through which the poisonous gases discharged themselves, their current through the chamber, had the chimney been open, would have been more rapid; but there was space enough between the chimney-piece and chimney-board, and through the imperfectly-fitting windows, for the passage of air; and probably the less impure air of the room passed up the chimney to give place to the poisonous products of the cinder heap.

To trace the communication between the ash-pit and the house, the loose stones, with their covering of earth, were removed to a considerable depth. Mr Haywood found the warm air from the burning cinder heap, passing through and below the broken foundation; and finding also that the oxide of iron on the stones was converted into a sulphuret, he concluded that the hydrosulphate of ammonia had, at the commencement of the burning, been driven in that direction, although he could not detect the presence of any of this substance, or any trace of its action on the metals in the house.

There was not the slightest indication of any vegetable or mineral poison in the parts which were analysed.

From the foregoing facts Mr Haywood concluded, that George Hall and his wife died from inhaling carbonic acid, and other products of the combustion of the burning cinder heap above described.

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Part Second.

REVIEWS.

Traté de Chimie, Anatomique et Physiologique. Par MM. Robin et Verdeil. Paris: 1853. 3 Tom. avec Atlas des Planches.

The science of life, or biology, like that of physics, is obviously divisible into two great sections, the limits of which it is always important to keep in mind, although they are intimately connected with, and mutually dependent on, each other. The former of these comprises the study of living beings in their inactive condition (vital statics), while the latter treats of the functions which organisms perform, or of their active or dynamical condition (vital dynamics). The first is considered, in general terms, to be equivalent to the science of anatomy, while the second corresponds to that of physiology.

The essential idea of organisation is that of the mutual depend-
ence, each upon each, of the various parts in relation to one another and to the whole. Hence it is impossible to form an adequate conception of an organised being by studying it in the statical condition only; for in doing so we, by the very act, exclude this fundamental idea from consideration, and look upon each part or organ as independent of, and out of relation with, the rest, and only connected with them on account of certain characters which they possess in common, and which constitute them organic. Although, however, these two aspects of the science of biology do not correspond to actual divisions which are capable of being comprehended, even in the slightest degree, independently of each other, the mode which we have alluded to of viewing the subject is useful as much facilitating its study. By so doing, we commence our investigation by making ourselves acquainted with elementary parts, confining ourselves to their intrinsic characters, and, by means of the combination of these, forming an exact notion of the general characters of the whole organism, which they themselves combine to form. This process constitutes the study of anatomy. It is capable of being continued in a similar manner into the limits of physiology. Arrived at this point of our progress, we investigate, first, the functions which elements perform taken separately; and secondly, their functions when taken in relation to each other and to the whole organism,—this last constituting the study of the special endowments of the tissues and humours, and of organs and systems of organs.

It being admitted that the outline which we have given above is in correspondence with the most natural method of viewing the science of biology, it is evident that the question which presents itself on the threshold of our acquaintance with vital statics must necessarily be that of the determination of the meaning to be attached to the term element. If a distinct definition in reference to this question be wanting, it is clear that further progress is impossible.

In the first place, an element in biology is obviously a body which either does live actually, or has lived at some former period, inasmuch as it forms an integrant of an organism, of every atom of which this may be asserted.

By observation it is found, that the two characters of organic beings which are most universally distributed throughout their substance are those of the possession, on the one hand, of a peculiar form, which we call organic, and, on the other, a peculiar chemical constitution. It is by keeping in mind these two characters, both of which are essential to the idea of organisation, that we arrive at an acquaintance with the two corresponding orders of biological elements. The first order consists of formal or anatomical elements, these being the last bodies, in respect of simplicity of form, to be arrived at by anatomical means, and which, at the same time, possess morphological characters which are not to be found in
nature, otherwise than in organised beings. The second order consists of what are called the immediate principles, being the last bodies to which we can arrive, without the employment of means capable of producing chemical decomposition, and the mode of existence of which is without analogue in the mineral kingdom, inasmuch as they form integrant parts of living structures.

In correspondence with this method of viewing the relation of the science of biology to the immediate principles, it must be admitted that the study of these last, both as respects their chemical characters and much more directly as respects the part they play in vital phenomena, belongs exclusively to the biologist. There exists no more immediate connection between the study of the "immediate principles" and the science of chemistry than between physics and the study of elementary organic forms, although no one will deny for a moment that the knowledge of chemistry is as essential to the biologist as that of physics. Of this doctrine, which has been alike overlooked, much to the prejudice of science, by chemists and physiologists, it is the object of the work before us to assert the importance. The title of the book is objectionable, as it fails to indicate with precision either its origin or its object. It is not a treatise on chemistry, it is a treatise on general anatomy and physiology. The "immediate principles" form the entire subject of the work, being described in the conditions under which they actually exist in the organism, and in relation to the functions which they actually perform. We should have much preferred that the work should have been entitled, according to the suggestion of a talented writer in the "Union Médicale," a treatise "On the Physical, Chemical, and Dynamical Properties of the Normal and Morbid Immediate Principles which constitute the Human Body and that of the Mammalia." For, in point of fact, as we shall find in pursuing our examination, it is one of the avowed objects of M. Verdeil to show that the subjects treated of are not included in the science of chemistry. He has especially endeavoured to avoid falling into the errors of that so-called "vital chemistry" which is now so prevalent,—a surreptitious science, which would endeavour by algebraic formula to represent the mode in which the most important functions of the economy are accomplished, and to submit to calculation the most inscrutable and intimate of the changes which take place in living beings.

In commencing the sketch we propose to attempt of the general arrangement of the work, there are one or two points worthy of being noticed in a preliminary manner. In the first place, it is evidently undertaken by its two authors with somewhat different objects. M. Verdeil is a physiologist as well as a philosopher, an original observer as well as an original thinker, and it is from him that all the facts newly acquired to science which are contained in the work are derived. M. Robin, who is well known to many readers of this Journal, is a disciple of the positive school, and is rather
occupied with the relation of biology to "la haute philosophie" than with actual investigation.

The first volume, of no less than 723 pages, is altogether devoted to the consideration of the immediate principles in general. It is for the most part contributed by M. Robin. In the first place, 100 pages, under the head of Prolegomena, are taken up with the demonstration that the study of the immediate principles is included in biology, and the discussion of the relation of that science to the other sciences of observation. With reference to this part the only remark which we can make is, that, as it appears to us, it might with ease have been condensed to one-tenth of its bulk, without losing in value. In a similar manner we feel obliged to pass over the first chapter, which contains 190 pages, and treats of the character of the proximate principles in general, as prefatory to the discussion of classification. These characters are discussed in their variations according to sex, age, race, different species of animals, and abnormal conditions; and are themselves sub-divided into five orders—namely, mathematical, physical, chemical, organoleptic, and organic. It seems to us that this multiplication of sub-divisions is decidedly objectionable; and that two out of the five orders—the first and fourth—might have been dispensed with. By an anomaly not very easily explained, one of the most important questions, that of the formation of the immediate principles, is discussed incidentally in this chapter, under the head of "variations according to age".

