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April 29. 1842

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OBSERVATIONS

ON THE

CHANGES PRODUCED IN THE BLOOD,

&c.

BY CHARLES J. B. WILLIAMS, M.D. F.R.S. &c.

[*From the London Medical Gazette.*]

OBSERVATIONS

ON THE

CHANGES PRODUCED IN THE BLOOD IN THE COURSE OF ITS CIRCULATION:

WITH EXPERIMENTS.

Read to the Royal Medical Society of Edinburgh in 1823*.

WITH ADDITIONS AND REMARKS ON DISCOVERIES AND OPINIONS SUBSEQUENTLY PUBLISHED.

By Chas. J. B. Williams, M.D. F.R.S. &c.

This essay is designed to comprehend a consideration of those changes in the proximate principles of the blood that are the result of the several organic functions of circulation, respiration, and secretion. Before we can satisfactorily enter on this subject, it will be necessary to examine briefly the general composition of the blood in health.

On the Composition of the Blood.

Since the researches of Berzelius, Marcet, and Brande (for I think it unnecessary to detail opinions of an earlier date), the blood has been considered as a rather complex fluid, containing at least four animal principles, namely, albumen, fibrine, a peculiar colouring matter, and an imperfectly defined matter, usually called extractive; the rest consists of water, holding in solution a quantity of saline matter.

[The recent analyses of Lecanu †, Berzelius ‡, and Babington, give a kind of fatty matter as a constant ingredient in the blood, which, according to the former chemist, contains five or six parts in a thousand; part of it is solid and crystalline, like cholesterine, and the remainder oily and saponifiable. This discovery throws some light on the deposition and removal of fat in the body*. This fatty matter, when in excess, gives a milky hue to the serum; and in a case examined by Dr. Christison, it amounted to five per cent. †]

It would be convenient, for the illustration of the questions we are entering on, to consider the blood as a homogeneous compound of the substances here enumerated, without allusion to the spontaneous separation which occurs when it is removed from the body; but since almost every writer has treated of the constituents as presented distinctly by this phenomenon, we shall briefly advert to it.

Blood, within a few minutes after its abstraction from a vessel (the time being liable to some variation from the influence of several circumstances), begins to exhibit a number of small coagula near the surface, which, by extending and cohering, gradually form one continuous stratum of weak aggregation. The blood below is shortly coagulated in like manner, and the whole mass appears then to have undergone a change. Presently, however, the edges of the coagulum on the surface begin to recede from the sides of the containing vessel; the size diminishing, and the consistence increasing, until, in the course of some hours, the blood presents the perfect separation into crassamentum and serum. This phenomenon has been generally attributed to the solidification of the fibrinous part of the blood, which entangles the colouring

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* In this essay will be found several opinions which have since been brought forward as new by others. It formed the subject of a thesis published in Latin in 1824; and in 1826 an abstract of it was published in the Transactions of the Medico-Chirurgical Society of Edinburgh. The additions are enclosed in brackets.

† Ann. de Chimie et Physique, xlviii.

‡ Traité de Chimie, 1833, t. vii.

* See the writer's Essay on Obesity, in the Cyclopædia of Practical Medicine.

† Edin. Med. and Surg. Journ. April, 1830.

matter, as in the meshes of a net, and by its contraction separates them from the serum. [Berzelius and Dr. B. Babington have confirmed this view, by shewing that fibrine exists in the blood in a liquid state, and that the firmness and contraction of the coagulum are due to the attraction which the particles of fibrine, on becoming solid, exert towards each other.]

It would appear from the experiments of Sir E. Home and Mr. Brande *, that carbonic acid gas is extricated from the blood in the act of coagulation. I am inclined to doubt that this occurs to any extent when the action of external air is excluded; for I have been unable to perceive any appreciable extrication of gas when blood is allowed to coagulate in a Torricellian tube inverted over mercury. [My doubts on this subject are further warranted by the subsequent experiments of Dr. John Davy and Dr. Christison, who could not extricate any gas from blood during coagulation when the air was excluded. Gmelin and Tiedemann have recently arrived at the same results †. When air is present, the extrication of gas certainly does take place in the manner described in a subsequent part of this essay, where Dr. Stevens' view of this matter will be noticed.]

John Hunter concluded, from an experiment on the blood of a turtle, that heat is evolved during its coagulation; and notwithstanding the disparity of cases, this conclusion appears to have been extended to the coagulation of blood in general. The author of an article on Blood, in Rees' Cyclopædia, and more recently, Dr. Gordon ‡, (although their experiments are not quite conclusive), have rendered it probable that a slight evolution of heat attends the coagulation of blood, both arterial and venous, of warm-blooded animals. [Sir C. Scudamore confirms this statement. This evolution of heat is doubtless the chemical result of the solidification of the liquid fibrine; but the coagulation of blood, and of lymph effused, as well as their fluidity in their proper vessels, are phenomena of *living chemistry*, which we do not yet understand. The reason of the fluidity of the blood assigned by Dr. Stevens, the presence of saline matter, is quite inadequate, for this undergoes no diminution or change during the few minutes which precede the coagulation; and this phenomenon takes place whether the blood be excluded from or exposed to the air. Warmth rather accelerates it; and Berzelius states that blood frozen before coagulation, will, on thawing, present the usual separation into crassamentum and serum *.]

