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ARTICLE VII.

Remarks on the Diagnosis of Dislocations of the Shoulder Joint.

By L. A. Dugas, M. D., &c.

The very frequent instances of permanent disability and deformity consequent upon the non-detection of dislocations of the Humerus, abundantly demonstrate the difficulty of their diagnosis, and would seem to demand at the hands of the profession a more careful study of the subject. The object of this paper is therefore to refresh the memory of those who will be more likely to read a periodical than to go back to their systematic treatises, and at the same time to suggest a means of diagnosis not referred to in works on Surgery, so far as my recollection serves me.

I will not stop to notice the various classifications proposed by authors, especially the French and Germans, who have perhaps multiplied varieties unnecessarily; but at once adopt that commonly admitted in this country. The head of the humerus may then be dislocated downward, into the axilla; forward, below the clavicle; and backward, upon the scapula. By far the most common of these accidents, is that in which the head of the bone is found in the axilla; whilst that in which the head of the humerus rests upon the scapula is so exceedingly rare that many experienced surgeons have never seen a case of the kind. The reason of this relative difference is easily ascertained by reference to the anatomical arrangement of the parts; for although the so-called glenoid cavity

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is in reality almost a plane surface, which could of itself offer no impediment to dislocations in any direction, the head of the humerus is effectually prevented from escaping in certain directions by the strong arch which overhangs the articular surface of the scapula. This arch is formed by the acromion and coracoid processes and the intervening ligaments, fortified by the capsular ligament and the deltoid muscle. Now, while the anterior margin of this arch, formed by the coracoid process, does not descend below the upper fourth of the glenoid cavity, its posterior margin, consisting of the root of the acromion, comes down sufficiently low to protect almost, if not quite, one half of the articular surface. The only space left, therefore, by which the humerus could escape backward, is that which intervenes between the acromion and the attachment of the great and strong tendon of the triceps extensor cubiti, which space is very small when compared with that between this tendon and the coracoid process. Hence the variety of one accident and the frequency of the other.

But, to return to our immediate subject, the first consideration should be, not the signs or symptoms by which we may discriminate between the different dislocations of the shoulder, but rather the means by which we may determine whether there is any dislocation at all, or not. It is self-evident that if it can be ascertained that the head of the humerus is not in the glenoid cavity, it is displaced, or, in another word, dislocated. This fact can usually be determined by the depression observable below the end of the acromion process and beneath the deltoid muscle, both by the eye and by the hand pressed upon the shoulder. But there are many cases in which the fleshiness of the patient, or the tumefaction consequent upon the injury, are such as to render this element of diagnosis of little value, especially when we recollect that more or less depression would attend a fracture of the humerus near its head, and even a fracture of the neck of the scapula. Such a degree of depression as we might by careful examination detect in a fleshy person, or when there is much swelling, would therefore scarcely be deemed sufficiently positive in itself for practical purposes. It would become necessary to determine whether or not this depended upon the existence of a fracture. This might be done by endeavoring to produce crepitus, and by looking for the head of the bone in the localities in which it might be carried by dislocation. The sensation of crepitus and the absence of the
head of the humerus from the axilla, from below the clavicle and from the dorsum of the scapula, would establish conclusively that the cause of the depression observed, is to be found in a fracture; whereas the detection of the head of the humerus in any other place than in the glenoid cavity would establish the existence of a dislocation as the cause of the depression in question.

Again: the existence of dislocation may be sometimes determined by the detection of the head of the os humeri in an abnormal situation; and this would be conclusive without reference to any depression below the acromion. But there are individuals whose embonpoint or whose muscular development is such as to render this detection extremely difficult. Moreover, a fracture near the head of the humerus might be attended with such a displacement of the upper end of the lower fragment as to allow this to be taken for the head of the bone in the axilla, especially in individuals presenting the peculiarities just mentioned, or in cases attended with much swelling. It should be remembered that the head of the humerus becomes more prominent and may be felt in the axilla when the elbow is elevated, although it may have escaped detection while the arm was allowed to hang down.

It is needless to dwell upon the value of the information to be derived from the history of the accident, of the degree or kind of disability experienced by the patient, of the change in the length of the arm and direction of the shaft of the bone, of the pain induced by carrying the elbow in different directions, &c. All these circumstances should of course be studied by the surgeon in doubtful cases; but neither of them can lead to anything positive.

From these reflexions the necessary inference must be drawn that a concurrence of circumstances can alone be relied upon in certain cases to make the diagnosis of dislocation clear and positive. The discovery, then, of some one, unequivocal, pathognomonic fact, or sign, by which a dislocation of this joint could in all cases be readily ascertained, must be a great desideratum. I believe that this will be found in the following maxim: if the fingers of the injured limb can be placed (by the patient or the surgeon) upon the sound shoulder while the elbow touches the thorax, there can be no dislocation; and if this cannot be done, there must be a dislocation. I am aware that most writers indicate the difficulty of bringing the elbow in contact with the thorax as a symptom more or less marked in the several dislocations of the humerus,
but it is obvious that they do not assign to it the degree of impor-
tance to which it is entitled when coupled with the injunction
to place the fingers upon the opposite shoulder, as just enunciated.
Indeed, most authorities merely mention the fact that the elbow
cannot be approximated to the trunk without pain. I insist that
the elbow cannot be brought against the thorax when carried
sufficiently in front to allow the fingers to rest upon the sound
shoulder, either by the patient or by the surgeon, not because this
would be painful, but because it is physically impossible without
a reduction of the dislocation or such a disruption of the shoulder
as no one would think of attempting. This becomes at once mani-
fest by looking to the skeleton. With this before us, let us place
the head of the humerus in the axillary region, and we observe
that the elbow is at once thrown out from the trunk; that if we
attempt to bring it towards the sternum the upper portion of the
shaft of the humerus will soon encounter the ribs, and that the
rotundity of the thorax will effectually prevent any contact be-
tween the elbow and sternum, unless, by using the humerus as a
lever, we rend asunder all the obstacles in the way of the rising
of the head of this bone. Our maxim then holds good with re-
gard to dislocations into the axilla. Let us now place the head
of the humerus where it would be found in a forward dislocation;
that is to say, below the clavicle. We will then perceive that the
elbow is still removed from the trunk, and that it is moreover
directed backward, thus presenting, perhaps even to a greater de-
gree, all the difficulties just specified, if it be endeavored to carry
the elbow against the sternum and the fingers upon the other
shoulder. With regard to the dislocation backward, the impossi-
bility of bringing the elbow and fingers in the positions indicated,
becomes so manifest that it requires no comment.

Now, can any other accident than a dislocation offer these ob-
stacles to placing the elbow and fingers in the above-mentioned
position? I unhesitatingly answer, there is none. It is true, that
a severe contusion may render this or any other motion more or
less painful to the patient, and that he will aver that he cannot do
as directed. But the surgeon will find no difficulty in accom-
plishing his design, especially if he proceed gently, as not to
encounter any muscular resistance, voluntary or otherwise. The
same remarks apply with equal force to fractures of the upper end
of the humerus or of the neck of the scapula. In all these cases
the administration of an anaesthetic will save the patient some pain and facilitate the examination.

If, then, a dislocation of the humerus in any direction, whatever, renders it impossible to place the fingers and elbow simultaneously in the position stated, and that no other accident can produce this inability, the corollary is clearly that such an impossibility is pathognomonic of dislocation of the humerus; and, conversely, that the absence of this inability as clearly demonstrates the absence of dislocation.

Having in this manner settled the all-important question of the existence or non-existence of dislocation, it will not be found very difficult to determine the new position assumed by the dislocated head of the humerus. Indeed the more direction of the axis of the humerus will suffice in most instances to lead us directly to its head. This axis can be usually traced without difficulty, with the fingers, from the elbow up to at least six or eight inches; a length sufficient to allow the imagination to prolong it to the upper end, if the tumefaction or other conditions should prevent more effectual means from being used. A straight rod placed over that portion of the axis which can be felt, would assist the determination of its extension, and this would lead either to the axilla, the acromial portion of the clavicle, or the dorsum of the scapula, according to the variety of the dislocation.

The next step should be to endeavor to feel the head itself, as distinctly as may be, for there can be no harm in multiplying our resources, and it is important that the new position of the bone be accurately determined. As already intimated, the head of the bone can be more distinctly felt in the axilla when the arm is elevated than when otherwise. If it be below the clavicle, it will be made more prominent by carrying the elbow forcibly backward; and, finally, if dislocated upon the scapula the elbow should, on the contrary, be forcibly carried forward, in order to increase the prominence of the head of the humerus.

I have thus endeavored to direct attention to what I deem an exceedingly important addition to our means of diagnosticating dislocations of the shoulder, and have done so as briefly as the nature of the subject would permit. I may have erred in claiming for my views any originality, for the "wise man" has long since said that "there is nothing new under the sun," and daily experience confirms the correctness of the apothegm. I have neither the
leisure nor the inclination to thumb the numerous tomes of Surgical literature for the purpose of settling this point. It is certain, however, that if the method of examination here recommended has ever been suggested before, it has not been insisted on by the systematic writers usually consulted by our practitioners. If I may, therefore, have simply revived and magnified the value of what was known before, so that the profession and humanity may derive any advantage from this paper, my object will have been attained.

ARTICLE VIII.

LETTERS FROM SAML. D. HOLT, M.D., UPON SOME POINTS OF GENERAL PATHOLOGY.

LETTER NO. 9.

Montgomery, Ala., Feb. 7th, 1856.

Messrs. Editors—In accordance with the objects which I have in view, it is necessary that I should make a few remarks respecting the therapeutic properties of Quinine, and lest it should be inferred from what I have already said, that I have fostered some hostility or prejudice against it, I may be permitted to state, that I entered the profession about the time, or soon after its introduction into practice, and from that time to the present, have been one of the most uniform and consistent advocates for its use; and though I administered quinine more than twenty years ago, in as large doses as I have ever found it necessary to administer since, I can lay no claim to credit for the progress which its introduction into practice has made in the healing art, nor have I been able to keep pace with the rapid strides which the profession in the South have made in the uses of quinine for the last few years. For a great while, and long before the discovery of quinine, Peruvian bark was held in high esteem as a stimulant and tonic, by Sydenham, Darwin, Cullen, and others, not only for the cure of simple intermittent and remittent fevers, but was freely used and recommended by them for the treatment of other forms and low grades of fever, both idiopathic and symptomatic, with the view and for the purpose of sustaining or building up the sinking or exhausted energies of the system; and there is no evidence that I am aware of, which goes to show that they regarded it in any other light, or as possessing any other valuable therapeutic properties than that of stimulant and tonic. Since the discovery and introduction of
quinine into practice, it has acquired a much greater popularity, and a much more extended application to the cure of diseases than the bark ever possessed; and more recently still it has grown so rapidly into professional favor, as not only to have led to the rejection of other valuable remedies, but has in a measure endangered, for a time at least, its high reputation and its intrinsic usefulness, by its application to diseases and conditions to which it is not suited, and ascribing to it properties and virtues which it does not possess, or to say the least, which are extremely problematical; such, for instance, as its antiphlogistic and sedative properties, which are well calculated to lead to error in practice, as well as to bring the remedy into disrepute. Now, I do not feel inclined at this time to enter into an argument with the ultra quininists upon these points; but, as their testimony tends most decidedly to establish the truth of one of my first and most important propositions with respect to the character of the diseases of our climate and latitude, namely, that they are generally not only periodic, but that their general tendency is to debility, or depression and congestion, and requiring stimuli and tonics for their cure, I may be permitted to examine into and make a few remarks respecting these alleged properties of quinine.

Well, we have it upon very good authority, that large doses of quinine will give a dog fits, and evidence is not wanting to prove that it will serve a man in the same way; but that does not prove that quinine is sedative, as I think I shall be able to show. Nor can it be received as a stronger argument against the proper use of quinine, than would be a like argument against the use of puddings, founded upon the popular belief, that "too much pudding will choke a dog." But as there are large numbers of the profession who contend for the sedative and antiphlogistic properties of quinine, and who persist in its use in fitiferous doses, it will not be amiss to enquire into the circumstances which have most probably led them to their conclusions, the chief of which, no doubt, has been from observing its beneficial effects in those diseases and constitutions of the system, for the cure of which the old writers spoken of recommended the free use of bark, under the belief of its stimulant and tonic properties; in which, however, they may have been mistaken. (?) Observing, I say, the beneficial effects of quinine in such affections as pneumonia, and other phlegmasiae of the lower grades, bilious fevers, irritant and congesto-irritant fe-
vers, and other fevers of like pathological character and conditions, manifesting the signs of excitement and periodicity, and still adhering to the preconceived and firmly established belief of their inflammatory character, requiring sedative and antiphlogistic remedies for their cure, when they came to "change their method," it became necessary to "disabuse themselves" of an improper use of the lancet, purgatives and other sedatives and antiphlogistic remedies, or to accommodate and reconcile the therapeutic properties of the "chemical," to the existing character and condition of the disease, which many have yet failed to do. But how it happens that quinine being sedative and antiphlogistic (?) should prove to be so proper and valuable a remedy in those diseases and conditions, in the treatment of which, the lancet, purgatives, and other sedatives and antiphlogistics once held the lead, but which are now laid aside as improper, and even hurtful, I must leave to be answered by those who believe quinine to be sedative and antiphlogistic, or the diseases and conditions in which it is most valuable and proper, to be inflammatory, requiring such remedies. For my own part, I reached the quinine platform by a different route, by assuming that the diseases in question were not inflammatory and phlogistic in their character, and consequently the lancet, purgatives, &c., were not proper remedies for their cure, but that generally they manifested not only the signs of periodicity, but evidently the signs of debility and depression, requiring the use of stimuli and tonics, of which quinine has been proven to be the best of the class. But it matters little what road we may have traveled, since we find ourself at last upon the great platform, which it seems is large enough to accommodate all the world of doctors, if not the rest of mankind; for, walking down the street not long since, I met a gentleman who presented me for inspection, a box of pills, which he stated he had just obtained from a professed homoeopathist. Curiosity prompted me to examine them, and on opening the box, I found it filled with 5 gr. quinine boluses. Well, I did not say so, but I thought that if our people had a right to steal their thunder, they had as good a right to steal ours.

But, to be serious in this matter, I really cannot perceive upon what good and sufficient grounds, the ultra quininists base their belief in the sedative and antiphlogistic properties of quinine. In all intermittent and remittent affections which are controlled by the laws which govern their type, each paroxysm consists of a pe-
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Periodic fall and rise of nervous excitement according to that type, with a corresponding ebb and flow in the circulatory system; and each paroxysm, of whatever type it may be, terminates within twenty-four hours from its beginning. Such appears to be the general law which governs all malarial and periodic diseases. This oscillatory state of the nervous system, and the corresponding ebb and flow in the vascular system, often constitutes the chief element of disease in simple intermittents, which being once interrupted, frequently puts a stop to their further progress; for this purpose, quinine is successfully used, not, however, with the view of subduing excitement, but for the purpose of preventing depression. Knowing the precise time at which the nervous depression, and the ebb in the circulation is to take place, from knowing the type of the disease, quinine is used for the purpose of raising a state of artificial excitement in substitution for the pathological state of depression and congestion. The excitement thus created continues until the time of its periodicity has gone by, and subsides with the stimulant effects of the quinine, while its tonic effects serve as a protection against depression at the period for the recurrence of the next paroxysm. If quinine will produce such results, where is the evidence of its sedative and antiphlogistic properties? Again: if quinine be administered in those forms and conditions of disease which I have already noticed as irritant, congesto-inflammatory and congesto-irritant, either with or without attending local inflammation, in which there is an evident "want of innervation," and in which there is an evidently diminished quantity of blood in the arterial vessels on account of the weakened and relaxed condition of the systemic capillaries, as manifested by a feeble and often exceedingly rapid or frequent pulse, and which condition has too often been the pretext for the use of sedatives and antiphlogistics, under the apprehension of the existence of inflammation. The effects of quinine in all such cases will be to increase the volume and strength, and diminish the rapidity or frequency of the pulse, whereas, in such conditions the direct effect of sedatives and antiphlogistic remedies, such as the lancet, purgatives, &c., is invariably to diminish the strength and volume and to increase the frequency of the pulse. Here then we observe, in juxta-position, the stimulant and tonic effects of quinine upon the general capillaries, whereby they are enabled to resist the too frequent transmission of blood through them, thus allow-
ing the arteries to become refilled, increasing the volume and diminishing the frequency of the pulse; while the effects of the lancet, purgatives, and other like remedies, tend still further to weaken and relax the capillaries, whereby the blood is allowed to escape more rapidly out of the arteries and thus the pulse becomes diminished in volume and increased in frequency. If such be the action or effects of these remedies, where is the evidence that quinine possesses sedative or antiphlogistic properties? Quinine may be, and often is administered with the happiest results in febrile affections with and without the existence of local inflammation, as pneumonia, bilious fever, &c., in which there is an evident preponderance of excitement. But it must be remembered that all such cases are amenable to the laws of periodicity, and few practitioners, I imagine, who profess to be governed in their practice by the rules of pathology, would venture upon the use of quinine in conditions of high nervous and vascular excitement or inflammation before depletion, unless they felt very confident of the approach of a period of remission, but upon that certainty, or expectation and belief, they are willing to run the risk of raising the excitement a few degrees in order to meet the approaching state of depression or the remission, with the tonic effects of the remedy. But it will not do to infer from the beneficial effects of quinine, in such cases, that it is either sedative or antiphlogistic, for the same general results will be obtained whether local inflammation does or does not exist, for quinine when used in such cases (pneumonia for instance) is not with reference to the inflammation in the lungs, but in reference to the character of the general febrile condition which accompanied it, one being amenable to the laws of periodicity, the other not so. And it must be remembered too, that while so few of our diseases of all descriptions are of such high inflammatory grade as to have lead to a general abandonment of the lancet, purgatives, and other sedative and antiphlogistic remedies, there are as few which do not admit of, and require the use of quinine for their cure.

