A REPORT

ON THE PROGRESS OF

ANATOMY AND PHYSIOLOGY

IN THE YEAR 1843-44.

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The condition of physiology as a branch of knowledge—as an able writer has lately remarked of politics—was, up to a late period, that which Bacon animadverted upon as the natural state of the sciences, while their cultivation is abandoned to practitioners; not being carried on as a branch of speculative inquiry, but only with a view to the exigencies of daily practice, and the *experimenta fructifera*, therefore, being aimed at almost to the exclusion of the *lucifera*.

To this circumstance, quite as much as to the inherent and peculiar difficulties attending all investigation into the phenomena of life, must be ascribed the slow progress of physiology, while it was cultivated, more or less, with a view to its subservience to the practice of medicine.

There is, still, too general a disposition among medical practitioners to rate discoveries in this science in accordance with their immediate and obvious applications to the healing art. But surely the adoption of such a low standard of value implies great short-sightedness, and still greater ignorance of the history of science; as if the more general and abstract the truth, the greater were not, of necessity, the number of practical consequences it involves, and the greater, ultimately, the practical gain. Were it needful, a thousand instances might be brought from the annals of invention to illustrate this truth.

When knowledge, from being made subservient merely to the bodily wants and comforts of man, came to be cultivated for its own sake, he first became conscious of his true dignity and power. The springing up of this impulse in the various branches of human inquiry, was a great epoch in the history of all sciences, or rather might be said to constitute their first origin; for it was thus that art became science. It was then that inquiry into Nature grew to be a worthy and ennobling pursuit; and in reward for this obedience of man to his higher instincts, once a right method of inquiry found, the introduction of this spirit into the cultivation of each department of knowledge marked the commencement of a brilliant series of discoveries, which brought immense accessions to his power. In the study of the phenomena of life, the change was slow to make. Happily, however, for the advancement even of practical medicine itself, physiology is now extensively cultivated as a pure science. Numberless works of great merit are written upon it; chairs are instituted for the teaching of it; endowments are made in support of its votaries; and, in all civilised countries, numbers of eminent men dedicate their lives to its advancement. Thus in this branch of human knowledge, also, the division between the practical and the speculative, the *experimenta fructifera* and *experimenta lucifera,* is final and complete.

We must all rejoice, therefore, that this Association has at length recognised this great principle, in requiring separate reports on the subjects of physiology and practical medicine. Other motives, no less imperative, had indeed rendered such division necessary; for, so great is now the number of labourers in the field, so rapid the accumulation of new facts, and so active the development of new ideas, that the task of the mere annalist in either branch is one of no small labour. In that of anatomy and physiology, indeed, it may be questioned how far still further division might not be of advantage; for the character of the investigations now made into the phenomena of life is so various and minute, and the record of them is made public in so many different languages, that to give a critical view of the whole—and such alone is of any great value—demands a combination of acquirements rarely to be met with in a single individual. For my own part, I have painfully felt, in many particulars, my own want of the necessary qualifications for the task. To be consistent, too, this, the pure science of our profession, should have been placed in the hands of a professed physiologist, and not in those of a practitioner; and this for many reasons.

To enter with confidence on a subject of such scope and difficulty; to fathom the various and profound questions which the record of even one year's progress raises for discussion, had required much sustained thought in calm and retirement. How utterly impossible it is to realise this, amid the incessant interruptions to which those engaged in the active duties of our profession are so peculiarly liable, is, practically, known to you all. Highly as I estimate the honour of being appointed to this office of being your annalist for the year in this most important branch of our studies, it would have been unpardonable in me to undertake it, had I not known that, perhaps, the majority of the members of this Association might plead these same obstructions; and many, probably, with still greater force. I trust, however, that they will not the less be admitted as a claim for your kind indulgence for the many imperfections of my work.

But, it is time to enter on my subject. Cuvier, in a fine passage in the *Régne Animal*, thus attempts to define it:—

"Life," he says, "is a vortex, more or less rapid, more or less complex, whose direction is constant, and which ever bears along molecules of the same kind; but in which individual molecules are ever entering in and passing out, in such sort, that the *form* of the living body is more essential to it than its *matter*."

This passage sums up, in lucid statement, the two fundamental conditions of animal life that lie open to observation;—to wit, chemical change, and a specific form. These two conditions, under the heads of animal chemistry, and structural anatomy, are now being investigated, in all their manifestations, with an activity unparalleled in the history of our science, and under all the advantages which improved methods and more perfect instruments offer. It was not, perhaps, surprising that Cuvier should have attached the greater importance to the *form*,—to the anatomy than to the chemical change.

For although the discovery of the nature of respiration,—the complement of that of circulation, and the most important in the whole history of physiology,—had already been made, and might alone, and above all others, one would have thought, have awakened men's minds to the fundamental importance of chemical change in the phenomena of life, yet was this by no means its immediate result. Scarcely, at first, were the simplest and most obvious of its profound and manifold relations to these phenomena clearly perceived; so often have the most striking facts to be impressed on our minds before we come to feel their full force and significance. And even now that organic chemistry has grown to a vast body of science, and that relations of the most interesting kind have been unfolded by its means, there are few among us, I believe, who yet fully apprehend its true scope and importance. Some there are who still refuse its help in our science, and who put no faith in its interpretations. Such persons would do well to consider, for a moment, the great fact of respiration, and its incessant necessity to the maintenance of the higher functions of life. Why must the animal take in oxygen every moment or immediately die, or, at least, pass into a state of vegetative life, in which all the higher faculties are dormant? Why, again, when more motor power is expended, is there commensurate provision for increase of respiration, for more rapid chemical change? We have lost sight of the deep significance of these facts through our familiarity with them; and their true relations have been further obscured to us by our involuntary habit of contemplating this function in the character of a *want*, as known to us by our own consciousness, and as expressed in the struggles of the breathing animal, when the access of air is limited or prevented. Seldom in our habitual view of the act of breathing do our thoughts carry us beyond this. But, to the chemist, it ever appears, in ultimate analysis, simply as the provision for the introduction of oxygen into the economy, for the purposes of chemical change,—of combustion of so much matter.

That this combustion should be the source of animal heat, is simple enough; that it should bear a direct and essential relation to the creation of mechanical force, in such sort, that the latter may be—to use the word in its mathematical sense—a *function* of the former, is what, from the numerous analogies in nature, we may well conceive, though the intermediate mechanism be as yet concealed from us; but that this mere chemical change should be equally, and as incessantly essential to the manifestation of the functions of mind, is a fact which must ever remain a mystery to us. But what fact so strikingly asserts the claims of organic chemistry upon our attention, and the deep and intimate relations it bears to the most subtle phenomena of life. But if its claims on our attention be thus great, not less are those on our confidence, which its methods of investigation warrant. For all its problems are, in the last resort, resolved by the balance;—that severe and impartial instrument, whose introduction into chemical investigation by Lavoisier has changed the whole face of science, and which, appealing as it does to the most unalterable attribute of matter, deserves on that account a degree of confidence in its indications which few other methods of investigation justify.*

I have premised these few considerations, partly, because they appear to me, in some measure, called for by the present unsettled state of opinion on these subjects among professional men; but still more, because they put in a clear view the motives that have guided me in the division of my subject, and furnish my best justification for having given more space and detail to one branch of it than many might think required by its importance. This division, I need scarcely add, is that already suggested by the passage I have quoted from Cuvier, and has reference to the great movement of

* "It was not lightly," says Dumas, in his admirable *Lectures on the Philosophy of Chemistry*, "that Lavoisier made choice of this instrument. In this choice he was guided by a new and profound thought. For him all the phenomena of chemistry were due to displacements of matter, to the union and separation of different bodies. *Nothing is lost, nothing is created:* this was his motto, this his thought; and in the very first application of it he did away, for ever, with a great error." It was the absence of this thought which limited the discoveries of Priestley, brilliant as they were, in their influence on chemical philosophy. "Without doubt," says the same eloquent teacher, "Priestley was ill prepared to work a revolution in natural philosophy; but with his singular penetration, and with his felicitous art of taking advantage of every chance observation, he would probably have done so, had he only had the idea to endow himself with one instrument more."

matter through the living body, on the one hand, and to the organised forms and the actions of this matter in the sense of vital functions, on the other; in other words, it is the now familiar division into organic chemistry and structural anatomy, looking upon function as the result of their mutual relations. I shall begin with recording the progress of the former.

Although the past year has not been so prolific in new and comprehensive principles as the years immediately preceding, its contributions are by no means few nor unimportant. Numerous original investigations, of great value in particular departments, have been made public; and the novel views with which Liebig, Dumas, and other eminent chemists, have lately startled and so deeply interested the public mind, have received much interesting illustration and development, together, also, with some correction.

The first in rank and importance, in both points of view, stands the admirable series of papers on "Aliments and their Transformations," by Dumas and his fellow labourers.

Two of these had appeared in the previous year, in the *Records of the Academy of Sciences;* but their recent publication, in a separate volume, (the admirable *Memoires de Chimie* of Dumas,) and the circumstance of their not having been noticed in any of our former reports, give me the opportunity of passing them in review.

In the paper on the "Neutral Azotised Matters of Organised Bodies," by MM. Dumas and Cahours, the authors set out by stating that in an *Essay on Physiological Chemistry,* published eighteen months before, by the former and M. Boussingault,* it was laid down in principle,—that albumen, caseine, and fibrine, exist as such in plants; that they pass, ready formed, into the bodies of herbivora, and through these into those of the carnivora; but that plants alone have the power to fabricate these three products, which animals afterwards appropriate, whether to assimilate or consume, according to the needs of their life; and, further, in the same essay it was held, that these same principles might be extended to fatty bodies, which, according to this doctrine, originate wholly in plants, and come to play in animals the part of matter for combustion, or to be temporarily laid up in the tissues.

* *Chimie Statique des Etres Organisés.*

That, moreover, in the essay alluded to, they had recognised the necessity of grouping together all the organic bodies which have the property of passing into lactic acid by fermentation, such as sugar, starch, and their equivalents; bodies which enter as a staple and important element into the nutriment of man and animals, and are likewise produced by plants alone; views which, together with further developments, were summed up as a consistent whole in the following table:—

THE VEGETABLE		THE ANIMAL	
Produces ..	neutral azotised matters,	*Consumes*	neutral azotised matters,
"	fatty matters,	"	fatty matters,
"	sugar, starch, gums.	"	sugar, starch, gums.
Decomposes	carbonic acid,	*Produces*	carbonic acid,
"	water,	"	water,
"	ammoniacal salts.	"	ammoniacal salts.
Extricates	oxygen.	*Consumes*	oxygen.
Absorbs ..	heat,	*Produces*	heat,
"	electricity.	"	electricity.
Is an apparatus of reduction.		Is an apparatus of oxidation.	
Is motionless.		Is locomotive.	

The authors add, that although these laws have reference to a certain number of facts and principles already known, they constitute as a whole, and in this, their correlation, a system which they have a right to consider as new.

As far as the neutral azotised matters are concerned, they remark, in the memoir of which I am now giving an abstract, that the indispensable obligation under which all animals are placed, of making these matters a part of their food, almost demonstrates of itself that they have no power to create them. But to place this result in its full evidence, it is necessary to follow these matters through the organism, and ascertain their final destination. Now, it is extremely easy to prove that they are essentially represented by the urea which in man and the herbivora forms the principal product of urine, and by the uric acid which in birds and reptiles takes the place of this principle.

Abstraction made of the excrements, an adult man absorbs each day a quantity of neutral azotised matters representing scarcely more than fifteen or sixteen "grammes" of nitrogen; a quantity which reappears in its whole amount in the 30.32 "grammes" of urea eliminated with the urine, in twenty-four hours.

It was with the object of verifying, controlling, and limiting whatever of truth these conclusions contain in relation to the neutral azotised matters, that the experiments which form the body of the memoir now under consideration, were undertaken.

My space does not allow me to do more than to give the general conclusions they serve to establish, and which are thus stated in the memoir itself:—

1. "Albumen possesses the same composition in all animals, and, therefore, *à fortiori*, in all the fluids of a single animal.

"Vegetable albumen differs in nowise from animal albumen, as to elementary composition, only it is not accompanied by free soda, (according to Liebig, bi-basic phosphate of soda,) the contrary being ordinarily the case with animal albumen.

2. "Caseine furnished by herbivorous animals is always of one composition, and as nearly as possible identical in properties.

"In woman, however, who by her habits of life approaches the carnivora, the milk furnishes a species of caseine, which, although similar in composition to that of the herbivora, nevertheless possesses properties of such kind, that it may be one day necessary to establish a distinction between these bodies.

"In the blood of the ox a substance is found which seems identical with caseine, both in elementary composition and in general properties.. The flour of the cerealia likewise contains a body which may be classed with caseine, on the same grounds.

"Moreover, the caseine of the milk of herbivora, that of the milk of women, the caseine of blood, and that of flour, possess exactly the same composition as albumen; so that these are certainly isomeric bodies."

Thus far it will be seen that M. Dumas' results correspond with those of the school of Giessen, with which Liebig's works have made us all so familiar, and which, collectively, constitute the great discovery of recent times in organic chemistry; but at this point there arises an important difference between them.

M. Dumas contends that fibrine is not, as Liebig and his followers assert, identical with albumen, inasmuch as, although the difference be slight, it indisputably contains more nitrogen and less carbon than the latter.

The following is the mean result of his analyses of this body, as found in the blood of man:—

Carbon	52.78
Hydrogen	6.96
Nitrogen	16.78
Oxygen	23.48

The recent investigations of Mulder, however, of which I shall have to speak further on, cast some doubt over these results, and explain the difference between them and those previously current, upon other grounds.

M. Dumas records, also, several analyses of proteine, which were made with the greatest care. The following is the formula which he regards as best representing the constitution of this body:—

Carbon	48	54.44
Hydrogen	37	6.99
Nitrogen	12	15.88
Oxygen	15	22.69
Total		100.00

In regard to the composition of legumine, also, which on account of its abundance, and the important part it evidently plays as nutriment, in the seeds of leguminous plants, demands particular attention, M. Dumas differs from Liebig. He denies that this body is, as the latter had asserted, identical with caseine, and states that it contains, without doubt, more nitrogen and less carbon. He is of opinion that it consists of a mixture or combination of albumen or caseine with some other body richer in nitrogen, which modifies its most important properties. As, however, on being dissolved in hydrochloric acid it communicates the same properties as albumen, it is easy to suppose that, under the influence of the gastric juice, this matter furnishes the same soluble products as albumen itself.

Besides these four principal products—albumen, caseine, fibrine, and legumine—the authors recognise two others—glutine and vitelline—so closely related to these by their mode of action with hydrochloric acid, as to blend with them in one group, however distinct their properties might at first sight appear.

This highly important memoir concludes with a theory of nutrition, which differs in many essential points from that generally received. Its peculiar features are these:—" That for the purposes of life, and especially for the maintenance of animal heat, it is necessary that the great process of combustion should incessantly go on. That the materials for this combustion are furnished *directly* by the blood; and that, as they consume away, they must be restored to the blood either by the food or, failing that source, by matter from the living tissues, which thereby suffer damage. That the art of nutrition consists, therefore, in protecting the tissues from this damage, by restoring to the blood, at the same rate in which they waste away, the materials of which this fluid is itself composed."

Now, in protracted abstinence, there is no doubt that the tissues may suffer serious damage in this way. The general waste of the body in long illnesses, and also many forms of ulceration of the cornea and of cartilages, together with various destructions of parts resulting from habitually defective nutriment, are instances of this. It is equally certain that a large class of diseases, having no fixed place in our present systems of nosology, and now, through our ignorance of their true pathology, vaguely and variously classed in accordance with functional characters, or other principles still more general and undefined, will, eventually, be found to consist in abnormal destruction of tissue, by excessive action of oxygen, the great agent of change in the living economy, on parts whose vitality and power of resistance have been weakened by other causes. That this is the case with various forms of insanity and other obscure affections of the nervous centres, now referred to the vague heads of softening, atrophy, and other merely descriptive terms, admits already of complete demonstration. But, surely, M. Dumas cannot mean to maintain, that in the normal state of nutrition the tissues themselves suffer no change nor destruction. The doctrine generally received, and it seems to be one founded on the strictest induction, is, that in every functional act, a part of their substance necessarily, and by the very fact, loses its vitality and suffers chemical change, in effect of the great process of oxidation incessantly going on. According to this doctrine, therefore, the blood is not, as Dumas assumes, drained of its albuminous principles in furnishing, directly, material for combustion, but in repairing the losses which the tissues suffer, in prey to this process, and of which, under this view, they, and not the blood, are, as far as these principles are concerned, the great feeders. But the truth will, probably, eventually prove to lie between these two doctrines; and, indeed, the researches of Mulder, to be presently mentioned, show that the proteine compounds of the blood are incessantly being oxidised in the act of respiration.

In the memoir before us M. Dumas follows these matters to their final destination, in the following terms:—

"As the nitrogen they contain is found again, entire, in the urine, under the form of urea, it only remains to inquire in what urea differs from the neutral azotised matters, from which it is derived. Now, the beautiful researches of M. Wöhler have taught us, that urea may be produced by a modification of cyanate of ammonia, which, itself, consists of an oxide of cyanogen and an oxide of ammonium. Thus, there pass out of the animal four oxides, namely, carbonic acid, water, cyanic acid, and oxide of ammonium, the two latter, in their modified combination, constituting urea; so that it is by a true process of combustion that the azotised matter of the food is converted into urea."

This deduction, which the admirable researches of Liebig* and Wöhler, on the transformations of uric acid, first enabled us to draw, and which is developed with great clearness by Liebig, in his work on *Organic Chemistry in its Applications to Physiology*, is one of vast practical importance. For it is well known, and especially in regard to urinary ingredients, though equally true of the other excretions, that the exact chemical form in which these excrementitious principles shall appear, is liable to wide variation, and that their extrication in the blood in forms difficult of elimination, or otherwise noxious, is the source of a class of diseases which, from the present habits of society, is necessarily one of the most numerous we are called

* Who is there that does not enter into the feelings of these eminent men, in reading the following fervid words, in which Liebig gives an account of their discoveries ?—" For my own part," he says, " I confess that I felt my whole nervous system thrilling, as if pervaded by an electric current, when Wöhler and myself discovered that *uric acid* and all its products, by a simple supply of oxygen, became resolved into *carbonic acid* and *urea*, thus showing that there existed a connection between urea and uric acid, such as had never before been dreamed of in its infinite simplicity; when our calculation proved that *allantoin*, the nitrogenous constituent of the urine of the fœtus of the cow, contains the elements of uric acid and urea; and when we succeeded in producing allantoin, with all its properties, from uric acid. Though few words passed between us in these investigations, how often have I seen the eyes of my friend glisten with delight."

upon to treat. But, if it be true that these principles are products of oxidation, and the fact admits of no doubt, it is easy to determine, with sufficient exactness, the conditions on which these variations depend. Where the weight of the body remains nearly constant, they must essentially consist in the amount and quality of nutritive materials absorbed, on the one hand, and on the volume of oxygen inspired on the other—conditions which, in the shape of diet, air, and exercise, the practitioner has largely under his control The great efficacy of a judicious regulation of these is practically known to us all, and such regulation forms a large part of all rational medicine. But it may still further enhance the value of these means in our eyes, to form a clear idea of the direct and certain power they give us, in availing ourselves of the natural resources of the living economy, of cutting at the root of numerous grave disorders, by altering the very form of the material cause on which they depend. We look in vain for such a power in the action of drugs, which, therefore, provide no substitute for it.

These considerations naturally lead us to the researches of Mulder,* on the oxidation of proteine. The pith of these researches lies in a very small compass. This chemist finds that fibrine, on being boiled in water in the open air, gradually absorbs oxygen. The result of this is the formation of two new products, one of which is freely soluble in water, the other insoluble. These products differ from proteine, in the former containing three, and the latter two more equivalents of oxygen than this body. It was simply to express this relation, and without wishing to attach any other significance to the terms, that Mulder named these products, respectively, tri-oxy, and bi-oxy-proteine,—names, however, which seem very ill-chosen.

Bi-oxy-proteine on being further boiled, with exposure to air, becomes, in the end, wholly converted into tri-oxy-proteine by the absorption of another equivalent of oxygen.

By treating albumen in the same way as fibrine, the tri-oxy-proteine is formed at once, without passing through the intermediate stage.

Mulder also finds these products in minute quantity in normal

* *Journal für Praktische Chemie;* Erdmann and Marchand, Marz 18, 1844.

blood, but in much larger proportion in the buffy coat, and in recent false membranes. He even asserts that the buffy coat contains no true fibrine, but is entirely made up of a compound of these two bodies. Does it not seem from this extremely probable, that the colourless corpuscles which so abound in inflammatory blood, and about which Dr. Carpenter and Mr. Wharton Jones are now waging war, may be nothing more than this so-called bi-oxy-proteine.

According to Mulder, the substance which Bouchardat obtained from the buffy coat, and which that chemist mistook for gelatine, is, in fact, tri-oxy-proteine. In accordance with this view, it will be remembered that Bouchardat obtained this substance, in large proportion from the buffy coat, but only in very minute quantity from healthy blood. But Mulder has also ascertained, by ultimate analysis, that it is perfectly identical in composition with the tri-oxy-proteine, obtained by boiling fibrine in water in the open air.

From this series of facts the author considers himself entitled to draw the following conclusions:—

1. "That, on exposure to air, proteine suffers oxidation at the temperature of the living body.*

2. "That this action is constantly going on in the lungs, at the expense of the fibrine of the blood.

3. "That the proteine thus oxidised is the principal, if not the only bearer of oxygen to the rest of the economy; and in support of this he argues, that the hæmatosine of the blood, from its small mass, cannot possibly suffice to absorb the large volume of oxygen taken into the body.

4. "That inflamed blood contains a much larger amount of oxidised proteine than normal blood, and, in fact, that the normal and inflammatory conditions differ chiefly in this circumstance; from which the author infers, that every cause which accelerates the breathing and the circulation, tends to bring on an inflammatory condition; and hence the readiness with which inflammation is set up in various febrile disorders, &c."