I shall now describe the nature and properties of the several component parts of the blood, as far as they have been investigated.

Albumen is known to exist in two states; in that of a colourless glary fluid, as in the white of an egg, or in a more diluted state in the serum of the blood; and in that of a white opalescent solid, varying in consistence according to the quantity of water with which it is united. Liquid albumen may be converted into this latter state by the operation of heat, galvanism, acids, and alcohol. The two latter have been supposed to produce this effect by abstracting the water which holds it in solution. The action of heat on albumen has not been so readily explained. The opinion of Dr. Thomson is, that heat, by increasing the elasticity of the water and soda, which by a weak affinity hold albumen in solution, gives their particles a tendency to separate, and thus enables the particles of albumen to obey the attraction of cohesion which exists among themselves, and thus to form a solid mass †. This explanation, which appears to me equivalent to the expression, that the addition of a certain quantity of heat *deprives* water of the power of holding albumen in solution, would still leave the relations of these bodies an anomalous exception to a law, otherwise general—namely, that heat *increases* the solvent power of water. [The opinion given by Dr. Turner does not appear to me more explanatory — "that albumen combines directly with water at the moment of being secreted, at a time when its particles are in a state of minute division; but as its affinity for that liquid is very feeble, the compound is decomposed by slight causes, and the albumen thereby rendered quite insoluble. Silicic acid affords an instance of a similar phenomenon ‡." Now there is this marked

* Phil. Trans. 1818.
† Pogendorf Annalen, xxxi.
‡ Ann. of Philosophy, vol. iv.

* Traité de Chimie, t. vii.
† System of Chemistry, vol. iv. p. 401.
‡ Elements of Chemistry, 1834, p. 938.

difference between liquid silica and liquid albumen; the former cannot be evaporated to dryness without losing its solubility: liquid albumen may be dried at any temperature below 150°, and the transparent solid thus formed is not only soluble again into liquid albumen, but, whilst dry, it may be heated to 212°, without losing its solubility. This fact proves that neither a nascent state, nor one of minute division, is necessary to its solution, and that a change of either the combination or arrangement of its particles takes place before albumen passes from the liquid to the coagulated state. This corresponds with the explanation given further on*.]

MM. Prevost and Dumas, of Geneva, in a memoir rich in observations of great value to the physiologist, have recently pointed out a number of properties which would seem to rank coagulated albumen as an acid capable of combining with bases; and render it probable that in serum and white of egg it exists in union with soda, which makes it soluble in water. Thus any thing which is capable of abstracting the soda will cause the precipitation of solid albumen. This effect is produced by acids and alcohol; the action of the former is obvious, and that of the latter probably depends on the affinity which caustic soda exerts for this vehicle, presenting a phenomenon the converse of that resulting from the action of water on some metallic salts. In confirmation of this view, I find that albumen shows stronger marks of alkalinity if a little alcohol has been added to coagulate it. [I think, however, that this effect of alcohol may with more reason be ascribed to its affinity for water as well as for soda. Alcohol in like manner precipitates gums from their solution in water.] But what sets the acid nature of albumen in a clearer light, is the effect of galvanism on it. To Mr. Brande we owe the discovery that when white of egg is subjected to the galvanic influence, coagulated albumen is deposited at the positive pole, while caustic soda may be detected at the negative pole. Prevost and Dumas observe, that when this experiment is performed with wires of an oxidable metal, the albumen forms a compound with the oxide. [Lassaigne, however, attributes the coagulation of albumen at the positive pole to free muriatic acid resulting from the decomposition of the muriate of sóda combined in albuminous fluids; but the quantity of muriatic acid thus developed is too small to produce such an effect. Galvanism is a far more delicate test for albumen than muriatic acid is, and *accumulates* at the positive wire a coagulum of albumen from the weakest solutions, which it could not do if liquid albumen were not (to use the language of Faraday) an *electrolyte* compound; for single *ions*, or uncombined bodies, are quite indifferent to an electric current.]

This view of the nature of albumen receives some support from some experiments which I have made to determine how far it is capable of neutralizing alkalies. Thus, to the white of egg, diluted with three times its bulk in water, weak muriatic acid was added, to such extent as exactly to neutralize the excess of alkali, without occasioning any coagulation. Some albumen similarly diluted was coagulated by muriatic acid, and pure potass very cautiously added until the coagulum was re-dissolved; the solution, when tested at this period, scarcely exhibited alkaline qualities. A similar result was obtained with the coagulum by heat. [Very similar results have since been obtained by Berzelius, who neutralized the alkali of serum by means of acetic acid, without causing any coagulation. He also formed a liquid albumen artificially, by dissolving fibrine or coagulated albumen in a solution of potass, and neutralizing the excess of alkali by acetic acid. The liquid albumen thus formed was coagulable by acid and alcohol, but not by heat.]