There is a point in relation to inflammation upon which I desire to say a few words, in order to prevent any misunderstanding of my views upon the subject: Heretofore, I have spoken of inflammation rather incidentally than otherwise, and may have used too indiscriminately the terms inflammation and its correlative inflammatory; and what I now desire to say is, that I regard in-
flammation as always a local affection of greater or less extent, and going through a series of phenomena in such location, having (according to some of the best physiologists and pathologists whose views and opinions I am rather inclined to adopt,) as its proximate cause, first, a state of debility, second, a state of irritation, and third, a state of inflammation which often does, but by no means necessarily, bring the general system under its influence to an appreciable extent. The term inflammatory, as it has been used, does not necessarily imply the existence of inflammation, but is used to express a general condition of the system characterised by excitement and increased vascular action, sustained by general vigor and tonicity of the system. And just here we must look for the characteristic difference between inflammation and irritation, as local affections, and the inflammatory and irritant, as general conditions, to be found as hereafter explained, in the condition of the skin, and the state of the pulse. With these explanations, I will proceed.

Although I am a decided advocate of quinine, in wholesome doses, in malarial fevers generally, and other affections which fall under this influence and the laws of periodicity, I cannot subscribe to its use in those "pernicious" doses which give dogs fits, and render men blind, deaf and crazy. Nor can I yield my ready belief, (although the declaration comes to us from high authority,) that quinine possesses the property of "aborting" fevers of a different type, such, for instance, as yellow fever, and typhoid fever, as my own observation and experience in those diseases (though limited, it is true) serves to raise a doubt, whether the cases so "aborted" may not have been so invested with malarial and periodic influences, if not to obscure their essential typical character, to have at least brought them legitimately within the dominion, and under the control of quinine. And my limited observation has taught me to believe that the typhoid condition is, at best, but indifferently amenable to the action of quinine, and that the nearer such diseases approach their true and essential, typical or typhoid character, the less they become so. This opinion and belief is strengthened and sustained by the fact, that quinine exerts no evident chemical or therapeutic action upon the constitution of the blood and other circulating fluids, and that its action is very feeble, if indeed it has any at all, in promoting the important functions of secretion, excretion, absorption, nutrition, &c. In such a
view of the case, I cannot well conceive how physicians can rely with confidence of success upon the action of quinine in the treat-
ment of yellow fever, and other fevers of like type, in which there is often not only an adynamic condition, but an evident depravity and vitiation of the circulating fluids, requiring not less the action of such remedies as promote the functions of secretion, and excre-
tion, than those strictly dynamic in their character.

Of the "modus operandi" of quinine, as little is known, beyond its effects, little is required to be said. But assuming that stimuli are those substances which produce excitement and increased vas-
cular action, by increasing the action of the heart and arteries, and the respiratory movements, such, for instance, as brandy, ether, ammonia, camphor, opium, &c., the slight and almost un-
appreciable effects of this kind, would lead us to infer that quinine is but slightly stimulant, and yet it has the power of creating and sustaining excitement, against the influences of depression, superi-
or to all other stimulants, which leads us to the further inference that, while brandy, ether, &c., exert their influence upon the brain, and other great nervous centres, creating general excitement, and increasing the action of the heart and arteries, the action of quinine is directed to, and exerted upon, those nerves especially which control the general capillaries, where its stimulant and tonic pro-
erties are chiefly manifested, as already has been explained. I would not deny that an impression is made by quinine upon the nervous centres, and that it will, when first administered, increase to a slight degree the action of the heart and arteries; but I am by no means satisfied that the pernicious effects of quinine, which we sometimes hear of, depend upon its sedative properties, but that the more probable solution of the matter is, that those effects are properly ascribable to its stimulant and tonic effects, in excess, upon the capillaries, giving rise to arterial plethora, thus over-
whelming the brain and other nervous centres, producing stupor and convulsions, and destroying the sense of hearing, seeing, &c.; or in less degree, giving rise to headache, dimness of vision, par-
tial deafness, ringing in the ears, &c., producing a condition very similar, if not identical, with that which I described in my last letter, as "a condition of oppression" which may usually be re-
lieved by bloodletting.

It was my intention to have taken a more extended view of this subject, but as it will necessarily come up again, in conne-
tion with the treatment of some of our most important diseases, according to the classification which I have made of them, I will dismiss the subject for the present, by saying, that one effect alone constitutes the basis upon which the deserved popularity of quinine rests, as a remedial agent, in the management of the diseases of our climate, which is, its unqualified, stimulant and tonic effect upon the nerves which control the capillary circulation, enabling them to resist the influence of periodic states of depression, and thus prevent the formation of congestion. It was my intention, also, to have given to Opium a separate consideration, but in consequence of its diversified modes of action, and its equally diversified applicability and suitableness to the perpetually changing conditions of the system, I have determined to consider it in connection with the treatment of diseases as just stated.

My next letter will be chiefly appropriated to a consideration of the uses and abuses of Calomel.

Yours, as ever,

SAML. D. HOLT.

ARTICLE IX.

Case of Peritonitis, terminating by the extension of Inflammation in Suppuration of the Testicles. By C.M. Rivers, M.D., of DeKalb County, Ga., formerly of Charleston, S. C.

While residing in Charleston, I was on the 17th August, '55, called to see a patient a few miles from the city. On my arrival, I found him suffering with an enormous Scrotal Hernia, extending to the knee. He had been lying in that situation twelve hours before I saw him. I attempted reduction, but there appearing to be some constriction (not, however, amounting to strangulation) about the external abdominal ring, and no assistance at hand, I determined to put him upon the use of tart. ant. grs. ii, tinct. opii, 5ii, aqua 5vi, in connection with warm poultices to the inguinal canal, with a view to promote relaxation, and ordered him to remain in bed.

18th. Had slept comfortably the previous night; felt little or no pain; had an alvine evacuation during the night, and from the peristaltic action induced therefrom, the greater portion of the protruded intestine had returned within the abdominal cavity. The reduction of the remainder was easy, owing to the amount of
relaxation produced by the previous day’s prescription. After the reduction, I immediately applied a suspensory bandage, and directed him to keep his bed for at least a week. I left him, seeing nothing in the case to call for another visit.

19th. To my utter astonishment I was again summoned to my patient, with the startling intelligence that “he is dying.” The request was made that Dr. Toomer, of Charleston, should see him with me in consultation, to which I agreed. He accordingly went, and we found him in truly a deplorable situation. Abdomen enormously distended; tongue fiery red, dry and cracked; pulse eorded; countenance anxious to the most intense degree; testicles and serotum one hard, indurated mass, red and exceedingly hot. Dr. T. (from my statement as to how I had left the patient the day before) was extremely puzzled, as well as myself, to account for this sudden peritonitic state; but by close examining and cross-questioning, we discovered that he had gone out the night before, barefoot, in a very hard rain, and then was the mystery solved. No leeches being at hand, we scarrified the serotum and applied warm fomentations and poultices of chamomile flowers, and administered, internally, eamphor gr. ½ (there being considerable prostration), hyd. sub. nur. gr. 1, pulv. opii. gr. ⅛, omni horā, and applied a blister 12 by 20 over the abdomen, and left him, ex¬pecting to find him dead the next day.

20th. Contrary to my expectations, I found the patient considerably improved: gentle ptyalism had been produced; tongue more moist, less red; pulse softer; skin warm and moist; abdomen less distended, and serotum in a somewhat less inflamed condition. But the patient being very weak, I administered, for that day, brandy, aqua ammonia and laudanum, in stimulant doses.

21st. Very much improved, every symptom of danger having disappeared, with the exception of the inflammatory condition of the serotum and testicles, the latter being so hard as to cause the apprehension that suppuratian would ensue.

22d. Still improving, with exception above mentioned; and so also on the 23d.

24th. Found the patient with his abdomen again tympanitic; tongue red, dry and cracked; pulse eorded; countenance utterly changed, almost of the Hippocratic stamp, and extremities very cold. “He had again been out in the wet.” I ordered friction of dry mustard to the extremities; the blister re-applied, and small
doses of mercury and opium given to keep up the effects of ptyalism. Iodine applied to testicles, the inflammation in which still continuing.

20th. Found him improved, having slept well the previous night. Ordered poultries of corn meal to be applied over the part blistered; continued the calomel and opium at longer intervals, and ordered a little beef-tea and rice gruel.

26th. Much better, with the exception of the testicles. Seeing I could not discuss the abscess which I felt sure had formed, or was about to form, I applied poultries to promote suppuration.

27th. Still improving. 28th. On arriving, I found him enjoying himself with his pipe—complaining of no pain, but saying that he felt perfectly easy. I dressed the wound with common tepid water dressing, applied adhesive straps around the scrotum, and left him. The last I heard of him he was at work, as hearty as ever before.

ARTICLE X.

A Bone lodged in the Rectum, simulating Dysentery.

MESSRS. EDITORS:—About the 20th of November, 1854, while living in Henderson, Texas, I was called by Mr. S., to see a man servant of his, æt. about 50 years. On my arrival, I was informed that the man had been complaining for two days, of sharp, cutting pains in the bowels, and that he had taken a blue pill the first night, and subsequently oleum ricini, which operated on his bowels without relieving the pain. He then alternately administered anodynes and cathartics, with no better success. From the patient, that morning, complaining of so much pain in the rectum, with a frequent disposition to go to stool, his master was fearful he was taking dysentery, as it then prevailed there. Upon examining the patient, I found the skin hot and dry, the pulse considerably accelerated, bowels rather tender; the recti abdominis muscles rigid, and the tongue foul, with red end and edges, though
moist; and great thirst. I was at first about to prescribe as if it were a case of dysentery; but not being satisfied, and as he complained of so much pain about the anus, I determined upon an examination, and having placed him upon his hands and knees, I immediately saw something projecting from the anus, which I discovered to be a bone. I interrogated him as to what he had eaten, in order to arrive at the species of bone, but could get no satisfaction. The projecting part was flat, and the bone seemed, what it afterwards proved to be, of triangular shape—the base being upward and the apex presenting. On seizing it with the forceps and using slight force, he complained loudly and trembled violently. I administered a large anodyne, but still he was no more quiet. The bone seemed to be bearded, and pulling at it only seemed to fasten it tighter. I then determined to use the speculum ani, by dilating the sphincter, so as to disengage the bone; but he would not allow me to introduce the instrument, as it gave great pain. I then administered chloroform by inhalation until he was intoxicated, and readily introduced the speculum on each side of the bone, and by dilating the parts largely, I saw a flat triangular bone, each line of the triangle being about 1½ inches long. It being still engaged with the mucous membrane, I attempted to cut it with a strong pair of scissors, but could not. I then gradually worked it out with difficulty, and some pain; considerable hemorrhage followed for an instant. The patient expressed great relief. I cleansed the parts, then administered full doses of opium for twenty-four hours, not allowing any passages for forty-eight hours. He was up and at work at the end of that time, and had suffered no bad effects when I left there, two months thereafter.

The bone proved to be a fish-bone—the fin of a large cat-fish; the point presenting was thin, while one corner of the articulating surface was engaged in the mucous membrane, rendering it hard to extricate.

Remark.—This case is not a novel one, as foreign bodies are frequently swallowed and lodged at the anus. Yet I have been thus particular in describing the symptoms, to show how easily a physician may be deceived in a case, and that he should be minute in his examination, or his reputation, as well as his patient, may suffer.

Respectfully,

Jas. C. Billingslea, M.D.

Foster's, Tuscaloosa Co., Ala.
Fevers, as we have before intimated, constitute a large proportion, perhaps not less than one-half, of all the ailments to which the negro race is subject in the southern States, requiring medical treatment. This fact appears to be the more remarkable, when we consider, that in their native Africa, the negro race is so much better able to resist the action of febrifuge agencies than the Caucasian, as to be considered almost exclusively qualified to inhabit extensive regions of tropical country, in which the latter race can scarcely manage to exist. Even there, however, we have reason to believe, that the two races differ in their susceptibilities to the influence of the cause of fever, only in degree; and this distinction may be dependent entirely upon the fact, that the native race has been, for many successive generations, inured and habituated to the constant action of such cause upon their systems, until it has ceased to produce its poisonous or morbid influences, or until it is capable of producing them to such an extent only, as to give rise to those more moderate attacks of fever which are easily cured, and which, in a majority of cases, come to a favorable issue under more dietetic treatment. We are the more willing to adopt this view of the case, from the constant illustrations it receives from southern experience; the distinction being scarcely better marked between the negro and the white races, either in Africa or America, than is the same distinction between the native and immigrant white population in New Orleans and the southern States generally.

Whatever may be the proper explanation of these existing distinctions between races, and people of the same race, in this and other countries, it is not more certain that they exist, than that fever prevails and always has prevailed among the slave population of the southern States, to a remarkable extent. No existing condition of life, occupation, exposure, or variety of location, affords our slave population any exemption from this form of disease. It affects indiscriminately both sexes, and all ages, from the new-born infant up to the centenarian, although, it is true, that the two extremes of the catenary are less commonly affected than the intermediate ages, and particularly the latter. As with the white race occupying the same localities, negroes of both sexes are most liable to attacks of fever between the ages of twenty and forty years, embracing the most active and vigorous period of life, as well as that in which men and women of the laboring classes are most exposed to those influences which are supposed to predispose them to attacks of disease. It is probably true of both races alike, that laborers in the open air, tillers of the soil, and all whose occupations subject them to constant out-door exposure, are more obnoxious to attacks of
febrile disease, than those who are engaged in branches of business which may be followed under the shelter of roofs.

Idiopathic fevers affecting negro slaves, are mostly of the periodic form. From thirty to forty years ago, this was, by most southern physicians, considered the exclusive character of such fevers among negroes; and although, latterly, since continued, or typhoid fever, has received so much attention from writers on medicine, many medical men in the South have recognized the existence of this variety of febrile disease among slaves, even in an epidemic form, there is still a respectable number of practitioners, of large experience in negro diseases, who do not acknowledge the occurrence of continued, or typhoid fever, within the range of their own observations; or that any change of note or importance has taken place in the character of the fevers prevailing in the South, either among the white or colored population, during the past quarter or half century. They are still content in the pursuit of the same plan, in designating the different forms and phases of fever, which was almost uniformly practiced, before typhoid fever was spoken of in connection with southern diseases. Besides the epithets having reference to the periods of remission, it has been customary, also, to designate different cases by such terms as indicate the seat of the most serious and obstinate local lesion, as well as the general pathological peculiarity. In this way have been brought into common and popular use, the terms intermittent and remittent, brain, gastric, enteric, pneumonic, bilious, congestive, malignant, etc. The use of these and many other expletives, multiplied and varied, as they doubtless have been by both fancy and fashion, in no way interferes with the opinion more or less prevalent throughout the southern States, that the idiopathic disease, primarily affecting the whole system, is the same at the present time, as that which came so constantly under observation in former years, exhibiting, so far as it may be uninfluenced by treatment, precisely the same symptoms, and exhibiting precisely the same therapeutical indications.

The differences of opinion now existing in reference to changes which are supposed to have taken place in the character of southern fevers and in reference to the introduction and prevalence of a new form of fever, of a continued and adynamic or typhoid type, entertained, as they appear to be, by parties of equal capabilities, must have arisen either from a misapprehension of the true character of fevers formerly prevailing, from the influence of novel plans of treatment, or from the fact, that fevers of the kind alluded to, have actually appeared in certain localities without showing themselves in others. Our own experience, extending over a period of more than one-third of a century, does not lead us to the conclusion that any material changes have taken place during that time in the character of southern fevers, as they affect either the white or the negro race. It has not happened to us to meet with a case of typhoid fever, as described by European authors, or one which ap-
Periodic Fever, in all its various forms, it cannot be denied, has been, since the first introduction of the negro race into the southern States of America, not only the principal kind of fever, but the principal disease, to which the race has been subject. Nor should this be ranked among the peculiarities of the negro, to distinguish it from the white race, during the same period; for this form of disease has attacked the latter even more generally and violently than the former; and it has, in some portions of the southern States, proved exceedingly destructive to the lives of both. Besides the fact that the white race is more liable to attacks of periodic fever everywhere, its greater suffering and mortality in some parts of the South may be accounted for from the fact, that, in making establishments for planting purposes, the owner of slaves has generally chosen for himself and family the most insalubrious locality. At the period when a large portion of the country, now occupied by cotton and sugar plantations, was in the course of its first settlement, an opinion prevailed—and the same opinion still has its advocates—that the greatest security from fever was to be found upon the most elevated and least productive grounds. These are generally situated, at a moderate distance from water-courses, and beyond the immediate neighborhood of the rich alluvial soils to be subdued and cultivated. Hence the early emigrants to Alabama, Mississippi and Louisiana, generally aimed, after establishing their slaves as a matter of necessity, upon the bottom lands, and upon the immediate shores of the rivers and lakes, to fix their own residences, whenever practicable, upon the neighboring bluffs or ridges of highlands, where they believed themselves less exposed to the causes of periodic fever, and where they could better enjoy the cooling breezes of the summer season.