This last statement, as to the existence of a larger quantity of

* Scheerer has shown, that at common temperatures moist fibrine absorbs oxygen largely.—*Annalen der Pharmacie*, Bd. xi., S. 13. See also his *Mikroscopische und Chemische Untersuchungen*.

oxidised proteine in inflammatory blood, accords well with the observations of Dr. Polli,* on the coagulation of blood, which may therefore be fitly introduced here. This physician finds that the presence of carbonic acid in blood always retards coagulation. The more carbonic acid the blood itself contains, the slower is the coagulation, and the greater the chance of a buffy coat being formed; and a buffy coat being formed, without froth and over a dark clot, is always a sign of the blood being surcharged with carbonic acid.†

Mulder further remarks, at the end of his paper, that the flesh we eat must become oxidised in cooking, and, therefore, that the chief part of the proteine compounds we take in must enter the body in a state of oxidation.

* *Gazetta Medica di Milano*, Aprile 15, 1843; and *Paget's Report on the Progress of Anatomy and Physiology* for 1842–43.

† Two other conditions co-operate largely in the result, namely, the increased proportion of fat in inflamed blood, and the tendency of the red corpuscles to adhere in rolls. The opinion that fat is one cause why the buffy crust swims on the blood, has lately been confirmed by Baumhauer, who found in the buffy coat from the blood of a horse :—

Fat, soluble in ether	3.276
Buffy coat, insoluble in ether	96.724

The following observations on the other point are taken from Mr. Paget's excellent "Report," *passim:*—

"*Buffy Coat.*—The exact mode of the formation of the buffy coat has been well illustrated by Mr. Wharton Jones, whose observations on the blood I can in nearly all points confirm. He ascribes it, as Nasse and others did, chiefly to the tendency of the blood corpuscles to arrange themselves rapidly in rolls, (like rolls of coins,) which form a wide-meshed network, (as seen in a single layer under the microscope,) or a kind of spongework when they are kept in mass. In the former case the liquor sanguinis coagulates within the meshes of the rows of the corpuscles, and hence the distinctly mottled aspect of the layer when coagulated, as well as when first drawn. In the latter case the spongework formed by the rows of corpuscles contracts and squeezes out the liquor sanguinis, and permits the greater specific gravity of the corpuscles to come into play, so that they sink quickly and the liquor sanguinis floats to the top and coagulates in a distinct layer. In both cases the pale corpuscles remain with the separated liquor sanguinis, and are imbedded in its white coagulum. The attraction for each other, by which the corpuscles tend to unite in rolls, is so remarkably increased in the state of the blood in which a buffy coat is formed, that the early or instantaneous occurrence of this arrangement of them, as seen by the microscope in a single drop of blood, affords all the evidence which could be derived from the formation of a buffy coat on a large quantity." And, further, quoting again from the same report:—"According to Mr. T. Wharton Jones, the congestion which succeeds to the temporary acceleration of the capillary circulation in an inflamed part, These investigations are undoubtedly very interesting, but, on the whole, perhaps the author attaches too much importance to them. Without the evidence they afford, it must have been clear to every one who had ever thought on the subject, that the proteine compounds undergo ready oxidation at the temperature of the living body, from the waste which these compounds suffer when the animal is deprived of food. Still more plainly does this appear in the carnivora, animals that subsist on food identical with the chief constituents of their own body; which last must, therefore, be incessantly furnishing fuel for the combustion by which their animal heat is maintained. Still, most physiologists will find it difficult to admit, without further investigation, that the fibrine of the blood is the chief bearer of oxygen to the tissues.

The observation that oxidised proteine is present in much larger quantity in inflamed than in healthy blood, is of value, were it only for its use in calling the attention of pathologists to the chemical phenomena of inflammation. Seeing the vast amount of chemical change that goes on in inflamed parts, the great destruction of old matter, and the formation of new products chemically different, it is surprising that these phenomena have so long been overlooked in the investigation. As the disturbance of the normal conditions of nutrition, by the introduction of a chemical agent, is the most frequent cause of inflammation, and the first step in the process, so an enlightened pathology will eventually show, that chemical agency plays, throughout, a fundamental part in the changes of which this process consists.*

is due to the red blood corpuscles adhering together, (in the manner already described,) and to the walls of the vessels till stagnation occurs; and he has shown that the same arrest of the blood takes place when capillaries are touched with a solution of salt, or when a stream of carbonic acid is directed against those of the frog's lung. From these last facts he suggests, with much probability, that the stoppage of the circulation in the capillaries when certain salts are added to the blood, and that which takes place in asphyxia, depend on a similar adhesion of the corpuscles. With regard to asphyxia, his observations agree in their tendency with those of Dr. John Reid on the stagnation of the blood, independent of any apparent mechanical hindrance, when nitrogen is inhaled; and the action of carbonic acid in making the corpuscles cohere in rolls and assume the most favourable condition for the formation of a buffy coat, gives additional probability to the observations already quoted from Dr. Polli."

* Scheerer has recently made some very valuable investigations in this direction, and has given the results in his *Mikroscopische und Chemische Untersuchungen*,— a very valuable work.

From the great amount of heat evolved in the seat of inflammation, and other equally significant circumstances, it might already be inferred, that increased oxidation is an important element of the morbid changes. All this was very clearly and explicitly developed by Liebig, in his chapter entitled "Theory of Disease."* Mulder's observations on the buffy coat are chiefly of value in substantiating this view, by precise and authentic evidence.

To state, however, as Mulder does, that the formation of a larger proportion of oxidised proteine in blood constitutes the only difference between the inflammatory and the normal conditions, is, I need scarcely remark, a very one-sided and limited view of the subject.

The next paper that falls under our notice is one by MM. Dumas, Boussingault, and Payen, on "The Fattening of Cattle and the Formation of Milk." The investigation, of which it communicates the results, was undertaken with a view to determine the source of the fatty matters which animals store away in their bodies, or furnish in the shape of butter.

The authors give, at the outset, a very full and fair statement of the grounds of the commonly received opinion, that animals form a part of their fat, at least, out of other principles. These gentlemen were, however, early led to believe that animals have no such power, but that they find their fatty, as well as their azotized materials, ready made for their use in plants. I need not give in detail the various considerations which first inclined them to this opinion, in spite of the great difficulties that stand in its way, and the numerous analogies in favour of the opposite doctrine. These considerations were principally founded on M. Payen's discovery of the great abundance, and almost universal diffusion of oleaginous principles in plants; on the fact, that certain oils taken in the food reappear unchanged in the milk; and, lastly, on Magendie's observations on the formation of chyle. This physiologist, in observing the phenomena of digestion in dogs, soon fonnd that this fluid varies greatly in constitution, in accordance with variations in the qualities of the food.

The chyle formed under the use of a vegetable diet rich in

* *Chemistry in its Applications to Physiology and Pathology.*

starch or in sugar, (principles out of which it is commonly supposed animals form a great part of their fat,) as well as that which results from the digestion of lean flesh, is very poor in globules. Such chyle is translucent, serous, and gives up to ether a very small amount of fatty matter.

These facts certainly furnish, as far as they go, a strong presumption in favour of the view taken by our authors. But they lay still greater stress on certain facts touching the conditions which affect the formation and nature of butter. "If one-half of a cow's allowance of hay be replaced by an equivalent of turnip cake, still rich in oil, the beast keeps up its condition, but the milk furnishes a more fluid butter, and this butter possesses, to an intolerable degree, the savour peculiar to the oil of turnip." "What," the authors ask, "can be opposed to this direct observation, and how not conclude from it, that the fatty matters of the food pass into the milk, often little or not at all altered, in order to form butter!"*

To determine between these rival opinions, as to the source of fat in animals, was a matter of no mean importance. The question involves a cardinal point in animal chemistry:—nothing less, in fact, than to ascertain in what degree animals also have

* I need not point out the great value of the practical application of these observations to the quality of woman's milk, which is, undoubtedly, liable to equal variation under the influence of similar conditions. The judicious regulation of these, by proper modifications in the food of the nurse, furnishes us with the only means we have in our power, (but, fortunately, they are direct and powerful,) of making changes in the natural food of the infant at the breast. All good practitioners are, more or less, in the habit of availing themselves of them; but there can be no doubt that many disorders of the sucking child are still treated by nauseous and irritating drugs, which should find their natural and appropriate remedy in the judicious regulation of the diet of the mother. To what an extent this may be brought to bear on the nature of the milk, will appear from the fact cited by the authors of this memoir—that the butter of the cows of a single locality may vary to such a point, according as they are fed on green forage, or on dry fodder, as to contain, as in the case of the butter of the Vosges, for instance, from 66 of margarine to 100 of oleine in summer, up to 186 of margarine to 100 of oleine in winter. In the first case the cows graze on the mountain side; in the second, they feed in the stable on dry fodder; for the rest, every one must have remarked the more solid state of butter in winter, and that quite independently of the lower temperature of the season. Another interesting observation to be found in this memoir is, that the milk of cows fed chiefly on carrots approaches very nearly in quality to that of women, a fact which, it appears, is taken advantage of in France, for the nourishment of infants deprived of their mothers' breast.

the power, by a process entirely different from the oxidation by which their principal chemical products are extricated, to form new principles out of the elements of their food; to what extent, in short, they share in that constructive power which reaches so high a point in the plant, and which modern chemistry has shown to be the great characteristic of it.

When we reflect, moreover, on the large proportion in which fat is found as a constituent of nervous matter; on its singular proneness to accumulate in some individuals, and the no less difficulty of its assimilation by others; and, lastly, on its vast importance as a store for the maintenance of animal heat in time of need; we shall gain some idea of the great interest which the ultimate decision of this question must have, both for the physiologist and the practitioner.*

It must be confessed that M. Dumas' solution of it has the merit of simplicity, and derives much plausibility from Magendie's observations on the chyle, which have already been quoted; insomuch that, the chief obstacle in the way of its general acceptance would, at first sight, seem to be the difficulty of supposing that the herbivora find in their food all the fat deposited in their tissues or furnished in their milk. The production of butter by the cow offered an opportunity of putting this to the test of experiment, and our authors, therefore, undertook to determine by exact analysis, the ratio of the fatty matter contained in the daily allowance of certain cows submitted to the investigation, to the amount of butter furnished in the milk of the same animals. The results fully confirmed their anticipations. The experiments on which they were founded, and which were conducted on a large scale and with great accuracy, form the basis of the memoir now under consideration. My space does not allow me to give more than their general results. These sufficiently establish the following conclusions, which I give in nearly the words of the memoir:—

* An important remark, in connection with the uses of fat, has reference to its insolubility in water. It is this quality which especially fits fatty matters for forming a magazine of combustible matter in herbivorous animals; for as these animals, like all others, have to produce in the day a certain amount of heat, they undoubtedly effect this at the expense of the soluble (saccharine and starchy) products their blood contains, before attacking the insoluble compounds, such as the neutral fatty matters, which the chyle pours into it without ceasing.

1. That hay contains more fatty matter than the milk it serves to form; and that the same is the case with the other kinds of food on which cows and asses are kept.

2. That oil-cake increases the production of butter, but sometimes renders it more fluid, and may also give to it the taste of oil of grains, when this article enters too largely into the daily ration.

3. That the very fattening quality of maize is a result of the large quantity of oil it contains.

4. That there exists the most complete analogy between the production of milk and the fattening of cattle, as graziers had already surmised.

5. That the grazing ox, however, appropriates for man's use less, both of fatty and azotized matter, than the milch cow; and that the latter, therefore, in an economical point of view, is greatly to be preferred, if the object be to convert a pasture into products useful to man.

6. That the potato, beet, and carrot fatten only when associated with other products containing fatty matters, as straw, to wit, or bran and oil-cake.

7. That, for equal weights, gluten mixed with starch, and flesh rich in fat, have, for the pig, a fattening power which differs in the ratio of 1 : 2.

In conclusion, the authors argue that these results fully bear out their doctrine, that animals derive their fatty matters, ready made, from plants; and that the old opinion, that they form these matters anew out of other principles, remains a gratuitous supposition without foundation in facts.*

It will at once be seen, however, that these results do not sufficiently warrant such a conclusion, and that they by no means definitively settle the great question touching the origin of fat in

* I regret that I have not space for the many important observations with which this memoir is interspersed. I cannot forbear, however, from quoting one which I am sure will interest those country practitioners who avail themselves of the delightful privilege their position gives them, of finding relief from the cares and anxieties of their profession in the more serene occupations of farming. The fact to which I allude is this, that it appears, as the result of unimpeachable experiments, that the milch cow appropriates, for man's profit, a quantity of alimentary matter, which may more than double that withdrawn by the grazing ox from the same pasture; whence it follows, as the authors well remark, that whatever may tend to place the milk market on a footing entitled to public confidence is, in the highest degree, worthy of an intelligent administration.

animals generally. In the first place, they do not prove *directly* that fat is never formed anew by animals, but merely that such a supposition is, in the case of certain species, unnecessary for the explanation of the facts; and although even this, taken in connection with Magendie's observations, gives some plausibility to the doctrine as far as the herbivora which inhabit the land are concerned, yet this is a very limited field of inquiry; and in order to establish the general proposition, other cases have to be considered. The inhabitants of the ocean, also, accumulate oil and fat in enormous masses. Do these, too, find these matters ready formed in their food? I need only instance the cetacea, to show the extent of the difficulty which this question offers; for although, as Dumas remarks in reply to Liebig, these are not herbivorous, but carnivorous animals, this is only shifting the difficulty, and not removing it. The question in this case can only be solved by an investigation of the same purport as that of which an analysis has just been given. For obvious reasons, the conditions of the problem can never here be determined with the same exactness, but approximate results of much interest might be obtained by ascertaining the amount of fatty matter which those marine plants contain which furnish food to fish.*

Other instances of great difficulty were not slow to occur to the minds of the authors themselves, to shake their faith in the conclusions they had drawn from their first investigations. One of a very special kind, but bearing closely on the general question, is the formation of wax by bees. Huber having ascertained that bees fed on sugar still continue to make wax, was naturally led to infer that these insects have the faculty of forming the latter at the expense of the former. Our authors felt, nevertheless, that this conclusion was not unimpeachable, since in Huber's experiment no account was taken of the fat pre-existing in the bodies of the bees, and which evidently might have been the source of the wax they continued to make. They therefore resolved to examine the question

* Probably, after all, as already surmised by M. Dumas, the solution of the difficulty will be found in the remarkable property, first discovered by Count Rumford, and since more accurately investigated by M. Morren, and more lately still by Wöhler and Pfankuch, which certain infusoria possess of decomposing carbonic acid, after the manner of plants under the influence of light; a property which many considerations render probable will eventually be found of the greatest importance in the economy of the ocean.

anew, under a method of investigation in which this, and all the other conditions of the problem, should be accurately determined by the balance. The inquiry was entrusted to MM. Dumas and Milne Edwards, so pre-eminently qualified by their respective pursuits to join in the task.

The method of investigation adopted was the following:—A given number of bees were secluded in a glass hive and fed exclusively on honey. Before the experiment was begun, a large number of working bees from the same hive were analysed, in order to ascertain the amount of fat the body of each contained. The honey on which they were fed was accurately weighed, and its content in wax exactly determined. Finally, at the end of the experiment, which lasted from the 7th of July to the 8th of August, a certain number of the bees were again analysed for their fat, and the wax produced was accurately weighed. The results obtained are expressed in the following figures:—

	Grammes.
The fatty matter pre-existing in the body of each bee was estimated at ..	0.0018
That furnished to each bee in the course of the experiment did not exceed ..	0.0004
Fatty matter, which could be ultimately traced to aliments, did not amount, therefore, in each bee, to	0.0022
Whereas, in the course of the experiment, each produced in wax	0.0064
And after having furnished this abundant secretion, each contained in its body, whether in wax or in common fat	0.0042
Making a total of	0.0106

So that the wax produced *alone* nearly tripled the amount of fatty matter pre-existing in the bodies of the bees, and that contained in their food, together; thus showing that a vast proportion of this wax must have been formed at the expense of the saccharine matter of the honey, and confirming in the most complete way Huber's original and beautiful observation.

This admirable investigation is deeply interesting in many points of view, but in none more, perhaps, than in the beautiful evidence it gives of the competence of chemistry, in all that concerns the movement of matter through the living economy, to deal with the highest questions of physiology, and in the exemplification it affords of the unimpeachable authority the balance gives to observations in this science.

In the discussion that ensued on the reading of this memoir, M. Milne Edwards argued that the results of this inquiry in no wise invalidate the inferences drawn from the former investigation on the fattening of animals, since the wax, the product here treated of, is a *secretion,* and is formed, therefore, under quite different physiological conditions. But the chemical fact does not the less remain the same; to wit, that in the bodies of animals fatty principles may be formed by a tranformation of sugar. The claims of wax to be considered as one of this family of products, have been completely established by the researches of M. Lewy,* proving the ready conversion of this substance into stearic and margaric acids; and, again, by those of M. Gerhard, which show that it furnishes, with nitric acid, exactly the same products as other fatty matters. That such transformation of sugar is not a peculiarity of bees, but is shared with them by vegetable feeders generally, so that these likewise form fat out of the saccharine elements of their food, is rendered all but certain by other considerations. A very interesting fact, showing the facility with which this chemical change takes place, has recently been discovered by MM. Pelouze and Gelis. These chemists found that, "If, instead of yeast, a small quantity of white cheese-curd be added to a solution of common sugar, and kept at a temperature of 76° to 80°, and, moreover, some chalk be added to maintain the fluid in a neutral state, a lively evolution of gas takes place; the sugar totally disappears, carbonic acid and hydrogen are given off in the gaseous state, and we find in the fluid a copious amount of butyric acid—one of the most interesting of the organic acids, which until recently was known only as a constituent of milk and butter."†

In connection with the physiological bearing of this experiment, MM. Pelouze and Gelis think it important to remark, "that this change of sugar into butyric acid takes place without any considerable increase of temperature, and without the presence of those energetic substances which could destroy the equilibrium, or affect the vitality of animal tissues, but that this transformation is

* *Journal für Praktische Chemie:* Erdmann and Marchand. See for another paper, by the same author, on the "Analysis of a Particular Wax of Vegetable Origin," No. 1, 1844, of the same Journal.

† *Lancet,* May 12, 1844.

effected under very simple conditions, and in substances employed by nature herself."

With evidence of such various kind, thus pressing upon us from all quarters, and all tending to one point, it becomes impossible to resist the conclusion that animals have the faculty of forming fat out of the saccharine elements of their food, as Liebig has all along held. It is no small testimony to the sagacity of this distinguished chemist, that the first complete and rigorous demonstration of the fact, (in the instance of bees,) should have been furnished by the very men who had done so much to render it probable that animals have no such power. Still, the original paper by Dumas and Boussingault on the fattening of cattle, and formation of milk, will ever be highly interesting from its great intrinsic value; and, for this reason, I have given an abstract of it a place in this Report, although the chief conclusion it was intended to establish is now set aside.

For the rest, all analogy concurs to show, as M. Dumas has so well remarked, that the conversion of sugar into fat in the living body most probably occurs in the *primæ viæ*, by a special fermentation, and not in the blood. The important remark, that the saccharine element of the milk of the *carnivora* must be formed by a process, which is probably exactly the reverse of this, namely, the conversion of fat into sugar of milk, appears to have entirely escaped the attention of physiologists. This product cannot be derived as such from the food of these animals, which contains no element of the kind, while many analogies render it probable that the fat of their bodies is its true source.* The subject is well worthy of investigation from its close bearing on the pathology of that most interesting and obscure disease, diabetes.

From these investigations into the nature and source of the constituents of animal bodies, furnished to them in their food, I pass on to the process of digestion by which they are prepared for absorption into the blood. During the past year many valuable contributions have been made to our knowledge of this function. The foremost in rank and importance is an elaborate work by M. Blondlot, entitled *Traité Analytique de la Digestion*. The method of investigation adopted by the author is one which, as the gift of

* Dr. Prout would probably say "gelatine."

accident, has already been fruitful of valuable results. Taking the hint from the now celebrated case of St. Martin, M. Blondlot conceived the idea of forming, by operation, fistulous openings in the stomachs of dogs. After many fruitless attempts he completely succeeded in two young dogs, and made the opening permanent in each, by fixing a silver canula in it. One of these dogs is still alive and in his possession, and although for two years past M. Blondlot has continued to make use of the fistula for procuring gastric juice and chyme, and for introducing thermometers, tubes, and sounds into the stomach, the dog does not the less remain in perfect health. Not only has it completed its growth, but it has grown fat, lively and alert, and enjoys an excellent appetite.

With these opportunities, alike favourable for observation and experiment, M. Blondlot continued his researches on the phenomena of digestion, for a long period. It will greatly simplify my task of giving an abstract of them, if I state at once that one of the chief conclusions to which they have led him is, that neither the saliva nor the bile take any essential part, whether in the process of digestion or in that of chylification.

The chief office of the saliva is, he asserts, mechanical, and intended merely to facilitate the acts of mastication and swallowing. He has found, by repeated experiment, that in the digestion of whatever kind of food, in gastric juice out of the body, the addition of saliva never accelerates the process in the slightest degree. These statements are in entire accordance with the observations of Berzelius and Dr. Beaumont; and although so widely at variance with the more recent and elaborate researches of Dr. Wright, there are many and weighty reasons for believing that they come very near the truth. At least, they derive very powerful support from comparative anatomy; for, in the gradual simplification of the digestive apparatus in the scale of animals, the salivary glands are the first to disappear, and that, too, in animals in which the digestive function is singularly active. I cannot but think that this fact alone offers a strong reason for believing that the last-named physiologist has erred in attributing to saliva such a very essential part in digestion. M. Blondlot further denies Liebig's assertion, that the chief office of this fluid is, (in virtue of its tendency to inclose air in the shape of froth,) to introduce air into the stomach; "for," he remarks, "if an animal be suddenly killed during digestion, and the stomach examined, its walls will be found closely applied to the alimentary mass, in which it is impossible to discover any gaseous mixture." M. Blondlot has been unable to detect sulpho-cyanogen in saliva, and is of opinion that the substance named ptyaline* is not peculiar to saliva, but common to all mucous fluids.