On this view, MM. Prevost and Dumas attribute the coagulation of albumen by heat to the saturation of the alkali by carbonic acid; which they conceive to be formed by the decomposition of a small quantity of the albumen. It has been ascertained by Fourcroy, that an egg is incapable of forming a firm coagulum unless it has for some time been exposed to the air. This exposure is attended with an absorption of oxygen; and I am inclined to believe that this oxygen, at a high temperature, abstracts a portion of carbon from the albumen, and thus carbonic

* Examen. du Sang. &c. Biblioth. Universelle, tome xvi. 1821.

acid is formed, which gives rise to the coagulation. I have failed in finding support in experiment for this part of the theory, for I have been able neither to extract any considerable quantity of carbonic acid from albumen coagulated by heat, nor to coagulate liquid albumen by passing through it a current of carbonic acid gas. These results do not, however, disprove the correctness of the explanation given above; for the first may in some degree be accounted for from the insoluble and impervious structure of the coagulum impeding the action of the agents; and the analogy in the latter case is not perfect, for in the first place the affinity of carbonic acid for soda may, at ordinary temperatures, be inferior to that of albumen; and secondly, at the degree of heat at which albumen coagulates, the change sustained in its composition by the loss of a portion of carbon, may also be requisite to render its affinity for soda inferior to that of carbonic acid.

[It now appears to me, that the coagulation of albumen by heat may be better explained by referring it to this change of composition than by assuming the formation and precipitating power of carbonic acid. The fact adduced above, that the white of egg will not coagulate firmly by heat, unless it has previously imbibed oxygen from the air, implies a condition which, under the influence of increased heat, is most likely to work a change of internal composition. Many other animal and vegetable bodies have this property of absorbing oxygen, and retaining it until it either affects their own composition or is transferred to other matters with which they may be brought in contact. On a review of all the characters of albumen above described, together with its power of uniting with the metallic oxides, as pointed out by Berzelius, we can scarcely evade the conclusion of this chemist, as well as of Prevost and Dumas, that it exerts many of the properties of an acid, and that its habitudes can be best explained on this view. That free or coagulated albumen does not redden litmus paper, does not disprove its acid nature, for its insolubility may prevent this; and, besides, there are several substances which are recognized as acids and bases, which exert little or no reaction on test paper; such as silicic acid, hydrocyanic and tannic acids, several vegetable alkalies, and the various ethereal compounds of hydrocarbon.]

The experiments of Berzelius* seem to favour the idea that albumen is capable of neutralizing acids; thus affording an additional example of animal matter possessing both acid and alkaline qualities. [Like *cystic oxide*, which Dr. Wollaston found to unite with acids and bases.] It is probable, however, that in these experiments the composition of albumen is changed; for I find that a solution of potass disengages ammonia from the compound of albumen and sulphuric acid.

The next principle to be noticed, is fibrine. Berzelius has shewn that its characters, in relation to re-agents, scarcely differ from those of coagulated albumen. Thus it is dissolved by alkalies, and the solution may be coagulated by heat, alcohol, and acids. The habitudes of acids towards it are nearly the same as with coagulated albumen. The only points which seem to characterize it are its fibrous structure and ultimate composition. [We are still without any exact means of distinguishing between these substances. The liquidity of fibrine during life, which will be noticed presently, can be chemically imitated only by the means which dissolve coagulated albumen. Neither can an organized structure be assumed as exclusively characteristic of fibrine, for it is not yet known whether the globules to be presently described are fibrine or albumen. As to their ultimate composition, a difference has certainly been found, but the nature of that difference has been so variously stated, by even the latest analysts, that it is a questionable ground of distinction †.]

Before describing the colouring matter, it will be necessary to consider the

* Med. Chir. Trans. 1812.

	Carbon.	*Hydrogen.*	*Nitrogen.*	*Oxygen.*	*Analysts.*
† Albumen	52·883	7·54	15·705	23 872	Gay-Lussac and Thenard.
	50·	7·78	15·55	26·67	Prout.
	52·650	7·359	15·550	24·484	Michaelis.
Fibrine	53·36	7·02	19 984	19·685	Gay Lussac and Thenard.
	50 440	8·228	17·267	94·065	Michaelis.

mechanical relations subsisting between it and the fibrine of the blood. The discovery that blood consists of a limpid fluid, with coloured particles suspended in it, very soon followed the invention of the microscope; but the form of these globules has been the subject of a wonderful variety of opinion. Passing over the earlier micrographers, I shall only notice the results of some recent observations.

Sir E. Home, from the observations of M. Bauer *, describes them as *spherical* bodies, consisting of an internal globule, which is colourless, enveloped in the colouring matter. In less than thirty seconds after the removal of the blood from the body, he thinks that they lose their proper form; the colouring matter separates from them, whilst the central globules run together, and coalesce in groups or lines. He found that the fibres of fibrine and muscle are composed of globules very similar in appearance, united so as to form threads.

The observations of Prevost and Dumas† appear to me more complete than any that have preceded them. From an attentive examination of the blood, both recently drawn and whilst still circulating in the vessels of a frog's web, and of a bat's wing, they were led to conclude (as Hewson did long before) that the form of the red globules in the mammalia is a *compressed spheroid*, or rather that of a *lens*, with a central *projection;* while in birds, and animals with cold blood, it approaches nearly to an *ellipse*. The central globules they found to be spherical, transparent, and colourless, and confirmed all the results of Sir E. Home's examinations as to the globular structure of fibrine and muscular fibre, and, moreover, identified the size of their component globules with that of the colourless globules of the blood.

