

## BIBLIOGRAPHIC NOTICES.

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*Præcos Medicæ Universa Præcepta.* Auctore JOSEPHO  
FRANK. Pars III. Vol. II. Sect. 1.  
*Continens Doctrinam de Morbis Tubi Intestinalis, quam  
exposuit.* FRED. AUG. BENJN. PUCHELT.

WE have received this portion of J. Frank's great work, with a prospectus of the whole now in the course of publication, and consisting of four parts, each part forming several volumes.

There can be little doubt, that, when completed, this will be the greatest, if not the best book on the practice of medicine extant. Its prodigious research will alone render it invaluable to the medical scholar. We fear that few students or practitioners of these degenerate days will be likely to make the ten or twelve bulky volumes of Latin their manual of physic; but they must have a place in every good library, and will meet with few that can bear comparison with the mass of practical information and extensive learning that they contain.

The contents of the portion submitted to our examination are Congenital Deformities, their abnormal form and situation; Enteritis; Suppuration; Ulceration; Wasting and Perforation of the Intestines; Induration; Tubercle; Melanosis; Fungus; Scirrhus and Cancer; Morbid Adhesions; Polypus; Œdema; Hydatids; Stricture and Narrowing of the Intestines; Intestinal Worms; Flatulence; Colic; Constipation; Ileus; Diarrhœa; Dysentery; Intestinal Hæmorrhages and Hæmorrhoids; and Cholera.

On these subjects countless authorities are adduced, and prove the Author, or his Editor, to be not only familiar with the standard medical writers of Europe, including our own Abercrombie, Baillie, Cheyne, Bright, Pemberton, Stokes (who, however, appears in his German dress); &c. &c. &c., but also well read in the periodical medical literature of this country and America. That these references are made from actual research

we have satisfied ourselves, by examining the works referred to, in several instances, and finding the very expressions of the authors employed in the quotation. This obviously gives to such references a value not always possessed by *professedly* learned books, and which is, moreover, increased by the judgment shown in the selection of points worthy of notice—as in the allusions made in two instances to Dr. Stokes, one referring to his valuable observations on the connexion between gastro-duodenitis and jaundice; the other, in the following passage, to his diagnosis of enteritis by increased pulsation of the abdominal aorta.

As such a work as the present does not admit of analysis, we shall merely take an example at random, from which our readers may form some opinion of its merit. The history and symptoms of mucous enteritis are thus sketched. We omit the references.

Mucous enteritis, i. e. inflammation which has its seat in the mucous membrane of the intestines, its follicles and glands, could scarcely have been rightly known before the time of Bichat; though it may be conjectured that to it must have belonged that which has been mentioned by the name of latent or mild inflammation of the intestines. Ludwig observed in his dissections of those destroyed by continued, malignant, or exanthematic fevers, that almost always some portions of the intestines appeared inflamed without any previous violent pain, and Morgagni adds, that in short fevers the intestine was inflamed, the villous coat only is no doubt meant. But subsequently Pinel, Perratean, Petit, Pemberton, Jackson, Crampton, mention the same thing in explicit terms. Ere long Broussais presents himself, who, indeed, correctly made the same observation as Ludwig; but thence drew the wrong conclusion, that this special inflammation was the source and centre of the malady wherever this might have its seat. There was subsequently much debating upon this subject among the French writers; and that the dispute might be settled, the Academy of Paris proposed their prize question (*Existe-t-il toujours des traces d'inflammation dans les viscerès abdominaux après les fièvres putrides et malignes?*) Abercrombie, also, with Gregory, Fosbrooke, Marsh, Bright, De Pommer, Bischoff, Leper, Becker, Horner, Christie, Wisbach, Kohler, have treated of the same subject. On the morbid anatomy, Billard and Gendrin deserve the highest praise.

Mucous enteritis either is confined to the mucous membrane only, or extends to the submucous and muscular coats; but, if it has its seat in the latter, it manifests the signs of general enteritis before enumerated. If, however, the villous coat only is affected, very different and milder symptoms appear.

The pains which mucous enteritis excites are not severe, nor are they much increased by pressure on the abdomen, but they come on in fits frequently, and resemble colic. The fever, when excited by the inflammation itself, remits, and is occasionally milder, and has short exacerbation; an increased pulsation of the arteries of the abdomen is felt (Stokes, *Vorlesungen*, p. 65). The tongue is covered with a white mucus; it is red in the centre, or at the apex, and the papillæ are prominent. The patient occasionally vomits; hiccup sometimes occurs. The substances taken for food excite a great feeling of heat, and a sensation as if they would pass rapidly along the intestinal tube. The alvine discharge is often infrequent where the mucous membrane of the ileum is inflamed, but constantly liquid and frequent when the mucous membrane of the great intestine is inflamed, at times being copious, at times scanty, sometimes attended with tenesmus; and mucous, serous, or bloody fluid fæces are discharged in some cases from the commencement of the disease; bile also healthy or depraved, and food and drink, little changed, are seen in the discharges.

Very frequently, when the disease runs its course favourably, coagulable lymph is excreted, appearing like shreds of boiled maccaroni; but, when the inflammation has been widely diffused, gangrene, ulceration, or peritonitis supervening, the patients not unfrequently die.

*A System of Clinical Medicine.* By ROBERT JAMES GRAVES, M. D., M. R. I. A., one of the Physicians of the Meath Hospital and County of Dublin Infirmary, formerly Professor of the Institutes of Medicine, Honorary Corresponding Member of the Royal Medical Society of Berlin, the Imperial Medical Society of Vienna, and of the Medico-Chirurgical Societies of Hamburg, Tübingen, Bruges, Montreal, &c. &c.

IN medical as well as general literature there is a class of works which always rank highly as books of reference, from the circumstance that their materials were originally published in separate parts; and that when the Author had full time for reflection, and the accumulation of new facts, he brought forward the whole in a combined form. Such are the standard works of Abercrombie, Andral, Lallemand, and others; and the present volume is no inconsiderable addition to this important list.

“Having been,” says Dr. Graves, “for many years, engaged in giving clinical instruction at the Meath and Sir Patrick Dun’s Hospitals, I thought it to be my duty occasionally to publish the results of

my observations in the form either of detached essays, or successive series of lectures; the former were printed in the *Dublin Hospital Reports*, and the *Dublin Medical Journal*, the latter appeared in various English periodicals, but chiefly in that excellent publication, the *London Medical Gazette*. Many of the detached papers were subsequently translated into French, German, and Italian, and several courses of the lectures were published by Doctor Robley Dunglison, in a separate volume at Philadelphia. This has encouraged me not only to continue my exertions in the cultivation of practical medicine, but to revise what I had written and compress the whole within the limits of a single volume. It is necessary to mention that the lectures were all originally delivered *extempore*, and were printed from notes taken by a short-hand writer. The reader being made aware of this circumstance, will kindly make due allowance for the many imperfections of style, and the redundancy and repetition which occur but too frequently in this work. In revising the whole, I have been at more pains to improve the substance than to polish the surface, and have rigorously excluded every assertion and conclusion which my subsequent experience has not verified."—*Preface.*

The introductory lectures, given at the commencement of the work, embrace many important views on medical instruction and general pathology. The Author dwells strongly on the importance of a proper system of clinical instruction, and refers, with a just pride, to the success of the German method, which he was the first to introduce into this country, more than one-and-twenty years ago, from which period we may date the advance of Dublin as a school of practical medicine. It is true, that we had organized schools of medicine, surgery, and anatomy, and that clinical lectures were delivered by authorized teachers; but it is equally true, that these means here, as elsewhere, had failed in making the student of medicine what it was most desirable he should be—namely, a practical physician; one who, by a proper training, had been taught to think correctly, to observe accurately, and to acquire that confidence in himself, only to be obtained by learning the phenomena and combinations of disease at the bedside of the patient.

"The importance of clinical instruction is so much felt in Germany, that each school has three distinct medical clinics attached to it, by which means the labour of teaching is divided among the professors, and the number of students attending each is diminished. There is one clinical hospital for the treatment of acute diseases, and another for chronic diseases, while a clinical dispensary is devoted to the care of extern patients. The pupils are divided into two classes—the more advanced, who get the care of patients, and the junior students, who merely look on and listen. When a patient is admitted, his case is assigned to one of the practising pupils, who, when the physician is visiting the ward, reads out the notes he has taken of the patient's

disease, including its origin, progress, and present state. This is done at the bedside of the patient; and before he leaves the ward, the physician satisfies himself whether all the necessary particulars have been accurately reported by the pupil. After all the patients have thus been accurately examined, the professor and his class proceed to the lecture-room, and a list of the patients and the practising pupils is handed to the professor; the cases admitted that day are first inquired into, and the pupils are examined concerning the nature of their disease, their probable termination, and the most appropriate method of treatment; each student answering only concerning the patients entrusted to his special care. During this examination, the pupil's diagnosis and proposed remedies are submitted to the consideration of the professor, who corrects whatever appears to be erroneous in either, and then the student retires to write his prescriptions, while the rest of the cases and pupils undergo a similar examination. At the conclusion, the prescriptions written by the students are read out in order by the professor, who strictly comments on and corrects any inaccuracy or inelegance they may contain. When the prescriptions have been revised and corrected, they are signed by the physician, and handed to the apothecary to be made up and distributed. In some clinics, the price of each medicine is affixed to the bottle or box containing it, in order that the students may become acquainted with the comparative expense of various prescriptions, and may thus be enabled, in private practice, to accommodate, as far as is possible, the expense of the remedies to the circumstances of their patients. The clinic for extern patients is conducted on the same principles; patients who are able to attend, are examined at the dispensary; those who cannot leave their homes are visited by the senior practising students, who always seek the advice of the professor when the case is urgent, or the treatment doubtful. Nothing, gentlemen, can be better adapted than this plan of clinical instruction for the improvement either of the beginner, or of the more advanced student; this daily deliberation and anxious discussion concerning the nature and treatment of each case, is peculiarly interesting, and serves to accustom the beginner to habits of accurate examination, whereby he is taught to interrogate nature for himself, and learns the history and treatment of disease, not from books and descriptions, but from direct observation. The advantages gained by the practising pupils are too obvious to require comment; being obliged to give reasons for every plan of cure that they propose, they are accustomed to a rational and careful investigation of disease; and enjoying the most important of all advantages the early correction of their errors—they commence private practice with a sufficient degree of experience to render them unlikely to commit any very serious mistakes.