In considering the uses of the bile, he discusses, one after another, the principal hypotheses which have been made touching the part it plays in chylification. He deals, first, with the office ascribed to it of neutralising the acid of the chyme, and inquires how the very small quantity of alkali this fluid contains can suffice for the purpose, since M. Thenard has proved that a few drops of vinegar are enough to neutralise all the bile found in the gall-bladder of an ox. He adds that the matters poured from the stomach into the duodenum retain their acidity after their mixture with the bile, to nearly the same degree as before, and only lose it as they travel along the intestine; and finally remarks, that if the bile were meant to neutralise the acid of the gastric juice, how should it happen that in some animals it is poured directly into the stomach, in such manner as must, under this supposition, destroy the chemical properties of the special fluid this viscus secretes, before chymification is completed. That the alkali of the bile assists in dissolving the fatty matters of the food, he considers as still less tenable, since what little this fluid contains must be already neutralised by the acid of the chyme. Nor does the author concede any office of the kind to the biliary matter itself, which has been thought to render fat more fit for absorption by forming an emulsion with it. To establish these opinions on sure grounds, he placed ligatures on the common biliary duct in several dogs. The intestines being now cleared out by a dose of croton oil, various aliments, such as bread and milk, flesh, and cheese, were eaten by the dog in each experiment with some eagerness. At the end of five or six hours, the animal being killed and immediately opened, M. Blondlot invariably found, in direct opposition to the observations of Brodie, and Tiedemann and Gmelin, a white and

* These two last statements throw some little discredit over the chemical part of M. Blondlot's investigations. Saliva certainly does contain sulpho-cyanogen; and the fact that this fluid has the power of converting starch into sugar, while other mucous fluids have not, is strong presumptive evidence in favour of ptyaline being, to some extent, a peculiar principle.—See Dr. Wright's papers on "Saliva," in the *Lancet*, *passim*.

well-elaborated chyle in the thoracic duct and in the lacteals of the mesentery.

All these questions are now in the way of being finally settled by the very ingenious experiments of Schwann, of which I shall have to give an account presently.

The pancreatic fluid, M. Blondlot is inclined to regard as chiefly intended to lessen the acrimony of the bile by dilution; a view which, I need scarcely remark, is extremely unphilosophical, and entirely without sanction in anything we know of the animal economy.

The saliva, bile, and pancreatic juice being thus disposed of, the investigation reduced itself in the mind of the author, to an exact determination of the effect of the gastric juice on the various aliments on which animals feed.

A great number of interesting experiments were made with this object, for the detail of which the reader is referred to the work itself. It will be sufficient here to state, that their general result was to lead M. Blondlot to divide the matters of which aliments are made up, into three separate categories, according to the nature of the modification they severally undergo.

The first comprises those substances which quit the stomach in the same form in which they entered it: such is the case with mucus, whether solid or liquid; with resins, woody fibre, and fecula. According to the author, not only do these matters traverse the stomach unaltered, but the whole course of the intestines also; and without giving to the system any nutritive principle. It is important to understand that by the word "fecula" is here meant starch in its raw state, and as it exists in the form of minute globules, now well known as objects of singular beauty when viewed through the microscope by polarised light. These globules may be discovered in great abundance in the *excrements* of many herbivora that eat largely of amylaceous food. The ruminants, however, and graminivorous birds seem to have the power of crushing them, and the true starch thus becoming liberated in a soluble state—a change effected under other circumstances by heat*—is readily absorbed.

* This fact renders it probable that great advantage would accrue from scalding the oats and other amylaceous parts of the food of horses that admit of such treatment. The globules would thus be made to burst, and the starch, consequently, would be liberated in a soluble form, fit for absorption.

In this condition it passes into M. Blondlot's second category, which includes all substances that dissolve in gastric juice, exactly as in pure water. To this group belong liquid albumen, pectine, sugar, gum, and starch, in the form just spoken of; so that, for these substances, chymification is in reality nothing more than simple solution, in the utmost rigour of the term.

This statement is, however, in several important points, at variance with the opinions of many eminent chemists.

Thus, M. Dumas, reasoning probably from the extreme readiness with which sugar passes into lactic acid under the influence of animal membranes, and from the inevitable conversion of this acid, in common with organic acids in general, into carbonic acid by the action of respiration, should it find admission into the blood, was led to believe that all saccharine aliments undergo lactic fermentation in the stomach,* in order to become better fitted for the purposes of combustion. One does not see, however, what should prevent sugar from sharing what would appear to be the necessary fate of all soluble matters, in being absorbed without change; and it will be seen from the researches of Dr. Percy, that under certain conditions, and perhaps generally, it does enter the blood unaltered.

As far as the production of lactic acid is concerned, M. Blondlot's statements are decided and unequivocal:—"I can affirm," he says, "that having frequently analysed the liquid pressed out of aliments that had been some time in the stomach, I have never found in it *the slightest trace* of lactic acid;" a statement which derives additional weight from its entire accordance with the experience of Dr. Prout on the same point.

In regard to the question of the conversion of starch into sugar during digestion, however, these two observers are in direct opposition. Dr. Prout holds that the *reduction*, as he terms it, of all the forms of the saccharine principle, (meaning starch and its equivalents,) is accompanied by the development of a *low* sugar; and the great frequency of such a transformation, in a variety of circumstances, under the influence of agencies nearly related in their character to those which play an important part in digestion, is a strong *a priori* argument in favour of the view. M. Dumas also was, until recently, of the same opinion.

* This notion has since met its final overthrow at the hands of Professor Liebig.

M. Blondlot, on the other hand, denies the fact in the most unequivocal terms; and after remarking that MM. Sandras and Bouchardat have positively ascertained, by means of M. Biot's delicate instrument, that sugar is never formed in the stomach in the healthy state, he adds for himself, that he has sought, in vain, for this principle in starch that had been submitted for many hours to the action of gastric juice, whether within the stomach or at an artificial temperature without. The author, however, actually records but one experiment of the kind, and that one cannot be considered decisive, as it appears to have been made on fecula, or raw starch, a form in which, as we have already seen, the true starch is, in a great measure, protected from change by the capsule in which it is inclosed. This is not the state in which it is generally present in the stomach of man and animals; for here it is, for the most part, found liberated in a soluble form, whether by trituration or previous cooking.

I do not know whether or not the same objection applies to the investigations of MM. Sandras and Bouchardat, but it is much to be regretted that M. Blondlot did not avail himself of his unlimited opportunities to make experiments on a larger scale, and on starch in a state of solution. This is a question which it is highly desirable to have finally settled, since, besides involving a cardinal point in the theory of digestion, it derives additional and peculiar interest from its intimate connection with the pathology of diabetes.

I now pass on to M. Blondlot's third and last category. This comprises those substances, on which the solvent action of the gastric juice is as nothing, or nearly so, but which, under its influence, lose their cohesion, and are broken down into a molecular pulp. On some of these this effect appears to be produced merely by the *acid* of the gastric juice, and has nothing special in it. Such are the fleshy parts of fruits and succulent roots. For the others the change is of a special kind, and consists, in a peculiar disintegration of the aliment, by the gastric juice acting in its specific capacity. Among these substances he numbers fibrine, animal and vegetable albumen in the concrete state, caseine hardened by heat, matters that furnish gelatine, and, finally, gelatine itself—in short, putting out the last, the great group of proteine compounds, out of which the blood and tissues are made.

The most startling part of this statement is, that, according to the author, and in direct contradiction to the inference to which MM. Sandras and Bouchardat have been led by their recent researches, as well as to the opinion of chemists in general, these substances are not *dissolved* by the gastric juice, but merely converted into a pulp of extreme fineness. On this head his statements are so clear and explicit that I will venture to give them in nearly his own words.

"Another point," he says, "which it is important to establish well is, that the gastric juice does not really dissolve any of these substances, or at least that the solvent action it exerts upon them is all but insignificant. In fact I have ascertained, by numerous experiments, that fibrine, coagulated albumen and caseine, hardened by heat, are not dissolved by gastric juice in greater quantity than in water acidulated to the same degree by phosphoric or any other acid; and we all know that, diluted to this point, acids, of whatever kind, scarcely attack these matters at all. We have seen also that, after prolonged contact with these matters, at a temperature of 40° *centigr.*, the gastric juice gives with nitric acid and with bi-chloride of mercury, a precipitate so scanty in comparison with what remains undissolved, as to be not worthy of any consideration. When its action is complete the larger undissolved portion appears broken up into particles of extreme minuteness, which occupy the bottom of the vessel, and which, when shaken up, give to the liquid the look of an emulsion. If, in this state, a drop be put on a slip of glass and examined under the microscope, an immense number of irregular particles of various sizes are seen. The smallest are scarcely as large as blood-globules, the largest much larger than these." M. Blondlot elsewhere remarks that the whole has the closest resemblance to what Mr. Gulliver has named the *molecular base* of the chyle, as, in fact, these are, according to the author's view, identical matters.

This doctrine, that the proteine compounds are not dissolved in digestion, is however one that we must be cautious of adopting, and the more so as, according to M. Bouchardat's observations, even dilute muriatic acid, of the same degree of acidity as gastric juice, does, after a time, completely dissolve muscular fibre.

Touching the absorption of alimentary matters, M. Blondlot is of opinion that the undissolved matters, which, according to him, comprise fat and proteine compounds, enter the system through the lacteals, while those held in solution, such as sugar and its equivalents, are taken up by the veins. As far as the general doctrine is concerned, he is probably right in this, since these are, undoubtedly, the channels through which the dissolved and the undissolved products of digestion severally enter the economy.

But the most startling statement in M. Blondlot's book is the assertion, in direct contradiction to Prout, Liebig, Gmelin, and a host of other eminent observers, that the gastric juice contains no muriatic acid. The experiments on which this assertion is founded, if exactly performed and rightly interpreted, would certainly be conclusive as to the fact; but there, surely, must be some fallacy here. It is difficult to admit on the faith of the observations of a single individual, however precise these observations may seem to be, that so many eminent chemists have been mistaken as to a fact so easy of determination. If it were really true that the gastric juice contains no muriatic acid, what a tissue of fine theories on the uses of common salt in the economy—giving, by decomposition, muriatic acid to the stomach, and soda to the bile, &c.—would at once vanish into air.

According to M. Blondlot, the true acid of the gastric juice is phosphoric acid; not free, however, but in combination, as an acid phosphate of lime. His inquiries as to the precise part which the different constituents of the gastric juice severally play in the digestive process, offer nothing sufficiently novel to require quotation or comment. As far as they go they confirm, in all fundamental points, the results arrived at by other physiologists. The following observations on agents that promote the secretion of gastric juice are of more interest, and may not be without practical value.

After speaking of the effect of mechanical irritation, he says:—"Chemical agents applied to the surface of the stomach, when its internal tunic has become turgid under the influence of food, have still more marked effects. Thus it has often happened to me, to render the flow of gastric juice much more rapid and abundant, in rolling the morsels of flesh I gave to my dogs in powdered pepper, in *sugar*, salt, magnesia, (usta,) carbonate of potash, &c. Daily observation shows that these substances favour digestion, and everything inclines us to believe that they do so in increasing the secretion of gastric juice, just as they do that of the saliva, pancreatic juice, and bile, when placed in relation with the excreting ducts of the glands which furnish these fluids."

Another French physiologist, M. Cl. Bernard, has since taken up the subject of digestion in a different way. His first object was to determine the exact amount and nature of the influence which the gastric juice has in the process. With this view he adopted the singular plan of injecting various aliments into the blood, now alone, and now mixed with gastric juice, and watching the difference in the result.* It is obvious however, that experiments made in such violation of all the conditions of nature, cannot lead to any profitable conclusion. In evidence of that indifference to the labours of the physiologists of other countries, which has become characteristic of his countrymen, M. Bernard shows his complete ignorance of many important investigations bearing on his subject, which have lately been undertaken in England and Germany. Had he been acquainted with the interesting researches of my friend, Dr. Percy, he would have seen at once that the elimination of sugar through the kidney, when this substance is injected into the blood, by no means warrants the conclusion he has attempted to draw from it.

Of Dr. Percy's researches, to which I now pass, the following are the principal results:—

1. When grape sugar is present in the blood in a certain quantity, a portion of it is speedily eliminated by the kidneys, and may be found in the urine.

2. When grape sugar is present in the blood only in small quantity, it does not pass into the urine in an appreciable degree. In this case it probably undergoes oxidation in the lungs.

3. When cane sugar is present in the blood in certain quantity, a portion of it passes into the urine as *cane* sugar; it does not appear to be converted, in the smallest proportion, into *grape* sugar during its passage through the blood.

4. When cane sugar is present in the blood in large quantity, it exerts a powerfully diuretic action, and the urine evacuated appears to be principally a solution of sugar.

5. When grape sugar is introduced into the stomach under conditions favourable to absorption, a portion of it is rapidly absorbed and passes into the urine.

* *Archives Générales de Médecine*, Mars, 1844.

6. When a dog is fed upon cane sugar and water, a portion of the sugar may be found in the urine.*

I need not point out the various interesting applications of these results to the pathology of diabetes, for whose especial elucidation they were undertaken.

Studying the phenomena of digestion from another point of view, MM. Gruby and Delafond appear to have made the curious discovery, that infusoria are developed in great numbers in the stomach and intestines of various animals. Four species are found in ruminants, seven in the horse, two in the dog, but only one in the pig. In the sheep they so abound, that the authors estimate their weight at one-fifth of that of the liquid in which they swim: they exist in enormous numbers in the two first stomachs, but in the third and fourth their skeletons only are found. In the intestinal canal of the horse, however, they are found alive in the narrow colon, and even as low as the rectum. From these circumstances, M. Gruby is led to infer that they perform an important office in the digestion of ruminants and other herbivorous animals, in converting their vegetable food into animal products, so as to render it more fit for their general nutrition.

Chemically speaking, however, there is no object to be gained in this; and it is, moreover, only throwing the difficulty on the infusoria, if difficulty there be. At all events, it cannot be admitted that the agency of these minute creatures has any essential physiological relation to the function of digestion, as M. Gruby would seem to believe.

To complete my account of this particular province of physiology, it will be necessary to advert to some recent investigations into the composition and uses of bile. A monograph on the subject has lately been published by M. Bouisson, which, although it does not contain much original matter, gives a very good epitome of our present knowledge upon it. The author begins with an account of the microscopic appearances of bile.

Healthy bile, if not concentrated, merely gives a yellow stain to the glass on which it is laid, and usually offers no definite objects. Sometimes there may be seen epithelial scales from the

* *Medical Gazette*, vol. xxxii., p. 124.

mucous membrane of the gall-bladder and duct, small crystals of cholesterine, and, if the bile be concentrated, irregular solid particles of biliary matter.

M. Bouisson next gives an historical account of the opinions that have prevailed at different times as to the composition of bile. He seems to have made no researches of his own on the point; and the conclusion he draws from a comparative study of the investigations of others, is, that the essential ingredient is a watery solution of a peculiar soap.*

He ascribes great influence to the bile in digestion, and holds that, as it travels along the bowel, by far the greater part of it is again taken into the system. He also considers that it promotes the formation and absorption of chyle in these several ways:—

1. By stimulating the intestine, and by preventing the decomposition of chyme.
2. By dissolving fatty matters, and thus favouring their absorption.
3. By taking part in the conversion of aliments, and especially in the formation of albumen out of other matters.†
4. By neutralising the acid of the chyme.

In his chapter on the differences of bile, according to age, sex, &c., the only part worth particular notice is that on the bile of the fœtus, which, according to the author, differs from the bile of the adult chiefly in the smaller proportion of choleate of soda, and corresponding predominance of colouring matter. The remaining portion of the work is chiefly pathological.

From this brief sketch it will be seen how widely M. Bouisson's opinions are at variance with those of M. Blondlot, as to the part which bile plays in chylification. There can be no doubt that the former physiologist greatly overrates its influence in this process. It may be remarked, in particular, that the recent investigations of chemists leave no foundation whatever for the opinion which M. Bouisson has adopted from Prout, that bile helps to form albumen out of other matters.

All these questions seem now to be in a fair way of being cleared up by the ingenious experiments of Dr. Th. Schwann, of

* See "Demarçay's Papers."

† Prout.

Löwen.* These experiments, which were made on dogs—those martyrs to experimental physiology—were performed in the following manner:—the common biliary duct was first tied, as in the investigations of Brodie, Blondlot, and others; but in order to prevent the poisonous effects which result from the detention of bile in the system, an artificial fistula was formed between the gall-bladder and walls of the belly, through which this fluid was allowed to escape. The experiment did not always succeed: many dogs died from the first effects of the operation, from peritonitis, and other causes. Some, however, suffered very little from it: in the course of a few days the wounds were perfectly healed, the artificial fistula was permanently established, and the animals had completely recovered from the effects of the operation, considered as such. The effects which followed were simply those which result from the absence of bile in the intestine, and from the waste of this fluid; effects which the professor had thus an opportunity of observing free from all complication.

The only object proposed in this, his first paper, was to determine how far the discharge of bile into the alimentary canal is necessary to life. It is clear, however, that these experiments cannot give a precise answer to this question, for the effects which followed were probably due quite as much, if not more, to the *waste* of bile as to its being shut off from the intestine. However this may be, the result was, that all the dogs experimented on gradually wasted away and died; and, what is very remarkable, died quite as soon as dogs starved to death by total privation of food. It is important however to notice, that for the first three or four days, many of them did not lose a single grain in weight; a result which cannot well be explained, unless by supposing that they ate more than before. But in the greater number there was very evident wasting as early as the third day, and this went on gradually up to their death, which was evidently the result of inanition. In two cases, however, there was a middle period in which the process of emaciation was for a time stayed. Thus, in the seventeenth experiment, after the first three days the dog gradually lost weight for three weeks, when it again began to gain, and continued to do so until it had nearly recovered its original weight; then followed another

* Versuche, um zu auszumitteln ob die Galle im Organismus eine für das Leben wesentliche Rolle spielt; *Müller's Archiv.*, heft ii., 1844.

period of wasting, which went on steadily until the dog died. In the fifth experiment similar alternations occurred. The author is of opinion that the only probable explanation of these anomalies is, that in the intermediate period the continuity of the duct was temporarily restored.

In some of the experiments the dogs were allowed to lick up the bile as it escaped on the floor, which they did very eagerly, but they did not the less continue to lose weight exactly as before.

The final result in all these experiments furnishes conclusive proof, if indeed proof were needed, that the bile is not a merely excrementitious fluid, but that, after its discharge into the intestines, it is destined to serve an ulterior purpose of essential importance to life. The measure of this is well given by the striking fact, which few, perhaps, would have anticipated, that when this fluid is allowed to run to waste in dogs, they die of inanition quite as soon as if starved to death by total privation of food. For the precise interpretation of this remarkable result we must await that section of Professor Schwann's researches, in which the part which bile performs in digestion and chylification is to be fully considered. Meanwhile, the observations made in his eighteenth experiment will be found of some interest in their bearing on this point. When the dog which was the subject of them was already at the point of death, Professor Schwann killed it by blood-letting, in order to procure its blood for analysis. He then made a careful examination of the body, and ascertained the following facts:—

"In the abdomen there were no traces of actual inflammation, and only a few partial adhesions. The gall-duct was so completely obliterated, that the hepatic duct passed by a continuous curve into the cystic duct, without showing any trace of the former insertion of the ductus choledochus communis. There was still some fat remaining about the mesentery.

"The lacteals of the mesentery were translucent and almost empty. The thoracic duct, within the chest, contained a moderate quantity of lymph of whitish colour, like milk diluted with water. This lymph coagulated in from ten to fifteen minutes, and, therefore, contained fibrine. Under the microscope there were found in it, besides lymph-corpuscles, a great number of fat-globules of all sizes, like those of milk, but some much larger than those usually found in that fluid. The stomach was filled with curdled milk—(the animal had been fed on milk the twenty-four hours preceding death.) The upper half of the small intestine, also, contained a milk-like substance, which was very fluid in the duodenum, but in the jejunum rather more consistent. The whole upper half of the intestine showed no trace of yellow colour."

These results certainly give some colour to M. Blondlot's opinion, that bile is of small importance in digestion; for the fluid here described as taken from the thoracic duct, had all the characteristics of well-elaborated chyle. But, however this may be, Professor Schwann's investigation, taken as a whole, gives strong support to Liebig's theory,—that the greater part of the bile is re-absorbed, and that its principal importance is as fuel for respiration. For numerous and well-authenticated cases have taught us, that the common gall-duct may be permanently obliterated in man, and yet life continue for many years; so that it seems fair to conclude, that the speedy death which followed Schwann's experiments on dogs, resulted not so much from detriment to digestion, as from the waste of bile as material for the use of the living economy. And since the bile which is re-absorbed never re-appears in any of the other secretions, its carbon and hydrogen must evidently pass off in combination with oxygen, as carbonic acid and water. That a great part of the bile is re-absorbed in the higher animals, might almost be inferred, without further investigation, from the enormous size of their livers; for the amount of solid matter secreted by glands, bears some general proportion to their size, and it surely cannot be supposed that the very small quantity of biliary matter voided daily, can represent more than a small proportion of the secretion of such a vast organ. I have no doubt that when we shall have succeeded in ascertaining the exact amount of bile secreted daily, it will be found far to exceed all our present estimates of it.

In the same number of *Müller's Archiv* which contains Schwann's investigations, is a communication by Dr. Platner,* of Heidelberg, stating, that he has succeeded in obtaining the electro-negative body, which is supposed to be the essential constituent of bile, in a state of crystallisation, both pure and in combination with

* "Krystallisation der Gallensäure und des gallensauren Natrons," beobachtet von Dr. E. A. Platner, Privatdocent in Heidelberg.

soda, as it is supposed to exist in bile itself. This, if confirmed, would be, I need scarcely remark, a result of considerable importance, since it would authenticate the views of Liebig, Demarçay, and Kemp, on the constitution of bile, by exactly that kind of evidence of which they now stand in need; and would for ever do away with the old notions as to the very composite character of this secretion.

Dr. Platner gives in detail the processes by which he obtains these products. Before, however, his results can be finally admitted, it will be necessary for him to prove, by ultimate analysis, the identity of his crystalline body with the electro-negative body of M. Demarçay. This he has not hitherto done.

Having now gone through the history of the *raw material* by which animal life is fed, as also that of the process by which it is prepared for use, I pass at once to consider the dead chemical forms in which it is again thrown off, after having played its part in the living economy.

Two principal organs, the lungs and the kidney, are charged with throwing off the effete material: by the former it is cast off in the shape of carbonic acid and water; by the latter in that of urea and other oxidised products.