[How often does it fall to the lot of the student in physiology to have to unlearn what he has been at pains to acquire! Truly, the microscope is a great discoverer; for each successive looker through it has found something new in the microcosm of the blood, and something wrong in the descriptions of those who have looked before. The account of Prevost and Dumas, complete and correct as it seemed, must now give way to the view given by the superior compound achromatic microscopes of my friend Dr. Hodgkin, and Mr. Lister*. These gentlemen, after minute and varied examinations, came to the conclusion that the particles of the blood are *flattened transparent cakes*, rounded at the edges, and with a slight central *depression*. They could perceive no central globule, nor did they observe that change of form described by Home to occur within a minute of the blood leaving the body, except when water was mixed with the blood, when the particles immediately became spherical. This account is the more entitled to our confidence, as it nearly accords with that given by that scientific observer, Dr. Young. They have been also in great measure confirmed recently by Professor Müller, of Bonn†, who found the particles in all the mammalia circular and flat. He discerned, however, in their centres a harder globular nucleus; and as this has been distinctly described by nearly all observers, we can scarcely doubt its existence. The entire particles preserved their shape for several days in serum, and in several saline solutions; but water first rendered them globular, and then jagged, from its solvent power on the colouring matter. The central globules are not acted on by water; and from this circumstance, and from their similarity in form, Dr. Müller considers them to be identical with the globules which he has found in lymph and chyle.]

Prevost and Dumas further satisfied themselves that the coagulation of the blood is caused entirely by these central globules abandoning the colouring matter, and coalescing so as to form one continuous mass, which is the fibrine; while the colouring matter is only mechanically retained by it. [This description of the coagulation of the blood, simple and tempting as it seems, must again give way to the older notion, revived by Berzelius, Babington, and Müller, that liquid fibrine exists in the blood independently of the central globules; by its concretion gives form to the coagulum, and constitutes the buffy coat, the

* Phil. Trans. 1818.
† Examen du Sang, &c. loc. cit.

* Appendix to the Translation of Edwards on the Influence, &c. 1832. P. 424.
† Pogendorf's Annals, xxv. 513. 1835.

coagulable lymph of inflammation, and the gelatinous clot of lymph, and of some dropsical effusions. Neither Dr. Hodgkin nor Professor Müller could perceive any disposition in the colouring matter to abandon the central globule when no water was added; on the contrary, the particles preserve their form, and unite together in rows, or *rouleaux*, in their entire state, until after many hours, when new chemical actions probably begin, and the particles then become notched, or jagged at their margins.]

MM. Prevost and Dumas also traced an exact similitude between the colourless globules of the blood, and those of chyle, milk, and healthy pus. But the most curious instance in which they recognized globules of a similar nature was in albumen, coagulated either by galvanism or by heat. This they describe as being composed of globules, which, as to form and size are identical with those of the blood, of milk, chyle, &c. My own examinations have only partially confirmed these results. I have perceived a distinctly globular appearance in albumen coagulated by galvanism, which in some instances appeared disposed in regular lines, decussating one another like the muscular coat of the urinary bladder*; but as to form and size, I have observed them not only different from the globules of blood, but presenting also a considerable variety among themselves; and I confess myself somewhat incredulous as to the reality of the uniformity mentioned by these authors. [It would appear that my incredulity did not go far enough; for the observations of Messrs. Hodgkin and Lister are opposed to the existence of globules, not only in pus and coagulated albumen, but also in muscle, nerves, brain, cellular, serous, and every other texture. If these should be confirmed, there is an end of the simple system of homogenesis of structure, of which M. Bauer's microscope seemed to have given us a glimpse†.]

The colouring matter has been generally considered soluble in water; but Prevost and Dumas have shewn that this is not the case. When a jet of water is made to play on the crassamentum, the colouring matter is conveyed away in a state of such minute division, that it is capable of passing through filters, and from its own transparency it does not impair that of the water; but its fragments, which are discoverable by the microscope, subside by rest in the form of a pretty dense red deposit. The nature of this matter has not been accurately determined. That it contains iron in a notable proportion has been long known; and the experiments of Berzelius seem to shew that the animal matter is of the nature of albumen and fibrine; but they are in some degree exceptionable, as he evidently operated on a mixture of colouring matter and white globules. The affinity between the iron and albumen must be powerful; for when thus united, the iron is not open to the action of the usual tests.

[Since this was written, the colouring matter, *hæmatosine*, or *hæmatine*, has been more minutely examined by Engelhart, Rose, and Michaelis, and they confirm the account given of it by Berzelius. Rose has shown that the presence of albumen in a solution always intercepts the action of infusion of galls and other tests on iron; but if the animal matter of the hæmatine be thrown down by a current of chlorine, the iron becomes open to detection. Michaelis found in hæmatine very nearly the same proportions of elements as in albumen, with the addition of a minute quantity of oxide of iron, not amounting to one per cent. I do not find that the remark of MM. Prevost and Dumas, on the insolubility of hæmatine, has attracted attention; for it is stated generally to be soluble in water. Probably part is soluble, and part is merely diffused in the minute gelatinous fragments which these observers saw by the microscope, and which subside after long standing.]

Synthetical experiments have not hitherto confirmed the analysis of this substance, for no compound has been formed artificially entirely resembling the colouring matter of the blood. The substance most nearly resembling it which in many experiments I have been able to obtain, was formed by coagulating by alcohol, or heat, the com-

* This appearance was obtained by the action of a low galvanic power on diluted albumen.

