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Medical College of Georgia.

"Je prends le bien où je le trouve."

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PART I.—ORIGINAL COMMUNICATIONS.

ARTICLE I.

Remarks on the Pathology and Treatment of Convulsions. By L. A. Dugas, M. D., Professor of Physiology and Pathological Anatomy, in the Medical College of Georgia.

The announcement by M. Trousseau of a new method of arresting Convulsions,* furnished me with an opportunity of making a few remarks on the Pathology of those affections, in the second volume of the Southern Medical and Surgical Journal, p. 546, published in 1838. The following article is designed as an extension of the views then presented.

The term Convulsion, in its most extended sense, has been applied to all morbid contractions of the muscular fibre. Thus considered, it comprehends all the degrees of disordered action to be found between the two extremes of mere tremor and permanent contraction. And, were the etymology of the term not adverse to this acceptation, there would be a manifest convenience in its adoption, for, as will be seen in the sequel, all these degrees of disordered action, may be regarded as mere manifestations of as many degrees of the same condition of the nervous system, or rather of innervation. Being, however, in possession of no term adequate to the expression of all these phe-

* Journal des Connaissances Médico-Chirurgicales, 1837.
nomena, and, looking to the true import of the word Convulsion, it should be restricted to designate all abnormal series of alternate contractions and relaxations of the muscular fibre. It is in this sense we shall use it.

Distinctive appellatives have been assigned to convulsions, according to the various circumstances under which they may occur; hence we have convulsions termed infantile, puerperal, hysterical, epileptic, hydrophobic, &c. It is evident, however, from our definition, that we comprehend under the denomination of convulsions, all abnormal series of alternate contractions and relaxations, without regard to the number of muscular fibres implicated, nor to the degree of violence with which they are affected. We therefore recognize as convulsions, local as well as general contractions, and tremors as well as epilepsy. Indeed, when examined with the lights furnished by modern physiology, they are, as already remarked, really but different degrees of the same modification of innervation.

Of the classifications of the affections of the motor system proposed by the several Nosologists, that of Sauvages appears the least objectionable. I will therefore insert it here for the purpose of subsequent reference, and offer one of my own, in which it is simply attempted to supply the deficiencies of that of this distinguished Nosologist.

**Classification by Sauvages.**

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<th>1st Order—Partial tonic spasms.</th>
<th>2d Order—General tonic spasms.</th>
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<td>Wry-neck.</td>
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**Classification by L. A. Dugas.**

*Affections of the Motor System.*

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<th>1st Order—Partial Tonic spasms. of small intestines.</th>
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<td>Trismus.</td>
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<td>Muscular stiff-joint.</td>
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<td>Priapism.</td>
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<td>Cramp.</td>
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<td>of voluntary muscles.</td>
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The causes of convulsions may with great propriety be divided into the predisposing and exciting. The former are either congenital or acquired. That the congenital predisposition to convulsions may be hereditary does not admit of a doubt, as the observation of all practitioners will attest. No children are so subject to these affections as those born of parents similarly predisposed; hence the offsprings of hysterical mothers are singularly prone to convulsions. There are many instances of this peculiarity being transmitted to several successive generations, and of mothers, all of whose children have fallen victims to it. The congenital predisposition is, however, not always the effect of hereditary transmission, for it is not uncommon to find children extremely susceptible to convulsions, when no such susceptibility could be recognized in their parents. The undue preponderance of the nervous system, and a general deficiency of tonicity or stamina may usually be observed in such individuals. According to Jolly,* "all the physiological and pathological conditions that increase the susceptibility of the brain to respond to external irritations, are efficient causes of convulsions." Baumes, who has certainly written the best practical work on infantile convulsions, holds the following language:† "The mobility of the system is in a direct ratio with the feebleness, laxity and delicacy of the various parts of the living body. Upon these evident peculiarities depend the extreme reaction

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* Dict. de Méd. et de chir. Pratiques, art. convulsions.
† Translated from p. 21 of the "Traité des convulsions dans l'enfance, par M. Baumes. 2me. edition. Paris, 1805.
of the sensorium, the great susceptibility of the nerves, and the undue mobility of the muscles.” It is in this mobility of the system, consequent on the deficient powers of resistance peculiar to all newly formed tissues, that we may refer the greater susceptibility of children than adults to convulsions, and, in proportion to the difference between the full development of energy and stamina in the male and female adult, will be found the disposition to such disorders. Females are, in general, therefore, more subject to them than males; and those reared under the encraving influence of city life, of sedentary habits, &c. still more so.

It has been remarked, that it is during the period of the evolution of the body, when new tissues are being formed, that the system presents the greatest degree of susceptibility, and is therefore most liable to convulsions. This is no doubt correct; but it is also urged by some that in females this period may be said to continue much longer than in males, inasmuch as even after the full development of her own system, the formation of new tissues is continued in the production of new beings—in the evolution of the fetus in utero. This, however, can scarcely be adduced as a reason of their greater aptitude to convulsions, for it is to the imperfect solidification or tonicity of newly formed tissues, especially the nervous, that this aptitude should be attributed, and not to the act of formation. At the same time that the mother contributes the elements of new tissues to her offspring, her own tissues cannot be said to undergo any special modification, save in the uterus. Unless, therefore, the mere development of the uterus under impregnation be regarded as a process of evolution, similar to that of growth, the argument cannot be valid. Yet it is true, that females are more subject to convulsions during pregnancy than during any other portion of their adult life, and that they cease to be so after the cessation of child-bearing. It is a remarkable fact that the disposition to convulsive affections decreases rapidly after this period, and that the aged of both sexes scarcely ever suffer from them—Baumes says they never do.*

The predisposition to convulsions may be acquired by any train of circumstances calculated to retard or to impair the solidification and perfection of the tissues, and, consequently, the acquisition of strength and stamina. Acute and chronic diseases, atmospheric vicissitudes, insufficient or unwholesome food and raiment, deficient ventilation,

want of exercise, &c. are among the most common causes of this acquired habit.

The exciting causes are as numerous as the agents capable of disturbing the function of innervation. These might, according to Hippocrates, be summed up under the head of repletion and depletion. Thus: "Convulsio fit, aut à repletione, aut ab depletione."* To which Galen, in commenting on this aphorism, very properly adds irritation. Convulsions, however, occur under circumstances that cannot be referred to either of these heads. They may be occasioned, for example, by peculiar odors, musical or harsh sounds, affecting sights, &c.; nay, even by the workings of imagination, as was strikingly exemplified by the fanatical sect of Convulsionists of St. Medard, who, in their meetings, frequently went into violent convulsions. Indeed the same may be occasionally seen in our own time and country under a somewhat similar state of the mind.

But direct or indirect irritation of the nervous system is by far the most common exciting cause of convulsions. In using the term irritation, it may be well to state that we do not thereby wish to designate one of the stages of inflammation, but merely an impression which, when perceived, is followed by a perturbation of some of the acts of the economy. Now, whilst sensations are usually but the perception of impressions made on the tissues by some mechanical or physical agent, there are sensations developed within the system independently of any antecedent impression that we can detect.—Hence, the latter are denominated internal sensations, and their cause is said to be physiological or functional. Let the nostrils be titilated by a feather, the mechanical irritation is immediately perceived, or a sensation is produced, and this is followed by a sudden, convulsive action of certain muscles, constituting sneezing. The convulsions will then intermit and return again, until the cause of irritation be removed. Yet sneezing will as effectually be produced by the irritation of the lining membrane of the nostrils consequent on a modification of its capillary circulation, as occurs in commencing coryza. Here we have an instance of internal or functional sensation, the irritation being independent of any mechanical or physical impression. In like manner, vomiting may be excited by irritating the fauces, and by functional disorder of the stomach; laughing may be the effect of tickling or of a physiological condi-

* Aphor. 39 sec. vi.
tion of the brain, or of the morbid state of the system termed hysteria. What is true in those cases, is equally so with regard to more violent convulsions. These may likewise be occasioned by mechanical and by functional irritations, by the pressure of a tooth making its way through the gums, by the presence of indigestible food in the stomach, by worms in the intestines, as well as by spontaneous gastric or intestinal irritation. Traumatic causes as well as occult ones, will induce epileptic convulsions; and intermittent fever, in which there is always more or less spinal irritation, may be attended by all the grades of convulsion, from the tremors of the cold stage to the most violent eclampsia. It is a singular fact that the violence of convulsions is by no means in a direct ratio to the violence of the apparent irritation, the slightest wounds occasionally giving rise to violent tetanus, and lesions so mild as to escape detection, causing epilepsy and eclampsia.

The tendency of the age is to the localization of maladies, and whilst we would be among the foremost in acknowledging the immense benefits which have accrued to the science of medicine by this course, we must confess that in the study of convulsions it has rather misled certain authorities, otherwise entitled to much respect. The chief purpose of Brachet, in the preparation of his valuable monograph on infantile convulsions,* appears to have been the demonstration of the existence of lesions in the brain, and especially of its meninges in the great majority of convulsive affections. Among the various authorities he invokes in support of his views, we find Portal, who, in summing up the lesions of the encephalon, detected in cases in which convulsions had occurred,† enumerates 1st, the collection of air, water and gelatinous matter; 2d, congestion of the blood-vessels or sanguineous effusions; 3d, engorgements composed of various substances; 4th, inflammation; 5th, induration; 6th, abscesses; 7th, ulcers; 8th, wounds; 9th, foreign bodies; 10th, increased or diminished volume; 11th, change of color, &c. But Portal takes care to remark that those lesions are not peculiar to convulsions. Brachet also cites a number of cases from Morgagni;‡ Lallemand,§ Abercrombie,¶ Andral,¶¶ &c., in many of which it is by no means demonstrated that the lesions were not rather the effect than the

cause of the convulsions. The congestions and effusions, for in-
stance, so frequently found in post-mortem examinations, may
assuredly be the effect of the convulsions, inasmuch as these, when
violent, always produce intense congestion of the organs contained
in the cranium and spinal column. So far then from considering
these lesions as the cause of the convulsions, we should expect to
find them as a necessary consequence of such disorders. That the
inference drawn from these cases is not always legitimate, will
appear from the first of the series he adduces. It is taken from
Morgagni, and is as follows:

"Obs. xlvii. A girl five months old was taken with fever and
looseness of the bowels. The following day the fever alone persisted.
The third day she was seized with strong chronic convulsions of the
upper extremities, extending with less intensity to the muscels pos-
terior to the chest, and still less to the glutei muscles. Towards the
end of the fifth day, the convulsions diminished, were manifested
only at intervals, and were entirely suspended when the child slept.
Universal jaundice now supervened.

"After death, the entire surface, but especially that of the back,
presented dark red blotches. In the abdomen, nothing remarkable
was discovered, save that the rectum was of a black color. The
pericardium was filled with yellow fluid; the right ventricle of the
heart contained polypous concretions which extended into the pul-
monary artery, and which resembled concrete mucus. The blood
every where else was liquid, but slowly coagulated when exposed to
the air. Within the cranium all was natural, with the exception of
a serous humour found between the dura mater and pia mater, and
which had formed concretions around the blood vessels, in the form
of gelatine."

In this case, Brachet thinks the convulsions were manifestly
casted by meningitis. Was there not also in this case intense dis-
order of the rectum and of the biliary apparatus? Was there not
an effusion in the pericardium as well as in the cranium? And if
effusions, (as our author seems to think, but to which we cannot ac-
cede,) be indicative of irritation, why not admit it also to have existed
in the pericardium as well as in the meninges?
If it be borne in mind that Brachet derives almost all his cases
from special works on diseases of the brain, the following passage
will no longer excite surprise:* "I will not quote a greater number

* Translated from op. cit. p. 345.
of autopsic examinations. Had I added even many more, the result would have been the same. They would all have presented the same lesions: congestions, inflammations, abscesses, membranous or gelatinous productions, tubercles, hydatids, osseous deposits, &c." Yet he unwittingly exposes the bias under which Morgagni's cases were drawn up in the following paragraph:* "This celebrated author (Morgagni) is so well convinced that convulsions depend on meningeal irritation, that, wherever he does not discover traces of inflammation, he considers as a sufficient cause of irritation the small quantity of serous matter found in the ventricles, and announces that we are not to judge of irritating causes by their quantity, but by their power, and finally admits that this serous matter possesses an acrid irritating property."

Believing convulsions to be symptoms and not special diseases, the only advantage we can perceive in these attempts to localize it, is the collection of facts that may be used to establish the relative proportions of the various exciting causes of those phenomena. The predisposition to convulsions might we think be regarded with much more propriety as a special disease than the exciting causes, for without that condition of the system, the same irritation will not excite convulsions.

