vertebra is the "homologue" of the same element. "This metamorphosis or degradation of a whole quantity," observes Mr. MacLise, "would appear to be the law by which Nature creates a serial skeleton axis;" and as "this law of metamorphosis" may subtract an infinitesimal part, the variety of forms is infinite. The departure in the serial spinal axis is most remarkable, of course, in the caudal region, where we find at length but the mere terminal ossicle of the coccyx. In the ophidian reptile, on the contrary, there is a "thoracic skeleton series," consisting from first to last of costo-vertebral quantities.

We must refer our readers to Mr. MacLise's work for further illustrations of his views, as no particular advantage would be gained by transferring them to our pages.

ART. VII.

Principles of Medicine: comprising General Pathology and Therapeutics, and a brief general View of Etiology, Nosology, Semeiology, Diagnosis, Prognosis, and Hygienics. By CHARLES J. B. WILLIAMS, M.D., F.R.S., Professor of the Principles and Practice of Medicine, and of Clinical Medicine, and First Physician to the Hospital, University College, London, &c. &c. Second Edition, considerably enlarged.—London, 1848. 8vo, pp. 533.

Elements of General Pathology. By A. F. CHOMEL, Professor of Clinical Medicine to the Faculty of Paris, &c. &c. Third Edition, considerably enlarged. Translated from the French by F. E. OLIVER, M.D., and W. W. MORLAND, M.D., Members of the Massachusetts Medical Society.—Boston (New England), 1848. 8vo, pp. 458.

WE always feel a peculiar pleasure in welcoming a new edition of a really good medical book. We have a sympathy with every deserving author, who earns for himself the meed of professional approbation, to which his merits entitle him. And we rejoice at every indication of progress in the collective mind of the profession, such as is afforded by its appreciation of that which is really excellent,—especially when the excellence lies rather beneath than upon the surface. There can be no question that the verdict of the profession upon the abstract merits of a production, so far as can be estimated from its sale, is not always a correct one. It is rare, we believe, for a really trashy book to find much acceptance with the *medical* public; the *general* public, we believe, being the chief readers of such as have an extensive and constant sale, altogether disproportioned to their deserts. But, on the other hand, works of really high character are frequently very far from obtaining the attention they deserve. We could point to several treatises, containing the details of important discoveries having an immediate bearing on practice, which have fallen almost still-born upon the press. Their failure has been due, in some instances, to the circumstance of their being too much in advance of the professional mind of the time; whilst in other cases it has seemed rather to result from some qualities of the books themselves, which rendered them unpalatable to readers, who desire that the information for which they seek should be communicated in the clearest

and most concise form, and who dislike the introduction of personalities into scientific treatises. We believe that the failure of any really good medical work, that is not too expensive for the professional pocket, may be accounted for in one or other of these modes; and we advise every unsuccessful author to consider which is the one that is most likely to be operating to his disadvantage.

Now had Dr. Williams's '*Principles of Medicine*' been in this predicament, we should have been at no loss to have assigned the reason; for whilst we entertained a very high opinion of its merits, both as regards matter and manner, we had our misgivings whether it was not *too* good,—that is, too abstract and scientific in its character,—to find favour with a body so eminently practical in its tastes, as is the great mass of the medical profession. The call for a new edition within four years, however, has pleasantly dispelled our apprehensions; and has made us feel that the time is advancing when medicine shall be elevated from the rank of an uncertain art, to a place as near the exact sciences as the peculiarities of its character and objects will permit. It is with a justifiable pride that Dr. Williams is able, in the Preface to this Second Edition, thus to express himself in regard to his efforts at combining science with art, and at placing the practice of medicine on a more rational basis. "In endeavouring to adapt the work to the rapid improvement in medical science, it is most satisfactory to be able to state that in very few instances has it been necessary to retract or supersede the inferences and views set forth in the First Edition; on many subjects they have been confirmed and extended by recent researches, to a degree that has not less surprised me, than convinced me of their truth." And we can fully sympathise with him in the satisfaction which he derives from finding that his scientific views on many subjects "point, as it were spontaneously, to remedial measures closely corresponding with those which the best experience has sanctioned; and that they simplify and facilitate the indications of treatment in a manner that suggests more efficient modes of practice than could be obtained by blind experience."

We are not, however, by any means disposed to abandon our critical function in behalf even of so meritorious a production. We are not, by any means, in accordance with our author upon every one of the many questions which his work embraces; nor do we find the arrangement of the subjects, and the proportionate space assigned to them, in entire harmony with our own notions of what such a work should be. We may express our opinion freely upon all these points, we trust, without being charged with arrogance or dogmatism; the more especially, when we state that they are subjects which have long occupied our own sedulous attention. And that we shall devote so large an amount of our space to placing the distinctive merits of his treatise before our readers, and to the examination of those portions of it which seem to us of most questionable merit, will, we trust, be accepted by its author as a tribute of respectful consideration, which we feel a satisfaction in paying to his labours in this field; a *Second* Edition not being usually regarded as having a claim to more than a notice of the *novelties* it may contain.

"The PRINCIPLES, ELEMENTS, or INSTITUTES OF MEDICINE," observes Dr. Williams, at the commencement of his treatise, "comprise those leading and general facts and doctrines regarding disease and its treatment,

which are applicable, not to individual cases only, but to groups or classes of diseases."

"These principles," he subsequently continues, "may be deduced in part from a knowledge of animal structure and function (anatomy and physiology), conjoined with an acquaintance with the agents which cause and remove disease; but chiefly they are derived from a generalization of facts observed in an extensive study of disease itself, and its effects in the living and in the dead body. But so far as they have been ascertained, they become more intelligible to the student if explained synthetically, by describing first the causes of disease, then their operation on the body, and lastly, the resulting changes in function or structure which constitute disease in its more elementary forms." (p. 1.)

We fully approve of the plan pursued by Dr. Williams in thus making Etiology the first subject of discussion; and we are glad to find such considerable additions to the first chapter, in which the causes of disease are set forth, as render it an excellent epitome of the present state of our knowledge on the subject, enlarged as this has been of late years by the data collected by the promoters of sanitary reform. We must pause *in limine*, however, to comment on the manner in which the subject of Causation is treated; since we here find such a deficiency in that logical precision at which Dr. Williams has aimed, and generally with success, as is very likely to mislead the student on one of the most fundamental points of the whole inquiry.

"A great variety of agents and circumstances may thus act on the body so as to produce disease; but in most instances there is not that uniform and constant relation between these as causes, and the diseases as effects, which we might expect from the analogy of causation in the simpler sciences. In chemistry or in mechanics, effects certainly and uniformly follow causes; in physiology or pathology, no doubt, effects also ensue; but whether these effects shall be manifest as disease or not, will depend on many circumstances, of which we often cannot take cognisance. It is true that when the causes resemble, and act like those of physics or chemistry, their proper effects will not fail to ensue. Thus, a cutting instrument, a red-hot iron, or a corrosive liquid, will not fail to produce disease; because its operation is so energetic, as to overcome all vital properties by physical and chemical force, and disorder must follow. Further, certain poisons and other potent agents, which act on, without destroying, the vital properties of living parts, may also, if of sufficient strength, pretty constantly produce morbid effects. Thus opium taken internally causes somnolency; tartar emetic excites nausea and vomiting; cantharides applied to the surface induces inflammation, &c.

"But the common causes of disease are seldom of this decided and positive character; they are often present without disease ensuing; and they are known to be causes only because disease is observed to ensue in a greater number of cases when they are present than when they are absent. Thus, improper food is a cause of indigestion, and exposure to cold is a cause of catarrh; yet many persons eat unwholesome food, without suffering from indigestion, and many are exposed to cold, without 'taking cold.' But those who *do* suffer from indigestion, observe that they do so more after taking improper food; and those who are affected with catarrh can often trace it to exposure to cold. The reason of this uncertainty of action is chiefly in various powers by which the body resists the morbid influence; which powers vary much under different circumstances. The failure or irregular operation of this power constitutes one predisposition to disease." (pp. 5, 6.)

Now the tendency of these statements appears to us to be, to put into the mind of the student the idea, that the relation of cause and effect in what concerns the living organized body is less certain and definite than that which prevails in the inorganic world; an idea alike false and

pernicious. It is false, because the very term *cause* involves the idea of invariable sequence. And it is pernicious, because the physiologist or medical practitioner having once adopted it, is likely to be continually recurring to it for an easy solution of all the difficulties he meets with; and to satisfy himself with the notion that he gives a sufficient account of a variation in the results, when he sets it down to the uncertainty in the action of the causes, and holds back from the inquiry into the *sources* of the variation, which will seldom altogether elude a patient, sagacious, and diligent investigation. We are far from imputing such a tendency to Dr. Williams himself; for the whole of his book is an evidence to the contrary. But a teacher cannot be too careful to give to his pupils at the outset clear and logical conceptions of the fundamental elements of the inquiry through which he desires to guide them; and we shall, therefore, not apologise to our readers for detaining them a little upon this topic. The following quotation from the writings of one of the greatest authorities of the day, places the matter in so clear a light, that we shall have nothing else to do than to illustrate and apply the doctrines there laid down. After stating that "the law of causation, the recognition of which is the main pillar of the inductive philosophy, is but the familiar truth that invariability of succession is found by observation to obtain between every fact in nature, and some other fact that has preceded it;" so that "for every event there exists some combination of objects or events, some given concurrence of circumstances, positive and negative, the occurrence of which will always be followed by that phenomenon;" Mr. Mill thus continues:—

"It is seldom, if ever, between a consequent and one single antecedent, that this invariable sequence subsists. It is usually between a consequent and the sum of several antecedents; the concurrence of them all being requisite to produce, that is, to be certain of being followed by the consequent. In such cases it is very common to single out one only of the antecedents under the denomination of a cause, calling the others merely conditions. Thus if a man eats of a particular dish, and dies in consequence, that is, would not have died if he had not eaten of it, people would be apt to say, that eating of that dish was the cause of his death. There needs not, however, be any invariable connexion between eating of the dish and death; but there certainly is, among the circumstances which took place, some combination or other upon which death is invariably consequent; as, for instance, the act of eating of the dish, combined with a particular bodily constitution, a particular state of present health, and, perhaps, even a certain state of the atmosphere; the whole of which circumstances perhaps constituted in this particular case the *conditions* of the phenomenon, or in other words, the set of antecedents which determined it, and but for which it would not have happened. The real cause is the whole of these antecedents; and we have, philosophically speaking, no right to give the name of cause to one of them, exclusively of the others. What, in the case we have supposed, disguises the incorrectness of the expression, is this, that the various conditions, except the single one of eating the food, were not *events* (that is, instantaneous changes, or successions of instantaneous changes), but *states*, possessing more or less of permanency; and might, therefore, have preceded the effect by an indefinite length of duration, for want of the event which was requisite to complete the required concurrence of conditions; while as soon as that event—eating the food—occurs, no other cause is waited for, but the effect begins immediately to take place: and hence the appearance is presented of a closer and more immediate connexion between the effect and that one antecedent, than between the effect and the remaining conditions. But although we may think proper to give the name of cause to that one condition, the fulfilment of which completes

the tale, and brings about the effect without further delay, this condition has really no closer relation to the effect than any of the other conditions has. The production of the consequent required that they should all *exist* immediately previous, though not that they should all *begin* to exist immediately previous. The statement of the cause is incomplete, unless in some shape or other we introduce all the conditions." (Elements of Logic, vol. i, p. 400.)