Within the past year Scharling* has published an extensive series of researches on the elimination of carbonic acid. He found, as might have been anticipated, that the amount evolved in a given time is liable to wide variations in the same individual; that it increases rapidly immediately after taking food, so as to reach its highest point soon after the principal meal of the day; that it sinks very low in the state of hunger; but that in sleep it reaches a lower point still. In one case, for example, the quantities evolved in the same space of time,—first, about an hour and a half after dinner; next, when the individual was very hungry, immediately before breakfast; and, lastly, when sleeping,—were represented by the figures 165, 130, 100. These results are scarcely worth the pains they cost, since our present precise knowledge of the chemical relations of food to respiration, would have allowed us to predict them with the utmost certainty. To the chemist they offer no new fact; and the only use they can have is in showing those

* *Valentin's Repertorium*, Jahrgang, 1843, p. 345.

whose studies do not enable them to appreciate other evidence, what reliance may be placed on the interpretations of chemistry in their application to the phenomena of animal life.

The very low point which the exhalation of carbonic acid reaches in sleep, is the combined result of the diminished activity of respiration and circulation during that state. In sleep, as is well known, the pulse falls, and the breathing becomes limited both in rate and amplitude; need must be, therefore, that the exhalation of carbonic acid diminishes in proportion. The knowledge of this fact furnishes us with an important rule of life for persons exposed to the various disorders that result from repletion; namely, never to indulge in sleep soon after a meal. To limit the action of respiration at the very moment when the materials that feed this process are being poured into the blood in large quantity, must, by protecting excrementitious matters from oxidation, inevitably tend to an accumulation of bile and lithic acid in the system. This explains the peculiar dangers of suppers in disorders of the class now alluded to, and shows how a single excess of this kind may, as practitioners have often an opportunity of observing, bring on a fit of gout or a bilious attack in persons predisposed

Of researches on the urine, by far the most important are those published in the *Lancet* by Professor Liebig. As these researches are not only the latest, but also supersede our former knowledge by new discoveries, they are the only ones of which I shall give an account. To discover anything new in the constitution of a fluid, the investigation of which has so long exercised the ingenuity of a host of eminent chemists, is in itself a proof of no small sagacity and practical skill. Now, in his paper in the *Lancet,* Liebig announces a twofold discovery of great physiological importance:—first, that lactic acid, which all other chemists have supposed to be an essential constituent of urine, does not exist in it; and, secondly, that hippuric acid, which was formerly considered to be peculiar to the urine of herbivora, is a constant and essential ingredient of that of man, and probably of other animals that subsist on a mixed diet. "All the urine," he says, "taken from individuals living upon a mixed animal and vegetable diet, contains hippuric acid, besides uric acid, and about the same proportion of both acids." I must refer my readers to the paper itself for the evidence on which these statements are founded, contenting myself here with remarking, that it seems perfectly conclusive on both points. The author gives a simple process, by which hippuric acid may be obtained even from very small quantities of urine. He was led to the discovery of this acid in human urine, by finding, as Proust had done before, that in putrid urine of this kind benzoic acid is a constant product. He argued that the latter acid could not be an ingredient in *fresh* urine, since Ure and Keller have proved that crystallised benzoic acid becomes converted in the living organism into hippuric acid, and appears in the urine as hippurate of soda. Now, it being already ascertained that in the urine of the herbivora benzoic acid is developed by the putrefaction of hippuric acid, he inferred that in putrid human urine it must have the same origin, and that this urine also, in its fresh state, must contain hippuric acid. Direct investigation established the truth of this inference, as already stated. Concerning the source of this acid he offers the following considerations:—

"As far as our investigations into the composition of the aliments of man will allow us to judge, they contain no benzoic acid from which hippuric acid might have been formed; and as the urine of cows is invariably rich in hippuric acid, no matter whether the cows have been fed upon hay or upon beet-roots,—of which latter plant we know positively, from the results of several examinations, and from the experience derived from the manufacture of beet-root sugar, that they contain no benzoic acid,—we can come to no other conclusion concerning the presence of hippuric acid in the urine of persons living upon a mixed vegetable and animal diet, than that it is a product formed in the organism, to the formation of which their non-nitrogenous aliments give birth."

These several points being ascertained, the author was enabled at once to point out the true cause of the acidity of urine, about which there has been so much debate. He first lays it down in principle that the inorganic bases present in urine, such as potass, soda, lime, &c., have entered the organism through the medium of the aliments; a proposition which is, in fact, self-evident.

Now, in the ashes of meat, and of the flour of cereals and leguminous plants, these elements are found in combination with phosphoric acid, in the form of bi-basic and tri-basic phosphates,—salts which have an alkaline reaction. In animals fed on these aliments, the urine should therefore be alkaline, if no acids were added to it by the living body. But the uric and hippuric acids, which are products of the vital processes, and are removed from the organism, together with the soluble phosphates, through the urinary organs, both possess the property of combining with the soda or potass of the alkaline phosphates, and acquiring in the combination a higher degree of solubility than they possess of themselves, at the common temperature of the body. "It is therefore obvious," as the author remarks, "that by the accession of these two acids, and by their action on the phosphates of soda, an urate and hippurate of soda must be formed, on the one hand, and an acid phosphate of soda, on the other; and that, consequently, the urine must acquire an acid reaction."

Another acid, the *sulphuric*, which is continually being formed in the living body by the oxidation of sulphur,—an element which is a tolerably large and a fundamental constituent of many kinds of food,—must also take its share of the alkaline bases, and further contribute to the same result; so that, in fact, the acidity of the urine is owing to the bases, which enter the economy as basic phosphates, being divided between the phosphoric acid they are originally united with, and three acids formed in the living body, namely, the uric, hippuric, and sulphuric acids. From this it would appear, that if, in addition to the basic phosphates, food be taken containing alkalies in combination with vegetable acids, the urine may become alkaline without the intervention of any other circumstance: for, in consequence of the oxidation which the acids of these salts undergo in the living economy, they all reappear in the urine as alkaline carbonates. Now, all vegetable aliments, without exception,—tubers, roots, greens, potatoes, turnips, &c.,—contain salts of this kind; and Liebig asserts, that after eating largely of vegetables, or of fruits, as strawberries and cherries, the urine is invariably alkaline. When they are partaken of in a certain proportion, he states that the carbonated alkali derived from them neutralises the acids that are present, and renders the urine neutral; when in larger proportion, it imparts to the urine an alkaline reaction: from which he infers that the acid reaction of healthy urine is purely accidental, and that urine of an alkaline or neutral reaction cannot be considered as a symptom of a diseased condition of the body. However this may be, all persons competent to judge in such matters will agree in the main with the author, in the following propositions:—

"First; that the analysis of urine, when made without respect to the inorganic salts, acids, and bases supplied by the aliments, teaches nothing whatever, and by no means justifies us in drawing therefrom any physiological or pathological inference. Secondly; that, from the nature of the ashes of the aliments, we are able to determine positively the inorganic constituents of the urine emitted. And, thirdly; that only when the amount and nature of these ashes have been distinctly ascertained, can we expect to derive from the analysis of the urine any correct information *with respect to the inorganic matters which have come to be present in it through processes of disease.* This, at least, is the *chemical method* of quantitative investigation."

Such is a very brief summary of the leading results put forward in this highly original and important paper. But the whole communication is replete with interest, and is characterised throughout by that ingenious application of principles to the interpretation of the phenomena of life, which so remarkably distinguishes all the productions of this eminent chemist, and makes them so rich in suggestions, as well for the practitioner as the physiologist. It well deserves a most attentive study at the hands of both.*

From this account of the chemical forms in which the materials of the body are discharged in their daily waste, we naturally pass

* The following extracts furnish a good illustration of these remarks:—"It will now be understood why the alkaline phosphates are generally absent from the urine of herbivorous animals, and also why, in certain cases, they may be found in the urine of these animals. If the nutriment of these animals contain no soluble phosphates, their urine cannot contain any, whilst if we add a certain proportion of grain to their food, the alkaline phosphates may be detected in their urine. Thus it is obvious, likewise, that the soluble phosphates in the urine of man are merely accidental constituents, and that by simply adding lime or magnesia to the aliments, and thus assimilating the constitution of these aliments to that of the food of herbivorous animals, the urine must become altered in its nature and properties. The knowledge of the influence which alkalies, magnesia, and lime, or acids, exercise upon the properties of the urine, or, in other words, upon the secretory process of the kidneys, in the healthy organism, is of the highest importance for the curing of diseases. I believe that there is now required only a small number of good and correct observations to establish a fixed rule for the remedies necessary in various cases. Future properly directed experiments will prove whether sanguification is absolutely dependent

on to the consideration of the various proportions in which these materials contribute to it, and of the phenomena which occur when this waste is no longer repaired by supplies of food. M. Chossat, a French physiologist, has lately undertaken to elucidate this subject by experiments, on a large scale. His plan was simply to starve a great number of animals to death, by partial or total privation of food, and to make accurate record, from day to day, of the principal phenomena that occurred in the course of the process. The results thus obtained are given in a work entitled, *Recherches Experimentales sur l'Inanition*; a work which appears to have gained great credit for the author in France, the Institute having conferred upon it the very high distinction of a Monthyon prize. To us, however, it appears at first sight as nothing less than wanton cruelty of the worst kind, to starve animals to death merely to have an opportunity of observing the phenomena of inanition, when these phenomena are, unfortunately, daily exhibited to us on a large scale, in the effects of a variety of wasting diseases, and, though more rarely, yet in all the simplicity of experiment, by the accidents of famine, shipwreck, and other forms of human calamity. Let us see whether or not there is anything in M. Chossat's results to lead us to abate the rigour of this first impression; whether the importance of the end be such as to justify the means.

The phenomena which engaged M. Chossat's chief attention were those which have reference to weight and temperature. As his victims were weighed every day, once at least, and at the same hour, he was enabled to ascertain very exactly the rate of diurnal

upon the presence of alkaline phosphates or not. We shall be able to determine whether weak solutions of alkaline phosphates are not the best solvents for uric acid deposited in the bladder, and likewise, what is the influence which aliments rich in sulphur, such as mustard, for instance, exercise upon the separation of uric acid in the bladder, in consequence of the formation of sulphuric acid. At any rate we may, by a judiciously selected diet, alter, with positive certainty and at pleasure, the nature of the urine; we may, without causing any injury to health, keep it alkaline for a long time by adopting a vegetable diet, and this is, certainly, the first condition necessary to insure the entire prevention of the formation of uric acid, as is the case with the herbivorous animals. By its combination with an alkaline base, uric acid must, in the organism, resolve itself into its ultimate oxygen compounds, with the same facility as other organic acids, if the physician prohibit all substances to be taken as food, which, like wine or fat, take possession of the oxygen necessary for the transformation of uric acid into carbonic acid and urea."

loss. The only point of any interest in these observations, was the rapid acceleration of this rate in the day or two immediately preceding death,—a period when common chemical agencies are beginning to resume their sway, and the enfeebled vital resistance is already giving up the body a prey to decay and dissolution.

In regard to the total loss, M. Chossat attempts to deduce a general proposition to the effect that, whether life during inanition be more or less prolonged, this loss, compared with the original weight of the animal, is always nearly the same in the same species. The average in warm-blooded animals he states to be 40 per cent. It is obvious, however, that such a proposition as this cannot be taken in an absolute sense, for the amount of this proportional loss must vary largely with the amount of fat previously accumulated; nearly the whole of which, as is well known, disappears in starvation. Thus, in the now well-known instance of the hog that remained alive for 160 days, buried beneath a cliff at Dover, and shrunk during that time from 160 lbs. to 40 lbs., the loss, instead of being less than one-half, was three-fourths of the original weight.* This is a point, in fact, which is so clear as to need no illustration.

M. Chossat made a great number of observations in order to ascertain in what proportions the various constituents of the body contribute to the general waste. The results he obtained are expressed in the following table, in which the loss sustained by each of these various constituents is set down in order, according as it stands above or below 0.40, which, it will be remembered, was the average loss of the whole body in M. Chossat's observations:—

Parts which lose MORE *than the mean* 0.40.		*Parts which lose* LESS *than the mean* 0.40.	
Fat	0.933	Muscular portion of stomach ..	0.397
Blood	0.750	Pharynx and œsophagus	0.342
Spleen	0.714	Skin	0.333
Pancreas	0.641	Kidneys	0.319
Liver	0.520	Respiratory apparatus	0.222
Heart	0.448	Osseous system	0.167
Intestines	0.424	Eyes	0.100
Muscles of locomotion	0.423	Nervous system	0.019

Many of these results, however, must be regarded merely as rude approximations; for it is plainly impossible to obtain with any

* *Transactions of the Linnean Society*, vol. xi., p. 411.

greater approach to exactness, the loss of weight suffered by many of the elements here specified; such as the blood, for example, the muscular portion of the stomach, and so on.

The next series of observations given, was made in order to ascertain the effect of starvation on the temperature of the body. Knowing, as we do, that the food is ultimately the source of animal heat, it was natural to expect that one of the first effects of starvation would be a fall of temperature in the animal. Observation has already taught us, as Liebig has well remarked, that a starving man is soon frozen to death; and that in gratifying our appetite for appropriate food, we obtain the best protection against cold.

In the animals starved to death by M. Chossat, the fall in temperature was uniform, but slow, up to the day before death; the total fall in the thermometer during this period being about 2°.5 *centigr.*; the daily fall about 0°.3 only; but on the last day, the thermometer fell rapidly from hour to hour, until the animal died, the temperature having sunk on an average 14° in the course of this day, and being at the moment of death, only 24°.9. As animals plunged in freezing mixtures generally die when cooled to this point, and as M. Chossat's victims perished with all the symptoms of death from cold, he was led to infer that the immediate cause of death in starvation is the failure of animal heat. The truth of this inference was fully established by the following experiment:—Several turtle-doves, at the very point of death, were placed in a vessel made for the purpose, and warmed by artificial heat. Before the heat was applied they were in a state of insensibility, with their bodies abandoned to the force of gravity, and without power to move; but as their temperature rose their senses and vigour returned; they began to flap their wings, then perched themselves on the edge of their stove, and next flew about the room and took what food was offered them. If they were kept warm for some time longer, most of them recovered, when well fed; but it is important to remark, that if the artificial heat was withdrawn too early, the food was not digested, and they soon died.

Such are the principal results of M. Chossat's investigations; the only ones, indeed, for which any importance can be claimed. Some of them are, without doubt, more or less interesting; but we must here repeat the question—Was it justifiable to subject hundreds of animals, endowed with great capacities of pain, to the lingering tortures of death by starvation, in order to obtain them? Was it even necessary for the end in view? To the shame of this cruel physiologist be it stated, that there is not one of these results which might not have been deduced, scarcely one, indeed, which had not already been elicited, and that not vaguely, but with every needful precision, from the principles of organic chemistry, and the facts which accident and the observation of wasting diseases had already thrown in our way.

In most recent works on organic chemistry, but especially in that of Liebig, they are already clearly and explicitly laid down; and not as in M. Chossat's investigations, in the form of blind and empirical facts, but in that of deductions from principles established on a scientific basis, and giving the rationale of the phenomena.

Liebig's account of the phenomena of starvation has all that superiority over that of M. Chossat which science has over dry empirical fact without interpretation.

But, to enter into particulars, let us ask—Was it necessary to starve great numbers of turtle-doves to death, in order to ascertain that fat and muscle and its equivalent, blood, and other kindred matters, contribute most to the waste of the body in starvation? If accidental starvation and wasting diseases had never been, would not the phenomena of hybernation and the mode in which the carnivora are nourished have sufficed to establish all these points on a basis that could not possibly be made more sure? Did we require M. Chossat's experiments to learn that the fibrous tissues, and those structures of which gelatine, with or without large quantities of earthy matter, is the basis, suffer but small loss in comparison? For the chemist, as well as the physiologist, the small supply of blood these structures receive, would alone have been sufficient evidence. Every day observation, too, had prepared us for the fact, that the nervous centres shrink but little, and science had already offered an explanation of it.*

* In reference to these points, I cannot refrain from quoting the following interesting passages from Liebig's *Organic Chemistry in Application to Physiology and Pathology*:—

"In the progress of starvation, however, it is not only the fat which disappears, but also, by degrees, all such of the solids as are capable of being dissolved. In the wasted bodies of those who have suffered starvation, the muscles are shrunk and unnaturally soft, and have lost their contractility: all those parts of the body which

It certainly was an unexpected result, that the waste of these structures should be less than that of any others; the bones not excepted. According to M. Chossat's observations, this waste is scarcely appreciable. I cannot but think that there must be some fallacy here, and that the fact will ultimately receive an interpretation, which will refer the maintenance of weight, in some part, at least, to the substitution of blood or other interstitial matter for wasted tissue. At least, there can be no doubt that the brain does very sensibly waste in the emaciation of disease. The manifestly shrunken state of the organ in the bodies of those who have died of diabetes, of cancer of the pylorus, and other diseases that kill by inanition, is almost sufficient evidence of it. But if this left any doubt on the point, it would be entirely removed by the more precise observations of M. Foville, which, on account of their intrinsic interest, I shall here quote entire. This author, in a recent work, which I shall have to pass in review in the sequel, makes the following remarks:—

"It is generally admitted that the cerebro-spinal system does not participate with the body in changes of volume corresponding to those which the latter undergoes in passing from a state of plumpness and muscular development to the wasted state which chronic diseases lead to. This opinion is without foundation.

"It is easy to satisfy one's self of this, on comparing the brains of fat and robust persons who have died of acute diseases, with those of others who have died in a state of marasmus. In the former, the convolutions of the brain, the lamellæ of the cerebellum, the peduncles, the pons, and even the spinal cord itself, show a degree of volume and roundness of form which is not met with in the latter. In these, in fact, the convolutions and lamellæ are shrunken; the broader inter-spaces between them are filled with a large quantity of serum; the peduncles, pons, and spinal cord are more angular in their forms, while their whole mass is diminished. The two offer, in one word, differences corresponding to those

were capable of entering into the state of motion have served to protect the remainder of the frame from the destructive influence of the atmosphere. Towards the end, the particles of the brain begin to undergo the process of oxidation, and delirium, mania, and death, close the scene; that is to say, all resistance to the oxidising power of the atmospheric oxygen ceases, and the chemical process of eremacausis or decay commences, in which every part of the body, the bones excepted, enters into combination with oxygen."—pp. 26, 27.

which the limbs themselves exhibit in their extremes of development and leanness; a practised eye cannot be deceived in it.

"These variations in form and volume do not, however, suffice to prove that the cerebro-spinal system contains more or less fat, according to similar variations in the general plumpness; but we may easily obtain direct proof of this, by suspending in a vessel filled with spirit, the brain and spinal cord of a robust and fat man, killed by accident; and in a similar vessel, likewise filled with spirit, the same organs of a man dead of emaciation.

"In the course of two or three days, the whole surface of the former will be found covered with fat in crystalline needles, which become so abundant as entirely to hide the structure beneath. In the other vessel, nothing of the kind occurs, as water is the only principle the nervous mass it contains yields to the spirit."

After making the interesting remark, that the brain of the *cetacea* so abounds in fat as to float in water, M. Foville adds— "That very often the peculiar aspect of the brain of aged men is due to emaciation; but that this must not, however, be confounded with the senile or morbid atrophy of this organ, and which has its own characters."*

But by far the most interesting result of M. Chossat's investigations—to which I now return—is that which shows that the immediate cause of death from starvation, is the failure of animal heat. This is a fact which has many important practical applications in the treatment of febrile diseases of long duration; but even in this fact there is nothing that might not have been strictly deduced as a logical inference, from the known chemical relations of food to respiration; nothing indeed that had not already been clearly laid down in principle, and almost in so many words, in various chemical works in which these questions are considered. In support of this statement I may confidently appeal, more especially to Liebig's work already quoted, and to various parts of M. Dumas' paper on "Neutral Azotised Matters," of which an analysis has already been given in these pages.†

After these remarks, I need scarcely add that I am at a loss to know on what scientific grounds the French Academy thought fit

* *Traité complet du Systéme Nerveux Cerebro-spinal*, pp. 120 et seq.; M. Foville, 1844.

† "Memoires de Chimie," see particularly pp. 340–41.

to confer so high and valuable a mark of distinction on this memoir. For thus giving, with so little to justify them, such an effectual encouragement to cruelty of the worst kind, they deserve the grave rebuke of all humane men. I am not one of those who entertain what may be held to be maudlin and sentimental objections to experiments on living animals. But while freely admitting that such experiments are justifiable, where we are warranted by previous and well-founded induction in the expectation of results of importance to the well-being of man, attainable by no other means, I the more feel bound, on the part of all true lovers of science, to enter an indignant protest against such wretched cruelties as these, undertaken almost at random for the chance of what may turn up, and for the production of phenomena which accident and disease are daily offering to our observation on a large scale, and into the nature of which science has already given us the clearest insight. I want words to express my own abhorrence of these practices, and my deep sense of the guilt of those who resort to them. The frequency of them in France, and the shocking levity with which they are undertaken, is the great blot on the present character of physiological science in that country.

I shall not weary my readers with any account of another series of almost equally cruel experiments, undertaken by the same physiologist with a view to determine the effects of a diet of pure sugar. I pass them over with the greater satisfaction, since the author of them has already received on their account a well merited chastisement from a more powerful arm than mine.*

To complete this review of the recent progress of organic chemistry, it only remains for me to briefly notice Liebig's remarks on the kindred processes of fermentation, putrefaction, and decay,—processes which, in one form or another, play an important part in many both healthy and morbid changes in the living body, besides their allotted task of resolving it after death into the chemical elements out of which it originally came. His observations are quite decisive in showing the purely chemical nature of the agency which the ferments and other bodies in a

* See "Observations on Organic Chemistry," by Professor Liebig; *Lancet*, June 29, 1844, p. 437.

state of change play in these processes. He well remarks, in objection to those who ascribe an essential part in them to microscopic beings, "If the fungus be the cause of the destruction of the oak, and the microscopic animal the cause of the dissolution of the dead elephant, to what cause then are we to ascribe the putrefaction of the fungus and the microscopic animal after their death? They also ferment, putrefy, decay, and disappear gradually, first in the same manner as the oak and the elephant, and in the end furnish the same products."

This single objection decides the question, and is quite unanswerable. But although Liebig thus shows the absurdity of the notion that animalcules play any essential part in putrefaction, he nevertheless admits that they greatly accelerate the resolution of dead matter into more simple chemical elements, and thus hasten the process. This is clear enough, since all animals are ever actively reducing, by the function of respiration, the complex atoms on which they feed into carbonic acid and water, which are two of the last products of decay.