Let us now analyze the phenomena presented during a paroxysm of convulsions, and in order to do so the more effectually, let us select hysterical eclampsia, or epilepsy. The individual is, to all appearances, in the enjoyment of perfect health, when more or less suddenly, he falls to the ground with violent, though intermittent, contractions of all the muscles of animal life, and with loss of consciousness. I say with loss of consciousness, rather because such appears to be the case, than from a settled conviction that consciousness is abolished at the onset of the attack. The mere facts that the patient cannot then manifest consciousness, nor remember after the paroxysm, what has occurred, should not satisfy us on this point, inasmuch as, in the first place, he has lost the control of the organs (the muscles) by which he could express consciousness, and in the second we have continually instances in which circumstances taking place without any apparent deterioration of this faculty, are not remembered a short time afterwards. Be this as it may—at the onset of a paroxysm of epilepsy, the patient soon passes into a state of stupor, with

stertorous breathing, full, strong and slow pulse, congestion of the superficial veins and complete relaxation of the muscles. He is then unquestionably unconscious, and will remain so until the equilibrium of the circulation be restored. Frothing at the mouth, symptoms of asphyxia, priapism, defecation, &c. may occur or not during the convolution, according to the intensity of action in special muscles. If, at the moment the contractions cease, the case were seen by one unapprised of the antecedents, it would certainly be regarded as presenting all the characteristics of apoplexy, viz. loss of consciousness, stertorous breathing, perfect relaxation of muscles, and full, strong and slow pulse. The stupor gradually subsides, the patient breathes better, begins to swallow his saliva, groans, turns over or changes his position, and finally opens his eyes and asks the by-standers, what is the matter? He is again a well man, with the exception of the lassitude consequent on the violent exertions, but which soon wears off. Such is, however, not the termination of all cases of convulsions. It not unfrequently occurs that as the stupor diminishes and consciousness returns, the patient will open his eyes, look about as though intending to speak, and immediately be again seized with convulsions as before; or the return of the convulsions may take place after the complete restoration of intellect and voluntary motions, even after all traces of the former attack had disappeared; or they may terminate fatally.

We have then, manifestly, in violent general convulsions, regardless of their cause, two distinct stages or conditions of the system—the one a state of violent muscular action, and the other of total muscular relaxation—the one a state of high excitement, and the other of torpor. Now, whence this change? Is the nervous system, or innervation in the same condition during these two stages? Certainly not. Can the same cause produce, directly, effects so opposite? I apprehend not. The explanation is found in the old maxim, that irritation causes convulsions, and compression paralysis. Whatever be the immediate cause of the first stage of the paroxysm, the phenomena are those that pertain exclusively to disordered innervation, to a modification of the functions of the nervous system that may be induced by various agents, mechanical, chemical, or physiological, acting in almost any portion of the system. That it is only through the intervention of certain portions of nervous system that such phenomena can be developed, will be subsequently shown. The violent contractions then may be
assigned to a modification in the supply of nervous influence to the muscles affected. But we have seen that violent and general convulsions are invariably attended by a stage of stupor, stertorous breathing, relaxed muscles, &c. This, the second, or the apoplectic stage, comes on more or less early in the attack, according to its intensity, usually in from 2 to 5 minutes. The more violent and general the convulsions, the sooner will the second stage ensue, and consequently the sooner will the paroxysm be terminated; whereas, if the convulsions be slight, less intense or partial, the apoplectic stage of stupor may be much longer in coming on, or may not follow at all. In such cases the continuance of the convulsions varies from a few minutes to hours, and even days. Partial convulsions or such as affect only a portion of the body, are those which usually continue the greatest length of time, and are very rarely attended with unconsciousness—never with the apoplectic state. Let me not be misunderstood: I am aware that in certain cases of Eclampsia and even of Epilepsy, followed by the stage of stupor, or apoplectic stage, it would appear that there is but one side of the body affected. Such however is not strictly true in such cases, for, although the muscles of one side are more violently agitated than those of the other, these are by no means in a state of relaxation, but will generally be found quite rigid. I repeat, if one side only is affected, the other being in a normal state, there will be no apoplectic stage and the convulsions will continue for an indefinite time. These remarks recall to my memory a case I witnessed about two years since, in which the lady's entire right side, including the right side of the face, was affected with strong convulsions for several hours, without being followed by stupor nor attended with impaired intellect. Indeed she retained the power of speech and deglutition during the whole time, crying out for relief and swallowing readily the remedies presented her. Cases more or less similar to this are by no means unfrequent among hysterical females.

If it be now asked why the apoplectic stage occurs in general and not in partial convulsions, the solution is quite easy. Examine for a moment how the circulation is affected under these circumstances. It will be seen at once that whenever the muscles are violently convulsed, the blood contained in the muscular as well as intermuscular veins must be forcibly driven out and accumulated in those veins so situated as to be exempted from the compression. These are found in the surface of the body and within the cavities of the chest, spine
and head, and observation demonstrates that the organs of the chest, spine and head are precisely those that suffer the greatest degree of congestion under such circumstances. Again, the act of respiration is materially impeded by the fact alone of its muscles and those of the larynx being affected. This impediment then, taken in connection with the congestion determined as remarked above, will account sufficiently for the deficient aeration of the blood, and for the symptoms of asphyxia presented during violent general convulsions. It is well known that asphyxia is always attended with congestion of the nervous centres; so that we have here two conditions of the system, each tending strongly to accumulate blood in organs contained within unyielding walls, and consequently where no such accumulation can take place without compression of the soft pulpy material of said organs. The nervous centres are really and truly compressed in this manner, until their faculty of perception as well as that of dispensing nervous influence be materially diminished or even irretrievably destroyed. If these faculties are merely diminished, so that neither the cause of the convulsion can be perceived or give offence, nor the muscles be supplied with nervous power to contract, the convulsions will cease, and consciousness will gradually return as the congestion subsides. If the cause of the convulsion still continue, however, the return of consciousness will be attended with a return of convulsions—thus constituting the alternations of convulsive action, apoplectic stupor, recovery and relapse already described. But if these faculties be entirely abolished, death will be the necessary consequence.

In cases of slight or partial convulsions the phenomena of congestion above recited must necessarily be correspondingly slight or partial. Hence the nervous centres retain the perceptive and dispensing faculties—the cause of convulsion continuing to act, continues to give offence to the nervous sensibilities, and innervation continues deranged. There are certainly many instances in which the cause of the convulsion not being apparent, we cannot perceive that it has been removed, and yet the convulsion ceases. May we not admit that the perturbation of the circulation consequent on convulsions may, of itself, so modify irritation or its cause as to lessen its intensity or bad effects? Many of our most efficient remedial agents act in this way. The warm bath, cold affusion, diffusible stimuli, &c. may be regarded as modifying the capillary and general circulation. Emotics, even in hysteria, and other cases in which the stomach cannot be considered the seat of irritation, constitute one of our
most effective means of arresting convulsions, and, doubtless, afford relief by the great perturbation of the circulation occasioned by the act of vomiting.

The study of the pathology of convulsive affections demands an acquaintance with the physiology of the organs implicated. These organs are the muscles and those portions of the nervous system by which they are supplied with nervous influence. The muscles themselves, being inert when cut off from the influence of the nervous system, it becomes highly important to determine the laws and circumstances under which such influence is manifested. These have of late years been studied with great success by Magendie, Flourens, Sir Charles Bell and Marshall Hall, and the conclusions to be deduced from their researches will be very briefly stated. The nervous system is now divided into three very distinct and well defined portions; the brain, the spinal marrow, and the ganglionic system; and to each of these portions, very distinct functions are assigned. The brain is the seat of perception or sensation, of volition, and of the mental faculties; the spinal marrow is both a medium of communication with the brain, and an independent excito-motory apparatus; the ganglionic system presides over the functions of organic life, as nutrition, formation, secretion, &c. To the brain then must be referred all the abnormal conditions of sensation, of volition, and of mind—pain as well as paralysis, perversions as well as loss of the will, mental aberrations as well as idiocy. Lesions of the senses, acts of violence, insanity,—all have their origin distinctly in the brain. Vivisections clearly demonstrate that injuries inflicted on the brain, induce neither pain nor muscular contractions, and, consequently that this organ is endowed with neither sensibility nor the vis nervosa of Haller. But when the brain is irritated, delirium ensues, when it is compressed coma follows, and when destroyed paralysis or loss of voluntary motion is the consequence. According to Dr. M. Hall, if other phenomena accompany diseases of the encephalon, they arise from the extension of the influence of these to the true spinal and ganglionic systems, through irritation or pressure, counter-irritation or counter-pressure. Let us here be permitted to quote the learned Doctor's own words on this interesting portion of our subject:*

* Mem. on some Principles of Pathology of the Nervous system.
"M. Andral speaks of irritation of the cerebrum as the cause of abnormal muscular contractions. Now, in our investigations into the nature of cerebral diseases, we must remember one circumstance; it is impossible to induce muscular action by any irritation of the substance of the cerebrum itself. Whenever, therefore, there are spasmodic affections in diseases of the nervous system, we must conclude that the spinal system is involved, either primarily or secondarily, in the disease. Irritation of the cerebrum may induce delirium and other disorders of the cerebral functions; congestion of the cerebrum may induce coma, paralysis, &c. But if these morbid conditions of the brain be attended by spasmodic or other deranged actions, it is because the true-spinal system is involved in the disease, or affected by it in the way of irritation, counter-irritation, or of pressure, or counter-pressure. Hence we observe the symptoms of spasm in various diseases of the encephalon, the condition being not the nature of the disease, but that they produce these intermediate effects. Time, as is well known, is a very important element in this problem; and why is it so? The fact is to be explained on the same principles. The very same lesion occurring quickly, will produce effects which will be totally absent if it creep on slowly. In the former case, we have the effects of irritation and pressure, or of counter-irritation and counter-pressure; in the latter, the cerebrum has so accommodated itself to the new state of things, probably by the altered condition of its vessels, as to avoid these effects, except towards the close of the disease.

"We need not, therefore, now view with surprise the fact that the same lesions as found post-mortem, had been attended by a totally different series of symptoms during life, any more than the other fact that, in the different periods of that lesion, the symptoms have been different.

"The symptoms frequently subside too and re-appear. If the disease be not regularly progressive, the encephalon accommodates itself as I have stated, and the symptoms disappear; if now the disease proceeds, the symptoms also return. At least all this may be.

"A rapid effusion of serum may resemble haemorrhage or ramolissement in its effects; or slow effusion may merely obscure the intellectual faculties." 105.

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"It is well known from the experiments of M. Flourens especially, that irritation of the cerebrum has no influence in inducing spasmodic action. Whenever, therefore, spasmodic symptoms occur in diseases of the cerebrum, it must, as I have already stated, be on a principle different from that of irritation of the cerebrum itself; it must be from an impression made upon parts of the nervous system in which the property of exciting spasmodic action on being subjected to irritation resides; these parts are the tubercula quadrigenina, the medulla oblongata, the intra-cranial nerves, &c.

"That undue counter-pressure on the medulla oblongata may, and
actually does, excite convulsions, is proved by the following facts:—

In the interesting case, most anxiously watched and accurately de-
tailed to me by my friend Mr. Toogood, of Bridgewater, of his own little girl, aged thirteen months, the croup-like convulsions occurred repeatedly, until one day, when the bones of the cranium separated, and the convulsion ceased. In a case of spina-bifida, related to me by Mr. Herbert Evans, of Hampstead, there was a croup-like convul-
sion whenever the little patient turned so as to press upon the tumour. In the case of an anencephalous foetus, described by Mr. Lawrence, convulsion was produced on pressing on the medulla oblongata. In a case of meningitis, given by Dr. Abercrombie, the anterior fontanelle became very prominent. Pressure upon it induced convulsion. Hy-
pertrophy of the brain affords an argument of the same kind: it induces convulsion, except in the case in which the cranium grows with the encephalon. These and other facts lead me to think that convulsion arising from cerebral disease is thus to be explained." 107.

We have stated that the spinal marrow is a medium of communi-
cation with the brain, and an independent excito-motory apparatus. That it is a medium of communication with the brain, is easily shown by sections of this cord, which deprive all the parts receiving nerves from below the section, of the influence of sensation and volition, and consequently of voluntary contraction. Yet contractility is not annihilated by such sections, in these parts—contractions may still be excited by irritating causes applied to the nerves, or directly to the spinal marrow. Hence the theory of the excito-motory process so happily developed by Marshall Hall. His views may be gleaned from the following paragraph:

"A peculiar set of nerves constitute, with the true-spinal marrow as their axis, the second subdivision of the nervous system. As those of the former subdivision (the brain) were distinguished into sentient and voluntary, these may be distinguished into the excitor and motory. The first, or the excitor nerves, pursue their course principally from internal surfaces, characterized by peculiar excita-
bilities, to the true medulla oblongata and spinalis;* the second, or the motor nerves, pursue a reflex course from that medulla to muscles having peculiar actions concerned principally in ingestion and eges-
tion. The motions connected with the former, or cerebral subdivi-

* Dr. Hall includes the tubercula quadrigemina in the true-spinal system. He regards as excitor nerves of this system, the fifth, the pneumogastric, and the posterior spinal nerves; and as reflex or motor nerves, the fourth, sixth, por-
tio dura of the seventh, the pneumogastric, the spinal accessory, the phrenic, the inferior external respiratory, and the spinal nerves. (Op. cit.)
sion, are sometimes, nay, frequently, spontaneous: those connected with the true-spinal are, I believe, always excited."

The muscles of voluntary motion then contract under the mandates of volition emanating from the brain, and also under the influence of irritation direct or indirect of the spinal marrow, and this whether the communication of their nerves with the brain be cut off or not,—whether sensation be developed or not. Nothing is more common than to find a limb so paralyzed as not to move in the least under the strongest efforts of will, and yet be affected with more or less violent contractions when a feather is passed over its surface, or it is otherwise irritated. In such cases, as has been justly remarked by Marshall Hall, the seat of the lesion must be that of volition—the brain; for the persistence of the reflex actions indicate an unimpaired condition of the seat of involuntary motions—the spinal system. If on the contrary it be found impossible to excite motions in the paralyzed limb, the lesion should be referred to the spinal system. This is an important means of diagnosis.