The third chapter is devoted to the development of the classification itself. In this respect the authors have succeeded perfectly. Setting out on the admitted principle, that every classification must be founded on the comparison of the ensemble of the characters presented by the bodies to be classified, MM. Robin and Verdeil have indicated three fundamental and very natural groups. Of these the first comprises all the immediate principles which are either crystallisable or volatile without decomposition,—a character which corresponds to their possessing a definite chemical composition, and being capable of combining with similar chemical compounds or elements in definite proportions; the second includes bodies which are neither crystallisable nor volatile without decomposition, and which possess, correspondingly, an indefinite chemical constitution. This group is formed of the albuminous and gelatiniferous compounds, which are extremely numerous, and, as respects their chemical composition and characters, pass into each other by insensible transitions.

The first group very naturally separates itself into two sub-divisions, of which the one includes the principles of mineral origin, and the other those of organic. The former of these enter the organism from without, in the same chemical form that they possess while constituting a part of it. The others occur only in organised structures, and their formation is a phenomenon of life. By an anomaly of arrangement, which has been already mentioned, we have to refer
for the justification of these divisions to the preceding chapter. There, in fact, we find discussed one of the most important questions in the history of the immediate principles—that of their formation and mode of termination. Let us endeavour to penetrate the views of our authors on this point. We shall find that the divisions which we have indicated above correspond as completely to distinctions in the dynamical as in the physical or chemical characters of the immediate principles.

With respect to the principles of the first group, it is to be observed that they generally remain in the condition in which they existed exteriorly. Of these some, such as phosphate, carbonate of lime, silica, etc., do not, however, remain in the body in the liquid form, but, in the condition of what has been termed incrustation, combine with the substance of various tissues. In this respect they are not exceptions to the general facts mentioned above, inasmuch as their chemical, though not their organic, condition is the same in the mineral kingdom. All these substances, which are insoluble in water, must obviously enter the organism in a state of liquefaction or solution. The method in which this is effected has been illustrated by M. Verdeil, in researches which were communicated to the Academy of Sciences only last summer. M. Verdeil’s investigations, which were conducted, in conjunction with M. Risler, at the Government School of Agriculture, were directed to the solution of the question of the mode in which silica, carbonate of lime, and other insoluble substances, enter into the tissues of vegetables. M. Verdeil has discovered that in all fertile soils there exists a substance, previously undescribed, soluble in water, possessed of very peculiar characters. This substance is easily obtained from the soil by percolation with distilled water, and slow evaporation of the filtered solution. It contains, as immediate principles, first, a compound analogous to sugar in properties and composition, which forms in general from 40 per cent. to 50 per cent. of the dried extract; and secondly, various mineral principles, several of which are perfectly insoluble in water,—as, for example, silica, which in some soils is found in the proportion of more than 16 per cent. of the dried extract, and carbonate of lime in that of more than 20 per cent. There could be no difficulty in referring the solubility of the substances in question to the presence of the organic principle; and M. Verdeil has since discovered, that it is possible to produce artificially a similar result by employing a solution either of grape sugar, cane sugar, or dextrine, which is capable of dissolving silica, phosphate and carbonate of lime, etc. The experiment is rendered even more conclusive by the observation, that no sooner is the sugar of the solutions in question allowed to undergo putrefaction, than the silica, or other insoluble principle, is at once precipitated. It appears to us that, not only with respect to the nutrition of plants, but in their bearings on the physiology of absorption in animals, and
their practical applications in dietetics and therapeutics, the facts demonstrated by M. Verdeil may probably be the first of a very important series.

The immediate principles of the second group, to which the name of organic substances has been given, are represented in animals by the so-called albuminous or protein compounds. The process of their formation, to which the term assimilation has been assigned, is one the analogue of which is not to be found otherwise than in connection with organisation. This process is divisible into two stages. Of these the first corresponds to the development of the compounds in question, at the expense of the mineral principles or of their elements. This development occurs exclusively in vegetables. The second stage corresponds to the transformation of the nitrogenous principles for the condition in which they exist in the food of animals, by a series of isomeric metamorphoses, to that in which they combine to form the living tissues. Of the nature of the first stage it is impossible, in the present state of science, to form an adequate conception, inasmuch as we are unable to compare it with any other process in nature with which we are acquainted, and, for want of observations made upon plants at different stages of their development, we are altogether without information as to its concurrent phenomena. The only undoubted fact which we can boast of is, that the organic substances cannot be formed otherwise than in living beings. The grounds seem, to us, at present insufficient for the admission of the doctrine which was first indicated by M. Robin, in his "Tableaux d'Anatomie," three years ago, and which has received its further development in the present work, of the analogy between the process in question and certain forms of chemical change dependent on catalysis. In supporting this doctrine, we think that it may be shown that our authors have neglected the warnings, so often repeated by themselves, of the danger of deriving explanations of organic phenomena from supposed correspondences with molecular acts occurring in the inorganic world. M. Robin observes:

"The act of the formation of elements which gives rise to the non-crystallisable principle can only take place, in so far as these materials exist, in the presence of a body which, without furnishing anything itself, is however necessary to the accomplishment of the combination."

In what example of catalysis do we find a catalytic body similar to that which is in process of formation? In every case the catalytic body is of a different nature from the ultimate result of the
process. Where is the similarity between diastase and glucose, or between the latter and sulphuric acid? Where, again, is the similarity between caseine and lactic acid, the ultimate result of the lactic catalysis? We, in fact, see no reason whatever to believe that the already existing organic principles stand in a relation to the process of the new formation of principles similar to themselves, which corresponds in the slightest degree to the relation of a ferment or catalytic body to the catalytic molecular changes produced, independent of vital phenomena. Not a single example is to be found of the production of a new compound in the course of such a process, differing in chemical composition from all previously existing compounds in the same situation, except the catalytic body itself. We are, in short, under the necessity of returning to the admission with which we set out,—namely, that organic substances, when in their dynamic condition, are endowed with the power of causing the elements of principles derived from the inorganic world to combine so as to form substances similar to themselves. The phenomena and subordinate conditions of this process obviously constitute a wide field for research, which to this moment has remained almost untrodden.