Taught to believe that the constitution and habits of negroes render them less liable to the pernicious effects of malarial exhalations, so called, the natural expectation was, that although their slaves were located in places where these exhalations are supposed especially to abound, this partial immunity of the race would preserve them from greater danger, than they themselves would be subject to in their chosen retreats. Hence there could be no moral wrong, in the adoption of different and separate places of abode. But rigid experience, which frequently puts medical theories to such severe tests, almost daily demonstrated the fact, that,
not only were the white race, under this arrangement of dwelling places, more liable to fevers than the negro, but that the slaves who were required to be in attendance upon the white family, and those who were engaged in the limited cultivation of the less productive soils of the homestead, were more obnoxious to febrile influences, than their more fortunate but less cherished compeers, employed upon the cotton and sugar fields below.

The difference in the susceptibility of the two races to attacks of febrile disease, has been less distinctly marked, whenever, from choice or necessity, they have fixed their homes in close proximity with each other upon the rich alluvial soils. This has been sufficiently observed to induce many southern planters, in emigrating to new and unimproved regions of country, to establish themselves with their negroes upon the low grounds, as a precautionary measure against fever, and, whenever convenient, upon the immediate shores of the rivers, bayous and lakes, which were formerly considered so detrimental to health. There are now large districts of country which have been settled in this way and brought under successful cultivation, the inhabitants of which bear testimony every year, in the excellent health they enjoy, to the truth of this position. In the early settlement of Louisiana, large plantations were opened directly on the shores of the Mississippi, with the broad river in front, and a vast extent of swamp in the rear, upon which both races have lived for many successive generations, without suffering more from fever than communities sometimes suffer in mountainous districts.

Negroes, in being prepared by nature and habit for the enjoyment of health and comfort in hot climates, might be presumed qualified to resist, to some extent, the action of those causes of disease which are dependent, to a partial extent at least, upon the agency of heat in their production. For this, or some other reason not yet discovered, they are known to be much less liable to the more violent and fatal forms of fever in Africa, than white persons, even although the latter may have been inured to the country and climate by long residence. This is true also, in a somewhat less marked degree, in the Southern States. The experience of the rice-growers of Carolina and Georgia afford, perhaps, the best illustrations of this difference in susceptibility in this country, while the cotton and sugar estates of Mississippi and Louisiana present only a little less striking and prominent examples; and this notwithstanding the greater exposure of negroes to the common causes of disease by their occupation and habits of life.

In periodic fever, as in many other diseases prevalent in the South, the prominent peculiarity of the negro ailment consists in its degree of violence, and, by consequence, in the extent to which it endangers the life of the patient, rather than in any peculiar pathological characteristics of the disease. A negro attacked by intermittent or remittent fever, presents to the eye of the physician
all the usual phenomena of those forms of disease, not to be distin-
guished from the symptoms of the same disease, as they appear in
the white subject. The premonitory symptoms are the same, and
the cold stage may be equally insidious, or equally violent, and sub-
ject to the same duration in time. The pulmonary and other con-
gestions are likely to be just as severe and urgent, and the following
reaction just as violent, with the same thirst, febrile heat, and pain,
restlessness, and even delirium, affording indications for the same
antiphlogistic and sedative treatment; yet when these remedial
measures come to be applied, the effects are found to be more de-
cided, and the danger of excessive medication to be proportionably
greater. This is more particularly true of those therapeutical
measures which cause sedation and refrigeration. A negro labor-
ing under high febrile excitement, may derive great benefit from
blood-letting, and under precisely the same conditions which render
this remedy so useful to the white man, but he will require it to be
applied to a less extent to produce the same results; and the same
is true of other sedative remedies. If, from the want of a proper
understanding of this matter, the physician fail to keep it in view,
he will frequently be disappointed in results, even although the
remedies used may be proper and necessary.

Previously to the discovery of quinia, and its introduction into
use in the treatment of periodic fever, the management of negroes
suffering from attacks of this disease was a much more difficult and
serious matter, than at the present day. Cinchona, although very
generally employed as a remedy, was not generally relied upon for
arresting and curing the disease in its very beginning. The abor-
tive treatment was not then common, nor was the bark resorted to
in most cases, particularly of the remittent type, until the prime
vahe had been well evacuated, and the excitement pretty well sub-
dued by antiphlogistic means, with such relief of the local lesions as
would enable the patient to bear the use of tonic remedies without
inconvenience. Few cases recovered so promptly under this plan
of treatment, as to save the patient from the inconvenience of sev-
eral successive paroxysms. Until alleviated, either by treatment or
by the powers of nature, each successive paroxysm became, of course,
more and more severe, causing, among other common effects of
such continuance of the disease, constipation of the bowels, and
rapid accumulations of fecal matter in the intestinal canal. For
the removal of these accumulations, the daily use of cathartic reme-
dies was universally approved and practiced, and considered of
essential importance to the safety of the patient. A class of reme-
dies of such obvious necessity in all cases of the disease, may well
be supposed to have been sometimes used to excess, and thus to
afford grounds for the condemnation which the purgative treatment
has received from certain authors. Various evil consequences
have been attributed to the irritation of the mucous tissues caused
by frequent purgations, under the mistaken idea that the disease
itself was more or less dependent upon some morbid condition of these tissues from the beginning. But our own observations have led us to the conclusion that the more common error in practice then committed, was in purging too little. Hence the fatal injury resulting from the influence of medical authors without experience in the treatment of the disease, about which they undertook to give instructions, and the impropriety of southern physicians taking for their guide, the teachings of men who write of the practice suited to fevers in the hospitals of the metropolitan cities of Europe.

No one feature in connection with the periodic fevers of the South, has been more generally recognized by southern physicians, than the surprising rapidity with which these fecal accumulations take place, and which are often entirely independent of ingesta. The outpouring of excretions into the intestinal canal, is pretty much in proportion to the violence and duration of the disease, or the number of its paroxysms. They are supposed to be excretions from the liver and pancreas, and from the follicular glands of the intestinal canal, and in part, perhaps, exhalations and hemorrhages from capillary vessels, independent of glandular action. The latter might, indeed, be supposed to be the principal sources, because of the fact that, when these accumulations take place most rapidly, the excretions of the system are known to be extensively suspended from the influence of the disease. In case of the eontinuance of the disease for several successive days, unabated by treatment, the quantity of fecal matter discharged from the bowels, even from the operation of mild cathartics, which do no more than cause a moderate increase in peristaltic action, is frequently so large as to strike both the patient and his friends with surprise, and the greater, as they are aware that little or no food has been used. This is often expressed in connection with the inquiry as to the possible source of such quantities of morbid matter.

The purgative treatment, therefore, while the disease could not be promptly arrested by any remedy then known, was essential to the safety of the patient, and could prove injurious only in case of hyperatharsis, or the hydrogogue, and consequently debilitating action of the remedies employed. Unless this indication for cathartics were daily observed, and particularly after every febrile exacerbation, tumefaction of the bowels, attended by tenderness upon pressure, oppression, and restlessness, were certain to supervene, with an enhancement of the febrile symptoms, a partial suspension of healthy secretions, and generally considerable gastric irritability. These symptoms were thought to contra-indicate the use of eihonah, the only anti-periodic then much relied upon, or to render its administration in effective doses impracticable, on account of its ejection from the stomach.

Although the treatment of fever then pursued may have been, with propriety, called active, on account of the amount of medication employed, it was in fact merely expectant, inasmuch as the remedies
employed were intended to do little more than to remove the products of the disease, and to guard against their cumulative influence, and tendency to enhance the gravity of the symptoms; thus enabling the powers of nature to overcome the action of the morbific cause, and gradually restore the patient to a state of health. Cinchona, in substance, was not unfrequently used in union with serpential, or some other of the bitter tonics, and sometimes with the further addition of the saline cathartics, and often with good remedial effects, but never with that confidence in its anti-periodic power, which is now accorded to its proximate principle, quinua. To this treatment was often superadded the use of tartar-emetic, or ipecacuanha, in nauseating doses, with a view to moderate and restrain the febrile excitement, and frequently with the unintentional effect of causing watery dejections of a debilitating character from the bowels.

It will be perceived, from this hasty outline of treatment, that its tendency was to subdue the vital forces and debilitate the system, thus favoring the accession of the adynamic condition, which is always found to present serious obstacles to the rapid recovery of negroes from febrile affections. Their physiological peculiarities, of which we have before spoken, might lead us to expect this. Recoveries from fever were, on this account, frequently slow and imperfect, causing the loss of much valuable time, and often resulting in the establishment of chronic lesions, not only rendering the slave of inferior value for the season, but in many cases laying the foundation for a tedious decline and premature death. An attack of fever of any considerable degree of severity and long duration, could scarcely be expected to run its course to final recovery, after beginning, perhaps, as an intermittent, and passing, for want of remedial efficacy, into the remittent type, and only subsiding after great exhaustion of strength and vital energy, and after the establishment of serious local lesions, without inflicting permanent injury upon the constitution. Tuberculosis, rheumatism, neuralgia, bronchitis, chlorosis, habitual constipation, various forms of indigestion and of uterine disease, were common consequences of the shock thus given to the system.

The introduction of quinua into use in the treatment of periodic fevers worked a great revolution in practice in the South, and particularly in plantation practice among slaves. Its true value to this population was slow to be discovered, and even at the present time is not, perhaps, fully appreciated, although it may be generally acknowledged, that it has added greatly to the productiveness of slave labor, and, by consequence, to the national wealth. The effect upon these, produced by a material abridgement of the time lost by sickness, the preservation of the negro constitution from the injuries inflicted upon it by protracted fevers, and the material prolongation of negro life, is not likely to be over-estimated. It tells largely indeed, upon the amount and value of all the agricultural products,
which are the results of slave labor, in all parts of the civilized world where negro slavery is known.

But the revolution in practice, brought about by the discovery and introduction into use of quinia, like most other revolutions in medicine, was slow and difficult of accomplishment. The new remedy being considered of the precise character of cinchona, it was regarded as a mere substitute for that article, and its equivalent quantity being nearly ascertained, the common practice at first was to give it only in very small and equivalent doses, and always under the same conditions, which had come to be considered, from long experience, indications for the use of bark in substance. Indeed, it was then shrewdly suspected by many, that greater mischief might arise from an untimely and immoderate use of quinia, because the stomach of the patient could not be relied upon to correct the error of the prescriber by rejecting it, when not in a condition for its use, as was supposed to be the case with thegrosser material. Fears were consequently entertained that mischief might often result from its use in too large quantities, and also from its exhibition without due regard to the period of apyrexia. On account of these apprehensions, and the uncertainty which always attends upon new and unproved plans of treatment, experiments were made with extreme caution, and with what are now considered minute doses of this remedy. In plantation practice, this minuteness of dose was, perhaps, favored to some extent by the costliness of quinia in the early period of its use.

It is unnecessary to enter here into a detailed account of those experiments, which led to the system of treatment in vogue at the present time, the object of which is to employ this powerful antiperiodic remedy, as to cut short or obliterate the fever in its very inception, and thus to obviate the establishment of those local lesions, the consequences of fever, which, while they tend to perpetuate the febrile excitement by the influence of local irritation, create a necessity for the use of alterative and contra-stimulant treatment for an indefinite period of time. The vast accumulations of excretions in the intestinal canal, before referred to, as the effect of repetitions of the febrile pasoxysm, being thus prevented, or very much lessened, cathartics become less important and often unnecessary. The consequent abatement in their use has led to the more confident condemnation of the former practice, without due consideration of the change which has taken place in existing indications. Northern and European writers exultingly refer to this change of practice, as evidence that the purgative treatment formerly so much relied upon was unnecessary; but every southern physician must be aware that, if the use of quinia were to be now interdicted, and thus deprive physicians of the means of subduing the disease at the very beginning, the same purgative treatment would have to be resumed.

These remarks are not less applicable, perhaps, to the white than
to the negro race in the South, but there are some peculiarities in
the treatment of the latter which deserve to be noticed. Either
from the higher susceptibility of negroes to the anti-periodic influ-
ence of quinia, or their exemption from the graver forms of periodic
fever, consequent upon their peculiar adaptation to climates where
these fevers most prevail, rendering their attacks less serious and
obstinate, and probably from both causes united, quinia exerts a
curative influence over them, if administered in the first remission
of the disease, more prompt and decided than over white persons
under similar circumstances. Smaller doses are therefore required
for the negro than for the white man, while the sedative influence
of large quantities of quinia upon the negro constitution is more
serious and alarming, sometimes calling for the use of diffusible
stimuli to a considerable extent to relieve it. The most effectual
remedy of this class which we have used is carbonate of ammonia.
This appears indeed, to be an antidote to what may be considered
the toxic effects of quinia upon the negro constitution.

It appears to be generally conceded that negroes are much less
liable to be attacked by that form of febrile disease, called yellow
fever, than white persons. In tropical cities, they are classed with
the exempts; and in New Orleans, Mobile, and other cities in the
South, they are also considered as being liable to the disease only to
a moderate extent, and in its mildest form. As we travel north-
ward, we find the negro race more liable to suffer from this form of
fever, whenever the cause of it exists with sufficient intensity to
produce an epidemic. Two reasons may be given for this peculi-
arity. The first is the fact that negroes, on account of their
adaptation to hot climates, may be placed among the most favored
of climatized persons; and the second, they are, perhaps, as a race,
and certainly from their habits of life, less liable to gastric inflam-
mation than the white race. Local lesions, therefore, which follow
attacks of fever, although, perhaps, quite as likely to occur among
negroes as white people, more commonly in the former, fix them-
selves upon other organs than the stomach, the implication of which
is the distinctive peculiarity of yellow fever. Whenever yellow
fever is prevailing as an epidemic, therefore, it is common to find
negroes attacked with every symptom of the early stage of the
disease, and with a dangerous degree of violence; but when the
gastric lesion might be expected to become developed in a white
person similarly attacked, the stomach of the negro remains intact.

The mulatto, and all the grades of admixture of white and black
blood, are obnoxious to attacks of yellow fever, pretty much in
proportion to the preponderance of white blood, and when persons
of this class do become affected with the gastric lesion, they are
perhaps, even less curable than white persons, on account of their
greater feebleness of constitution. Nevertheless it is no less true,
that every variety of mixed bloods is capable of becoming so fully
climitized, even although they may be natives of northern countries,
as to be classed among those who are exempt from attacks of yellow fever; and this may happen either from a long residence in the South, or from having suffered an attack of the disease.

[Memphis Med. Recorder.]

The Medical Virtues of the Compound Fluid Extract of Tephrosia Virginiana. By B. O. Jones, M. D., Atlanta, Georgia.

Fully impressed with the many improvements and discoveries of the age, both as regards science and literature, as well as every other department of life, it would scarcely be expected that one so humble as myself should bring much that is important to the shrine of medical literature at the present day; yet, when we reflect that age after age—from the days of Æsculapius down to the present time—have each brought in their contributions to swell the volumes of medical literature, and that discovery after discovery, the result of thorough and scientific investigation, have greatly astonished the world, still may we be permitted to hope that the end is not yet, and that the half has not yet been told, and that the "great healing art" shall take her position co-equal with the wonderful advancement of other departments of knowledge, and with healing on her wings give health and comfort to the afflicted of every land and every clime beneath the sun.

Under such impressions as these, I have ventured to introduce to the medical world the result, to some extent, of my experience in the treatment and cure of diseases with the Compound Fluid Extract of Tephrosia Virginiana, or as I have more commonly called it, in honor of the first discoverers "Indian Sarsaparilla;" and if, on farther investigation, it shall meet with the approbation I contemplate it will, I shall feel I have discharged an obligation, long resting upon me, in giving the history and virtues of this plant to the profession.

Tephrosia Virginiana is of the order Leguminosa; sub ord. Papilionacea, a well known indigenous perennial plant, growing abundantly throughout most of the Southern States, putting up early in the spring, growing to the height of eighteen or twenty inches, flowers in June and July, and attains its maturity in August and September. The whole plant is medicinal, and should be taken up by the root in early summer, whilst in full bloom and carefully dried in the shade.

Preparation of Compound Fluid Extract Tephrosia Virginiana:

Tephrosia Virginiana, . . . 8 ounces.
Rumex Acutus . . . . . 2 "
Aqua Font. . . . . . 4 quarts.

Boil down to one quart.

With the above mixture prepare the following for use, which will keep for any length of time without injury:
Comp. Fluid Ext. Teph. Virginiana. . . 4 ounces.
Alcohol Dilutum, (Brandy,) . . . 4 “
Saccharum Album. . . . . . . . . . . . . . . 2 “

Mix and digest for several days, and then strain through muslin and it is ready for use. Dose for an adult from one-half to one fluid ounce, repeated as often as symptoms demand, with a less dose in proportion for children. It should be given at all times with a view to its tonic and stimulant effect, and continued for some time. I have seen results from this medicine, or from other unknown causes, nature perhaps, truly astonishing; so much so, that I should forbear to mention them, preferring that others should test its virtues also, and decide accordingly, receiving only the genuine coin and admitting only that which has been unmask- ed, weighed in the balance, and amply sustained by tangible data.

I will venture, however, to mention in this connection a fact well known to every practitioner of medicine, that there is a time in the treatment of many diseases, and especially Typhoid fever, when there can be little if any use for active medicine—the patient not yet sufficiently recovered to dispense with medicine altogether —when one imprudent act of the physician will prove almost fatal; still it is of the greatest importance that something should be done to perfect the cure already commenced. Some mild stimu-lating tonic, having a slight action on the bowels and the secre-tive organs generally, would seem to meet the indication. This, we feel authorized to say, will be accomplished as well by the use of this medicine as any other that ever came to our knowledge.