These observations are so conclusive that I may dispense with giving an account of the otherwise interesting experiments of Helmholtz, who succeeded in infecting organic mixtures with an active state of putrefaction, by introducing into them other putrid fluids by a process of endosmosis, whereby all living organisms were effectually shut out.*

I cannot close this part of my Report without expressing my regret and humiliation at having, through its whole course, scarcely once had occasion to speak the name of an Englishman. It is true that in this province we have one or two individual names, of which we may well be proud; but we shall look in vain for any thing worthy to be called a *school* of organic chemistry in this country, and still more, for anything worthy to emulate the noble schools of which Dumas and Liebig are the heads in France and Germany. I earnestly hope that we shall not long allow ourselves to be thus outstripped in this noble rivalry of nations, but that our own countrymen will strive to share in the glories that are

* See Erdmann and Marchand's *Journal für Praktische Chemie*, No. vii., 1844, p. 429: "Ueber das Wesen der Faülniss und Gährung," Von Dr. Helmholtz. In many respects an interesting paper.

yet in store for men of genius, in the bright career which these illustrious chemists have opened to us.*

I may here be allowed, in particular, to express a hope, that in the changes now in contemplation in the education of members of our profession, the teaching of chemistry on a grand scale, both in theory and practice, may be made a chief object. In the education of the physiologist, chemistry should be put on a level with anatomy, *since chemical change is, at least, co-ordinate with structural form as a condition of life.* Of the two, I am not sure whether a knowledge of the former does not give us greater insight into the most remarkable of these phenomena, seeing the intimate and material relationship which the consumption of matter bears to the various forms of force which the animal has at its command. I am deeply convinced, that until these truths are clearly seen and acted upon, it will be vain to hope for any great advance in medical science.

Before I pass on to the next division of my subject, I must refer to some recent observations on the physical phenomena of the circulation and of absorption. Those to which I chiefly desire to draw attention will be found in a paper by Dr. Marshall Hall,† on "The Circulation in the Acardiac Fœtus;" in another, by

* It is deeply to be deplored that this science, and the sciences generally, receive so little encouragement in this country at the hands of the State. It surely would be both wisdom and economy in a great nation like ours, which has taken to itself the high task of subduing every power, and appropriating every product of the earth to man's use, to set aside and endow a great body of eminent men for inquiry into Nature. A vast step will be gained in civilisation when our legislators shall come to see in all the clearness in which this truth must ultimately appear, that in this direction lies all future extension of our power. It is surprising that such a truth should still be but vaguely and obscurely apprehended, when the intimate connection between the most abstract discoveries in science and the greatness of nations, is daily set before us in such a striking point of view in the connection of astronomy with navigation. National astronomical observatories are kept up, because without them commerce must cease, and the ocean become what it once was—a trackless waste. But because the arts and agriculture still go on, and even advance on their present purblind methods and by the help which science already gives them, would it answer the less to institute great chemical observatories also? The works of the laboratory of Giessen are a sufficient answer. Already, on the faith of discoveries made there, a fleet of British merchantmen are floating their freight of guano to our shores; a freight which, before another year is over, will bring its return in corn and oxen.

† *London and Edinburgh Monthly Journal of Medical Science*, June, 1843.

Spengler,* on "The Strength of the Arterial Stream;" in Dr. G. Burrowes's excellent Croonian Lectures on "The Circulation in the Brain;" and in some papers by Mr. G. Robinson, in the *Medico-Chirurgical Transactions* and the *Lancet*, on "Venous Absorption." All these papers will be found worthy of an attentive study, and I regret that I have not room for more than this bare mention of them.

I would only remark, that Mr. Robinson's papers are distinguished by great clearness of ideas on the particular subject in hand, and that his observations on absorption, especially, will, if confirmed, tend to give great precision to our views of this process, and to fix more exactly than we have hitherto been able, the conditions by which it is regulated. These are points of great practical importance, because on our knowledge of these conditions, so far as they are under our control, must depend the proper application of remedies in a great variety of cases.

It is to be hoped that our increasing knowledge of these subjects will soon enable us to dismiss such vague phrases as "stimulating the absorbents," and the like; a phraseology which belongs to the infancy of science and a barbarous age, but is quite intolerable in the present day.

I now pass on to the other great division of my subject, namely that which relates to the anatomical form of the structures of the body, and to the functions of these structures considered in the sense of vital endowments. From the vast number of observations in this wide field, I am compelled to restrict myself to a selection of the more interesting and important. Many things must be omitted from this selection, which of right should find a place in a report of progress. In excuse for this I must plead my own want of time, and the limits of the space allowed me in these pages,—limits which, I fear, even with the reserve which circumstances have imposed upon me, I may still be tempted to transgress, to the exclusion of more important matter. I feel it necessary to say thus much, for the satisfaction of those who may be disappointed at finding no notice of their labours here. I may also add, that a great number of observations, of various kinds, although both novel and interesting in themselves, have been left out designedly;

* *Müller's Archiv*, 1844, heft i., p. 49.

for the object I proposed to myself in this Report, from the outset, was, not to give a complete and detailed analysis of the labours of the year in the form of dry catalogue, but rather a connected, and, in so far as in me lay, a critical view of such among them as touched on great questions in physiology, or seemed striking from their novelty and individual importance; venturing here and there, while surveying the past, to point out in what direction lie our hopes for the future.

In my choice of subjects I have also uniformly given a preference to those which illustrate the principles of investigation which should guide us in physiological inquiry. By the adoption of these restrictions, a large mass of matter was at once excluded from this Report. In the subjects belonging to this division of it, almost the only labours of the past year that answer the conditions just named, are two or three works on "The Anatomy and Physiology of the Nervous System," Matteuci's striking discoveries on "The Electric Phenomena of Animals," and some recent investigations into the nature of certain functions connected with reproduction.

The chief part of what space remains to me will be given to the consideration of these subjects. In order to enter upon them the sooner, I shall, therefore, pass very rapidly over some other objects of minor importance, beginning with the blood, as being the first step in the passage of the food from the state of raw material into that of organised forms.

In the *Zeitschrift für Rationelle Medicin*, Bd. i., s. 238,* there is a very interesting paper, by Scheerer, on the cause of the difference between the colour of arterial, and that of venous blood. This paper contains a great number of observations, all tending to confirm the opinion already advanced by Dr. John Davy, as also by Henle and Nasse, that this difference of colour depends rather on physical than on chemical conditions. Scheerer is of opinion that it is related to differences in the form of the blood-corpuscles, affecting the manner in which light is reflected by these bodies. His researches seem to render it probable, that the bright colour of arterial blood is connected with a bi-concave, the dark colour of venous blood, with a more globular form of the corpuscle. In support of this, he has ascertained by the microscope, that salts

* See also *Schmidt's Jahrbucher*, No. ii., 1844, from which the account in the text is taken.

and oxygen, which alike have the property of communicating an arterial colour to venous blood, have this effect in common: that both, namely, impress a very marked bi-concave form on the blood-corpuscle; whereas, water and carbonic acid, which give a dark hue to blood, cause the corpuscle to swell out and become bi-convex. That the colour of arterial blood is not the effect of mere chemical change in the colouring matter in consequence of its uniting with oxygen, would appear from the fact, that if a stream of oxygen be passed through venous blood previously diluted with water, no change of colour ensues, but the fluid remains dark as before. The same is the case if, instead of oxygen, salts be added to the diluted fluid; the reason being, according to the author, that the blood-corpuscles retain the globular form in consequence of their having imbibed water by endosmosis. In conclusion, he points out some other conditions, which probably have a share in causing the change of colour which the blood undergoes in its passage through the lungs.

The next observations that claim our attention are taken from a monograph by E. Horn, and relate chiefly to the *genesis* of the blood, and especially to the relationship which its various corpuscles bear to one another.* The lymph-corpuscles he regards as the true development-cells of this fluid; he finds them extremely abundant in blood taken about an hour after a meal, as also in the blood of pregnant women. The coloured corpuscles are, according to the author, the immediate offspring of these. In the course of development each lymph-corpuscle becomes a parent cell, containing a progeny of several smaller ones, these last being the embryo state of the coloured corpuscle.

These observations agree, in the main, with some formerly published by Dr. Martin Barry, and, according to Valentin, they have also been confirmed by Remak. Besides these two kinds of corpuscle, Horn describes, under the name of nucleated cells, other bodies of round or oval shape, and from two to four times smaller than the developed blood-corpuscle. These bodies are exceedingly numerous in the blood of birds, amphibia, and fish, but less so in that of mammals.

* *Das Leben des Blutes und die Gesetze des Kreislaufes, nach neuen Untersuchungen bearbeitet*, Wurzburg. See also *Valentin's Repertorium* for 1844, from which the account in the text is taken.

Finally, he recognises other bodies again, of still smaller size, and of yellow or reddish colour, swimming free in the *liquor sanguinis*. The author looks upon these last as the first solid forms arising out of the liquor sanguinis, and has named them "primordial nuclei."

The work also contains a great number of observations on the formation of the blood-corpuscle in the adult, on the seat of the colouring matter of blood, and on the chemical and physical characters of this fluid.

In *Müller's Archiv*, Meyer has described some singular varieties of form in blood-corpuscles, found in the blood of various reptiles,—varieties which have some interest in their bearing on the mode of development of these bodies. Thus, scattered among the normal corpuscles, were some provided with tails, others spindle-shaped, and others, again, contracted in the middle and swelling out at each end in a vesicular form, so as to offer some resemblance to dumb bells. This last form, in particular, acquires interest from the fact, that from the observation of similar, but more definite appearances in cells of various kinds; in the blood-corpuscle, namely, in the transitory cells of the yolk, and in those of which the simplest infusoria are made up, Dr. Martin Barry has been led to the theory, that by far the most general, if not the only mode in which cells multiply, is by fissiparous division. According to Dr. Barry, the process begins in the nucleus of the cell, or, as he calls it, the hyaline body, and is afterwards extended to the whole cell.*

The minute structure of the spleen still continues to baffle the skill of anatomists. Among others less distinguished, MM. Flourens and Bourgery have made a very laborious examination into the anatomy of this organ, but without any result of much novelty or importance. M. Bardeleven has endeavoured to gain some insight into the nature of its function, by watching the consequences of its extirpation in dogs. He found, as others had done before him, that those animals that survive the operation, retain their health and are, to all appearance, exactly as before. He even obtained the same negative result in animals from which he removed the thyroid body also. In neither case did any function

* *Jameson's Journal*, 1843.

appear to suffer. In particular, he ascertained that there was no diminution of the venereal appetite or procreative power. In no case did he witness anything like an attempt to form a new spleen, as Meyer, of Bonn, pretends to have done.

I regret that I can give no account of Mr. Simon's prize essay on "The Anatomy and Functions of the Thymus," in consequence of not having yet had an opportunity of reading it.

In *Müller's Archiv,* (1843, heft. iii.,) are two papers on "The Anatomy of the Liver," by Weber and Kruckenberg, which contain a good account of the vascular element of this organ. They point out, as its leading characteristic, the immediate breaking up of large vessels into minute capillaries, forming a network throughout the liver;—a description which accurately agrees with that of Mr. Bowman. The only novel part of their papers concerns the biliary ducts. Weber distinctly asserts that these do not terminate in blind ends, but anastomose with one another, so as to form a similar capillary network to that of the blood-vessels with whose meshes they are interwoven. Strange to say, they neither of them make mention of the proper cells of the liver, which their mode of examination, namely, by injection, had probably hidden from them. They both agree in stating that there are no such things as distinct *acini* in the liver, separated from one another by cellular tissue, a statement in which they are again in agreement with Mr. Bowman, but which Müller endeavours to controvert in some critical remarks appended to their papers.

On other parts of anatomy there is nothing that need further detain us from the more important labours which concern the nervous system, and the other subjects to which the remainder of this Report will be given.

Various motives still continue to render the nervous system and the great function of reproduction, the favourite subjects of investigation with physiologists. The brilliant light already shed by the microscope on the most obscure parts of the physiology of reproduction, with its rich promise of further illumination, might well give to this subject more than common attractions for the student, ambitious of distinction. But, in addition to this, the revival of the discussion touching the relation of the periods of *rutting* and menstruation to the spontaneous ripening and shedding of ova, has given all that activity to the investigation of this subject, which the introduction of an important theory invariably communicates to every branch of natural inquiry.

In the anatomy and physiology of the nervous system, the impulse given by the brilliant discoveries of Bell, and more recently by those of Marshall Hall, Flourens, and others, is still manifest in the ardour with which numbers of eminent men follow out, in various directions, the paths which these have opened. As yet, however, it is evident that in this branch of knowledge, discovery is only in its infancy. When we contemplate the vast number of problems as yet undetermined that here wait for solution,—problems whose data palpably lie within the limits of precise determination, and which already shape themselves out in the form of definite questions,—it is evident that this will long continue to be the most stirring field of speculation in the whole domain of physiology.

What subject, indeed, is there in the whole range of human inquiry, so fit to excite the imagination and rivet the attention of a speculative being, as the construction and laws of action of those structures, wherein mind is so mysteriously blended with matter, and that union of will with mechanical force is effected, which, while it has no parallel in nature, alone gives to man and animals their command over the world without?

During the past year, many physiologists of high name, some of whom have already earned distinction in the same field, have, with various objects in view, and with various success, devoted their time and energies to several branches of this difficult inquiry. Many reasons, however, deter me from attempting anything like a complete analysis of their labours. Of these reasons, the following will, I doubt not, be deemed sufficient:—

In the first place, several of the works in which these labours are set forth, are loaded with a vast mass of dry detail, which, from being by its very nature not susceptible of condensation, and from having no present connection with physiological speculation, any attempt to present, in the form of abstract, would be alike wearisome and unprofitable. In the next place, it will be readily understood that descriptions of anatomical connections, so minute and intricate as those of the more complicated parts of the nervous centres, are utterly unintelligible without the help of plates. Indeed, even when the descriptive power of the anatomist is illustrated by the pencil of the artist, it is often extremely difficult to gather the meaning of the author. To transfer such descriptions to these pages, therefore, would mislead rather than inform, and tend to retard science instead of advancing it.

But a more weighty reason still, and one that of itself would justify me in the course I take, is that the authors to whose investigations I shall have to refer—physiologists of high repute, and of almost equal authority in such matters—are often in direct contradiction with one another, touching points of the most elementary kind and of cardinal importance. Thus M. Foville, whose name has acquired an European celebrity from his researches in this branch of anatomy, teaches, in his description of the spinal cord, that through the whole length of this structure there is a decussation of its anterior columns, so that ultimately the whole of the fibres of the right side of these columns are found to have passed to the left, and *vice versa*. In the upper part of the cord, between the pyramids, some such crossing is very generally admitted, and is, as one would fancy, very obvious to sight. M. Foville states, that the same thing may be shown through the whole cord by a very simple anatomical manipulation. He states further, that in the act of crossing these fibres dip inwards, so as to become lateral, and at the same time so entirely change in aspect as from being white to become grey. Still more than this, M. Foville adds, that at his suggestion M. Gruby has examined these parts most carefully with the microscope, and has entirely confirmed every one of these observations. Moreover, to illustrate the text, several figures are given, in which all these details are exhibited with such clearness and distinctness as would seem to leave no doubt of their being true to nature. Among M. Foville's plates there is certainly no figure more clear and defined than that which exhibits the decussation of the anterior columns. If this figure is not to be trusted, one really cannot know what faith to put in the rest. What shall we say, then, when we find Dr. Stilling,—who claims the greatest accuracy for his observations, and who has examined the cord throughout most minutely with the microscope,—stating, that if there be two points in its anatomy that may be considered absolutely clear and certain, they are these:—that there is no decussation of the anterior columns, and that nowhere are the white fibres found passing into grey. Hannover, again, fully confirms these statements by independent observations; and the apparent truthfulness of his plates, and the fact of his having received a prize for his investigations from a body well qualified to pass judgment in such matters, would seem a sufficient guarantee for his trustworthiness.

Now, when men of the first authority differ on such a cardinal point as this, and one, as it would seem, so easy of determination by methods at ready command, one is really at a loss to know what to think of any of their observations. The fact should certainly impose on us extreme caution in admitting any such into a report of progress, unless they come recommended by various and weighty support of other kinds.

For these several reasons, in my review of the progress of this branch of anatomy and physiology, I shall, as a general rule, only touch on such observations as seem to be established on good grounds, or to involve doctrines of cardinal importance; reserving, however, the privilege of extracting, now and then, any isolated fact that may appear to be of more than common interest.

Of the works I shall have to pass in review, that of M. Foville, *On the Anatomy of the Brain and Spinal Cord*, is by much the most voluminous; and, as far as evidence of years of unwearied labour goes, although this by no means constitutes its highest pretensions, it comes before us with stronger claims to our notice than any of the others. Unfortunately, however, for these same reasons, the difficulties in the way of an analysis of his researches are greater in the same proportion, and, in fact, under the circumstances, quite insuperable. To attempt to give anything like even a cursory view of the whole, within the narrow limits of this report, would be to do the author great injustice; and those who desire to obtain it, must refer to the book itself. I must content myself here with a sketch of the most general and important of the results he has arrived at, offering, in conclusion, a few remarks on his methods of investigation, with a view to determine the degree of confidence to which they entitle his researches.

The decussation which M. Foville describes as taking place between the anterior columns of the cord, through the whole length of this structure, has already been spoken of. In addition to this, it is only necessary to state that, with most other anatomists, he divides the cord into three principal fasciculi,—the anterior, lateral, and posterior,—each giving origin to its own particular class of nerves; those from the posterior being exclusively sensory, those from the anterior exclusively motor.

It is right, however, to add, that those who will take the trouble to consult the work itself, will find in it a most accurate description of the cord in its external form and relations, and, as far as the methods of the author carry him, a most minute and elaborate investigation into the connections of its white and grey elements; into the form and direction of its commissures, and the ultimate relations of the roots of the nerves to these several parts. It is evident from the details into which the author enters, that he must have spent a vast deal of time and labour on this investigation, and must have subjected a countless number of specimens to careful and various examination.

Touching the general idea to be formed of the brain and cerebellum, the most prominent view put forward by M. Foville,—and it is evidently one to which he attaches great importance, as it is brought up again and again in the course of the work,—is, that these structures are to be considered as the ganglia of the nerves of special sense. He describes himself as originally indebted for this idea to M. de Blainville, who has long taught that the brain and cerebellum are to be looked upon as ganglia; differing from ganglia in general, however, in being without external apparatus. (*Sans appareil exterieur.*) But, in this particular, M. Foville regards M. de Blainville as fundamentally wrong; and he labours to show that the anatomical relations of these ganglia to the nerves of special sense are, in all essential conditions, perfectly analogous to those of the ganglia of the sensitive roots to the spinal nerves; that, like these, the nerves of special sense, also, arise from the central nervous axis by two roots, and that it is on one of these roots only that these ganglionic enlargements are developed. I must refer to the work itself for the anatomical evidence on which this view is sought to be established.

As to the originality of the idea, it is one that, in slightly varying shapes, has long been current among physiologists of all countries; and so far from there being in it anything to wonder at, still less any ground of merit, it is one that, in some sense or other, must naturally suggest itself to every one at all versed in comparative anatomy, by the division of the encephalon, as we descend the scale of vertebrata, into a series of lobes or ganglia, each appropriated to its own nerve of special sense, and developed in exact proportion to the perfection of the organ which this nerve goes to supply.

But, after all, it may well be questioned whether the use of this language in its present vague sense be not, as the French so well express it, "*se payer de mots,*"—to substitute a phrase for real knowledge. To call the brain or cerebellum a ganglion,—a name, by the way, in itself of trivial meaning,—brings but small enlightenment while we are still in such profound ignorance of the attributes of the simplest organ of this class.

The great anatomical fact, in regard to the brain and cerebellum, which it appears to be one of the objects of M. Foville's work to establish, is that both may be separated into two grand elements, fundamentally different in character; namely, a central nucleus, formed upon, and constructed out of, the great central axis of which the cord is the continuation, and a covering to this nucleus, enveloping it in the form of a cap, and consisting of the convolutions and the fibrous expansion on which the cortical matter of these is laid down. It is in the brain, especially, that this admits of the most complete demonstration. The author gives a method of effecting this separation, and numerous figures of the two parts thus divided, which in the plates certainly look very definite and shapely, as if they were true and genuine anatomical, and not artificial elements of structure. The peripheral part, he labours to show, has with the nucleus only two principal physiological connections; one, by a broad central mesh of fibres, which comprises, in fact, the whole mass of fibres radiating from the great central axis to the convolutions; the other, by the convolutions themselves, which, in one point of their circumference only, are intimately united to the nucleus at the border of the perforated space—a region to which M. Foville attaches extraordinary importance, and which he regards as the grand centre and rendezvous of the most important elements of these structures. Thus, for instance, by the union here mentioned, the cortical membrane is brought into perfect fusion with the origins of the great cerebral nerves, the optic, and the olfactory.

In separating, therefore, the nucleus from the external cap, the only two genuine anatomical connections that have to be severed are, according to M. Foville, the central mesh of fibres, and the junction of the convolutions with the perforated space. The central mesh, which is very broad, and springs from a curved line along the whole convexity of the nucleus, includes fibres from the posterior and lateral columns of the cord, and also nearly the whole of the anterior columns, which pass up almost entire from the central axis to the convolutions in this assemblage. The fibrous element of the connection between the convolutions and the perforated space is, on the contrary, wholly furnished by the posterior columns. The precise way in which this connection is established is somewhat difficult to apprehend; but as far as can be gathered from the description, the most important feature of it is, that the fibres from the posterior column here split into a double membrane, which incloses the convolutions in the space between its layers. The external layer constitutes the outermost white stratum, first well made known by M. Baillarger, and more recently accurately studied by Lelut; the inner one is here described, for the first time, by M. Foville himself, and must be added to the series given by M. Baillarger, whose anatomy of these structures, however, with this important addition, our author adopts entire. His description of this layer is very minute and circumstantial, and as this, if confirmed, must prove to be one of the most interesting results of M. Foville's labours, I shall give it in his own words.