† I confess myself to have been one of the many who saw through the microscope an apparently globular structure in muscle, brain, &c. If this was a fiction of the instrument, on it be laid the blame. There are now, surely, good instruments enough, and it is to be hoped that some of their owners will apply them to these important objects.

pound of peroxide of iron with albumen made by galvanism with an iron wire; but the colour of this was orange-red, and wanted the crimson hue which characterizes blood.

[The sulpho-cyanate of iron is nearly of the colour of blood; but although the sulpho-cyanate of potass has been found in the saliva, nothing of the kind has been ever detected in the blood *. The opinion of Mr. Brande, that hæmatine is a peculiar animal colouring principle, is still the most probable. Raspail asserts that blood may be imitated by the spontaneous evaporation of a menstruum containing in solution albumen and colouring matter.]

The remaining animal substance, to which Dr. Marcet gave the name muco-extractive, is evidently of a complex nature. Berzelius considers it to consist chiefly of lactate of soda, and perhaps the remainder is a kind of mucus. [M. Lecanu, whose analysis of the blood is well known, did not find this compound extractive to amount to two parts in a thousand of blood.

Another curious principle is supposed by M. Baruel to be a constant ingredient in blood—a volatile odorous matter. He found that on boiling blood with sulphuric acid, a strong odour was disengaged, exactly like that of the perspiration of the individual or animal from which the blood was obtained. If this is really an original constituent, and is not produced by the action of the acid, I suspect that it must be joined with acetic acid, which exists in sweat, and has lately been detected in minute quantities in the blood †.]

The saline contents of the serum are chiefly alkaline muriates and phosphates. The effect of serum on vegetable colours indicates the presence of an alkali in a free, or, perhaps, in a carbonated state; and it has been clearly proved by Dr. Marcet that this alkali is soda ‡.

In order to convey a clear idea of the composition of the blood in the living system, it will be useful to recapitulate the most important conclusions to which the foregoing description seems to lead.

The blood consists of a limpid serum, through which particles of a red colour and determinate form are diffused.

The serum consists of water holding in solution, besides some saline matters, a little fibrine, and a compound of albumen and soda, which may be termed an albuminate of soda.

The particles are composed of two distinct parts—1. an internal spherical body, transparent and colourless, whose nature is closely allied to coagulated or uncombined albumen*; and 2. a colouring matter which envelopes it, of a gelatinous consistence, composed of an animal principle resembling albumen, united with a small proportion of iron.

On the Changes of the Blood produced by the Function of Respiration.

We are now prepared to enter on the alterations which the blood sustains in the course of its circulation. That important change which takes place in the lungs, as being necessary to prepare this fluid for its other offices in the animal economy, will naturally first claim our attention.

To discuss the theory of respiration in the most satisfactory manner, it would be right to collect impartially all the facts relating to the subject that have been well ascertained, and from a careful analysis of these arrive at such conclusions as may best refer the phenomena to known laws. This method, however, is incompatible with the plan of an essay of this nature; and for the same reason I must forego the interesting occupation of tracing the progress of medical science in an examination of the numerous opinions on this subject which have been heretofore advanced. It is my intention here to examine the only two theories that in any degree explain the phenomena of respiration, and by an impartial comparison with ascertained facts, endeavour to determine which of these, in the present state of our knowledge, is best qualified for this end.

The first theory to be noticed is that

* My lamented friend, Dr. Maton, once described to me the case of a lady, whose perspiration, when profuse, dyed the clothes on some parts of her person, particularly the wrists and neck, of a bright crimson colour, but without any marks of blood. This, he added, was supposed by Dr. Prout to be caused by the sulpho-cyanate of iron.

† Thomson's Records of Science, June, 1835.

‡ Nicholson's Journal, vol. xxxi.

* [This still seems to me the composition of the internal globule most consonant with analysis; for if it be fibrine, as commonly supposed, how comes it that Lecanu found less than four parts of fibrine, while the colouring matter amounted to 130, and albumen to 60 or 70, in 1000 of blood? It is impossible that these four parts could have included the central nuclei of the red particles, as well as the fibrine dissolved in the serum.]

founded on the views of Black, Priestley, Lavoisier, and Crawford, and further modified into the form in which it is now generally received, by Mr. Ellis. I shall first state the theory in distinct propositions, and proceed afterwards to consider the facts and arguments that may be adduced for and against each.

1. Venous blood differs from arterial in its ultimate composition, in its containing a larger proportion of carbon. In their capacities for caloric, likewise, there is a considerable difference; that of venous blood being = 0·8928 (water being 1000), while that of arterial blood is = 1·0310*.

2. In the lungs the venous blood is freed by a process of secretion from its superfluous carbon, which immediately unites with the oxygen of the inhaled air. This union proves a source of heat, which becomes latent in satisfying the increased capacity of the blood now rendered arterial.

3. In the course of the circulation the arterial blood is converted into venous by the acquisition of carbon; and its capacity for caloric diminishing simultaneously with this conversion, causes an evolution of sensible heat.

Let us now examine the grounds on which these several propositions are supported.