Seeing that voluntary movements emanate alone from the brain, independently of excitement or irritation properly so called, and that all involuntary movements are manifestly produced by mechanical, physical, or functional excitement or irritation, the true pathology of convulsions is perfectly plain. Convulsive contractions are involuntary—therefore not produced by the organ presiding over the functions of volition, but by that organ which presides over motions susceptible of being excited by stimuli other than that of volition. Excite the nostrils, or the fauces, or the anus, and, despite of the efforts of the will, sneezing, vomiting, or contraction of the sphincter ani will ensue, and these sequences will obtain whether the irritation have been perceived by the brain or not, as may be demonstrated in vivisections in lower animals and in cases of paraplegia and hemiplegia in man. If such things take place in localities exposed to our view, there is no reason why they should not in those more deep seated or hidden.

In the physiological or healthy state of the system, these phenomena of reflex action are essential to the normal performance of the acts of the economy. It is only when they occur under the influence of a pathological state of the system that they constitute convulsions. In

idiopathic convulsions the lesion is in the centre of the true spinal system—and in sympathetic convulsions at its periphery. In the former, the movements take place without the intervention of the excitor nerves; in the latter they result from irritation transmitted by these excitors to the centre. To the former class may be referred the trembling of the cold stage of intermittent fever, chorea, carphology, claudication, &c. and sometimes hysteria, epilepsy, &c.; whilst to the latter class belong traumatic tetanus, and the great majority of the convulsions thrown under the head of eclampsia in the classification we have proposed, as the infantile, puerperal, hydrophobic, &c.

"Tetanus," says Dr. Hall, "is, in every respect, the most unequivocal example of an affection of the true-spinal marrow, through an incident and the motor nerves. All the functions of this subdivision of the nervous system are affected in the most violent form, whilst the cerebral functions are unaffected: the dyspnœa, the dysphagia, the constipation, the frisnum, the emprosthotonos, the opisthotonos, the extreme susceptibility to causes of physical impression and agitation, and of mental emotion,—all mark an affection of the true-spinal system; whilst the freedom from all affections of the senses and of the intellect, the absence at once of delirium and of coma, denote the normal condition of the cerebral system. Hydrophobia is in the same category."

Again, according to Dr. Hall, "in Epilepsy the very first symptom is generally, if not always, one of the true-spinal kind. The first symptom is constriction about the throat, and closure of the larynx, more or less complete; then follow violent inspiratory efforts and convulsive movements of the trunk and limbs. Intermediately, and even without the convulsive movements, the cerebrum is affected with congestion, and a multitude of cerebral symptoms occur: flashes of light, tinnitus aurium, the aura epileptica; a momentary oblivion; a state of terror, of delirium, or of unconsciousness, &c; as parts of the general convulsion, the tongue is protruded and bitten, the feces, the urine, or the semen expelled; as consequences of that convulsion, the cerebrum is congested, and there is coma. If this state continues, another order of symptoms takes place; the respiration becomes stertorous, and, at length, affected with mucous rattle, the true spinal and ganglionic systems becoming fatally involved in the disease.

"It is the constriction about the throat which assimilates epilepsy to the state of things which exists in strangulation, and which distin-
guishes it from hysteria. It is this circumstance which associates epilepsy with the crowing inspiration of the convulsions of children; all are laryngismal. In epilepsy, there is sometimes a crowing inspiration and convulsion of infants are sometimes followed by epilepsy in subsequent years."

Shall we be told now that convulsive affections are for the most part to be referred to lesions of the brain or of its meninges? that congestion of the brain and effusion are among the most efficient causes of convulsions? that involuntary movements emanate from the organ of volition? It is evident that those who entertain such views have mistaken the effect for the cause, in not duly attending to the order in which are developed the several phenomena presented before, during, and after a convulsive paroxysm.

The main object of this communication has been to elucidate the true character of such affections, in order that correct views of treatment might be attained. If in certain cases the origin of the perverted movements must be traced to the centre of the true-spinal system, whilst in certain others it must be found at the periphery of this system, all that is necessary will be to distinguish these cases from each other, and to localize the peripheral portion affected. If the excitation proceed from the gums, the indication is plain; if from the stomach, the intestines, the cutaneous surface, or even the encephalon, it is equally so. There can be no difficulty in the matter, once that the true nature of the case is ascertained. Each case should be treated on its own merits—due regard being paid to the consequences as well as to the cause of the convulsions. But there can be no question that the same degree of local irritation that will occasion convulsions in one individual, or at one time, will not have the same effect in another individual, or at another time. Hence, as has been already stated, there is a convulsive predisposition sometimes existing in the system, that must be combatted as well as the paroxysm itself and its effects. The treatment of convulsive diseases should therefore be directed against this predisposition, against the immediate cause of the paroxysm, and against the effects of the convulsive struggles.

To the predisposition must be opposed all the means calculated to increase the solidification of the tissues and the stamina of the system. Tonics, especially the anti-spasmodic tonics, wholesome diet, exercise, change of scene, diversion, regular habits, &c. will accomplish these purposes. The predisposition being known to exist, due atten-
tion must be paid to the condition of the localities from which excita-
tion most frequently proceeds; and these will vary according to the
age and sex of the individual. In children, they are the gums, the
stomach, the bowels, the encephalon; in adult females, the uterus.
These localities should therefore be maintained, as far as practicable,
in a state of integrity.

Should an attack nevertheless supervene, it must be treated accord-
ing to the seat and nature of the exciting cause. Lancing the gums,
emetics, cathartics, enemata, anthelmints, emmenagogues, warm
bath, revulsives, &c. will furnish us the means most generally needed.
During the paroxysm of violent general convulsions very little can
be done to moderate its intensity; the supervision of cerebro-spinal
congestion will of itself arrest it. But the paroxysm of partial or of
mild convulsions, may be mitigated, and the threatened return of
violent ones prevented, by the use of the above means, and also, ac-
cording to the suggestion of Dr. Hall, by titillating the fauces and
the nostrils, and by dashing cold water in the face, so as to modify
the condition of the respiratory muscles; inspiration, &c. being pro-
voked by such impressions.

The effects of the convulsive struggles should not be overlooked.
The most formidable are cerebro-spinal compression from congestion
or effusion, and asphyxia from accumulated mucus in the bronchi, or
from constriction of the rima glottidis. The means instituted to ar-
rest the bad effects of the former must be proportioned to its intensity.
Knowing that congestion is the necessary consequence of violent
convulsions, and that it usually subsides, more or less early after the
cessation of the struggles, without leaving any bad effects, we should
not too hastily resort to depletion. The abstraction of a portion of
the circulating mass should be reserved for those cases in which it
may be deemed requisite for the removal of the exciting cause, or for
the moderation of the determination to the cerebro-spinal centres.
Now it is exceedingly rare that the exciting cause is such as to re-
quire active depletion, if any; and I believe it perhaps equally rare
that the congestion is such as to excuse the profuse waste of blood
advised by most practitioners. The expediency of blood-letting, and
the quantity to be taken, should be determined by the antecedent, as
well as the actual condition of the circulation. If the exciting cause
of the paroxysm, and the general state of the system, would not have
warranted the abstraction of blood before the paroxysm, such abstrac-
tion can certainly be tolerated during or after the paroxysm, only in
so far as it may be imperatively called for by the violence of the con-
gestion of the nervous centres. How often have not children, (at
which age the loss of blood is always a serious matter,) and women
in child-bed, when the process of parturition must of itself tax the
system with loss of blood, how often, I say, have not patients of these
classes been hastened to the grave by injudicious depletion? Do we
not continually hear of copious blood-letting having been practised in
convulsions occurring after parturition, and consequently after the
blood-vessels have already been more or less unloaded?—and all this
in the very class of individuals whose constitutional peculiarities
evince that general want of tonicity and stamina which predisposes to
convulsions! If the irritability of the nervous system be in a direct
ratio with the feebleness, laxity and deficient tonicity of the tissues,
should we not pause, should we not carefully weigh the case in all its
bearings, before we resort, in convulsive affections, to the most direct
method of impairing the strength and stamina of the body? I am
persuaded that the indiscriminate and lavish abstraction of blood,
recommended in convulsive affections by some authors, and instituted
by too many practitioners, is most pernicious, and probably one of
the most frequent causes of the fatal result. Opiates and narcotics
in general have been too sparingly resorted to, owing to the erroneous
apprehension of cerebral congestion. This class of remedies, by
subduing the irritability of the nervous centres, accomplishes precisely
what is most desirable when the patient is threatened with convul-
sions. There can be no doubt that by their judicious administration
on the first appearance of the symptoms denoting a convulsive ten-
dency, a large number of cases of puerperal convulsions would be
averted, and that if given after the paroxysm they would prevent their
recurrence. Although these remarks are applicable to almost every
convulsive affection, they are peculiarly so in those cases in which
the spasms are induced both by pain and by irritation, as in those of
the puerperal and traumatic state. Brachet recommends very
highly in infantile convulsions a combination of Ext. Hyosciam. niger
and ox. zinc.

With regard to the tendency to asphyxia manifested in certain
convulsive affections, as epilepsy, spasmodic croup and asthma, puer-
peral eclampsia, hydrophobia, &c. it demands our special attention,
as it is in many cases the immediate cause of death. The rima
glottidis, being in such affections more or less closed, respiration is
correspondingly impeded. It is particularly under such circumstan-
cests that Marshall Hall recommends dashing cold water in the face, and tickling the fauces and nostrils, so as to induce inspiration. They have been proposed also in the asphyxia of new-born infants. The operation of tracheotomy under impending asphyxia in hydrophobia was, I believe, proposed by Dr. Physick, since when its necessity in all similar spasmodic conditions has been strongly urged by Dr. M. Hall and others. Some years ago a patient, apparently dying from asphyxia, consequent on deep intoxication, was saved by the timely performance of this operation, by Mr. Sampson, of Salisbury in England.

ARTICLE II.

An Essay on Auscultation—By Wm. H. Cumming, M. D.*

Since the great discovery which has so illustrated the name of Laennec, the subject of Auscultation has received more attention than any other in the whole range of medical science. The accuracy of its results, the important information which it gives concerning the nature and progress of a numerous and important class of diseases, often existing in an isolated state, and perhaps more frequently still intercurrent with other disorders, sufficiently account for the enthusiasm manifested in the cultivation of this most important mode of observation. We shall attempt in this essay to show the falsity of the generally received theory of the production of the sounds perceived in the respiratory organs. Without devoting any time to the useless and therefore irksome task of stating preliminaries now common in the science, we proceed at once to the consideration of our subject.

In its examination, we shall pursue the following order:—1st. We shall state in all their simplicity the facts which present themselves

* It is but justice to Dr. Cumming to state that this essay was drawn up before he left for China, two years ago.—Edts.
to the observer. 2nd. We will give the explanation proposed by Laennec. 3rd. We shall offer various objections to this explanation. 4th. We shall conclude with an exposition of the theory of Mr. Beau.

In ausculting the various portions of the respiratory apparatus of an individual in good health, different impressions are received by the organs of the observer. These sounds are generally admitted to be three in number. The first, which may be heard in the larynx and superior portion of the trachea has been called the tracheal souffle. This division was not admitted by Laennec who confounded this sound with the second—it is supposed to be produced by the friction of the air against the different surfaces of the pharynx and larynx. The second, which differs from it in some respects, though preserving the same general character, may be most readily perceived by applying the ear to the back, between the scapulae opposite to the root of the lungs. This sound is too well known to need any description. It was styled by Laennec the bronchial respiration. The third sound is more generally diffused, and may be heard on applying the ear to any portion of the chest whose internal surface is applied to the lungs. It was said by Laennec to resemble the deep inspirations which occasionally take place in quiet sleep.