It may not be improper, indeed I think it necessary, to men-tion a few important cases treated mainly with this medicine:

CASE I.—Fanny, a colored woman, the property of N. M., of Coweta County, Georgia, had been sick with general dropsy for some eight or ten months, occasioned, in all probability, by Inter-mittent fever, attended with enlargement of the liver and spleen, together with obstruction of the catamenial discharge. Had been treated with the usual remedies in such cases, to wit: Cathartics, Diuretics, and Tonics, to which may be added mercurials, iodine, &c.; indeed all the common medicines recommended by the books of the day for the cure of those diseases, with but little if any effect. The disease continued gradually, to get worse, until it assumed strong indications of a fatal result, and thus remained for several days, when I became satisfied if something more was not done, and that speedily, the patient would surely die. I then men- tioned the fact to the owner of the woman and requested consultation, to which he seemed unwilling, saying at the time he had no hopes of her recovery now or for some time past, and that he thought it best to let the woman die in peace; besides he was unable to de-fray the expenses of such policy, and that he preferred my contin-uing in the case to the end, and if I could see at any time that there was anything possible to be done, to fill the indication at
once. I mentioned that the Indians used the Tephrosia Virginiana in the treatment of dropsy with seeming good effect, and proposed its use in this case, to which he readily assented. The patient was immediately put under the full influence of this medicine, and kept so for several days, when the symptoms became greatly improved, and in the course of five or six weeks was almost entirely cured.

CASE 2.—A. M., aged 25 to 30 years, of Fayette County, Georgia, Typhoid fever.

Symptoms.—Pain and stiffness in the back of the neck, extending down the left hypocondriac and lumbar regions. Pulse 120 to 130. Skin of head and upper extremities hot and dry; inferior extremities cold and shriveled; a dark brown incrustation upon the tongue, and the teeth and gums covered with sordes. Had been confined some eight or ten days; had taken no active medicine, nor could be prevailed upon to do so; however was induced by my suggestion to use the Compound Fluid Extract of Tephrosia Virginiana, which he continued with a few other mild remedies, such as friction and sinapisms to the spine and extremities, until a cure was effected.

CASE 3.—Philip, a black boy, aged some 18 or 20 years, had rheumatism of several years' standing. Left inferior extremity useless, stiff and emaciated. General health impaired; subject to occasional acute attacks in other parts of the system, attended with much pain, swelling and fever. Put him under treatment with use of the Compound Fluid Extract of Tephrosia Virginiana, and continued with little if any other medicine for some two or three months, when I had the satisfaction of finding the disease entirely removed, and he has been well, with the exception of a slight stiffness in the hip joint, ever since.

Thus will be seen, I hope, sufficient to cause at least a farther investigation, and to the ordeal of critical analysis and experiment these observations are submitted, with a desire upon my part that the truth should be evolved, whether the virtues of the article, which I have brought to the notice of the profession, shall rise or fall.—[Atlanta Med. and Surg. Journal.

Curative Treatment of Prolapsus Uteri, by the local application of Tannin. By CHARLES A. BUDD, M. D., Fellow of the New York Academy of Medicine.

The many and varied forms of uterine disease resulting from an injudicious use of pessaries, as well as the irreparable damage done by means of cauteries, either actual or potential, or by extirpation of portions of mucous membranes, which have for their object the diminution of the capacity of the vagina with a view of euring prolapsus, have led me to add my testimony in favor of a practice first recommended by Prof. B. F. Barker, of this city. I allude to
the application of lint soaked in a saturated solution of tannin; and if the perusal of the following cases, selected from a variety which have come under my observation, will induce a trial of the treatment alluded to, I feel confident that in all cases to which it is applicable, the result will be equally gratifying. There are, to be sure, certain abnormal conditions in which it is not applicable, as, for example, a very straight sacrum, or a loss of substance in the perineum due to laceration or other causes; or if there be any pathological condition of the cervix, it must be removed by appropriate treatment; and then, if the prolapsus continue, the utility, I may say infallibility, of the method alluded to, becomes apparent. But in simple cases of prolapsus, whether incipient, partial, or complete, depending upon a mere relaxation of the uterine supports, I have never known it to fail where the treatment has been faithfully persevered in. The manner recommended by Prof. B., and the one I have followed, in its principal details, is as follows:—From a double thickness of lint a triangular portion is cut out, of a sufficient size to fill the capacity of the vagina when rolled up so as to form a cone, near the apex of which is attached a piece of string to facilitate withdrawal. The patient being placed upon her back, with the hips slightly elevated, the uterus is replaced in situ, and the lint, soaked in a saturated solution of tannin, is applied with its apex downward and its base immediately in contact with the os tincæ. This is repeated once in twenty-four hours, for a period of time in accordance with the extent of the displacement. I have usually found a daily application for a period of about a month, to be sufficient to perfect a cure. During this time, and subsequently, constipation must be rigidly guarded against, and the state of the general health attended to. The vagina soon begins to acquire its wonted tonicity and contractility, and the lint is consequently obliged gradually to be lessened in quantity; the strain being taken off the round ligaments, also allows them to return to their normal condition. The following cases exemplify the admirable effects of this plan of treatment.

Case I.—Mrs. G., age 24 years, the mother of two children, the youngest fifteen months of age, had always enjoyed good health until the birth of her last child. The placenta in this last accouchement (I having attended her in both) was retained over three hours, in consequence of irregular uterine contractions. She has been complaining ever since of pain in the lumbo-sacral region, with bearing-down pains in the hypogastrium; great constipation, with vesical and rectal tenesmus; and a sensation of faintness after an evacuation, leucorrhœa, &c., &c. Upon examination, I found the cervix protruding at the vulva, extensive ulceration extending into the canal of the cervix, inflammation of the posterior wall of the uterus, and enlargement of the organ itself, its long diameter measuring 4½ inches. After three months' treatment, these conditions, save the prolapsus, were all removed, the applications to the
cervix having been made weekly, and without the aid of a speculum, and the uterus at this time measuring less than three inches in its long axis. The treatment consisting of the lint and tannin, was soon after commenced; and in about three weeks' time she was enabled to resume her ordinary duties. She is now four months pregnant with her third child, all treatment having been suspended about six months ago.

Case II.—Mrs. S., æt. 20 years, the mother of one child aged two years, applied to me in July, 1854, suffering from all the symptoms of uterine prolapsus. She had aborted with a three months' fœtus about a month previous, but had been complaining for two years before. An examination revealed incipient prolapsus, the cervix lying on the floor of the perineum, and slight epithelial abrasion of its mucous surface. Two applications of nitrate of silver removed this; and the use, for ten days, of the lint and tannin, effected a perfect cure.

Case III.—Mrs. G., æt. 54 years; a widow, the mother of five children, the youngest sixteen years of age. Had ceased menstruating about ten years previous. She stated to me that upon using the slightest exertion, such as lifting or straining at defecation, her womb would entirely protrude from the vulva. She had used a variety of abdominal supporters, and had attempted on several occasions to wear pessaries of different kinds, and at that time, was wearing constantly a T. bandage. Upon examination I found the uterus just within the vulva; and, requesting her to lift a chair, the whole organ was protruded, dragging with it the posterior wall of the bladder; it was perfectly healthy in appearance, though somewhat atrophied. I commenced the treatment with the lint and tannin, interdicting active exercise, and in six weeks ceased making any applications. She gradually resumed her ordinary duties, and is now (some two years since) perfectly recovered, and is considered a very active old lady. She has not had the slightest disposition to a return of the displacement, and enjoys excellent health.

I have here given an example of the three different degrees of prolapsus,—incipient, partial, and complete,—illustrating the curability of this treatment in each. I could, if it were desirable, cite many others which have been under my observation, and which have resulted, without a single exception, in a perfect and complete restoration.—[N. Y. Med. Times.

Experiments with several pretended Substitutes for Cinchona in the Military Hospitals at Rome. By Dr. Felix Jacquot.

1. Arsenic.

This paper is a summary of a memoir addressed to the Conseil de Santé des Armées, on the employment of arsenic in the treatment
of intermittent fevers in general, and of those of Rome in particular, and which was based upon 282 observations.

1. Mode of Experimenting. — In order to establish the efficacy of arsenic as a febrifuge, its administration should be limited to those cases which have resisted treatment without the use of quinine. The author of the paper before us has not strictly followed this course, since, giving the arsenic at the outset in the majority of the cases, he had no means of judging whether the fever was about to proceed steadily with its paroxysms, or whether, on the other hand, it had a tendency to spontaneous disappearance. But as the sulphate of quinine was administered in the same way, it was at least in a position to establish the comparative efficacy of the two medicines. His researches, too, permit him to consider separately the treatment with arsenic alone, and the complex treatment by this remedy, emetics, &c. Arsenic alone cut short the fever only in 8:33 per cent. of the cases, but the complex treatment in 16:66. But while the efficacy of the arsenic is doubled by the conjoined use of emetics, the febrifuge powers of the sulphate of quinine are so great, that those of emetics simultaneously employed are lost, or absorbed in them; thus, the percentage of fevers cut short by sulphate of quinine without emetics is 49:52, and by sulphate of quinine with emetics 50:47, as calculated on 210 fevers.

2. Formula, Dose, Duration of Use of the Arsenic. — The formula used was the following: — Arsenious acid, 1 gramme; distilled water, 1 kilogramme. The arsenic is boiled with more than this quantity of water till dissolved, and the latter reduced to the prescribed quantity, some soda being added should the solution be imperfect. The dose of solution was administered in canella-wine. The author could derive nothing but confused ideas of the proper dose from writers on the subject, nor yet of the rapidity of its action.

3. General Accidents, Tolerance. — Most subjects bear without general accidents, three centigrammes at the outset, though the author has seen one centigramme produce serious local and general accidents; yet, on the other hand, the tolerance has persisted sometimes in spite of long continued large doses. Out of 72 cases treated by arsenic, he has only noted general accidents six times, never fatal, and only once a source of anxiety. The local and general tolerances are quite independent of each other. The author considers the action of arsenic to be sedative, hypothenic. In one of his subjects the pulse fell to fifty. General loss of strength, lassitude particularly affecting the legs and loins, have appeared to him the earliest phenomena of poisoning by moderate doses of arsenic; and while he thus differs from those who class it among the tonics, he asserts that it has no tonic operation, even upon subjects suffering under marsh cachexia.

4. Local Accidents, Tolerance. — Out of the 72 cases treated by arsenic, 24 or 25 presented gastro-intestinal accidents. The first
dose of one centigramme may cause vomiting and epigastric pain; but, on the other hand, he has seen six centigrammes given by the mouth tolerated; and in others he has seen the arsenic continued for a month without the stomach revolting against it. Although the conditions favourable to the tolerance are not well known, yet he can mention the smallness of the dose, its ingestion in divided portions, and the quantity, and perhaps nature, of the vehicle. The local accidents are nausea, vomiting, diarrhoea, malaise, and sometimes pinchings at the epigastrium, and an insurmountable disgust at the medicine. Either general or local accidents followed in 31 out of his 72 cases, or in 43 per cent.

5. Autopsies of Individuals treated with Arsenic.—In three subjects examined, nothing was discovered which could be imputed to the employment of the arsenic, either in the heart, or in any other part of the body.

6. Degree of Efficacy of the Arsenic, Comparison with Sulphate of Quinine, &c.—The cases on which M. Jacquot founds his comparison, are those which had not received any previous treatment calculated to interfere with the accuracy of his experiments. He thus tabulates his results:

<table>
<thead>
<tr>
<th>Sulphate of quinine</th>
<th>Arsenic</th>
</tr>
</thead>
<tbody>
<tr>
<td>50-00</td>
<td>13-88</td>
</tr>
<tr>
<td>25-71</td>
<td>22-22</td>
</tr>
<tr>
<td>7-61</td>
<td>12-50</td>
</tr>
<tr>
<td>5-23</td>
<td>34-72</td>
</tr>
<tr>
<td>11-42</td>
<td>16-66</td>
</tr>
</tbody>
</table>

The arsenic, with or without the emetic, has cut short the fever 13-88 times per cent.; the sulphate of quinine, with or without emetics, 50- times per cent.; that is to say, the arsenic has been efficacious as 1, the sulphate of quinine three times and a fraction. The arsenic with emetics cut short the fever 16-66 times per cent.; the sulphate of quinine without an emetic, 50-47 times per cent.; that is to say, the arsenic has been efficacious as 1, the sulphate of quinine as 3 and a fraction. The arsenic without emetic has cut short the fever 8-33 times per cent.; the sulphate of quinine without emetic, 49-52 times per cent.; the proportion being arsenic as 1, to sulphate of quinine as 5 and a fraction. Lastly, in comparing the cases the most favourable to the arsenic—viz., those in which it was administered in large doses, three to ten centigrammes, accompanied by emetics, and a diet whose only limit was the appetite of the patient, with the cases least favourable to the quinine, we arrive at the following results:

Fevers cut short by the arsenic, 96-8 per cent.

Fevers cut short by quinine, 49-52
As respects the cases not cut short, it will be perceived, on referring to the first four figures of the two vertical columns of the table, that in the instance of the fever treated with quinine, the numbers are smaller and smaller according as we examine the categories of cases more and more refractory, whilst the contrary is noticed in the instance of the arsenical treatment. The contrast is perfect.

In about 35 cases it was possible to compare the effects of the quinine and arsenic, the two medicines having been administered in succession to the same patient, either for the same fever, or in two separate attacks. In a sixth of the cases, the arsenical and quinine treatment were of little efficacy; in another sixth, the two medications were followed by some success; in the four other sixths, the sulphate of quinine showed itself the more active, or the only active remedy of the two; and one observation furnished a very marked instance of fever resisting the sulphate of quinine, and cured by arsenic. In short, the author concludes that we see more fevers which resist arsenic yielding to quinine, than we do fevers refractory to quinine disappearing under arsenical treatment. He believes, also, that he has established the fact of the greater activity of the sulphate of quinine in the cases which have received no previous arsenical treatment (54 per ct. cut short), than in those first submitted to the action of arsenic (40 per ct. only cut short).

The general conclusion he draws is, that sulphate of quinine is not replaceable by arsenic; and especially is this true in respect to the fevers of hot climates, where it is necessary to apportion the dose to the intensity of the malady; under the latter circumstances we are immediately arrested in the arsenical treatment by the fear of poisoning. In those countries where from one paroxysm to another, the pyrexia may become more severe, remittent, and pernicious, arsenic should not be employed during the endemicoepidemic season.

Confirmation of results by other observers.—After mentioning MM. Mayer, Cordier, Pasquier, Armand, and Gougé, as arriving at similar conclusions to his own, he states that in the Pontine marshes, Dr. Minzi, physician to the central hospital of that country, has experimented with arsenic in more than 400 cases, giving it to the extent of three centigrammes a day, and at last abandoning it from want of success. M. Salvagnoli Marchetti also, out of 16 cases, found 15 resisting arsenic.

Arsenic in invertebrate Fevers, and in Marsh Cachexia.—The observations of M. Jacquot do not encourage recourse to arsenic in invertebrate fevers; and M. Cordier also concludes from his experience in Algeria, that it is the more recent and slighter cases which yield most readily to arsenic. In the palustrial cachexia he thinks that arsenic may perhaps be used as an alterative, but that it is incapable of replacing iron and other tonics, which it is necessary to conjoin with it.
Relapses.—In preventing relapse, arsenic is inferior to sulphate of quinine. Out of the 72 cases treated with arsenic, the relapses were 22 or 30 per cent.—certainly a large proportion. They were less frequent in the cases treated with quinine. The relapses occurred even during the period of administration of the arsenic, which was continued after the cessation of the fever. This was not observed in the instances of the quinine treatment.

Arsenic in the Ingravescent and Remittent Fevers.—In 5 cases it was observed that, in spite of and during the employment of arsenic, the simple fever became aggravated, remittent, sub-continued, and pernicious—*a fortiori*; then, this medicine would have no action upon a fever already of this character.

Conclusions.—Arsenic is not for a moment to be regarded as a substitute for sulphate of quinine. It will probably find a limited place in the treatment of indigenous intermittent fevers, but it has absolutely no pretensions against the recent endemo-epidemic fevers of hot countries. We are scarcely authorized to employ it except in the fevers which resist all the preparations of bark. Uncertainty and contradiction reign over almost all points relative to arsenic. It is a medicine which we cannot yet handle with the double certainty of obtaining the effect desired, and of avoiding the dangers connected with its administration.

2. Parsley Oil (Apioi), or Juice of Parsley Seed; Colophane treated by Nitric Acid.

The author condemns the colophane almost absolutely. Of the efficacy of the parsley oil he expresses great doubt. The single case out of six trials in which the fever appeared cut short by it, might have been an instance of spontaneous recovery, since 7 out of 19 cases submitted to expectation terminated in this way.

