wise adherent to the bone contained within it. The deceased (H. V.) was a soldier, apparently about thirty years of age, and, so far as I know, had never complained of unceasiness in the left hypochondriac region.

I kept this specimen of bone, with others which I had collected, intending to send them to the Army Medical Museum at Washington; but being ordered away suddenly for a campaign in Florida and Alabama, I was obliged to deposit, with other articles, my specimens in a quartermaster's warehouse at New Orleans. On my return I learned that the property, belonging to several regiments, which had been deposited in the above mentioned warehouse, had been removed to other buildings, and that my specimens had been lost.

Bony deposit in the spleen is of rare occurrence, I believe, and so I have thought this instance of sufficient interest to pathologists to be recorded.

J. T. PAYNE, M.D.,
Late Surgeon Volunteers.

New Orleans, La., July 18th, 1868.

UNSUCCESSFUL ATTEMPT TO CLOSE A FACIAL FISTULA.

Mr. Editor,—P. B. was stabbed in his left side with a two-edged dirk, an inch above the ilium, penetrating the descending colon just beneath the external wound. Fecal matter issued through the external wound ever afterwards. Nine months after the accident, this wound having partially closed, fecal matter burrowed in the cellular tissue, and ulcerated out at two points, half way between the spine and the opening made by the dirk. Under this time he hoped to recover, but now he felt that without some surgical operation he must be worn out with the discharge and die.

At the urgent solicitation of himself and friends, and after some delay and much hesitation, I consented to operate on him, and give him what slight chance there was for life. He was much emaciated from a whole year of confinement.

Dec. 17th, 1867, a year from the time of the stab, I operated on him by opening the abdomen, finding the hole in the intestine, and sewing it up. The perforation in the intestine was more than an inch in length, I took eight interrupted silk sutures, and cut the ends off close to the knots. He bore the operation well, and was more comfortable the forty-eight hours that he lived afterwards than he had been for a long time. He died from exhaustion.

A post-mortem examination showed no traces of inflammation, or attempt at repair.

N. L. Folsom, M.D.

Portsmouth, N. H., July, 1868.

Bibliographical Notices.


This is a remarkable book, as those familiar with the previous writings of the author will be prepared to believe. It contains accounts of discoveries startling alike to the pathologist, chemist, and physiologist. They are announced, too, in a manner quite as novel as their character is surprising. It has been the custom of students of natural science to substantiate each step claimed in advance of previous knowledge by carefully recorded observations, and to offer in support of new theories at least some data which can be generally accepted. This custom the author has largely ignored, so that in reading his book one is obliged to take his statements on faith, or else refuse to accept views so strange until they are corroborated by the observations of others. Unfortunately, Dr. Salisbury advances so rapidly from one discovery to another that it must be a long time before we shall obtain from observers capable of expressing an expert opinion confirmation of their claims, unless it be that some of them are so utterly at variance with firmly established doctrines that they will remain unnoticed by men of science.

It may be already forgotten to what an extent Dr. Salisbury has been a discoverer. It was only in 1862 that he announced that camp measles, then so prevalent in our armies, was caused by the presence of a straw fungus. The fallacy of this theory has been repeatedly shown by army surgeons who have had the largest experience with this disease, among the latest of whom is Dr. Bartholow, in Dr. Flint's recent volume on army diseases.

In 1866 he announced the discovery of the cause of intermittent and remittent fevers. The enthusiasm with which this was received is well remembered, as well as the claims to priority of discovery which were started in various parts of the world; of a discovery which rested on investigations unsatisfactory at the time, and which so far
as we know have never since been pushed to a conclusive termination either by Dr. S. or any other observer. Without discussing this question at length, it may be stated that it has not yet been demonstrated that the vegetable organisms to which the origin of the disease is attributed are not found in localities where intermittent fever does not exist; or that they ever produce the disease when transported to regions where this affection does not prevail. Such a theory in no way furnishes a satisfactory explanation of the long periods of incubation so characteristic of intermittent; of its return after prolonged intervals and removal to non-malarial districts; and of its non-contagiousness under all circumstances, notwithstanding the great production within and constant excretion from patients of the very germs which are claimed to be the origin of the disease. Furthermore, persons suffering with this affection here do not appear to excrete any such organisms as Dr. Salisbury has described. The whole theory seems to rest at present upon evidence quite as unsatisfactory as that with regard to measles which preceded it.

In 1866 he also published an article on the functions of the spleen and some remarkable peculiarities in some of the elements of the blood, which will again be referred to below.