To the explanation of the causes of the bronchial respiration, Laennec does not seem to have given much attention. It is evident, however, from one or two incidental remarks, that he considered it to be produced by the friction of the air against the internal surface of the bronchia. His attention was much more excited by the third sound, styled by him the vesicular murmure. He asserts that it may be readily perceived, and that it indicates the penetration of air into the pulmonary tissue and its subsequent expulsion. He says that the impression conveyed to the mind of the observer, is that of a fluid passing from a narrow canal into a more ample space. We will not here state the many peculiarities of this sound, and of the conditions under which it is produced. These we shall reserve for our chapter of objections, as we shall find in each one a stubborn fact refusing to pass under the yoke of this explanation. And as a theory is only valuable as a general expression of individual facts, we shall go far toward the destruction of the explanation of Laennec, if we can show that most of the facts so well observed by himself and others remain still isolated and refusing to be admitted into this unnatural generalization.
Before entering upon an exposition of the many direct and (as we think) unanswerable objections, we must present a few reflections on the indispensable prerequisite to the exactness of this theory, viz. the existence of the cellular structure of the lungs. This cellular structure once generally admitted, is now not only doubted but even denied by many anatomists. The latest researches on this subject seem to show that the lungs are nothing more than an agglomeration of bronchial tubes ever decreasing to their termination—lined by a mucous membrane in which ramify the terminations of the pulmonary arteries. These tubes are united by a tissue resembling the general cellular tissue of the body, forming however a smaller portion of the pulmonary mass than is generally supposed. Besides this conclusion derived from actual inspection, there is a consideration drawn from the philosophy of anatomy which has presented itself to my mind with great force, probably because it has originated there. It is evident that the lungs were intended to expose an immense surface to the action of the atmosphere. Their structure must however occupy but a small space that it might not interfere with the functions of the adjacent organs. A moment's consideration will show that the existence of vesicles at the extremity of the bronchia is not compatible with the exposition of the greatest possible surface to the atmospheric air. Though this consideration may not be conclusive, it must have great weight with those who have carefully studied the structure of the body, and observed the wonderful wisdom every where displayed in our physical frame. These two considerations render the vesicular or cellular structure of the lungs highly improbable, and therefore tend to invalidate a theory based on that supposition. But we go farther, and assert that we doubt not only the mode of formation generally assigned, but even the locality. We are disposed to deny that this respiratory murmur is formed at or near the termination of the bronchia in the lungs. Let us notice some of the peculiarities of this sound. One of those most worthy of remark is the superior intensity during inspiration. It is indeed heard at expiration, but is feeble and seems distant. This fact did not escape Laennec. It has been supposed that the explanation of this fact might be found in the introduction of a larger quantity of air than that expelled by expiration. But this difference is found to be very small if it really exist. It is also fully compensated by the increased quantity of watery vapour and carbonic acid, while at the same time the elevation of temperature which the air necessarily undergoes in
the lungs, must render the volume of gaseous matter expelled fully equal to that inhaled. It has been also urged in reply to this objection, that the air enters the lungs with greater velocity than that with which it is driven off. This is, however, a pure hypothesis, based on nothing but the determination to find a reason for an unreasonable notion. The time employed in expiration is equal to that in which the air is introduced, and as we have shown the quantities to be equal, there is no reason for an increase of velocity. It has also been observed that this respiratory murmur is much more intense in women and children than in men. So uniformly does this exist in children, that an unusual degree of intensity in the murmur of adults has been styled puerile respiration. This fact had evidently puzzled Laennec. He explained it by supposing that children breathe more largely than men, forgetting, as it would seem, that the same could not be true of women. But we know that there are found men in good health, of active habits, indeed differing in none of these respects from others, who yet exhibit this puerile respiration. In certain diseases, when a large portion of the lungs is affected, we frequently find this puerile sound. This was most readily explained by Laennec, who asserted that when a large number of cells were closed those which were pervious were more forcibly distended than under ordinary circumstances. But this triumph was short-lived, for it was found that in certain cases of violent pneumonia or pleurisy, when large portions of the lungs were unable to perform their functions, and when therefore the pervious cells must be much distended, no puerile sound was observed. To comprehend in its embrace these three apparently conflicting facts is above the power of the theory of Laennec. It is also found that in certain animals, e.g. the horse, the ass, the ox, the camel, the camel-leopard, &c. who breathe freely, whose lungs fully expand, the respiratory murmur is not heard in ordinary circumstances. If the force and frequency of respiration be augmented in these animals by rapid and violent exertions, we shall succeed in hearing this murmur as distinctly as in the human subject. We have thus cited many facts observed and stated by Laennec which he was unable to explain. They are inexplicable by his theory. The last and strongest objection we shall reserve until we shall have presented the theory of our adoption, for it acts at once on the offensive and defensive, overthrowing the old and establishing the new doctrine on a firm and immovable basis. We think that enough has been said to show that the theory generally received
is opposed by many well-observed facts, which it cannot explain, with which it is inconsistent, and with which therefore it cannot be consistent. Shall the facts or the theory yield? We prefer the latter alternative, and as the former are universally admitted, we shall adhere to them. And before proceeding farther, we will present a few thoughts which seem to be peculiarly appropriate here. After considering the structure of the bronchia, lined as they are by a smooth membrane lubricated by a thin mucus, remembering at the same time the small velocity with which the air traverses them, we are surprised that any appreciable sound should be produced. When we auscult an artery in the healthy state of the system, differing as it does from the transient bronchia, by its superior size, by the density and velocity, and consequently by the momentum of the fluid passing through it, we find a sound far inferior in intensity to that perceived on ausculting the chest.

Proofs.

Is the sound more intense at inspiration than at expiration? What more natural or consistent with every day's experience, than that a sound conveyed to us by a current of air should be louder than the same sound compelled to force its way to us against the direction of that stream? Is it stronger in women and children than in men? A knowledge of the comparative anatomy of the larynx in different sexes, and at the various periods of life, offers a ready solution. Do we not know that the cavity of the larynx undergoes great changes in males at the age of puberty—that the ventricles are dilated, the thyroid cartilage enlarged, the opening of the glottis increased? Do we not know that in females this change is slight? What then more reasonable than that the intensity of the sound should increase whenever the conditions favorable to this modification exist? And if the original sound be more forcible, it will be more intense at each point where it is examined. But actual observation excludes all necessity for a resort to reasoning. If we auscult individuals of the different sexes and children, we shall find that the laryngeal sound of men is feeble than that of women and children. Do we find some men who present this puerile respiration? On ausculting such we shall find an unusual intensity of the laryngeal sound. We shall find that their thyroid cartilage is not much developed, that their voice is high, and that this change which occurs in all men has been slight in these cases. Do we observe this puerile respiration existing in certain
diseases of the lungs? On examination we shall find the laryngeal sound proportionably increased. Do we fail to observe it in certain cases where the violence of the disease is the same or even greater? We shall find that these individuals present no sensible augmentation of the laryngeal sound. In no disease can this fact be more readily observed than in pneumonia. We frequently see patients of this kind who produced great sound in the larynx. This should not be confounded with that formed in the nasal fossæ. We find others, who, while breathing as rapidly, and with as great difficulty, present no increase of the natural sound. The position of the patient, the state of his nervous system will introduce remarkable modifications in his respiration, and by placing in supination an individual occupying the sitting posture, we may frequently augment the laryngeal sound and the consequent respiratory murmur. An objection must be met here. We have said that on ausculting the chests of certain animals we could not perceive the respiratory murmur. It will be asked, did not the laryngeal sound exist? We answer that it does, but is prevented from reaching the chest by a peculiar circumstance. We have cited the horse, the ox, the giraffe, the camel, as instances of this kind. Though differing widely in their organization, they have one thing at least in common,—a long neck. In many of them, the laryngeal sound is feeble, and is lost before arriving at the terminal bronchia, or is rendered so soft that it is not perceptible. It will be noticed that these animals have low voices, proving thereby that the aperture of the glottis is large. But if the laryngeal sound be much augmented by rapid and violent exercise, we are then able to perceive the respiratory murmur. So that this objection, apparently so formidable, readily enters with the others and acknowledges the justice of our generalization. In the third portion of this essay we mentioned the reservation of a powerful objection to the vesicular theory. We did so in order to bring it with more effect to the establishment of our adopted doctrine. It contains within itself the root of the matter, and is amply sufficient to answer all our enemies of demolition and edification. It is the fact, that we all possess the power of diminishing at will the intensity of the respiratory murmur without affecting the plenitude or rapidity of respiration. The only condition is an enlargement of the aperture of the glottis. So far from a necessity of diminishing either the fullness or frequency of respiration in order to produce this effect, we may increase both almost indefinitely without endangering the success of the experiment.
With a little practice any one will soon be able to respire largely and rapidly without producing an appreciable sound in the larynx or chest. To such a fact commentary is injurious. All possible inductions will readily be made by the feeblest intellect. For the information of those who desire to repeat the experiment, we shall give a few directions. Three persons are necessary to its proper performance: one to auscult the trachea, another the lungs of the third. The diminution of the laryngeal sound, as observed by the first, will be found by the second to coincide with the diminution of the respiratory murmur; and when the former ceases to perceive the sound in the trachea, the murmur will be imperceptible to the latter.

The arteries are frequently more favorably situated for the transmission of any internal sound to the ear of the observer, as they are far more superficial. We indulge therefore in a legitimate astonishment when we are told that so rare a fluid passing with so small a velocity, through so small a vessel and at so great a depth, should produce a sound so superior in energy. And under such circumstances we are justified in demanding an explanation which shall solve all the phenomena. On finding that the generally received theory is thus at variance with so many well known facts, we are led to inquire on what has been founded the favorable reception which has been given to it by the members of the medical profession. It is the appearance of simplicity which it presents that has secured for it such general support. Indeed, the first impression is that the sound perceived is formed immediately under the ear of the observer. But this is at best but a small probability. Nothing is more fallacious than the decisions of our minds in regard to the direction of sounds. Were we guided by them, we should suppose that the cliff addressed us, because the sound is reflected from it to our ears. We shall here conclude our objections to the former theory, and proceed to expose and develope that whose defence we have assumed.

In a preceding part of this essay we have examined the sounds according to the scholastic mode. We have gone to their study in the manner counselled by the dogmatists of our science—we have studied them "secundem artem;" let us reverse the method, and trusting to the guidance of another leader, let us examine the same phenomena secundem naturam. And let us set out on this course assured that we are under the direction of a safer, if a less brilliant guide. Let us then follow the air as it penetrates the respiratory passages—The first point open to our investigation is the larynx.
Here we find a sound of peculiar character and of considerable energy. The cause is readily discovered. The slightest knowledge of the anatomy of the larynx convinces us that all the conditions necessary for the production of such a sound are present in that organ. The column of air, increased in diameter by the existence of the ventricles, is compelled to traverse the narrow passage offered by the glottis. If we examine it at inspiration and at expiration, we find that it does not vary much in intensity at these different times. This might have been admitted a priori, for the glottis is inferior in diameter to the trachea. The induction which we have made as to the cause of the sound is then entirely conformable to the physical laws of sound. Admitting that we have discovered the cause of the sound in the larynx, we follow the trachea down, ausculting it at short intervals. We observe a modification of the sound which has become very apparent before we have reached the sternum. We are here compelled to leave the trachea in front; but, nothing daunted, we recommence our examination in the rear. On applying the ear above the spinal column, near the level of the superior extremity of the sternum, we perceive a sound very similar to that of the trachea, and yet differing in some respects. We now proceed either to the right or left from this point, and we find a decided but gradual change, until we arrive at the axilla, where we receive the respiratory murmur in all its purity. By this mode of observation, instead of three sounds, we have an indefinite number, which pass from the laryngeal to the tracheal, to the bronchial, and finally to the vesicular, by insensible gradations. What cause has effected these changes? The changes are gradual, i.e. composed of a large number of slight modifications, requiring for their production an equally large number of efficient causes. We may examine the trachea, the large and smaller bronchia, but we find no anatomical condition capable of explaining these phenomena. We have found that each sound in the trachea is merely a modification of that heard in the larynx. We have traced the same relation between the tracheal and bronchial sounds. The idea then irresistibly suggests itself, that there is an original sound, and that it is the laryngeal. Where then are the modifying agents? for we have admitted that they must exist in indefinite numbers. They are the different distances of each successive point from the glottis. The sound is borne by the current of air, and is thus carried to the terminal ramifications of the bronchia, where, from the distance, and the small size of the tubes, it assumes the peculiar charac-
ter of the vesicular murmur. The increase of the aggregate diameters of the bronchia may also exert some influence upon the quality of the sound. Here is the conclusion of the whole matter. Are our proofs demanded? We take the facts which we have before urged against the former theory. The reverse experiment will be still more readily performed. By increasing the sound of the larynx, the respiratory murmur may be made to surpass in intensity the loudest puerile sound. To our mind this experiment is satisfactory—It fixes every point which was before doubted, and bestows the fullest assurance of the truth of the theory which we here advocate. Before concluding, we beg leave to introduce an argument from analogy which must please, if it do not instruct. If we apply an ear to the chest of a person speaking, we observe a confused sound somewhat resembling the noises of a distant crowd. If on the contrary we examine the trachea, or bronchia, of an individual engaged in speaking, the sound is louder and more defined. We seem to have the tube under our ear. We observe here the similarity of the modifications impressed upon these different sounds. The seat of the voice is, without doubt, the glottis. But this similarity is still greater in morbid states of the pulmonary organs. In those cases where the minute bronchia are rendered impervious to the air, either by the extravasation of the blood or by external pressure, we find certain modifications of the respiratory and vocal sounds. And it is remarkable that the writer, to whom we have so frequently referred, has used almost the same language in speaking of these modifications. In these cases he says that the bronchial souffle is heard so distinctly that it seems as though some one was blowing softly into the stethoscope. In speaking of bronchophony, he remarks that the voice appears to be produced within the same instrument. In describing the higher degree of these two phenomena which exist in cases of extensive excavations, he observes that the respiration produces an impression similar to that resulting from the breathing forcibly in the stethoscope, the sound seems to reach the ear. This very remark is applied to pectoriloquy. The voice seems to rise into the tube, and in some cases appears even to reach the ear, and in a few instances resembles the sound produced by the act of speaking aloud in the ear of an individual. How striking is this unintentional tribute to our theory! It could scarcely have been rendered more flattering. An important induction from these facts must here be noticed. In examining patients affected with diseases of the chest we
are often unable to auscult the voice, either from its weakness or from the unwillingness of the patient to speak. We need never be uneasy on this account. Do we hear a souffle? There is bronchophony. Do we hear the cavernous respiration? There is pectoriloquy. Have we a patient who is constantly talking? Allow him this privilege—It does not interfere with the auscultation of the rales, nor do you need any other information as to the state of the respiration. Is the voice indistinct and murmuring? Rest assured that the respiration is good. Have you bronchophony or pectoriloquy? If the patient were silent, you would hear the bronchial or cavernous souffle. Does the voice assume an amphoric sound? Believe that the amphoric souffle is not absent. Do you expect the existence of some compression on the trachea or large bronchial trunks? Examine the voice. Formed, as it invariably is, during expiration, it is so much louder than the murmur of expiration that it will advantageously replace the latter as an object of examination. We are aware that other inductions may be made from these facts, but we are disposed to leave them to the ingenuity and reflections of the reader.

Part II.—Reviews and Extracts.


(Concluded.)