We can scarcely have a better illustration of this principle than is afforded by the analysis we have elsewhere given (p. 81 et seq.) of the circumstances under which the fearful outbreak of cholera at Kurrachee took place. When a destructive epidemic occurs, we are a great deal too apt to fix upon the morbid agent supposed to be conveyed by the atmosphere, or to be communicated from individual to individual, as *the cause* of the fatal results. It is allowed that the influence of this cause may be modified by certain conditions affecting individuals or classes; which render some of them more and others less subject to its operation. But from the prominence of the *event*, we are too prone to overlook the fact that the previous *state* is as necessary for the concurrence of the conditions requisite to produce the result in any one case, as is the atmospheric or contagious poison. The fact that, of a large number of persons equally exposed to the same hypothetical agent, only a certain proportion will be affected by it—some fatally, others comparatively mildly, whilst others escape its influence altogether,—does not by any means indicate an uncertainty in the action of the supposed cause; but merely shows that it is only one out of several causes, all of which must be in concurrent action to produce an uniform result. Thus, in the case in question, we find that the access of the choleraic poison was followed by a mortality of only 6·91 per cent. in the division of the army least affected by it; that the result, in another division of the army, was a mortality of 10·86 per cent.; whilst in a third the mortality was as high as 21·8 per cent. We cannot but be here struck by the fact, that the difference between the highest and lowest rate of mortality should be positively greater than the latter itself; showing that some other conditions must be in operation, at least equally tending to a fatal result, with the access of the cholera poison itself. And from our analysis of the circumstances of each case, we have found reason to believe that the comparative exemption of the first division was due to the favorable character of its residence; whilst in the second and third cases imperfect ventilation of the tents was a concurrent condition; the injurious influence of this being aided in the latter, by the depressed condition of the system induced by previous fatigue. Now it would be just as philosophical to say, that in the first case 6·91 per cent. died of cholera alone,—in the second, 6·91 per cent. of cholera, and 3·95 per cent. of insufficient ventilation,—and in the third, 6·91 per cent. of cholera, 3·95 per cent. of insufficient ventilation, and 10·94 per cent. of previous fatigue,—as it would be to say that, in any one case, death took place from the agency of the choleraic poison *only*. In all cases, a certain concurrence of conditions is requisite; and *all* these conditions have an equal right to the appellation of *causes*. The deficient ventilation of the tents, and the fatigue of a long previous march and excessive drill, were just as direct causes of the severity of the cholera epidemic at Kurrachee, as was the presence of the cholera-poison itself. Neither could have produced the actual result, without the concurrence of the

rest. The cholera-poison could have no more killed 10·86 per cent. of the second division, without the concurrence of bad ventilation, than bad ventilation could have killed them without the cholera-poison; the fatigue induced by marching and drilling could have no more killed 21·8 per cent. of the third division, without the cholera-poison and insufficient ventilation, than these, separately or in conjunction, could have produced this mortality in the same period without the cholera-poison.

But whilst the (presumed) access of the cholera-poison was an *event*, which completed the required concurrence of conditions,—the other conditions being *states* of the bodily system induced by bad ventilation, fatigue, &c.—it is easy to show that it does not thence become entitled to rank as more of a cause than the rest; since it cannot be doubted that the same result would happen, if the order of action were inverted. Thus we will suppose the cholera-poison to become endemic (as it has sometimes appeared to be) in a particular district; so far as regards those habitually exposed to it, therefore, its influence will be a *state*, not an event. Then let a regiment of soldiers, living in a choleraic district, be confined for a time in close tents, or be forced to perform a fatiguing march, or be subjected to both these conditions, concurrently; there can be no question that the mortality will undergo an enormous increase, of which we might seem justified in asserting that these *events* are the cause.

Hence we arrive at the conclusion that it is utterly unphilosophical to single out any one of the conditions whose concurrence is necessary to produce the result, as *the cause par excellence*, and to look upon the rest as accidental or subordinate concomitants; whilst it is equally unphilosophical to entertain any doubt as to the certainty of the result, whenever the concurrence of causes or conditions is identically the same. A difference in the results as certainly implies a difference in the causes, in physiology and pathology, as in physics and chemistry. When we bring individual cases into comparison, it is often very difficult to fix upon the points of difference to which the diversity of results may be due; for, on the one hand, they may be so numerous as to leave us at a loss to discriminate between the important and the trivial; whilst, on the other, there may be such an absence of any ostensible variation, as to leave us only the convenient but most unsatisfactory resource of idiosyncrasy or individual peculiarity of constitution. It is in such investigations that statistical data afforded by large numbers of individuals are so important; since they both serve to fix our attention prominently upon differences of results occurring under apparently the same circumstances; and, when their basis is sufficiently extended, they substitute the average of the mass for the isolated phenomena of idiosyncrasy, and force us to the conviction that any decided difference in result (as, for instance, in the mortality of different districts, or of different collections of individuals, during a given period, or during the prevalence of a particular epidemic) must be due to a *discoverable* difference of causation,—such individual peculiarities as are really undiscoverable forming so small a proportion of the whole, that in an investigation of this kind they may be practically disregarded.

It might be thought that our ordinary etiological language is sufficiently correct, to render it unnecessary that we should dwell upon this topic; since it includes the previous *states* and *events* alike under the designation of *causes*, predisposing and exciting. But although this may

be the case in formal scientific treatises, we do not think that the truth is recognised in ordinary professional habits of thought. Thus in the manifold discussions which have taken place on the contagion of fever, cholera, &c., it has been maintained by one party that contagion is *the* cause of the propagation of these diseases, whilst another, with similar exclusiveness, finds in neglect of hygienic principles alone the source of their development and extension. Yet the former cannot but admit that certain predispositions, established by previous inattention to the laws of health, most powerfully concur with the contagious miasm in the production of an epidemic; whilst the latter can scarcely blind their eyes to the proofs, that in the epidemic prevalence of many forms of fever (to say the least) contagion plays a very important part. Each party, when the case is logically stated, must thus admit the action of at least two concurrent causes; and the difference between them lies chiefly in the relative *degree*, in which these are supposed to operate in the production of a given result.

Returning now to Dr. Williams's 'Principles,' we may dismiss the portion of the etiological chapter which relates to the Predisposing causes of disease, with the simple remark, that a very comprehensive, and at the same time clear and concise, summary is given of these; such as will furnish the student with a useful guide in the direction of his observations, without burthening his memory with a mass of details. We are surprised, however, to find no notice under this head of the predisposition to disease induced by defective cleanliness, ventilation, &c.; which are treated under the next head, as Exciting causes of disease. In this section, considerable additions have been made; especially in the portions which treat of mechanical, chemical, and dietetic causes of disease, defective cleanliness, ventilation, and drainage. The following table shows the classification adopted by Dr. Williams, and the range of subjects embraced under this head. The division between predisposing and exciting causes appears to us to be purely artificial; and we are not sure that any real benefit is derived from the perpetuation of it. Many of the causes here set down under the latter head may be as well, or perhaps better, classified under the former. Thus the effects of deficient or improper food, of imperfect ventilation, of mental exertion, &c., very frequently manifest themselves, not in the production of any specific disease, but in predisposing the system to suffer from the attacks of almost any disease whose special exciting cause may be brought to bear upon it. In fact, as we have attempted to show with regard to the Indian cholera, almost any class of causes may be either predisposing or exciting, according to the manner and degree of their operation.

	{ 1. Mechanical 2. Chemical 3. Ingesta 4. Bodily exertion 5. Mental emotion 6. Excessive evacuation 7. Suppressed or defective evacuation 8. Defective cleanliness, ventilation, and drainage 9. Temperature and changes
I. Cognisable Agents . . .	
II. Non-Cognisable Agents	{ 1. Endemic 2. Epidemic 3. Infectious.

The attention that has been recently paid to the subjects of ventilation, drainage, &c. by (we may presume) the great mass of our readers, spares us the necessity of noticing, at any length, the additional matter introduced by Dr. Williams under these heads. We may remark, however, that the personal experience of the practical physician, together with the acute perception of the scientific physiologist, manifest themselves throughout. The latter is displayed in the following suggestion with regard to the cause of the oppressive feelings produced by respiration in a damp atmosphere.

"The ill effects of deficient ventilation are increased by heat and moisture; the former operating not only by increasing the animal exhalations, but also by rarefying the air, and thus reducing the amount of oxygen in a given bulk: moisture probably acts in a degree in like manner, but also, as I conceive, by removing the difference between the air respired and that in the lungs, which promotes that diffusion or interpenetration of gases, on which the access of oxygen to the vesicular structure of the lungs depends. For, be it remembered, the air taken in at each inspiration is not enough to reach far in the tubes; its transfer into the air-cells is accomplished by the law of diffusion of gases, which operates in proportion to the dissimilarity between the gases; and difference in amount of contained watery vapour must exemplify this law." (p. 48.)

The following remarks we quote on account of their practical importance. The medical practitioner may think the subject of them out of his province; but he must learn to think the investigation of the *causes* of disease to be really of more certain benefit to mankind, than his attempts at the *cure* of diseases already established. The results of the removal of the former are capable of being determined with considerable accuracy; those of the latter are as yet problematical, and are liable to be called in question by the opponents of received systems. If a doctor does not think it beneath him to inquire into the drainage of his patient's system, to test his urine, or to inspect his fæcal discharges, we cannot see why he should hesitate to make personal inquiry into the state of the drains of his patient's house, when he suspects that their inefficiency is a cause of his disease. After speaking of the noxious operation of the cesspool, Dr. Williams thus continues:—

"Scarcely less injurious, and more insidious in its operation, because the effluvium is less offensive, is the untrapped drain in connexion with the sewers of large towns. This cause of disease exists extensively in London, not only in the street drains, which are always open, and emitting the gases of the sewer, the bad odour of which is perceptible in certain winds; but also in the drains of houses, which are either intentionally or negligently left open, or are not air-tight, from the absence of water in the traps. Nothing is more common than to perceive the peculiar smell of the drain on entering a house, and in many instances I have found that this has proceeded from the trap left open, or dried up, and therefore inoperative, and requiring only the simplest expedient to stop the evil. When a single trap is open in a house, especially in the winter, when doors and windows are closed, and there is no adequate supply of air for the fires in the house, the foul air is drawn up from the sewer in a strong current, and quickly pervades the house from bottom to top, carrying with it a pernicious influence. It is surprising how ignorant servants and employers, and even professional men, are on this point, which so immediately concerns their health and comfort; and I have visited in many houses where this has seemed to be a cause of illness or impeded convalescence, in low nervous fevers, bowel complaints, influenza, neuralgia, headaches, and other ailments. In some instances, the leakage may be in consequence of the

inroads of rats, or the displacement of the brickwork of the drains. It may be useful to state, that besides by the smell, which is not obvious to every one, the effluvia of drains may be detected by the darkening of white paint, and the early spoiling of meat in the lower basement story of the house." (p. 50.)

We now pass on to the second chapter, which treats of "Pathology (proper)—the Nature and Constitution of Disease;" and as we deem our author's method of discussing this subject to be highly philosophical in its conception, and to be the one best adapted to meet the progressive requirements of both teacher and student, we shall place it before our readers as fully as our limits will allow; not, however, without some criticism upon certain of the details of Dr. Williams's system. He commences by remarking, that as the change in the natural, functional, or structural condition of the body, which we call disease, is generally more or less compound, involving several elementary functions or structures, it is obvious that we cannot obtain an accurate knowledge of the nature of disease as a whole, until we have ascertained that of its component parts. And as the chemist applies the term *primary element* to substances that cannot be analysed or divided further, and the term *proximate element* to compound bodies of simple constitution, which act as single bodies in forming and giving the character to more complex compounds,—so the physiologist has his primary or ultimate elements of structure (the *elementary tissues*), and primary elements of function appertaining to that structure, which, when disordered, become to the pathologist the primary elements of disease; whilst there are secondary or proximate elements of structure (the various *organs*), and corresponding elements of function, which, when disordered, become the secondary or proximate elements of disease. The varieties of disorder affecting the proximate or the ultimate elements may be comprehended under the heads of *degree* and *kind*; degree including *excess* or *defect*, or alterations of plus and minus;—and kind relating to changes not comprised under these heads, but otherwise expressed by the term *perversion*. The following is Dr. Williams's table, representing his classification of the primary or ultimate elements:

TABLE I.—PRIMARY ELEMENTS OF DISEASE.

PRIMARY CONSTITUENT.	FUNCTION.	DISORDER.	STRUCTURAL DISEASE.
Contractile Fibre	{ Irritability { Tonicity { Sensibility { Vol. Motion { Reflex action { Sympathy { Secretion		
Nervous Structure			
Secreting Structure			
Constituents of the Blood:—			
Red particles			
Fibrin and white globules		{ Excess { Defect { Perversion	Hypertrophy
Albumen			Atrophy
Oil			Degeneration, &c.
Salts			
Water			
Changes of the Blood:—			
By Respiration			
— Secretion			
— Assimilation			
— Foreign matters			

(p. 68.)