In 1867 he published an account of three new chemical varieties of rheumatism caused by the presence in the blood of crystals of oxalate of lime, cystin, and phosphates, which can be noticed only in the briefest manner. Oxalic acid, as is well known, stands in intimate relationship to uric acid, and it needs but a slight jostling of the equilibrium which exists between the successive processes by which the metamorphosis of our tissues is effected, such as disturbances of respiration or digestion, to convert effete materials into the former in place of the latter. So readily is this substitution effected, that it can scarcely be looked upon as abnormal, at least not as a serious deviation from the law. Whenever oxalic acid is formed in our economy, there it must necessarily be converted into oxalate of lime, which in the blood is held in solution by other salts there present. No observer has ever seen it in the blood in its crystalline state in all the innumerable examinations of this fluid hitherto made. Dr. S., however, says the blood is full of such crystals in one variety of rheumatism, and that its presence there is the specific cause of the disease, the incubative stage of which, according to him, may last "from a few months to many years." Oxalate of lime, he states, is "formed by the diseased parent blood gland cells, and being insoluble in the fluids of the body accumulates in the blood." With this sentence is set aside the accumulated observation of the most renowned physiologists and chemists of modern times in relation to the formation of this substance. When opinions clash in this irreconcilable manner, we must fall back upon the character and reputation of the observers in determining which we are to accept.

The same may be said with regard to his cystinic rheumatism. Cystin, according to the most recent investigations of Gorup-Beszanez, Kühne, Robin and others, is known only as an extremely rare ingredient of urinary calculi and sediments. It has also been extracted from the kidney and liver in one or two instances and in very small quantity. As to its physiological relations we are in almost complete ignorance. It has never been observed in the blood or perspiration by any reliable observer. It has a distinct crystalline form and well-marked chemical reaction. Now Dr. S. would have us believe that this very rare substance occurs in the blood "often in great abundance," where it produces puerperal mania in pregnant women, and is a frequent cause of insanity in young ladies. It produces also the enlarged joints of chronic articular rheumatism. These views seem to be based upon the discovery in the blood and excretions of certain granular or semi-crystalline bodies which he calls cystin. He does not say that he has analyzed them, but they certainly in no way resemble the crystalline forms of cystin known to all the rest of the medical world, and without the stamp of the chemist cannot pass as such. It is upon such evidence that his new varieties of rheumatism rest.

In January, 1868, an article from his pen appeared in the American Journal of the Medical Sciences, entitled "Description of two new Algoid Vegetations, one of which appears to be the specific cause of Syphilis, and the other of Gonorrhoea." One of these plants, called crypta syphililitica, although of the very lowest order of vegetation, is possessed of wonderful faculties, as will be seen, if we believe with the author that while being the cause of the primary sore, as well as of the succeeding constitutional symptoms, it may yet remain in the system for years or a lifetime without producing serious trouble, or may be transmitted by a father, once syphilitic, to and
be formed in the blood of the wife and children many years afterwards without producing upon them any visible impression. It seems to us that this subject scarcely deserves the serious attention of the reader or reviewer as thus presented to them, for not a shadow of evidence is offered that the forms seen and figured by Dr. S. have anything to do with the cause of syphilis or gonorrhoea.

In the next number of the same Journal there is another article on the parasitic growths connected with the urine, in which a dozen or more of new cryptogamic plants are described, all of which are supposed to be more or less productive of disorders of the urinary and genital organs. Dr. Salisbury is certainly fortunate as a discoverer. An extensive acquaintance with the microscopic appearances of urine in health and disease will satisfy any observer of the frequent occurrence of fungoid growths in it in warm weather, and of their multiform appearance, but we doubt if one conversant with the latest phases of cryptogamic botany would think of dividing them up into individual species as Dr. S. has done, or consider it necessary to look elsewhere for their origin than to the spores with which terrestrial space is crowded.

With this passing glance at the announcements of our author during the last two years, which is offered in no way as a thorough criticism of their character, we shall be prepared the better perhaps to understand the nature of the volume before us.

The first chapter treats of the microscopic examination of the blood, and has already appeared in the New York Journal of Medicine of the present year. The author begins by stating that he has already made over thirty-five thousand individual examinations, that an hour may frequently be spent with profit in exploring a single drop of blood, and that not one microscope in fifty of those in present use is suited for this kind of study. He then gives a list of "some of the conditions, states, and pathological products to be sought for in blood," which amount to sixty-seven in number, without counting "many other things to be sought for in pathological blood." To attempt to criticize the details of this list would be to review the whole field of modern physiological and pathological chemistry, so that only the few points which are enlarged upon in subsequent pages can be noticed here.