Phthisis.—Although Dr. D. classes this too-frequent scourge of France among the chronic Phlegmasieæ, he expressly says that he does not regard it as at all of an inflammatory character in its early stages. During the course of the disease, there is a strong tendency, as every one knows, to the frequent occurrence of a pneumatic and pleuritic attack; but this is to be regarded only as an epi-phenomenon, and not as a necessary symptom. With respect to treatment, we find that our author has almost entirely renounced—after the experience of their utter inefficacy—the employment of nearly all remedies, save and except the insertion of a seton in some part of the
chest, and the persevering use of Iceland-moss jelly in large quantities, not forgetting the Opium or Belladonna once or twice in the course of the 24 hours. He recommends the same line of treatment in those cases of chronic purulent Catarrh, the symptoms of which so closely resemble those of genuine tubercular Phthisis; and, in not a few instances, has a cure taken place under their use, when the case had seemed to be utterly hopeless.

From the Chest we pass on to the Stomach. After delivering some excellent remarks on the mode of distinguishing gastralgic from gastritic pain, our author exposes, with no less truth than severity, the melancholy mistakes that have been committed of late years by so many of his countrymen, since the prevalence of the Broussaiian doctrine. He shows that the existence of an inflammatory state of the stomach may generally be diagnosticated by observing the effects which different kinds of food have upon the gastric pain. If, for example, farinaceous and milky substance can be taken well, while those of an animal nature give rise to a sense of uneasiness, we may very generally presume that there is a greater or less degree of actual gastritis. If the reverse be the case, and if light animal food, such as chicken-tea or mutton-broth, be borne best, we may feel assured that there is no inflammation, however troublesome the gastric uneasiness may be. The diet may therefore be regarded as a most useful exploratory means of diagnosis.

"It often happens that the epigastric pain does not yield to leechings and low diet; and we be to the physician who pertinaciously seeks to combat it by continuing the use of the same means, and who has not learned to modify his treatment according to the varying condition of each case. For the relief of the gastric pain, which resists the application of leeches, &c., opium is often an excellent remedy; a light preparation should always be preferred, and it will be well to exhibit it in some mucilaginous vehicle. If, however, this does not succeed in the course of a day or two, we should then have recourse to a volant blister. Should this also fail, we shall have good reason to believe that the epigastric uneasiness is more or less dependent upon an atomic state of the stomach; and this we can generally determine by having recourse to the explorative diet of which we have spoken. Should such be found to be the case, we must allow the patient more nourishing food, and we should try the effect of an infusion of Rhubarb or Columba-root, to which may be added a weak opiate, if deemed necessary."

In Chronic Hepatitis, our author strongly recommends the use of emollient poultices on the hypochondriac region, applied every night, and also during the day, if the patient keeps his bed. They produce

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A favourite formula of our author is this:

\[\begin{align*}
\text{H.} & \quad \text{Aquæ lactæ} \quad 1\text{ij}. \\
& \quad \text{Laudanii Sydenhami} \quad 3\text{ij}. \\
& \quad \text{Gum. arabic} \quad 3\text{ij}. \\
& \quad \text{Syrupi simpl.} \quad 3\text{ij}. \\
& \quad \text{Bicarbonat. sodæ} \quad 3\text{ij}. \\
\end{align*} \]

A tablespoonful or two to be taken twice or thrice a day.
a local diaphoresis, which is often very serviceable in relieving the internal congestion. The occasional use of a tepid bath at the same time will much promote the cure; for the skin is generally very dry and lichenous in chronic liver complaints. Emollient aperient enemata are also very useful. Saline purgatives, dissolved in a large quantity of herb-tea, to be followed by repeated doses of rhubarb—which has long had the reputation of directly promoting the flow of the bile—are always more or less necessary.

In various chronic hepatic affections and visceral obstructions, the following formula has been found by Dr. D. to answer exceedingly well.

Dr. Pul. Aloes .... 3ij,
Sapon Hispanic.
Pulv. Rhei.
Ferri Subcarbonat. aa 3iv.
Potassii Ioduretti .... 5ij. In pil. 120 divide.

Dose—From two to six pills in the course of the day.

If these pills do not prove to be sufficiently purgative, the patient should be instructed to drink some aperient mineral water to aid their action.

Erysipelas.—"The treatment which we usually adopt in recent cases of this disease, before the formation of pus has taken place, is the abortive and empiric plan followed by Dupuytren, and which he derived from the practice of M. Pétit of Lyons. This method consists in applying a volant blister to the very centre of the inflamed part. The object which we have in view by this bold and seemingly not very rational mode of treatment, is to arrest the internal inflammatory action, and to cause it, so to speak, to abort, by drawing to one circumscribed spot of the skin all the violence and raptus of the existing disease. It is quite true that this powerful concentration and sudden localization of the inflammatory action may, in consequence of being excessive, induce gangrene of the blistered part; but this accident is of very rare occurrence indeed. Out of between thirty and forty cases treated by us, we have only met with a single instance of it; and this occurred in a cachectic patient, whose constitution was altogether unhealthy. The application of a blister to the knee was in this case followed by the formation of a gangrenous eschar in the part, and the eventual consequences were extensive detachment of the surrounding skin, and considerable suppuration. Of late years, however, it would seem that this unpleasant result of the vesicatory treatment has been observed several times in the Hôtel Dieu, at Paris, and that antiphlogistic measures have been on the whole very successful; whereas during the years 1813, 1814, and 1815, it was remarked, by all who followed Dupuytren's practice, that bleeding, &c. produced but little benefit, while blisters seemed to be quite a specific remedy."

Chronic Cutaneous Diseases.—Our author, without troubling himself with the divisions and subdivisions of these diseases adopted by most dermatological writers, groups them together under the general appellation of dartres, and lays down some general therapeutic direction that may be applicable to all. A mild unirritating diet, more vegetable and milky than animal, emollient refreshing drinks—one of the best of which is whey—and the more or less frequent use of warm-baths, should never be omitted. Dr. D. throws overboard, as
being utterly useless, the sarrago of what have been called *depurative* remedies, such as the infusion of Fumaria, Dulcamara, Bardanum, Saponaria, Scabiosa, &c. &c. and he supports his own opinion on this point by quoting that of Alibert. The only internal remedy that he uses is the following:

\[\text{R. Flor Sulphuris} \ldots \ldots \text{3iv.}\]
\[\text{Sulphureti Antimon. rubri} \ldots \text{3j.}\]
\[\text{Calomelanos} \ldots \ldots \text{gr. xij. M.}\]

To be divided into 40 powders, of which one is to be taken twice a day.

The external treatment is, according to his experience, of much greater importance than any internal remedy. As a matter of course, as long as there is any irritation present, the more simple the baths and other external applications are, the better; but when this has been subdued, we should as early as possible have recourse to such as are slightly stimulant and exciting. Of these a sulphur bath—prepared by adding five or six ounces of sulphuret of potash to an ordinary bath—will be found very convenient. The use of this should be continued steadily for two or three months at least. If the *dartrous* affection is limited to the legs, as is often the case, it may be sufficient to use a partial sulphur bath to them alone: from one to two ounces of the sulphuret may be added to the requisite quantity of water. This is a far better application than all the ointments and lotions that are so generally in use. "If the local atonic *dartres* prove very obstinate, and will not yield to sulphuretted baths and lotions, we are in the habit of trying an ointment—composed of ten drams of the sulphuret of potash to six or eight ounces of lard, flavoured with oil of thyme—and usually with good effect: the strength of the ointment must be varied according to the degree of irritability in the affected parts." Dr. Debreyne tells us that, for the last five and twenty years, he has made use of no other application than the sulphuretted ointment—weak or strong, according to circumstances—in the numerous varieties of *Tinea*, or Scalled-head.

Of the Asthenic class or division of chronic diseases, none is of such frequent occurrence, and therefore so important in the eyes of the practical physician, as atonic Dyspepsy, or, as our author designates it, *Gastro-atology*. The following remarks are picked out from the description which he gives of it.

This disease is of very frequent occurrence, especially among women who are subject to leucorrhœal and chlorotic affections. Its most obvious symptoms are loss of appetite, uneasiness and sense of distention after eating, flatulence and often nausea and sickness, a feeling of sinking weakness and craving, more rarely of dragging, pain about the stomach; constipation; tongue white; taste more or less depraved, but without being bitter or clammy, as in bilious derangements; loss of muscular strength; tendency to nervous ailments, and these usually accompanied with great irritability of temper. Very generally (and this remark, by the by, it is especially important
to attend to, an invigorating animal diet agrees better than a farinaceous and vegetable one—a circumstance that is very significant, and obviously excludes the idea of any phlogistic or irritative element being present. The mere circumstance of there being some degree of pain in the epigastrium, even although this be increased on pressure, is by no means a sufficient reason for suspecting the existence of any inflammatory action; for this pain may be truly called an atonic pain, and can be relieved neither by opiates nor antiphlogistics, but only by appropriate tonics.

With respect to the treatment of Gastro-atony, the mere regulation of the patient's diet will often suffice to relieve the milder and less chronic forms of it. He should avoid the use of much vegetable or farinaceous food, and live chiefly on animal meat and good bread, with or without an allowance of a light sound wine, according to circumstances. The drinking of large quantities of hot drinks, such as tea, coffee, &c. is most injurious. Our author very pithily remarks, 'pour les maladies chroniques apyretiques, medicaments secrès; et pour les affections aigues, medicaments liquides:' the remark would apply better to food than to medicines. Steel and vegetable bitters are by far the best remedies that we can administer, more especially when there is any leucorrhœal or chlorotic ailment. Rhubarb is the preferable aperient medicine in such cases. If the patient be subject, as occasionally happens, to attacks of nervous or spasmodic vomiting, Dr. D. always has recourse to the Columbo powder. If much pain accompany this unpleasant symptom, a small portion of opium should be added to the Columbo. Ice too will often be both grateful and useful, not only in allaying the irritability of the stomach, but also in giving it tone.

When Leucorrhœa accompanies this atonic state of the digestive organs, Dr. Debreyne recommends the use of the following pills:

- Fe. Ferri Subcarbonat. . . . 5iv.
- P. Cassuvii (Cashew) . . . . 5iv.
- P. Aloes . . . . . .5j.
- Terebinth. Veneti . . . . q. s. ut fiat massa in pil.

Dose: one or two to be taken, along with some bitter tincture or infusion twice or thrice a day.

A somewhat similar formula will be found very efficacious in most cases of Chlorosis.

Dropsy.—The following extract gives a good summary of our author's views respecting the treatment of this disease, when it is not dependent upon any organic visceral lesion.

"We should always make sure of one of the outlets, by which nature usually seeks to evacuate the serosities effused within the splanchnic cavities. Now, as in the cure of Dropsies, the serous evacuations most frequently take place by the bowels and kidneys, it will be prudent to act upon both of these emunctories, by combining the use of diuretics with that of hydragogue purgatives. At the
same time, we should prescribe a dry and tonic diet, consisting chiefly of broiled or roasted meats, bread, and a certain allowance of light wine. The patient should be directed to take as little fluid food or medicine as possible, and he should therefore seek to quench his thirst with fruit, ice, and such like things."

The favorite medicine of Dr. D. in dropsy is a medicated wine, composed of

Rad. Jalapae contus. . . . 3 iiss.
— Scillae contus. . . . 3 iiss.
Pot. Nitratis . . . . 5 v.
Vini Albi . . . . 1 b j.

Dose: from one to three tablespoonfuls thrice daily.

The number of alvine evacuations need not exceed six or eight in the 24 hours. The remedy acts in some cases chiefly on the bowels, in others chiefly on the kidneys, while in a third set of cases both emunctories are powerfully affected at the same time. When patients object to the use of this wine, or when it appears to disagree with the stomach, we may have recourse to the following pills:

R. Pulv. Digitalis . . . 5 j v.
Pulv. Scammoniu . . . 5 j i
Pulv. Scillae . . . . 5 i j.
Extract. Juniperi . . . q. s. Ut fiat massa in pil.

cxx. dividenda.

Dose: from one to two pills three times a day, washing them down with three or four spoonfuls of white wine, in a bottle of which half an ounce of nitrate of potash has been dissolved.

Dr. D. says that he has found these pills especially servicable in cases of Hydrothorax and Hydropericardium. He is too experienced and candid a practitioner not to admit that we can seldom, or never, hope to effect a permanent cure in such cases; still it is an important thing to relieve our patients for a time, and prolong, if it be not given us to save, life.

In cases of Organic Diseases of the Heart, our author mainly relies on the internal administration of the nitrate in combination with tincture of Digitalis—given in much larger doses than are usually recommended—and on the application of leeches and blisters over the cardiac region. He seldom varies his plan of treatment, and assures us that, with these simple means, followed out for a due length of time, he has succeeded in relieving a great many patients, who had derived no benefit from a variety of other remedies.

In the closing Chapter of his Work, Dr. D. lays down some excellent general rules for the treatment of disease in the members, male and female, of those austere religious orders, who insist so rigidly on the observance of numerous and prolonged fasts, in addition to other modes of penance. Of these orders the most conspicuous are the Chartreux and the Trappists, "whose establishments," we are told, "are now so numerous, and whose moral and religious influence, along with the benefits of agricultural improvement which they have introduced, are every day more and more felt and appreciated in many of

[February,
the finest districts of France." A vast deal of harm has been done for many years past by the too common adoption of Broussaian principles, by the medical practitioners of the Provinces; but now, thank God, the errors of Physiologism are most fully acknowledged and repudiated. As a general truth it may be asserted, that the diseases of austere religionists will not bear well much deprecation.

Even in acute disorders, the lancet should be sparingly used; and, instead of repeated bleedings, recourse should be had to the internal use of antimonials, and to blisters, &c. In fevers, when there is no distinct inflammatory localization, general bleeding should very rarely, if ever, be practised. Of chronic diseases, by far the most common among the Monastic Community are Gastro-atony and its usual concomitants of Dyspepsia, Colic, Gastralgia, general weakness, and so forth.