We fully admit the difficulty of carrying out the views just expressed with entire consistency; but we think that our readers must be struck, like ourselves, with one glaring error in this table;—namely, that *changes* of the blood are set down under the head of structural constituents. Surely these changes are *functions* of the constituents of the blood, and should have stood opposite to them in the second column of the table. We shall have another fault or two to find, when we have brought the second table under consideration; but it will be but just to Dr. Williams, first to quote his preliminary explanation:

“In the choice of proximate or secondary elements of disease, we must be the more arbitrary and less comprehensive; otherwise we shall encroach upon the domain of special pathology. The vascular system and the nutrient function so intimately connected with it, present us with the best and most important examples of proximate elements, comprising, as they do, two or more of the preceding ultimate elements (irritability, tonicity, constituents of the blood, secretion, &c.) yet so universal throughout the body as to belong to the province of general pathology.” (p. 69.)

TABLE II.—PROXIMATE ELEMENTS OF DISEASE.

The Blood in Circulation	Defective—Anæmia	{ General Partial	{ Circulation	Increased—Sthenic Diminished—Asthenic
	Excessive—Hyperæmia	{ General—Plethora Partial—Local hyperæmia	{ Circulation	Increased—Determination Diminished—Congestion Partly incr. } Inflammation Partly dim.
	Perverted—Cachæmia			
Nutrition of Textures	Defective—Atrophy Excessive—Hypertrophy	{ Degenerations Deposits Growths		
	Perverted			

(p. 69.)

Now, it strikes us very forcibly, that Dr. Williams has not by any means carried out his own plan in this distribution of his subjects; as must, we think, be at once apparent to our readers from the repetition in the second table, under the head of “Nutrition of Textures,” of some of the very same subjects—for anything, at least, that appears to the contrary—as were comprehended in the first. And we believe that a careful analysis of both tables will show that each includes subjects which properly belong to the other. It is in no fault-finding spirit that we make these remarks; but simply with a desire to aid the laudable endeavours of Dr. Williams in giving to pathological inquiry its right direction. Now commencing with the primary constituents of the animal body, we find contractile fibre occupying the first place; and its two modes of action, designated by the terms irritability and tonicity, hold a corresponding

place under the head of function. The function may be disordered by excess, defect, or perversion; and the structure may exhibit hypertrophy, atrophy, or degeneration. Coming next to the nervous structure, we find, to our surprise, sensibility, voluntary motion, reflex action, and sympathy set down as its elementary functions; no distinction being made between the vesicular and tubular forms of nervous matter, and no recognition being given of the fact that they have distinct and independent functions, which must be considered as the primary or elementary operations of the nervous system, analogous to contractility and tonicity in the muscular; whilst sensibility, voluntary and reflex action, and sympathy, must be regarded as compound functions resulting from the association of the simpler ones, just as certain associations and connexions of vesicular and fibrous matter make up the complex organs termed brain, spinal cord, or ganglia. The elementary structures and functions concerned in animal life having thus had the first places in the classification, we might have expected next to meet with an enumeration of those by whose aggregation the vegetative life of the organism is made up; from these, however,—on what account we cannot divine—we find only the secreting structure and the constituents of the blood selected; the other tissues, their properties and functions, being wholly passed by.

In regard to the proximate elements of disease, enumerated in the second table, we have already remarked that the simple changes in the nutrition of elementary textures, which form its lower division, constitute a mere repetition of a part of the preceding group. The condition of the blood in circulation, on the other hand, we regard as a very fair specimen of the real “proximate elements of disease;” and it might not have been difficult to extend the list by introducing similar groups founded upon the disorders incident to other complex functions, such as those of the nervous system, which we would transfer from the first table, with those of respiration, secretion, &c., which are themselves complex as regards the number of elementary changes on which they depend, but are simple in their relations to the system as a whole. We fully admit that, in the present state of pathology, it would be impossible to establish a really definite and stable classification of ultimate and proximate elements of disease; but this is no reason for not making the attempt. If the chemist were to wait for his ultimate elements and proximate principles to evolve themselves, he might wait long enough; and the only true way of finding truth in any science is first to find out where she is to be looked for. We do not say that if we were to make a classification of our own, it would be, on the whole, more free from objections than Dr. Williams’s; and we would again beg that our criticisms may be viewed simply in the light of contributions towards a possible arrangement that should be superior to either. We again repeat that his *method* appears to us to be thoroughly philosophical; and the more strictly it is carried out in accordance with sound physiology, the more perfect will be, in our apprehension, the basis thus laid for a sound pathology.

The treatment of the individual subjects embraced in the foregoing tables is on the whole very clear and satisfactory, considering the very narrow limits within which it was necessary to condense a great mass of details. In regard to muscular irritability, Dr. W. holds the doctrine which we have always advocated, that it is a property inherent in muscular

tissue; and in speaking of its excess, he clearly distinguishes between the increase of irritability as shown in a readiness to contract on a slight stimulus, and the increase manifested in the force or duration of the contractions. As we have shown on a former occasion (vol. i., p. 358), the use of the term irritability in two distinct senses has led to much confusion, and will continue so to do, so long as it shall be persisted in; and we could wish, therefore, that Dr. Williams had not applied the term in a manner which recognises both these senses, involving, as it does, the palpable inconsistency of setting down two contrary states as manifestations of the same power. For we are told (and correctly so) that "inordinate mobility of muscle often co-exists with want of power or completeness in the contractions; as is exemplified in the irritable heart, which, although acting very frequently, does not expel its contents so vigorously as in health." (p. 71.) But in the next page we learn that deficient force of contraction is a manifestation of deficient irritability; and as the proposition just quoted might be as correctly put in this form,—"Want of power or completeness in the contractions often co-exists with inordinate mobility of muscle,"—we might have the very same condition of the heart ascribed to either increased or diminished irritability, according as we intend specially to designate its mobility, or its contractile force.

The section on tonicities we regard as peculiarly valuable; since it gives a definite and local meaning to terms which practical men have employed vaguely and erroneously; and thus lays a foundation for the correct apposition of a considerable number of morbid phenomena, the relation of which is commonly lost sight of. We quote the following paragraph as a good illustration of Dr. Williams's method of treating the several ultimate elements of disease:

"Where tonicities are *defective*, the muscles are flabby, and incapable of continued exertion, but are sometimes too irritable with the tremulousness of debility. The heart likewise is irritable, and often exhausts its strength in palpitation; the pulse is soft and yielding; it may be full when slow, and sharp when frequent; but it is without firmness or endurance, and is easily accelerated. Another destructive character is its retardation, increasing the interval between the heart's beat and distant pulses; so that the radial pulse is often felt after the second sound of the heart is heard; the tubes being less tense, the pulse-wave is slower than usual. Sometimes the absence of that tightening of the walls of the arteries by which the tonic fibres control their movements, permits their mechanical elasticity to come into play, and this reacting after each stroke of the heart, gives that peculiar reduplication or rebounding of the pulse, which has been long described under the term *dichrotous* pulse. This is often observed in convalescence from fevers, and other diseases after the subsidence of vascular excitement. A loose, relaxed state of the vessels renders the circulation in distant parts weak, so that the extremities are cold, whilst the head and internal organs may be congested. Sudden exertions or change of posture may disturb the circulation, and cause faintness or giddiness. Want of tone also in the stomach and intestines causes indigestion and costiveness, and permits them to become distended with wind and accumulating fæces. The secreting organs, irregularly supplied with blood, are also liable to disorder, being either scanty, depraved, or profuse and watery.

"It is quite obvious that a person in such a condition must be prone to various diseases. He has no resisting power against malaria, infection, or other depressing agents. If he is exposed to cold, the blood is readily driven through the weak vessels into the interior, where it causes congestion or inflammation. The weak intestines have no power to expel offending matter from them. Thus

the system in a state of atony is open to the action of many exciting causes of disease; besides being itself, in many respects, on the verge of disease, especially congestion and its consequences, and other derangements of the circulation." (pp. 77, 78.)

The section on the functions of the nervous system presents little that calls for remark. We have already pointed out what we regard as a fault in this part of Dr. Williams's classification; and we need not here do more than express our opinion, that nearly the whole of this section might with propriety be transferred to the chapter on the *proximate* elements of disease. The functional derangements which constitute the *ultimate* elements of disorder of the nervous system, appear to us to be those which relate to its general power of receiving and responding to impressions; whether these be of a kind which produce sensations and thus arouse the higher mental operations, excite reflex motions, or modify the vegetative functions. There is sufficient reason, we think, for regarding all these disorders as dependent upon similar derangements of the property of excitability; one phase or another presenting itself, according to the part of the system more especially affected; and all not unfrequently manifesting themselves together or in succession in the same individual. There seems to us to be the same kind of fundamental relation between these disorders, as there is between the weak and irregular action of the heart and the torpor of the bowels, which, as just shown, are manifestations of the general deficiency of tonicity in the muscular system. The state to which we refer must be familiar to every experienced practitioner, and it is not passed over by Dr. Williams. After noticing the excessive sensibility manifested in determination of the blood to the encephalic nervous centres, and in the early stage of inflammation, as shown in intolerance of light, noise, or motion,—Dr. Williams refers to the phenomena of hydrophobia and tetanus, which he speaks of as proceeding from excitement without inflammation. Now, whatever may be the case with regard to the former of these diseases, we think it must be apparent that those of the latter are not dependent upon an increase of *sensibility*; since they present the most complete and typical illustration possible of increased excitability, manifesting itself in the production of reflex motions from the slightest impression, in which sensation does not necessarily participate. Let this increased excitability affect the spinal cord, and we then have tetanic spasms. Let it affect the ganglia of sensation, and we then have intolerance of light, sound, &c.; and if it also involve the motor apparatus connected with them, we have the paroxysms of hydrophobia. If the cerebral structure be the part specially affected, we shall have excessive mental excitability; which may be manifested either in undue but imperfect activity of the intellectual powers, or in violent emotional agitations. Now all these conditions, which are seen in their completest development when specially and singly exhibited, are presented in combination, though less obviously and strikingly manifested, in persons suffering under undue excitability of the nervous system in general, rather than (as represented by Dr. Williams) under excess of sensibility. His description of this state is a true and graphic picture.

"Such persons are commonly called nervous; they are worried with trifles; startled at shadows; distracted by noise or bustle; never free from some ache or pain; for almost every feeling is suffering; and what in others would be slight

pain, in these amounts to agony. Hence they are perpetual invalids, quite unfit for the rugged path of life, over which they, as it were, walk barefooted and thin-skinned. If real disease attack them, its nervous symptoms are so much exaggerated, that a medical attendant is apt to fall into the error of either ascribing all to the 'nerves,' or of measuring the disease by the severity of the symptoms. This over-sensibility is generally conjoined with excess of irritability and want of tone. Other nervous functions, such as sympathy and reflex actions, are also augmented or in disorder. The symptoms connected with sensation most frequently present are neuralgic pains of various parts, excessive sensibility of the surface, headache, pain in the back or left side, and spinal tenderness." (p. 80.)

This undue excitability of the nervous system is one of the most prominent features of the disorder which, for want of a better name, we term hysteria; and any one who will take the trouble to analyse its proteiform manifestations, will come, we feel assured, to the conclusion that one general state is common to all those in which there is an excess of action, whether of the sensory or motor system; the variations in the phenomena being due to differences in the part of the nervous system chiefly affected.

Considerable additions are made in this edition, under the head of "Diseases of reflected and sympathetic nervous influence;" and these show a full acquaintance with all that physiology can contribute towards their elucidation. As, however, they contain nothing of absolute novelty, we need not detain our readers with any further reference to them.

The next section, on Diseases of Secretion, undoubtedly stands in its proper place, as treating of one of the really ultimate elements of disease. There is no operation in the body whose nature is more elementary; and there is none which is more intimately connected with the maintenance of life and health, through its influence on the purity of the blood. The subject is treated by Dr. Williams with great clearness; considering, however, its vast importance, both in a scientific and practical point of view, we think that this section might have been a little more expanded with advantage. We quote the following passages as an illustration of the important aid derivable from the study of the function in its totality, according to the method pursued in this work.