Of the transformation of the organic principles, as they occur in the food of the higher animals, into the elements of the living tissues, it is the most remarkable feature, that it is brought about in two consecutive stages, which, in the order of results, are mutually converse to each other. The albuminous compounds are introduced into the stomach as articles of food, in the form in which they exist in the tissues of plants or animals (albumen, caseine, musculine); in the intestine they assume an intermediate form, under which they enter into the blood; while, in the capillary circulation, they regain the properties which they originally possessed. Thus we have, in the case of the ingestion, muscular fibre, musculine as the point of departure, albuminose¹ as the intervening condition, and musculine again as the ultimate result. The phenomena of the first stage of the process have been well observed; and we are able in so far to explain them, as we can compare them to changes which take place outside of living organisms. We are certain the action of acids similar to that of the gastric juice actually does produce the same changes in albuminous substances submitted to its influence, under the same conditions of temperature and dilutions, whether the operation be performed in the stomach or in the laboratory of the chemist. We know also that, under similar conditions, the substance called

¹ The term "Albuminose" was first applied by M. Mialhe ("Mém. sur la Digestion et l'Assimilation des Mat. Albuminoïdes," Comptes Rendus, 1846, t. xxiii. p. 260) to the form to which all the albuminous substances are reduced in the small intestines, and under which they enter the blood by the portal veins. It is incapable of coagulation, either by heat or by the action of a ferment, such as rennet. The precipitate, which is obtained on the addition of alcohol to its solution, is soluble in water.
pepsine is capable of producing the same result outside as inside of the intestinal canal. There seems no reason to dispute the correspondence between this process and that of the conversion of gluten and other analogous substances into diastase, as the point of departure, and the result produced, as well as the essential conditions, are similar. Admitting, however, that the primary digestion of the albuminous compounds is a catalytic phenomenon, we do not see how it is possible, at the same time, to admit that the converse process,—namely, the transformation of albuminose into albumen, fibrine, caseine, musculine, etc., can be explained in the same manner, or can be supposed to be of the same nature. Here we have no correspondence, either in the ultimate result or in the existing conditions. Here, again, we find a process of assimilation without analogue, and which occurs only in living beings. The same remarks apply to it as to the original development of the albuminous compounds, which we have already considered, as it occurs in vegetables.

MM. Robin and Verdeil consider all the immediate principles of the second group, those, namely, which possess a definite chemical composition, and do not exist in nature otherwise than inorganised beings, as products of disassimilation, or, in other words, of the decomposition of the highest results of assimilation. The conclusions which we have already arrived at, with respect to the nature of this process, in the nature of this process, being admitted, it is obvious that this must actually be the case. The principles in question do not, in general, form constituent parts of the substance of the tissues, but represent the forms into which the organic principles resolve themselves, for the purpose of separation from the organism.

It is obviously impossible, in the present state of science, to indicate the very limited conclusions which the scattered facts in our possession enable us to admit, in a general view, of what may be called the disassimilative process. It may be stated the albuminous compounds correspond to the point of departure, while the ultimate results are numerous, and differ widely from each other. We shall, we think, avoid the omission of any fact of importance in discussing, as separate divisions of our subject, the mode of origin, in the first place, of the crystallisable immediate principles containing nitrogen; secondly, the fatty compounds, the sugars, and other principles derived from them. We do not, however, engage to confine ourselves strictly to this arrangement.

It will be denied by no one, that all the azotised principles of definite composition which exist in the economy are derived from the disintegration of the organic substances, and that they correspond to different stages in a process by which the elements of these compounds are restored to the inorganic world. Of the order of succession of these stages we as yet know very little. We are certain, however, that several of these principles, as, e.g., urea, creatine, creatinine, etc., are actually ultimate results, as far as the disassimi-
lative process is concerned, or, in other words, they are destined to pass out of the organism without undergoing further change. With respect to urea, we cannot determine whether it is derived from the albuminous compounds, as the result of one or of several transformations. As to creatine and creatinine, we may conclude, from their constant association with muscular fibre, that they owe their origin specially to muscular. Another fact, to the knowledge of which the invaluable researches of M. Bernard have led us, is that of the formation of grape sugar, from the transformation of the albuminous compounds. As the so-called "vital air," as grape sugar, we shall ultimately find that, on the other, there originates simultaneously with it a principle containing nitrogen. However this may be, it is evident that all of the phenomena to which allusion has been made, and many others yet to be observed, must be taken into account, before we shall be able to form a correct conception of the general nature of the dis-assimilative process.

We are perfectly aware that, in accordance with the doctrines of the so-called "vital chemistry" of the day, the formation of urea, uric acid, and other similar compounds, constitutes, in each case, a stage in a supposed process of combustion, of which the albuminous compounds are the subjects, and carbonic acid and ammonia the ultimate results. It is one of the principal objects of MM. Robin and Verdeil to combat this theory, which has exercised so remarkable an influence on physiology, and is looked upon, not only by chemists, but, in a great measure, even by anatomists and physicians, as the great key to the explanation of all the mysteries of living beings. Let us, before proceeding further, examine, with our authors, the mode in which this theory originated, and the reasons which can be given to account for its present ascendancy.

Everyone knows that it was Lavoisier who first demonstrated that oxygen, or, as he called it, "vital air," is absorbed during respiration. He believed, but did not demonstrate, that the decomposition of this vital air gives rise to the production of animal heat, water, and carbonic acid. At this day the theory remains, although modified according to the new facts which have been since discovered. It has now been shown that in the higher animals carbonic acid is set free in the proportion of from 70 to 90 per cent. to the oxygen absorbed. Further, the experiments of Magnus, the most important in their results since those of Lavoisier, have shown that this last was mistaken in supposing that the development of carbonic acid and
water takes place in the lungs, but that both oxygen and carbonic acid exist in solution in the blood, and that consequently the latter enters into the combinations for which it is destined in the capillary system. Oxygen exists in the arterial blood in a proportion more than double of that in which it exists in the venous blood. Carbonic acid also rather preponderates in the venous blood, when compared with the arterial. Oxygen, according to the researches of Berzelius, is held in solution principally by the globules which, in common with all organised structures, whether dead or living, as was first shown by Spallanzani, have an extraordinary power of absorbing and fixing this element.

The foregoing constitute all the facts which we are possessed of in relation to the respiratory changes in the blood. We have, at one end of a series of molecular acts of the most complicated character, a highly oxidised product, and, at the other, oxygen. While we admit this as an undoubted fact, we do not, at the same time, admit that each of these intermediate changes is an act of combination. We do not admit, for example, that urea only differs from uric acid in being more thoroughly burnt than it, or that the essential dynamical distinction between albumen and the neutral fats is identical with that which exists between the noble and base metals in the mineral world,—namely, that the one is much more easily oxidised than the other. Between the fixation of oxygen and the exhalation of carbonic acid, a succession of other acts is brought about, which, taken individually, are much more complicated than those to which, in its ordinary and historical sense (the production of flame), or even in its subjective sense, the term combustion is applied.