As for Gastritis, the term might be erased from the peculiar nosology to which we are at present alluding. Opium, either alone or in conjunction with other remedies, according to circumstances, is an admirable remedy in a vast number of the gastric and enteric disorders to which the Trappist brethren are liable. It would seem, from the statements of our author, that these monks are singularly exempt from the epidemics which prevail in the neighborhood of their establishments. Even the cholera in 1832, did not enter one of them throughout the whole of France. This exemption he attributes to the temperance of their diet, and the calm unruffled tenour of their lives. He paints in glowing terms the joys of the peaceful life of the pious Cenobites.

"How greatly mistaken," exclaims our worthy author, "are they who suppose that religious penitents are gloomy, melancholy and hard-hearted men, or that they become the early prey of a tedious and painful death! No; their life is one long and blessed repose; or rather, as the Prophet says, it is a river of peace which calmly bears them on to the everlasting rest of God. They seem to the eyes of the worldly, who are altogether absorbed with the frivolities of the passing scene, to languish and die; and yet they are full of health and life, for they taste a peace and happiness of mind which the world cannot know: Visi sunt oculis insipientium morti; illi autem sunt in pace."

Observations on the Use of Blisters in Acute Inflammatory Diseases, and on the Effects of Bleeding, Blisters, and Mercury, on the Blood. By James Turnbull, M. D., Physician to the Liverpool North Dispensary.

In the Lancet of the 31st of August, 1844, my attention was drawn to the treatment of acute inflammation by large blisters, from observing some excellent practical remarks on the subject by Dr. Henry Bennet, in which he has shown that they may be safely and beneficially used at an earlier period of acute inflammatory diseases, espe-
cially of those of the organs contained in the thoracic cavity, than they are generally employed. The kind of cases to which blisters are best suited; as well as the period of the disease at which they may be most advantageously applied, are matters of great practical importance, owing to the extent to which they are used; and I take, therefore, the opportunity of making some observations on these points, as well as on the effects produced by bleeding, blisters, and mercury, on the different constituents of the blood.

My attention was first particularly directed to the safety and advantage of applying blisters in the early period of acute inflammatory disease, from observing, when in Paris, nearly five years ago, the treatment adopted by M. Gendrin in pneumonia and other acute inflammations, and I am glad to find that the results of his mode of treatment have been made known to the profession by one who has had the opportunity of closely observing it while holding, during nearly three years, the office of house-physician under him. I observed, as has been already pointed out by Dr. H. Bennet, that the greater part of M. Gendrin's treatment consisted in the application of very large blisters, and that their repeated application, with a very moderate use of bloodletting, was attended with very successful results in his practice at La Pitié. From having witnessed the safety and efficacy of this mode of treatment, which was different from what I had seen elsewhere, in the hospitals, either in this country or in Paris, I was led to entertain some doubt as to the correctness of the opinion held by most of our best writers, in regard to the injurious exciting effects of blisters in all acute inflammations during the early period. I have in consequence made use of blisters more freely, and at an earlier period, than I would otherwise have done, and have a high opinion of their value in the treatment of acute inflammations, especially after general or local depletion.

In robust and plethoric subjects, in whom the proportion of globules in the blood is, at the healthy standard, 127 in 1000, or above it, the treatment by general bleeding being found the speediest and most effectual in cutting short inflammation, has been almost universally adopted; and there can be no doubt of its superiority to every other in such cases, as it has been founded on the solid basis of practical experience. Looking, also, to the effects of general deprecation upon the blood itself, though we are unable to trace clearly the mode in which it reduces the inflammation, we see no reason to doubt its superiority, for though it does not directly lessen the quantity of fibrin, the increase of which forms the essential change in the composition of the blood in acute inflammations, still there is reason to believe that it may indirectly prevent its increase, and in plethoric individuals the removal of a portion of all the constituents of the blood is assuredly the most effectual means which we possess of reducing inflammation.

Local deprecation, by cupping or leeches, does not differ from general bleeding in its effects upon the blood, except in degree. It acts, how-
ever, less directly and powerfully upon the nervous system, and the
action of the heart, from the blood being abstracted both more slowly
and less copiously, but its less powerful action on the system is, in
many cases, fully compensated for by its more direct effect upon the
seat of the disease, and I agree with Dr. Stokes in considering it as a
remedy of great importance, even in the treatment of pneumonia.
Dr. Stokes has stated that he found the bold and repeated use of the
lancet to be unnecessary in the great majority of cases, a single, or at
most two bleedings, being sufficient; also that he regards general
bleeding as only a preparation for other treatment, and that he con-
siders local bleeding as the principal remedy. A case lately came
under my care at the Liverpool North Dispensary, which may serve
to illustrate the fact, that in large towns cases of acute inflamma-
tion of the lungs do occur, in which general depletion may be superseded
by local blood-letting, assisted by blisters, and other antiphlogistic
means. A middle-aged man, of large and rather robust frame, em-
ployed in a brewer's establishment, was attacked with well-marked
acute inflammation of the lower and posterior part of the left lung.
He was treated by a single cupping to the extent of about ten ounces,
and by two large blisters, and, as it was also found that slight effusion
had taken place into the pleura, the effect of mercury was pushed, so
as slightly to touch the mouth. In this case, though general bleed-
ing was not practised, the recovery was almost as rapid and perfect
as I have ever seen it.

The preceding observations apply to cases in which the quantity of
globules in the blood is about the healthy standard, or not materially
below it; there are, however, especially in large towns, a numerous
class of individuals also liable to be attacked with acute inflamma-
tion, though of a less active kind, in whom the proportion of globules,
in the blood has fallen considerably below the healthy standard. From
the operation of debilitating causes, such as sedentary occupations,
the respiration of impure air, or deficient nourishment, as well as in
consequence of the presence of other disease, such as tubercles, or of
imperfect recovery from previous disease, such as fever. I have
satisfied myself, from very frequent examination of venous and arte-
rial murmurs, which are indications of this anaemic condition, that it
is extremely common, and that it occurs, too, in persons who are not
always deficient in flesh, or remarkably pale. Now, it is in inflam-
mation occurring in individuals in such condition that blisters have
been found most useful, for, in addition to their counter-irritant ac-
tion, they produce local depletion, without, however, withdrawing that
part from the blood—viz. the red globules, which is already deficient,
and the deficiency of which constitutes a state of disease which is
directly aggravated by the abstraction of a portion of the whole of the
constituents of the blood. The correctness, therefore, of the treat-
ment, which experience has established, very generally, in such cases,
is confirmed by an examination of the effects of blisters on the differ-
ent constituents of the blood.
Dr. H. Bennet has shown that, "both in non-inflammatory and in inflammatory diseases, blisters do not give rise to as much general excitement as is generally believed;" and in this I coincide with him, though I am disposed to regard their exciting effects, and the possibility of their doing harm, as greater than he has estimated them. We must hesitate, too, before we conclude that large blisters are devoid of injurious effect when applied in the early stage of acute inflammations, except in very rare cases, when we find that such writers as Copeland, Stokes, and Alison, express themselves strongly against the use of blisters before vascular depletion; and when, too, we find that Andral has expressed a doubtful opinion as to whether, by their influence on the fibrin of the blood, they may have a beneficial or injurious effect.

Dr. Stokes, in regard to the treatment of acute bronchitis, observes, "that counter-irritation may be considered inapplicable in the earlier periods of the disease so long as the skin is hot, the pulse strong, the expectoration scanty and difficult; in fact, so long as the first stage of the affection continues, that stage in which bleeding and tartar emetic are useful, blisters are inefficacious and often hurtful. It may be laid down as a general rule, that the longer we can with safety postpone the application of a blister, the greater certainty will there be of its favorable action." His views of their action in pneumonia are similar; for in treating of that disease, he refers to the observations he had previously made as to the treatment of bronchitis. We have here, then, the experience of one of our best practical writers, supported by the opinion of others of equally high authority in this country, opposed to the treatment of M. Gendrin.

Let us see what light is thrown upon the subject by investigating still further the effects which are produced upon the blood by the action of blisters. Andral has shewn that every local phlegmasia has the effect of raising the quantity of fibrin in the blood above the natural standard of three parts in a thousand, and that this increase is in relation to the extent of the inflammation, and the degree of febrile movement excited in the system, averaging, in acute inflammations, from six to eight parts, but in inflammation of the lungs, rising higher than in any other disease, in one instance having been found as high as ten and a half. This increase of the fibrin occurs, not only in inflammation of internal organs, but also in that of the skin as well as in burns, which most nearly resemble the effects of a blister.

We have no experiments to shew directly how far blisters may have the effect of increasing the fibrin in the blood; but from what has been observed in those cases of pleuritis in which a portion of the serum and fibrin have been separated from the blood, we have reason to think that the discharge of serum, or even of fibrin, at the surface of the wound caused by a blister, tends less to diminish than the artificial inflammation does to increase the quantity of fibrin. Andral has shewn that in two cases of pleurisy without effusion, the quantity of fibrin was 5.8 and 5.9, while in eight recent cases with effusion, it
varied between four and six. It would seem from this, that no very decided diminution of the fibrin of the blood follows immediately from the effusion in pleuritis. Direct experiment only can, however, decide how far the quantity of fibrin in the blood is affected by blisters. In treating of the effects of revulsive remedies upon the blood, Andral expresses his opinion on this point as follows: "Thus, a large blister takes from the blood a certain quantity of its serum; but besides, some fibrin is deposited at the same time at the surface of the wound produced by the action of the cantharides. Where there exists in the blood a superabundant proportion of fibrin, would this be the means of diminishing the excess of this principle in the blood? Or, on the contrary, if the action of the cantharides be exercised on a pretty large surface, if the inflammation which results from its application have a great intensity, if especially it augment the febrile movement already existing, may there not arise from this a new cause for the superabundant formation of fibrin, and may not this cutaneous phlegmasia, artificially produced to diminish the intensity of another by the kind of influence which it may exercise upon the blood, have the effect of increasing the morbid condition, which represents in the blood the phlegmasius state, and from which its intensity is derived?" (Essai d'Hematologie Pathologique, p. 124.)

The same distinguished pathologist has himself furnished us with the means of pursuing this inquiry some degree further, in comparing the action of blisters with the effects produced upon the blood by mercury, when given so as to act upon the mouth. He examined the blood in four cases in which the mouth was affected with different degrees of severity, and it is a singular fact, that, instead of finding a diminution of the fibrin, as we should have expected, from our knowledge of the influence of mercury in subduing inflammation, he discovered that there was an increase in the quantity, which corresponded with the severity of the specific mercurial inflammation of the mouth, and the degree of febrile action excited in the system.

In the first case, in which the pulse was 80, the inflammation of the mouth slight, but the salivation abundant, the quantity of fibrin was 4.5. In the second, there was more inflammation, and more febrile action; the pulse was 100, and the quantity of fibrin 5. In the third, there was more intense inflammation; the febrile action was nearly the same as in the second case; the pulse was 96, and the quantity of fibrin 6.4. In the fourth case, the face was greatly swelled, and the mouth crusted with false membranes; the pulse was 120, and the quantity of fibrin 6.6. In the last case, the blood had been examined a few days before, when the patient had been bled, on account of a slight attack of apoplexy, and only 3.5 of fibrin had then been found. In all the other cases, there was no other disease present that could have affected its quantity.

We see, from these cases, that mercury, by its specific effect upon the mouth, acts not from any power of causing a state of dissolution of the blood, but as a counter-irritant or derivative, and that its power
of subduing inflammation arises from a local counter-action, similar to that caused by a blister. Both remedies produce more or less febrile excitement, both seem to exercise a similar effect upon the blood, and both cause more or less secretion of fluid. The similarity in their mode of action being such, we must inquire how far they agree as to the circumstances in which they are useful in the treatment of inflammation. In this, too, we find that there is a similarity; for, in treating acute inflammatory diseases, we do not give mercury at once with the view of inducing its specific action on the mouth, but first make use of depletion, where it is admissible, with the double intention of reducing the inflammation, and of rendering the system more easily affected by the mercury. Both remedies, too, possess a powerful influence in causing absorption of the fluid which has been effused from inflammation of a serous membrane.

For these facts in regard to the effects of bleeding, blisters, and mercury, upon the blood, we are chiefly indebted to the recent searches of Andral. They seem to confirm the common opinion, that abstraction of blood should, in all cases in which it is admissible, precede the use of blisters and mercury; but while they throw some light upon the obscure but interesting subject of the effects of remedies on the blood, they would scarcely justify us in drawing any other positive inference.—London Lancet.

PART III.—MONTHLY PERISCOPE.

Paracentesis Thoracis in Acute Pleurisy.—At the latter end of last year M. Trousseau communicated to the Paris Academy a case in which he had successfully resorted to paracentesis in acute pleurisy. He has just published two other cases in which the operation was performed under similar circumstances; one of the patients recovered, the other died.

The first patient was a young woman, twenty-three years of age, who was attacked with acute inflammation of the left pleura on the 9th of June. She was actively treated, but without any amelioration taking place. The effusion was very considerable, the entire thoracic region on the left side being dull, the intercostal spaces dilated, and the heart thrown underneath the right cartilage. On the 21st the menstrua appeared, but stopped in the evening. During the night the oppression increased to such an extent that M. Trousseau, who was consulted, decided on performing the operation of paracen-
thesis as the only chance of saving the patient. A small incision was made in the skin, between the seventh and eighth rib, a little to the outside of the breast. The skin was then raised until the incision corresponded to the intercostal space immediately above, and the ordinary abdominal trocar was introduced the depth of about two inches. On the spear being withdrawn the fluid rushed forth impetuously. In order to prevent the air from penetrating into the cavity of the chest, M. Trousseau wrapped round the pavilion of the canula a strip of very thin skin, which the fluid raised easily in passing out, but which, falling on the orifice during deep inspirations, effectually closed it. An assistant compressed the abdomen, so as to push up the diaphragm, as also the parietes of the chest. Four pints of serosity were thus withdrawn. The canula was then rapidly withdrawn, the skin being pressed down at the same time. The incision regained its position, below the puncture, and was covered with a small piece of court-plaster.