After noticing the chief peculiarities of the biliary and urinary excretions, and pointing out the necessity of a balance or due proportion between them, the author continues:—

"Whether the materials from which these eliminating processes are supplied be the principles of the blood itself, or the decayed constituents of the tissues, or matters derived from the food, the co-operation of all these processes will be generally required to maintain a uniformity in the composition of the circulating fluids; so, too, if one of these processes is more active than the others, the blood must suffer by the excess of those matters which the less active processes allow to accumulate in it. A clinical illustration of this position may be found in cases of bilious diarrhœa or cholera. This flux of bile is either accompanied by a highly loaded state of the urine, or by fever; in the latter case, the fever does not subside until the urine becomes very copious, or deposits an abundant sediment. The most probable interpretation of this fact is, that the excessive secretion of bile disorders the composition of the blood; so long as the kidneys rectify this disorder, by separating in greater abundance the solid contents of the urine, no fever results; but if the kidneys fail in this task, fever ensues, and continues until they accomplish it; then a free secretion and copious deposit is symptomatic of the decline of the fever." (p. 103.)

"If an excessive secretion have already caused febrile disturbance, great advantage will be found to result from the use of means which increase other secretions, and thus restore the balance before explained. Thus, in bilious cholera, saline diuretics and diaphoretics are highly serviceable. In renal irritation with copious secretion of lithic acid, blue pill, which augments the secretion of bile, is often beneficial. These means may be supposed to operate partly as derivants; but the manner in which they remove the febrile irritation, after the reduction of the excessive secretion, renders it most probable that they act also by removing from the blood dregs left by the inordinate separation of the matter of the single secretion which has been in excess. No practical physician can doubt that we possess medicines which often augment the secretions of particular organs (mercury that of the liver and salivary glands, colchicum that of the kidneys, &c.), yet there is a limit to the operation of these agents; but this limit may be increased by simultaneously acting on other organs which maintain the balance. Thus, in any disturbance of the secretions, especially if it continue long, combinations of medicines are much more useful than those fulfilling one indication only; and thus experience has sanctioned the practice of conjoining mercurials with diuretics, and antimonials with salines, &c." (p. 105.)

The following remarks are equally philosophical and practically valuable:

"The positively noxious properties which excrementitious matter retained in the blood is known to possess, must be taken into account when we attempt to explain the states of constitutional irritation and depression, with perversion of functions, which fevers so generally present. The changes in the blood, manifest in some such cases by its fluidity and by petechial appearances, may also be in part referred to defective elimination of effete matter;* and it is when the secreting organs recover their power, and a diarrhoea occurs, or a copious discharge of highly-loaded urine, that these appearances cease. It is very probable that severe mechanical injuries or shocks, and animal and other poisons, operate by thus injuring the vital powers by which the blood is continually purified from its own noxious products; and that this is a part of their mode of action seems almost certain, from their effect in suppressing or impairing the natural excretions. Accordingly, in such cases urea has sometimes been detected in the blood." (p. 107.)

An extremely interesting case, fully bearing out this view, has recently been put on record by Dr. Shearman. (Edinb. Monthly Journal, March, 1848, p. 666.) A boy of eight years old was run over by a truck, which passed across his loins, evidently inflicting some considerable internal injury. From the collapse which at first supervened, he recovered, under the influence of warmth and stimulants; but he passed no urine for thirty-six hours after the accident, and that which was then discharged contained a large quantity of blood. Dr. Shearman examined this bloody urine most carefully; but failed to detect the least particle of urea or urates in it. About sixty hours after the accident, there was a considerable access of fever, with increased pain in the region of the kidneys; and these symptoms, in the course of two days, were succeeded by coma. The boy was then bled from the arm; and on making a chemical examination of the blood, urea was most distinctly detected in it, and in considerable quantity,—the urine, at the same time, not containing a particle of urea, urates, uric acid, or albumen, and its specific gravity being only 1.005. Dr. Shearman then got his little patient under the influence of mercury as quickly as possible; and as soon as the constitutional effect of this

* "Purpura I have found to be often connected with hepatic congestion and imperfect excretion of bile, and to be most effectually removed by remedies which promote the restoration of the proper secretion."

agent was produced, urea gradually reappeared in the urine, and its specific gravity increased. By degrees the comatose symptoms subsided, and in the course of five weeks his usual health was re-established. This case is headed "Suppression of the Secretion of Urea by the Kidneys, and Absorption of Urea into the Blood;" but we apprehend that it would be more correct to substitute for the latter half of the title, "Accumulation of Urea in the Blood;" since there could have been no *absorption*, unless the urea had been first *produced* by the action of the kidney, which would not seem to be the case. When the liberation of carbonic acid by respiration is checked, and it consequently accumulates in the blood, we do not speak of this gas as absorbed into the blood; since its presence there is the result of the changes which take place during the passage of the fluid through the systemic capillaries; and we apprehend that the same is the case with regard to the presence of urea in cases of suppressed secretion.

We must indulge our liking for this chapter by another quotation, in which a sound principle of treatment is well enforced.

"The specific stimuli of the secreting organs, if used in excess, or too long, may not only cause general weakness, but also exhaust the vital powers which they excite; and the result may be a diminution either of the secreted fluid, or of its most characteristic constituents. Hence the long and excessive use of mercury causes torpidity of the liver; that of purgatives, imperfect action of the bowels; that of diuretics, scanty urine, or albuminous or watery urine, defective in urea. These facts point out the expediency of intermixing the use of these agents, and of alternating or conjoining them with others calculated to improve the vital properties of the textures generally, which may often be effected by the medicines called tonic, and by regiminal means which improve and equalize the state of the circulation, and preserve the digestive and assimilative functions in the best order. In illustration of this position, I may refer to the acknowledged advantage of giving bitters with or after mercurial courses; chalybeates with or after saline aperients or diuretics, when these are long used; and these additions, which alone, or used at first, would check the secretion to be increased, now sustain it, and render it permanent. Some medicines which are inferior in efficacy to those already named, are yet, in some instances, more eligible for chronic cases of defective secretion, because they are less exhausting, and combine some measure of tonic influence with that of increasing the secretions. As examples of this kind may be named taraxacum, preparations of iodine, sarsaparilla, nitric and nitro-muriatic acids. Courses of these medicines are sometimes of great efficacy in keeping free the secretions, after they have been restored by more powerful means; and they likewise often improve the functions of digestion and nutrition." (pp. 108-9.)

We are rather surprised not to meet in this chapter with a distinct recognition of the importance of the large extent of glandular surface presented by the intestinal walls, especially in the large intestine, as a means of depurating the blood. It has been too much the custom to regard the *faecal* evacuations as little else than the indigestible residue of the food, mingled with portions of the biliary and pancreatic secretions; whereas we think that a little consideration serves to show that the peculiarly *faecal* matter is a real excretion, which must have been eliminated from the blood by the glandulæ of the intestinal walls. The undigested residue of the food may form a greater or smaller proportion of the *bulk* of the evacuation, according to the nature of the ingesta and the completeness of the digestive process. When the alimentary canal is in an irritable state, and the ailment is hurried through it without time being

allowed for the proper action of the gastric and other secretions, a considerable part of it may be recovered from the fæces in almost unchanged condition. It is well known that the coats of seeds and the skins of fruits resist the gastric solvent; so that, if they have not been mechanically ruptured, their contents will pass out unchanged, the seeds not being in the least the worse as regards germinating power for having passed through the intestinal tube. It has been found that even starch-vesicles, if not ruptured by the masticating process, or by the heat employed (by cooking-animals) in preparing the food, resist the digestive process so completely, as not to give up their contents, being readily detectable by the microscope in the fæces. Further, there is no evidence whatever, that the undigested residue of the food *could* acquire the faecal character during the short period which suffices in the state of health for its transmission along the alimentary canal; and there is every reason to believe the contrary; since the substances which resist the action of the gastric solvent are precisely those which have least tendency to this kind of decomposition. Moreover, in purely carnivorous animals, and in man, when he adopts the same diet, the food is *completely* soluble, and there is consequently no indigestible residue; yet fæces are still voided, though in smaller quantity than in herbivora. The case is still stronger in regard to sucking animals. The continued evacuation of faecal matter, when little or no food is taken in, the large quantity brought off by purgative medicines after the bowels have been completely emptied of their solid contents, and the colliquative diarrhœa which so frequently occurs at the close of exhausting diseases, are so many obvious confirmations of the same view. To many of our readers it will doubtless be familiar; yet we are disposed to think that the idea of the intestinal glandulæ as performing a most important part in the depuration of the blood, by eliminating from it the *putrescent* results of the decomposition of the solids and fluids of the body, is not generally entertained with sufficient definiteness. It would be doing injustice to Dr. Williams, however, if we were not to state that he has elsewhere recognised the importance of some of the phenomena in question, and has put what we believe to be the true interpretation upon them. We find the following note in reference to the follicular enteritis of typhus in a later part of the volume:

“I have observed an extraordinary development and inflammation of the isolated and grouped glands of the intestines in the bodies of persons poisoned with arsenic. Their enlargement in epidemic cholera, and in the severe form of sporadic cholera, diarrhœa, and dysentery, caused by putrid effluvia, is well known. Are these glands excretory organs for the elimination of poisonous or noxious matters from the system? And in typhus fever, do they become inflamed and ulcerated by the continued operation of the poison in the exercise of this function? The favorable influence of moderate diarrhœa in fever, the uncommon fetor of the stools, the general relation between the duration of the fever and the affection of these follicles, the salutary operation of mild mercurial remedies which promote their secretion, and other facts that might be adduced, give so much countenance to this question as to make it worthy of attention.”

“The preceding surmise, put forth in the first edition of this work, has received corroboration from numerous facts which I have subsequently observed; and it seems to me to indicate the true cause of the intestinal complication in fevers and other diseases induced by a morbid poison in the system.” (p. 248.)

The doctrine that the proper *faecal* matter is not derived from the food,

but is an excretion from the blood, has been recently advanced as new by Professor Liebig (*Animal Chemistry*, 3d ed., pp. 143 et seq.); but, strangely enough, he found himself at a loss for a glandular apparatus which he could regard as the special instrument of the function. He has added, however, one new fact of much interest; which indicates that the substance to which the *fæces* owe their peculiar fætor is due to the imperfect oxidation of albuminous compounds.

"If," he says, "we heat in a retort one part of white of egg and three of hydrate of potass, so as to melt the mixture, and continue the heat till the disengagement of ammonia has nearly ceased, and if we then supersaturate slightly the contents of the retort, after cooling, with dilute sulphuric acid, and distil, we obtain, along with a disengagement of carbonic acid and sulphuretted hydrogen, a liquid which is slightly acid, from the presence of acetic and butyric acids, and which has the most horrible fæcal smell. The substance to which the smell belongs is soluble in water and alcohol; it combines with alkalies, without, however, neutralizing them. When exposed to the air, it is rapidly changed. By means of caseine, gelatine, and fibrine, when treated in the same way, we can procure all the different varieties of fæcal odour." (*Op. cit.*, p. 154.)

We look upon this question as one of great importance; since on the view we take of it much of our practice will depend. If it be true that the intestinal surface contains an extensive glandular apparatus, whose special function is the elimination of certain products of decomposition from the blood, the facility with which we can stimulate this to increased action, by certain kinds of purgative medicine, gives us a most valuable means of augmenting the depurative operation. We are no friends to the indiscriminate use of purgatives, which is the vice of many practitioners who pride themselves on their active treatment; but, on the other hand, we cannot regard them with the horror which some entertain. Seeing, as we think that no observant practitioner can avoid doing, how frequently Nature herself employs this means of eliminating morbid matter from the system,—as is shown by the immense relief often given by a spontaneous attack of diarrhœa,—we look upon this apparatus as one which, like the liver, the kidney, or the skin, may frequently with propriety be stimulated by medicines which have a special action upon it, and one through which many morbid matters may be eliminated more certainly and speedily than through any other channel. But we would on no account encourage the system of violent purgatives, which is the facile resource of the routine practitioner who wishes to gain credit for the vigour of his remedial measures, but who does not know what else to be doing; and which is the favorite prescription of the amateur doctor or doctress, who thinks that no medicine can be good for anything, if it does not produce some such ostensible result.