If, commencing with carbonic acid, we endeavour to follow backwards the series to which we have alluded, the first point at which we shall be arrested will be that of the alkaline carbonates, as it is certain that a great part, if not all, of the carbonic acid exhaled from the pulmonary surface, is derived from the decomposition of these salts. In process of time it may probably be established, that the alkaline re-action of the blood is owing exclusively to the alkaline carbonates, although the question remains yet to be determined positively. It is upon facts derived from the analysis of the ashes of the blood, that Liebig has founded the conclusion which he states in the “New Letters,” published only last year, that in this fluid in man, and in general in all more or less carnivorous animals, alkaline carbonates do not exist. It is a fact, however, admitted by all, especially with respect to alkaline carbonates and phosphates, that, according to the method of incineration employed, great difference may be found in the constitution of the ashes of the same animal substance (see Lehmann, Bd. ii. s. 240). It is maintained by Liebig, that in man and the carnivora the alkalinity of the blood is owing not to carbonate of soda, but to the tribasic phosphate (3Na.0, P0₂).
The existence of this salt in the blood is, however, by no means proved; for it is certain that, during incineration, if both the carbonate of soda, and the neutral phosphate of soda \(2\text{Na}_0\text{H}_0\text{P}_0\text{S}_3\), a re-action will take place, carbonic acid will be disengaged, and the tribasic phosphate formed. All the expedients which have been hitherto devised to obviate this fallacy have proved more or less futile; and the only means which is left us to ascertain the condition in carbonates and other salts, capable of alteration by the process of incineration, really exist in the living organism, is that to which M. Verdeil has given so much prominence, and of the value of which he has given so many proofs in the work before us. This method consists in causing the salts in question to crystallise, by slow evaporation of the liquid or extract, after the removal of the albumen by coagulation. In this place we content ourselves with a mere allusion to it, reserving details for a future occasion. Admitting that alkaline carbonates exist in the blood of all animals in considerable quantity, as has been proved with respect to a certain number, as we have endeavoured to point out, and has not been disproved with respect to any, we have to take into consideration another class of immediate principles before we shall be enabled to assign them their true place in the series at present in question. We allude to the organic acids, whether introduced from without (vegetable acids) or of internal origin. We know that the neutral potash and soda salts of the vegetable acids, when received into the blood, undergo a series of changes which result in their conversion into the corresponding carbonates. This takes place especially in herbivorous animals, in which the carbonates thus produced are excreted by the kidneys, and appear in large quantities in the urine. If, on the other hand, free organic acids be ingested as food, as is obviously constantly taking place in all herbivora, the existing carbonates are decomposed, carbonic acid is set free, and a salt of the organic acid formed, which itself undergoes the process of change mentioned above. If, however, there be an excess of the acid in question, it passes out of the organism, and appears in the urine unchanged, it being a fact which may be considered as demonstrated, that no organic acid is capable of being decomposed in the blood, or, in the language of the chemists, burnt, otherwise than in the condition of an alkaline salt.

Without, however, referring to the ingestion of vegetable organic acids as an article of food, a phenomenon of constant occurrence only in the herbivora, we have in all animals the conditions required for the admission of the theory of the direct development of carbonic acid by the decomposition of the alkaline carbonates, in the fact of the existence of free acids as a constant condition of most of the tissues in the living organism. The existence of free lactic acid in the muscular tissue is already a fact familiar to physiologists. The acid re-action of the kidney is well known, but not altogether accounted for. The fact, however, which has above all tended to the corroboration of the theory, is that of the constant occurrence of a free
acid, of well-defined properties, in the lungs of all animals which have been hitherto examined. This acid, which was discovered only a year ago by M. Verdeil, who is at present engaged in investigating its chemical characters, exists in the free state as a constituent of the tissue of the lung, to which it communicates a very well-marked acid re-action. It crystallises from its alcoholic solution in extremely beautiful groups of prisms, which usually radiate from a common centre, each terminating in a pointed extremity. Along with the free acid, M. Verdeil has demonstrated the presence of its soda-salt, which he considers to be produced by the decomposition of the alkaline carbonates, in accordance with the theory to which we have already drawn attention. Pumic acid, like lactic acid, which probably performs the same functions in the economy, disappears in the blood after assuming the form of its soda-salt. The presence of the acid itself has been demonstrated in the blood, but it does not exist in the urine. Of the mode of its origin, M. Verdeil observes that, like the acids of the bile, it is obviously a product of the decomposition of a more complicated compound, although he is unable to indicate its nature, or that of other immediate principles, which may be supposed to originate at the same time with it.

Unfortunately, with respect to a large number of the immediate principles of definite composition, our existing knowledge does not allow us to express ourselves in a more determined manner. We know that numerous compounds, differing widely enough in characters, such as, for example, grape sugar, the neutral fats, creatine, urea, etc., are all derived more or less directly from the albuminous compounds. Considering that the latter are more complicated in their chemical composition than the former, we are justified in believing that the transition must take place by the splitting of the more complicated body into two or a greater number of more simple ones. We may also conclude, that this is rather brought about by a series of successive changes, and in a gradual manner, than by an action of so violent a nature as that to which we unavoidably attach the term combustion.

In a succeeding paper we shall complete the remarks which have preceded on the mode of origin and disappearance of the second group of immediate principles in the living organism; and, in conclusion, we shall endeavour, in the first place, to indicate the method which is to be employed in the separation of the immediate principles, and in the investigation especially of their dynamical characters; and, secondly, we shall discuss the nature of the evidence on which we ought to depend, in assigning to any chemical compound the place of an immediate principle, as well as the sources of fallacy to be guarded against.

(To be continued.)
In pursuance of our examination of Mr. Headland's therapeutical doctrines, we come, now that we have traced his remedies into the blood, to inquire what effects they there produce. We shall allow our author to state his views almost entirely in his own words:

"Prop. VII.—That a first class of medicines, called \textit{Hæmatics}, act while in the blood, which they influence. That their action is permanent.

1. That of these some, called \textit{Restoratives}, act by supplying, or causing to be supplied, a material wanting, and may remain in the blood.

2. That others, called \textit{Catalytics}, act so as to counteract a morbid material or process, and must pass out of the body.

Supposing that a medicine has fairly passed into the blood, and circulates round with it, there are now two ways in which it may behave itself.