1. It was the opinion of Lavoisier † and Crawford ‡, that the difference between the composition of venous and arterial blood consisted in a quantity of hydro-carbonic matter existing in the former in a state of loose combination. This they inferred from the changes which they found the air expired from the lungs to exhibit—namely, the loss of a portion of its oxygen, and the acquisition of carbonic acid gas and watery vapour, both of which were supposed to be formed by the union of the hydro-carbon of the venous blood with the oxygen which was found deficient in the expired air. The later experiments of Allen and Pepys §, however, which were conducted so carefully that more confidence may be placed in their results, prove that the carbonic acid expired contains just as much oxygen as has disappeared from the air inspired. It was therefore concluded that carbonic acid is the only real *product* of respiration, and that the aqueous vapour in the expired air proceeds merely from evaporation from the extensive warm and moist surface of the bronchi, glottis, &c. Mr. Ellis, in his exposition of this theory *, has therefore considered the difference as I have stated it.

[The subsequent researches of Edwards, Dulong, Collard de Martigny, and others, shew that there is in the respiration of most animals more oxygen consumed than carbonic acid produced; but it does not appear that this result is constant with regard to man; and although all arguments founded on parity of volumes of oxygen lost and carbonic acid produced must hereby lose their force, yet still respiration has not been proved to yield any other product than carbonic acid. Water certainly may be produced by the union of its elements, the oxygen being derived, in the way just mentioned, from the air; but this surplus of oxygen is quite as likely to be consumed in the formation of other matters, which are known to be produced in the blood, and which will be noticed hereafter.]

If the venous blood owes its peculiarities to its containing an additional quantity of carbon, a question naturally arises, in what state does the carbon exist? In the theory of Crawford, the hydro-carbonic matter was conceived to be *loosely combined* with the constituents of the blood: here the expression was vague; but when employed to signify the relation between simple carbon and the blood, it is admissible only on the view recently advanced, that the carbon may exist in its free state of charcoal powder, "by its dingy hue obscuring the colouring matter of the blood †." But such a quantity of free carbon as must be requisite to darken arterial blood into the venous hue would be easily detected by chemical analysis; and this has never been done with venous blood. [Dr. Clanny, of Sunderland, has indeed asserted the existence of a notable quantity of free carbon in venous blood; but this assertion is not supported by any experiment on record.]

* [This and the last clauses of the subsequent propositions have been abandoned by all the later supporters of this theory, who therefore exclude the production of heat from their view. As these clauses are separately discussed, they do not interfere with the arguments relating to the other parts.]

† Mem. de l'Acad. des Sciences, 1777 and 1780.

‡ On Animal Heat, *passim*.

§ Phil. Trans. 1808 and 1809.

* Inquiry into the Changes induced in Atmospheric Air, &c. Part II.

† Good's Study of Medicine, Proëm to vol. i.

Mr. Ellis, although he directly expresses no opinion of the state in which carbon exists, says that it is by no means proper to ascribe to it the dark colour of venous blood. His view, however, assumes that the carbon is in some way combined in some principle of the blood; and this being the only peculiarity in venous blood, it is impossible in this view to avoid referring to it the peculiar colour. Now there is a total want of experimental proof for this view. Analogy seems even to oppose it; for carburetted hydrogen gas, which is likewise capable of uniting with the blood, gives to it a florid hue, whilst hydrogen has a contrary effect.

[The difference, as here represented, between venous and arterial blood, has therefore no other foundation than a gratuitous conjecture, plausibly shaped in accordance with the theory in question. It would, indeed, not be difficult to devise other modes in which venous blood might contain the supposed excess of carbon; but it is useless to frame conjectures for the purpose of falsifying them.]

The estimates of the specific heat of arterial and venous blood, given in the latter clause of this first proposition, are the results of the experiments of Dr. Crawford. It is well known that these have not been confirmed by the trials of Dr. J. Davy, who estimated the capacity of venous blood at ·903, and that of arterial ·913*. Between these conflicting statements it is difficult to decide justly. From a consideration, however—1, of the small difference in composition between arterial and venous blood, which is so slight as to have eluded the detection of chemists; 2, of the errors discovered in others of Dr. Crawford's estimates; and 3, of the probability of these also being erroneous, inasmuch as if calculated on they would be found greatly to overdo the effects here assigned to them, I am led to prefer Dr. Davy's results, the accuracy of which seems now to be generally admitted.

[The discovered errors here referred to, are in Crawford's estimates of the specific heats of oxygen and carbonic acid, which are stated very differently by subsequent experimenters. As to the composition of venous and arterial blood, recent analyses are said to have certainly detected trifling differences, but not enough to invalidate the remark given above.]

2. *In the lungs the venous blood is freed, by a process of secretion, from its superfluous carbon, which immediately unites with the oxygen of the inhaled air. This union proves a source of heat, which becomes latent in satisfying the increased capacity of the blood, now rendered arterial.*

M. Caron was the first who suggested that the superfluous carbon, supposed to exist in venous blood, may be separated by a vital action of the pulmonary exhalants, and that the oxygen of the inhaled air is only necessary to unite with, and convey away this excreted matter*. A similar opinion has been advanced, and supported with considerable ingenuity, by Mr. Ellis. Having an impression that the intervention of a living membrane precludes any direct action between the air and the blood, he was led to conclude that the arterialization is effected by the exhalants of the pulmonary arteries and veins secreting carbon from the blood. How far this impression is founded we shall examine hereafter; we have here to inquire whether the hypothesis is consistent with the phenomena. In discussing it, we must for a moment grant all that is contained in the first clause of the former proposition, without reference to the objections to which we have shewn it to be liable.