We must not dwell long on the next section, which treats of the "Diseases of the Constituents of the Blood;" but have pleasure in expressing our opinion that it is the most complete summary anywhere to be met with, of what is definitely known on the subject. Independently of the condensed but discriminating account given by Dr. Williams of the labours of others, numerous original observations are introduced, which show that he has not been a slothful labourer in the same field. Of these we may select the following as an example:

"In several cases of Bright's disease of the kidney, I have observed the blood-

discs jagged or crenate at their margins, and otherwise imperfect; and the same remark has been made by Simon of Berlin, and others, and by Andral in a case of chlorosis. In one fearfully rapid example of albuminuria, which proved fatal in six days, with effusion of pus in the joints the day before death, I found the colouring matter dissolved in the blood-liquor after death, and scarcely any red discs remaining. There were numerous pus-globules in the blood. A similar total destruction of the blood-discs was observed in University College Hospital, in the blood of a person who died of malignant scarlet fever with purpura. I have met with similar proofs of breaking-up of the red particles, but to a much smaller extent, in acute purpura connected with jaundice, and in cases of disturbed function of the liver without jaundice: is this due to the remarkable solvent power exercised by small proportions of bile on the red particles, noticed by Simon and others?" (p. 115.)

To the crenate margin of the blood-corpuscles our own observations do not lead us to attach much importance; as we have found it so constantly presented by the blood-corpuscles of certain individuals of our acquaintance, of whose healthy condition we could feel assured, that we cannot regard it as indicating any considerable departure from the normal condition of these bodies. But the other facts recorded by Dr. Williams are valuable contributions towards a knowledge of the pathological conditions of the blood; as are also the following observations, which tend to correct an erroneous notion which has of late gained currency in regard to the increased amount of fibrin in the blood of scrofulous subjects.

"It is a fact of great importance, that the quantity of fibrin in the blood, and the facility with which it may be effused, are by no means in proportion to its plasticity, or capacity to become organized; thus it is abundant in the blood, and freely effused in the inflammations of scrofulous and tuberculous subjects, although in such the products of these inflammations and of nutrition are commonly caco-plastic or aplastic. The fibrin of the blood or coagulable lymph in these cases is more opaque and less elastic than in healthy subjects, and under the microscope presents a predominance of granular matter and fat-globules, and less of the finely-defined fibres and regular nuclei, as if it were imperfectly elaborated, and resembled coagulated albumen rather than the more animalized form of protein. Even the more perfect forms of fibrin, if in a position in which their vitality is not sustained by becoming organized, tend to degenerate and become disintegrated into an opaque aplastic matter (Gulliver), and this eventually may undergo a further chemical change into fatty and calcareous matter, like other aplastic deposits. It is interesting to observe that in these cases also the red particles are deficient in number; and this suggests a probable cause of the imperfection of the plasma." (p. 131.)

We might dwell much longer on this part of the work, and might point to many important practical suggestions which arise from our improved and improving acquaintance with the pathology of the blood. If no other good result had accrued from the investigations of Andral and his coadjutors, than the knowledge we have gained of the real condition of blood in chlorosis, the worse than absurdity of bleeding, and the rationale of the remedial action of iron, in that disease, the labour bestowed upon them would have been far from unproductive.

Under the head of Changes in the Blood by Respiration, we find (p. 143, note) a very just comment on the purely mechanical view of this function taken by Professor Liebig; who seems to consider the increased extrication of carbonic acid during exercise or exposure to cold, as a necessary consequence of the greater amount of air inhaled, in one case by accelerated movements of the chest, in the other, by greater density of the cold air. We

quite agree with Dr. Williams in the belief, that the proportion of oxygen absorbed and of carbonic acid expired depends chiefly on the condition of the blood brought to the lungs; and that the respiratory movements are regulated by this. For the hypothesis of Professor Liebig is totally inadequate to account for the great disproportion between the amount of carbonic acid liberated by warm-blooded animals at a high and at a low temperature. Thus it appears, from the experiments of Letellier, that a turtle-dove exhaled 0.974 grammes of carbonic acid per hour in an atmosphere of 32° (Fahr.), whilst it only exhaled 0.336 grains per hour in an atmosphere between 86° and 106°. Thus the quantity of carbonic acid exhaled was very nearly *three times* as great at the lower temperature as at the higher; and if Professor Liebig's explanation were correct, the density of the air ought to be three times as great at 32° as at 106°, so as to cause three times as much oxygen to be introduced at every inspiration; or conversely, the bulk of air at 106° should be three times that of the same weight at 32°. But how stands the fact? Air expands 1.480th of its bulk for every degree of Fahrenheit; consequently the whole expansion between 32° and 106° would be 72.480ths or 3.20ths; an amount too small to have any considerable share in the result. And further, if, as Dr. Williams justly observes, the increased liberation of carbonic acid and evolution of heat, produced by exercise, were due merely to the acceleration of the respiratory movements, the increase could be produced at any time by a voluntary acceleration of these movements, which is far from being conformable to fact. Everything appears to us to indicate that the rate and extent of the respiratory movements are governed by the *besoin de respirer*, which itself depends upon the various changes taking place within the system; the adjustment being made altogether involuntarily and even unconsciously, and being one of that series of marvellous adaptations which so forcibly indicates an all-wise and all-powerful design.

The section on Changes in the Blood by excretion does not seem very distinct in its subject from that on diseased secretion already noticed; and as many of the facts which it includes had been previously stated under the former head, and as all might have been with equal propriety, we think that it would have been preferable if the two had been united under one designation, unless some more definite distinction could have been drawn between them. Little or nothing material is added in this section to what had been previously stated as to the results of imperfect elimination of biliary and urinary matters from the blood; and we have to notice the same absence of any allusion to the intestinal excretion; but the results of disorder of the perspiratory secretion are more fully dwelt on in the following passage, from which we perceive that Dr. Williams has formally adopted views that have for some time been acquiring a sort of floating credence as to the *materies morbi* of rheumatism.

“The perspiratory secretion contains lactic acid and lactates of soda and ammonia, which probably proceed from the transformation or decay of the textures, particularly the muscular, which the recent researches of Liebig have shown to contain a preponderance of this acid. Hence these products abound during great muscular exertion; and when perspiration is checked by external cold they may be retained in the blood, causing rheumatism, urinary disorders, or various cutaneous diseases. The very serious effects sometimes resulting from sudden cold on the

perspiring body may be partly owing to the same cause, as well as to the disorder produced in the circulation. Rheumatism is especially liable to occur as an effect of cold, where the body is fatigued with much muscular exertion; and I have frequently observed that the rheumatism chiefly affects the limbs which have been most exercised. Where the skin fails to excrete, an increased task is thrown on the kidneys, whence may result various diseases of these organs; and if these organs fail in the task, the lactic acid accumulates in the blood, and, probably acting as a ferment, causes the formation of more, and of the kindred products, lithic acid and its compounds and products: these, in inflammatory subjects, excite rheumatic fever: in cachectic persons, miliary fever, erysipelas, or pemphigus; and in more torpid frames, various local rheumatic or gouty affections. All these cases are frequently remarkable for the acid character of the cutaneous and renal excretions;* and in a few instances the blood has been found to possess acid qualities, or to be deficient in its usual alkaline reaction. In low forms of rheumatism, especially the neuralgic, the *materies morbi* is probably oxalic acid, as originally suggested by Dr. Prout; for I have, in numerous instances, found an abundance of the octohedral crystals of oxalate of lime in the urine, especially when the patients began to convalesce.

"The remedy for rheumatism and other diseases arising from defective excretion, therefore, should not be merely antiphlogistic, but also of a kind calculated to eliminate the morbid matter from the blood. In slight cases of rheumatism, sudorifics may suffice; but in others, the kidneys and liver should also be excited to assist in the process of elimination, and various combinations of colchicum and alkalies with mercury, opium, and iodide of potassium will generally effect this purpose very satisfactorily, and both speedily and permanently remove the disease.† Where the disease is more decidedly asthenic, and the urine exhibits a deposition of oxalate of lime with or instead of lithates; or acid phosphates, great advantage may be often derived from the use of means calculated to raise the tone and vital energies of the circulating and secreting organs, such as bark, quinine, arsenic, and iron; and they are the more eligible in cases of neuralgic rheumatism, because the attacks are periodic, with intervals of depression highly favorable to the use of these remedies." (pp. 151, 152.)

If Dr. Williams really does not deceive himself in regard to his success in the treatment of rheumatism, we are almost driven to conclude that the cases which fall under his notice must be of unusual mildness; since we have long known the very same treatment put in practice, with results by no means as favorable as regards *time*. We cannot feel by any means sure that, in imputing the entire mischief to an undue development of lactic acid in the system, we have by any means the whole truth; or that the severity of the symptoms bears any proportion to the amount of this substance abnormally present in the system. And we think that the matter is at present rather one for inquiry and investigation, than for dogmatic assertion. Dr. Williams does not mention the hot-air bath as one of his remedies; we have seen it of much service, especially in those cases in which the skin was unusually dry and the perspiration deficient.

Under the next head, "Changes in the blood from the transformation of chyle and of the textures," we find included gout and other lithic-acid diseases, diabetes, both saccharine and ureal, and obesity. All these

* "In patients with acute rheumatism, I have frequently found the perspiration of the affected joints more strongly acid than on other parts."

† "The advantages of this due regard to the essential elements of disease in the treatment of rheumatism may be shown by the fact, that, in upwards of two hundred cases of various forms of rheumatism, under my care in University College Hospital, the convalescence was established in from three to six days on an average, and the patients were dismissed cured in from one to three weeks after."

states are dependent upon disordered conditions of the processes of primary and secondary assimilation; and though apparently so dissimilar in themselves, they are very rightly brought together. It would not have been difficult, we think, to have referred certain disorders of the biliary secretion, or rather of the general system, to the same class of causes. Certainly the "bilious" habit of body, the bilious diarrhœa of summer and autumn, and other functional derangements of the same kind, are more frequently referable, in the first instance, to dietetic causes, than to disorders of the liver itself, which would discharge its duty properly if other organs did theirs. Dr. Williams adopts the fashionable creed that lithic acid is the *materies morbi* of gout, as lactic acid of rheumatism; but he does not afford any explanation of the fact, that the lithic acid and the gouty diatheses present points of difference which appear sufficient to indicate a want of absolute identity between them,—a fact which led one of our predecessors to suggest that it is the *lithate of soda*, rather than free lithic acid, which really constitutes the offending matter in the latter disease.

"This appears to us to be indicated by the fact, that the substance named is that which is separated from the blood in gouty deposits;* and still more by the known connexion of gout with biliary as well as with urinary derangements, and by the beneficial result of treatment directed to *both* these excretions. Under the influence of particular circumstances, *lithic acid* has a tendency to accumulate in the blood; and it seems to us quite possible that, so long as it retains its uncombined form, gout may not result. But if, by deficiency in the secretion of bile, *soda* also be allowed to accumulate, the two will combine, and lithate of soda will be formed. This hypothesis accords well with the fact that peculiar advantage, in the prophylactic treatment of gout, is derived from the occasional administration of blue pill or other mild mercurials." (Brit. and For. Med. Rev. vol. xvi, p. 468.)

