacuum which the mind suffers that has no object commensurate to its capacity, or whose faculties of action and of feeling, although in a state of requisition, are not summoned by an imperious necessity, or other motives of sufficient power, to regular and interesting occupation. To the proper and healthy state of man daily exertion is no less necessary, than the diurnal motion of the earth he inhabits is to its existence and continued preservation.

servation. Without intellectual, bodily exercise is comparatively of little avail to one whose understanding has been enriched and exalted by literary cultivation. "I will not hesitate to assert, that to have the mind ardently engaged in a pursuit that totally excludes exercise of the body, is much more favourable to the spirits than a languid mixture of both."* Of the important effects arising from bodily labour, when united with mental excitement, we have recorded a remarkable instance in the "Monita et Precepta" of Dr. Mead.—"A young student at college became so deeply hypochondriac, that he proclaimed himself dead, and ordered the college-bell to be tolled on the occasion of his death. In this he was indulged; but the man employed to execute the task appeared to the student to perform it so imperfectly, that he arose from his bed in a fury of passion to toll the bell for his own departure. When he had finished, he retired to his bed in a state of profuse perspiration, and was from that moment alive and well.—"*Vitam autem reddidit iste labor, et convalescit.*"

J. REID.

Grenville Street, Brunswick Square, August 25, 1805.

MEDICAL AND PHYSICAL INTELLIGENCE.

[FOREIGN AND DOMESTIC.]

A Memorial of the Medical Committee, addressed to the Inhabitants of the City of Norwich, and of the County of Norfolk, in support of a plan for the extinction of the small-pox by a general inoculation for the cow-pox.

In this age of mutual charity and benevolence, any address, from any class of men, in behalf of their fellow creatures, daily suffering from a dangerous and loathsome disease, might be thought unnecessary by superficial observers. It is, however, a painful reflection, that this is not the case: for the constant existence of the casual Small-Pox, in the united kingdom of Great Britain and Ireland, and its increase, during the last few months, in this city and county, afford an ample proof, that it is an evil of great ex-

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* Dr. Aikin's "Letters to his Son."

tent, not only requiring the serious attention of an enlightened legislature, but more particularly appealing to the judgment and feeling of such persons, the object of whose duty and professional employment is the prevention and removal of disease. In speaking of the nature of casual Small-Pox, it may with truth be observed, that it has been the scourge of human nature for many centuries, and that it has surpassed every other disease in virulence and devastation: even the plague itself, whose very name spreads terror through the world, yielding to it in the mortality and loss of human life which it occasions, although armed with the power of attacking the same individual more than once. The reason of this greater mortality from small-pox than plague, or any other disease, will readily occur to the thinking mind; the ravages of the plague are softened and restricted by season, and receive limitation from soil and climate; but no controul of this nature arrests the progress of small-pox, its baneful influence existing at all times and seasons, and visiting every soil and every climate: so that it may be emphatically termed, a general and perpetual plague. That this character, however dreadful, of small-pox is true, we need only recur to the bills of mortality, where an account of the deaths arising from both diseases is given, and from them it appears, that the deaths from plague within the London bills, during a century, viz. from 1601 to 1701 inclusive, were 188,571, but from 1701 to 1801 inclusive, it appears, that in a century, at least 250,000 persons have perished of this fatal disease, the average of deaths being, in the same bills, considerably more than 2000 a year. And we may with safety presume, that a similar ratio of the mortality of small-pox and plague obtained, during the same period, in those parts of the kingdom, unnoticed by the London bills.

But this statement taken from records, warped by no theory, and subservient to no speculation, shews only the immediate or direct mortality of small-pox, and affords us no clue to estimate the morbid effects, which it exerts upon the human frame, in producing death more remotely, or by the intervention of other diseases; such as its power of forming scrophula, or calling into action its latent seeds, the most fertile cause of consumption, a disease too fatal, and too general, in the British isles. These indirect effects of small-pox, in producing death, or entailing upon mankind other diseases, as permanent weakness of body, diminished strength of limb, loss or defect of sense, as blindness and deafness, can only be accurately known to medical men, whose daily practice brings them acquainted with the victims of small-pox, whether from its direct or remote influence. It is not to be wondered at, then, that the generous and enlightened should, for a long period, have been employed in inventing means adequate to the resistance and suppression of so great an evil to society as small-pox; and we find, so long ago as the beginning of the last century, about 1722, a method of communicating this disease,