In the first place, it may have a tendency towards some tissues or parts of the body, on which to exert its powers, as the nerves, or the glands, or muscular fibre, and may use the blood only as a vehicle by which most readily and easily to attain to these. Such are neurotics, astringents, and eliminatives. They may not affect the blood, but they must pass through it.

But there is another and still more important class of medicines, whose action is particularly directed towards the blood itself. The blood, after their action, is different from what it was before. It may be a change for the better or for the worse, but there certainly is a change. Medical authors, with few exceptions, have been very backward to acknowledge the existence of medicines of this description. But even those who would fain have classed all medicines as stimulants or sedatives, differing only in the kind or degree of their action on the nervous system, have in many cases been obliged to confess, that there is a set of remedies which they call 'Alteratives,' whose action, though slower, is more certain and more durable than that of the former. It is allowed that they alter the condition of the blood. To suppose that they do so by first influencing the nerves, is to adopt a circuitous and uncalled-for explanation. It is proved that they pass into the blood. It is known that when actually applied to nerves, they do not affect them. From these considerations merely, without further evidence, it would seem tolerably clear that they act by influencing the blood itself, simply and solely. But this it will be my business to prove more at length directly.

Such medicines, then, I have designated hæmatics, a simple and expressive term which has been used by others before me.

Hæmatics are very numerous and very important. I shall thus devote some space to their consideration. But I must first lay down a broad distinction between the two divisions of hæmatic medicines. The diseases in which they are used appear all to originate in the blood, however they may manifest themselves.

Now some of these diseases originate in a want of some principles or constituent of the blood, which want causes an aberration of the vital functions.

Thus in anaemia there is a deficiency of the hæmatosin of the blood corpuscles. In simple debility a want of a similar nature probably exists.
matic fever and other disorders an excess of acid is formed and eliminated, possibly from a want of the alkali by which it should be neutralised. In common inflammatory fever there is an abnormal oxidation of the proteinaceous compounds; possibly arising, as we shall see hereafter, from a failure of some principles which are the proper food of the oxygen. In diseases causing urinary deposits, there is a want of those principles which should naturally retain them in solution. In typhoid fever there is said to be an excess of basic matter, and a deficiency of acid, in the blood. In pulmonary phthisis there is a deficiency of fat in the system. In the latter stage of malignant cholera there is an absence of watery particles in the blood. Some suppose that in scurvy there is a want of the salts of potash in the blood.

These diseases, then, in some of which the want is proved, in others partly hypothetical, may be treated by medicines which supply the deficient matter, and thus restore a right state of things. They may supply it to the blood directly, or else cause it to be generated there. The former of these modes of restoration seems to be the most frequent, and may possibly, when we shall know more of such matters, be found to occur in all cases. This division of haematics I have named restoratives (restaurantia). Their action, as we shall see, is in some cases apparent, in others more obscure. They restore the blood directly to its proper condition, if there is only a deficiency; but they do not in general seem to have the power of counteracting any morbid or active material that may exist in the blood. Nor do they, except in large doses, exert themselves any peculiar action on that fluid. In these respects they differ from the other division of haematics. They also differ in another important character. Each restorative has in healthy blood a substance analogous to, or identical with, itself. It replaces this when deficient.

Not so with other Haematics. There is in general nothing in the blood corresponding to them—or if there be in some cases, they are not introduced with the intention of supplying its want. Thus Restoratives may remain in the system, and are intended so to do; but these may not remain. They must pass out. In so doing they come under the head of eliminatives, or that of astringents. This is their secondary action, distinct from their primary and most important operation. What then is the curative action of these remedies?

A large class of diseases depends on the presence in the blood of a morbid material, or, what amounts to the same thing, on the constant working of a morbid process in that fluid. Some of these, as the eruptive fevers, will run a certain course, and then come to an end. These we cannot generally stop, but can only alleviate. But others, more in number, and more commonly met with, tend naturally to run on for an indefinite period, unless by any means we can arrest their progress. Some depend on a contagious virus, communicable from one person to another, as syphilis. Some, as ague, are dependant on atmospheric or terrestrial influences. Others are due to some derangement of the secondary assimilative processes, as scrofula, scurvy, gout, and rheumatism. Others again, to causes that are ill understood, as convulsive disorders and skin diseases. Lastly, some may be caused in many different ways, as common inflammatory fever.

Now the object in the treatment of such diseases is to obtain in each case some remedy that shall be able to counteract this process, something that shall destroy the morbid influence at work, and thus restore health. Medicines that are used with this intention form the second division of haematics, which I have named catalytics (catalytica) from a Greek verb, signifying to destroy or to unbind."—Pp. 94-98.

It appears, we think, from these extracts, that Mr Headland is a very decided example of the partizans of the humoral pathology and consequent therapeutics which prevail so much at present in the schools. It is true that he gives at page 101 a disclaimer of his being a follower of the old humoral school, who attributed every-
thing to peccant humours; but in reality this does not amount to much, as his own words will testify.

We frankly confess that modern humoralism has, for a long while, been to us a subject of some anxiety; for we fear that its tendency is to exert an influence very unfavourable to the ultimate progress of pathology and therapeutics. At the same time, we are quite prepared to admit that we have derived, and are deriving from it, benefits which are not to be despised by us as practical physicians. There can be no doubt that in a great many, we might almost say in most, diseases, the blood at one time or another is in a state which is not normal; but the recognition of this most interesting and important fact has led too many writers of the present day to confound this which is only a phenomenon of disease with the pathological cause of disease; and when they can demonstrate that the blood is not quite what it ought to be, they have been too ready at once to jump to the conclusion, that the malady in question is a "blood disorder," and therefore that the remedies which cure it are haematic medicines. Nay, not content with this, they assume many diseases to be of blood origin, in which nothing abnormal can be demonstrated in the blood at all; and take refuge in the assumption, that there is circulating in this vital fluid a poison of some kind, but which is too subtle for our present limited powers of observation to perceive. Mr Headland appears to partake largely of these ideas, when he tells us that the diseases in which his so-called haematics are used, "appear all to originate in the blood, however they may manifest themselves;" and when, at page 173, he says, "the inflammatory process, the chief seat of which, however it may originate, is on the circulating blood."