This view is corroborated by the following facts, which we quote from Dr. Williams's appendix:

"Dr. Garrod has, in several other cases of gout, detected lithate of soda in very appreciable quantity in the blood; whilst at the commencement of a fit of gout there is a marked diminution of it in the urine. On the abatement of the attack, the lithic acid, or its compounds, appears in increased quantity in the urine, and that in the blood is therefore diminished. This exactly accords with the view that I have always taken of the nature of acute gout in common with other febrile diseases excited by a morbid matter in the blood, that the febrile excitement is the result of a reaction which may succeed in eliminating the offending matter, and in relieving the system from its influence. It appears further, from Dr. Garrod's analyses, that slight traces of lithic acid may be detected in the blood of persons who are comparatively healthy, or who are affected by other diseases than gout; but in this malady the amount is much greater. My chemical assistant, Mr. Edward Palmer (on whose accuracy I can fully depend), has separated crystals of lithic acid also from the blood of two of my patients suffering from degeneration of the kidneys with albuminuria. In acute rheumatism, Dr. Garrod found no increase of lithic acid in the blood; but I have little doubt, from its abundant appearance in the urine in many cases during the decline of this disease, that something analogous and easily convertible into it does exist in the blood of rheumatic patients, and that further researches will establish the chemical relation between two diseases which

* Dr. Williams states,—"A case of chronic gout, at present (June, 1847) under my care, has afforded an opportunity of verifying this observation: the matter obtained by puncturing the white tumours of his fingers is of the consistence of thick cream, and consists of very fine acicular crystals of lithate of soda, with a trace of lime."

are known to border so closely on each other, in regard to their symptoms and treatment." (p. 514.)

The following observation is interesting as showing how frequently, by a careful scrutiny, a satisfactory *rationale* may be discovered for symptoms, which we have been accustomed to consider as ultimate facts in the history of disease.

"I have in several instances found in the cortical and tubular structure of the kidney, clustered crystals of lithic acid, which under the microscope exhibited such sharp angles and dagger-shaped projections, as would afford an easy explanation of the pain, inflammation, and hemorrhage, often attendant on an attack of renal gravel, even when none is obvious in the urine." (p. 154, *note*.)

The last section of this chapter, on the "Changed properties of the blood from the presence of foreign matters," is rather disappointing from its meagreness; this being one of the departments of pathology in which the greatest advance has been made within the last few years, and in which the greatest progress may be expected. It must be admitted, however, that much of what we are accustomed to regard as *proven* under this head of the inquiry, rests upon indirect rather than direct evidence; since neither chemistry nor microscopy have yet been able to detect in the blood of patients labouring under *zymotic* diseases the morbid matter, to whose introduction the obvious depravation of the fluid and the disturbance of the whole series of functions are attributed. But the evidence of the existence of such morbid matter, derived from the comparison of the phenomena in question with those resulting from the agency of undoubted poisons, is so strong as to leave little room for hesitation with respect to its right to be considered as a *vera causa*. It is to be remembered that even in organic chemistry, we are continually in the habit of resting satisfied as to the presence of particular bodies, from the result of certain *tests* applied to the mixture or combination supposed to contain it, when circumstances do not admit of the ready isolation of the body itself. And there is one case—we refer to fluorine—in which chemists were long unable to isolate the body at all, owing to its strong disposition to combine with all other substances, and yet allowed themselves to feel no doubt whatever with regard to its real existence, since its reactions with other bodies were so decided and constant. It seems to us that we have a right, in many instances, to draw similar conclusions, in regard to the existence and agency of morbid matters, from observed phenomena; and that no unreasonable doubts ought to be raised on the ground of deficiency of purely chemical or visual proof of the presence of a foreign substance. For we think that the actions of the living body should be regarded as affording a series of *tests* of the purity of the blood, from which evidence as valid and satisfactory may be drawn (when we know how to appreciate it aright) as that on which we are in the habit of resting in ordinary chemical investigations. And on this account we think that the attention of pathologists ought to be specially directed towards the phenomena attending the action of poisons, which undoubtedly act by absorption into the blood, and to the parallelism of various morbid processes with these. Dr. Williams is by no means insensible to the importance of these considerations, as we judge from many passages scattered through his treatise; but we think it would have been better for the interests of science, as well as more consistent

with his general plan, had the inquiry been carried out more fully under the present head. As we purpose touching upon it, however, in another part of our present Number (Art. X), we shall not dwell further upon it here.

We have dwelt long upon this portion of Dr. Williams's 'Principles,' because it constitutes the distinctive feature of the work; the "Proximate elements of disease," which are next considered, as well as the "Diseases of nutrition," afterwards treated of, forming, in one shape or other, the great bulk of most treatises on general pathology. We shall not follow him through the succeeding chapters, therefore, as systematically and minutely as we have done through that which we have now passed under review; but shall content ourselves with a notice of such points as appear to us to be worthy of special attention, or to require comment or correction. We have already placed before our readers Dr. Williams's tabular arrangement of the proximate elements of disease, and have offered some observations thereon. With regard to the sections on anæmia and plethora we have only to remark, that they display the author's usual ability, and are admirable summaries of what is known of these pathological conditions and of the indications for treatment which they present. We must break a lance with him, however, on an old subject of debate, the discussion of which commences under the head of congestion,—namely, whether there is any cause, originating in the reactions between the blood and the surrounding tissues, which can modify the flow of blood through the capillaries, acting (in the normal condition) as an auxiliary to the heart's action, but capable, when perverted, of retarding or even checking the motion of the fluid. We are among those who have maintained, upon general physiological grounds, the affirmative of this proposition; since a comprehensive view of the phenomena of the circulation appears to us to afford ample evidence that the variation in the rate of movement of the blood through different parts, or through the same part under different circumstances, is mainly dependent upon the relative activity of the changes which take place between the blood and the tissues; and that there is a force generated in the capillary circulation, which, to say the least, exerts a *distributive* power over the blood, such as the heart's action is obviously unfitted to generate. We never felt, with Dr. Alison, the necessity for assuming the existence of a set of *vital* attractions and repulsions as the hypothetical cause of these phenomena; and were always disposed to believe that their explanation would be found in the *physical* conditions under which they occur,—a belief which has been fully confirmed by the researches of Professor Draper, elsewhere alluded to. (p. 45.) The phenomena of asphyxia have always been urged by Dr. Alison, Dr. John Reid, and other physiologists who have adopted corresponding views, as affording conclusive evidence that the cessation of the changes normally taking place in the blood during its passage through the capillaries produces a stagnation of its movement; the venous blood transmitted by the pulmonary artery to the lungs being prevented by the stoppage of the oxygenating process from passing through the pulmonary capillaries; and the non-oxygenated blood transmitted through the systemic arteries being retarded in its flow through their capillaries, because its want of oxygen prevents the occurrence of the reactions to which it ought to be there subservient. In his former

edition Dr. Williams stated that it remained for future observers to determine whether these obstructions are connected with contraction of the vessels, increased spissitude or cohesion of the blood, or other simply physical cause; or whether they depend on peculiar (vital) attractions and repulsions exerted between the vessel and its blood. He now states (p. 186) that the researches of Mr. Erichsen on asphyxia have satisfactorily settled the question in favour of the first of these alternatives; having shown that the presence of venous blood in the minute arteries excites a contraction of their walls, which is a sufficient obstacle to the flow of blood through them. We must own ourselves at a loss to perceive how these observations prove anything of the kind. The following is the essential part of Mr. Erichsen's account of the phenomena from which Dr. Williams draws this inference :

“On asphyxiating a young rabbit, a portion of whose mesentery had been conveniently fixed under a powerful microscope, the following phenomena will be observed to ensue. For about a minute after the struggles of the animal have ceased, the circulation appears to be going on with its usual rapidity; it then gradually becomes somewhat slower, the arteries contracting in size, containing less blood, and assuming a lighter and more tawny colour than before; whilst the veins become congested, and evidently fuller, assuming, when viewed by transmitted light, a very beautiful crimson hue. As the circulation becomes more languid, the arteries continue contracting, and acquire a lighter colour; the diminution in their size, and the difference in the quantity of blood contained in them and in the veins being most marked. The motion of the blood in the capillaries now becomes oscillatory, the whole mass of blood being at each impulse from the heart slowly propelled forward, and then moving backwards. This to and fro motion continues for some time, and then ceases entirely. On restoring the heart's action by setting up artificial respiration, an impulse was evidently transmitted from the blood in the arteries to that in the capillaries, in a pulsatory and jerking manner, which was soon communicated to the veins, driving forward the whole mass of globules accumulated in them, and gradually becoming more equable and powerful until the circulation was completely restored. . . . The diminution in the diameter of the smaller arteries, and the proportionate difference between them and the neighbouring veins, was most evident, and was such as could leave no doubt in my mind as to the important part that the contraction of these vessels plays in giving rise to an obstruction to the passage of blood through them in asphyxia; in which I have no doubt that it is the principal, if not the sole, agent.”

“These observations,” continues Dr. Williams, “correspond perfectly with what I have many times observed in the frog's web; and we may fairly infer that the same obstruction which operates in the systemic circulation will suffice also in the pulmonary. The notion of vital attractions of blood for the vessels, or even of the aggregation of the red corpuscles (suggested by an imperfect observation of Mr. Wharton Jones) as causing the obstructed circulation of asphyxia, is not only superfluous but untenable; for, were the obstruction in the capillaries and veins, the arteries, for a time at least, should exhibit distension, which the above description proves not to be the case.” (pp. 187-8.)

Now let us inquire how far the facts of this case warrant the deductions of Mr. Erichsen and Dr. Williams. If contraction in the arteries were really the cause of the obstructed capillary circulation, the first effect ought to be that the stream would be accelerated; just as a force-pump sends a more rapid stream through a small jet than through a large one, and as, in the ordinary circulation, the blood moves through the arteries more quickly than through the veins in proportion to their difference of calibre. But no such acceleration takes place; on the contrary, the first

change in the rate is one of retardation ; and this retardation continues to increase with the diminution of the supply transmitted from the heart. This supply becomes smaller and smaller, as less and less blood is returned from the lungs to the heart ; and at last ceases altogether. What else is to be expected from the known properties of the arterial walls, than that the arterial tubes should gradually contract as they become emptied, just as they do after death ? The observations in question thus become, to our minds, clearly indicative of the incorrectness of Mr. Erichsen's inference ; and the objection raised by Dr. Williams to our view of the case, turns out, when properly examined, to afford an additional argument in its favour. If obstruction to the flow of the blood through the systemic capillaries be, as we maintain, the first change which the state of asphyxia produces in the systemic circulation, there will be for a time increased *pressure* within the walls of the arteries ; but there will not be *distension*, unless the walls yield to that pressure. The negative evidence of Mr. Erichsen, as to the absence of arterial distension, by no means proves that there is not an increase of pressure ; whilst the evidence of Dr. Reid, obtained by careful measurement with the hæmadynamometer, is positive proof to the affirmative. For he found that, in the first stages of asphyxia, there *was* increased pressure within the systemic arteries ; indicative of an obstruction to the flow of blood through the capillaries, whilst the supply from the heart was but little if at all diminished. Very soon, however, the quantity returned from the lungs became rapidly smaller and smaller, so that less and less was propelled by the heart through the aorta ; and the pressure within the arteries, as indicated by the hæmadynamometer, diminished in like proportion,—exhibiting an immediate increase whenever air was admitted to the lungs. As Dr. Williams is evidently not unacquainted with these experiments of Dr. Reid's, having referred to them in a preceding page, we are at a loss to understand how he could overlook their bearing upon this question.

The same fundamental error, as it appears to us, pervades Dr. Williams's account of the state of “active hyperæmia” or “determination of blood ;” and we are particularly struck by the inconsistency between his ideas of the nature of this state, and those which he has put forth as to the causes of passive congestion with diminished movement of the blood. The latter state he considers, and we believe very justly, to be frequently attributable to diminution of tone on the part of the walls of the veins, causing them to yield to distension instead of resisting it, and thereby producing a mechanical impediment to the onward movement of the blood ; and he has devised an ingenious experiment which affords proof that such a state will really act in the manner supposed (p. 188). But he attributes the determination of blood to a part whose functional activity is exalted, as a result of a corresponding deficiency of tone in the *arterial* walls, producing an enlargement of their tubes, and a consequent increase in the amount of blood passing through them ; and he thus endeavours to explain away the inconsistency of viewing the two opposite conditions as results of a common cause :

“It has been objected that I assume enlargement of vessels to be the cause both of increased motion (in determination) and diminished motion (in congestion), which seems contradictory ; but if the objector had duly considered my explanation, he would have found no contradiction in it. In determination, the vessels enlarged

are the arteries, which being near to the source of motion, and highly charged with its propulsive power, give vent to the current as from a reservoir of high pressure; whereas in a tonic congestion, the vessels enlarged are the veins and capillaries, which are remote from the source of motion, and receive their impulse only through the arteries, which are not enlarged, or are even contracted; and thus the accumulated blood becomes comparatively stagnant. There is nothing contradictory in this simple application of hydraulic principles." (p. 205.)