Mr. Ellis derives his arguments chiefly from the ratio which the production of carbonic acid is observed to bear to "the due circulation and distribution of the blood." Thus Spallanzani found, that, in snails exposed to a cold equal to zero, the circulation ceased, and with it the effect produced on the air; but on raising the temperature, both were proportionally reproduced. The same was observed of the marmot, in its torpid state†. It is obvious, however, in whatever light we view the re-

* Phil. Trans. 1814.

* Recherches critiques sur l'ouvrage de Goodwyn, sur la Respiration. 1798.

† Mem. sur la Respiration, trad. par Senebier. [For fuller and clearer illustrations of these points, see Dr. Edwards's interesting researches "On the Influence of Physical Agents," &c. translated by Drs. Hodgkin and Fisher. The effect of extreme cold is to suspend both the vital functions and those chemical changes which are connected with them; hence it arrests not only the process of arterialization, but that of reconversion of arterial blood into venous also.]

lation of the blood and the air, that, in order to continue their changes, the blood must circulate and be successively subjected to whatever influence is exerted in the lungs. So, likewise, whether carbon be exhaled or not, exercise, by augmenting the periods of this influence, may cause a corresponding increase of the production of carbonic acid. But the production of carbonic acid is not, as Mr. Ellis supposes, universally proportioned to the vigour of the circulation, for spirituous and fermented liquors diminish the former, but increase the latter*.

It might be a question whether the influence of any secretory organ can so prevail over ordinary chemical affinity as to isolate a simple substance; but our knowledge of the nature of the secernent power is yet too imperfect to authorize us to set limits to its operation. But in a physiological point of view, an argument of great weight against the theory which ascribes the arterialisation of the blood to a secretory function, may be derived from the experiments of Brodie†, Legallois‡, W. Philip§, Chossat‖, and others. In these it was found that the removal of the brain, or the destruction of its influence by means of a poison, or the division of the nerves supplying the organ, is uniformly succeeded by a cessation of the secretion of every secretory organ examined: yet the experiments of all prove that the blood, and those of Brodie shew that the air likewise, sustain the same changes in these circumstances, when artificial respiration is performed, as when all the functions are entire. This is not a fit place to discuss the question whether secretion is to be ascribed to the agency of the nerves; nor, indeed, is it necessary: it is sufficient for our purpose that these experiments prove that a certain injury done to the nervous system is *succeeded* by a cessation or material derangement of the secretory function in general. If we are to consider the function of the lungs as one of secretion, I cannot perceive why it should not obey the same laws as the other secretions. [This still seems to me an insuperable objection to the notion that the change of blood in the lungs is vital, any further than that it depends on the continuance of the circulation. Yet this notion still prevails extensively, and we shall have to notice it further on.]

* Dr. A. Fyfe, Annals of Philosophy, vol. iv.
† Phil. Trans. 1811.
‡ Exp. sur la Principe de la Vie.
§ Experimental Inquiry, &c.
‖ Mem. sur l'Influence du Système Nerveux sur la chaleur animale.

Mr. Ellis offers no conjectures as to the state in which carbon can be excreted by the pulmonary exhalants. The only form in which it can be obtained by decomposition is that of a black insoluble matter; and we should expect this to be its condition in the present instance. This conjecture appears to be countenanced by the fact that the lungs of the human subject are frequently found pervaded in many parts by a dark-coloured matter, consisting principally of carbon*. Dr. Pearson, however, to whom we owe all that has been ascertained on the subject, explains it differently, and perhaps more consistently with many collateral circumstances. He considers this matter to consist of the sooty particles more or less diffused in the air of large towns, in the form of smoke; this being deposited on the bronchial surface, is taken up by the lymphatics, but in quantity so minute that it only becomes appreciable by many years' accumulation. Accordingly it is found most abundant in old people. It is less frequently observed in the lower animals, perhaps from their not attaining a sufficient age; but Dr. Pearson found it pretty copious in the lungs of a cat which had been much confined.

[The black carbonaceous matter has been, as I had here anticipated, brought forward, in support of this view of respiration, by Mr. Brande and others; but the researches of several pathologists have since confirmed Dr. Pearson's opinion †.]

* Dr. G. Pearson, Phil. Trans. 1813.