Now this is precisely the pathological doctrine with which we profess ourselves dissatisfied, and which feeling we consequently extend to the therapeutics founded thereupon. We say it is most important and essential to us, as practical men, to be aware of these changes in the blood; they are of the greatest consequence to us in the exercise of our art; they must be especially borne in mind by us in arranging the dietetical and hygienic management of our cases, and are not to be neglected in planning their medicinal treatment. But as pathologists and therapeutists, we must look not only at them, but beyond them; and put to ourselves the question—How do these morbid conditions of the blood arise? Is it really true that such and such disease arises in the blood, or is not this change in the blood only a
result of something which is previously wrong in the living solids? And, consequently, is it true that our remedies act only haematistically by adding something to the blood or subtracting something from it, or do they not rather, in curing disease, exert their influence on the living solids, and, by controlling or modifying their actions, bring back the blood indirectly, but not less surely, to its normal state? We would carefully guard ourselves against the imputation of denying altogether the existence of blood diseases and haemastic remedies, which would be absurd with the example of anaemia and chalybeates before our eyes. What we protest against is the unwarranted extension of this doctrine. We are quite prepared to admit it to a certain extent. There are such things as diseases, if not essentially originating in the blood, at least inseparably connected with a depraved state of that fluid, and most appropriately treated by medicines which restore it to a healthy condition; but we believe that their real number is very small, and consequently that the number of medicines which are entitled to be considered as true haemastics is very small also.

But this is a question which has a much higher import than merely to settle the modus operandi of certain individual remedies or classes of remedies. It involves the whole foundation of therapeutics as a science. If we are to follow Mr. Headland, and the school which he represents, and are to believe that a large proportion of remedies, and these the most important in the Materia Medica, cure diseases merely by the process of supplying some deficient, or removing some redundant, matter in the blood, we look upon the living organic solids in these instances as being little else than passive, and their vital properties—physiological or pathological—need give us comparatively small concern. Therapeutics thus becomes reduced to little better than a department of applied chemistry; our remedies are merely a set of re-agents, and our treatment of disease a process of synthesis or decomposition with which the living body scarcely has more to do than the flask of the chemist has with the compounds which he concocts within it. And this is very much the result to which the over-zealous chemical school of the present day, now in the ascendant, are leading us.

If we had nothing else to depend upon than the modern humoralism, the hopes of the advent of a great era in therapeutics, formerly expressed by us, would be small. We owe much to modern chemistry for what it has taught us as to the fluids of the body; but at the most it has unveiled to us only some of the phenomena of disease; it has taught us but little of its essential pathology. It is not to her, therefore, that we look hopefully for great general doctrines as to the real nature of disease, and the real methodus medendi, except so far as she is the handmaid of physiology. It is from physiology that we expect some important revelation of the vital actions of our bodies, which shall form the basis of a scientific pathology and a scientific therapeutics. It is to attain this vantage ground that the battle of
science must be fought, and modern "rampant humoralism" opposed and kept in check by a vigorous defence of solidism.

If we wished to illustrate what we conceive to be the unfortunate tendency which this humoralistic view of the nature of so many diseases and the action of so many remedies has, to arrest the full development of therapeutics as a science, we could not find anything more to our purpose than the two following sentences of Mr Headland's book, which show in the instance of this acute and reflecting writer, that the mind which can rest contented with these doctrines runs great risk of losing sight of the importance of attaining, or indeed of aiming at, general principles, which alone can effectually raise therapeutics to its proper position as a science.

Speaking of Catalytic Hæmatics generally, he says at p. 98—

"Some have tried to give a general explanation of their action, and have talked of it as if it were easily understood by their known properties. I am not of their opinion; and when I speak of Catalytic, I shall give my reasons for disagreeing with them. Now each Catalytic has peculiarities and affinities that distinguish it from all others. I have not thought that I could arrange them more truthfully than by subdividing them according to the diseases which they tend to counteract. How inadequately is the action of Mercury and of Iodine expressed by calling them special stimulants, alteratives, or absorbents! Is it not better and more correct to say at once that Mercury is useful in checking inflammation in general, and in counteracting the poison of Syphilis in particular; and that Iodine is effective in secondary forms of the latter disorder, as well as in Scrofula?

"These medicines, then, are specifics, in so far as they are particularly useful in certain disorders, and in those excel other remedies; but they are not, in the vulgar acceptance of that term, the only medicines which can be employed in such a disease, nor is their use to be restricted to it alone."

And again in propounding his classification, at p. 102 he adds a foot note to this effect—

"The details of this arrangement are founded solely upon the therapeutic operation of medicines, as used to cure diseases, and not in any way upon their physiological action upon a healthy man. Much unnecessary confusion, in works on Materia Medica, has arisen from these two different matters being taken into account at the same time. Food is the only restoration of wanting material needful to a healthy man; neither is there in such a case any need or opportunity for the counteraction of morbid agencies."

Now to our eyes nothing can be more decidedly retrograde in character than any doctrine which tends to make us regard remedies as specifics even in the sense (the only intelligible sense of the word) as used by Mr Headland. By this we are asked to content ourselves with a set of bare isolated facts, and we are thereby precluded from attempting to trace out any possible connection or similarity in modus operandi between this one and that other; each is a specific and nothing more. Mr Headland, who reflects and thinks, although in our opinion somewhat erroneously now and then, may be proof against such a misfortune; but what is so apt to lead weaker brethren into an unreflecting stupid routine style of practice, than the idea that such and such a remedy acts specifically on the
materies morbi of such and such a disease? Then, again, his opinion that the considering together of the physiological action of medicines and their curative effects in works of Materia Medica has led only to confusion, appears to us to be exactly the reverse of the truth. We believe that the only remedies, of the modus operandi of which we have at this present moment anything approaching to a clear idea, are those which we have been enabled to trace as producing their therapeutical actions in disease in virtue of their physiological action in health. Whenever we acknowledge that we cannot do this and take refuge in the doctrine that their effects are due to a mere specific action on a morbid matter or morbid process, and irrespective of any power of affecting the living tissues as constituents of the living organism, then we make simply a confession of ignorance. Contrast our ideas as to the action of opium and the action of quinine. What enables us with so much freedom and satisfaction to make so many and so varied uses of opium, but that we know its physiological action as a narcotic on the healthy body, and can with tolerable certainty calculate from this its effects on it when diseased? How comparatively limited, on the contrary, our use of quinine, and how unsatisfactory our confession that we are yet ignorant of its physiological action; and that we employ it in a few forms of disease because we know that it has been found to answer by those who have preceded us. Can it be doubted that if we knew as much of the effects in quinine in a healthy body, as we do of those of opium, that we should find for it many valuable uses besides those to which it is now applied?