“It is evident that, according to the German method, no regular clinical lectures are necessary, as the pupil becomes accurately acquainted with the physician's views of each case, and no step is taken in the treatment without the reasons for it being given. This is the best sort of clinical lecture; the pupils have their doubts solved, and their erroneous views corrected, while the professor is enabled to mei

tion, as the disease proceeds, every thing which he thinks is illustrative of its nature.

“NOTE.—Eleven years' experience enables me strongly to recommend the method of instruction pursued in Germany. Since my appointment to the Meath Hospital, I have had extensive opportunities of observing its good effects. Not a session has elapsed without furnishing proofs in its favour. This system, however, at first met with much opposition, and its introduction was ridiculed in every possible manner; even now it may be doubted whether its well-wishers are as numerous as might be expected. It is still opposed by several narrow-minded persons, whose opinions have much weight with the pupils.”—pp. 9, 10.

Ten years have elapsed since the lecture, from which we have quoted, was delivered; and during that time the system has produced the most excellent results; indeed, nothing is more remarkable than the superiority of the clinical pupils to their fellow-students, who unfortunately have been permitted to go on in the old way. The clinical student, under the system first pursued at the Meath Hospital, becomes speedily a practical physician; his eye, touch, and ear are all educated; no mean knowledge, yet one unattainable by books or lectures; he learns the real difficulties and advantages of the healing art; he sees disease, not as nosologists have described it, but presenting infinite varieties and complications. He learns the great art of mental combination—of grasping *all* the phenomena of a case, be they physical or vital, or having reference to time, and of thinking justly upon them. In this way his *medical mind* is formed, the one thing needful to the good physician, and which no course of reading, no multiplication of lectures, can ever create. But he learns more.

Surrounded by suffering, disease, and death, in the fever ward, and in the dissecting-room, he learns to cultivate the noblest qualities of the mind and heart—modesty, charity, patience, caution, promptitude, and courage; and thus qualified, he goes forth on his mission of good, ready to combat disease in whatever clime his fate may throw him, and, in his turn, to become a teacher and disseminator of all that he has learned. We have already spoken of the responsibilities of the clinical teacher. The following specimen of right feeling and manly eloquence we gladly transfer to our pages:

“I would not be understood here as wishing to depreciate any department of human knowledge. Far be it from me. Besides, the attempt would be useless. But I am anxious that you should concentrate all your energies on the proper objects of medical pursuit, and devote the largest share of your attention to those acquirements which will render you good practitioners. I have seen students led astray by false notions, wasting half of the time which should be spent

in hospital, and by the sick bed, in wandering through the fields on botanical excursions, or working in the laboratory, engaged in the solution of some unimportant problem. Now this is not what will teach them to relieve suffering, and cure disease. When I look round me, and behold so many young gentlemen entering upon an honourable and important profession, I feel that my responsibility is great. I consider you all as instruments of good or evil, and cannot help being conscious that I should be guilty of a great crime, did I not use every means in my power to render you able and efficient practitioners. The teacher of clinical medicine, gentlemen, occupies in every nation a post of heavy responsibility. But when he happens to preside over the medical education of those who resort to the wards of a metropolitan hospital—when the metropolis is a British one, and the hospital destined to send forth annually practitioners to every quarter of the globe—to North and South America, to New Holland, to the Cape of Good Hope, to the East and West Indies, and the countless isles which, in either hemisphere, are visited by the British flag, then indeed does that teacher become himself an instrument of good or evil, to an extent which it is fearful to contemplate.

“He who gives instruction to a clinical class in Berlin, Stockholm, Vienna or Paris, has much to answer for, if he discharge not his duties with zeal and diligence. Yet if he fails to make his pupils good practitioners, their errors, however deplorable, are circumscribed within comparatively narrow bounds, and limited in a great degree to their own countrymen. But the British teacher sits in the centre of a circle far wider than Sweden or Prussia, Austria or France; his pupils are to be met with practising in every climate, exercising their art in almost every habitable region of the globe and dispensing the blessings of health to all races of mankind; to the hardy white settlers of Canada, the aboriginal red-skins of North America, the negroes of Jamaica, the Hottentots and Caffres of Africa, and the countless tribes of Hindostan.

“In truth, gentlemen, the British teacher of practical medicine exercises an influence without parallel in importance and extent, and his opportunities of benefiting or injuring his fellow-men are incalculably great. If he neglects his duty, if he teaches erroneously, his negligence and his errors in practice are multiplied indefinitely, by means of those whom he ought to have better instructed; the scene of his guilt—for it deserves no better name—becomes fearfully enlarged, for there is no country so remote that it may not contribute victims to the incapacity of his pupils. But if, on the contrary, he works with zeal and diligence; if he labours conscientiously and perseveringly in performing the important task he has undertaken, a compensation awaits him, to which scarcely any member of any profession can attain. Can any reward exceed in value the reflection that he has assisted, materially assisted, in imparting practical knowledge to multitudes of enterprising young men, who, year after year, leave our hospitals to engage in the sacred duties of the medical profession, throughout the world? Is it not a high privilege to be enabled to combat death, and conquer disease, as it were by proxy, in so many different

localities? Can man enjoy a purer, prouder, more gratifying reflection? When I hear that a favourite pupil who has acquired a solid stock of practical knowledge in this hospital, has settled in any particular town or district, I cannot help feeling, on the part of my colleagues and myself, that we have been the humble means of conferring a blessing on the people entrusted to his care; and I cannot refrain from congratulating myself upon holding a situation which multiplies a thousand fold our efforts to be useful, and enables us to stretch forth our hands to heal men of all nations and languages. The hero and the despot may extend a sovereignty over distant regions—may exert an unlimited control over millions of vassals—may dispense honours and rewards, or inflict punishment and death; they may, like Alexander, grieve at the narrow limits of a conquered world, and sigh for other scenes of glory;—but they cannot chase away pain; they cannot bid the burning thirst to cease, or give back repose to the sleepless; they cannot impart feeling or motion to the paralysed, or sight to the blind; and above all they cannot imitate that almost godlike function of the healing art, by which man is enabled to recal to his fellow-man reason long banished, and restore to society the hapless victim of insanity.”—pp. 39–41.

Well do we remember the system of teaching, or rather not teaching, previous to the period alluded to. The hospital officer walked rapidly round the wards, followed in some cases by a clerk; with whom he communicated in a low voice, as if ashamed that his prescriptions should be known to the crowd of students, who followed mechanically and uselessly in his wake—worse than uselessly, for they only learned to be careless, slovenly, and supercilious. The physical examination of the patients was conducted in silence; no observation escaped which could tend to instruct; and, if an unfledged pupil had the folly to ask a question, he was met with a stare or a sneer. Such was the method everywhere adopted, when Dr. Graves laid the foundation of a new system, and effected the most important revolution in medical instruction which this country has seen: he was a reformer, and, as such, had old prejudices to contend with, and personal animosities to encounter. He taught, that as medicine was an honourable profession, its students should be treated with courtesy; that, as these students paid for instruction, something more should be given them than the signature of a certificate; that the mere seeing a sick man was of but little utility to him who sought to learn the disease, and its mode of cure; that the teacher should make no secret either of his knowledge or his ignorance, but ever combine the character of a learner and instructor; and, finally, he proclaimed, for the first time, that the clinical teacher had numerous and heavy responsibilities; that the talent had been entrusted to him, not to be buried in the earth, but to be taken to the money-changers, and, at his master's call, to be returned with interest.



We cannot leave the introductory part of the book without observing, that the new *Chemical Physiology and Pathology* by Professor Liebig, finds but little favour with the Author. After quoting the opinions of Liebig, on the *modus operandi* of contagion and miasms, he observes:—

“Such is Professor Liebig’s theory of poisoning and contagion—a theory which, though it comes to us recommended by the abilities of the first organic chemist of the age, and sanctioned by his anonymous but able reviewer in the *Quarterly*, can nevertheless be easily proved to rest upon almost as many assumed as *proven* facts. Thus how can Liebig so positively assert that there is no poisonous substance in the fatal sausages? True it is that no chemist has yet insulated such a substance; but Liebig knows better than one else how profoundly concealed any particular animal principle may be by being mixed with a great variety of other animal principles. Thus how long did sugar, in the blood of diabetic patients, elude the searches of chemists? and yet they were looking for a principle with whose chemical qualities they were already accurately acquainted. How much more difficult of detection must the poisonous principle be which exists in so compound a body as a Würtemberg sausage? Besides what chemist was ever sure that he was actually analyzing a poisonous sausage? Here a special difficulty lies, for hitherto there has been discovered no *a priori* method of distinguishing a poisonous from a wholesome sausage until both have been eaten, that is too late for analysis. How long has the poisonous quality of ergot of rye been known? and yet the principle to which its effects are owing, though often sought, has been only lately insulated.

“It is obvious, therefore, that Professor Liebig’s main example of his new pathological explanation is not by any means *proven*, and consequently it is unnecessary to follow him into the regions of fancy where he has been enticed by a specious and seductive analogy. Pathology will cease to be a science when the study of facts gives place to such reveries as the above cited passage contains—relative to miasms, contagions, mild cases, severe cases, diseases occurring but once in life, &c., &c. And yet I am sorry to say that one of our most distinguished lecturers, Dr. Watson, has, in the number of the *Medical Gazette* for July 29, 1842, fully adopted these opinions.

In order to give the reader some idea of what Dr. Watson considers to be ‘*distinct conceptions*,’ and ‘*lights supplied by a theory*,’ I beg leave to quote from the Doctor’s lecture the following paragraphs:—

“‘Moreover, the light supplied by this theory gives distinctness to our conceptions respecting certain deviations from the regular course and type of these diseases; which deviations are not uncommon.

“‘Thus the symptoms which precede and usher in the eruptive are sometimes slow, halting and irregular in their progress; appear and then recede, and reappear, so that we are in doubt what is about

to happen, until at length the disease declares itself in its decided and authentic form.

“ We may suppose this to depend upon some tardiness or interruption of the process, whereby the virus is (to use the ancient term) concocted.

“ Again, the series or combination of symptoms that mark the specific disease is sometimes, as I stated before, *incomplete*. We have the eruption of measles without the catarrhal symptoms; the sore throat without the rash, of scarlet fever. And experience has found that, where the malady is thus imperfectly developed, the protection it confers against its own recurrence is also incomplete. To explain this double failure we may reasonably infer a corresponding defect in the series of changes which the poison tends to produce in the mass of the blood.

“ Glandular enlargements and chronic abscesses are frequent *sequelæ* of those exanthematous disorders. They may be considered to represent the dregs of the reproduced virus, which has been imperfectly eliminated from the system by the usual channels.’