disease, by inoculation, was brought to this kingdom, and which certainly disarmed it of its danger, in a very great degree. Humanity received this discovery with eagerness, and reason augured from it the greatest good; and it was thought, that human nature would no longer be the victim of so malignant a disease. It appears that these hopes were too sanguine, for history informs us that this mode of giving the disease by inoculation, was at first partially received, and of course but slowly practised; and that after a lapse of a few years, it became more general among the higher orders of society, and institutions were formed in the metropolis, and in other parts of the kingdom, for the gratuitous inoculation of the lower.—It was hoped, by these salutary proceedings, not only, that great restraint would be put upon the progress of a disease, destroying in the first fifteen years of life, more than the combined power of all other diseases, but that ultimately the extinction of it would be effected. This goodly prospect, so far from being realized, has been wholly darkened by some melancholy facts, which inquiry has established; and from which it appears that more deaths have taken place since the practice of inoculation than before; an effect which is only to be ascribed to the practice of inoculation among the higher orders; keeping up the casual small-pox among the lower, in a great degree;—for compared with casual small-pox, the inoculated may be said to be almost free from danger; the proportion of deaths from the inoculated being only one in three hundred, whereas it appears, from the casual small pox, at a moderate calculation, about one in six falls a victim to the disease. It was soon discovered, that, to remedy these mischievous effects of partial inoculation, and to protect human nature from the casual small-pox, it was necessary to extend the practice of inoculation generally, or, more strictly speaking, universally — and it has been the aim of the first medical and political characters to inculcate the necessity of substituting universally the inoculated for the natural small-pox. — It might have been supposed that a measure dictated by reason and common sense, and by which the fatality of small-pox might have been so greatly abridged, would have met with little difficulty in the execution—but these humane exertions have failed altogether, as within the last few years the mortality from small-pox has been annually increasing; for it appears that in the first thirty years of the eighteenth century, before the effects of inoculation could be shewn, in every 1000 deaths, the proportion arising from small pox amounted only to 74, but in the last thirty years of that century, the deaths from small-pox amounted to 95 in 1000. It is our opinion, that this failure, in extending generally the inoculation of small-pox, is not so much to be ascribed to the prejudices of the poor, as to a supineness of character respecting disease, or insensibility to their own safety, the necessary atributes of poverty, or of minds whose exertion is unceasingly directed to procuring daily sustenance for themselves and families. This is evinced by

the readiness with which the poor, when called upon, submit to parochial inoculation.

But, whatever melancholy impressions these failures may have made upon the minds of medical men or others, they have been greatly removed, by a new æra in medicine, commencing with the discovery of cow-pox inoculation, by Dr. Jenner, as a preventive of small-pox. A discovery which should be received and rewarded, not only by individual, but by national, gratitude. In this discovery, as far as the experiment has been made, there is reason to believe, that he has afforded mankind a complete antidote, a certain protection against small-pox, and has, probably, given them the means of extinguishing the small-pox altogether. In giving our suffrage to this discovery, and to the application of it, it is our wish to impress upon the minds of the inhabitants of this city and county, our thorough conviction of its safety, and of its efficacy, when duly employed in preventing, small-pox—that we consider its action upon the human frame the most mild and innocent, never proving fatal, and requiring no assistance from internal medicine—and lastly, that it is not contagious—and on these accounts, it requires no suspension of intercourse or industry, among the different members of society. And as far as our experience has gone, the cow-pox never calls forth or is followed by any other disease, such as the evil, general disability of body, or loss or imperfection of any of the senses. In addition to these facts, we wish further to remark, that in our experience during the last five or six years, we have never witnessed an instance of a person receiving the small-pox, after having been duly and attentively subjected by inoculation to the cow-pox; and we are of opinion, that the numbers which have been inoculated for it, in the united kingdom of Great Britain and Ireland, (not to mention the other parts of the world) for the last five or six years, afford an ample testimony, an incontrovertible experiment, of its preventive power; not knowing any analogy of action, or law of the animal œconomy, by which it is rendered doubtful, that the preventive effects of cow-pox, upon the human body, after being exerted, for four or five years, should not continue to be exerted through life. And that this is the fact is proved by many cases on record, of persons receiving the disease from the cow, in early years, and never afterwards, through a long life, taking the small pox, although exposed to its action. With this conviction, we are called upon as friends of science and humanity, to recommend to the inhabitants of this city and its hamlets, the adoption of the general cow-pox inoculation, proposed at the General Meeting of the inhabitants of this city, on Monday last, July 29th.