3. Hydrochlorate of Ammonia.

The doses employed were eight to twelve grammes in the day, and the experiments were made upon 21 subjects. The following table represents the results:

<table>
<thead>
<tr>
<th>Description</th>
<th>Result</th>
</tr>
</thead>
<tbody>
<tr>
<td>Fevers cut short</td>
<td>6, or 28 per cent.</td>
</tr>
<tr>
<td>Fevers which presented one paroxysm</td>
<td>1, or 4 per cent.</td>
</tr>
<tr>
<td>Fevers which presented two paroxysms</td>
<td>1, or 4 per cent.</td>
</tr>
<tr>
<td>Fevers which presented three or more paroxysms in spite of the medicine</td>
<td>11, or 52 per cent.</td>
</tr>
<tr>
<td>Fevers which cannot be placed in the above categories, but which were not cut short</td>
<td>2, or 8 per cent.</td>
</tr>
</tbody>
</table>

The first number of 28 per cent. of fevers cut short would show a powerful febrifuge operation, were it not for the fact that more than a third of the cases submitted to expectation recover spontaneously. The remaining numbers, too, are little in favor of the efficacy of the medicine, since more than half were uninfluenced by it. The observations of M. Jaquot have not only established that the greater part of the fevers are completely refractory to this
Notes on the Application of Statistics, to Questions in Medical Science, particularly as to the External Causes of Diseases. By W. P. Alison, M.D., Edin., D.C.L. Oxon., Emeritus Professor of Practice of Medicine, Edinburgh.*

It has so frequently and so plausibly been urged, against all the inquiries and studies which are termed Statistics, that the form of such reasoning may be applied to the support of almost any proposition, that it becomes an object of very considerable importance,—in the view of any one who is truly convinced of the importance and frequent practical application of such inquiries,—to point out the circumstances of any question, or departments of any science, in which this kind of information is truly requisite, and the conditions under which it may be trusted. This is especially true of the science of Medicine, because there is one great department of that science,—that which we term Etiology, or the doctrine of the external causes of diseases, in which our knowledge is acquired almost entirely in this way. It is in very few cases only, that our knowledge of the essential or intimate nature, either of diseases or of the powers in Nature which excite them, enables us to form any anticipation of the effects of those powers; and it is simply by empirical observation,—facts observed and recorded, and the frequency of their recurrence noted, although not explained, i.e., it is by the mere force of numbers, or by statistics, whether stated exactly in that form or not,—that our information on that subject, and practical rules for the prevention or treatment of diseases, founded on that formation, are acquired.

I have elsewhere stated,† what seems to me to be sufficient reasons, for the application of Statistics to inquiries of this kind being more frequently and obviously required, than in any of those which are made, either into the nature of diseased actions (or of vital actions in general), or the power and mode of action of remedies. The questions which we propose to ourselves in Etiology are truly simpler than either in Pathology or Therapeutics,—they in-

* Read to the Statistical Section of the British Association at Glasgow, Sept. 1855, (by Sir Archibald Alison.)
† British and Foreign Review, etc., January and April, 1854.
volve little or no exercise of judgment, simply the observing and recording of facts, according to directions which are made known, and the sufficiency of which can easily, at any subsequent time, be estimated;—the sources of fallacy connected with them are less numerous; and, more especially, we can usually remark this, as to the observations by which we can fix on those antecedents of a disease, to which we ascribe the power of producing it,—that the positive observation, of the alleged effect following the application of the cause, is supported by a large body of negative observations, often not stated in words, but truly essential to the validity of the inference; and, therefore, this class of observations is often more truly and efficiently statistical,—the number of individual cases really contributing to the result obtained, is very much greater, and the evidence afforded more decisive, than it can easily be made in the other departments of medical inquiries; sometimes, therefore, greater than those who have accustomed themselves only to physiological or pathological inquiries, or to watching the effects of remedies, can easily perceive.

The importance of this observation will not be denied, when we remember that this is the department of Medical science which is truly of the greatest practical importance. The knowledge of the external causes of diseases, is that which leads most directly to their prevention; and to the preservation of those lives especially, from which the greatest amount of labor of all kinds may be obtained, and which are, therefore, generally regarded as most valuable to a state.

In the course of the present century, improvements have been made in Medicine, which will bear comparison in their practically beneficial tendency, with those which have made this age and this quarter of the globe so illustrious, as regards the applications of any other Sciences to practical purposes. These have been almost exclusively in this department of medicine, and may be truly said to rest, as yet, almost exclusively on Statistics; anticipating, probably by several ages, any information within the power of the human race, as to the intimate nature of the phenomena which are thus recorded. It is simply by the force of numbers, attesting the simple fact of a disease, easily recognised, showing itself in many persons within narrow limits of time and space, and there only, that we have learnt how the poison of Small-pox may be diffused; and that it may be disarmed of its power over the human body, by being first taken into, and modified by its passage through, the body of the cow, and then applied, in almost infinitesimally small quantity, to the human body itself, and there exciting a certain specific inflammatory process, absolutely devoid of danger, and incapable of communication through the medium of the air, and, what is still more inconsistent with the knowledge we have of other changes in nature; that this modifying and preserving effect on the human body, produced in infancy, continues equally per-
fect, in a great majority of cases, after 60, 70, or 80 years, if life shall continue so long,—i.e., after we are certain that the body on which it has been produced, has been repeatedly worn down and built up again, so that the poison introduced into the structure in advanced life, can no more be said to come in contact with living matter which has gone through the process of vaccination in infancy, than, according to the paradox of an ancient logician, a man can be said to have bathed twice in the same water, because he has bathed twice in the same river.

So also it has been simply empirical observation, and, therefore, by reference to statistics, that we have acquired within these few years, information touching the extension of another epidemic, less frequent, indeed, but attended with peculiar interest, and often with extreme fatality, the Puerperal fever, which enables us, with almost absolute certainty, to predict that its propagation, after the manner of an epidemic, may hereafter always be prevented. For, I believe, I may safely assert, that the statistical observations of Dr. Semmelweiss, in the great Lying-in Hospital at Vienna, where upwards of 6000 births take place in a year,—being in exact accordance with what has been seen in this country, have unequivocally shown:

1. That this disease is, in fact, a case of the Diffuse inflammation or Erysipelas, attended with the same peculiarities in the extension of the inflammation, and in the nature of the effusion, and the same variation as to the nature of the accompanying fever in different epidemics.

2. That one immediate exciting cause of this form of inflammation, which may always be suspected when it prevails epidemically, is the cadaveric poison,—often evolved, during the decomposition of the human body, from whatever cause death may have taken place; but during one stage of that decomposition only, viz., that early stage during which the post-mortem inspections for the investigation of the cause of death are most frequently made.

3. That when this form of inflammation assumes the form of epidemic puerperal fever, the mode of its transmission from one patient to another, is by accoucheurs or nurses, themselves in good health, but to whose persons or clothes minute portions either of the effluvia from others already affected, or of this cadaveric poison, have become attached; and,

4. That when the obvious precautions are taken, to prevent any persons to whom, in either of these ways, the poison can have thus adhered, from acting as accoucheurs or nurses until effectually purified, no "epidemic extension" of puerperal fever is seen.

It is seldom, no doubt, that this last proposition can be submitted to a searching scrutiny, as in the year 1846, at the Vienna Hospital, under Dr. Semmelweiss, when the number of deaths after these precautions were adopted, was diminished by no less than 400 in that hospital alone, and in a single year; but when it is
added, that each of the propositions above stated is quite in accordance with observations made and recorded (although the bond of connection among them had unfortunately not been duly observed) in this country, whenever puerperal fever has been epidemic, whether on a large or small scale,—so that it would be easy to collect statistical evidence of the same kind from every such epidemic, wherever any record of the facts has been left, to establish each of these propositions,—the force of this statistical evidence becomes such as to justify the sanguine anticipation above announced.

Our information as to the mode of extension of the malignant Cholera, is, unfortunately, as yet less certain or precise; yet I think we may say it is so far advanced, that we may entertain a confident hope of its being very soon such as to disarm any epidemic cholera, in this climate, at least, of all its terrors.

I allude to that subject here, chiefly because I think it affords an example worthy of notice, of what I have already stated, as to the frequent misapprehension of the strength of evidence, simply empirical, or founded on statistics, which may often be obtained in inquiries of this kind.

I mentioned formerly in a paper on this subject, which I had the honour of laying before this Association at Birmingham, a single case which I saw in the year 1832, and which I have always maintained to have been sufficient, for any one who duly attended to the statistical evidence it afforded, to establish the proposition, that the disease is capable of propagation in this climate, by the intercourse of the sick with the healthy, without pledging us to any opinion as to the mode of communication, or as to the existence of other modes. Now that it is generally admitted, that the assertion then made was correct, the question for discussion now being (as stated by the Editor of the British and Foreign Medical Review) "not whether cholera is contagious or not, but how often it spreads by the agency of human bodies (i.e. by contagion), and how often without that agency," (Journal for January, 1854, p. 298.)

I think myself justified in drawing attention to the grounds on which it was made; and which, I still think, amply sufficient to establish that mode of communication, without, of course, excluding others.

The reason of this strong expression of opinion was, that the case furnished a remarkable example of the evidence which, according to what was formerly stated, a single positive fact may afford,—quite of the nature of the instantia crucis, when supported by a large body of negative observations. The anxious expectation of the disease in the town, in 1832, the careful division of the town into districts, and appointing of stations and medical men to each, and the number, zeal, and intelligence of these observers, were known to be such as to justify our saying, that this was the very first case of the disease which ever originated in Edinburgh.
or Leith, in a person who had not left the town; and that for a
time it was an isolated case, no other appearing for ten days, in a
population of above 140,000 persons, many of whom were in cir-
stances, in all other respects at least, equally favorable to the
appearance of Cholera, as this woman was;—as the subsequent
appearance of the disease in many of them, during nearly a year
that the disease afterwards existed in Edinburgh, proved. If the
poison producing this "nova pestis," had no contagious property,
this woman was not more exposed to it than any other of this
large body of people; but if it had that property—in whatever
other way it might be communicated,—she was undeniably and
peculiarly exposed, as she was engaged in nursing her son, in a
close confined room, who was ill of the symptoms of cholera, du-
dring the whole of the day preceding that in which she sickened
and died; and he had passed the next preceding night in a house
in Musselburgh, where patients in the malignant disease then were,
and had been for weeks previously. Here, then, was a fair instan-
tia crucis, to determine whether the transmission of the disease,
from place to place, known to be at that time a frequent event,
was or was not dependent on intercourse of healthy persons, with
those previously sick of it; and the affection of this one person,
who had that intercourse, contrasted with the non-appearance of
the disease in 140,000 was, as I maintain, decisive evidence.

Many cases, equally decisive, have since occurred, of which I
have been informed, on the first appearance of a disease previously
unknown in a town or district, carefully observed;—e.g., at Ar-
broath in 1853, where there was clear proof that the two first per-
sons of 15,000 inhabitants of that town, who took Cholera, "had
just returned from Dundee, where they had visited persons ill of
cholera;" their affection, therefore, proves nothing as to the effica-
cy of intercourse with the sick, in exciting the disease, rather than
merely visiting a particular locality;—but the next six who took
it in Arbroath,—the first six inhabitants of that town who took
cholera without leaving the town, had repeated and close inter-
course with those already affected before they took it; and the
inference as to the contagious property is drawn, not from their
taking the disease, nor even from their taking it at that time, and
in rapid succession, but from their being the only inhabitants of
Arbroath who took it, during the first week of its existence in that
town;—i.e., the positive evidence of the six who were known to
be exposed to that cause, is supported by the negative evidence of
the fifteen thousand who could not be shown to be, and the immense
majority of whom certainly were not, so exposed.

I have great hopes that we shall soon have statistical evidence
to establish a proposition which no other evidence known to us
could justify our admitting, but which, if established, would not
only reconcile most of the conflicting statements on this subject,
but serve as a guide to almost complete security from any epidem-
ics of this disease in future; viz., that the poison of cholera, like other known animal poisons, is developed during the decomposition of the animal matter, the appearance of which is most characteristic of the disease,—that which constitutes the "rice-water stools,"—but only in a particular stage of that decomposition, not immediately after its formation in, or discharge from, the body; and again, not after the decomposition has gone to a certain length. All this can be proved only by Statistics, but is quite susceptible of proof in that way. The last proposition above stated—that no poisonous property is attached to any part of a body dead of cholera, after a certain stage of decomposition has been passed, seems nearly ascertained from the number of instances in which dissecting-rooms have been supplied with bodies of persons dead of cholera, for many weeks together, without any of the students attending them being affected. That the peculiar matter of the cholera evacuations, during its decomposition, perhaps especially in dry air, has a peculiarly poisonous quality, was, I believe, first suspected by Liebig, and partly by the analogy of the poison already mentioned, causing erysipelasous inflammation and puerperal fever,—and also of the sausage poison, repeatedly observed on a large scale in Germany, which is developed during a certain stage of the decomposition of the animal matter of those sausages, and disappears when their putrefaction has so far advanced. That the cholera can be communicated to animals by inoculation with this matter, of the peculiar rice-water stools, has been sufficiently proved by the experiments of Dr. Lindsay in this country; and if a few more experiments shall give results similar to those of M. Thiersch, at Munich, I think we may assert that the proposition above stated, is statistically proved. "Dr. Thiersch collected the intestinal contents, or the evacuations, of cholera patients, and let them decompose under the influence of air and heat. From day to day he dipped into this matter pieces of filtering paper, which he dried, for subsequent experiments on white mice. Two of these animals at a time were exposed to infection for four days, by having a square inch of the filtering paper, thus prepared, moistened with water and mixed with their food. Each mouse took thus 1-2000th gr. daily. The results were as follows:—The preparations from the matter during the first day of decomposition were innocuous. To this succeeded a period of from six to nine days, during which decomposition went on, and preparations from the matter in this second period of decomposition, caused disease in 30 out of 34 animals, and death in 12 of the 34. The symptoms were peculiar and characteristic. The hair fell off, the ears dropped, there was languor, then discharge from the bowels, first of white, then watery matter, the urine lost its smell, then was suppressed, the appetite became depraved, so that the animals would fill their stomachs with wool; there was no apparent sickness, but such tonic muscular contractions that they seemed dead some time be-
fore death. On dissection, accumulation of blood in the vessels of the small intestines was invariably found, their contents watery, with abundant epithelial flakes (just similar to those found in persons dead of cholera); the cortical substance of the kidneys passing into fatty degeneration, the bladder empty, the blood and the contents of the intestines answering to the test of ready mixture with amylce, as in the cholera of man,—which will not appear in the healthy animal. To this a third period of decomposition of the matter under trial succeeded, in which these poisonous effects were very slight, or not observed at all."—(See Med. Times and Gazette, Nov. 25, 1854.)

We have already a statement by Dr. Budd, of statistical observations made on villages in England, where the entrance of Cholera appeared to be prevented by such expedients as these observations immediately suggest, for receiving the rice-water evacuations of cholera patients on linen or cotton, and burying or burning, or otherwise effectually destroying their substance, during that period, thus indicated, after they have been passed, and before they have entered on the morbid decomposition; and we have sufficient statistical evidence of the importance of another measure,—which was first adopted, I believe, at Edinburgh, in 1832, and has since been recommended by the Board of Health in London, and adopted in different places, although not so generally or satisfactorily as could be wished,—founded on the merely empirical statements I have made as to the communication of the disease from the sick to the healthy, and its apparent adhesion to particular, often very limited, localities, viz., the establishment of houses of refuge for the reception of all inhabitants of houses or rooms which might become infested with cholera in any town; not themselves affected, nor required for the care of the first cases that might occur. Here such persons might be lodged in pure air, regularly fed, preserved from cold and from other (frequently concurrent) exciting causes of the disease, and treated with due attention to cleanliness, immediately on any symptoms of cholera showing themselves. The London Board of Health report, that they had information of 1691 persons taken into these houses of refuge, from rooms where there were patients in cholera, and of these, only 33 became affected with cholera, and only 10 died. In cases of which I was myself informed, in Edinburgh, at Glasgow, and at Oxford, during different epidemics, 1010 persons were admitted from sick rooms into such houses of refuge, of whom 40 took the disease, and 15 died; whereas the experience of Dr. Hamilton of Falkirk, of 251 cases of cholera appearing in 86 houses, where no such means of separation existed, gives only a fair idea of the extent to which successions of cases will often be observed, in confined air and dirty districts, possessing no such resource.

I am happy to say, that so far back as November last, having written to Dr. A. Smith, at the head of the medical department of
the army, on the subject of the decomposition of the rice-water stools, as the probable cause of the propagation of cholera, and of districts becoming tainted with the poison of that disease,—I was informed by him that he had directed the attention of the medical officers in the Crimea to the facts now stated, so that if the disease shall appear in a malignant form in that army, we may hope for at least accurate and truly statistical information as to the reality of that opinion.

Again, as to the Yellow fever, so frequently becoming epidemic in the hot climates, although we cannot boast of having acquired information either as to the nature of its cause, the essential character of the morbid change it produces, or the power of any remedy over it,—yet by simply empirical observations, i. e., by Statistics, we have information to the following effect, as stated in reports in Germany and France, on inquiries conducted by order of those governments, on a large scale, and considered by committees containing the names of Humboldt and Dupuytren, that it is a disease endemic, almost exclusively, "in districts nearly on the level of the sea, never appearing beyond 48° of north latitude, nor without a previous temperature of 72°,—only in certain circumstances propagated by contagion," but when epidemic, always confined strictly to certain localities; so that the practical rule of immediately evacuating, i. e., removing all the inhabitants of places where it is declared to exist, and has formerly prevailed, is incontestable, and "of such proved utility, as will always justify its rigorous execution."

Without dwelling farther on the effects of Malaria, in this or any other climate, which we can only expect to be satisfactorily explained when pathology shall be considerably more advanced than at present, I may merely add, that such collections of facts have been made, and are now frequently repeated, as to the places and circumstances in which it arises from the earth, and the laws according to which it extends and multiplies,—considered merely empirically or statistically,—as we may confidently expect to be successful in disarming this cause of disease likewise of its terrors, long before the nature of the change produced by its action on the living body, or the rationale of any line of treatment of those who may be affected with it, shall become known.

The cure of Epidemic Scurvy, resulting in different cases, as is now satisfactorily established, from different deficiencies in the Diet habitually taken,—the efficacy, therefore, of different kinds of diet in counteracting this form of disease,—still more remarkably, the power of small quantities of vegetable Acids in producing the same effect, and the extraordinary rapidity with which such changes of diet, and these acids, will produce their effect,—may also be stated as examples, on a large scale, and of the most satisfactory kind, of what is generally called the power of Art over a most loathsome and virulent disease,—but in reality must be re-
garded now, and probably long after our time, as results obtained by simply empirical observations of the course of Nature, statistically arranged, fortunately facilitated by so many of the subjects being organised bodies of men;—and which have distinguished the present age to a degree, which those who are not familiar with the medical writings of the last century will hardly conceive.