“ Very few observations are called for by these surmises of Dr. Watson; and certainly the learned Doctor is rather guarded in his expressions, thus admitting that though he has given in his adhesion to Liebig’s theory, yet he seems to view the deductions to which it leads with considerable distrust. Indeed it is difficult to rest satisfied with reasoning which not only assumes gratuitously a certain thing to be the cause of a certain effect, but considers it a corroboration of that assumption, that whereas the effect is irregular in its progress *we may suppose* the cause is so likewise.

“ It is still a greater triumph of logic to infer that because a disease is incomplete that we gain anything towards the establishment of the true nature of its cause by saying that we may reasonably infer that a corresponding defect exists in the cause itself. To me the whole line of argument appears delusive, and as to the last paragraph concerning glandular enlargement and chronic abscesses, it seems that Dr. Watson’s conclusion involves a contradiction, for he attributes to the virus itself, and that by virtue of its chemical action, the production of several exanthematous diseases, each specifically distinct, and indeed as different from each other as an acid from an alkali, while to the dregs of the reproduced virus, he attributes *sequelæ*—those glandular enlargements and chronic abscesses which so frequently appear after small-pox, scarlatina, or the measles. According to this hypothesis, three different animal poisons, all acting chemically, produce at first three different diseases, and at last the same disease. With regard to this hypothesis, I may further remark, that when a brewer takes a certain quantity of sweet wort, puts it in a vessel, and adds a given portion of yeast to it, he knows that if he simultaneously fills in the same way fifty similar vessels, the process of fermentation will produce in each thirty times as much yeast as was originally added to the wort. But when the virus of small-pox is introduced into the blood of fifty individuals, is a multiplication of the small-pox matter

thus proportioned to the quantity of blood in each? It certainly is not; a *fact* conceded by the supporters of Liebig's hypothesis, but which they try to evade by saying that the particles of the blood which are susceptible of this particular decomposition and metamorphosis exist in different proportions in different individuals.

"This method of ratiocination is as inconclusive as it is novel, and may be aptly termed, arguing not *in* but *outside* of a circle."—pp. 34-37.

We shall not follow Dr. Graves through his observations on the Theory of Animal Heat; but, before entering on the next topic, shall offer a few remarks on Liebig's Theory of Disease; premising, that we cheerfully admit his claims to the character of a philosophical investigator, in the highest sense of the word. Like most other inquirers into obscure and difficult subjects, his opinions, on many points, have been misunderstood and misrepresented, not from wilful error, but simply from the nature of the topics which he has handled, and handled, we are free to say, in a modest and truth-seeking spirit. It is supposed by some, that he wishes to establish an exclusively chemical or humoral pathology; but his own words contradict this supposition. We quote from the preface to his Organic Chemistry:

"Before the time of Lavoisier, Scheele, and Priestley, chemistry was not more closely related to physics than she is now to physiology. At the present day chemistry is so fused, as it were into physics, that it would be a difficult matter to draw the line between them distinctly. The connexion between chemistry and physiology is the same, and in another half-century it will be found impossible to separate them.

"Our questions and our experiments intersect in numberless curved lines the straight line that leads to truth. It is the points of intersection that indicate to us the true direction; but owing to the imperfection of the human intellect, those curved lines must be pursued. Observers in chemistry and physics have the eye ever fixed on the object which they seek to attain. One may succeed, for a space, in following the direct line; but all are prepared for circuitous paths. Never doubting of the ultimate success of their efforts, provided they exhibit constancy and perseverance, their eagerness and courage are only exalted by difficulties.

"Detached observations, without connexion, are point scattered over the plain, which do not allow us to choose a decided path. For centuries chemistry presented nothing but these points, and sufficient means were available to fill up the intervals between them. But permanent discoveries and real progress were only made when chemists ceased to make use of fanc to connect them.

“ My object in the present work has been to direct attention to the points of intersection of chemistry with physiology, and to point out those parts in which the sciences become, as it were, mixed up together. It contains a collection of problems, such as chemistry at present requires to be resolved ; and a number of conclusions drawn according to the rules of that science from such observations as have been made.

“ These questions and problems will be resolved ; and we cannot doubt that we shall have in that case a new physiology, and a rational pathology. Our sounding line, indeed, is not long enough to measure the depths of the sea, but is not, on that account, less valuable to us : if it assists us, in the mean time, to avoid rocks and shoals, its use is sufficiently obvious. In the hands of the physiologist, organic chemistry must become an intellectual instrument, by means of which he will be able to trace the causes of phenomena invisible to the bodily sight ; and if among the results which I have developed or indicated in this work, one alone shall admit of a useful application, I shall consider the object for which it was written as fully attained. The path which has led to it will open up other paths ; and this I consider as the most important object to be gained.”

Some believe that vital phenomena have not only been hitherto unintelligible to us, but must ever continue so—that while the laws of physics and chemistry can be established, the nature of life can never be reached ; but this seems a rash opinion, at least there can be no objection to our using in this investigation those instruments of observation and induction which have led to such good results in other sciences. At all events, we are not yet warranted in giving up the attempt, or in believing that the nature of life must ever be hidden from us. The known laws of physics and of chemistry, in relation to inanimate matter, and our observation of vital phenomena in health and in disease, seem means given to us for the great end of the discovery of the nature of life, in which, so far as we can see, there is nothing inherent which is for ever to baffle all attempts at discovery. But, in the meantime, let us not mar the inquiry by premature conclusions, nor seek to embody in expressions what is still obscure, floating, and remote. Here, we apprehend, lies the chief error of Liebig, whose opinions, when examined by the practical man, fail to bring conviction, because the number and variety of the cases which occur to him are so great, that the attempt to “ *stretch the formula*” ends in its being broken to pieces.

But we cannot accuse Liebig of attempting to found an exclusively chemical physiology or pathology ; of being, in other words, a modern humoralist. He admits the existence of a vital force, which is not chemical, nor electrical, nor magneti-

cal; and which, though having properties in common with all causes of motion, and change in form and structure, in material substances, is yet peculiar, because it exhibits manifestations that are found in no other known force. In this doctrine all must agree; but, when he teaches that this vital force is a peculiar property, becoming sensible when the elementary particles of certain material bodies are combined in a particular arrangement or form, our knowledge, such as it is, of physiology and pathology, forbids our assent to the doctrine.

Again, to the properties ascribed to this vital force, we have nothing to object; it is a force regulated by certain laws, which may be investigated; a cause of growth in the mass of the living animal, and of resistance to external agencies, which would alter the form and composition of the living tissues; a cause of motion and change in the form of material substances; a destroyer of the force of attraction between the molecules of the constituents of food; while it arranges them in new forms, either identical with or differing from the living tissues; a force of attraction between parts having the same composition, and a force of resistance, giving to the living tissues the power of withstanding the influence of external agencies. To all this, and to more that has been written, we fully assent; but, when we come to apply these views of the vital force to the explanation of life, or, what is more important, of the phenomena of disease, we find ourselves again at fault. And, accordingly, Liebig's chapter, which he entitles "The Theory of Disease," will be found the most weak and inconsequential part of the entire work. We shall here quote from Liebig:

"Every substance or matter, every chemical or mechanical agency, which changes or disturbs the restoration of the equilibrium between the manifestations of the causes of waste and supply, in such a way as to add its action to the causes of waste, is called a *cause of disease*. *Disease* occurs when the sum of vital force, which tends to neutralize all causes of disturbance (in other words, when the resistance offered by the vital force), is weaker than the acting cause of disturbance.

"Death is that condition in which all resistance on the part of the vital force entirely ceases. So long as this condition is not established, the living tissues continue to offer resistance.

"To the observer, the action of a cause of disease exhibits itself in the disturbance of the proportion between waste and supply which is proper to each period of life. In medicine, every abnormal condition of supply or of waste, in all parts or in a single part of the body, is called disease."

And again:

"A deficiency of resistance, in a living part, to the causes of

waste, is, obviously, a deficiency of resistance to the action of the oxygen of the atmosphere.

“ When, from any cause whatever, this resistance diminishes in a living part, the change of matter increases in an equal degree.

“ Now, since the phenomena of motion in the animal body are dependant on the change of matter, the increase of the change of matter in any part is followed by an increase of all motions. According to the conducting power of the nerves, the available force is carried away by the nerves of involuntary motion alone, or by all the nerves together.

“ Consequently, if, in consequence of a diseased transformation of living tissues, a greater amount of force be generated than is required for the production of the normal motions, it is seen in an acceleration of all or some of the involuntary motions, as well as in a higher temperature of the diseased part.

“ This condition is called *fever*.

“ When a great excess of force is produced by change of matter, the force, since it can only be consumed by motion, extends itself to the apparatus of voluntary motion.

“ This state is called a *febrile paroxysm*.

“ In consequence of the acceleration of the circulation in the state of fever, a greater amount of arterial blood, and, consequently, of oxygen, is conveyed to the diseased part, as well as to all other parts; and if the active force in the healthy parts continue uniform, the whole action of the excess of oxygen must be exerted on the diseased part alone.

“ According as a single organ, or a system of organs, is affected, the change of matter extends to one part alone, or to the whole affected system.

“ Should there be formed, in the diseased parts, in consequence of the change of matter, from the elements of the blood or of the tissue, new products; which the neighbouring parts cannot employ for their own vital functions;—should the surrounding parts, moreover, be unable to convey these products to other parts, where they may undergo transformation, then these new products will suffer, at the place where they have been formed, a process of decomposition analogous to fermentation or putrefaction.”