That this recommendation may receive every assistance that a candid and impartial inquiry can give it, we shall briefly examine the force of the objections brought against cow-pox inoculation, and draw a parallel between its effects and the effects of inoculated small-pox upon the human body.—It is admitted on all hands, that the

the cow-pox is never fatal, whereas in the inoculated small-pox, one in 300 perishes; a circumstance of no trifling consideration. It is asserted that cow-pox is not, universally, a security against small-pox, there being instances alleged of persons taking the small-pox, after having been inoculated for the cow-pox. To give this argument its full force, let us admit all the alleged cases to be true, and then proceed to ascertain the proportion they bear to the whole number of persons who have been inoculated for the cow-pox.—From this inquiry it appears, that of 250,000 persons, who have been inoculated for the cow-pox, only 50 persons have been alleged to have suffered from subsequent small-pox; thus, even in this view, the cow-pox is highly to be preferred to the small-pox, as, from this estimate, *only one person in* 5000 *is liable to small-pox, whereas in inoculated small-pox, it is admitted, that one person in* 300 *perishes.*—But a more minute investigation has shewn, that of these fifty alleged cases, only ten have been substantiated by evidence admissible and adequate; and that it is probable, among these ten cases, some deception or mistake may have taken place, on the same grounds, as in some of the asserted cases of small-pox subsequent to small-pox, and in which the chicken-pox has been taken for small-pox.—Admitting, however, these ten cases to be established, the conclusion from such admission strongly proves the superior advantages of cow-pox inoculation; as in that case, instead of one person in 5000, only one person in 25000 would be liable to small-pox. And farther supposing in the 250,000 persons inoculated for the cow-pox, that ten of them (as asserted) should be liable to small-pox, and should actually take it, and in the casual way, and that of these ten one in five should die, which is a greater proportion than really obtains, it would then appear, that of 250,000 persons inoculated for the cow-pox, only two persons would have died, and those from subsequent small-pox; whereas, the deaths from the same number of persons (250,000) inoculated for small-pox (taking the received proportion of one in 300), would be about 834.—Thus it is proved, that the fatality of small-pox inoculation, compared with that of persons taking the small-pox in the casual way, subsequent to the cow-pox, is, as near as may be, 834 to 2; a fact at once strongly exhibiting the superior advantages and mildness of the cow-pox, when compared with small-pox.

With this fact we shall conclude our remarks, trusting, that enough has been said to incite the inhabitants of this city, to adopt the proposed plan of general cow-pox inoculation, and that the poorer classes of society will, with gratitude, listen to these friendly councils, and practice a plan so necessary to their safety:—and that the Court of Guardians of the Poor, the Clergy, and leading inhabitants of this city, will assist and promote measures so beneficent and salutary, not only by their influence but by their example, by discouraging, on the one hand, the pernicious practice of inoculating for the small-pox, and, on the other, advancing, by their utmost endeavour, the adoption of the cow-pox.—

And

And for the same reason must we appeal to the Ministers, to the parish officers and leading men of the several parishes in this county, for their co-operation, in discouraging the inoculation of the small-pox, and in adopting that for the cow-pox; knowing as we do, that the characters mentioned, have with parental attention, and from the best motives, encouraged every few years, in their several parishes, a general inoculation for the small-pox, a practice eventually highly pernicious, as, at the same time that it gives security to the parishes inoculated, it carries danger and death to the adjoining parishes, in which inoculation has not taken place.