We have good reason to hope, that inquiries now on foot as to the external causes of Scrofulous, or what is now usually called Tubercular disease, including pulmonary Consumption,—an inquiry which we must perceive to be more complex, and in which the operation of various causes must be recognised,—will be effectual in pointing out the means of counteracting that tendency in a very large proportion of cases of persons liable to it,—simply on the principle of empirical observation, enlarged and arranged in the form of Statistics, long before we shall have information as to the essential nature of the vital process, or mode of operation of the causes in question.

In illustration of this, I need only mention two facts, recently ascertained on so large a scale, that we have no doubt of their truth and importance, and which, even at present, may be said to be guides to successful practice in many cases only recently thought hopeless, although all that was previously known on the subject was certainly rather adverse than favorable to the supposition that they would ever be established. These are,—1. The good effect of the Cod-liver oil—if not of other animal Oils, on many cases of tubercular disease, in their early stage—provided only that it can be retained on the stomach to the extent of an ounce and a half or two ounces daily; and, 2. The almost complete exemption of the inhabitants of the Faro Islands from tubercular disease, notwithstanding that their climate, as regards cold and damp, is exactly that which, in this country, has been thought most favorable to it.—[Edinburgh Med. and Surg. Journal.

M. Brown-Séquard's Discoveries of the Functions of the Spinal Marrow.

Seldom has the scientific world been taken more by surprise than when M. Brown-Séquard announced his recent discoveries relative to the functions of the spinal marrow. Whatever may be wanting to complete our knowledge of the action of this portion of the nervous system, the brilliant investigations of Sir Charles Bell seemed to have set at rest forever the question as to the particular fibres which communicate motion to the muscles, and sensation to the brain. The theory of Bell, in a few words, is as follows: "The spinal cord has two functions, relative to the two substances of which it is composed. It serves as an independent organ, detached from the brain, for the performance of reflex actions, a property which it owes to the grey matter contained in
its centre. By the white substance it acts as a medium of communication between the brain and the parts to which the nerves are distributed, the posterior columns conveying sensations upwards, and the anterior and lateral columns transmitting the power of motion in a downward direction. This theory was less the result of experiments upon living animals, than of a process of reasoning, Sir Charles having always manifested a strong repugnance to vivisections. M. Longet, however, demonstrated, by the application of galvanism to sections of the spinal marrow of animals, that irritation of the posterior columns caused no movement, while that of the anterior columns occasioned no pain. On the contrary the galvanic current caused extreme pain when applied to the posterior columns above the transverse section of the medulla, and excited movements when directed through the anterior columns of the lower segment. The grey matter was found to be insensible to the irritation of electricity. The theory of Bell, so remarkable for its simplicity and apparently so perfectly supported by the demonstrations of one of the most eminent experimental physiologists, could not fail of universal adoption, and although pathological facts were occasionally made known which appeared to contradict, to some extent, its conclusions, it seemed natural to believe that these were inaccurately reported.

It will be observed, that in the experiments of M. Longet, the spinal cord was always completely cut across. We may not unreasonably ask whether the organ thus divided is in the same condition for transmitting sensation and the power of motion, as when its continuity is in a great part preserved, and why this method of experimenting was employed, instead of cutting through each portion in succession, and observing the effect produced upon the function attributed to that part? In reply to the latter inquiry, M. Longet states that the operation of laying bare the spinal marrow, and evacuating the fluid which is contained in the cavity of the arachnoid, is always followed by paralysis, both of sensation and motion, of the posterior extremities, thereby rendering further investigation impossible. Here was the great obstacle to researches in the functions of the spinal cord, and the removal of this obstacle was the first step taken by M. Brown-Séquard. He ascertained that the nervous disturbance following the opening of the spinal canal was caused by the loss of blood and by the pain and shock consequent upon the operation. By operating in such a manner as to prevent a great flow of blood, and by allowing the animal time to recover from the depressing effects of the operation, he found that both sensation and motion returned to the posterior extremities in almost, if not quite, their original degree.

Thus enabled to experiment upon the cord in a normal state (as far as its functions were concerned), he proceeded to isolate various portions of the different columns by sections made with ex-
treme care, and demonstrated a series of laws relative to the spinal functions, the principal of which are the following:

1. The posterior columns may be divided without destruction either of sensation or motion.
2. Sensation and motion are destroyed when the grey substance is cut across.
3. Integrity of the antero-lateral columns does not interfere with the loss of motion, nor does integrity of the posterior columns prevent loss of sensation.
4. Division of the posterior fibres of the cord, so far from abolishing sensation in the parts to which these fibres are distributed, appears, on the contrary, greatly to increase it.
5. When the posterior columns are divided, sensation continues to be transmitted between the lower portion and the grey substance, which transmits the impression to the sensorium by means of fibres descending from the upper portion, and joining obliquely the grey substance below the point where the section is made.

Our limits forbid us to detail the experiments upon which the above conclusions are founded. They have been repeated over and over again with the same results, in the presence of a committee appointed by the Société de Biologie, consisting of MM. Claude Bernard, Bouley, Broca, Giraldés, Goubaux and Vulpian, to whom was referred M. Brown-Séquard's memoir, and who were entirely satisfied with his conclusions. The interesting report which they made to the Society is the most convincing evidence of M. Brown-Séquard's skill as an experimenter and his eminence as a physiologist.—[Boston Medical and Surg. Journ.

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**On Chalk Metastases (Kalk Metastasen). By Rudolph Virchow. (Virchow's Archiv für Pathologische Anatomie, &c.)**

Some years ago Professor Virchow performed the post-mortem of a young lady whose case had thoroughly puzzled all the leading physicians of Berlin. She had complained of pains throughout the body, but these, which were attributed to rheumatism, had resisted all the means employed for its removal. After death numerous and large nodes of cancer were found in almost all the large bones, occupying cavities of a corresponding size in the osseous tissue, and no where rising above the level of the bone. A large quantity of white sandy deposit was found in the calyces and pelvis of the kidneys, and on dividing the lungs considerable deposits of a hard greyish white substance were found, and a similar deposit occupied the mucous membrane of the stomach. The salts which had been absorbed in consequence of the cancerous deposits in the bone appeared to have found a fresh nidus in the kidneys, lungs, and stomach. Professor Virchow has recently met with four similar cases; the following is a brief abstract of the
characteristic features found on cadaveric inspection. It may be premised that in all four there was evidence of recent degenerative nephritis, in the second or third stage of the disease, in that period at which the disturbance of the secretion is greatest; a point upon which Professor Virchow lays great stress, as he attributes the chalky deposit to impeded secretion of the urine. In the first, a maid-servant, aged forty-three, the upper lobe of the left lung exhibited posteriorly a hard spot, the size of a nut, on section yellowish white, dry and friable, which proved to be chalky infiltration. Similar smaller spots were disseminated through the lung. The second case occurred in a girl, aged fifteen, who presented a large mass of sarcoma, which had perforated the temporal bones from without inwards. The right parietal bone was occupied by a similar growth; the posterior part of the inferior lobe of the right lung presented several dense, dry, hard nodes of chalky infiltration. In the third case, a young man of nineteen years, who died of necrosis of the left femur, the mucous membrane of the stomach was found infiltrated with calcareous matter. The fourth case occurred in a man, aged seventy-three, who had labored under a caneroid affection of the lip, of the clavicles, and several ribs, and died of gangrene of the lung. Here the lungs exhibited, in addition to spots of caneroid disease and gangrene, dense calcareous infiltration in a part that was very emphysematous, so that the septa of the tissue resembled hard spiculae, and the inner surface of the pleura was invested with thick deposits. The last three cases resembled that of the young lady in Berlin in regard to the co-existence of extensive bone disease. In the first there was no apparent source from which the lime salts could be derived, but the bones were not especially examined. The chalky infiltration of the mucous membrane of the stomach, which is noted in two cases, deserves a little further attention. In both, the altered parts were altered in appearance and to the touch. They appeared opaque, whitish, more or less spotted, feeling dry and resistant; in the first case observed the tissues crepitated on section. The microscope demonstrated a fine granular deposit (blackish by transmitted, white by reflected, light), external to the follicular structures, which would probably mean that a portion of the glandular tissue had been absorbed to make way for the deposit. Acids dissolved the granules, and caused an evolution of carbonic acid gas; after the application of sulphuric acid, sulphate of lime crystals formed. The co-existence of extensive disease of the bones with extensive ossification of the vessels of the medullary portion of the brain, in the case of a young man aged twenty-six, is also quoted in illustration of the view advocated by Professor Virchow.

Professor Virchow was of opinion that in these cases he had to deal with a directcretification of the tissues, bearing, as he thinks, very materially upon the doctrine of metastases, on which account
he has selected the title for his paper that we have placed at the head of this notice.—[British and Foreign Med. Chir. Rev.

On the Healing of Abscesses by the First Intention. By M. Chassaingac.

M. Chassaingac has for some time past endeavoured to unite abscesses by the first intention, after their complete evacuation; and he reports that his success has been very considerable. His method of procedure may be judged of by the narration of a case which recently occurred at the Lariboisière Hospital and was observed by all who attend there. A healthy man, aged 19, was admitted February 17th, presenting all the symptoms of an acute abscess of the axilla, which had been about a week in forming. On the 19th, chloroform having been given, a considerable quantity of well-conditioned pus was discharged by the bistoury, pressure being exercised in all directions for the purpose of securing complete evacuation. The cavity of the abscess was next thoroughly washed out with water introduced through the tube of an irrigator, in order to bring away any remaining pus, the injection being continued until the water returned completely limpid. Pressure was again employed to force out every drop of the water, and the orifice was strapped up. A large pad of charpie was introduced into the axilla in order to make pressure over that region, and the arm was confined in one of Mayor's bandages, as if for fracture of the clavicle. On the 21st cicatrization was complete, no discharge of pus whatever being visible. The bandage was continued as a matter of precaution for two or three days, and then the arm was allowed to hang down, no pain being reproduced. In the site of the abscess a little indurated spot could be felt. On the 27th he was discharged quite well.—[Gaz. des Hop., and Ib.

On the Removal of Articular Bodies by the Subcutaneous Section. By M. Chassaingac.

The author prefers this designation to that of loose cartilages, as prejudging in nowise the nature of the bodies. In the present paper he relates two cases in which the operation devised by M. Goyraud of Aix was performed with success. This consists in opening the joint by the subcutaneous section, and forcing the articular body through the track of the incision into the cellular tissue, and leaving it there for future removal. The following are the conclusions the observations of these and other cases have induced M. Chassaingac to arrive at: 1. The pain, which is induced by the pinching exerted by the surfaces between which they are compressed, is not felt when the bodies are voluminous. 2. The character of this sudden pain is not pathognomonic, pain quite similar to it accompanying certain invasions of rheumatism or gout,
and the dislocation of the semi-lunar cartilages. 3. Our diagnosis may be at fault from our mistaking the slipping of the fingers over the walls of the articular sac for the displacement of a mobile body—a mistake that may far more easily occur than would be supposed. We have also to distinguish these bodies from partial indurations of the capsule, and from inequalities of the edges of the osseous articular extremities. 4. Among the concomitant affections that may be produced by the presence of these bodies are hydrarthrosis and ankylosis. 5. Although when hydrarthrosis produces great tension it is an obstacle to our diagnostic examination, a moderate repletion of the capsule favours the exploration. 6. The place of election for the operation on the knee-joint is the lower cul-de-sac of the synovial membrane on the inner side. 7. Before commencing any operation, small articular bodies must be previously fixed by acupuncture, as there is always great danger of their escaping at the moment of operating. 8. When these bodies are multiple, we should collect them all at one point, so as to expel them by a single operation. Still, where one or two escape us, the operation does not always fail. 9. Articular bodies left under the skin for a considerable period, undergo a great diminution, so that a secondary operation for their removal is not always required. 10. So important is it to avoid suppurative inflammation after the operation, that leeches should be freely applied at the root of the limb, both as a preventive and curative measure. 11. Angioleucitis is the species of inflammation most to be dreaded after operation upon the knee. 12. In expelling the foreign body from the joint, we should endeavour that its course should be made as long as possible, experience showing that in such cases a portion of the track may suppurate with impunity, or at all events without the pus invading the cavity of the joint.

[Revue Med. Chirurg., and Ib.]

Comparative Value of the Different Hemostatic Agents.

A correspondent sends us the following translation, which we publish as conveying valuable information upon an important subject.—[Boston Med. and Surg. Journal.

The Gazette des Hopitaux of Sept. 29th, in an article on the comparative value of different substances as means of arresting hemoptysis, after remarking that bleeding for this purpose has deservedly fallen into general disfavor, alludes to the clinical researches of Dr. Aran, published in the Bulletin Gén. de Thérapeutique, and gives a résumé of the interesting and valuable results to which he had arrived. We translate passages which seem to us of considerable value.

M. Aran has successively tried agents belonging to the class of hemostatics, properly so called, such as resinous substances, the ergot of rye and common salt; then astringents—acetate of lead,
alum, *eau de Rabel*, tannin, and gallic acid; nauseants and emetics—ipecac, tartar emetic, veratrine; and sedatives of the circulation—nitre and digitalis.

Of the agents belonging to the first group, haemostatics proper, the essence of turpentine has seemed to M. Aran especially to deserve the attention of physicians. He has prescribed it pure, in doses of from ten to thirty drops, in a glass of water, or made up into a bolus with magnesia, and taken enveloped in moistened wafer (*pain à chanter*). Generally within a few hours after the patient commences taking it, there is a very marked diminution in the amount of the haemorrhage, and in twenty-four or thirty-six hours at the most, it is reduced to a very small quantity or entirely ceases. On the other hand, M. Aran is convinced, as many English and German physicians have already proved, that the essence of turpentine is less suitable in haemoptysis, with a tendency to inflammatory action within the chest, a febrile movement, or when it occurs in young or rather plethoric subjects, than when it happens in debilitated, cachectic subjects, with characters of passivity or atony.

Ergot of rye and the ergotine of M. Bonjean, have shown much less efficacy against haemoptysis than essence of turpentine. The former, even, when given in a very large dose, has seemed to exert only the most moderate influence upon the haemorrhage.

The same is not the case with chloride of sodium or common salt, which has been proved to possess an undoubted efficacy in doses of from sixty to one hundred and fifty grains taken in the course of a few hours in solution, or in the form of powder. It is particularly deserving of recommendation in such cases, as it is constantly at hand.

Among the astringents, M. Aran has found none worthy of confidence except tannin and gallic acid. Gallic acid seems to him preferable to tannin, as, with the same styptic properties, it has not the same drying action upon the tissues, and does not produce the obstinate constipation which occurs when the latter is employed. The medium dose of gallic acid, as he administered it, was, from ten to twelve grains in twenty-four hours, in powders of two grains each, given at intervals of two hours.

M. Aran acknowledges the power of nauseants and emetics to arrest haemoptysis, such as tartar emetic, ipecac and veratrine. With regard to the first two this property has been known for a long time. As for veratrine, in three cases in which it has been prescribed, the haemoptysis was arrested as if by enchantment as soon as nausea and vomiting took place. These agents would deserve, then, to be placed in the first rank of haemostatics, if there were not others of equal efficacy, which do not produce nausea and vomiting, effects which are always painful or disagreeable to the patient.

Nitre and digitalis have been equally, and with good reason,
extolled in this case by the name of sedatives to the circulatory system. Following the example of Schmidtmann, who conceived the idea of combining sea salt with digitalis to combat hæmoptysis, M. Aran, for the same purpose, combined digitalis and nitre. This mixture, it appears, produced very remarkable results.

In ordinary cases he gave in the course of twenty-four hours four grains and a half of digitalis and twenty-three grains of nitre in four powders. But when the hemorrhage was very profuse the quantity of nitre was carried as high as thirty-eight grains, and that of digitalis to eight or even twelve grains; in some very grave cases the quantity of digitalis given was carried to twenty-three grains, and of nitre to sixty grains. A remarkable circumstance noticed was, that when these remedies were given in this quantity the system was not affected in any unfavorable manner; the pulse did not suddenly abate in frequency, nor was there a very abundant diuresis. On the other hand, the effect upon the hæmoptysis was most marked; in a few hours the flow of blood was considerably reduced, and often after twenty-four or thirty-six hours there remained only a little bloody expectoration. The diminution of the hemorrhage was generally accompanied by a great calm. Nevertheless M. Aran observed that never, after the administration of essence of nitre and digitalis, was the arrest of hemorrhage so sudden as after the administration of turpentine or gallic acid.

M. Aran sums up his opinion of the respective value of the different agents in question, in the following words:—In profuse hæmoptysis, but not immediately threatening life, the physician may take his choice of either of the preceding remedies. In very profuse hæmoptysis, on the contrary, where it is necessary to arrest the bleeding as soon as possible, and by means the least likely to depress the system, the physician cannot trust the tardy remedies. Neither the ergot, nor sugar of lead, nor eau de Rabel, nor alum, nor rhatany, &c., will be equal to the emergency. Only turpentine, gallic acid in a large dose, salt, nitre combined with digitalis, can be employed with success; but the necessity of proportioning the dose of the medicine to the intensity of the hemorrhage, in administering the chloride of sodium, but particularly the nitre and digitalis, is productive of great inconvenience; the danger of too great a depression from too large a dose, or from too long a continuance of the remedy.