We have given this quotation as showing the slenderness of the grounds on which Liebig has attempted to found his theory of disease. It will be seen, that to *diminished resistance* he attributes a very large proportion of diseases; so that it is in the modification of but one of the characters of his vital force, namely, *resistance*, that we are to find the explanation of most diseases. It is not uninstrucive to compare the different attempts towards making a formula for disease, which we have seen in latter times. We refer especially to those of Brown, Broussais, and Liebig; in all of which disease is explained by a modification of vitality, but under different aspects. With Brown, there were the conditions of *sthenia* and *asthenia*, affecting the

whole system ; with Broussais diseases were referrible to an exaltation or depression of local vitality ; and the first condition was by far the most frequent. While, with Liebig, we have a minus vitality, a diminished resistance, from which flow all those diseases which his predecessor attributed to the opposite condition. It is also remarkable, that as none of these proposers of formulæ were men of sound medical knowledge, so we find that the facts of pathology are inconsistent with their doctrines ; and, though the theories and formulæ differ so widely, yet it is the same set of facts that overturns them all. The phenomena of fever, of the neuroses, of specificism, and of the alteration of the fluids, are as subversive of the formula of Liebig, as they are of that of Broussais ; for, in essential fevers, we have not only less evidence of the transformation of living tissues than in local disease ; but, in many cases, we cannot demonstrate the existence of such a condition at all. The paroxysm of an ague, which arrives at its height in a short time, if produced by change of matter, should be accompanied by remarkable organic alterations. But such is not the fact. The history of the neuroses shows that the most extraordinary examples of the production of force consumed by motion, extending to the apparatus of voluntary motion—we quote the expressions of Liebig—are those in which the least organic change occurs. And, finally, it is impossible to explain the specific nature of diseases, or the specific action of remedies by any theory in which the phenomena of life are made to be dependant on modifications of matter. Giving all credit to Liebig for his efforts to discover the results of chemical action in the living organism, we feel that he has overstretched his formula ; and that, were he as intimate with disease at the bedside, and with its results as seen in the dissecting-room, as he is with physical science, he would admit that there are numerous modifications of the living principle, which may or may not produce organic change, but which arise and subside without any alteration that can be demonstrated, of the material parts of our complicated systems.

The Author devotes fifteen lectures to the pathology and treatment of the typhus fever of these countries, and particularly of Ireland. Out of this mass of important matter we shall select a few topics of paramount interest. We may presume that Dr. Graves has ever been a steady upholder of the doctrine of essentialism in fever ; and, while giving full weight to the occurrence of local changes in this disease, he has always held them as secondary to the general disturbance.

“ In fact, gentlemen, the knowledge we possess of the pathology of typhus fever, is of a *negative* character. Pathology teaches us what typhus is *not*, rather than what *is* ; it shows us that it is neither cerebritis, meningitis, pneumonia, pleurisy, gastritis, or enteritis, for

it may exist without any of these, and they may exist without typhus fever; but it also shows that one or other of these lesions frequently arises in the course of that fever, and these require special attention."—p. 54.

The Author does not enter on any explanation of the singular prevalence of typhus in Ireland. The return of mortality by him, and the general mortality for ten years, in the province of Leinster, ending in June, 1841, has been furnished by Doctor Wilde:

"From this document it follows that the mortality from fever in Leinster amounts to a fraction less than one-tenth of the whole mortality, whereas in London the fever deaths do not amount to more than one-fiftieth of the total deaths. This difference becomes more striking from considering that deaths in Dublin from fever are actually nearly double the deaths from the same cause in London. The last census made the population of London amount, I believe, to one million and a half, while that of Dublin is three hundred and sixty two thousand.

"The admirable papers of Dr. Cowan have thrown much light upon the comparative frequency of fever in different parts of Britain, and his tables prove that Glasgow is more unfavourably situated, as regards fever, even than Dublin; for in 1835, 1836, 1837, the deaths from fever alone were 412, 841, 2,180, being, in the relation to the mortality from all diseases, one in 15·6, 10, and 4·7 annually; but as the year 1837 was remarkable for a fearful epidemic, this mortality is over the average, for Dr. Cowan in another place shows, that while in Glasgow, with a population of 200,000, the annual average of fever, deduced from seven years, ending with 1836, has been 1842 cases; in Manchester, with a population of 228,000, it has been for the same period only 497; in Leeds, with a population of 123,000, only 274; and in Newcastle, with a population of 58,000, so little as 39. These numbers bring out, in striking contrast with Ireland, the immunity from fever enjoyed by large English towns, and corroborate the remark already made, that the eastern and central parts of Britain, enjoying a climate more different from that of Ireland, so likewise are much freer from fever than the western parts of Britain, whose climate approximates more to the Irish. It is curious, that in those towns in England which have greater intercourse with Ireland, as Liverpool, Manchester, Bristol, typhus predominates more than in others not similarly circumstanced. It was on this account that Dr. Lombard concluded that maculated typhus fever was imported into England and Scotland by Irish labourers, who go over in such numbers every year to reap the harvest. But from the statistical reports of Dr. Cowan and others, it appears that, as regards Scotland, this explanation is any thing but satisfactory, and it seems more probable that the west of England, Scotland, and Ireland, in which the climate is almost the same, possess the same combinations of circumstances which produce typhus. Nothing, indeed, can be more remarkable than the facility with which a simple cold (which in England would be perfectly



devoid of danger) runs into maculated fever in Ireland, and that, too, under circumstances quite free from even the suspicion of contagion—in truth, except when fever is epidemic, catching cold is its most usual cause. Much has been said and written about epidemics among cattle being simultaneous with human epidemics, and we have the testimonies of Homer and Herodotus in support of the popular belief. I am quite sure that various diseases, such as ague, remittent and bilious fever, &c., &c., may be brought on by miasmata, which, emanating from the earth, may likewise produce epidemics among cattle.”—pp. 46, 47.

A little further on he remarks :

“With reference to this question I may remark, that although I have carefully watched the progress of *fever* in Ireland for more than a quarter of a century, I have not been able distinctly to connect its epidemics with any epizootic disease—true it is, that occasionally typhus fever is prevalent at a time that some fatal epidemic affects horned cattle, pigs, and sheep, and from such an occurrence an incautious reasoner might be led to assume a natural connexion between the two epidemics as both proceeding from one cause ; a more protracted series of observations will, however, dispel this illusion, for he will then see that the connexion is only accidental—of this the years 1841 and 1842, afforded a remarkable example ; for during both, the cattle of Ireland were decimated by a most malignant epizootic, while during the same period I never recollect a greater immunity from typhus ; in fact, the wards of the Meath Hospital were often destitute of a single specimen of that disease.”—p. 49.

And again :

“In the first place, typhus fever is endemic in this country ; at no period from the earliest records down to the present, has it been entirely absent—a fact of which you can easily satisfy yourselves by consulting our old authors, and by referring to the annual reports of the fever hospitals, established through different parts of Ireland. Fever, as I have said, is always endemic in Ireland, but occasionally for one year or one season, or a succession of years or seasons, it becomes much more than usually rife, and then is said to be epidemic. In my report of the fever which devastated the west of Ireland in 1822, I advanced the opinion that such epidemics are brought on by a great dearth of provisions, and their unwholesome quality. These are, no doubt, aggravating circumstances, but that they are not the sole or even the chief causes of typhus epidemics, is evident from what I have since frequently witnessed, viz., the occurrence of fever epidemics during years of plenty, of which 1826 was a remarkable example.

“That fever, in Ireland at least, depends on some general atmospheric change, which affects the whole island simultaneously, independent of situation, aspect, height above the level of the sea, dryness or moisture of the soil, or any other circumstance connected with mere locality, is proved by the fact, that when typhus begins to in-

crease notably in the Dublin hospitals, we may always rest assured that a nearly simultaneous increase of fever will be observed in Cork, Galway, Limerick, and Belfast, as I have on more than one occasion ascertained by writing to the physicians of fever hospitals in those cities.

“For a considerable period there was a great tendency among physicians to refer the origin of typhus, and almost every variety of fever, to malaria, or unwholesome emanations from the soil, produced by the decomposition of vegetable matter. In Ireland facts do not bear out this hypothesis, for as already stated, when an epidemic of fever has become established, it breaks out simultaneously in situations the most different, and in some where no such emanations can be supposed to exist. Thus I have seen a whole family affected in the telegraph, situated at the summit of Killiney, a mountain formed of bare granite—and indeed the granite and mountain districts beyond Rathfarnham, Tallaght, and Killikee, supply the Meath Hospital with its worst cases of typhus. The malarious origin of fever in general has, I may remark, become much less probable since the publication of the official documents connected with the sickness and mortality of the British troops in the Colonies, and from which, as Major Tulloch reports, it clearly appears that fevers of the most malignant character frequently arise in places presenting, to all appearance, a combination of circumstances most favourable to the exclusion of malarious influence, while fever is never endemic in other stations, where all the reputed sources of malaria exist together.”—pp. 41, 42.

With reference to the question of contagion, it will be seen that like other accurate observers in this country, Dr. Graves believes that its existence is established beyond doubt; but he is not an exclusive contagionist—that is to say, he admits that typhus fever may and does often arise from other causes, and thus be propagated under a twofold influence; and this opinion must be received by all right-thinking men. It is more probable, too, that the relative activities, so to speak, of these influences, vary in different epidemics, and thus modify their respective characters. So that in one epidemic there may be the clearest evidences of contagion, while in another these may be very doubtful. As, however, the public mind is still unsettled on this point, we shall here reprint, from a pamphlet published by Dr. Whitley Stokes, late Professor of the Practice of Physic, some important calculations made, at his request, by the lamented Bishop of Cloyne:

“I proposed the following problems to a friend particularly acquainted with this species of computation:

“Problem the first.

“An epidemic prevails so severe that one out of seven sickens. A family of twelve is selected in a particular district, before the epidemic has visited it; what is the chance that eleven out of that family shall take the disease, supposing the sickness of one of the

family does not promote the sickening of another, and supposing the family not unusually liable to disease?

“Answer.—The probability against the event is, 189,600,000 to one, if the population amount even to seven thousand.

“Problem the second.

“The same general conditions being assumed, and also that the number of the inhabitants in the district in question is seven thousand. What is the chance that in any family of twelve, within the district, eleven will sicken?

“Answer.—It is above 300,000 to 1 that no family of twelve persons in a population of 7000 will have eleven sick.

“All this according to the conditions that the sickening of one does not promote the sickening of another.”—pp. 23, 24.

We cannot enter into a detailed analysis of the Author's lectures on fever, which are full of important and practical precept; but we may draw attention to three points of great importance, which may be considered the most distinguishing features of this part of the work. They are:

1st. The employment of nutriment in fever.

2nd. The exhibition of tartar emetic and opium in certain cases of delirium.

3rd. The use of wine.

4th. The employment of mercury.