And as experience has shewn the little progress which has been made at all times, by the small-pox inoculation, among the poor, when left to themselves, and unsolicited to apply it in practice; a fact established beyond a doubt, by the reflection that only the small number of 25,000 persons have been inoculated during the last forty years of the last century, at the Inoculating Hospital, in London, a very extensive institution;—we therefore recommend that the children of the poor, or other persons, who have not had either the small-pox or the cow-pox, be once or twice a year, or occasionally, inoculated at their own houses; a measure necessary to render permanent the good effects of a general cow-pox inoculation, in extinguishing the small-pox. We think, moreover, in this universal cow-pox inoculation, it will be prudent for a time, as there is frequently no visible constitutional disturbance of the system, denoting its efficient agency, that the part inoculated should be submitted to the inspection of some medical men, or to some person conversant in the practice; by these means mistakes may be prevented, prejudicial to the individual, and to the extension of the cow-pox inoculation; and should hereafter any solitary case of small-pox arise, from any secret or unknown source, it is hoped that all intercourse will be stopped immediately, between persons so affected, and such as are liable to the disease.

That these hopes are not too sanguine, the present state of Vienna, the metropolis of Germany, evinces; for in that city, containing a population of 254,000, after a general cow-pox inoculation, only two persons have died of small-pox during the year 1804.

By enforcing these measures, we shall soon see the united kingdom of Great Britain and Ireland rival the other states of Europe, not only in religion, science, and morals, but in consulting the safety, and securing the lives, of our fellow creatures, by the extinction of the small-pox—and that our commerce will no longer be charged with carrying the seeds of death and destruction to distant quarters of the globe, at the same time that it conveys the products of our industry, and the arts of civilized life.

(SIGNED)

RICH. LUBBOCK	WM. BACK	WM. FELL RAND
WARNER WRIGHT	JAMES KEYMER	C. W. STARKEY
EDWARD RIGBY	JAMES ROBINSON	SAM. S. DEACON.
P. M. MARTINEAU	WM. DALRYMPLE	

Extract of a Letter from Dr. Benjamin Waterhouse to the Editors, dated March 13, 1805.

Dr. De Carro sent me some vaccine matter on an ivory lancet enclosed in a wooden case; the letter which accompanied it was dated Vienna, October 28, 1803. I made use of it November 27, 1804; when it communicated the genuine pustule and disease. Does the history of vaccination afford another instance of the virus communicating the true disorder after being taken from the subject thirteen months?

The Plates for Mr. SAUNDERS's Work on the Structure of the Ear, are in the hands of Mr. HEATH, and it is expected to be ready for publication early in October.

A new volume of the valuable Transactions of the London Medical Society is announced as ready for publication.

Mr. JAMES BRIGGS will shortly lay before the public, Practical Observations on the principal Diseases of the Eyes, illustrated by Cases, translated from the Italian of Antonio Scarpa.

Mr. DONOVAN is printing an Epitome of the Natural History of the Insects of New Holland, New Zealand, New Guinea, Otaheite, and other Islands in the Indian, Southern, and Pacific Oceans; including the Figures and Descriptions of one hundred and fifty-two Species of the most splendid, beautiful, and interesting Insects hitherto discovered in those Coun tries

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The second volume of BELL's Surgery, containing operations of surgery, may be expected in a short time.

UNIVERSITY OF GLASGOW.

The Medical Lectures in the University of Glasgow, will begin on Tuesday the 5th of November, at the following hours:

Dietetics, Materia Medica and Pharmacy, by Dr. MILLAR, at ten o'clock forenoon.

Midwifery, by Mr. TOWERS, at eleven.

Theory and Practice of Physic, Dr. FREER, at twelve.

Anatomy and Surgery, by Dr. JEFFRAY, at two o'clock afternoon.

Chemistry and Chemical Pharmacy, by Dr. CLEGHORN, at seven.

Clinical Lectures on the Cases of Patients in the Royal Infirmary, by Dr. CLEGHORN and Dr. FREER. The First Lecture by Dr. CLEGHORN, on Tuesday evening the 12th of November, at six o'clock.

Dr. BROWN will begin his Lectures on Botany, about the beginning of May next.