Dr. Williams seems to have forgotten, however, one very important element in the comparison: namely, the *rate* at which the blood moves onwards. We are very ready to admit that, on simple hydraulic principles, more blood will flow, in a given time, through a part whose arteries are large, than through one whose arteries are of smaller calibre; but on the very same principles the *rate* of movement will diminish with the increase of the calibre of the tube; and this the more as the pressure is greater. At any rate the increase of the calibre of the tube cannot, under any circumstances, produce an increase in the rate of movement of the contained fluid, such as, on Dr. Williams's own showing, exists in active determination of blood to a particular organ. We must, therefore, look for some other cause of the increase; and we do not see where it is to be found, save in the general principle to which we have already drawn the attention of our readers.

Our want of agreement with our author on the subject of the capillary circulation, occasions a fundamental difference between our respective interpretations of the phenomena of inflammation. It is not our intention, however, to enter at the present time upon a discussion of this much vexed question; and we shall confine ourselves to a brief indication of the points of divergence, with the view of guiding our readers in their own investigations. This we can best do by quoting Dr. Williams's summary of the "local elements of inflammation."

"Determination of blood towards the affected part.

"Obstruction of the vessels most affected; by a tonic enlargement of the capillaries; by production and adhesion of white corpuscles in the vessels.

"Distension of arteries and capillaries *before* the obstruction, causing increased effusion of serum, lymph, pus, &c.

"Emptiness of veins *beyond* the obstruction, causing increased absorption; hence softening, &c.

"Impeded or arrested circulation *at* the obstruction, causing a reduction or abolition of vital properties; hence the death of the part, and its removal by ulceration and suppuration, or its decomposition by gangrene.

"Increased circulation of blood *around* the obstruction, causing exaltation of vital properties; hence spasm, pain, sympathetic irritations, increased secretion, &c." (p. 321.)

There is no considerable difference of opinion between Dr. Williams and ourselves in regard to the ostensible phenomena of the inflammatory process. The retardation or total stagnation of the circulation *in* the inflamed part, and the acceleration of the flow in the *surrounding* vessels, are admitted by all observers. The diminished vitality of the solids of the inflamed part, as indicated by the suspension of their normal actions of nutrition, secretion, &c., or their absolute death and disintegration, is a point on which we fully agree with him. And we further accord with him in considering the increased production of white corpuscles in the capillaries of the inflamed part, communicating to the blood circulating

through it (and thereby to the entire current) an increase of plasticity, as one of the essential phenomena of the process. But we are at issue with him in regard to the interpretation of these phenomena; and especially with respect to their relation of mutual dependence. Dr. Williams considers that the disordered condition of the vital functions of the part is due to the retardation of the flow of blood through it; and that the retardation is dependent upon an altered state of the walls of the blood-vessels. It is in these, therefore, that he looks for the essential or fundamental phenomenon (or proximate cause) of the inflammatory process. In our apprehension, on the contrary, a disturbance of the ordinary relation between the blood and the tissues, usually depending upon a state of depressed vitality in the latter, is the origin of the mischief; a disturbance of the capillary circulation will ensue as a necessary consequence; and this disturbance tends to increase the mischief. In the mean time, a counteracting or conservative change takes place in the blood; the increase in the plasticity of which is destined to repair the mischief occasioned by the disintegration of the solid tissues. We thus should designate inflammation as a local perversion of the ordinary nutritive process; the circulation being affected only as it is influenced by the reactions taking place in the capillaries: whilst Dr. Williams would consider it as a local alteration of the capillary circulation, which induces consequential changes in the nutritive functions to which that circulation ministers. Those of our readers who may wish to see what is to be advanced on either side, should carefully study this chapter of Dr. Williams's treatise, and then peruse the review of the corresponding part of the former edition in the 'British and Foreign Medical Review,' vol. xviii, p. 103 *et seq.* We should be doing injustice to our author, were we to allow our difference of opinion with regard to the theory of inflammation to prevent us from recommending this portion of his treatise, as containing a masterly summary of the causes, symptoms, and results of the process, with an admirably condensed view of the principles to be followed out in its treatment.

In the fourth chapter we enter upon the "Structural diseases or diseases of nutrition;" which, it is admitted by Dr. Williams, might be regarded, like those of secretion, as belonging to the primary elements of disease; but which he deems it better to consider under a separate head, "both because the previous consideration of disorders of the blood and its vessels gives the best introduction to them, and because we cannot strictly distinguish structural disease into ultimate and proximate elements. By analogy, indeed," he continues, "we might infer that ultimate structural disease is that which affects elementary structures singly, such as muscular fibre, nervous matter, cellular texture, &c.; but we find structural disease to be rarely thus confined to one anatomical element, but rather to affect structures as they exist combined in more or less complexity." (p. 353.) We regarded this chapter, in the former edition of the work, as constituting one of its most distinctive and meritorious features; and we consider that Dr. Williams is fully entitled to speak in the following terms of the essential truthfulness of the views which he there put forward.

"Although considerable advances have been made in pathological anatomy, and several new works devoted to the subject have appeared since the publication of the first edition of this treatise in 1843, it is satisfactory to me to find that in no

material points has the advance of knowledge superseded the views there given of the elements of structural disease, but in many instances the views have been signally confirmed and extended. Under these circumstances, I have added to, rather than modified the text; and in preference to new classifications and uncouth nomenclature, which it has been attempted to introduce from various foreign writers, I have retained my former division of the elements of structural disease, designated by terms in common use among British pathologists." (p. 354, *note*.)

In his classification of the elements of structural disease, Dr. Williams includes under that term the morbid changes, "which take place independently of distinct inflammation, and which are mere modifications of the process of nutrition or reparation, which is continually going on in the textures of the living body." (p. 355.) In isolating these changes from the alterations of structure produced by inflammatory action, Dr. Williams is evidently influenced by his ideas with regard to the essential nature of the latter: for this, primarily consisting (as he thinks) in a disorder of the circulation, cannot be associated with processes whose essential character is a perversion of nutrition, such as no alteration in the simple rate of the blood's movement, or in the contractile state of the walls of the vessels, could of itself produce. But we cannot but think that it would be much more accordant with the mutual relations of these phenomena, as seen in the progress of disease, if they were all to be considered under one category; for it seems to us impossible to draw a distinct line between several of the elementary changes included in the foregoing group, and those which are always ranked, and which have been already described by Dr. Williams, under the head of inflammatory products. This indeed is virtually admitted by Dr. Williams himself; for when treating in detail of "Induration and softening, Deposits," &c., he refers to the fact that these have been already considered in part under the head of inflammation; and he only ranks them as a distinct group because they occur sometimes independently of inflammation;—although he remarks, in regard to the former processes, that "they probably, in most cases, depend on some of its elements;" and with respect to the latter, that they "result from an overflow of the nutritive material beyond what is necessary to nourish the textures themselves," which must be accounted as one of the conditions of inflammatory deposits. We cannot but believe that further consideration of the subject will lead him to see, that *all* structural diseases depending upon a perversion of the nutritive process must be arranged in one class, whether or not inflammation participate in the production of them. Thus in regard to the deposition of tubercular matter, we think it can scarcely now be doubted that it may take the place either of the ordinary material of textural nutrition, or of a fibrinous inflammatory effusion; and that whilst its production may be quite independent of inflammation, it is often the immediate effect of that process, and may be considered as one of its results. Our limits do not allow of our taking any lengthened notice of this excellent chapter; and we shall content ourselves with an extract relating to the gradual contraction of cacoplastic deposits, which frequently leads to the most serious consequences, without any ostensible indication for treatment.

"The microscopic examination of cacoplastic deposits at different periods gives much explanation of these contractile changes. When recent, it consists of fibres with a great amount of granular matter with and without cells, and more or less

amorphous material. When examined older, and after having contracted, it is much more tough, and is found to be more distinctly fibrous, much of the granular and amorphous matter having disappeared. Old contractile adhesions and cicatrices are still more dense, and are found to consist almost entirely of closely interwoven fibres, differing from those of fibro-cartilaginous tissue only in being less regular and distinct. It appears therefore that it is by the partial absorption of the granular amorphous portion of the deposit, and by the closer approximation of the fibres or more organized constituent, that the condensation takes place. Hence it is that the structure so contracted is less liable to the further degeneration to which cacoplastic deposits commonly tend. Certain it is, that of different portions of cacoplastic deposit in the lungs and elsewhere, those that are contracted remain unchanged, whilst others pass into the aplastic state of opaque and softened tubercle. This contractile process seems, therefore, to raise the deposit to a higher standard, in which, although below them, it is tolerated by the adjoining textures. But this very change may seriously injure the textures of organs, by contracting and compressing their vessels, and interfering with their nutrition and other functions; and in this respect it may be classed with the highest or most animalized variety of degeneration, the fibrous which has already been described. This is the chief mode in which the contractile diseases of the liver and kidneys gradually infringe on the circulation and secretion of these organs, and thus may eventually prove fatal. The contraction which takes place near the summits and roots of the lungs, in the more limited and therefore chronic forms of pulmonary tuberculous disease, often lays the foundation of emphysema of the lungs and habitual asthma. Similar contraction occurring in the deposit under mucous membranes of the alimentary and urinary passages constitute strictures, which occasion much distress and serious disorder." (pp. 389-90.)

Under the head of "Treatment of cacoplastic and aplastic deposits," Dr. Williams has introduced a statement of his opinion with regard to the remedial value of cod-liver oil; which is highly favorable. He considers that fixed oils in general have a solvent power on the fat of tubercle, which may enable them to remove the deposit, if carried into the circulation in sufficient amount; and that the superiority of fish-oils especially depends upon their being more permanently fluid, and less liable to become rancid. He lays great stress on the purity of the oil, as allowing a sufficient quantity to be ingested; and altogether dissents from the opinion of those, who think that the less pure forms of it are more efficacious. Where it can be borne in sufficient doses,—

"It increases rather than impairs the appetite; and, if continued for some weeks or even months, promotes in a marked degree the function of nutrition, increasing the strength as well as the flesh, and giving increment to all the textures. Nor is this surprising when we consider that the nuclei or rudimental molecules of all structures appear to consist of fat, which the oil in its highly divisible state supplies and renews in the manner most conducive to active and healthy nutrition. Its peculiar fluidity and little proneness to change also enable it to pervade all structures, and to penetrate even into imperfectly organized deposits, and by softening their concrete fatty molecules, and rendering more permeable and supple their whole mass, brings them more under the influence of the adjoining living parts, through the circulation in which either their vitality and nutrition are improved and maintained, or if incapable of improvement they are gradually dissolved and absorbed away.

"Such appears to me the mode of operation of the cod-liver oil, assuredly the most efficacious of all medicinal agents in the treatment of cacoplastic and aplastic deposits, and one which, after two years' constant experience in its use, is still frequently surprising me by the wonders that it occasionally works even in aggravated

and advanced cases of scrofula, mesenteric disease, pulmonary consumption, chronic pneumonia and pleurisy, and chronic rheumatism.

"But no remedy, however beneficial, should lead us to neglect attention to those general and hygienic measures by which the constitutional health is promoted and sustained; or, in other words, the great functions of circulation, respiration, digestion, absorption, and excretion are promoted." (p. 405.)