"It is then to gallic acid and to turpentine that I give the preference in these grave cases; yet, under the apprehension of their insufficiency, I do not think the physician should limit himself to their use. It is under such circumstances that bandages applied to the limbs, which are very useful in other kinds of hemorrhage, and ice applied to the chest, have saved the life that was in danger, by stopping the hemorrhage for the moment, and allowing the internal remedies to complete the work."
On Methodical Cauterization of Abnormal Divisions of certain Organs. By M. Jules Cloquet.

In two memoirs recently read at the Académie des Sciences, M. Cloquet furnishes some account of his mode of cauterizing fissures and fistulous openings, founded upon the observation of the powerful contractile effects exerted by cicatrical tissues acting at the angles of wounds caused by burns. He adopted it first in the case of fissure of the velum palati. The object was not to cauterize the entire extent of the edges of the division, and bring the granulating surfaces into contact by means of sutures and apparatus—a plan long tried, sometimes succeeding, but oftener failing. The caustic is applied over a very limited surface, at the exact angle of the fissure; and, after the cicatrical tissue, which results has had time to produce its retractile effect, the caustic is again applied to the angle of the remainder of the division. The application is repeated again and again, at intervals, so that the fissured parts are thus brought towards each other bit by bit, and united by a series of cauterizations that may be regarded as so many points of successive suture. The operations for this infirmity, devised by Grafe and Roux, are difficult of execution, and not infrequently fail in success. M. Cloquet first put his plan into execution in a case in which the whole left side of the palate was fissured as a result of syphilitic ulceration. From eighteen to twenty cauterizations with the acid nitrate of mercury sufficed to effect a complete reunion. In another case of congenital fissure, the patient had already been operated upon by Roux’s method, but violent coughing had caused the sutures to tear through. Successive cauterizations firmly united one-half of the fissure, with which the patient was satisfied, and refused to persevere. In a third case, M. Nelaton healed a traumatic division by a similar employment of the electric cautery. In 1851, a congenital fissure existing in a child eleven years old, was completely closed after twenty applications. In all these cases the pain was slight, no change had to be imposed in the regimen or mode of life, and no accidents resulted. Even unpractised surgeons may perform so simple an operation without the aid of an assistant. It is slow in the production of its results, but this is one of the conditions of its success, and is of little consequence, as it does not interfere with the business of life. M. Cloquet thinks the actual cautery is the best means to employ, but for patients who dread this the electric cautery may be substituted.

Another affection usually intractable is recto-vaginal fistula, and encouraged by the above success, M. Cloquet applied successive cauterization to its management. The index finger, guarded against the heated body, is introduced into the rectum, and, the external parts, being held open, the apex of the fissure is touched by a small cautery, the patient being able to at once get up and pursue her occupations as before. The pain and inconvenience of the
application are so slight, that the women readily submit to it. A more considerable amount of union is produced by the early cauteries than by those made later, so that the complete cure becomes tedious—a less inconvenience from the fact that the most serious consequences of the infirmity disappear after the early cauteries.—[Gazette Médicale.

[M. Cloquet refers to but six cases of care of this fistula, and to six of fissured palate: and we cannot but feel surprised that a procedure—known by him since 1826—so easy of execution and so fertile in success as he states it to be, does not exhibit more abundant results.]


Dr. Washington Atlee gives a summary of all the cases, thirty in number, in which he has performed the operation of ovariotomy. Such an assemblage of facts constitutes a valuable contribution to the history of this subject. We can only give a condensed view of the leading facts.

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<td>1. Sixth day—peritonitis</td>
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<td>Yes</td>
<td>Double ovarian</td>
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<td>Yes</td>
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<td>Yes</td>
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<td>5.</td>
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<td>6. In 3½ years, from progress of disease</td>
<td></td>
<td>Yes</td>
<td>Cystiform</td>
<td>Yes.</td>
</tr>
<tr>
<td>7. Thirty-nine days after, of cholera, caused by eating duck</td>
<td></td>
<td>Yes</td>
<td>Extra-uterine fibrous</td>
<td>Yes.</td>
</tr>
<tr>
<td>8. Sixth day—peritonitis</td>
<td></td>
<td>Yes</td>
<td>Cystiform</td>
<td>Yes.</td>
</tr>
<tr>
<td>9. Third day—exhaustion</td>
<td></td>
<td>Yes</td>
<td>Cystiform</td>
<td>Yes.</td>
</tr>
<tr>
<td>10. Third day—exhaustion</td>
<td></td>
<td>Yes</td>
<td>Cystiform</td>
<td>Yes.</td>
</tr>
<tr>
<td>11.</td>
<td></td>
<td>Yes</td>
<td>Cystiform</td>
<td>Yes.</td>
</tr>
<tr>
<td>12.</td>
<td></td>
<td>Yes</td>
<td>Cystiform</td>
<td>Yes.</td>
</tr>
<tr>
<td>13.</td>
<td></td>
<td>Yes</td>
<td>Cystiform</td>
<td>Yes.</td>
</tr>
<tr>
<td>14. Pregnant two months at time of operation; died in 90 days of exhaustion</td>
<td></td>
<td>Yes</td>
<td>Double cystiform</td>
<td>Yes.</td>
</tr>
<tr>
<td>15. Third day—peritonitis</td>
<td></td>
<td>Yes</td>
<td>Cystiform</td>
<td>Yes.</td>
</tr>
<tr>
<td>16. Third day—haemorrhage</td>
<td></td>
<td>Yes</td>
<td>Extra uterine fibrous</td>
<td>Yes.</td>
</tr>
<tr>
<td>17.</td>
<td></td>
<td>Yes</td>
<td>Extra-uterine fibrous</td>
<td>No.</td>
</tr>
<tr>
<td>18.</td>
<td></td>
<td>Yes</td>
<td>Cystiform</td>
<td>Yes.</td>
</tr>
<tr>
<td>19. Thirteen hours—exhaustion</td>
<td></td>
<td>Yes</td>
<td>Cystiform</td>
<td>Yes.</td>
</tr>
<tr>
<td>20. Nine hours—exhaustion</td>
<td></td>
<td>Yes</td>
<td>Cystiform</td>
<td>Yes.</td>
</tr>
<tr>
<td>21. Third day—peritonitis</td>
<td></td>
<td>Yes</td>
<td>Three fibrous extra uterine</td>
<td>Yes.</td>
</tr>
<tr>
<td>22.</td>
<td></td>
<td>Yes</td>
<td>Cystiform</td>
<td>Yes.</td>
</tr>
<tr>
<td>23.</td>
<td></td>
<td>Yes</td>
<td>Cystiform</td>
<td>Yes.</td>
</tr>
<tr>
<td>24.</td>
<td></td>
<td>Yes</td>
<td>Cystiform</td>
<td>Yes.</td>
</tr>
<tr>
<td>25. Fifth day—exhaustion</td>
<td></td>
<td>Yes</td>
<td>Cystiform</td>
<td>Yes.</td>
</tr>
<tr>
<td>26.</td>
<td></td>
<td>Yes</td>
<td>Cystiform</td>
<td>Yes.</td>
</tr>
<tr>
<td>27. Fifth day—haemorrhage</td>
<td></td>
<td>Yes</td>
<td>Cystiform</td>
<td>Yes.</td>
</tr>
<tr>
<td>28.</td>
<td></td>
<td>Yes</td>
<td>Cystiform</td>
<td>Yes.</td>
</tr>
<tr>
<td>29. Sixth day—haemorrhage</td>
<td></td>
<td>Yes</td>
<td>Cystiform</td>
<td>Yes.</td>
</tr>
</tbody>
</table>

From the tabular statement we have thus constructed, it appears, that out of 30 cases in which the operation for ovariotomy was undertaken, ovarian tumours were found in 22 cases, and fibrous tumours of the uterus in 8 cases; that in 4 cases the operation was abandoned, in as far as related to the object of the operation—the removal of the tumour; that 13 cases only recovered with life, in 2 of these the tumour being left behind; that in 17 cases the pa-
1836.]

Epithelial and Villous Growths.

The patient died, at intervals of from nine hours to thirty days after the operation, with the exception of one that survived six months.—The causes of death were—in 3 cases, hemorrhage; in 6, exhaustion; in 4, peritonitis; in 1, gangrene of jejunum; in 1, cholera from indigestion; in 1, erysipelas. It is right to state, that in some cases the author attributes the death to causes independent of the operation. In 7 cases the operation was undertaken under desperate circumstances, and with a view of arresting impending death; 5 of these died; 2 survived.—[Amer. Journ. Med. Soc.

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On Epithelial and Villous Growths. Py Prof. Bruch, of Bâle.

The majority of pathologists are at present agreed that cancerous growths are primarily of local origin, and not the result of a cancerous dyscrasia; the proofs are also being multiplied that a transition from non-malignant to malignant growths may take place. Professor Bruch enters into an interesting disquisition on the nature of epithelial and villous growths, with a view to determining their pathological classification. He admits, with Lebert, Schuh, and others, the occurrence of new growths, consisting of cylindrical epithelium, which have hitherto been observed only in parts normally presenting that variety of epithelium, viz., stomach, rectum, collum uteri, male urethra, eye, and fourth ventricle of the brain. Professor Bruch regards epithelial growths as the result of a morbid development of the epithelial investment and epidermis, of the papillary body and of the cutis vera, which he considers to be non-malignant; and he establishes a subcutaneous variety of epithelial growth, which he regards as malignant. "An essential characteristic," he observes, "of epithelioma, papillary, hypertrophy, and epithelial growths generally, is an intimate union and adhesion of the cells, which prevents their extrusion and causes their accumulation."

The villi of the chorion of mammalia are regarded by Professor Bruch as the prototype of all normal and abnormal villous or papillary formations; all being originally formed as simple elevations of the integuments of the surface, and especially of the basement membrane. After reviewing the various forms of villi occurring in the body, he concludes that the growth of the papillary, villous, or fringed formations is an attribute of all the surfaces of the body, and that it is impossible to draw the exact line of demarcation between that which is normal and that which is abnormal. Many warty excrescences, most condylomata, Clarke’s cauliflower excrescence of the os uteri, the incipient condition of some villous cancers, are mere varieties of the same thing in different parts of the body. We cannot conclude this brief notice without advert-
Vapor of Iodine in the Treatment of Ophthalmia. Reported at a Meeting of the Society for Medical Observations, Oct. 15th, 1855. By Calvin G. Page, M. D.

Case I.—John Williams. Married. Coal-heaver, aged 35—a dispensary patient. First applied to me in March, 1855, for treatment of his eyes. He had partial capsular cataract of one eye, with absence of the crystalline lens, caused by a wound with a pointed arrow, when he was a boy. The lids of both eyes were swollen and everted, and covered with exuberant granulations. There was great intolerance of light, pain and chemosis, with sclerotic and conjunctival injection, and constant lachrymation with some discharge of pus. I commenced using freely the sulphate of copper with cooling applications, and in a fortnight he was able to work a little, which he had not done for four months previously. Dr. H. W. Williams saw him with me about this time. He has been under my care since that time, but the improvement would not progress beyond a certain point, though all means known to me to be used in such cases were applied. His constant exposure to coal dust probably prevented the usual action of remedies. I lost sight of him during the month of August and the early part of September.

About the middle of September he again applied to me, when the condition of the eyes was as follows. The lids of both eyes were somewhat swollen, the inner surfaces were covered with granulations; there was some injection of both conjunctiva and sclerotica, intolerance of light, dimness of vision, and lachrymation with a small amount of pus. All other means having failed, I availed myself of the fact, that iodine, when dissolved in chloroform, evaporates without leaving the stain of iodine, and I determined to apply this vapor to his eyes. I commenced using it on the 20th of September—and continued it daily until the 29th. After two applications the injection about the eyeball disappeared, leaving it in a perfectly normal condition. At the end of eight
days there was a spot near the inner canthus, on the upper lid of each eye, entirely free from granulations. He has been seen seven times since the 28th of September, and the vapor has been applied. The granulations have nearly all disappeared from the upper lids, except at points near the outer angle. There is no intolerance of light, and the dimness of vision has disappeared.

CASE II.—Annie Fowler, aged 11 years, No. 11 Friend st., a dispensary patient, was sent by a benevolent lady to Dr. Reynolds, Sen., who sent her to the Eye and Ear Infirmary, where she was somewhat benefited. She has scrofulous tarsal ophthalmia. Her mother, seeing the benefit to Williams (the patient first mentioned,) requested me to take charge of her. I have applied the vapor eight times. One eye is nearly well, the other very much improved. Both these patients are still under treatment.

The advantages of this method of applying iodine seem to me to be that the effect of the agent is obtained more rapidly and without the usual discoloration. The sensations to the patient are not disagreeable; the effect of smarting, &c., passes away in less than a minute. In applying it to the eyes, the lids should be closed. The vapor seems to penetrate through them. It appears to be applicable wherever iodine is called for, as in scrofulous glands, hydrops articuli, &c. The atmosphere should be excluded from the surface during the application of the vapor.—[Boston Med. and Surg. Journal.

Spontaneous Escape of Fluid from the Peritoneal Cavity. (From the American Medical Monthly.)

Mr. Editor:—The following case was recently communicated to me by Dr. Wm. Burns, of Littleton, N. H., and I have thought it might prove interesting to the readers of the Monthly.

Mrs. H., of Bethlehem, N. H., was married at the age of 32, having always enjoyed good health. Four years after, she became pregnant, and during pregnancy she became dropsical. It was believed that she had not less than fifty pounds of dropsical fluid in the peritoneal cavity at the end of pregnancy; but all of this escaped spontaneously, per vaginam, during and immediately after delivery. This occurred in the year 1791. Two years after the birth of the child (1793,) the peritoneal cavity has again become largely distended with the dropsical accumulation, and Dr. Moore, of Bath, N. H., performed the operation of paracentesis abdominis, removing sixty-four and three-fourths pound of fluid.

During the following twenty-six years, up to 1819, Mrs. H. had filled with dropsical fluid nineteen times, for which she had been tapped six times, and had had thirteen spontaneous evacuations per vaginam. At neither of the six tappings had she lost less than sixty-three pounds of fluid; and at each spontaneous evacuation
the fluid had drained off in about forty-eight hours, flowing from the vagina once in two or three minutes. After the water had been removed, either by tapping or spontaneously, the sternum was so prominent relatively, on account of the collapsed state of the abdominal walls, that a common quarto family bible could stand on end in the hollow beneath its lower extremity. The ribs, also were correspondently prominent, and two of them had been broken by the distension.

Dr. Burns tapped the patient, October 21st, 1819, removing twenty-two quarts of fluid, which weighed forty-nine pounds. She was then in the sixty-fifth year of her age, and had been blind four years. She had another spontaneous discharge of the fluid (the fifteenth in all) per vaginan ten months afterwards, August, 1820. At this time more than fifty pounds were supposed to be removed. Not long afterwards she died of some disease not connected with the dropsy.

In this case there is reason to believe that the distension became so great that the wall of the vagina gave way from the downward pressure, at the cul de sac, between this canal and the rectum; and thus the fluid was spontaneously discharged in the course of about forty-eight hours. Afterwards the rupture probably healed by the first intention, and everything remained in the natural state till the tension again produced the same result. And this state of things continued for more than thirty years, without much impairing the general health. The only thing worthy of remark was an occasional attack of vomiting during the last ten or eleven years.

This case illustrates the method adopted by nature in performing the operation of paracentesis abdominis; and I have for some time past been convinced that it is better to imitate the example she has given in this instance, and tap from the vagina, in all cases in which there is such a projection downwards of the cul de sac between it and the rectum as to indicate the presise point where the puncture should be made, and enable the operator thus to make it without risk to any vessel or any neighboring part. A report of a case under my direction, in which this operation had several times been performed, is contained in the American Journal of the Medical Sciences, for January, 1855. In every instance the puncture healed by the first intention, and no unpleasant symptom occurred.

Yours truly,


Luxation of the Knee.

M. Royer communicated to the Imperial Academy of Medicine, 18th Sept. 1855, a case of complete luxation of the knee forward, the tibia mounting many centimetres before and above the articulating surface of the femur, without tearing of the external parts. This luxation was easily reduced by forced flexion of the leg on
the thigh, and a prompt cure was effected. In a month the patient could walk, and in six weeks the cure was completed.—[Revue de Therap. Med. Chiurg. Medical News and Library.

EDITORIAL AND MISCELLANEOUS.


To those who are fond of the multum in parvo, this work will doubtless prove quite acceptable. It is a very fair condensation of the principles and practice of Medicine. Whether such studies ought to be condensed at all or not, is a question upon which we have repeatedly expressed our opinion. While a manual is well suited to the beginner, those more advanced should not be afraid to encounter more elaborate works which can alone convey a full knowledge of disease. The additions to the work before us, by Dr. Condie, have very materially enhanced its value.


The profession, already indebted to Prof. Budd for one of the best monographs on diseases of the Liver, will not fail to appreciate the present excellent work on gastric affections. As the stomach is one of the most important organs in the body, and at the same time one very often invaded by disease, any new light or even a compilation of accumulated lore on the subject, must be valuable to every practitioner. The author has accomplished his task in an able manner and shows himself to be a sound observer.


Among the several Reports presented to the Association, is one on the subject of "Home Adulterations of Drugs," from which we make the following extract:

Balsam Peru has been met with, possessing none of the characteristics of genuine balsam except in color and consistency, and upon analysis affording no cinnamic acid.

Pulv. Capsicum.—The sample examined had a brick dust color, little pungency, and filled with yellow specks and strong odor of turmeric. It was a mixture of turmeric and American capsicum, and, of course, almost inert.
Castor is found with the follicles filled with saw dust to half the weight of the castor.