Regulations

Regulations enacted by the Senate of the University of Glasgow, respecting Degrees of Medicine.

1. That before any person can be allowed to be a Candidate for a Degree in Medicine in this University, he shall appear personally before the Senate, and lay before them evidence, that during the space of three years, or sessions of six months each, he has regularly attended in some University or Universities, or in some medical school or schools of reputation, the following medical classes, viz. Anatomy and Surgery, Chemistry and Pharmacy, the Theory and the Practice of Physic, Materia Medica, and Botany.

2. That he shall bring forward evidence, that during one year at least, he has attended Medical Lectures in this University.

3. That the Candidate shall undergo three separate examinations in private, by the Medical Professors of the University, and write a Commentary on an Aphorism of Hippocrates, and another on a case of disease propounded to him by the said Examiners. The first examination shall be on Anatomy and Physiology; the second, on the Theory and Practice of Physic; and the third, on Chemistry, Materia Medica, Pharmacy and Botany.

4. That the Examiners shall report to the Senate, their opinion respecting the medical knowledge of the Candidate; and if their report be favourable, his name as a Candidate for a Degree, shall be entered in the minutes of the Senate, and a day fixed, when the Candidate shall read his Commentaries on the Aphorism and Case, and answer such questions on the several branches of medical science, as shall be put to him by the Examiners, in presence of the Senate. If the Senate be of opinion, that the Candidate has shewn himself worthy of a Degree, it shall be conferred in presence of the Senate, by the Vice Chancellor, provided the Candidate has not published a Thesis; which he may, or may not do, according to his own option; but if he has published a Thesis, he must defend it, and the Degree must be conferred in the Comitia.

5. That the whole of the examinations shall be carried on, and the Commentaries on the Aphorism and Case must be written in the Latin language.

Medical Lectures.

We recur with satisfaction to our annual task of announcing the various Medical, Surgical, and Scientific Lectures delivered during the Winter Season in this Metropolis. The experience of the various Lecturers, their extensive practice in this populous City, and the numerous cases always furnished of every disease by our great Hospitals, necessarily render London THE FIRST SCHOOL OF PRACTICAL MEDICINE IN THE WORLD. We are happy to find that this truth begins to be properly understood, and that the classes of the various Lecturers are every year greatly increased in number, not only in native Students, but

but in Foreigners from every University in Europe and America, so as to make a total number of six or seven hundred in every Season.

The following Courses of Lectures will be delivered at the Medical Theatre, St. Bartholomew's Hospital, during the ensuing winter:—On the Theory and Practice of Medicine, by Dr. ROBERTS and Dr. POWELL. On Anatomy and Physiology, by Mr. ABERNETHY. On the Theory and Practice of Surgery, by Mr. ABERNETHY. On comparative Anatomy and Physiology, by Mr. MACARTNEY. On Chemistry, by Dr. EDWARDS. On the Materia Medica, by Dr. POWELL. On Midwifery and the Diseases of Women and Children, by Dr. THYNNE. The Anatomical Demonstrations and Practical Anatomy, by Mr. LAWRENCE. The Anatomical Lectures will begin on Tuesday, October 1, and the other Lectures on the succeeding days of the same week. Further particulars may be learned by applying to Mr. Nicholson, at the apothecary's shop, St. Bartholomew's Hospital.