It is hardly necessary to premise, that the Author looks on fever as an essential disease, the nature of which has not yet been revealed by morbid anatomy. The following observations must not be omitted:

“Now, when called on to treat a case of fever, there are several things which require your attention. In the first place, you should examine the state of the family arrangements. This is a matter which men are apt to overlook or treat as a matter of indifference, but in my mind it is of no ordinary importance, and should always be attended to. You should never, if possible, undertake the treatment of a case of fever where the friends or relations of the patient supply the place of a regular fever nurse. The mistaken tenderness of relatives, and their want of due firmness, presence of mind, and experience, will frequently counteract your exertions and mar your best efforts. Affection and sorrow cloud the judgment, and hence it is that very few medical men ever undertake the treatment of dangerous illness in the members of their own families. The sympathy which a nurse should have for her patient should be grounded on a general anxiety to serve, and a strict sense of duty, as well as a laudable desire of increasing her own reputation; it is, in fact, a sympathy analogous to that which should actuate a physician. Again, it will not do to have a nurse who has been usually employed in other diseases; your assistant must be a regular fever nurse, and the man who undertakes the treatment of a long and dangerous case of fever without such an assistant, will often have cause to regret it. I could mention to you many cases illustra-

tive of the truth of this assertion. I could tell you, that where I have permitted the continuance of the services of one of the family, or of a common nurse, I have been almost invariably annoyed and disappointed. I now make it a general rule to refuse attending any dangerous and protracted case of fever without a properly qualified nurse."—pp. 60, 61.

After speaking of the importance of having competent medical assistance to stay by the patient and watch every change of his malady, he recommends a practice which was adopted by the late Dr. Cheyne, of having two beds in readiness, from one of which the patient should be changed every twelve or twenty-four hours. The rooms should be properly ventilated by a fire, and the temperature regulated by a thermometer.

"Having made these few general observations on the steps to be taken by those who enter on the treatment of typhus, I shall now proceed to speak of diet and medicines. In a disease like fever, which lasts frequently for fourteen, twenty-one, or more days, the consideration of diet and nutriment is a matter of importance, and I am persuaded that this is a point on which much error has prevailed. I am convinced that the starving system has, in many instances, been carried to a dangerous excess, and that many persons have fallen victims to prolonged abstinence in fever. This was one of the errors which sprung from the doctrines of those who maintained that fever depended on general or topical inflammation. They supposed that fever arose from inflammation, and immediately concluded that, to treat it successfully, it was necessary to reduce the system by depletion and low diet, and to keep it at this point during the whole course of the disease. Hence the strict regimen—the *diète absolue*—of the disciples of the physiological school, and of those who looked on inflammation as the essence of fever. The more the symptoms appeared indicative of inflammatory action, the more rigorous was the abstinence enforced. If a patient's face was flushed, or his eyes suffused, no matter what the stage of the fever was, they said, 'here is inflammation of the brain, and nourishment will exasperate it.' If he had red or dry tongue, and abdominal tenderness, they immediately inferred the existence of gastro-enteritis, and all kinds of food even the lightest, were strictly forbidden. That this proceeds from false notions on the nature of fever is beyond doubt, and I pointed out this fact many years ago, long before the appearance of Piorry's work. Let us, in the first place, examine the results of protracted abstinence in the healthy state of the system. Take a healthy person and deprive him of food, and what is the consequence? First, hunger, which after some time goes away, and then returns again. After two or three days, the sensation assumes a morbid character, and instead of being a simple feeling of want and a desire for food, it becomes a disordered craving, attended with dragging pain in the stomach, burning thirst, and some time afterwards, epigastric tenderness, fever, and delirium. Here we have the supervention of gastric disease, and in-

inflammation of the brain as the results of protracted starvation. Now, these are in themselves very singular facts, and well deserving of being held in memory. Read the accounts of those who perished from starvation after the wreck of the *Medusa* and the *Alceste*, and you will be struck with the horrible consequences of protracted hunger. You will find that most of the unhappy sufferers were raging maniacs, and exhibited symptoms of violent cerebral irritation. Now, in a patient labouring under the effects of fever and protracted abstinence—whose sensibilities are blunted, and whose functions are deranged—it is not at all improbable that such a person, perhaps also suffering from delirium or stupor, will not call for food, though requiring it; and that if you do not press it on him, and give it as medicine, symptoms like those which arise from starvation in the healthy subject may supervene, and you may have gastro-enteric inflammation, or cerebral disease, as the consequence of protracted abstinence. You may, perhaps, think that it is unnecessary to give food, as the patient appears to have no appetite and does not care for it. You might as well think of allowing the urine to accumulate in the bladder, because the patient feels no desire to pass it. You are called on to interfere where the sensibility is impaired, and the natural appetite is dormant; and you are not to permit your patient to encounter the horrible consequences of inanition, because he does not ask for nutriment. I never do so. After the third or fourth day of fever, I always prescribe mild nourishment, and this is steadily and perseveringly continued through the whole course of the disease.”—pp. 62, 63.

The Author declares, that, if he has had more success than others in the treatment of fever, it was owing to the adoption of the advice of a country physician of great shrewdness, who recommended him never to let his patients die of starvation. He quotes from Huxham the history of a gentleman,

“Who obstinately starved himself to death, and would not, for many days, either by force or persuasion, swallow any kind of food, or a drop of liquor. He soon grew feverish, flushed in his face, and very hot in his head; his pulse was small but very quick, in four or five days his breath became exceedingly offensive, his lips dry, black, and parched, his teeth and mouth foul, black, and bloody, his urine vastly high coloured and stinking as much as if it had been kept a month; at length he trembled continually, could not stand, much less walk, raved and dozed alternately, fell into convulsive agonies frequently, in which he sometimes sweated pretty much about the head and breast, though his extremities were quite cold, pale, and shrivelled; the sweat was of a very dark yellow colour, and of a most nauseous stench.”—p. 66.

But the Author is extremely cautious in the administration of food, and gives excellent directions for its management.

One of the most troublesome symptoms in fever is tympanitis, a subject on which we have a separate paper, by the Author, in the *Dublin Medical Journal*. The most novel feature in the

Author's treatment is the exhibition of the acetate of lead, in cases where bowel complaint is associated with tympanitis; he was first led to use this medicine on the recommendation of Dr. Bardsley, for the purpose of preventing the follicular ulceration of the intestines. After speaking of its use in cholera, in which there is commonly a copious secretion of air into the bowels, Dr. Graves observes on the anti-tympanitic powers of acetate of lead, both in cholera and typhus; and dwells with satisfaction on the success which this remedy has had in his hands. With respect to hiccup, which is so often associated with tympanitis, his experience does not accord with that of his colleague, who taught that the period of fever furnished a guide for treatment, and that hiccup, in the commencement of fever, requires a local antiphlogistic treatment, while, in the latter periods, it was to be treated with opiates, antispasmodics, and even stimulants. Notwithstanding this decided difference of opinion, the treatment recommended by our Author squares very nearly with that of his colleague.

The Author is favourable to the employment of venesection, used with caution and during the *stage of rigor*; by which is meant the period of formation, during which the patient complains of recurrent chills, although his skin feels hot to the touch, when examined by another person; it is only during this stage, which may last from twelve to thirty-six hours, that we have a chance of extinguishing fever by the abstraction of blood from the system.

“The physician seldom sees a case of fever until the third or fourth day, and then it is too late to think of general depletion by the lancet. This explains why venesection is so seldom employed in typhus in our hospitals. Moreover, in entering on the treatment of any case of fever at present, you should bear in mind the nature of the prevailing epidemic, and be careful how you proceed with respect to bleeding; and if you take away blood, do not go so far as you would if treating a case of fever under different circumstances, and of a genuine inflammatory character. I know that many persons have asserted that you can bleed in all cases of fever, no matter what the state of debility may be; because this, they say, is only apparent, and depends upon congestion and oppression of vascular action. I do not know how far this doctrine may be applicable to other epidemics, but in the present fever it certainly does not hold good; and no man in his senses would think of adopting it as a guide for his practice. I have seen some of the most intense, dangerous, and protracted cases of fever, commence without any appreciable increase of vascular action, with a soft slow pulse, a cool skin, no symptoms of congestion of any internal organ; in fact, without any thing which would, even in the youngest and most robust habits, call for the use of the lancet. Increased vascular action, and this you should always bear in mind,

is not in itself a proof of an inflammatory diathesis in fever, but rather one of a set of symptoms produced by the same morbid cause. The heat of skin and rapidity of pulse are, just like the debility, products of the same morbid cause, and not the results of inflammation. You should also recollect that in fever, as well as in other diseases in which the nervous system is greatly deranged, the pulse is not unfrequently a very deceptive guide. In many cases of fever, where the patient happens to be of an irritable habit, the pulse exhibits a degree of thrill and apparent hardness, which might lead an inexperienced or inobservant practitioner into serious errors. I do not mean to say that an inexperienced finger will not be able to distinguish a pulse of this kind from one of genuine hardness, but I know that many persons have been misled by it, and I warn you against the danger.

“Again, never use the lancet when there is any, even the slightest, appearance of maculæ, no matter how intense the headach, heat of skin, or signs of general vascular action, may be. I have seen some cases in which the lancet was used during the presence of maculæ, and I have seen its employment followed by the most lamentable consequences. You should, therefore, never omit to examine the skin, for circumstances might occur which would authorize a moderate use of the lancet, provided there was no sign of maculæ present. Formerly, persons were very much in the habit of employing arteriotomy when the headach and delirium were violent, regardless of the period or stage of fever; and nothing was more common than to see a physician ordering the temporal artery to be opened on the eighth, ninth, or even tenth day. This was very much the practice during the time when the doctrine of typhus being the result of inflammation of the brain prevailed in this country and England, and a very unsuccessful practice it was. You perceive we seldom have recourse to arteriotomy here; it may be occasionally necessary, and when it is, we employ it; but as a general practice it does not appear entitled to any merit, nor can we give it our recommendation.”—pp. 113, 114.

In these observations we cordially agree, and only wish that Dr. Graves had still further limited the employment of this means in typhus. Indeed, the practice of bleeding in the fever of this country is the source of great mischief; it is a practice which may possibly arrest the disease, though it has never fallen to our lot to see such an occurrence; but, if it does not do so, it places the patient in a still more unfavourable position. Many of the most dangerous, slow, and complicated cases met with in hospital are those in which bleeding has been performed within the first few days of the disease. To bleed and violently purge a fever patient is decidedly the very worst preparation that can be conceived for a successful struggle with the disease. The practice, too, is seldom adopted from judgment or experience, but in consequence of the defective medical education, which so many of our surgical students receive. If hot skin and a throb-

ing pulse are to them unequivocal signs of inflammation or inflammatory action—one of those meaningless terms which pass current with teacher and pupil. To practitioners, thus educated, many of whom have never even been within the walls of a fever hospital, pain of the head, intolerance of light, and delirium, are signs of cerebral inflammation, hurried breathing and sighing, pneumonia, and vomiting, swelling and tenderness of the belly and gastritis; and so they bleed, ignorant alike of the nature of the general disease, or its local symptoms—ignorant of the law of periodicity—of the conditions of debility—and, above all, ignorant of the leading or master fact, that symptoms, which are pathognomonic of local disease, where there is no typhus, nearly, if not altogether, lose their value, pending the continuance of this peculiar state. With respect to arteriotomy, we find it seldom practised now in Ireland; the followers of Clutterbuck are now not numerous, and have fallen away from the bolder practice of arteriotomy to the less striking but often equally dangerous system of repeated leeching the head. In the removal of delirium occurring in the advanced stages of fever, we have occasionally witnessed the success of this practice, accompanied, it is true, by the untoward event of the death of the patient, while the leeches still adhered to the temples.