The heart immediately returned to its natural position, and all dyspnea disappeared. The patient slept seven hours the following night, and rapidly recovered. A fortnight after the slight operation she was able to walk out; the respiratory murmur had returned, her general health was good, and the only abnormal symptom which she presented was a certain degree ofanity in the inferior part of the left thoracic region.

The second patient was a young woman, twenty-five years of age, who, a couple of days after a laborious accouchement, was seized simultaneously with symptoms of pleurisy, enteritis, and peritonitis. She was bled, a blister was applied to the parietes of the chest, and calomel was given internally. The abdominal symptoms became less intense, but the thoracic increased in violence, and the effusion became so considerable on the twelfth day that death appeared imminent. The heart was displaced, and the intercostal spaces thrown out. The operation was performed as in the former case, and three pints of purulent serosity extracted. The patient became much easier, but in the course of five days the fluid accumulated to such an extent as to render the operation again imperative. Four pints of serosity, containing an enormous quantity of pus, were extracted, to the great relief of the woman, but the skin not having been used, a small quantity of air entered the thoracic cavity. The following day there was pneumothorax. Four days later the puncture again became indicated, and about four pints of fetid pus were withdrawn. The patient died a few days afterwards. At the autopsy the lung was found firmly bound down by false membranes. The pleural cavity contained about three pints of fetid pus. Slight traces of peritonitis were found in the abdomen, and an abscess of the large ligaments.

M. Trousseau remarks that the second case, one of intense puerperal fever, with a purulent diathesis, was of so unfavorable a nature as not to give paracentesis a fair trial. In the first case the opera-
tion gave immediate relief, and was followed by nearly immediate recovery. He thinks it of great importance to keep the air out of the pleura in order to prevent the decomposition of the pus, and considers that the state of his second patient was aggravated by the omission of this precaution. In addition to the instantaneous relief produced by the subtraction of so large a quantity of fluid, M. Troussseau says that great benefit is experienced from the air, which immediately rushes down into the bronchi, breaking the adhesions which bind the lung down.—Journal de Médecine.

M. Castelnuau on the Causes of Anasarca—M. Andral professes, at present, that anasarca is always occasioned either by disease of the liver, of the heart, by some other obstruction of the venous system, or by Bright's disease. This assertion is not the result of theory, but the expression of his clinical experience. He has, he says, always found dropsies which do not depend on some obstruction to the circulation of the blood to coincide with albuminous urine. M. Castelnuau questions the accuracy of this opinion, and founds his doubts on four cases of anasarca in which the urine was not albuminous, and there was no perceptible obstruction to the circulation in the heart, liver, or any other organ. In these cases the anasarca appeared to be the result of a state of anemia or chlorosis, demonstrated by all the symptoms which indicate the existence of such morbid conditions.—Archives.

Observations on the Treatment of Acute Rheumatism by Cinchona Bark. By John Popham, M. D.—The cases in which it was most successfully employed were those of fibrous rheumatism or rheumatic fever properly so called. When it appeared at all probable that either the pericardium or heart was affected, the bark was not exhibited, at least until the inflammatory symptoms were checked. In capsular rheumatism the bark seemed to disagree with the acute stages, aggravating the symptoms, but in very chronic cases it seemed of service.

The conclusion at which Dr. Popham appears inclined to arrive, with regard to the administration of this medicine, are these:

"That it is important to procure due evacuations previous to the exhibition of the bark, except the patient be greatly deteriorated by constitutional debility, or the protraction of the disease.

"That it is more quickly successful when the disease is early combated by depleting measures, than when inefficiently managed at the onset, and allowed to take root in the system.

"Hence that is more likely to extinguish the disease and prevent chronic infirmity in the sequel of first attacks being uncomplicated, than when a habit has been formed by reason of repeated relapses.

"That the periodicity of the symptoms either peculiar to the attack, or produced by treatment, and the duration and apyresia of the intervals, afford strong presumptive arguments for the use of bark.

"That bark is especially called for in cases where there is complete atony of the cutaneous vessels, so that the skin is unceasingly pouring out acid colliqua..."
tive sweats, giving it a dull and parboiled appearance, at the same time that the pains are abated, and the pulse small and indicating debility.

"That to produce its effects, quantity is not by any means so essential as in intermittent, and that large quantities, especially of the sulphate of quinine, de-

range the stomach in many cases, and bring back the fever.

"That it is judicious to administer it at the periods of remission, and stop it at the return of the exacerbations.

"That it is injurious when important visceral disease co-exists, and is espe-

cially contra-indicated in cerebral or the acute stage of cardiac complications.

"Lastly, that in the synovial variety, it is inferior to other modes of treatment; but in persons of a rheumatic diathesis, when, from the long continuance of the disease the strength has suffered, and disfigurement of the joints has occurred

without serious destruction, a course of bark, combined with sulphur, &c. often prevents the recurrence of subacute attacks, and promotes the absorption of the effused synovia.—Dublin Med. Jour.

On the utility of Surgical Operations in Cancerous Diseases.—The grand points of this most important surgical question are to de-

termine—1, if it be really true that Cancerous disease is primarily of a local nature, and subsequently degenerates into a constitutional malady; and 2, if extirpation, performed at an early period, pre-

vents the occurrence of this degeneration. With the view of elu-

cidating these matters, M. Leroy d'Etiolles has collected the following statistical observations. Of 801 operations, 117 were performed within a twelve-month after the first manifestation of the disease.

Of these 117 cases, there were 61 in which the disease had returned at the time when the reports reached me. It is more than probable that this proportion would be found to be still higher, if we knew the actually present state of these cases.

The results of operations for Cancer of the Lip are curious and worthy of notice, in consequence of the difference in this respect observed in the two sexes. Of 633 cases of Cancer in the male sub-

ject, 165 were examples of Cancer of the Lip: of these 114 were treated with the knife—12 with caustics. There were 15 relapses in all: that is, about an eighth of the whole. On the other hand, of 2,148 cases of Cancer in the female, there were only 34 instances of the disease in the lip; of these, 22 were treated by excision; and in seven—nearly a third—there was a return of the disease.

This difference does not hold good of Cancer of the Tongue: for then the disease is equally fatal in both sexes. Of nine operations, in which a cancerous tumour of this organ was extirpated, three were performed within one twelve-month after the earliest appearance of the disease. In the other six cases, the patients died, the disease having previously returned.

As respects Cancerous diseases of the Mamma, we find the follow-

ing data. Of 277 operations, 73 were performed within the last two years: as yet we cannot say positively what are the results. Of the remaining 204 cases, 22 of them proved fatal in the year after the operation, and in 57 others there was a relapse of the disease.

M. Leroy deduces the following conclusions from his researches:

1. Extirpation does not arrest the progress of Cancerous disease.
2. This operation should not be resorted to, as a general method of treatment, except for Cancer of the skin and lips.

3. There is no necessity to extirpate Cancerous disease of other organs, except when an alarming haemorrhage supervenes.—Comptes rendus.

The Academy appointed MM. Roux, Velepeau, and Serres, to report upon this communication of M. Leroy.—Medico-Chirurgical Review.

_Treatment of Hydrocele with Ioduretted Injections._—In more than 300 cases of this complaint treated with an ioduretted injection, (composed of tincture of iodine 4 parts, and distilled water 125 parts,) by M. Velepeau, not a single accident or unpleasant symptom has ever occurred. One of the patients indeed died; but the fatal result in this instance proceeded from a purulent inflammation of the cellular tissue of the pelvis, quite unconnected with the operation, and not having any communication whatever with the affection of the scrotum. The average period for effecting the cure was 15 days. In one case only the injection found its way into the tissue of the scrotum, in place of the tunica vaginalis: notwithstanding this misadventure, no appearance of gangrene supervened, and the patient recovered without any unpleasant accident.—L’Experience. Medico-Chirurgical Review.

_Ovariotomy._—Dr. Churchill.—Ovariotomy is, just now, a formidable rival for fame, with Mesmerism or Hydropathy. The two former, indeed, ought to go hand-in-hand; for as ovarian tumours seldom grow in any but the patrons and recipients of animal magnetism, it would be a great advantage to those who come under the scalpel, to have its pains and penalties annihilated by the passes of an adroit mesmerist. Be this as it may, the ovarian operation can be tested only by time and statistics—the advocates and opponents steering such opposite courses, and using such ingenious arguments, as to puzzle the practitioner. Statistics will settle the question. Dr. Montgomery has gone into considerable detail on this point, and collected from various points of the compass a mass of materials that may greatly assist our prognosis—perhaps even our diagnosis, in these dangerous cases.

Dr. M. properly remarks, that, under the head of ovarian dropsy, are comprehended many swellings very different from dropsy. There may be a single—or many cysts—and the contents of the cysts may and do vary from clear serum to an almost wholly solid substance. The ovaries may consist of malignant deposits—and last, not least, they may be detached, or they may have acquired extensive adhesions to various adjacent parts, rendering a successful operation all but impossible.

Mr. Southam has published the result of 20 cases of paracentesis—ten from Bright—five from Barlow—and five of his own. Out of these, 14 died within nine months after the first operation. Of
the remaining six, two died in 18 months—and four lived for several years, from four to nine.

Of eleven cases of ovarian dropsy admitted into Guy's Hospital, seven were tapped, three of which were unsuccessful. The proposal of injecting stimulating fluids into the emptied sacs, has, we believe, either never been tried, or entirely abandoned.

The following three tables will exhibit a coup d'œil of the results of almost all the cases on record. It has been constructed with great care and labor by the able and indefatigable author.