First, some remarks upon the fibrin in the blood of health and disease claim our attention. They are introduced with the strange announcement that "there is no possible doubt that the fibrin filaments exist ready formed in the blood stream, and that these filaments are developed from fibrin cells, the nucleus or yolk of the fibrin cell forming the blood disc, while the portion of the cell outside of the nucleus is spun into a fine fibrin thread." Now physiological chemistry teaches us that the substance we are familiar with in blood outside the body as fibrin may be made to assume a filamentous or gelatinous structure while coagulating as we please to change its external surroundings; that we may prevent its formation altogether if we choose; and further, that in all probability there does not really exist ready formed in the blood any such substance as fibrin, but two albuminoid bodies, called by the latest German observers Paraglobulin and Fibrinogen, by the French collectively Plasmine, which after escape from the vessels by chemical action unite to form for the first time the Fibrin, and which are prevented from such union, or in other words coagulation, within the economy by the vital influence of the walls of the blood vessels. This is to a certain extent a new doctrine, but one accepted by such men as Virchow, Schmidt, Kühne, Brücke, and by Robin in his recent "Lecons sur Les Humeurs." Do we want any higher authority of the present to confirm the opinion always and universally held that blood does not coagulate until it has left the body?

But what are the grounds on which Dr. Salisbury's statements are based? He says "by a little practice, the eye can begin to explore a drop of blood under the microscope, in one second after leaving the blood vessels. * * * * In a few moments will be noticed faintly delineated filaments, crossing and recrossing each other, forming a mesh-work. * * * * This net-work of organized fibrin gradually loses the almost perfect transparency it has in the blood stream, and becomes little by little more and more opaque and visible in outline, till in the course of five or ten minutes after it is drawn the net-work of threads reaches its maximum opacity, the filaments being to the educated eye well defined." Is any comment necessary? Can any one but Dr. S. believe that those filaments existed prior to "the few moments" which elapsed before he saw them? Is not something more than this required to convince us that our physiology is in error on this point? Such being the foundation of his premises, it would be useless to more than mention the pathological deductions he has drawn from
them. Upon the size of the meshes formed by the interlacing of these filaments in the vessels and the consequent free or obstructed passage through them of the morphological elements of the blood, depend in great measure, according to him, such morbid conditions as rheumatism, phthisis, thrombi and emboli, &c.

The second chapter is entitled "the blood disc, a vehicle for transmitting nerve food," and its object appears to be to show that the blood discs carry cholesterol, or nerve fat as he calls it, to the nerve tissue, and that the excess or diminution of this substance in the blood, as shown by microscopic examination, gives rise to some very ill defined symptoms, which he calls cholesteremia. Cholesteremia, so far as we know it, is not really a fat, but plays no doubt an important role in the economy, as it is found in so large proportion in the nerve tissue, and possibly does not exist in the blood at all, although its elements do. Our knowledge of its physiological or pathological relations, very small before, is not enlarged by what is here written.

The remainder of the volume is devoted to the description of cryptogamic vegetations found in both healthy and diseased blood. Two species are represented as always occurring in blood both healthy and diseased, though one of them is stated as difficult of demonstration until the blood has been allowed to stand from one to three days at a temperature of 60° to 70° F. in a bottle, conditions which certainly would not remove the scepticism of one predisposed to deny the existence of such organisms3 in that fluid. If we accept the statement as a fact, which is in no way improbable, it does show that the blood may contain cryptogamic growths without in any way affecting the health; an important point to be remembered in our study of this subject.

An algoid vegetable, Crypta carbunculata (Salisbury), is then described and figured as the cause of carbuncle. The author states that whenever he has found this plant in the blood, the patients have been more or less affected with carbuncles, and the carbuncular sloughs have also contained the same kind of filaments.