Another routine treatment, and one founded on the same set of errors, is the employment of mercury. Let us hear the Author :

“ I shall close this lecture with a few observations on the use of mercury in fever, and this will include all I have to say at present on the remedies most generally employed in the treatment of typhus. Are we to have recourse to mercury, or not, in typhus? I do not allude here to its use as an aperient, but when called to treat a case of fever, are you to proceed at once to bring the patient's system under the influence of mercury? Are you, in addition to the other measures usually adopted in the treatment of fever, to go on with the administration of mercury until you affect the mouth, and bring on salivation? This was the practice in my earlier days, and great confidence was placed in it by the majority of practitioners. It has been also very extensively recommended by army and navy surgeons, in the treatment of tropical fevers, but I must confess that I am not at all inclined to adopt this practice, and that I have seen abundant reasons why I should neither employ nor recommend it. In the first place, we have observed in our wards that patients with other diseases have frequently caught fever from exposure to infection, at a time when they were fully under the influence of mercury. In the next place, we have observed that persons who were thus attacked with fever while in a state of salivation did not escape better than others, and that in them the disease ran its full course, aggravated rather than diminished in its danger by the pre-existing mercurialisa-



tion. These facts I have frequently seen verified in hospital and private practice. You perceive, then, that mercurialisation neither protects a man from the contagion of typhus, nor does it produce a favourable modification in its type or progress. Again, I have repeatedly witnessed the daily and continued exhibition of mercury in fever, and I cannot recollect a single case in which it appeared to check the disease, moderate its symptoms, or bring about a favourable crisis. I am aware, that in entering my protest against this practice, I dissent from a very considerable body of my brethren, who, from the beginning to the end of fever, never cease in their attempts to bring the patient's system under the influence of mercury. I am convinced, that in the cases in which recovery is stated to have followed this practice, the *post hoc* has been mistaken for the *propter hoc*. Besides, fever is one of those affections in which you will find it extremely difficult, and often impossible, to bring the system fully under the influence of mercury. There are certain states of the system which prevent altogether the full operation of mercury, and bad typhus is one of these states. Where fever has laid deep hold of the constitution, you cannot affect it with mercury. When a patient recovers who has been mercurialised during the course of fever, he does not recover because his system came under the influence of mercury, but he comes under the influence of mercury because he recovers from the fever. Add to this, that mercury is a remedy which requires a peculiar regimen, and that it is very apt to engross the practitioner's attention, and prevent him from the exhibition of remedies which are more directly indicated, and in reality more useful. These considerations, and others, have convinced me that the exhibition of mercury in fever, with the view of touching the gums, is injudicious and unnecessary. There are, however, cases in which you will be compelled to have recourse to mercury, whatever the stage or type of the fever may be. Whenever inflammation of some internal organ—as, for instance, of the lungs—arises during the progress of fever, you must employ mercury at once; and cases of pneumonia, which would have proved fatal, have, on numberless occasions, been treated successfully by mercurialisation. But under ordinary circumstances, and where there is no indication similar to that which I have just pointed out, I do not see any advantage to be derived from the use of mercury. I am not, therefore, in the habit of employing mercury in fever. Sometimes I use calomel as an aperient, and I frequently prescribe small doses of hydrargyrum cum cretâ, with the view of gently stimulating the liver, and preventing the tendency to congestion of the intestinal canal; but farther than this I am not in the habit of going; and I never, except in cases of pneumonia, or inflammation of some internal organ, attempt to bring the patient's system under the influence of mercury during the course of typhus.”—pp. 118, 119.

We come now to consider one of the most important additions to practical medicine of modern times—namely, the exhibition of the tartrate of antimony, with opium, in the advanced

stages of the worst nervous fevers; a practice which, whether we consider the extent of the induction which has led to its use, or the sound pathological knowledge and diagnostic tact which have marked its adoption by the Author, entitle him to the gratitude of every British practitioner. After speaking of the use of tartar emetic in some cases of sthenic delirium, Dr. Graves observes:

“In the two preceding cases I was guided by ordinary principles, recognized by all physicians, and according to which the exhibition of tartar emetic is recommended in fever whenever there is undoubted evidence of determination of blood to the head, producing headach, loss of sleep, and delirium. In the cases which follow, tartar emetic was exhibited at a period of fever, and under circumstances that were, with respect to the exhibition of this remedy, not less novel than important. The principles which led me to this practice have long been established, but, nevertheless, the practice is entirely new, and (I say it with pride for it has already been the means of saving many valuable lives) it is entirely my own.

“Shortly after the commencement of our present session, Mr. Cookson, a pupil at this hospital, and remarkable for his diligent attention to clinical pursuits, caught fever while attending our wards, in which many cases of the present epidemic were then under treatment. His fever was of an insidious nature, not characterized by any prominent symptom, not exhibiting any local disease to combat, or any tendency to crisis. For the first seven or eight days, with the exception of headach, which was much relieved by leeching, he seemed to be going on very well; his skin was not remarkably hot; he had no great thirst, nausea, or abdominal tenderness; his pulse was only eighty-five; and he had sweating, which was followed by some relief. About the eighth or ninth day the pulse rose, and he began to exhibit symptoms of an hysterical character. Now, in every case of fever, where symptoms resembling those of hysteria come on, you should be apprehensive of danger. I do not recollect having ever met with a single case of this kind which did not terminate in nervous symptoms of the most formidable nature. I prescribed at the time the usual antihysterical medicines, but without any hope of doing good, knowing that these symptoms were only precursory to something worse. I also, as a precautionary measure, had leeches applied to his head. The fever went on, the headach became more intense, he grew nervous and sleepless, and fell into a state of great debility. On the fourteenth day of fever his tongue was black and parched, his belly tympanitic; he was passing every thing under him unconsciously; he had been raving for the last four days, constantly attempting to get out of bed, and had not slept a single hour for five days and nights. Dr. Stokes, with his usual kindness, gave me the benefit of his advice and assistance at this stage of Mr. Cookson's illness, and we tried every remedy which experience could suggest. Blisters were applied to the nape of the neck, the head was kept cool by refrigerant lotions, the state of the belly attended to, and, as we perceived

that the absence of sleep was a most prominent and distressing symptom, we were induced to venture on the cautious use of opium. It was first given in the form of hydrarg. c. cretâ, with Dover's powder, with the view of relieving the abdominal symptoms as well as procuring sleep. This failing in producing the desired effect, we gave opium in the form of enema, knowing its great power in the delirium which follows wounds and other injuries. This was equally unsuccessful with the former. He still was perfectly sleepless. We came again in the evening, and as a last resource, prescribed a full dose of black drop, and left him with the conviction that if this failed he had no chance of life. On visiting him next morning at an early hour, we were highly mortified to find that our prescription had been completely unsuccessful; he had been more restless and delirious than ever. Here was the state in which we found him on entering his chamber at eight o'clock in the morning of the fifteenth day of his fever. He had universal tremors and subsultus tendinum, his eye was suffused and restless, he had been lying for some days entirely on his back, his tongue was dry and black, his belly tympanitic, his pulse 140, quick and thready, his delirium was chiefly exhibited in short broken sentences and in a subdued tone of voice; and it was now eight days and nights since he had slept. Here arose a question of great practical importance. How was the nervous agitation to be calmed and sleep produced? Blisters to the nape of the neck, cold applications, and purgatives had failed; opium in various forms had been tried without the slightest benefit; if sleep were not speedily obtained he was lost. At this emergency a mode of giving opium occurred to me which I had never thought of before. Recollect what his symptoms were at this period: quick, failing pulse, black, dry, tremulous tongue, great tympanitis, excessive prostration of strength, subsultus tendinum, extreme nervous agitation, constant muttering, low delirium, and total sleeplessness. I said to Dr. Stokes that I wished to try what effects might result from a combination of tartar emetic and opium; I mentioned that I had given it in cases of delirium tremens with remarkable success, and thought it worthy of trial under the circumstances then present. Dr. Stokes stated in reply, that he knew nothing with respect to such a combination as adapted to the case in question, that he had no experience to guide him, but that he would yield to my suggestion. We therefore prescribed a combination of tartar emetic and laudanum in the following form, which is that in which I generally employ the remedies in the treatment of delirium tremens. ℞. Antimonii tartarizati grana quatuor, tinct opii drachmam, misturæ camphoræ, ℥ viij. Of this mixture, a tablespoonful to be taken every second hour. The success of this was almost magical. It is true that it vomited him; after taking the second dose he threw up a large quantity of bile, but it did him no harm. After the third or fourth dose he fell asleep, and awoke calm and refreshed; he began to improve rapidly, and soon recovered."—141-143.

A great number of illustrative cases are given by the Author, accompanied by important remarks. It is somewhat difficult to

convey in words a proper idea of the state which indicates this treatment; particularly, if we have to instruct those who have no real familiarity with fever at the bedside. But it may be described to be a state generally met with in the advanced stages of bad nervous fevers, and more particularly in those of the upper classes of society—a state in which many symptoms of extreme nervous excitement are met with, such as subsultus, watchfulness, muttering, delirium ferox, or even convulsions, and all this without cerebral congestion or inflammation; although, in the earlier periods of the same case, these very conditions may have existed. This is the condition so constantly maltreated by the exclusively antiphlogistic school.

But even a certain amount of congestion does not contraindicate the remedy. In such cases, the tartar emetic is increased to the amount of four grains in the eight ounces, while the laudanum should not exceed half a drachm; but, where the nervous symptoms predominate, the laudanum may amount to one drachm, and the tartar emetic to two grains. No general rule, however, can be laid down; and the practitioner must, in all cases, watch the effect of the medicine from hour to hour, until he ascertains whether it agrees with the patient or not.

“Where a life is at stake, you must spare no pains, and must not reject a remedy, because its power is rendered an instrument of good or evil, accordingly as it is administered carefully or otherwise.”