**Table I.—Cases of Extirpation of the Ovary.**

<table>
<thead>
<tr>
<th>No. and Date</th>
<th>Operator</th>
<th>Age</th>
<th>Incision.</th>
<th>Result.</th>
<th>Character of Disease</th>
<th>Adhesions</th>
</tr>
</thead>
<tbody>
<tr>
<td>2-1829</td>
<td>Dr. M'Dowall.</td>
<td></td>
<td>9 do.</td>
<td>do.</td>
<td>Gelatinous matter.</td>
<td>Adhesions.</td>
</tr>
<tr>
<td>4</td>
<td>do.</td>
<td></td>
<td>...</td>
<td>do.</td>
<td>do.</td>
<td>Adhesions.</td>
</tr>
<tr>
<td>5</td>
<td>do.</td>
<td></td>
<td>...</td>
<td>do.</td>
<td>do.</td>
<td>Adhesions.</td>
</tr>
<tr>
<td>6</td>
<td>do.</td>
<td></td>
<td>...</td>
<td>Died.</td>
<td>do.</td>
<td>Adhesions.</td>
</tr>
<tr>
<td>7-1821</td>
<td>Dr. N. Smith.</td>
<td>33</td>
<td>3 inches.</td>
<td>Recovered.</td>
<td>Cyst, fluid.</td>
<td>Adhesions.</td>
</tr>
<tr>
<td>8-1825</td>
<td>Mr. Lizards.</td>
<td>36</td>
<td>Long.</td>
<td>do.</td>
<td>do.</td>
<td>Adherent.</td>
</tr>
<tr>
<td>9-1825</td>
<td>do.</td>
<td>33</td>
<td>...</td>
<td>do.</td>
<td>Died.</td>
<td>Adhesions.</td>
</tr>
<tr>
<td>11</td>
<td>Dr. Quittenbaum.</td>
<td></td>
<td>About 4 in.</td>
<td>do.</td>
<td>do.</td>
<td>Adhesions.</td>
</tr>
<tr>
<td>12-1829</td>
<td>Mr. D. Roziers.</td>
<td></td>
<td>About 3 in.</td>
<td>do.</td>
<td>Solid and fluid.</td>
<td>Adhesions.</td>
</tr>
<tr>
<td>13</td>
<td>Dr. Granville.</td>
<td>47</td>
<td>Long.</td>
<td>do.</td>
<td>do.</td>
<td>Adherent.</td>
</tr>
<tr>
<td>15</td>
<td>do.</td>
<td>33</td>
<td>...</td>
<td>Recovered.</td>
<td>Honey-like and green</td>
<td>do.</td>
</tr>
<tr>
<td>17</td>
<td>Dr. Ritter.</td>
<td>21</td>
<td>do.</td>
<td>do.</td>
<td>do.</td>
<td>do.</td>
</tr>
<tr>
<td>18-1826</td>
<td>Mr. King.</td>
<td>87</td>
<td>Short.</td>
<td>Recovered.</td>
<td>Cyst, fluid.</td>
<td>Adhesions.</td>
</tr>
<tr>
<td>19-1826</td>
<td>Mr. Jeffreys.</td>
<td></td>
<td>...</td>
<td>do.</td>
<td>do.</td>
<td>Adhesions.</td>
</tr>
<tr>
<td>21-1832</td>
<td>Dr. West.</td>
<td></td>
<td>Short.</td>
<td>Recovered.</td>
<td>do.</td>
<td>Adhesions.</td>
</tr>
<tr>
<td>22</td>
<td>do.</td>
<td></td>
<td>do.</td>
<td>do.</td>
<td>do.</td>
<td>do.</td>
</tr>
<tr>
<td>23</td>
<td>do.</td>
<td></td>
<td>do.</td>
<td>do.</td>
<td>Died.</td>
<td>do.</td>
</tr>
<tr>
<td>25</td>
<td>Mr. Hargraves.</td>
<td>40</td>
<td>do.</td>
<td>do.</td>
<td>Multiloc, cysts.</td>
<td>Adhesions.</td>
</tr>
<tr>
<td>26</td>
<td>Dr. Clay.</td>
<td>46</td>
<td>27 inches.</td>
<td>Recovered.</td>
<td>Cysts, sol. and fluid.</td>
<td>do.</td>
</tr>
<tr>
<td>27</td>
<td>...</td>
<td></td>
<td>67 14 do.</td>
<td>do.</td>
<td>do.</td>
<td>do.</td>
</tr>
<tr>
<td>28</td>
<td>...</td>
<td></td>
<td>39 28 do.</td>
<td>do.</td>
<td>do.</td>
<td>do.</td>
</tr>
<tr>
<td>29</td>
<td>...</td>
<td></td>
<td>40 14 do.</td>
<td>Died.</td>
<td>do.</td>
<td>do.</td>
</tr>
<tr>
<td>30</td>
<td>...</td>
<td></td>
<td>23 14 do.</td>
<td>Recovered.</td>
<td>do.</td>
<td>do.</td>
</tr>
<tr>
<td>31</td>
<td>...</td>
<td></td>
<td>40 14 do.</td>
<td>Died.</td>
<td>do.</td>
<td>do.</td>
</tr>
<tr>
<td>32</td>
<td>...</td>
<td></td>
<td>43 14 do.</td>
<td>Recovered.</td>
<td>do.</td>
<td>do.</td>
</tr>
<tr>
<td>33</td>
<td>...</td>
<td></td>
<td>59 16 do.</td>
<td>Died.</td>
<td>do.</td>
<td>do.</td>
</tr>
<tr>
<td>34</td>
<td>...</td>
<td></td>
<td>46 16 do.</td>
<td>Recovered.</td>
<td>do.</td>
<td>do.</td>
</tr>
<tr>
<td>35-1840</td>
<td>Mr. B. Phillips.</td>
<td></td>
<td>2 inches.</td>
<td>Died.</td>
<td>do.</td>
<td>Adhesions.</td>
</tr>
<tr>
<td>31-1841</td>
<td>Dr. Stillig.</td>
<td></td>
<td>6 do.</td>
<td>do.</td>
<td>do.</td>
<td>do.</td>
</tr>
<tr>
<td>37-1842</td>
<td>Mr. Waine.</td>
<td>68</td>
<td>Long.</td>
<td>Recovered.</td>
<td>do.</td>
<td>None.</td>
</tr>
<tr>
<td>38-1843</td>
<td>do.</td>
<td>57</td>
<td>do.</td>
<td>do.</td>
<td>do.</td>
<td>do.</td>
</tr>
<tr>
<td>39</td>
<td>do.</td>
<td>57</td>
<td>do.</td>
<td>do.</td>
<td>do.</td>
<td>do.</td>
</tr>
<tr>
<td>41-1843</td>
<td>Mr. Morris.</td>
<td></td>
<td>do.</td>
<td>do.</td>
<td>Cystic sarcoma.</td>
<td>do.</td>
</tr>
<tr>
<td>42-1845</td>
<td>Mr. Southam.</td>
<td></td>
<td>do.</td>
<td>do.</td>
<td>Cyst and fluid.</td>
<td>do.</td>
</tr>
<tr>
<td>43-1841</td>
<td>Dr. F. Bird.</td>
<td>3 or 4 in.</td>
<td>do.</td>
<td>do.</td>
<td>Cysts and solid mat-</td>
<td>do.</td>
</tr>
<tr>
<td>44-1841</td>
<td>do.</td>
<td></td>
<td>do.</td>
<td>do.</td>
<td>ter.</td>
<td>do.</td>
</tr>
<tr>
<td>45</td>
<td>Mr. Allen.</td>
<td>3 inches.</td>
<td>do.</td>
<td>do.</td>
<td>do.</td>
<td>Adhesions.</td>
</tr>
<tr>
<td>46</td>
<td>Mr. Lane.</td>
<td>Long.</td>
<td>do.</td>
<td>do.</td>
<td>do.</td>
<td>do.</td>
</tr>
<tr>
<td>47</td>
<td>Mr. Key.</td>
<td>19</td>
<td>do.</td>
<td>D. ed.</td>
<td>do.</td>
<td>do.</td>
</tr>
<tr>
<td>48</td>
<td>Mr. Greenhow.</td>
<td>29</td>
<td>do.</td>
<td>do.</td>
<td>do.</td>
<td>do.</td>
</tr>
<tr>
<td>49</td>
<td>Mr. B. Cooper.</td>
<td>32</td>
<td>do.</td>
<td>do.</td>
<td>do.</td>
<td>do.</td>
</tr>
</tbody>
</table>
TABLE II.—Cases of Ovarian Disease, in which the operation could not be completed.

<table>
<thead>
<tr>
<th>Date</th>
<th>Operator</th>
<th>Cause of failure</th>
<th>Result</th>
<th>Incision</th>
</tr>
</thead>
<tbody>
<tr>
<td>51</td>
<td>Mr. Lizars.</td>
<td>Solid and very vascular tumour.</td>
<td>do.</td>
<td>do.</td>
</tr>
<tr>
<td>52—1826</td>
<td>Dr. Granville.</td>
<td>Firm Adhesions.</td>
<td>do.</td>
<td>6 inches.</td>
</tr>
<tr>
<td>53</td>
<td>Dr. Dieffenbach.</td>
<td>Vascularity.</td>
<td>do.</td>
<td>Long.</td>
</tr>
<tr>
<td>54—1826</td>
<td>Dr. Martini.</td>
<td>Solid and fixed tumour.</td>
<td>Died.</td>
<td>do.</td>
</tr>
<tr>
<td>56</td>
<td>M. Dolhoff.</td>
<td>do.</td>
<td>do.</td>
<td>Long.</td>
</tr>
<tr>
<td>57</td>
<td>Dr. Clay.</td>
<td>Exten. Adhesions.</td>
<td>do.</td>
<td>5 inches.</td>
</tr>
<tr>
<td>58</td>
<td>Mr. Walne.</td>
<td></td>
<td>Recovered.</td>
<td></td>
</tr>
</tbody>
</table>

TABLE III.—Cases in which the Operation failed from Error in Diagnosis.

<table>
<thead>
<tr>
<th>Date</th>
<th>Operator</th>
<th>Result</th>
<th>Disease</th>
</tr>
</thead>
<tbody>
<tr>
<td>59—1823</td>
<td>Mr. Lizars.</td>
<td>Recovered.</td>
<td>No tumour found.</td>
</tr>
<tr>
<td>60—1834</td>
<td>Mr. King.</td>
<td>do.</td>
<td>do.</td>
</tr>
<tr>
<td>61</td>
<td>M. Dolhoff.</td>
<td>do.</td>
<td>Uterine tumour.</td>
</tr>
<tr>
<td>63</td>
<td>do.</td>
<td>Recovered.</td>
<td>Pelvic tumour.</td>
</tr>
<tr>
<td>64</td>
<td>do.</td>
<td>Died.</td>
<td>Uterine tumour.</td>
</tr>
<tr>
<td>65</td>
<td>do.</td>
<td>do.</td>
<td>do.</td>
</tr>
<tr>
<td>66</td>
<td>Mr. Heath.</td>
<td>do.</td>
<td>do.</td>
</tr>
</tbody>
</table>

Thus, the entire number amounts to 66, of which 42 recovered and 24 died—or about 1 in 2 1/2. Of the 49 cases in which the ovary was extirpated, 16 died, or 1 in 3. Of the nine cases in which the operation could not be completed, four died—or 1 in 2 1/2; and of the eight cases where the operation was unnecessary, 4 died, or 1 in 2.

Age does not appear to have had much influence, beneficial or otherwise, and the same may be said of marriage. Adhesions render the result of the operation much more dangerous than freedom from the same, and yet not so much so as one would, a priori, except. Where other organic diseases co-existed with ovarian, the termination was almost always fatal. It is strange that the operation should have been ever performed, where no tumour has existed; yet the mistake has been made by eminent surgeons, and without any negligence on their parts.

Dr. Montgomery mentions a case where he felt a distinct tumour in a female's abdomen, which suddenly vanished in the very act of
examination! The abdominal muscles, in fact, often act in such a way as to imitate organic enlargements of the liver, spleen, ovaries, &c., and thus deceive even the most careful practitioners. After many judicious remarks, cautions, and comparisons, our author comes to the following conclusions:

"Even after the details I have given, it is very difficult to come to a definite and perfectly satisfactory conclusion, because 1, we have not sufficiently accurate data to estimate the progress of the disease unaided by surgery. 2. The table quoted from Mr. Southam is clearly too limited to afford a fair average of the results of tapping, and it is not easy to obtain sufficient facts to enlarge it. 3. The cases in which ovariotomy has been performed are of such a mixed character, that it is impossible to select with fairness those cases in which the operation was demanded for the relief of urgent suffering, and suitable to the nature of the disease, without the appearance of partiality. And 4, from the obscurity of the diagnosis, it is too much, perhaps, to expect that our practice in future will be free from those drawbacks on the operation.

"But bearing in mind these difficulties, and making allowance for those drawbacks, I think we may conclude that there are cases in which the operation would be justifiable; and on these grounds,—we find the general opinion is against the curability of the disease by medical means:—that after a time the patient will die from local disease or accident, or constitutional disturbance, and that meantime she suffers more or less inconvenience:—that tapping in almost all cases affords but temporary relief:—and that, as far as the limited statistics we have adduced are admissible as evidence, it is attended with great danger: i.e. 1 in 5 died of the first operation, and of twenty patients, fourteen (more than two thirds) died within nine months of the first tapping; whilst of the entire number of those who underwent the operation of ovariotomy, about one half have absolutely recovered so far."

The foregoing paper is very creditable to the industry, the talents, and the judgment of its author.—Dublin Journal, July, 1844.

FROM THE LONDON LANCET.

Camphor a Preservative of Ergot of Rye.—Sir: I was not a little surprised to read some remarks by Mr. Rawle, stating that he had discovered camphor to be a preservative of ergot of rye. I can only say that I have been in the habit of using it for the last nine or ten years, but not exactly in the manner prescribed by him. I order the camphor to be mixed with the powdered ergot, in the proportion of a grain in every scruple. By this means I think the camphor is more intimately diffused throughout the whole than can possibly take place by the plan proposed by Mr. Rawle. I do not give this either as a new, or, indeed, my own discovery; for I adopted the method by having seen it in the practice of Mr. Spurgin, an old practitioner at Saffron Walden, and from whom I have every reason to believe that your correspondent also obtained the same information, he having been engaged in the same gentleman's practice.

If you think the above worthy of notice, you will oblige, Sir, yours respectfully,


Staines, August 28, 1844.
Simple Method of Preparing the Pilula Ferri Iodidi.—Take of iodine 127 grains, iron wire, about the thickness of a thin quill, half-an-ounce, distilled water 75 minims. Agitate them briskly together in a strong ounce-phial, provided with a well-fitted glass stopper, until the froth which forms becomes white, which will happen in less than ten minutes. Pour the liquid upon two drams of finely-powdered loaf-sugar in a little mortar, and triturate immediately and briskly for a few minutes; add gradually a mixture of the following powders, viz: liquorice powder half-an-ounce, powder of gum arabic a dram and a half, and flour one dram. Divide the mass into 144 pills.

Each pill contains about a grain of iodide of iron.

In operations on the large scale, the bottle ought to be wrapped in a strong towel, in case of an explosion being caused by the evolution of steam from the heat produced; and even on the small scale, the stopper must be held firmly, otherwise it will probably be blown out and the materials lost.—Pharmaceutical Journal. Medico-Chirurgical Review.

Rhatany in Chronic Catarrhal Ophthalmia.—M. Reveillé-Parise strongly recommends the decoction, or a strong infusion, of Rhatany root, as a lotion with which the affected eyes are to be bathed. Besides acting as an astringent, this remedy seems to have some other mode of operation; for we do not find that similar preparations of oak-bark or of gall-nuts—although both of these contain a large portion of tannin—are equally efficacious, as Collyria in the Ophthalmia alluded to. The application should be used lukewarm, and a few drops of Goulard’s Extract may be added to it, if deemed proper.—Medico-Chirurgical Review.

MEDICAL INTELLIGENCE.

The January No. of the New-York Journal of Medicine, (just received,) contains a short biography of its late Editor, Dr. Samuel Forry; and also a history of his last illness, prepared by Charles A. Lee, M. D. Professor, &c., and entitled, “Epilepsy terminating fatally; with hypertrophy and induration of the cerebral substance, induced by excessive mental application.”

We are pleased to find that the Journal is to be continued, notwithstanding the decease of its late able Editor.

Fiske Fund Prize Questions—The Trustees of the Fiske Fund, in Rhode Island, propose the following questions for 1844-45: 1. “The best mode of treating, and the best apparatus for the management of, fractures of the thigh.” 2. “The character, causes and best treatment of bronchitis.” For the best dissertation on each of these questions, the sum of fifty dollars will be paid—the dissertations to be sent, previous to May 10, 1845, to Dr. L. L. Miller, of Providence, Dr. T. C. Dunn, of Newport, or Dr. Jabez Holmes, of Bristol.—Y. Y. Journal of Medicine.