We now come to the two most important chapters of the book, that is if the opinions therein stated are correct. The first contains the "description of a new vegetation having fungoid and algoid phases of growth found in the blood and eruptions of variola and vaccina, and which appears to be the specific cause of these diseases." This plant Dr. S. describes under the name of IoS variolosa vacciola, and is represented as possessing two phases of growth, a fungoid, which he calls I. variolosa, and an algoid, called I. vacciola. Both forms are met with in the eruption and blood of smallpox according to his statement, and if the vegetations of a smallpox pustule be inoculated into a person who has never had variola, the same contagious disease, in a modified form, and the same vegetation is produced. If, however, the same be inoculated into a cow, a pustular disease is produced, but the eruption is found to contain only the algoid stage of growth, the IoS vacciola. The fungoid phase is not produced in the cow. If now this matter from the pustule of cow-pox is inoculated into the human subject, a pustule like vaccina and variola is produced, but it contains only the algoid stage like the cow-pox sore. The vegetation of the vaccine pustule like that of cow-pox is supposed to have no power of penetrating the epithelial envelope of the body, and is therefore only transmitted by inoculation. The other form, that of smallpox, is supposed to be an active contagion, the spores of which "falling upon epithelial surfaces that have not had the impress of immunity stamped upon them by a previous invasion, penetrate to the deeper tissues, and pervade the entire organism."

This is the theory of Dr. S., stated nearly in his own language, and in his opinion it throws "a flood of light upon some singular features of the disease." Not a single experiment is referred to, nothing is stated as to the number of cases of the disease he has examined, of the number of persons or cows inoculated by him with smallpox, or of the number of cows affected by cow-pox observed to teach the truth of his theory, or of the method of conducting his investigations. We are expected to believe without the exercise of our judgment. But if we accept every statement as well established by faultless and repeated experimentation, and admit that one and the same plant is always found in smallpox and its allied affections and under no other circumstances, would this satisfactorily explain all that we know of their manifestations? How, for instance, does it account for that "modified form" which Dr. S. says is produced by inoculating the vegetation of a smallpox pustule into a person who has never had variola, when the same vegetation according to him is produced as when the disease is taken by contagion? How can vaccination protect against variola, or in other words, how can the growth in the blood of one
plant at a previous period prevent the growth of the same or of another plant at a remote period in this ever-changing fluid? These are but a few of many questions to which Dr. S. makes no allusion in his book.

But Dr. Salisbury is not the only person who claims to have discovered the cause of smallpox in the presence of vegetable organisms in the system. Prof. Hallier, of Jena, has been working in the same field, and has just published the results of his investigations in a volume of 80 pages, entitled "Parasitologische Untersuchungen bezüglich auf die pflanzlichen Organismen bei Masern, Hungertyphus, Darmtyphus, Blattern, Kuhpocken, Schafpocken, Cholerax., &c." Prof. Hallier seems to possess, in the first place, the requisite intimate knowledge of cryptogamic botany to be able to distinguish the old from the new in these low forms of plant-life, and the plan he has pursued in his studies is in theory the correct one so far as it goes, though in practice open to grave errors. He finds in the fluids and excretions of these diseases certain forms which he considers as belonging to vegetable life, but in so low a stage of development that the microscope cannot distinguish one from the other in all cases, or of what nature plant or individual species or genus they are the representatives. These forms he calls micrococcus, and they may be so small as to be scarcely recognizable when magnified 1000 diameters. In order to determine, therefore, their individuality, he cultivates them in apparatus isolated, as he thinks, from all connection with the air, and upon other substances as milk, fruit, cork, starch, eggs, &c., and then examines the forms developed as he assumes from such primitive germs. These mature forms he regards as the plants which cause the diseases in which the micrococcus is found. The error of this process lies in the impossibility, one may almost say, whatever be the precautions taken, of destroying similar germs which preexist in the substances used in such experiments, as the history of the modern controversy concerning spontaneous generation has shown.

The experiments of Prof. II. are important and highly interesting, but it will be impossible to give an account of them here. What is of immediate interest to us in this connection is to mark how two observers, working independently of each other and both claiming to have discovered the cause of smallpox, arrive at the same end, as of course they must, if they have discovered its cause. We have seen that Dr. S. describes this cryptogam as a new plant and calls it "variolosa vacciola," but Prof. II. tells us that the micrococcus of vaccine lymph develops into Torula rufescens, a common mould, while the corresponding form from variola lymph exhibits, when mature, a modified growth of the same fungus, neither of which, however, are identical with or in any way resemble that figured and described by Dr. S. Here, then, two observers discover two distinct causes of smallpox. They cannot certainly both be right in their inferences; but one of them has given us proper scientific data by which we may judge of the merits of his investigations, the other leaves us wholly in the dark. It is to be feared that we must yet wait for the discovery of the cause of smallpox.