Speaking of this layer in its relation to the stratum of cortical matter, he says,—" The former follows, within, all the folds of this stratum, in the same way in which the pia mater follows them without. The pia mater throws off from the surface corresponding to the cortical matter numberless vessels, which penetrate perpendicularly from without inwards; the deep layer of fibrous matter which we here propose to consider as an essential element of the cortical stratum, throws off likewise from its surface processes of nervous matter of extreme tenuity, which penetrate the cortical stratum from within outwards, in a direction the exact reverse of that of the vessels. Thus, according to this view, four layers of white and three of grey matter constitute, by their assemblage, the cortical membrane of the convolutions of the brain and of the lamellæ of the cerebellum."

This account would seem to agree pretty well with Hannover's statement, that in the layer of white substance immediately below the grey, the fibres run parallel to the surface; unless, however, this should be found to apply to other fibres from the central mesh already described, which, in certain regions, also follow a horizontal course.

In the cerebellum, the analogue of the white membrane here described as lining the convolutions of the brain on the inside, is formed, according to M. Foville, by a membranous expansion, chiefly and jointly furnished by the roots of the seventh, or auditory, and fifth pairs of nerves. This expansion, he says, spreads itself out from the very substance of these nerves, in the same way as the retina from the optic nerve, and it lines the whole extent of the cortical layer of the cerebellum, in the same way as the retina lines the globe of the eye.

This, again, is one of the most remarkable results of M. Foville's labours, and, if found to be true, cannot fail to prove one of the highest possible interest in the physiology of these structures.

In concluding his account of the anatomy of the brain, he thus sums up the principal relations here passed in review:—

"By its circumference, therefore, the cortical layer is attached to the cerebral nerves and to the posterior column of the cord, and the elements it receives from these nerves and from this column, are continued over its whole extent.

"In its central parts this same cortical layer is penetrated from within outwards by fibres, which are continuous with the anterior column of the cord. The manner in which these latter fibres are combined with the cortical layer, is different from the mode of combination between this same layer and the fibrous elements it derives from the posterior column.

"These last spread themselves out into membranes of extreme tenuity over the whole extent of the cortical layer. Of this layer they are constituent elements.

"The parts derived from the fasciculated region of the peduncle, after having undergone all their transformations in the central nucleus,—in the great radiating layers of the hemisphere, and, finally, in the processes furnished by the convex surface of these radiating layers,—penetrate the cortical layer by their extremities at the summit of the convolutions. They represent a sort of sheaf, whose ears, only, belong to the cortical layer. The sheaf itself may be divided into a considerable number of layers placed side by side, holding by one end to the summit of the convolutions, and passing in the other direction, by a course more or less oblique, to the great radiating fibres of the internal face of the hemisphere."

In addition to the details by which these cardinal relations are sought to be established, the work contains an elaborate description of the more minute and intricate fibrous connections between various parts of the brain and cerebellum; a minute account of the relations, external and internal, of the ventricles; and a very interesting study of the convolutions, in which, although there is a good deal attempted to be laid down, that seems forced and artificial, yet there is also much that is both novel and instructive.

M. Foville's researches into the origin of the cerebral nerves, also, although, for reasons already assigned, they can find no place here, are made with praiseworthy care and minuteness. They deserve the greater consideration, because, from the greater simplicity of the inquiry, it is less open to sources of error from defect in the methods and instruments of investigation. This and other reasons incline me to believe, that these researches will eventually be found to be the most important of M. Foville's labours. This is a branch of anatomical inquiry which has not hitherto been followed up with that constancy and perseverance which might have been expected from the rich promise it holds out to the physiologist; for, from the perfect knowledge we now possess of the functions of nerves, it is evident that in tracing them back to the particular regions from which they emanate in the central organs, we have one of the most trustworthy clues within our reach, to guide us in our inquiry into the attributes of the latter.

Among the more isolated observations scattered through the work, these few remarks on the pituitary body are worth quoting:—

"That which most strikes the observer in the pituitary body, is its situation in the central part of the base of the skull, in a case lined by the dura mater, and protected by marginal folds of the same membrane.

"The pituitary body is by its situation, and by the structure of the walls of its case, more protected from injury than any other part of the brain. An organ which nature has placed in such conditions ought, by the fact, to be an organ of great importance."

The author adds in a note, that having made numerous observations on the state of this body, it has often appeared to him diseased.

The leading impression left on the mind by an attentive study of M. Foville's work is, that in relation to the more minute and intricate parts of this anatomy, his methods are gross and coarse when viewed in connection with the known minuteness of the objects, and the extreme softness and delicacy of the tissue under investigation. These methods are not of a character to satisfy the requirements of anatomists of the present day, with their recent acquirement in the microscope in its present perfect state, of what may almost be called a new sense, and with their more enlarged basis of inquiry. What we have already learnt of the anatomy of nervous tissue by these means, is sufficient to show, not only that such procedures as those employed by M. Foville must, in a vast number of cases, be quite inadequate to lay open the true elementary relations of structure, but that they must often do violence to connections of perhaps the first importance, and thus lead us into serious error. This is a conclusion which may be drawn with absolute certainty from our present knowledge.

I trust, however, that in the observations I am here making, I shall not be misunderstood. It must not be lost sight of, that we here have the fruit of twenty years' labour, bestowed upon a favourite subject, and with a degree of constancy and devotion but too rare in these days;—by an observer, too, of remarkable talent, a practical and skilful anatomist, and having unlimited opportunities at command. We can scarcely estimate too highly the claims on our favour and attention which these circumstances bespeak for them. Whatever may be the ultimate fate of these researches,—whether to take a permanent place in the science, or to give way to the results of more intimate and searching methods,—no one can refuse to admit that they form a large and valuable contribution to our knowledge. But highly as we may estimate these researches as a whole, it is requisite, for the interests of science, to add, that, for reasons already assigned, we cannot show too much caution in admitting any demonstrations of the more intricate parts of this anatomy, until they are ratified by the work of finer and more delicate instruments, or shown to have an especial warrant in analogy, or, still more, some essential connection with living phenomena and function. The absence of all elucidation from the two last sources seems to me to be the defective point of M. Foville's work. To find no reference whatever to embryology and comparative anatomy, is a great disappointment in these days, when these two sources of information are in general so largely drawn upon in similar works, and with such great profit to the inquiry.

The next works to be noticed are those by Drs. Stilling and Hannover, already cursorily mentioned, as containing the results obtained by these anatomists in their first attempts to make out the structure and relations of various parts of the nervous system, by help of the microscope. The account here given of them must necessarily be brief, as, without the plates which illustrate them, the greater part of the details of which they are made up are absolutely unintelligible. The methods employed, and the objects in view, were somewhat different in the two cases.

Dr. Stilling's investigations were confined to the spinal cord and *medulla oblongata*,—already, however, a sufficiently wide field. His mode of examining these structures was this:—to cut from a variety of specimens, *previously hardened by being steeped for some time in spirit*, extremely thin slices, both lengthwise and crosswise;—to lay these without covering on a glass, and examine them with great care under a low power. With a keen razor, and with the skill soon acquired by practice, Dr. Stilling states that it is very easy to obtain slices so thin as to be perfectly translucent, showing, as they lie on the razor, the bright lustre of the blade beneath. In these slices the course of the fibres can, for the most part, be distinctly followed, and their principal connections traced.

The following are the chief points of physiological importance in the author's account of the structure of the cord, as far as this can be understood without his plates, which I may remark, by the way, are extremely beautiful:—

His examination has led him to recognise in the cord three principal anatomical elements, namely, longitudinal fibres, transverse fibres, and certain corpuscles which, on account of their specific form and distribution, he names "spinal bodies."

The longitudinal fibres are of two kinds, white and grey, which, except in colour, only differ from one another in that the grey are softer than the white. According to the author, these two kinds of fibres form respectively the white and grey columns of the cord; for, in contradiction to former observers, he affirms that the grey matter of the cord, as well as the white, is entirely made up of fibres. This is rather a startling announcement for physiologists, since a great deal of present speculation is founded on the opposite opinion. Still more so is Dr. Stilling's statement, that these fibres hold an uninterrupted course from the *pons* to the *cauda equina*, without making any contributions to the roots of the nerves. In what way they terminate in the *cauda equina* he is unable to determine, but states that in their intermediate course they are nowhere seen to form anastomoses with one another.

The "spinal bodies" are minute corpuscles of irregular form, three, four, or five cornered, provided with a nucleus, and with long radiate processes, resembling on a larger scale those of the bone-corpuscles. Dr. Stilling is of opinion that, by means of these processes, they form a connected system, but cannot speak positively as to the point. Their exact relation to the fibres is not determined. The most important point in their history is, that they are only found among the fibres of the anterior grey matter. Here they are distributed with some regularity through the whole length of the cord, being much more abundant, however, at those points from which the nerves pass off to the extremities. From this, and other relations which they have with the anterior roots of the nerves, Dr. Stilling infers that their office is dynamic, and is connected with the motor power of the roots. For similar reasons he supposes that the *substantia gelatinosa* bears an analogous relation to the functions of the posterior roots.

The transverse fibres of the cord are, according to the author, nothing more than the immediate prolongations of the roots of the nerves. The course he assigns to these is very remarkable, since, if his accounts be true, the anterior and posterior roots are continuous. The afferent fibres which enter the cord on one side pass out as efferent fibres on the other, a great part of them crossing to the opposite half of the cord. Some of these pass in front, and others behind the spinal canal; and, in consequence, a double crossing and manifold intermingling of afferent and efferent fibres take place,—the whole forming the anterior and posterior cross commissures. Under this view, the nerves may be regarded as great circles or loops, of which a very small segment is brought in contact with the longitudinal fibres proper to the cord.

The anatomy of the *medulla oblongata* is more complicated. The elements already described are interwoven in a more intricate manner, and new elements are added. The description of these last would be unintelligible without plates. The most interesting point in the account of this structure is the statement that the *vagus, glossopharyngeus,* and, in part, the *accessorius,* bear the same relation to the *hypoglossus,* as the posterior to the anterior roots of the spinal nerves. The remaining roots of the *accessorius* have a similar origin to that of the anterior spinal roots. It must be understood that here, as below, the roots of the nerves are continuous, and form in the same way the transverse commissures.

If we now review this brief summary of Dr. Stilling's observations, the facts that most strike us are those which concern the nature of the grey matter, the continuity of the anterior and posterior roots of the nerves, and, lastly, the relation of the fibres composing these, to the longitudinal fibres of the cord. Without adopting his anatomy of the two last-mentioned structures, which is, moreover, at variance in many essential points with Hannover's account of the same parts, I may remark that it derives support, as far as analogy can give it, from some recent researches by Mr. Newport, who has described exactly similar relations in the nervous system of the myriapoda.

The "spinal bodies" to which the author attaches so much importance will, probably, be considered by most anatomists as nothing more than a variety of the nucleated cells which are so abundant in most parts of the nervous system, of which grey matter is a constituent. For, although the greater number of such cells are round, yet many of them are furnished with processes exactly like those of the "spinal bodies," so that every gradation, in fact, may be found between the two. Not the less important, however, would be the fact, if true, of their being found exclusively among the fibres of the anterior grey tract.

I must remark that it rather damps our faith in Dr. Stilling's competence as a microscopic observer, to find that these bodies were described, in his first series of papers, as *dilated or varicose capillaries.*

But what casts still greater doubt over the results of his observations, is the fact of their having been made, exclusively, on specimens hardened by immersion in spirit. For, in spite of the statement that he has ascertained by comparison with fresh specimens, that the ultimate anatomical elements do not suffer much change from this treatment, the contrary is known to be the case,—at least, to a sufficient extent, to render microscopic examination confused and obscure. The increased hardness of the tissue, to obtain which the spirit is used, is the best of all proofs of a material chemical change.

It is to be hoped, however, that the evidence of observations, the greater part of which are at once so novel, and of such great physiological interest, will soon be examined anew by other anatomists, and under more varied modes of investigation.

Hannover's researches are, as far as the brain and spinal cord are concerned, of a more elementary kind. Like other observers, he finds that these structures are made up of two ultimate anatomical elements,—nucleated cells and fibres.

Cells are present in both wherever the substance of these structures is not white, but in the purely white matter not one can be found. They vary much in size in different parts, are generally round, often oval, sometimes triangular, or with one end drawn out into a point, while the other is rounded. He does not describe any differences between the cells of the brain and those of the spinal cord. In both, they are often seen, giving origin to fibres. He speaks very positively as to this fact; gives numerous drawings of it, and, indeed, seems to be of opinion that all the fibres of the brain, at least, originate from cells.

In the layer of white matter that has already been mentioned in another page, as forming the outermost layer of the convolutions, the fibres run parallel to the surface; but in the interior of the brain they pass from the convolutions to the base, and have, therefore, for the most part, a perpendicular direction. Numbers diverge into the cerebral nerves, while others proceed downwards, to form the cord. In direct contradiction to Dr. Stilling, Hannover states that the longitudinal fibres of the cord emerge in bundles to form the roots of the nerves, which are, in fact, according to him, continuations of them. In thus becoming nerve-fibres they offer no appreciable change of appearance.

Hannover also describes transverse fibres in the cord, but confesses that the great difficulty of the investigation prevented him from determining their true relations. He states that the peripheral terminations of the fibres of nerves are of several kinds. He agrees with other observers that in muscles they always end in loops, but denies that this is the case in the retina, or that they here form a plexus, as some have asserted. In his drawings of the retina the fibres are shown lying parallel to one another, and he gives it as his own opinion that they terminate in free ends. In the skin also, according to the author, they often end in the same way.

His description of nerve-fibres agrees pretty well with that of Remak, and he gives abundant proof that the central axis is a natural element of it.

The fibres of the sympathetic differ from the cerebro-spinal, in being much smaller and of more simple structure; in the absence of the central axis; and, also, in being garnished with nuclei along their whole course. He states, that in the ganglia they can be very distinctly seen originating from the true ganglionic cells—a point of some importance in relation to discussions now pending as to the independence of the ganglionic system.

Besides these, Hannover's work contains some observations on the nervous system of young and fœtal vertebrate animals, as also on that of a few invertebrata. The results offer no particular interest beyond that of confirming, in a general way, those already passed in review.

Lastly, must be mentioned his very accurate and excellent account of the retina, and of the expansion of the acoustic nerve. The anatomy of the retina, in particular, is admirably done, and appears to form the chief merit of the work: but as it would be doing this observer a great injustice to quote it apart from the beautiful plates by which it is accompanied, and without which it would be all but unintelligible, we may pass on to other matter.

It is pleasing to the mind to leave these regions of the vague and doubtful, for the more precise and purely experimental investigations of Dr. Marshall Hall. In this difficult and intricate part of anatomy there is good reason to believe that *function*, like optical phenomena for crystals, will for some time yet, continue to be a more searching and intimate exponent of structure, than any to be found in our present methods of direct examination.

In the *new* memoir on the *Nervous System*, by the author just named, there is not much to justify the title. With a good deal of old matter in a new dress, there are to be found a few novel remarks and suggestions of great interest; while through the whole we have continual occasion to admire that clearness and originality of thought, and that matchless ingenuity, which communicate a never-failing interest to all the productions of this distinguished physiologist, and may well give us cause to pardon occasional repetitions.

The following remarks on the functions of the *medulla oblongata*, although not strictly original, have much interest at the present time:—

"I have mentioned that the cerebral system is the seat of the emotions, passions, and sensations. This part of the nervous system seems, indeed, to be the precise point where the *psychical* and *physical* functions are *combined;* the cerebrum being otherwise the exclusive organ of the former, the true spinal marrow of the latter. When the cerebrum and cerebellum are removed, and with these all perception, volition, &c., there is still the unequivocal expression of *pain,* when any part naturally endowed with great sensibility is forcibly seized with the forceps."*

After quoting the well-known experiments of M. Flourens, on this part of the nervous system, he adds:—

"The medulla oblongata seems, therefore, to be the central organ both of psychical and excito-motor phenomena, and to differ from the cerebrum in possessing the latter faculty, and from the spinal marrow in possessing the former. It is like certain nerves, *mixed* in its functions, and the common boundary of the cerebral and true spinal systems inclusive. It is probably by irritating the medulla oblongata that tumours in the median lobe of the cerebellum excite the sexual faculty and organs."†

But the chief interest of the memoir consists in the beautiful and complete demonstration it gives of the purely reflex character of the respiratory acts.

In none of the former productions of this, or any other

* *Op. cit.*, pp. 33, 34, 35.

† *Op. cit.*, p. 51.

physiologist that I remember, was this demonstration so complete and satisfactory, that doubts on the point might not still be legitimately held, especially by those acquainted with M. Flourens' investigations. Dr. John Reid had, indeed, come very near it. But, with these new researches of Marshall Hall, this great truth is finally made a part of science. As its acquisition will, probably, in the end, be considered one of the greatest achievements of modern physiology, I shall transfer to these pages the author's account of the principal experiments on which the demonstration rests.

"If in a kitten, within the first nine or ten days after birth, we remove the cerebrum, divide the pneumo-gastric nerves, and open the trachea, the respirations gradually fall in number, until they become repeated but three or four times in a minute. If now, during the intervals between the respirations, we direct a stream of air forcibly upon the animal, or irritate the nostril, the anus, the tail, the foot, or, in a word, any part of the surface, or jar the table, an act of inspiration is immediately excited out of the usual course of these acts. This phenomenon is constant. It is the more marked, the younger the animal. In a kitten within ten days from birth, it continues for an hour or two. It is still observed in kittens of three, four, and five weeks old; but at a later period the experiment is apt to fail: much, too, depends on the degree of loss of blood which has been sustained during the removal of the cerebrum. In favourable circumstances, the slightest breath of air, the slightest motion, &c., induces an immediate inspiration.

"Such are the proofs of the influence of the tri-facial and spinal nerves in exciting acts of inspiration. The proof of the influence of the pneumo-gastric in exciting the same acts, is afforded by removing the cerebrum, and then dividing first one, and then the second of these nerves: the number of the acts of inspiration, *before* and *after* each of these steps in the experiments, gives us the measure of the influence of each organ, as they are successively removed. In one experiment the respirations were very frequent before the removal of the cerebrum; about forty after its removal; about ten after the division of one pneumo-gastric; and three after the division of the second. Acts of inspiration were still excited in the manner already described, by directing a current of air upon the general surface, or applying any other form of irritation."*

It is, certainly, a very beautiful result to show that such an important function as that of breathing, depending for its exercise on rhythmic movements continued incessantly through life, is maintained by a *purely physical* principle of action. The fact, however, is not difficult to conceive, seeing what modern invention has succeeded in accomplishing, by combining a simple moving power with mechanism.

In the rhythmic movements of the steam-engine we may find a not unapt parallel to the phenomena of breathing, which they so well mimic,—a parallel in which, perhaps, there is more and deeper analogy than meets the sense in the outward circumstance,—and one which may well abash the boldness of those who deny the theory here given of these phenomena, on the ground of the insufficiency of the means to the end attained. If man, with his limited powers, can accomplish so much, what may not the Great Contriver of the universe perform by similar means.

From other parts of this memoir it appears, that Dr. Marshall Hall still adheres to the theory he has already given of the acts of coughing, sneezing, vomiting, &c.; and although he admits that the last-named of these acts is sometimes of *centric* origin, he seems to be of opinion, that it is far more commonly a reflex phenomenon. In illustration of this view, he introduces into his diagrams, nerves from the vagina, uterus, ureter, gall-duct, and other parts, through which the impulse is supposed to find its way to the common centre of reflex acts—the cord—thence to be reflected on the stomach.

Nowhere, however, is it explained, how phenomena that are brought about by a principle of action admitted to be purely physical, and for the direction of which on particular groups of muscles, in order to the performance of these special acts, a corresponding grouping of nerves more or less closely about a central point, would seem to be a necessary condition, can be

* *Op. cit.*, pp. 262–6. Again,—"The same phenomena are observed in cases of partial asphyxia, induced by retaining the animal for a certain time in a limited portion of atmospheric air. When removed and laid upon the table, acts of inspiration may be excited by any of the means I have mentioned. A current of air induces an act of inspiration only: irritants applied to the general surface induce common reflex movements, together with the act of inspiration."—p. 265.

caused alike by impressions on nerves which impinge on the common centre at such various and distant points. It is remarkable that this, among the many apparently insuperable difficulties in the way of this theory, should not have occurred to its ingenious author. Some misgivings of it, however, may perhaps be discovered in the following passage:—"It is remarkable that the medulla oblongata should be the central organ of the reflex acts in so *many* physiological arcs!"

I would venture to suggest, that in connecting this idea with that of the possession of *sensation* by this structure, we shall find the best key to the difficulty. But, probably, the author considers this difficulty already solved by the instance of the act of breathing; an act quite as special in character as any of the group, and which experiments already cited clearly prove may be excited (alike) by nerves arising from the most distant points of the surface, and which enter the spinal marrow at points as remote from each other, as those supposed to convey the exciting impulse in the other instances.

But, on close examination, the cases will be found to be essentially different. Breathing is a permanent function, incessantly necessary to life, and the mechanism for it is permanently laid down in a fixed anatomy: and while experiments show, conclusively, that it may be kept up as a purely reflex act, by impressions on nerves of very distant parts, the experiments of M. Flourens prove, on the other hand, that, to this end, the integrity of some such central grouping as that already surmised is necessary, since the slightest damage done to *one point* in the medulla oblongata, instantly and finally abolishes the act. Is there a corresponding and independent arrangement of the same kind for each of the other acts here considered—for vomiting, sneezing, coughing, &c.? Admitting the agency concerned to be purely physical, this would be absolutely necessary under the supposition; for otherwise, the only conceivable source of variation that could be introduced to account for the varying re-action in these several acts, would be the admission of the excito-motory impulse to the cord, in more than common force, through particular channels.

Something, perhaps, may be claimed for differences in the *mode* and *intensity* of the impressions communicated. But, although it must be admitted that variety in these conditions will, of necessity, introduce some difference into the character of the resulting act, yet it can be clearly proved, by conclusive facts, that this is quite insufficient to account for the change in the whole course of innervation, and for that special direction of it into particular groups of muscles, while neighbouring muscles are thrown into complete relaxation, which characterise the acts here considered, and distinguish them from one another.

This is not the place to bring forward the evidence, by which I think it may be shown, that another and wholly different principle of action is here introduced, and is an initial and essential condition of the phenomena; in short, that the old theory, as Dr. Alison has all along held,—that these acts are governed by peculiar and appropriate sensations,—is the true one. It may, however, be remarked, that the act of laughter caused by tickling, alike, whether the *sensation* of tickling be excited through the nerves of the foot, or those of the sides, may be taken as an exponent of the true physiology of the whole group, and also as a clear illustration of the views here maintained.