The Winter Course of Lectures given at the adjoining Hospitals of ST. THOMAS'S and GUY'S will commence in the following order.—At St. Thomas's:—Anatomy and the Operations of Surgery, by Mr. CLINE and Mr. ASTLEY COOPER, on Tuesday, October 1, at one o'clock. Principles and Practice of Surgery, by Mr. COOPER (illustrated by select Cases under his care in Guy's Hospital), on Monday, October 7, at eight in the evening. —At Guy's Hospital:—Practice of Medicine, by Dr. BABINGTON and Dr. CURRY, Wednesday, October 2, at ten in the morning. Principles and Practice of Chemistry, by Dr. BABINGTON and Mr. ALLEN, on Thursday, October 3, at ten in the morning. Midwifery, and Diseases of Women and Children, by Dr. HAIGHTON, on Friday, October 4, at eight in the morning. Pathology, Therapeutics, and Materia Medica, by Dr. CURRY, on Friday, October 4, at eight in the evening. Physiology, or Laws of the Animal Economy, by Dr. HAIGHTON, on Monday, October, 7, at a quarter before seven in the evening. Experimental Philosophy, by Mr. ALLEN (Lecturer at the Royal Institution), on Tuesday, October 8, at half past six in the afternoon. Clinical Lectures on select Medical Cases, from November till May, by Dr. BABINGTON, Dr. CURRY and Dr MARCET. Besides these, a Course of Lectures will be given on Veterinary Medicine, by Mr. COLEMAN, Professor at the Veterinary College. And one on the Structure and Diseases of the Teeth, by Mr. FOX, Surgeon-Dentist. These several Lectures are so arranged that no two of them interfere in the hours of attendance; and the whole is calculated to form a complete Course of Medical and Chirurgical Instruction. Terms and other particulars may be learnt by applying to Mr. STOCKER, apothecary to Guy's Hospital; who is also empowered to enter gentlemen as pupils to such of the Lectures as are delivered at Guy's.

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ST. GEORGE'S HOSPITAL.—The first Monday in October next will commence a Course of Lectures on Physic and Chemistry, at the Laboratory in Whitcomb Street, at the usual morning-hours, viz. on the Therapeutics at a quarter before eight; on the Practice of Physic at half after eight; and on Chemistry at a quarter after nine o'clock; by GEORGE PEARSON, M. D. F. R. S. of the College of Physicians, and Senior Physician to St. George's Hospital, &c. &c. These Lectures are delivered every morning, except on Saturdays, when a Clinical Lecture is given, on the cases of patients in St. George's Hospital. Proposals may be had at St. George's Hospital, and at No. 14, Leicester Square.

Mr. HEADINGTON and Mr. FRAMPTON will commence their Autumnal Course of Lectures at the Theatre of the London Hospital, on Anatomy, Physiology, and the Principles and Operations of Surgery, on the 1st of October, at two o'clock. The Anatomical Demonstrations and Dissection by Mr. ARMIGER. —Dr. DENNISON will Lecture at the same place on the Theory and Practice of Midwifery.

The established plan of instruction for the benefit of pupils attending the WESTMINSTER HOSPITAL, will be pursued throughout the ensuing winter, under the direction of Mr. Lynn and Mr. Carlisle. Further particulars may be had at the Hospital in James Street.

Dr. BADHAM'S Autumnal Course of Lectures on the Institutions and Practice of Medicine, Chemistry, &c. will be commenced on October the 15th, at his Laboratory, in Clifford Street, at eight o'clock, and continued at the same hour. To this and to the succeeding course of lectures, those gentlemen who had become perpetual pupils to Dr. Crichton, will be admitted. A prospectus may be had.

Dr. BATTY'S usual Course of Lectures on the Theory and Practice of Midwifery, and on the Diseases of Women and Children, will commence on Monday, October 7, at his house in Great Marlborough Street, at half past ten o'clock.

Mr. BLAIR'S Lectures on the Natural History of Man (for the information of scientific and professional gentlemen, amateurs of natual history, students in the liberal and fine arts, &c.) will recommence on the 28th of January, at the Bloomsbury Dispensary, Great Russell Street; to be continued every succeeding Tuesday and Friday evening, at eight o'clock precisely, until the termination of the course, which will consist of about twenty lectures.

Dr. BRADLEY will commence his Autumnal Course of Lectures on the Theory and Practice of Medicine, in the second week of October.

Mr. BROOKES will commence his Autumnal Course of Lectures at the Theatre of Anatomy, Blenheim-street, Great Marlborough-street, on Anatomy, Physiology, and Surgery, on Tuesday, Oct. 2, at two o'clock. Spacious apartments, thoroughly ventilated, and replete with every convenience, will be open in the morning till two, for the purposes of dissecting and injecting, where Mr. Brookes attends. An extensive museum appertains to the Theatre.

Mr. CARPUE will commence his Anatomical Lectures on the 30th of September. The Dissecting Room will be open from eight o'clock in the morning till five in the afternoon. Three courses are given in the year. Further particulars may be had of Mr. Carpue, at his house, No. 50, Dean Street, Soho.