Dr. Williams has acted very judiciously, we think, in not devoting any large portion of his treatise to mere Nosology. That some kind of classification is needful, is obvious enough. That a *natural* classification, in which diseases, or collections of symptoms should be arranged according to the morbid states on which they are dependent, is the one most to be desiderated, will be admitted, we think, by every one who has any definite idea of the sciential part of medicine. But that we are still in the dark with regard to the essential nature or proximate cause of a large proportion of the morbid phenomena which we witness, is a truth which there is no gainsaying; and to attempt to establish a natural classification of diseases in the present state of pathological science, would be just as absurd as to propound a natural classification of a set of plants or animals, of whose internal structure we are entirely ignorant. The great use of a natural arrangement is the large amount of information which we gain respecting any object, when once we are enabled to refer it to its proper place; thus when by the study of its characters we have determined the natural family of a plant or animal, we thereby come to know much of its general structure, habits, properties, &c.; and in like manner, when the observation of symptoms or characters shall enable us to refer a disease to its place in the natural arrangement, we shall be immediately put in possession of the general laws of its progress and consequences, and of the principles which should guide our treatment. This is the case already to some extent with regard to certain well-marked groups, such as that now designated by the term zymotic diseases, in which, as is generally admitted, the disorder of the system is due to the introduction of a poison into the blood; the phlegmasiæ, in which a local inflammation is the essential change; and those which depend upon particular diatheses, such as the tubercular, the gouty, &c. The study of these truly natural groups is laying a good foundation for a really scientific nosology. But in the mean time, if we adopt any system at all, it must be one of a more or less artificial character; and we are inclined to think that Dr. Williams is correct in saying that a classification primarily founded on *locality*, although less pure and methodical than some others, is the most useful in practice.

The chapter on "Semeiology and Diagnosis" might, we think, have been extended with advantage. These are topics on which the student and young practitioner peculiarly need instruction, and which, not being systematically treated in any works save those on general pathology, should not be dismissed too summarily in a treatise like the present. It is always to be remembered that the mental operation by which medical knowledge is acquired from books or lectures, is entirely the reverse, in point of order, of that by which medical practice is to be guided. In the former case the student has before his mind a systematic array of diseases, and endeavours to store his memory with the series of symptoms which are described as respectively characteristic of each. In the latter, the symptoms are first presented to him; and it is his primary object to deter-

mine from them the nature of the disease. How far the most complete systematic knowledge is from sufficing for this purpose, and how much the sagacity derived from long observation of disease is superior to the most comprehensive erudition without the power of applying it to the case in point, must be obvious to every practitioner. Indeed it is the universal perception of this fact, which has given rise to the low estimate commonly entertained of the scientific character of medicine; for it is tacitly believed, if not openly avowed, by a large part of the profession, that although it is all very right for young men to read and study and attend lectures, yet that they obtain thereby little beyond a general preparation for learning that which alone can be practically useful to them. Now we go a great deal further than this; and are fully prepared to maintain that if a student have fully mastered the contents of such a work as that now before us, and have learned *pari passu* to apply this knowledge at the bedside, he will be far better qualified for the treatment of disease, than any one can be, who has devoted the same amount of time to clinical observation only. But it is only by knowing how to apply his knowledge, that the studies of the former can be of any avail to him; and for imparting this power, we deem a sound system of clinical instruction to be the most effectual method. This system should have for its especial object to conduct the student to the right interpretation of the phenomena presented to him, and to bring them into connexion with the scientific principles in which he has been instructed; in other words, to teach him the art of medicine on the basis of the sciences of pathology and therapeutics. Practical instruction in semeiology and diagnosis forms one very important part of this process; and we should have been glad, therefore, to find a more ample exposition of their principles in such a work as the present. The semeiological states of the pulse, the skin, the tongue, and of the alvine and urinary excretions, are all discussed within the space of two pages; and a large number of important indications are not noticed at all. "How to observe," and "what to observe," should be the mottoes of such a chapter; and a physician of Dr. Williams's large experience in practice and in teaching may do immense service to the rising generation of students by working upon these hints. One of the most essential questions of diagnosis, which seems to us to be progressively increasing in importance, either from a gradual change in the type of disease, or from an increase of discrimination on the part of the profession, or from both causes combined, arises from the very frequent complication, especially in the female sex, of real disorders of structure or function, by pain and other symptoms of great violence, which are immediately referable to the nervous system. We have known the time when all such symptoms were set down as inflammatory, and were met by bleeding, blistering, and the whole armamentum of antiphlogistic remedies. And we have often seen them treated, on the other hand, as if they were entirely "hysterical," with little better success. We believe that, in the class of cases to which we refer, and which the increased and increasing prevalence of an asthenic constitution seems to have rendered of more frequent occurrence than formerly among the male sex, the only true way is to put aside the nervous symptoms altogether in forming a diagnosis, and to endeavour to determine what kind and amount of disorder of structure or function may be diagnosed from other indications.

To the young practitioner such cases are often extremely perplexing; and the more so because he usually sees less of them in hospital practice, than he will do when his attention is directed towards the middling and higher classes.

We shall not dwell upon the portion of the next chapter which relates to Prognosis; and shall limit our remarks upon Dr. Williams's method of treating the *Modes of death* to a few particulars. We do not find any such modification in this edition, as sets it free from the objection raised against the arrangement adopted in the former one,—namely, that death from Coma and death from Paralysis are both to be attributed to Asphyxia; that state being engendered by want of power in the nervous centres to keep up the respiratory movements. Nor do we find any allusion to the proofs afforded by the experiments of Chossât,—that, in a great variety of exhausting diseases, death takes place from the *cooling* of the body as a consequence of the deficiency of combustible materials. We consider this to be a point of great practical importance; since it affords an obvious indication for treatment. Where there is reason to believe that there is no lesion inconsistent with the recovery of health, and where the prolongation of life for a certain time (until, for example, the poison of fever shall have been thrown off) is the great object, we can understand the benefit derivable from alcoholic liquors, which act not so much as stimulants, as by supplying material for the combusive process; and as an aid to these or as a substitute for them, we believe that artificial heat may be applied with great advantage. Dr. Williams has the merit of having formally directed attention to a cause of death, which had not been previously sufficiently distinguished from others; namely, *Necræmia*, or death beginning in the blood, such as is seen in its extreme form in the operation of the poison of serpents, or of the most malignant cholera or typhus poison, the direct influence of which, when introduced into the blood, is to produce a speedy alteration in its vital and physical properties, and thence to act through the whole system, producing a state of adynamia or prostration of all the powers alike, which soon ends in death.

“But if the noxious influence is in smaller quantity or more diluted, the vital powers react against it in various ways, the object of which can often be plainly discerned to be its expulsion from the system. The shivering, hot stage, and sweating termination, of paroxysms of intermittent fever; the similar but less marked series of febrile movements which occur in slight forms of remittent and continued fevers; the profuse and violent fluxes from the stomach and intestines in cholera, dysentery, and epidemic diarrhœa, and the similar discharges induced by poisonous ingesta, are instances of the operation of the vital reaction attempting the expulsion of the noxious matter and of that part of the animal fluids that had been corrupted by it. But these struggles in many instances constitute serious diseases, in which life may be compromised by the violent and exhausting effect of the reaction, as much as by the prostrating influence of the cause of the disease: in these more complex affections, individual organs may especially suffer in different cases, and the danger and the cause of death may be less in the changed condition of the blood, than in the affections of particular organs, or the exhaustion consequent upon them, which destroy, not by necræmia, but by coma, asphyxia, or asthenia, modes of death already considered.

“The injurious effect of these poisons may be still more completely prevented when their quantity is small and the living powers are vigorous. A diarrhœa, a profuse sweat, or a free flow of urine, sometimes carries off the commencing disease.

The intestines, the skin, and the kidneys, appear to be the proper emunctories, through which the morbid matter is expelled. The peculiar factor of the secretions from the bowels in typhoid fever, the beneficial influence of moderate diarrhœa, which removes them in the early stage of fever, and appearance of a foul fibrinous matter (typhus-material of German writers) in the intestinal glands, seem to be examples of the elimination of a morbid matter; and I have before suggested that the follicular inflammation, ulceration, and sloughing of the intestines in fever may arise from the excessive irritation of the follicles in the exercise of this eliminating function. Again, with regard to the kidneys, it has been before mentioned that granular degeneration, which impairs their function and power of elimination, renders the body peculiarly liable to contract epidemic and infectious diseases, and to succumb under them. This renders the prognosis unusually unfavorable in these cases. The same remark extends, and for the same reasons, to persons who have been habitually intemperate. On the other hand, those whose kidneys are naturally active, more effectually resist disease, and more readily throw off its effects. In the like manner, it is well known that persons with a naturally dry skin do not so readily get rid of a fever as those in whom perspiration is readily excited." (p. 467.)

These views we regard as of great importance. Like others already quoted, they show how much may be done, by a sagacious collocation of phenomena, towards establishing the *rationale* of their mutual dependence; and how beneficially that knowledge must act in practice, if only in giving warning as to what ought *not* to be done, it is surely unnecessary for us to point out.

To the present edition has been added a chapter on Prophylaxis and Hygienics; into which Dr. Williams has managed to compress as much sound and concise information on these topics, as a limited space would allow. As we do not find in it, however, anything specially worthy of comment or criticism, we shall not further occupy the attention of our readers; but shall content ourselves with again recommending this treatise as the best exposition in our language, or we believe in any language, of *Rational Medicine*, in its present improved and rapidly improving state. We remember hearing Professor Retzius remark, during his late visit to this country, that he considered the best English medical writers to stand pre-eminent in the art of teaching; combining simplicity and definiteness of expression, and condensation of material, with sound philosophical habits of thought; and being free from the grandiloquent wordiness of the French school, and from the vague mystical abstractions and the overloading of details which characterise the German. And we do not know that we could anywhere point to a specimen more deserving of this commendation, than the treatise before us.

We must say a few words in conclusion respecting the other work before us,—the General Pathology of Professor Chomel; which is, as we need scarcely observe, one that well deserves the attention of the student of Pathology, although by no means so well adapted to give a general view of the present aspect of the science, as is the treatise on which we have been so fully dwelling. Its especial excellence consists in the fulness and clearness of its chapters on Semeiology and Diagnosis, the subjects which we regard as least perfectly treated by Dr. Williams. And the well known practical skill of its distinguished author in the detection of obscure diseases, gives to these chapters a far higher value than they could have

possessed as the productions of a mere theorist, however ingenious. Hence they may be advantageously perused as a supplement to Dr. Williams's 'Principles,' and as an introduction to clinical study. It is to be regretted that the work should have fallen in many parts so much behind the present state of science. The last French edition, from which this translation is made, bears the date of 1840; and those who can look back to the uncertain state of opinion prevalent at that period on several of the most important questions of pathology which may now be regarded as satisfactorily settled, will know that considerable modification would be necessary to place it *au courant* with the present state of the science. This the translators have not attempted; and we consider that they have acted wisely in their forbearance. Their translation reads well, and appears to have been carefully executed. A few notes have been appended: but we think that these might have been almost as well omitted, as they cannot be said to do much towards giving consistency and completeness to the work.

ART. VIII.

An Account of the Cultivation and Manufacture of Tea in China; derived from Personal Observation during an Official Residence in that Country, from 1804 to 1826, &c. By SAMUEL BALL, Esq., late Inspector of Teas to the Hon. United East India Company in China. With Three Plates, and numerous Wood-Engravings.—London, 1848. 8vo, pp. 382.

OUR readers may at first, perhaps, be surprised at our reviewing a work written expressly on the Cultivation and Manufacture of Tea in China; notwithstanding that it forms an article of diet of a large proportion of the British population, and is esteemed as a medicine in many parts of the Continent, as well as in other parts of the world. But there are many points connected with the manufacture of tea, in which all are interested; and there is no question of the kind which is more frequently asked of medical men, than the cause or causes of the difference in colour and properties between black and green tea. All are familiar with some of the statements made on this subject, as for instance, that the green colour was due to the tea having been manufactured in copper vessels; also, that the colour was due entirely to the employment of colouring ingredients, such as turmeric, indigo, prussian blue, with gypsum, kaolin, &c. Mr. Warington has shown that what is called the fine bloom of many green teas is ascribable to the employment of such materials; though it does not follow that the colour of the best green tea is entirely due to this cause. The most reasonable explanation seemed to be that, which ascribed the difference to green and black teas having been produced by two different plants. This explanation also, has been proved to be incorrect, first by Mr. Fortune, and now by the author of the present work.

It must not, however, be inferred from this statement, that the real facts of the case had never previously been made known; but in this, as in many other cases, truth had been so mixed up with fable, or rather the accounts of the manufacture of the adulterated had been so confused with those of the genuine article, that it became extremely difficult to