The concluding chapter of Dr. Salisbury's book is on the "Vegetation in Typhoid Fever which appears to be the specific cause of the disease." This, he states, is a peculiar minute algoid plant, which develops upon all the epidermic and mucous surfaces and penetrates to all the tissues of the body, flourishing with special luxuriance in the glands of Peyer, and increasing "until the organism finally becomes so poisoned and vitiated, that it seems to be no longer able to propagate the vegetation that has been the specific cause of all the trouble." This plant he calls Biolysis typhoides. If we turn again to Prof. Hallier's pages, we find that he has also been investigating the cryptogamic nature of this affection, and has concluded, from the cultivation of the spores or micrococcus he finds in the blood and intestinal evacuations, that it is caused by two fungi, Rhizopus nigricans and Penicillium crustaceum, both well-known moulds. These plants also resemble in no way that described as the specific cause of typhoid by Dr. Salisbury.

The most that can be claimed for these so-called discoveries is the presence in the blood and tissues of persons affected by certain diseases, of forms of cryptogamic vegetation. Even should it be subsequently demonstrated that their presence under such circumstances is constant, and that they never occur at other times, it would not follow necessarily that such plants are the causes of these diseases; for according to the observation of Dr. S., there are two such plants always existing in the blood without affecting its condition. Why may not the new forms be due simply to a chemical change in the nature of this fluid during disease, for instance, for we know how materially the growth of such plants is in-
fluenced by variety of soil or food? The whole subject is one of great importance, and will very likely some day be found to comprehend the laws which govern the maintenance of some of our most fatal diseases; but it is still almost an unknown region, the secrets of which are to be reached not by hasty generalizations, but by patient observation and self-sustaining experiment.

J. C. W.


During the past year extensive additions to the buildings of this Asylum have been made, making it now one of the most complete in its arrangements, as well as one of the largest institutions in the country. Five hundred patients can be comfortably accommodated; and this is as large a number as it is deemed expedient to collect under the care of one institution, in view of the practical details necessary in the oversight, proper classification, medical treatment, employment and amusement of that number of insane persons. As a large number still remain unprovided for in New Jersey, a new hospital is to be built and located in the eastern part of the State.

The number of patients in the asylum, Nov. 30, 1866, was 409: 205 males and 204 females. During the year, 212 were admitted, 85 males and 127 females; 171 were discharged—72 as recovered, 54 improved, 8 improved, and 37 died; leaving in the asylum, at the close of the year, 450—200 males and 250 females.

From the report of the Superintendent, Dr. H. A. Buttolph, we learn that since the opening of the asylum, May 15, 1848, 2986 patients have been admitted. Of these 1178 have been discharged recovered, 818 improved, 96 unimproved, 9 closed, 4 not insane, and 436 died.

As the Legislature of the State had not decided to build another hospital when this report was written, the Doctor argues the necessity for additional accommodation, and in doing so quotes his remarks made at the previous meeting of the Association of Medical Superintendents of American Institutions for the Insane, while the subject of making provision for the chronic insane was under consideration. Among the most important of these remarks, are the following:—"I believe it to be the duty of the National and State governments, assisted by such voluntary contributions as wealthy and benevolent individuals may offer, to provide hospital and asylum accommodation for all insane persons who need and desire them." "The advantages of the district system of providing for all classes of insane are, first, the least possible exposure, fatigue, and expense in removing patients to and from the institution; secondly, the greater readiness with which private individuals and public authorities living near them avail themselves of their benefits; and last, though not least, the greater ease with which the friends of insane patients can visit them when it is deemed suitable for them to do so."

The general operations of the asylum, it is stated, have been conducted with average success and satisfaction; while nearly one hundred more persons have received its benefits than in any other year.

C. K. B.

Medical and Surgical Journal.

Boston: Thursday, October 1, 1868.

The Diseases of Children.

Few physicians have forgotten what doubt and perplexity in their student days and in their earlier years of practice, obscured their diagnosis of children's sicknesses. To most of us, for aught we were taught, or for aught we could gather from the bewildering compilation called our textbook of such maladies, the illness of little children remained a mystery, a sealed book, to be opened with difficulty, and to be read only by the confused light of our own small experience. To many of us, still, this class of affections remains the most difficult to master; and it has been well said that the physician must have children of his own, in order to comprehend the multiform phases and changes of mind and body among the children of his patients.

Any new work, then, that can throw additional light on such a subject must be doubly welcome; and we have seen none since the treatise of West, which pleased us more than the volume before us.* The French schools have produced the best clinical teachers on this subject, owing to the