Had Mr Headland in the note which we have quoted from p. 102, contented himself, à propos of classifications, with saying that in making a classification of remedies, we should take either the physiological or therapeutic actions as the basis of our arrangement, and not both, we should agree with him entirely. But when he says that it only confuses works on Materia Medica by taking both into account at the same time, we look upon him as gravely heterodox. Make your classification a good therapeutical one if you can, or make it a physiological one if you choose, but in the one case trace back your therapeutical effects when you possibly can, to physiological cause for their production; in the other, trace your physiological action forward if you can to its resulting, when applied to a diseased body, in a therapeutical effect. When you cannot do this, honestly confess your ignorance, and keep this in view as a matter to be excogitated and investigated. It is when you take into account, both physiological and therapeutical action, and then only, that you aim at, and can possibly obtain a full elucidation of your subject. It is just because it leads us to fix our eyes solely on the therapeutical, and makes us shut them to the physiological, that we look upon the modern haematic humoralism as one of the most obstructive and dangerous shoals (for it is shallow) in the ocean of therapeutics. Oh for better soundings and truer charts!
We have been induced to offer the above remarks, not so much to put forth our ideas upon therapeutics generally, as to save much detailed discussion of the contents of the next 200 pages of the book before us. Our chief object is to continue our abstract of Mr Headland's remaining propositions, and these we shall set before our readers with a very few observations.

We have seen that our author divides his so-called Haematics into Restoratives and Catalytics. The following is his arrangement of the former of these:—

"I will divide them into six orders, which are all distinct and characteristic in their mode of action.

RESTATRANTIA.

Ord. 1. Alimenta.
Ord. 2. Acida.
Ord. 3. Alkalia.
Ord. 4. Tonica.
Ord. 5. Chalybeata.
Ord. 6. Solventia.

On reverting to Proposition VII., which treats of the action of the first class of medicines, it will be seen that what has been there stated with respect to the action of Restoratives resolves itself into the following simple affirmations or minor propositions.

m. p. 1.—That they act in the blood, and that their effect is permanent.

m. p. 2.—That there are naturally in the blood substances which resemble or coincide with them.

m. p. 3.—That they are not of necessity excreted, but may remain in the blood.

m. p. 4.—That they are of use when a disease depends on the want of one or more materials in the blood."

On the subject of the first order, Alimenta, we have nothing to say. Their title to the appellation of Restoratives, will not be questioned. Nor do we mean to put in a demurrer to the claims of Chalybeates to each as such. We look upon iron in anaemia, as the best example that can be found of a medicinal substance, acting by directly restoring something wanting in the blood. The truth is, its administration in cases of chlorosis, may in great measure be looked upon rather as dietetical than medicinal, and it is best taken soon after meals, so as to be digested and assimilated with other alimentary principles. We are not however inclined to agree unreservedly to the following remarks of Mr Headland.

"I believe Iron to be simply and solely a Restorative remedy. By improving the condition of the blood, it appears similar in action to Tonics; but it is not a true Tonic, for neither in debility nor in Ague is it of any service, unless there is at the same time Anaemia. In some cases, even of Anaemia, Iron will not effect a cure. This must be because the appropriative power of the system is so weakened and degenerated, that it is incapable of consigning even this needful substance to its proper destination."

No doubt anaemia is the great indication for its employment, and it is not of much use in simple debility, from exhausting disease for instance, because there we require rather some of those bitters which
more directly increase appetite and sharpen digestion; nor is it rapid enough in its action as an anti-periodic to be of general value in agues uncomplicated with the anaemic state; but in cases of neuralgia, for example, iron has often been found beneficial where no signs of anaemia exist. We are not disposed to infer, that in these instances there must have been a hidden anaemia because iron worked a cure, and we therefore prefer thinking that, in addition to its being a haemastic restorative, it is a tonic also. Its so-called emmenagogue effects are only an indirect result of its combined restorative and tonic action.

The following passage, in the same page from which we have just quoted, invites a single remark: —

"Anemia is the one disease in which iron is of use. Purgative medicines form a valuable adjunct to the treatment in most cases. Dr G. Owen Rees has suggested that they may be of use by removing some of the water of the blood, so that the corpuscles, being then shrivelled by exosmosis of their contents, may be in a fitter condition to absorb the 'ferriferous chyle.' And yet, if so, the drinking of a single glass of water would probably be sufficient to swell out the corpuscles again, and thus put a stop to the process. It is just possible that a cathartic may be serviceable by purging away from the blood some effete matters, as sulphuretted hydrogen, formed by the continual decomposition of the tissues, which would have hindered the operation of the chalybeate in the blood, by decomposing it."

We make Mr Headland quite welcome to dispose of Dr Rees' suggestion as he thinks fit, contenting ourselves with merely remarking, that it was a source of amazement to us how so intelligent a physician as Dr Rees could ever have propounded so singular a hypothesis. But what we would now point out is, how thoroughly modern humorism displays itself in the opinions of both these gentlemen as to the use of cathartics. Instead of hunting in the blood for an explanation, why not look in the bowels themselves? Is it not the simple fact, that chlorotic women, and such anaemic subjects, have not only a very impressionable nervous system, but at the same time generally very torpid and ill-conditioned bowels, and is it not much more likely that cathartics are beneficial in removing from their intestines matters prone to set up reflex irritations, than either by creating in the blood-corpuscles an exosmotic vacuum into which ferriferous chyle will be ready to rush, or in removing from the chalybeate medicine so dangerous a neighbour as sulphuretted hydrogen? No doubt, wonderfully fetid matters will be evacuated under such circumstances; but we question if they ever came from any more distant region than the intestinal canal. We shudder to think what would be the complexion of a chlorotic patient if there were sulphuretted hydrogen encountering iron in the alkaline blood. The disease would then be, we suspect, more appropriately called melanosis, were that not already the patronymic of a much more formidable enemy. We may remark, that in many of that very class of cases to which chalybeates are in general so applicable, the cathartics alone without the iron are no bad remedy. It is not long since we
cured, in the same establishment, two white-faced dressmakers each of a facial neuralgia by merely unloading their costive bowels by a few doses of scammony and calomel. Whether would we be more probably led to the very laudable practice of moderately purging such a patient—by the good old-fashioned Hamiltonian notion of getting offending matters out of the intestines and away from the nervous system, or by the new-fangled idea of altering the shape of blood-corpuscles or extracting iron-precipitating sulphuretted hydrogen from the liquor sanguinis?

Mr Headland's next section, Acids, demands, assuredly a full discussion; for it appears to us to be one of the most interesting problems in therapeutics yet awaiting solution, how acids act on the system. We cannot assent to all his doctrines regarding them, but we cannot in this matter find any great fault with him, for nothing that we have read or cogitated upon the subject has appeared satisfactory to us.