It may also be added, that the prerogatives of sensation and emotion,—for the two blend in one principle,—in directing motor power into particular groups of muscles, with singular variety of result,—prerogatives, co-ordinate as they are in this respect with those of the *will*,—have never been sufficiently considered in their applications to physiology and pathology. It may be safely predicted, that much that is now, with a confidence that knows no doubt, very differently interpreted, will one day find its true explanation in these applications. In support of this, I need only point to the phenomena of hysteria and mesmerism, to be sufficiently understood, although the observation may easily be extended to other subjects.

I am sorry to be obliged to dismiss in few words Mr. Newport's "Anatomical and Experimental Researches on the Nervous System of the Myriapoda," published in the last number of the *Transactions of the Royal Society*. The gist of Mr. Newport's paper lies in the description of what he supposes to be a true reflex anatomy in these animals, in the shape of certain fibres, which, entering the nervous cord or axis from without, do not pass on to the analogue of the brain, but after a short and uninterrupted course, again emerge, without any break or any junction with other fibres, to supply corresponding parts to those, from which they were traced inwards. Other fibres of the same class pass directly across the cord from one limb to its fellow on the other side. In their passage through the cord all these fibres lie imbedded in grey matter, which is made up of nucleated cells. It need scarcely be remarked, how forcibly these details recal to mind the very analogous relations described by Dr. Stilling in the spinal cord of man.

Besides those already described, there are two other principal elements in the central nervous axis of these animals,—a ganglionic and an aganglionic tract,—which, as they may be traced directly downwards from the analogue of the brain, Mr. Newport regards as ministering to sensation and volition.

Moreover, as, after the removal of the cerebral ganglion, the motions of these animals, although still active, cease to give evidence of volition and sensation, (the entire absence of the latter, however, not being always perfectly clear,) Mr. Newport considers himself entitled to infer that these motions are purely reflex, and are executed through the fibres that take the peculiar course already described.

It is highly desirable that these observations should be repeated by other anatomists, especially as Helmholtz* regards the analogous relations, formerly described by Mr. Newport in the nervous system of the crab, as, in great part, the artificial result of the mode of examination.

However this may be, the present memoir is full of interesting matter of various kinds, and will well repay an attentive study. The physiologist cannot, in particular, be too strongly recommended to the study of the motor powers of animals, in their involuntary exercise, under every variety of anatomical condition in connection with which they may be exhibited.

There is, indeed, already much to hope from the continued and active investigation of this branch of physiology by so many able inquirers, and under such various modes of investigation. For it is not difficult to see that among the so-called reflex phenomena of the spinal cord, and the equivalent phenomena of the

* I have been unable to obtain this author's work on the microscopic anatomy of the nervous system of invertebrata—*De Fabrica Systematis Nervosi Invertebratorum.*

invertebrata,—in short, among the *purely automatic* phenomena of the nervous system,—lies the great theatre of future discovery in the physiology of this system at large.

Besides the beautiful applications of the discoveries already made, in this department, in elucidation of various functions of vital importance, the discoveries themselves have another and yet wider interest, in revealing the nervous agency to us under conditions of manifestation, in which its laws of action may be made the subject of precise and direct investigation. For a long time physiologists were baffled and embarassed in this inquiry, by having to observe this agency more or less in complication, fancied or real, with sensation and volition. Having, for their contemplation in these latter endowments, powers that are interpreted by no analogy, and that have no standard of comparison beyond their own sphere of action, an element was thereby introduced, which enveloped the whole inquiry in impenetrable and hopeless obscurity. But this obscurity has been finally dispelled by the discovery, for the first time consummated by the admirable investigations of Dr. Marshall Hall, that the motor powers of the nervous system, exercised through the spinal cord, survive the entire extinction of the psychical powers, by the removal of the structures which minister to these last, and may thus be obtained for observation in the most complete isolation.

Few seem to have apprehended that of this discovery, this may one day prove to be the most important result. Hereby stripped of the complication in which it usually acts in the living animal, the nervous agency is brought under observation in its simplest mode of manifestation, under the form of an impulse, acting at considerable distances, and through large masses of structure, so as to offer great advantages for determining the laws of its propagation. So that here, in our investigation into one of the highest prerogatives of animal life, we unexpectedly meet with the facilities and conditions of common physical inquiry; having, moreover, for our subject, the properties of a power, acting in implicit submission to mere physical impressions, and, as it would seem, with a purely physical result; for the ultimate result of all these spinal acts, call it muscular contraction, or what we will, *is an exertion of motor power that may be measured in mechanical units.* However this may be, if we would discover the nature and properties of this power, and its relations to other agents, the most philosophical course to take, and that which has been followed with greatest success in other branches of science, is to determine the laws of the phenomena effected by it. To whatever this may lead us, whether—as Dr. Holland puts the question—"to infer the identity of the nervous agency with any of the powers surrounding us in nature, or to admit that it has no type elsewhere in creation," the result must be, in either case, of vast importance.

To determine, therefore, *in their most general expression*, the laws of the phenomena now styled *reflex*, and to ascertain, in their simplest and ultimate form, the essential anatomical conditions of the manifestation of these phenomena, would, in itself, be a discovery of the highest order. Besides the vast addition that would thus accrue to our knowledge of the properties and nature of the nervous power, it is impossible to foresee what light it might not throw on the phenomena of sensation and will.

It is evident that these two principles of action, also, re-act on the world without by help of this same agency. The singular and varied adaptation of means to ends observed in numerous purely spinal and automatic phenomena, has already led physiologists to remark with what curious accuracy these phenomena "mimic the motion excited by the will and guided by the understanding."* Indeed, it was the very perfection of this mimicry that so long misled Legallois and other physiologists, and veiled from them the true character of the phenomena they observed.

And even in regard to other sensorial functions not resulting in muscular contraction, and which to us, in our present knowledge, seem of an *essentially* different character, in so far as these are the work of physical instruments, and are by that much subject to physical conditions, who can tell what elucidation may not come from this line of investigation? Relations of the most interesting kind may thus be unfolded, of which we can at present form no adequate or distinct conception. Of the possible extent of such elucidation, we may form the best estimate in considering the immense variety of phenomena, scarcely less different in outward character than are these varied actions of the nervous system, which, in optics and, still more, in electricity, the discovery of a

* See a very able article on "The Researches of Stilling, Hannover," &c., in the *British and Foreign Medical Review*, July, 1844.

single law, or the detection of a single agency, has brought under the interpretation of one common principle.

It must be remembered, too, that these actions of the nervous system are exercised through parts which are of common aspect, and consolidated in structure and functions; so that in discovering the essential anatomical conditions of the automatic phenomena, and in ascertaining the true relations of the ultimate elements of this anatomy, we may best hope for such interpretation as may be within our reach, of the relations of the more complicated structures which minister to the higher functions.

In the investigation of this department of the physiology of the nervous centres, pursued with these views, we have as yet only broken ground; for the admirable discoveries of Marshall Hall, important as they are, and well deserving to immortalise his name, carry us but small way towards these objects. Perchance their author might consider such objects altogether unattainable, or the pursuit of them, through this channel, a retrogade movement. And, undoubtedly, the difficulty, so well stated by Dr. Holland, does occur here, namely, "Whether we can reason upon a single agent, where the functions differ thus widely in their nature?" "What we may justly infer, however,"—to use the words of the same writer,—"looking to the common aspect of nervous structure, and to the connection of nerves of different classes with each other and with common centres, is its being an element of the same nature, capable of similar relations of quantity or intensity, of translation to different organs along the course of nerves, and of suspension by like causes of injury or disturbance."

If this be so, who can estimate too highly the privilege with which late investigations have endowed us, in presenting this agency to our observation under a form of manifestation so open to precise investigation; or who can judge of the degree of light which the discovery of its laws of action by these means, may probably throw on the phenomena of sensation and will. But if this discovery may justly be expected to be of the highest importance in a purely physiological sense, still more instructive must its other relations be in their bearing on the nature of the mechanical powers with which animals are gifted. For although it has been shown, by direct experiment, that spinal innervation is not the source, or, at any rate, not the only source of the contractile power of muscle, yet the astonishing effect of strychnia in increasing this, sufficiently shows an intimate and essential relation between the two.*

The nature and source of these powers in their joint exercise have long been a subject fruitful of speculation. Hitherto, however, the field has been almost wholly left to the metaphysician and the natural philosopher. Among physiologists, only the more speculative have ventured to break ground in it. The more practical men have always held aloof, either regarding such inquiries as a waste of time, or looking upon all hopes of their leading to any material discovery, as vain and delusive. Of late, however, clearer ideas have begun to prevail on the subject, and most physiologists of the present day see, with more or less distinctness, that the motor force which animals have at their command must be a purely physical power, and that its relation to other physical agents is not only a legitimate object of investigation, but one that almost certainly lies within the limits of discovery. Few, however, even now suspect the deep interest such a discovery would necessarily have for the practice of medicine, and its intimate bearing on every part of the same. Yet this strikingly appears from the large part which the motor powers of animals fill in the business of life, from the need of their incessant exercise for its maintenance in higher animals, through the acts of breathing and circulation; from the frequency of the sudden extinction of these powers by various deadly agencies; from their susceptibility of more or less rapid exhaustion; and the need of rest and *new material* for their repair.

Recently this, along with almost every other high subject in physiology, has been dealt with by the chemists; and it must be confessed that they have handled it with great boldness, and have apprehended its true relations, as an object of inquiry, with great clearness of thought. To them we are mainly indebted for the more distinct ideas upon it, which are beginning to make their way among physiologists; and in connecting with one another, by material and quantitative relations, the two grand characteristics of animal life, namely, the combustion of matter and the production of mechanical force, they have probably thrown out an idea

* I need scarcely remark, that the direct action of strychnia is confined to the cord, and only reaches the muscle through the nerve. This admits of experimental proof.

of deeper significance than any hitherto developed by those who have only looked at the subject from other points of view.

But the defect of the chemists has been, in confining themselves to the consideration of this force in complete abstraction from organisation and structure, so that when we come to apply their views to the actual physiology of the structures through which this power is exerted, we find ourselves as far back as ever. This gap the physiologist alone can fill up, and it is to his investigation of the laws of spinal or automatic nervous action that we must look as the most likely path to discovery. Here, then, he may find objects of sufficient scope and elevation to satisfy the cravings of the most lofty ambition. But he cannot expect to attain results commensurate with the most elementary of the objects here proposed, unless by keeping these objects in view, clear and well defined, and by following them out by a large scheme of investigation. Nothing less will avail than the systematic study of the agency which we have been considering, under all the various anatomical conditions in which it may be observed through the animal kingdom, or to which it may be reduced by experiment. It is only thus that we can hope to arrive at the essential conditions of its manifestation, and through these to gain some insight into its relation with other powers in nature, whose laws are better known. A scheme of such scope and purpose has never yet been acted upon.

In the anatomical part of the investigation, more systematic reference than is apparent in any of the works passed in review, should be made to embryology, where adult anatomy is so intricate and involved. It was an ingenious and fertile thought, to unravel the mazes of finished texture by observing the tissue in its laying down; to watch the shuttle of the weaver and mark the pattern of his woof.

More, still, as indeed may already be gathered from Mr. Newport's instructive investigations, is to be expected from comparative anatomy, with its yet more simple analysis of structure, and with all its chances of pregnant instances. Already one such—and, as it appears to me, one of no mean import—has come to light from this quarter, in the shape of a small fish, named the lancelet. (*Amphioxus lanceolatus.*) The anatomy of the nervous system of this fish, of which the absence of all outward distinction between the brain and spinal cord is, in itself, a very singular feature, is so remarkable throughout, that I shall quote, entire, Mr. Goodsir's account of it as given in the last number of Dr. Todd's *Cyclopædia*, referring the reader for further details to the original paper by the former, by whom it was first accurately described:*—

"When a portion of the spinal cord is examined under a sufficient magnifying power, it is seen to be composed entirely of nucleated cells, very loosely attached to one another, but inclosed in an excessively delicate covering of pia mater. The cells are not arranged in any definite direction, except in the middle third of the cord, where they assume a longitudinal linear direction, but without altering their spherical form. The black pigment formerly mentioned as existing, more particularly on the upper surface and groove, is observed to be more abundant opposite the origin of the nerves; and as it is regularly arranged in this manner in dark masses along the anterior and posterior thirds of the cord, the organ, in these places, resembles much the abdominal ganglionic cord of an annulose animal. Along the middle third the pigment is not so regular, but appears in spots at short intervals. When any portion of the cord, however, is slightly compressed, and microscopically examined, it becomes evident that there is, along the groove and mesial line of its upper surface, a band consisting of cells of a larger size than those composing the rest of the organ. Some of these cells, only, are filled with black pigment, but all of them contain a fluid of a brown tint, which renders the tract of large cells distinctly visible. When the compression is increased the cells burst, and the fluid which flows from the central tract is seen to contain jet black granules, which may be detected as they escape from the cells. The nerves consist of primitive fibres, of a cylindrical shape, with faint longitudinal striæ.

"The primitive fibres of a trunk pass off into a branch, in the usual way, without dividing; and where the trunks join the spinal cord, the primitive fibres are seen to approach close to it, *but without passing into it*. The greater part of the slightly protuberant origin consisted of the nucleated cells of the cord, with a few pigment cells interspersed; the exact mode of termination of the central extremities of the primitive nervous fibres could not be detected.

* *Transactions of the Royal Society of Edinburgh*, vol. xv., Part 1.

"One of the most remarkable peculiarities in the lancelet is the absence of the brain. Retzius, indeed, describes the spinal marrow as terminating considerably behind the anterior extremity of the chorda dorsalis, in a brain which exhibits scarcely any dilatation; but careful examination of the dissection of my own specimen, which I have also submitted to the inspection of Dr. J. Reid, and of other competent judges, has convinced me that the spinal cord, which may be traced with the greatest ease to within $\frac{1}{16}$th of an inch of the extremity of the chorda dorsalis, does not dilate into a brain at all. It may be urged that we ought to consider the anterior half of the middle third of the spinal marrow, where it is most developed, to be the brain, and all that portion of the chorda dorsalis which is in connection with the branchial cavity, as the cranium. That this does not express the true relation of the parts, is evident from the fact, that this portion of the cord, to its very extremity, gives off nerves which are too numerous to be considered as cerebral, but more especially from the mode of distribution of the first and second pairs, which in my opinion, proves the anterior pointed extremity to be the representative of the brain of the more highly developed vertebrata. A brain of such simplicity necessarily precludes, on anatomical grounds alone, the existence of organs of vision and hearing. These special organs, which are developed in the vertebrata, at least, in a direct relation with the cephalic integuments and the brain, could not exist, even in the form of appreciable germs, in the lancelet.

"The peculiarities in the structure of the spinal cord are not less remarkable than those of its configurations. It is difficult to understand, according to the received opinions on the subject, how a spinal cord destitute of primitive fibres and tubes, and composed altogether of isolated cells arranged in a linear direction, only, towards the middle of the cord, can transmit influences in any given direction; and, more especially, how the tract of black or grey matter, if it exercises any peculiar (excito-motory?) function, communicates with the origin of the nerves. The nerves also are remarkable, originating in single roots, and containing in their composition only one kind (cylindrical) of primitive fibres."

There can be no doubt that as our knowledge of this subject enlarges, and our inquiries begin to shape themselves out into definite questions, we shall find in this singular creature, in which the functions of the nervous system are reduced to such simple anatomical conditions, much elucidation of what is obscure and complicated in vertebrata of higher organisation. It is not difficult to see the close and important bearing which the anatomy of its spinal cord already has on the present course of speculation, on the subject of the so-called reflex phenomena.

I shall bring this part of my subject to a close with a brief notice of the observations, by which Bidder and Volkmann have lately sought to prove, on anatomical grounds, that the *sympathetic* is not, as some have taught, an offset from the brain and spinal marrow, but must be regarded as an independent and separate system. The evidence on which they lay most stress, and which they seem to consider as in itself sufficient to establish the point, is set forth in the following statements:—

1. The fibres of the sympathetic are much smaller than those of the cerebro-spinal, insomuch that the largest fibres of the former are separated from the smallest of the latter kind, by a broad gap, which plainly divides them into two distinct groups.

2. That in the frog, all the sympathetic fibres to be found in the roots of the cerebro-spinal nerves, would not, if added together, make one-third of the whole amount of such fibres contained in the peripheral branches of the sympathetic system.

3. That a very large proportion of the fibres of the sympathetic are derived from its own ganglia, in which they may be seen plainly originating from the well-known corpuscles which form the granular matter of these bodies. An observation to this effect has already been quoted from Hannover.

In further support of this statement it is asserted, that in the ophthalmic ganglion of the cat, the efferent fibres are four times, and in the cœliac ganglion of the same animal, three times as numerous as the afferent.

In addition to these more important facts, the authors bring forward certain anatomical characters, which serve to distinguish the two classes of fibres from one another. As these have already been quoted in nearly the same terms from Hannover, they need not be repeated here.

In answer to these statements, Valentin, who is the great champion of the doctrine that the sympathetic is a mere offset of the cerebro-spinal system, makes the following objections:—

1. That although the fibres of the sympathetic be, on the whole, smaller than those of the cerebro-spinal, yet there is a gradual transition from one series to the other. That no such gap as that mentioned by Bidder and Volkmann, as dividing these fibres into two distinct groups, really exists, since the space it fills in linear measurement is so small, as to fall within the common and necessary errors of observation. That, moreover, the fibres of all nerves vary much in size, and that many single fibres do so, in different parts of their course, so that the fact does not justify any physiological inference.

2. That the fibres which originate in the ganglia are not true nerve-fibres, but the pseudo-nerve-fibres described by Remak, and that these last make up the whole excess of efferent over afferent fibres.

According to Valentin's description, these pseudo-nerve-fibres are merely empty sheaths, thrown off from the ganglionic corpuscles, of the same kind as the exactly similar sheaths, which this anatomist describes as passing off from the same corpuscles to invest the true nerve-fibres.

These objections have drawn a rejoinder from Volkmann in defence of the original paper, and a very acrimonious discussion is now being carried on between these anatomists, into which it would be improper to enter here. It may be remarked, however, that this appears to be a question which cannot be finally settled on purely anatomical evidence; for the only absolute proof, on whichever side it may be, must rest on dynamic, or physiological grounds.

Terminating here this rapid sketch of the recent progress of the anatomy and physiology of the nervous system, we may pass at once to the electric phenomena of animals.*

* I regret being obliged to pass over a great number of original communications on points of minor importance. For the convenience of those who may desire further information, I subjoin a list of references to a few of the more interesting among them:—On "The Weight of the Brain and Cerebellum," see a paper by Dr. J. Reid, *Edinburgh Monthly Journal of Medical Science*, April, 1843; also in *Paget's Report*.—On "The Direct Influence of the Nervous Centres," by Volkmann, *Paget's Report*, and *Müller's Archiv*, 1842, heft. v.—On "The Structure of Ganglia and Origins of Nerves in Invertebrata," by Dr. F. Will, *Müller's Archiv*, 1844, heft. i. and ii.—*Observations on the Nervous System generally*, by Heine and Klencke. (An abstract of this work is given in *Valentin's Report*.)—*Physiologie des*

That such a transition should not be felt to be abrupt, but, on the contrary, should be almost expected, bespeaks, perhaps, better than anything that could be said, the high interest of the subject. A very elaborate work upon it has recently been published by M. Matteuci, whose numerous detached observations on these phenomena, which have, from time to time, appeared in the *Reports of the French Academy*, are, probably, already known to the members of this Association. The present work contains these observations in a collected form, together with many new experiments, and the author's matured views on the theory of the phenomena.

The first part of the work is taken up with proving:—First; the existence of electric currents in all *living* muscles in their passive state. Secondly; that of a current of constant course and direction peculiar to the body of the frog. And, lastly; the occurrence of an electric discharge in all muscles at the moment of contraction. All these points are well made out, clear from all fallacy arising from chemical change, difference of temperature, or other sources of electric disturbance at the point of contact between the apparatus and the subject of experiment.

The dependence of the phenomena on the living state, and their relation to certain organic conditions, are also clearly established. But the difficulty runs through them all, as to how far they have any essential *physiological* connection with the function of the living part, or be not rather mere epiphenomena of secondary importance.

Nevertheless, all these interesting relations are well worthy of being kept in view, in the present state of speculation in this part of physiology.

In regard to the exact source and nature of the phenomenon which occurs in muscles at the moment of contraction, the

Rückenmarkes, by Schultz. (This work contains an account of "Purkinje's "Observations on the Dura Mater of the Spinal Cord.")—*On the Physiology of the Nerves and Spinal Cord*, a Monograph, by W. F. Arnold.—On "The Nervus Accessorius," by Morganti, *Schmidt's Jahrbücher*, No. vi. 1844.—For Observations on the same Nerve, by Vrolik and Spence, see *Paget's Report*.—"Recherches Experimentales sur les fonctions du Nerf Spinal," by Cl. Bernard, *Archives Generales de Medicine*, Avril et Mars, 1844.—"Revision of some of the Doctrines contained in former contributions to the Physiology of the Eye," by Volkmann, *Schmidt's Jahrbucher*, No. v., 1844.

author expresses himself with great reserve and caution; and, in particular, he confesses himself unable to determine what relation, if any, it bears to the electric current, which may be discovered in all muscles in their passive state. But M. Becquerel, who has repeated M. Matteuci's experiment with him, asserts that the fact cannot be explained without admitting the occurrence of an electric discharge in the muscle; and seems to be confident in the opinion, that this discharge must have an essential and physiological connection with the act of contraction.