Mr. CHEVALIER, surgeon extraordinary to the Prince of Wales, and surgeon to the Westminster General Dispensary, will begin his Winter Course of Lectures on the Principles and Operations of Surgery, on Monday the 7th of October, at seven o'clock in the evening, at his house in South Audley Street, Grosvenor Square, where printed particulars may be had.

Dr. CLARKE will begin his usual Course of Lectures on the Theory and Practice of Midwifery, and the Diseases of Women and Children, on Friday the 4th of October, at the Lecture Room, No. 10, Upper John Street, Golden Square. For the convenience of gentlemen attending the different hospitals, these lectures will be given from a quarter past ten to a quarter past eleven in the morning. Particulars may be known by applying to Dr. Clarke, Burlington Street, or to Mr. Clarke, at the Lecture Room.

Mr. MILBURNE's Physiological Lectures, illustrated by Anatomical preparations, casts, drawings, &c. &c. will recommence the first Monday evening in October; to be continued every succeeding Monday evening, as eight o'clock precisely.

Dr. REID's First Winter Course of Lectures, considerably enlarged, will commence early in October, and will be delivered in some part of the city. Particulars may be learned from Dr. Reid, Grenville Street, Brunswick Square.

Mr. JOHN TAUNTON, Member of the Royal College of Surgeons in London, Surgeon to the City Dispensary, &c. will commence his first Winter Course of Lectures on Anatomy, Physiology, Pathology, and Surgery, in October next, at the Theatre of Anatomy. An ample field for professional instruction will be afforded by the privilege which the pupils may enjoy, by attending the clinical practice of both the City and Finsbury Dispensaries. Lectures will be delivered on the Theory and Practice of Medicine, by Dr. REID; and on Midwifery, including the Diseases of Women

men and Children, by Dr. SQUIRE. Further particulars may be known on application to Mr. Taunton, No. 10, Pater-noster Row, Cheapside.

Mr. THELWALL, whose intention of commencing a Course of public and private Instructions in Elocution, in the metropolis, during the ensuing winter, we have already noticed, has, in the mean time, opened a seminary at Brownlow Hill, Liverpool, for the Relief of persons afflicted with *Impediments of Speech*, and the instruction of foreigners desirous of being initiated in the idiom and pronunciation of the English language.

At the Theatre of Anatomy, in Great Windmill Street, Mr. WILSON's Lectures on Anatomy, Physiology, Pathology, and Surgery, will begin on Tuesday, the 1st of October. Two courses of lectures are read during the winter and spring seasons.—In the first course is explained the Structure of every part of the Human Body, so as to exhibit a complete view of its Anatomy, as far as it has been hitherto investigated; to which are added, its Physiology and Pathology. In the second course, the Structure of the Human Body is again explained; after which follow Lectures on the Operations of Surgery; and the course concludes with the Anatomy of the *Gravid Uterus*. A Lecture is given daily from two till four o'clock. Practical Anatomy in the mornings as usual. A plan and terms of the course may be had at the Theatre.

INDEX TO THE FIRST SERIES OF THE MEDICAL AND PHYSICAL JOURNAL.

The numerous friends and purchasers of the Medical and Physical Journal, are respectfully informed, that it is intended to prepare and publish, after the completion of the TWENTIETH VOLUME, *and of every future twentieth volume, a very full and complete Index to the preceding series; such Index to include separate references to the Diseases, Remedies, and Writers.*

Prefixed will be given a general History of Medicine, Surgery, and the Physical Sciences, during the period of the publication of the twenty volumes, with particular reference to the valuable communications of the Correspondents of the Medical and Physical Journal.

It is supposed that the Historical Introduction and the Index will, together, form a volume nearly equal in size to an ordinary volume of the Journal, and perhaps not inferior in value, as a body of useful reference, to any medical work in the English language.

To CORRESPONDENTS.

Communications are received from Mr. Dunning, Mr. Wadley, Mr. Weston, Cantab, and S. M.

ERRATUM.

P. 242—245, Running Title, for Mr. Cooper, read Mr. Cribb.