Mr Headland is of opinion that acids enter the blood as such. He rejects the idea of Dr Pereira,¹ that they are neutralised by the alkaline matter of bile, saliva, pancreatic juice, etc., in the alimentary canal before absorption. We are inclined to agree with Mr Headland thus far. But we do not receive from him any satisfactory solution of the difficulty that awaits us, as to how the acids act when they are once in contact with the alkaline fluid, the blood. That their action must be in some respect quid acids, is obvious from the fact, that the same amount of acid, if administered in the form of a neutral salt, does not produce the same effects; but we must say, that the following explanation by Mr Headland does not satisfy us, nor does it even appear plausible, simply because it is founded upon an assumption (excess of alkaline matter in the blood in disease), which we believe to be without foundation:—

¹ The blood is alkaline; which is due either to the presence of carbonate of soda, or (according to Liebig) of an alkaline phosphate of that base. So that the acid, on entering into the blood, passes at once into combination with this alkali, and the result of this is a general diminution of the amount of basic matter in the system, and an increase in that of acid. Thus a free acid may act as a restorative in cases where there is an excess of alkali in the blood. It may either remain in the blood, after entering into combination, or it may pass off by the urine, supplying there the place of a natural acid, which it leaves behind in the system. It is on such a theory as this that the action of mineral acids in typhoid and putrid fevers has been explained. I do not mean

¹ We cannot mention the name of Dr Pereira without indulging the melancholy pleasure of passing a tribute to his personal and scientific worth. We respected Dr Pereira as an author, and esteemed him as a man,—admiring especially the catholic spirit in which he received the opinions of others, and endeavoured to give their full measure of justice and due weight to all. His sudden and premature death, at the age of forty-nine, will be lamented by all who take an interest in that department of science which he so assiduously and successfully cultivated.
to affirm positively that there is in these cases an excess of alkali in the blood. Although likely, it is not proved. The explanation is plausible."—Pp. 109, 110.

Of the action of acids on the urine Mr Headland says very little, even when discussing them, under the order of solvents, as "antiphosphates." We allude to this interesting subject only to point it out as one which is most imperfectly understood, or rather not understood at all,—one which would well reward the labour of a patient investigator.

Acids are among the foremost of those substances which are called Refrigerants; and of them, in this capacity, Mr Headland gives the following account:

In febrile cases of all kinds it is often found that diluent drinks containing the free vegetable acids, or solutions of their salts with alkalies, act beneficially in lowering the pulse, and in moderating the progress of the disorder. These should both be distinguished from mineral saline drinks; for mineral salts, from their known effects on the blood, would seem to belong simply to my division of catalytics. But the effect of these vegetable acids seems to be to restore the blood to a more natural condition, and this independently of the action of the diluent with which they are administered. Now, of this effect there is no certain explanation; but a theory, which may or may not be true, may be ventured to account for it. Should it prove correct, it would seem that in such cases the free vegetable acid acts as a restorative; and that the alkaline salt of this acid has at first the same action, but adds to it afterwards a catalytic operation. Let us then attempt to clear up this matter.

"It has long been considered probable, but may now be said to have been proved by the researches of M. Becquerel, that in febrile disorders and inflammations there is excreted in the urine an excess of urea and of urate of ammonia, substances which are formed by the oxidation of the nitrogenous tissues. This extra-oxidation probably arises from a deficiency of that matter which is the proper food of oxygen in the system. This, as we have seen, being the step between grape-sugar and carbonic acid, must either be lactic acid, or something similar to it. It must be remembered that no food is usually taken in fever; this would at length quite cut off the usual source of this lactic acid, which is the starch and sugar of the food, and render it necessary that the animal tissues should continue to undergo oxidation, to maintain the animal heat. Now, if we compare the commonly received formula of proteine with those of lactic acid and tartaric acid, it will at once be seen, as was pointed out some time ago by Dr Murray, that the latter contain more oxygen, in proportion to their carbon and hydrogen, than is found in proteine or albumen.

\[
\begin{align*}
\text{Proteine} & = C_{140}H_{31}N_{5}O_{19}+\text{equiv. of K}\text{ or Na}+2\text{H}2O. \\
\text{Lactic acid} & = C_{6}H_{6}O_{5} \\
\text{Tartaric acid} & = C_{8}H_{4}O_{9}+2\text{H}2O.
\end{align*}
\]

Thus, while for 40 equivalents of carbon, proteine contains only 12 of oxygen; lactic acid contains 40, and tartaric 50, of that element. So it seems that albuminaceous matters, containing less oxygen, would require much more oxygen for their combustion; this would produce more heat, augment the number of respirations, and keep up the fever. And though it has been proved by Wöhler that free vegetable acids pass out in the urine without having undergone oxidation, yet the condition of fever would probably be an exceptional case. Lactic acid, the natural fuel, being deficient, the alkali with which it should combine must be present in some excess; so it seems likely that a free vegetable acid would combine at once with this alkali as the lactic acid would have done, and thus be burnt or oxidised instead of the latter. Its action would
then be simply restorative. Requiring less oxygen than the proteine would need to transform it into carbonic acid, it would thus diminish the number of the respirations, the frequency of the pulse, the temperature of the body, and in this way allay the fever. It would be strongly confirmatory of this idea if it were proved that the amount of urea and urates in the urine is actually diminished by the use of acid drinks and fruits in febrile cases. This hypothesis can only be admitted on the supposition that fever constitutes an exception to the general rule, that the vegetable acids pass through the blood without undergoing change.”—Pp. 111-113.

We need hardly offer any remarks on a hypothesis which requires so unwarranted assumption, as that the behaviour of vegetable acids in the system in fever is exactly the opposite of what occurs in health. We think it a much more important question to ask ourselves, whether the statement made by Mr. Headland, in common with most authors, that the so-called refrigerants do lower the pulse and check the progress of fever, be a medical fact or not? Our own observations have certainly never enabled us to witness anything of the kind. The acids are most grateful to the fevered patient, who, by a natural instinct, quite independent of all therapeutic ideas, is constantly asking the doctor if he may not have something “sharp” to drink; and the benefit which the sense of a gratified desire confers, in making the patient more comfortable for a while, is not to be overlooked. We have often endeavoured to observe whether they ever do produce any real diminution of the animal heat, and whether, therefore, their title, refrigerant, is really merited. We have not been able to satisfy ourselves of this; but we know, both from active and passive experience, that they give for the moment a sense of coolness, which renders them truly delicious either under a fever or a hot sun. This is true equally of mineral and vegetable acids; equally of hydrochloric acid, which contains no oxygen at all; and of tartaric, the relative amount of oxygen in which is so much insisted on. We have never thought that the chemistry of the matter had anything at all to do with it. We believe that it is at most (if the circulation of the surface be affected at all) only the result of a grateful impression made on the nerves of the throat and stomach; and we are the more inclined to this belief, that the cooling and refreshing effect is always very temporary, and is felt at the moment of swallowing the acid drink, and not afterwards, as ought to be the case, were it due to any modification of the amount of inspired oxygen consumed in the system.

(To be continued.)