The experiment itself is very simple; and for the information of those who may wish to repeat it, I will here copy the author's own account of it, which is given in these words:—

"I place a frog, prepared in the ordinary manner, as in the original experiment of Galvani, on an isolated plane covered with oil-cloth. I then prepare another frog, so as to retain only one of its legs, with the bundle of nerves, from the spinal cord to the muscles of the leg, left hanging from it. To avoid error, it is necessary to have all the muscles of the thigh removed, and the nerves laid bare, and clear of other tissue. I then lay this filament of nerves on the thighs of the first frog, in such a way that the leg itself does not touch the thighs, and that the nervous filament connecting the two lies loose and free from traction. I then wait until the convulsive movements caused by the rapid preparation of the frog have entirely ceased. The experiment might indeed be made without this precaution, seeing the striking difference between these convulsive movements and the phenomena about to be described. If we now touch with a galvanic couple, the lumbar nerves of the first frog, the muscles of its thighs immediately contract, *and at the same instant the leg whose nerves lie across these thighs is suddenly convulsed.*"

When very lively frogs are used, the experiment succeeds equally well, on merely irritating the lumbar nerves of the first frog, without the galvanic couple. Numerous other experiments are related, which prove that when the galvanic couple is used, the convulsion in the second leg is altogether independent of any direct influence from it. M. Matteuci has also made the experiment, with the same success, in laying the nerves of a prepared frog's leg on the bare muscles of a living rabbit.

Whatever interpretation this phenomenon may ultimately receive, the fact is rendered doubly interesting by the discovery, recently made by M. de Quatrefages, that in certain transparent *annelida* every act of muscular contraction is attended *by a flash of light;* this flash being, in fact, the cause of the phosphorescence of the animals in question.*

The current which M. Matteuci describes as being peculiar to the body of the frog, he very satisfactorily shows to have been the source of the phenomenon originally discovered by Galvani, and well known as the first germ of the vast science which still bears his name. Of this current, all that seems needful to be said here is, that it passes through the whole mass of the animal, and is quite independent of the nervous system. In the prepared frog, in fact, it keeps its direction and full intensity, though the animal be stripped of spinal marrow, of the spinal and crural nerves, and of every visible nervous filament in the muscular mass of the thigh.

In his inquiry into the conditions which affect the manifestation and intensity of the proper current of the frog, the author found them identical with those which affect, in the same way, the muscular current already spoken of, as common to the muscles of all animals. What adds to, or takes from, the intensity of one of these currents, has the like effect on the other. Whence the inference is pretty plain, that these phenomena probably have their source, at bottom, in the same molecular changes. Nevertheless, many remarkable points of difference are observed between them, not the least of which is the fact, that whereas the muscular current is found alike in the muscles of all animals, the "*proper current*" can be detected in the body of the frog only.† In salamanders, eels,

* See several numbers of the *Annales des Sciences Naturelles*, for 1843, and also the *Comptes Rendus de l'Académie des Sciences*, for January 15, 1844, which contains a Report, by Milne Edwards, on M. de Quatrefages' researches. In this Report Milne Edwards states, that in examining very attentively certain "Beroés" of the Mediterranean, which shed a vivid light around them, he had already remarked that the light proceeds from the ciliated ribs with which the bodies of these zoophytes are garnished,—these being the very parts in which the organs of motion are seated. I regret that it does not fall within the scope and objects of this Report to notice, at length, the admirable series of researches by M. de Quatrefages, which are so rich in observations of general physiological interest.

† One effect of this current is, that whenever the circuit is closed by connecting the upper parts of the animal with the muscles of the lower extremities, a convulsion ensues. This observation acquires collateral importance, from the fact that this cause of muscular contraction has been entirely overlooked by those physiologists

turtles, and other animals among the nearest akin to frogs, it was sought for in vain.

In another part of the work there is a very important investigation into the phenomena that occur on making and breaking contact, when weak galvanic currents are made to pass through the nerves of living animals, or those of the prepared frog. I have not space to enter into the details of these experiments, but the most important result of them, stated in general terms, was this;—that after the first lively susceptibility has passed away, the passage of the current through the nerve in one direction, excites the muscles to contraction, while, in the opposite direction, it is without any effect of this kind. It is plain that this may one day become a fact of great importance, since it seems to imply a *dynamic* relation between the two agencies—galvanism and nervous force.*

But by far the most interesting part of M. Matteuci's work is that which relates to the phenomena of electric fish. This arises, no doubt, in part, from the inherent and undying interest of the subject. In the single instance in all nature, which these creatures afford, of a common physical agent placed under implicit submission to the *will* of a living animal—an agent, whose artificial development is the result of familiar and every-day experiment—there is, surely, something that must arrest the attention of the dullest witness of the fact.

In the possession of voluntary power over muscular contraction, we have, no doubt, *a fact of the same order;* but, here, the true nature of the phenomenon is obscured to us through our familiarity with it, and through our involuntary habit of con-

who have occupied themselves with investigations into the functions of the spinal marrow, and with whom, for obvious reasons, the frog has always been a favourite subject of experiment. It appears not at all improbable that some, at least, of the phenomena observed in these investigations, and set down to spinal agency, were, in fact, electrical. There is the more reason for supposing this, because M. Matteuci states that the electric phenomena occur, even when the circuit has to be completed through the body of the observer and the ground. We must infer, at least, from these facts, that no convulsions in the frog can be admitted as spinal acts, unless it be shown that they cease on destruction of the cord, or continue when the animal experimented on is placed on an isolated support.

* Through the kind courtesy of the author I was enabled to witness the whelo series of highly interesting experiments on which the statements in the text are founded, as they were performed by him in King's College, London, on his return from York.

templating it as a vital endowment, common to the whole animal kingdom indeed, but peculiar to it. But in the electric fish we have a case, in which the purely physical nature of the agent declares itself to sense, and has been made the object of experimental proof. Surely this is an instance of contact between the immaterial and the material—of submission of physical agency to psychical power, wonderful to contemplate.

May it not be viewed, without profaneness, as a symbol, humble, indeed, but distinct and, for that reason, valuable, of that entire submission to an INFINITE WILL, which reason and revelation alike teach us, is the condition of the powers of the universe.

The chief physiological interest of the subject consists, precisely, in the limitation of this extraordinary power to so few creatures. Here, for the first time in the animal kingdom, the physiologist meets with an anomaly, where no true anomalies are known; with the introduction of a new element, where all elements are in common; with a break in a chain whose every link is perfect. Without further inquiry, his mind revolts at the conclusion, and refuses the fact. And presently, and more and more as he dwells upon it, the conviction becomes pressing, that this must be only some other mode of manifestation of a power possessed by animals in common, and of whose nature it may thus become the exponent. And he is not slow to mark the numerous and deep-seated analogies which point to such a power in muscular contraction. In both, we find the same contact with the will, established by nerves, and subject to interruption by injury to, or division of, these; and, what is of deeper significance still, by nerves from the self-same anatomical tract. In both, the function *is exalted by strychnia and like agents*, and depressed by agents of opposite effect. In both, the exercise of function is followed by the same general exhaustion of nervous power, and, to each function, of its own force, requiring for the joint exercise of these *rest and a supply of food;* and, lastly, to the maintenance and full exercise of both, free respiration is, alike, essential. Surely, these numerous and deeply significant relations bespeak something more than a superficial analogy.

In particular, who can reflect on the intimate and perfectly equivalent relations of these two agencies to the nervous system, and fail to see that we are here upon the verge of discovery.

There is no part of M. Matteuci's work more interesting, than that which illustrates, in various ways, the entire dependence of the electric force of the torpedo on the nervous system, and the *dynamic* nature of the relations by which the two are connected. "I have observed," he says, "that when two or three of the nerves of the electric organ are divided, the discharge is afterwards limited to that part of the organ whose nerves are left entire."

Again, when all the nerves are divided, and the electric function is thereby destroyed, a discharge may be obtained by pulling with a forceps any one of the nerves, by the end in connection with the electric apparatus, just as a muscle may be made to contract, under the same conditions. But what is still more worthy of remark, is, that in this case the electric discharge is not general, but is strictly and accurately limited to that part of the organ which is supplied by the irritated nerve.

All these relations are illustrated, in a still more striking way, by the action of strychnia, which causes, on the one hand, a tetanic state of the muscles of the fish, and, on the other, and at the same moment, a rapid succession of electric shocks, *occurring of their own accord.* This is a fact that almost realises our idea of "a glaring instance." The direct and dynamic action of the drug, be it remembered, is *single*, and confined to the nervous centres. Only by its influence on these, does it affect the electric organ and the muscle. How clear, then, that its effects on these last are exact equivalents, and how striking the evidence of the intimate relation of both to the nervous force. Who can contemplate this circle of relations, without being reminded of the strictly analogous relations between electricity and magnetism, and other forms of the same power, which, to the minds of many, fore-shadowed the fundamental identity of these forms, long before this was finally established by experimental proof.

It has already been shown that, for the muscular force, and the electric force of electric fish, the conditions of action are identical, down to the minutest particulars. If this be true, of the conditions of their exaltation, it is not the less so, of those of their exhaustion, and of the resources needful for their repair.

"The Indians assured us," says Humboldt, in his *Personal Narrative*, "that when horses are put into a pond crowded with electric eels, not a single horse is killed the second day. These fish require *rest and abundant nourishment,* in order to produce or accumulate a large quantity of electricity."*

M. Matteuci appears to have felt the force of the various relations sought to be unfolded in the preceding pages; and they are presented in a condensed form in his book, in a table drawn up by M. de Blainville. The work, or rather M. Matteuci's part of it, closes with a philosophical discussion on the relations of electricity to the nervous force. In this discussion, so far from showing any undue bias to physical views,—which we might naturally have expected from the nature of his pursuits, and for which this might well excuse him,—he goes so far as to declare, that not only can no evidence of an electric current be detected in nerves, but that with a knowledge of the properties at present assigned to electricity, and of its established laws of propagation, it is impossible to conceive such a current to be confined within the limits of a nerve.

Having stated thus much, however, he adds, as the final result of all his meditations on the subject, that there does exist between electricity and the unknown force of the nervous system, an analogy, which, if not of the same degree of evidence, is never-

* All facts that put broadly and prominently forward the great and fundamental relation between the consumption of material, and the various forms of force, which animals have at their command, are of uncommon interest in the present state of speculation. The passage quoted in the text always struck me as being peculiarly interesting in that point of view. The following principles, laid down with such admirable clearness by Faraday, in his discussion of the *contact* theory of galvanic force, apply with the same strictness to these relations:—

"The contact theory," he says, "assumes, in fact, that a force which is able to overcome powerful resistance, as, for instance, that of the conductors, good or bad, through which the current passes, and that, again, of the electrolytic action where bodies are decomposed by it, can arise out of nothing. *That, without any change in the acting matter, or the consumption of any generating force,* a current can be produced, which shall go on for ever against a constant resistance, or only be stopped, as in the voltaic trough, by the ruins which its exertion has heaped up in its own course. This would indeed be *a creation of power*, and is like no other force in nature. We have many processes by which the form of the power may be so changed, that an apparent *conversion* of one into the other takes place. So we can change chemical force into the electric current, or the current into chemical force. The beautiful experiments of Seebeck and Peltier show the convertibility of heat and electricity; and others, by Oërsted and myself, show the convertibility of electricity and magnetism. But in no cases, not even those of the gymnotus and torpedo, is there a pure creation of force, a production of power, without a corresponding exhaustion of something to supply it."

theless of the same kind, as that which exists between heat, light, and electricity; a conclusion, we may remark, to which many other philosophers have come, and to which the present course of inquiry in all these subjects naturally tends.*

To complete my task, little more remains beyond a brief notice of the contributions made, during the past year, to the physiology of reproduction.

There must be few who have not been made aware, through the public prints, of the recent revival, with a feeling of very general interest, of the old theory of the analogy between the phenomena of *rutting* and menstruation, and of the essential relation of both, to the ripening and shedding of ova.

For the first promulgation of this theory in a complete form, we are, it seems, indebted to our countryman, Dr. Power. Unworthy attempts have been made, in various quarters, to rob him of his just merit in this matter. Although he brought forward no direct and undeniable proof of his theory, there can be little doubt that it was founded on a large induction, from analogy of various kinds; and it is not to be wondered at that he was satisfied with this, in a case in which all must admit that the force of analogy is unusually great.

Since the first publication of his views, Dr. R. Lee, Dr. Martin Barry, Dr. Paterson, M. Gendrin, M. Negrier, M. Pouchet, and others, have made investigations, which have contributed in no small degree to their final establishment. This seems to be now completely accomplished by the recent labours of Bischoff and Rokitansky. The former in discovering, at the period of *rutting* or *heat*, in the Fallopian tubes of various animals to which the male had never had access, the ova that had evidently just been cast off from the ovary, and the latter in proving, by an immense mass of evidence, the complete analogy between this function and that of menstruation, have furnished the only links that were wanting in the whole chain of evidence. It would be unjust, however, to the labours of former observers, not to state, that they had left but little room for doubt as to the last mentioned point.†

* I rejoice to learn that the Royal Society have awarded the Copley medal to M. Matteuci, for these most interesting researches.

† See, in particular, Pouchet's work, entitled *Theorie Positive de la Fecondation des Mammifères, basée sur l'Observation de toute la Serie Animale.* Paris, 1842.

The two works under review, and especially that of Bischoff,* are full of collateral matter of much interest. The impregnated ova which he found in the Fallopian tubes, had not acquired new envelopes, but were all naked, and in none was there any intimation of that peculiar division of the yolk, which marks the first stage of development of the impregnated ovum. He again brings forward numerous observations to prove, in opposition to Dr. Martin Barry, that the *vitelline membrane* of this physiologist has no real existence.

As an infallible sign that the ovum is becoming ripe and ready for discharge, he mentions the drawing out of the cells of the *discus proligerus* into forms approaching that of fibres; and, lastly, he states, that in animals, at least, there is no difference whatever between the *corpus luteum* of an unimpregnated, and that of an impregnated ovum.

With this account of the recent progress of science in relation to the preparations for the development of the new being, I might, not inappropriately, close this Report. But the physiologist is no longer confined to the consideration of the phenomena that occur within the narrow limits of living bodies. With the recent discoveries in chemistry he has extended the field of his speculation into realms that have the globe, with its atmosphere, for their narrowest boundary, and has recognised in one of the most universal powers of creation,—namely light,—the first cause of all organisation.

This sketch would, therefore, be very imperfect, were I to pass over without notice the speculations of the past year, touching that great interchange of matter ever going forward, in which the plant, by help of light, redeems back into organisation and life, for the future use of the animal, the matter which the latter is constantly reducing into dead chemical forms, and casting loose into air and earth.

Of these speculations, those of Professor Liebig on the supposed gradual defertilisation of the soil of England, claim our first attention, as they concern us all so nearly. His argument may be thus briefly stated:—

* *Beweis der von der Begattung unabhängigen periodischen Reifung und Loslösung der Eier der Säugethiere und des Menschen.*

The tissues of all animals are, it is well known, ultimately derived from plants. Plants must, therefore, contain, as essential constituents, the inorganic elements, as they are called, which are necessary to animal bodies. These elements are, again, derived from the soil; but analysis has shown that, here, their supply is limited. To maintain the fertility of the soil, therefore, and restore the balance, need must be, that the animal returns to the earth the whole amount of these elements which the plant has taken from it. The elements thus limited in their supply are, it is scarcely necessary to remark, the phosphates.

Now, Professor Liebig argues, and the fact cannot be gainsaid, that in effect of their domestic arrangements, the English annually waste into the sea a vast amount of these phosphates. In this he foresees, unless remedy be applied, the gradual defertilisation of our land, already to be discerned, he says, in the barrenness of numerous large tracts, that were once known to reward the toil of the husbandman with an abundant harvest. Of course, these remarks do not apply to the great extent of waste land, which has never yet been brought into sufficient cultivation to yield food for cattle. In these, he admits, we still have a large source in reserve.

In regard to these views, the first thing that strikes one is, that they seem very like an impeachment of the wisdom of Providence. That the fertility of a whole country, and the welfare of a vast people, should be made contingent on the practices which this people might adopt to ensure cleanliness, does not look like that self-acting compensation—those beneficent provisions for the maintenance of life, which our investigations have, hitherto, everywhere disclosed to us in the great scheme of animated nature. And although arguments from final causes are unsafe guides, and are inadmissible as substitutes for direct evidence, in dealing with scientific questions, they at least require that the evidence of conclusions that stand in striking opposition to them, should be well sifted.

In the present case, it will be found, on further examination, that Liebig has over-rated our danger, and, in particular, that he has not taken into account the various channels through which the ocean is made to give back her tribute. Nothing is said of the enormous weight of fish eaten every year by the inhabitants of these islands, a great part of which, at least, leave their inorganic remains with us.*

Again, is it a work of accident, that six hundred British ships laden with guano—with phosphates, in short—are at this moment bending their sails to our ports; or, rather, may we not discern, here, one of the thousand ways by which, in the great scheme of Providence, this wonderful balance is ever being restored. In this great economy, in fact, loss is impossible. We may cast our phosphates into the sea, but there will be no waste. In the next moment, the floating and errant particle is redeemed into life by the plant or animalcule; these, again, are made the food of fish, and, through these last, the ocean is made to disgorge her prey to the inhabitant of the land. And thus is closed up and made perfect, that great cycle of life, of which the law is, never-ceasing change without loss.

The recent investigations of Professor Edward Forbes, touching the distribution of living beings in the depths of the ocean, have a very curious bearing on these speculations. The doctrine already incidentally alluded to, that plants, alone, have the power to organise simple chemical bodies, and that the tissues of all animals ultimately come out of plants, is well known, as the great deduction of modern chemistry. It is therefore rather startling to find the researches of Professor Forbes giving, at first sight, a direct and practical denial of it. The general result of his observations, first in the British seas, and more lately in the Ægean, was "to define a series of zones, or regions, in depth, and to ascertain *specifically* the animal and vegetable inhabitants of each. Regarding the tract between tide-marks as one region, which may be termed the *littoral zone*, we find a series of equivalent regions succeeding it in depth. In the British seas, the littoral zone is succeeded by the region of *laminariæ*, filled by forests of broad-leaved fuci, among which live some of the most brilliantly-coloured and elegant inhabitants of the ocean. This is the chosen habitat of Lacunæ, of Rissoæ, and of nudibranchous mollusca. A belt,

* It may seem a strange question, but it is one that admits of being asked,—Whether, if these phosphates were cut off from the sea, we might not shorten the breed of those fish that frequent our shores? Certainly, at least, we should diminish the supply of the mineral constituents of the microscopic beings which pullulate in such myriads in our deltas; beings, in which fish find one of their chief sources of nourishment, and which form one of their chief attractions to our coasts.

generally of mud or gravel, in which numerous bivalve mollusca live, intervenes between the laminarian zone, (in which the flora of the sea appears to have its maximum,) and the region of corallines, which, ranging from a depth of from twenty to forty fathoms, abounds in beautiful flexible zoophytes, and in numerous species of crustacea and mollusca, to be procured only by means of the dredge. The great banks of monomyarious mollusca, which occur in many districts of the northern sea, are, for the most part, included in this region, and afford the geologist his richest treasures. Deeper still is a region, as yet but little explored, from which we draw up the more massy corals found on our shores, accompanied by shell-fish of the class *brachiopoda*. Vegetables become fewer and fewer as we descend, and, at the end of one hundred fathoms, are reduced to a single species,—a *nullipora*. A small space below this, and no plant is found by the present means of search." But, strange to relate, through a vast region below,—a region which, in the Mediterranean, exceeds all the others together,—Professor Forbes finds the depths still inhabited by animals. These are, indeed, few in number; but even at the depth of 230 fathoms, as many as eight species of testacea are found. If it be true that animals ultimately derive all their materials from plants, where do these *testacea*,—creatures, for the most part, fixed to the bottom,—find their vegetable food? Doubtless, it will in the end be found that, even here, some kind of vegetation still survives; plants floating, perhaps, in shapeless and imperfect forms, that elude our present means of search, and that catch a feeble spark of life, from the last few rays that find their way to these regions of perennial gloom.

Perchance, too, we may discover some clue to the solution of this enigma, in the presence of some of those singular species of infusoria, which, by virtue of their green parts, share with plants in the power of decomposing carbonic acid, and organising dead matter; while, in all their other functions, they resemble other animals,—curious creatures of bi-fold life, which recent researches tend to show, are employed on a vast scale in the living economy of the globe. However this may be, when we reflect that light is the very life of plants, and its agency the first condition of their existence, this discovery of animals in regions, in which we know eternal darkness reigns, must, at least, be regarded as one of peculiar interest at the present time. The fact naturally leads us to the last subject to be treated of here, namely, the influence of light on vegetation. Among the many important investigations lately made in this matter, those by Dr. Draper have the most interest, in relation to the point under consideration. This distinguished observer justly remarks, that the investigation of the decomposition of carbonic acid by leaves has, by its connection with modern chemistry and physiology, acquired extraordinary interest. For, if we bear in mind, that this decomposition is the starting point of the organisation of dead matter; that, with this agency of leaves, the series of organic atoms begins, which becoming more and more complicated as they pass through the plant, finally end in flesh, blood, and nervous matter; it is evident that correct views on this first conversion must be of no common importance.

Now, Dr. Draper has shown by accurate experiments, and in opposition to what might have been surmised, that the elements of the spectrum concerned in this agency, are not the heating, nor the chemical, or, as he calls them, the "tithonic," rays,—for the help which these give is quite insignificant,—but the rays of highest illuminating power. That it is light, itself, in fact, in the common acceptation of the term, that is the great creative power.

It would lead me too far from the scope and objects of this Report, to pass in review the kindred researches of Herschel, Melloni, Talbot, Hunt, and others. Suffice it to remark, that at this point physiology connects itself with the most beautiful investigations of our time,—investigations from which will date a new era in *physics*, and which, in the discovery, in the sunbeam, of agencies so widely differing in their sensible qualities, and in the nature of their effects, but yet identified by common properties of refraction and polarisation, already seem to point to that great consummation, which the most eminent philosophers of modern times have ever anticipated as the ultimate and highest generalisation of science—namely, the blending all the imponderable agencies of the universe into one essence.

What sublime views here open before us! Already a beautiful truth broke upon our minds, in the discovery, by Dr. Draper, that the same agent which brings in its beams the witness, at once, of countless worlds at infinite distances, and of the objects immediately around us, in all the rainbow hues with which itself has decked them, should also be God's great dispenser of life, and first cause in the work of organisation. Withdraw its lovely and benignant influence, and the earth would be, again, without form and void, and would float through space a lifeless corpse.

It is only when we strive, as far as our limited imagination will allow us, to contemplate this radiant and ethereal messenger, in all these its wonderful attributes, that we can rise to some vague sense of the vast moment of that mandate, which went forth at the beginning of the world, and which is announced in Holy Writ in these words of sublime simplicity:—

AND GOD SAID, LET THERE BE LIGHT; AND THERE WAS LIGHT.