This practice, like many other improvements, has been misunderstood, cavilled at, and its originality questioned, when its adoption became inevitable.

“Some there are who will take occasion to remark that I can have no claim to originality on this occasion. But all who have watched my practice in the hospital, nay, all who have taken the trouble of reading my lectures and successive publications on this subject, will at once acknowledge that I proceeded on this path of investigation with no other guide but an analogy derived from an observation of the effects of tartar emetic and opium in delirium tremens, a disease undescribed in the time of Marryatt. Every one the least conversant with the treatment of fever in private and in hospital practice in Dublin, London, and Edinburgh, will allow that no one during the present century ever taught or practised the exhibition of tartar emetic at the stage of typhus fever in which I have recommended it. Not a single hint at such a treatment is given in any of the numerous contributions on the treatment of typhus, which form the valuable work edited by Dr. Barker and Dr. Cheyne. Where is there even one allusion to this practice in Armstrong, Smith, Tweedie? And what is said of it in Good, Thomas, Mackintosh, or in the *Cyclopædia of the Practice of Medicine*? Where is it mentioned or inculcated in the *Edinburgh Medical and Surgical*

Journal, or in Johnson's Medico-Chirurgical Review? No where; although the treatment of fever is often the subject of anxious discussion.

"So far suffices with regard to the novelty of the matter, for it is useless to argue with persons so stupid as to confound the practice I recommend with the well known and popular use of tartar emetic as an emetic or a diaphoretic in the commencement of febrile diseases generally. That I did not come upon this method sooner, I regret infinitely, for since its adoption, my practice in hospital and in private has been much more fortunate than formerly."

Our experience fully bears out the statements of the Author; and we have repeatedly seen cases in which opium alone failed to procure rest, yet in which the combination had the happiest effect. We had an opportunity of witnessing a most remarkable instance which lately occurred in the practice of Dr. Lees, of this city, who has kindly furnished us with notes of the case; the remedy appeared to have an almost magical effect. The patient, a gentleman of nervous temperament and high literary attainments, and suffering under his first attack of typhus, had an imperfect crisis on the fourteenth day.

His pulse had been 140; he was incoherent, looked wildly about him, and towards morning became extremely low, and said he was dying. He then broke out into profuse perspiration, had great tremor of the hands, and appeared in extreme terror. He got some stimulants, and soon fell asleep, awaking in four hours much refreshed, with his pulse having fallen to 112. On the sixteenth day he again became greatly excited. He struggled violently, his features twitching, and his pupils greatly dilated. His state now became terrible. The subsultus amounted to almost convulsion; his pulse 132, and miserable; opium and musk were given without effect. The head was shaved; but he continued as violent as ever, thrusting downwards in the bed, sobbing and screaming, and with the subsultus like tetanic shocks. Under these circumstances, the following was ordered by Dr. Lees:

R Tartrat. Antimonii gr. vi.  
Liquoris Opii sedativi ʒi.  
Misturæ Camphoræ ʒ viii.

Of this mixture he got one ounce in a single draught. He fell asleep, and slept calmly for three hours, when he awoke in great terror; all the bad symptoms returned. Another ounce was administered; and soon after a third, with the same happy effect. Thus he continued to the evening of the next day, comparatively rational, and taking nourishment, when he began to look wild, got restless, and tossed himself about; his tongue was dry and glazed. Another dose was administered, which did not

produce sleep; but the patient lay quiet, passed much urine, and from this time began to recover. The gentleman is now in perfect health. And we hesitate not to say, that in our experience of typhus fever occurring in the upper ranks of society, we never witnessed a recovery so distinctly attributable to medicine.

After giving the results of Dr. Stokes's investigations on the state of the heart in typhus fever, and the conclusions to which his observations led, Dr. Graves observes, that, though the doctrines are entirely new, and to some rather fanciful, yet that he can vouch for their general accuracy. We may here recall to the memories of our readers, that the paper alluded to consists essentially in the record of a long series of accurately observed facts, and that it is not a paper of speculation. The only point of suggestion or speculation in it, relates to the nature of the softening of the heart, which is put forward more as a suggestion than a point of doctrine. What Dr. Stokes did, was to point out the means by which a certain condition of the heart—long ago recognized and described by Lænnec, Louis, and others—could be discovered during life, to show the description of case in which this condition occurred, and to establish its value, in giving an important rule in practice. But before going farther, we must quote from Dr. Graves :

“ I cannot agree, however, with Dr. Stokes, in attributing the phenomena of a *debilitated* heart to a *softening* of that organ, much less to the interstitial infiltration of a peculiar secretion, analogous to that which Staberoh states he has observed on the mucous surface of the intestines in dothionenterite. On the contrary, I consider the heart, in typhus fever, to be affected with debility from the same cause which induces a debility of the voluntary muscles, and of the bladder and sphincter ani—that cause is a general prostration of nervous energy. That Dr. Stokes has seen the heart softened in the examination of subjects that had been affected with typhus fever, I have no doubt; but I would impute this condition to the effect of putrescence, a process which it is well known sets in with great rapidity in cases where death has been caused by any malignant disease. It seems difficult to conceive how the heart could contract in a case where ‘ the *right cavities* were softer than natural, admitting the fingers through their walls without much resistance; and in which, in the muscular structure of the left cavities, this change was much more remarkable, the weight of the finger being almost sufficient to penetrate its walls, they were so exceedingly softened; it was very easily torn, and the edges thus separated had no longer the moistened appearance, but seemed as if quite dry. The septum cordis was equally softened; there was some dark fluid blood in the right cavities.’

“ But the fact cannot be denied, that in many cases of typhus the heart becomes weak, that this weakness is manifested by a de-

crease in the strength of its impulse, or in the intensity of its sounds, or a change in their relative loudness and duration—and though I have never witnessed these changes without accompanying debility of the entire muscular system, and other evidences of prostration, yet I fully agree with Dr. Stokes ‘*that in the diminished impulse, and in the feebleness or extinction of the first sound, we have a new, direct, and important indication for the use of wine in typhus fever,*’ and one from which the junior practitioner in particular will derive the greatest assistance.”

It is true that Dr. Stokes did not point out the diagnosis between a debilitated heart without softening and the same condition with softening. This he left an open question. He states in the outset, that in typhus fever two opposite conditions of the heart may be observable; in the one the impulse becomes extremely feeble, or altogether wanting, while the sounds are greatly diminished in intensity; while in the other the heart’s action and sounds continue vigorous throughout the whole course of the disease. He observes farther, that these opposite states are not revealed by the state of the pulse, or the warmth of the surface, but must be determined by the application of the hand and stethoscope, and he showed that these conditions indicated certain modifications in practice, and, above all, had great importance with reference to the use of wine.

Dr. Stokes did attribute, and, we believe, does still attribute, the phenomena in question to a *debilitated and softened* heart, for the following reasons:—

I. That a softened heart had been shown to exist in many cases of typhus, by Louis and others.

II. That in the dissections made by Dr. Stokes, in cases where the peculiar physical signs existed, a softened heart was found.

III. That where the opposite signs existed the heart was not softened.

IV. That the signs of debility often occurred at an early period of fever, and when there were no evidences of the other muscles, voluntary or involuntary, being affected.

It is plain that the main questions are, whether a debilitated state of the heart is, or is not, an important element in certain cases of typhus? and secondly, whether it can be recognized with certainty during life? and thirdly, whether that recognition leads to any important indication of practice? We believe that Dr. Stokes has shown that all these things do really occur, and, in a practical point of view (so far as we know at present), it matters little whether there be an organic change or not.

But we are entirely at issue with Dr. Graves when he attributes the condition observed by Louis and Dr. Stokes to pu-

trefaction. It is not likely that the latter observer would commit the unpardonable error of mistaking putrefaction for organic change occurring during life ; and it is still less probable that Louis would fall into the same mistake. The exact correspondence between the vital cardiac phenomena in the cases of recovery and those in which death took place, makes a strong case in favour of the similarity of the organic state of the heart in both sets of cases. The early appearance of the change, as observed by Louis, and of the physical signs, as recorded by Stokes, is another strong argument ; and the question seems set at rest by the fact that the change of the heart is often partial, affecting the left ventricle, and that in most of the observed cases *there was no analogous condition observed in the voluntary muscles.*

Finally, as to the question of the softening being produced by a peculiar infiltration, Dr. Stokes has put this view forward more as a suggestion than as a matter proved. Changes of organs may be observed in typhus fever, inflammation, congestion, putrefaction, and softening with infiltration of a peculiar matter. We believe that the morbid condition in question cannot be referred to any of the three first states, and the whole history of the disease is reconcileable with the supposition of softening by infiltration. Analogy, too, of which Dr. Graves so often makes successful use, is in favour of this supposition. Indeed the progress of observation for some years back tends strongly to the opinion that typhus is analogous to other diseases, acute or chronic, in which a peculiar matter is formed and deposited in the tissue of organs. This is the *typhus-gebilde* of Rokitansky, a substance which we have every reason to suppose passes through transformations, and is deposited and absorbed with or without accompanying irritation, as in other instances of the production of the non-analogous compounds in the animal economy.

In the preceding notices of the Author's views of fever, we feel that we have given but an imperfect sketch of the subject as detailed in the book itself. We now enter on another portion of the book, devoted to the consideration of a number of local and constitutional affections. The chapters on syphilis contain by far the best account of the actual state of our knowledge of this disease and its treatment, with which we are acquainted. Dr. Graves gives many extracts from the latest reports on the mercurial and non-mercurial treatment in Germany ; and, although by no means excluding the use of mercury in proper cases, shows with great force the lamentable consequences which still result from the repeated use of this medicine in patients, who really labour more under mercurial than syphilitic disease. We have next



the subjects of inflammation, paralysis, nervous fever, scarlatina, influenza, gout, and hepatic and splenic disease, illustrated by many new and important facts; and the latter part of the book contains the Author's researches on a variety of local diseases. We trust on a future occasion to continue our notices of this important work, so creditable to its Author, and to our country. It is no ephemeral publication, nor is it a book got up for a special purpose, but may be taken as a true exposition of *practical medicine*, as we have it up to the present day, by an experienced physician, one long distinguished in the annals of European medicine, as a man of high education and great practical attainments; and, as such, must be considered an addition to the medical literature of Ireland.

*The London Journal of Botany; being a New Series of the Journal of Botany.* By Sir W. J. HOOKER, K. H., LL.D., F. R. A., L. S., &c. Nos. 1 to 14. H. Bailliere, Regent-street, London.