Opium.—Since the circular of the Secretary of the Treasury fixing a percentage of morphia for this drug, a more uniform quality has been found in market; but a great many samples have been observed the past season with foreign substances, most commonly lead, inserted in the lumps, in some instances equal to 20 per cent. of the weight of the mass. We are of the opinion that this was done abroad, and probably at the port whence shipped. The different examiners should seek to detect this fraud before passing it.

Musk in pod has been observed loaded in the same way, to the amount of 20 grains in a single pod.

The Essential Oils are largely adulterated in this country.

Oil of Peppermint sometimes contains 50 per cent. of alcohol. Oil of Rosemary is adulterated largely with turpentine, and in short, the whole class are shamefully sophisticated.

Otto of Rose in the same class.

Cream of Tartar, adulterated with carbonate of lime, some samples to the extent of 33 per cent., others in less proportion. Sul. potash is also used for this purpose, and alum largely. Of six specimens examined by a gentleman of New York City, purchased at various shops, but one was found pure, some of them being adulterated 30 per cent. The same gentleman says, in reply to our enquiries, that from twenty-two specimens or samples of essential oils, fourteen were found to contain turpentine and other impurities. The same gentleman reports samples of powdered opium adulterated 50 per cent.

Cod Liver Oil.—All kinds of fish oil may be found neatly bottled and carefully labelled as the genuine article.

Sulphate of Quinine.—Samples have been detected with the old adulteration of mannite, and one gentleman reports quinine mixed up with fine picked raw cotton, adding to the bulk so as to fill the vial without using the requisite quantity of this valuable chemical.

Ipecacuanha in powder and Jalap in powder, each mixed with spurious matter, and English rhubarb in powder, put up for fine powdered Turkey, are not uncommon in all the markets.

Of crude materials, Nitre, or Saltpetre is one of the most commonly sophisticated, being adulterated with common salt and nitrate of soda largely.

These are some of the reports made to us, all from reliable sources.

The Committee have endeavored to establish points of observation in different sections of the United States, and as far as possible to obtain the names of houses from whom these various sophistications have been obtained. Such information they deem it best to withhold from publication at present, lest they might do injustice to parties ignorantly sending out such drugs; but they also intend from time to time to compare notes, and when satisfied of continued practices of this kind, will report such names to the Association.

In the mean time, they cannot too strongly urge retail apothecaries especially, to be cautious of whom and what quality of medicines they purchase. It is to the dispensing apothecary that medical men and the community look for such medicines as are pure, not only "good of their kind," but of the best kind.

The Association offers the following prizes:
1st.—Twenty-three volumes of the American Journal of Pharmacy. For the best Essay which shall develop the commercial history of all drugs indigenous to the United States, as Senega, Spigelia, Serpentaria, &c., as regards the manner and places of their collection and preparation for the supply of commerce, the amount annually collected, and the channels through which they enter general commerce.

2nd.—Six volumes of Gmelin's Hand Book of Chemistry. For the best Essay on any question relating specially to Pharmacy.

Committee of Judges.—Charles Ellis, William Procter, jr.

All Essays contributed for the Prizes must be delivered free of charge to Charles Ellis, Philadelphia, on or before the second Tuesday in August, 1856.

A Plea for the establishment of Veterinary Colleges in the United States. By James Bryan, M. D., Prof. of Surgery in Phila. College of Medicine.

This is quite an eloquent appeal to the Agricultural Society of Pennsylvania, in behalf of our domestic animals, who would doubtless vote the learned author a service of plate, if it were as fashionable with them as it is with their masters. Veterinary institutions of learning exist in every country in Europe, and number among their professors some of the first order of talent and scientific acquirements. We see no reason why similar ones should not exist in our country. Boston, we believe, has established one, or is about to do so.

The Monthly Stethoscope and Medical Reporter.—Under this title we find another periodical issued at Richmond, Va., edited by G. A. Wilson, M. D., and R. A. Lewis, M. D. The original "Stethoscope" having been combined with the Virginia Medical and Surgical Journal, the new, or "Monthly Stethoscope" is started for the purpose of continuing the advocacy of the principles and policy of its predecessor. Hoping that the old Dominion may afford sufficient patronage for two respectable medical journals, and that the parties will not deem it advantageous to science to quarrel about their subscription lists, we wish them both success in their enterprise.

Deaths from Chloroform.—Fatal cases of chloroformization continue to multiply. One has lately occurred in Edinburgh, and another more recently in Boston. How many more will the profession, and especially dentists, need to make them more circumspect in the use of this valuable, but dangerous agent?

American Medical Association.—The Ninth Annual Meeting of the American Medical Association will be held in the City of Detroit, Michigan, on Tuesday, May 6th, 1856.

The secretaries of all societies and other bodies entitled to representation in the Association, are requested to forward to the undersigned correct
lists of their respective delegations, as soon as they may be appointed; and it is earnestly desired by the Committee of Arrangements, that the appointments be made at as early a period as possible.

The following extracts are from Article 2d of the Constitution:

"Each local society shall have the privilege of sending to the Association one delegate for every ten of its regular resident members, and one for every additional fraction of more than half this number.

"The Faculty of every regularly constituted Medical College or chartered school of medicine, shall have the privilege of sending two delegates. The professional staff of every chartered or municipal hospital, containing a hundred patients or more, shall have the privilege of sending two delegates; and every other permanently organized medical institution, of good standing, shall have the privilege of sending one delegate.

"Delegates representing the Medical Staff of the United States Army and Navy, shall be appointed by the Chiefs of the Army and Navy Medical Bureau. The number of delegates so appointed shall be four from the Army medical officers, and an equal number from the Navy medical officers."

The latter clause, in relation to delegates from the army and navy, was adopted as an amendment to the Constitution, at the meeting of the Association held in New York, in May, 1853.

**Medical Journals, &c., please copy.

William Broddie, M.D., Detroit, Mich.,
One of the Secretaries.

State Medical Society.—The Seventh Annual Meeting of the Medical Society of the State of Georgia, will be held in the City of Macon, on the 2d Wednesday (9th April) next, and the annual Address will be delivered at 12 o'clock, M., on that day.

D. C. O'Keefe, M.D.,
Greensboro' March 1st, 1856.

Recording Sec'y.

Notice to Subscribers.—The Editors beg leave to say that they have no pecuniary interest whatever in the subscription list of this Journal, and that all communications relating to money matters should therefore be addressed to the publisher, Mr. Jas. McCafferty, who is alone concerned in the management of the financial affairs of the work.

On a New Parasite in Man (Pentastomum Denticulatum Rud.) By Dr. ZENKER, of Dresden.—It was Professor Siebold who first demonstrated from the observations of Drs. Pruner and Billharz, Physicians in Cairo, the existence of a species of the genus pentastomum living in the intestines of man; Siebold gave it the name of P. constrictum. The author states that Egypt is not the only country which has the good fortune to possess a pentastome; another species, the pentastomum denticulatum Rud., which had hitherto been met with only in animals, is found in man; and is even very common in Germany. The author has observed this worm seven times, and always in the same organ, on the superior surface of the liver, under the peritoneum. It is contained in a dense fibrous capsule which adheres to the parenchyma of the liver and to the peritoneum, but which admits of being easily detached; it appears under the form of a little tubercle of from
2.25 to 3.37 millimetres (.0935 to .1326 of an English inch), usually filled with a calcareous deposit with which the animalcule is itself incrusted. The capsule is proportionally very thick, and it is difficult to extract the worm from it uninjured; sometimes, however, the capsule separates easily from the earthy concretion, and the worm can then be withdrawn.

The author gives a detailed description of the animal, and the description is accompanied with figures to exhibit more clearly the form of the worm, and especially that of the tentacula with which the head is furnished. — [Zeitschrift für Rationelle Medizin and Gaz. Med. de Paris. American Med. Monthly.

Glass Female Syringes. By Frank H. Hamilton, M. D.—Gradually the old pewter female syringes have gone into disuse, and physicians have substituted the neater, and perhaps, cheaper glass syringes; but the occurrence of several accidents, in some of which my surgical services have been required, induce me to question the value of the substitution.

In three instances I have been called upon to remove from the vagina the broken fragments of these syringes. In one instance the accident occurred in consequence of the sudden alarm of the female while she was using the syringe, and was in no measure, probably, due to the imperfection of the instrument; but in the two other cases the glass gave way from the mere pressure of the fluid while the piston was ascending—the accident, certainly, might occur when the piston fits snugly even though there was no defect in the glass; but if there chances to be the slightest irregularity in the walls of the syringe, or a fissure, such as might easily escape detection, or an unusual tenuity of the round extremity, a fracture would be almost certain to follow. It was this latter circumstance, viz., the extreme thinness of the extremity of the instrument, which occasioned the accident in two of the cases mentioned. Two other cases have been related to me as having occurred in the practice of neighboring physicians, making in all five that have come to my knowledge.

It is probable that no examination, however critical, would enable us to determine, before the fracture has taken place, whether the end of the instrument has a suitable thickness, and it is very likely that a majority of them are blown too thin for safety. I have found the fragments not as thick as my finger nail.

Of late I have, therefore, uniformly recommended either some appropriate metallic instrument, or perhaps more generally the gum elastic bag with an ivory nozzle.—[Buffalo Med. Journal.

On the Utility of Decoction of Rhatany in Keratitis. By Dr. A. Quadri.—Dr. Quadri observes, that of all the inflammations of the eye, keratitis is one of the most frequent and most obstinate. Experience has proscribed the employment of mineral astringents. Among those of the vegetable kingdom the laudanum formed by the combination of crocus and opium sometimes produces excellent effects; but in scrofulous ophthalmia, which is frequently but a keratitis, it occasionally gives rise to prolonged and mischievous irritation. The author had tried various other substances, as tannin, calumba, &c., without any definite results, when he resorted to rhatany. The experience of six years has convinced him of its value. Its application merely induces a sensation of dryness in the interior of the eye,
and in a short time the pain and photophobia are mitigated, and the weeping is much diminished. When the irritation has thus become calmed in two or three days, the rhatany may be replaced by the more powerful laudanum, more or less diluted. The rhatany is insufficient in the corneitis accompanying blennorrhoeal ophthalmia, but in scrofulous and all other forms of keratitis its efficacy is constant. It is prepared by boiling half an ounce of the root in twelve ounces of water, or decoction of elder-flowers, down to half the quantity, and filtering. It should be freshly prepared, and may be used three or four times a day.—[Annales d'Oculistique. Med. Chir. Rev.

**Hydrocele.**—At the Clinique of Dr. La Farge, (Gaz. Med. Italiana Toscan,) first surgeon of the hospital Del Greve at Tolosa, it is stated on the authority of Dr. Bellucci, that five cases of hydrocele had been cured by an ointment of digitalis leaves. Dr. La Farge declared that he had cured a hydrocele the size of a pear, in seven weeks, which had been in existence for many months. The ointment is made by uniting one part of the digitalis leaves to five parts of lard.—[Charleston Med. Journ. and Rev.

**Female Medical College at Boston.**—This institution, which a stupid Legislature incorporated as a concession to “Woman's Rights,” we understand has recently underwent that interesting process called a “blow up;” and Miss Doctor H. M. Gasset has published a pamphlet which implicates the directors in the perpetration of numerous fraudulent transactions, by which the fund that has been raised for the endowment of the Institution has been ignominiously squandered, or rather perhaps, concentrated in wrong pockets. *Sic transit gloria mundi.*—[Ohio Med. and Surg. Jour.

**Statistics of Insanity in France.**—From an official document, published by the Minister of Commerce and Agriculture, on the Statistics of France, it appears that there are at present, for every one hundred thousand of the population, 105 persons blind, 82 deaf and dumb, and 125 insane.—[American Jour. of Insanity.

**Anaesthetics in the Austrian Army.**—A circular has just been issued, ordering that in future the army medical officers shall always employ, for the purpose of inducing anaesthesia, a mixture consisting of one part chloroform and nine parts ether, this being the proportion long employed by Dr. Weiger, a Vienna dentist.—[Medical Times and Gaz.

**Puerperal Convulsions prior to Labour.**—During some recent clinical remarks, M. Paul Dubois observed that it was formerly generally believed, and is so still by many, that in these cases we should induce premature labour. He does not agree to this view, and thinks the practice may even be injurious. It is indeed useless, because labour always comes on of itself, under the influence of convulsion, and it is injurious, by reason of its irritating the uterus, and thus aggravating the convulsions. For the same reasons, he objects to the use of ergot. He employs general bleeding, as far as the strength will permit it, leeches behind the ears, and purgatives. Of these last, calomel and jalap, mixed up with honey, and laid on the tongue every hour until it operates, is the best. Sinapisms should also be
applied to the feet, and ice to the head. The only operation justifiable is the application of the forceps, when the head is engaged in the pelvis, and solely in aid of the last expulsive efforts.—[Med. Times and Gazette, from Gaz. des Hôpitaux.

_Sweet Whey in Pertussis._—Dr. Lowenthal states, as the result of numerous trials, that this substance given several times a day in doses beginning with half a spoonful, cures the disease more rapidly and more pleasantly than any other means.—[Medical News and Library.

_Painless Tooth Extraction without Chloroform. With Observations on Local Anaesthesia by Congelation in General Surgery._ By Walter Blundell, Surgeon Dentist.—Though written in too party a spirit, Mr. Blundell’s tract may be recommended to perusal as containing much interesting matter. Dr. Arnott’s proposal to produce temporary annihilation of local sensation by intense cold was at first disregarded; but now almost every day furnishes us with some testimony of its value. We have no doubt the method here proposed will become still more general with respect to the minor and superficial operations, and it may be applicable, for aught we know, in the practice of dentistry. Nevertheless, we shall require far more evidence than at present exists in support of its power of curing (!) cancer, its availability where deep incisions have to be made, and its _neverfailing_ immunity as respects devitalization, &c., of the tissues to which it is applied. Mr. Blundell has invented a particular apparatus for the production of intense cold, capable of producing it in very limited spots, and such as are difficult of access. Stumps and large molars are removed, according to the author, without the least inconvenience.—[London Lancet.

_Local Anaesthesia._—We have seen and conversed with Dr. Branch, of Galena, who has called our attention to the following notices, published in the Galena Daily Advertiser, and Weekly Northwestern Gazette. He states further, that he has tested it in nearly two hundred instances, and finds that it more than meets his expectations. He is now in New York, having manufactured the apparatus necessary for its use, which, when completed in sufficient number, will be offered to the profession, together with proper instructions for its use. We have strong hopes it will prove to be all that it promises; we have not witnessed any operation performed under its influence, but from our knowledge of the man and circumstances connected with it, we are led to expect something really useful in connection with it; we are assured that delays in the mechanical portion have prevented a demonstration of its effects in time for us to speak more positively in this number. He says he anticipates that in two weeks he shall be ready to demonstrate its power and offer it to the profession generally.

The editor of the Advertiser says:

_New Anæsthetic Agent._—The following communication of Dr. Branch, in relation to the application of a new anæsthetic agent, which he has used successfully in preventing pain in dental operations, will attract the attention, certainly of the humane and the suffering. Our personal knowledge of the matter is limited to a particular case, where a friend of ours was relieved, and where the effects appeared to warrant all that is claimed for it by Dr. B., as a uniform rule. From all we know, we are led to hope much
from it. But, it is one of these matters around which doubts cannot long
hang, for we conclude that when a man having a tooth pulled, declares that
the operation is accompanied by no pain, his testimony should be taken as
valid in all charity, though he be not under oath.

*Is it true?—Mr. Editor:*—Will you allow me to answer through your
paper the many questions propounded to me in reference to the success of
the new agent for producing insensibility to pain, or rather doing away with
the pain of dental operations. Those questions are—*Is it True? Will it do
it? Are you humbugging? &c.*

As to the first two questions, I will answer them by quotations from what
my patients have said of it. One lady, the first case I used it in, July 27th,
said of the extracting of six strongly rooted teeth: “all the suffering ex-
perienced was fear that it would hurt.”

A little girl six or seven years old, having a large double tooth extracted,
though persuaded in every way to tell exactly how it was, asserted and
reasserted that “it did not hurt a bit.”

Another, a lady, said “I did not know it was out.”

Another, a man, who was told if it succeeded he would be charged a fee
for its use, if not he would not, (this was a large, long-rooted wisdom tooth,) said when through, “I am perfectly satisfied, and willing to pay the extra
fee,” and paid it accordingly, notwithstanding he was under no obligations
to do so, except on the above conditions, &c. &c.

Those are the facts on which I built my advertisement of its application.
If any wishing operations performed by me, doubt the above, they can
have reference to the individuals or their friends.

As for humbug, I wish to live by other means; if this is *humbug*, I hope
my patients will always humbug me in the same way, by telling me and
others that I don’t hurt them when I extract their teeth.

As to its safety, there is no possible chance of doing injury with it, ex-
cept that which might result from ignorance and carelessness.

As to its discovery, I claim my own compound, apparatus and manner
of using it. Also, *as far as I know*, its first successful use in dental opera-
tions.

Very respectfully yours,

I. B. Branch, Dentist,

Galena, August 2, 1855.

No. 85 Main street.

Then follows another of a later date, which says:

“Painful as pulling teeth,” will soon cease to be a proverb. The new
application of Dr. Branch, in preventing pain in the most difficult cases of
tooth-drawing, works like a charm, as far as we are able to learn. A gen-
tleman called on us on Saturday, who had just been relieved by the process,
of a deeply rooted grinder, which had become highly inflamed, and like all
aching teeth, about “as painful as it could be.” He informed us that
scarcely a twinge of pain attended the extraction. That the application will
become universal in such cases, hardly admits of a doubt, and as it is per-
fectly safe, we do not see why it will not answer a similar benevolent
purpose in most cases of surgery.—*[Dental News Letter.]