BOTANY is a science that appears to be either studied by medical students with too exclusive an ardour, or, what is still more frequent, is too much neglected. It is true, that a knowledge of plants is not a knowledge of disease; and that many physicians, the most eminent, have been utterly ignorant of the structure, functions and classifications of the vegetable kingdom. But it is not the less certain, that our acquaintance with the laws of life is not so complete as to permit us to despise its manifestations in those examples, when it presents its simplest problems; nor is the "medical mind" so easily educated, that we are entitled to reject the best means of exercising one of its most difficultly acquired modes—that of *methodical investigation*.

Hear what Frederick Tiedemann (no mean authority) says on this subject:

"Natural history is important to the young physician, inasmuch as it exercises his intellect and judgment. They are (he continues) the same intellectual faculties which operate, whether he employs himself in determining a plant or an animal, to find its place in system; or is occupied in the diagnosis of disease by the bedside of a patient. Natural history recommends itself to the young physician, as one of the best preparatory exercises to his principal studies."

Botanists have done much for pharmacologia; witness Murray (Author of the Apparatus), De Candolle, and Barbieri; and we question if the latter, the writer of the best work extant upon therapeutics, does not owe much of his competency for the

task to his intimate acquaintance with the structure and functions of vegetables.

Viewing, as we do, the study of botany as an excellent addition to the education of the medical man, if retained subordinate to more practical pursuits, we strongly recommend to our professional brethren the periodical at the head of this notice. The name of the Editor, Sir William Hooker, is sufficient to insure the scientific and valuable nature of its contents; and when we mention that Lindley, Berkeley, Bentham, Gardiner, Watson, Asa Gray of the United States, &c., are among its principal contributors, our readers may rest assured of the talent of the staff by which he is supported.

One of the most remarkable features of this periodical is the information which it contains from all parts of the habitable globe. From his long scientific career, and the favourable circumstances in which he is now placed, as director of the gardens at Kew, Sir William Hooker has established a most extended correspondence; and accordingly we find in this Journal letters from Surinam, Australia, Southern Africa, Russia, China, and many other countries, not only replete with botanical news, but extremely interesting, as affording an insight into the peculiarities of many and far distant lands.

Thus our enterprising and enthusiastic countryman, Mr. James Drummond, in writing from King George's Sound, describes a cave in York District containing drawings similar to those described in Governor Grey's highly interesting book on Western Australia. We extract Mr. Drummond's description:

"A curious cave, called, by the natives, the Moon's House, is situated in a solid granite rock, near the left bank of the Avon river, and about two and a half miles above the residence of Mr. Hardy. This cave is remarkable for having imprinted in the living rock a circular figure, about eighteen inches in diameter, together with several mysterious prints of the human hand. The circular figure resembles what might be drawn with the tip of the fore-finger, dipped in some white colouring matter; and the circle is subdivided into small squares, by means of five perpendicular white lines, placed at rather unequal distances, and crossed again by eight lines. An indistinct outer circle appears, in some places about two inches distant from the inner one. On laying the eye to the level surface of the granite, the white lines appear rather below the level of the other parts. The whole interior of the circle, and the interstices between the white lines, are deeply stained with what appears to me to be iron ochre, but others have pronounced it blood. The prints of the human hand are of two kinds. About a foot above the circular figure that I have described, are two marks, each exactly resembling such an impression as would be left by the hand of a full-grown native man, dipped in blood, and pressed flat on the surface of the rock, with the

fingers outspread. But the most remarkable figure is one, apparently of the same hand, which is distinctly visible as far as the elbow. This is below the circle, and, instead of appearing of the same bloody-colour as the one above, seems to be of a light grey granite, surrounded by the same rock of a red hue. Three fingers of another hand are discernible in a small hollow of the rock; but these are not easily discoverable, being not very different in tint from the surrounding rock; and several indistinct impressions of what may be called "the bloody hand," exist to the right and left of the circular figure. No difference is perceptible in the structure of the rocks where these marks appear, nor could I detect that the surface was either elevated or depressed. The stone itself is hard as flint; and when trying to chip it with the best tempered chisel I could procure, the fire flew at every stroke, and scarcely any impression was made. The more the figures are scrubbed with sand and water, the plainer do they appear. The tradition of the natives is, that the moon made these marks, when he existed here in the shape of a black fellow, and I regret that I cannot give any more satisfactory account of them."

The foregoing description corresponds, in the most striking manner, with the account of the caves discovered by Governor Grey, near Port George IV., in the north-western part of Australia; but what makes the similarity most remarkable, is, that the places where these drawings were found, are no less than seventeen degrees of latitude from each other!

In another letter from Swan River, Mr. Drummond gives particulars with respect to the action of a poisonous plant, which had been described by him in former communications.

"Our readers will, perhaps, be surprised" (says the editor) "to learn, that the heaviest drawback to the prosperity of the Swan River colony, consists, not so much in storms, drought, failure of crops, and the various causes which attend the emigrant's earlier operations, and teach him, by painful experience, the disadvantages of his adopted land, but by the destruction among the flocks and herds that a deleterious plant is apt to occasion. At one time, before instituting the rigid experiments which follow, Mr. Drummond attributed the mischief to a species of *Lobelia*, a genus of acknowledged acrid properties; and it is to be feared that more than one vegetable shares in producing the injurious effects. The finest and strongest animals are the first to perish: breathing seems difficult, they stagger, fall, and die; while the evil effects which attend the eating of their flesh augments the calamity to the owners. The specimens to which Mr. Drummond refers, and which he has sent home, constitute a remarkable exception from the usual qualities of the natural order *Leguminosæ*."

Of all the bold and ardent naturalists we have ever read or heard of, Dr. Hostmann, of Surinam, appears to be the most enthusiastic and self-abandoned. This gentleman, a frequent

contributor to the Journal, has abandoned the practice of his profession, to follow his favourite pursuits. For this purpose he has accepted the situation of Government resident among the ferocious bush negroes, at a miserable stipend; to live, as he himself expresses it, secluded from all civilized society—to make his abode among sombre and interminable forests, the secure haunts of wild beasts, and still more ferocious men, and to find in the society of trees, and flowers, and harmless animals, a compensation for all privations.

Some of the adventures of this intrepid man are equal to any thing ever read of in De Foe's romances. The climate of Surinam being constantly humid, there is not much comfort lost by travelling in the rainy season; and as this period of the year presents some peculiar advantages for botanizing, Dr. Hostmann and another gentleman, assisted by the precarious help of the bush negroes, dared innumerable dangers during a four months' excursion from Paramaribo to Wana Creek. But we will permit Dr. Hostmann to relate some of his own adventures. He and his companions encountered one of those hideous snakes, known by the name of Jarrakooka (*Trigonocephalus rhombiatus*), which, having been roused from his dark abode, nearly killed one of the men; for there is no difference in this case between wounding and killing.

“The mere sight of this animal is sufficient to inspire dread, and to apprise the most careless wanderer of what he has to expect: a flat cordate head, fixed to a very slender neck, large protruding glittering eyes, with still larger cavities below them; the body inflated, and terminating in a spine, and covered with carinated scales, horny to the touch; these form an aspect which cannot be contemplated without making one feel sick. Nothing can be compared to the virulence of the poison ejected from the fangs in the jaws of this monster, except it be the astonishing rapidity with which it takes effect. The soundest frame is immediately affected, excruciating pain ensuing as soon as the wound is inflicted, followed by an effusion of *white blood* from all the apertures of the body, the muscular parts above the injured part are instantaneously destroyed by gangrene, and these awful symptoms suddenly terminate in death, the event being announced by putrefaction; but though the offensive exhalations never fail to attract a number of vultures, even the most voracious kind of this bird (*cathartes aura*) refuses to touch the putrid corpse. The snake, whose bite occasions these truly awful effects, is generally six feet long, of a dirty yellowish colour, with dark rhomboidal spots.”

We shall quote a few more passages from Dr. Hostmann's letters, abounding as they do in stirring incidents:

“There is nothing interesting to the naturalist at the Port Armina, unless it be the hundreds of thousands of *bats*, which may be seen

clinging under the roofs of the thatched dwellings, their whistling noise occasionally breaking the monotony of the day, and increasing with the advance of evening. As soon as the declining sun has sent forth his last gleam, off they all start, and, like a dark cloud, take the direction of the east, when immediately a small *black falcon* (*le Chasseur des Chauvesouris*, Buffon) pounces among them, and never fails to capture a few. This *bat*, a small animal, is that species which sucks the blood of all warm-blooded animals, and preferably that of the human race; and I must declare, having myself submitted to the experiment, that the way in which the creature sets to work is highly interesting. Guided by instinct, it selects the most remote part of the body, where there is least risk of being caught. I offered my bare feet to the attack of these bats, and soon had the satisfaction of seeing two of them busily engaged, one on each great toe. So gently did they lay hold, vibrating the air incessantly the while with their expanded wings, that I could not ascertain the precise moment of the bite. In a few minutes they appeared to have taken sufficient, and dropped off to the ground, when, to my great astonishment, I found the wounds to be pretty large, and of a triangular form  $\Delta$ ."

We cannot forbear making room for the following description of an Indian settlement :

"The departing sun gilded the shore of Cayenne, when we perceived, on the opposite side, an Indian settlement which seemed hospitably to bid us look thither for shelter, and on approaching its copper-coloured patriarchal inhabitants, their kindness did not belie the promise of their simple huts. These people belonged to the Gallina tribe, and their chief immediately ordered his men to aid us in passing a shoal, behind which our little fleet might find safe anchorage. An entire hut was placed at our disposal, and these good-natured aborigines, far from asking any present, offered us, with the utmost frankness, their little gifts. The chief set the example, with easy and unembarrassed manners, just as if it were a thing of course, he brought us a fowl, and the women each gave a cake of manioc, neither of which was at all despicable food. With equal frankness, these people requested some rum, and though the men seemed to relish this nauseous beverage, I am sorry to say, their partners far exceeded them, their passion for spirits being perfectly insatiable. Soon the liquor did its wonted work, and the aged chief, with savage eloquence, began relating his military exploits, just as if the affair had occurred yesterday, whereas, he was referring to the conflict between the free inhabitants and revolted slaves, upwards of eighty years ago! He must have been nearly one hundred years old to have taken part in this battle, and yet there was not a single white hair to be seen on his strong-looking skull, and he lived with three wives, the youngest of whom had an infant at her breast, and he looked as if he might last for half a century more."

We recommend the London Journal of Botany most strongly to all those that feel an interest in this delightful science; and the extracts we have quoted show that it contains also much that may interest the general reader.