[† See a notice, and writers quoted, in my work on *Diseases of the Chest*, 3d edit. p. 155, from which I insert one passage:—" Its deposition and permanency in the tissue of the lungs is a proof, not (as Magendie maintained, in the analogous case of the carbonaceous matter of tattooed skins, and of the insoluble oxide in persons coloured by the internal use of nitrate of silver) that there are no textural reparation and absorption, but that this absorption cannot act on insoluble solid matter. I believe that this black matter finds access to the pulmonary texture principally through abrasions, softenings, or other lesions of the bronchial mucous membrane, slight injuries of this kind being common accompaniments of an ordinary cold or cough. This sooty dust does not appear generally to produce any injury to the function of the lung; but getting into any corners out of the immediate sweep of the circulation, such as in the angles of the lobules, on the sides of large vessels, in the cicatrices of old lesions, and in the bronchial glands, it remains slowly ac-

It appears difficult to reconcile some of the phenomena of arterialization with the view that is here given. Were it true, we should expect that this process might, for a short time, be continued without the access of air; since the action of the exhalants, by which the carbon is supposed to be excreted, must, to a certain extent, remain until their orifices are so choked as mechanically to arrest it. To shew how different the actual case is, I need only appeal to the experiments of Bichat*. By these it is sufficiently proved that oxygen gas is so immediately necessary to the process of arterialization, that its abstraction from the lungs is simultaneously attended by a cessation, and its re-admission as instantaneously followed by a restoration, of this process.

Still more difficult of explanation by this theory is the phenomenon observed by Legallois†. I allude to the fact, that after the irritability of the heart has been destroyed, artificial respiration can communicate a florid hue to the blood, not only of the pulmonary veins, but also of the left side of the heart, and even as far as the carotids. [I must here remark, that this fact does not bear on the point so much as I formerly supposed; for it probably depends on certain powers which continue to move the blood after the ordinary pulsations of the heart have ceased. Of these powers, the action of the capillaries, and the tonic contraction of the arteries, may be mentioned as those best established; but the movements of the lungs, and, according to Treviranus and others, an intestine motion of the particles of blood themselves, may possibly contribute‡.]

cumulating until death, or until it is carried off in expectoration by some pulmonary disease. But there are some curious instances on record, in which this accumulation has taken place so rapidly and extensively, as to infringe upon the function of the lung, producing œdema and a black consolidation of the tissue, which tends to ulceration and the formation of cavities."]

* Recherches sur la Vie et la Mort.

† Exper. sur la Principe de la Vie.

‡ A fact worthy of notice, in connexion with this subject, was observed in my late experiments on the causes of the sounds of the heart. In one ass, after the ventricles had ceased to pulsate, the auricles continued to contract at intervals, and a slight vibratory motion only was visible in the ventricles for a considerable time, during which the artificial respiration continued to render florid the blood in the arteries. Here was circulation without any perceptible arterial pulse. This is probably the condition of the circulation in those fainting fits, and other cases of apparent death, in which no pulse can be felt or heard in the arteries or heart.

Strongly as the preceding facts oppose the theory, I shall urge yet another objection, which appears to be of still greater force. Supposing carbon to be secreted by the exhalants of the lungs, does any analogy justify the opinion that this carbon can unite with the oxygen of the inhaled air, at the temperature and in the circumstances in which they here exist? I am disposed to think, that whoever examines attentively those natural processes in which carbonic acid is formed, will concur with me if I reply in the negative. It is true that in some putrefactive processes carbonic acid may result from the union of the carbon of the putrefying matter with the oxygen of the air at a low temperature; but let it be remembered, that the union is in such instances effected by the co-operation of other affinities, and, consequently, that the cases are not parallel. The case under our notice is that of simple, isolated, and therefore solid, carbon, in its habitudes with atmospheric air. Nor can any aid be derived from the agency of a vital principle (that omnipotent power to whose operation the most marvellous effects have been ascribed); for, according to this theory, the union can only take place after the carbon has been excreted, and, consequently, removed beyond the sphere of such influence. [This objection still appears to me a very strong one, and I can confirm it by my late researches on low combustion, in which I found that simple charcoal does not unite with oxygen at any temperature below 400°. Some compounds of hydrogen and carbon undergo a peculiar combustion at 300°, but without producing carbonic acid*.] Such being the state of the case, the legitimate conclusion is, that unless it can be shewn why this constitutes a deviation from the common course of chemical phenomena, the theory that assumes it must be abandoned.

From the experiments of Priestley, Girtanner, Hassenfratz, and others, we find that a change similar to arterialization may be effected out of the body by the simple exposure of venous blood to air or oxygen gas. This must be referred to the operation of chemical affinity only. It is true that some have maintained that we are not justified in

* Fourth Report of the British Association for the Advancement of Science. Transactions of Chemical Section. Just published.

considering this change the same as that which takes place in the lungs. But let me ask, in what points do they differ? What the nature of the change is we infer from examining the circumstances necessary to effect it, and by comparing them with the products which constantly result from it. In the cases now before us both of these are precisely the same; the presence of oxygen is necessary to both; carbonic acid results from both; both are attended with the same change of colour; and this being all we know of either, we are not justified in assuming any difference between the two cases.

It is obvious, however, that the theory under notice cannot ascribe to secretion the phenomenon occurring where no organic structure, and no living action, exist to separate the carbon. Mr. Ellis, aware of this difficulty, has offered the following remark:—"In substances deprived of life, the carbon may rather be said to escape by evaporation than by exhalation, which, physiologically speaking, is a living action*." If by the word *evaporation* is meant conversion into an elastic state, I shall only observe of this conjecture that it attributes to carbon a property of volatility which it is known not to possess, and therefore cannot be received. I shall presently have further occasion to urge the absurdity of supposing that carbon is separated in such processes.

Lastly, Mr. Ellis has endeavoured to prove that carbon may escape from respiring animals, and from animal substances, when no oxygen gas is present. To this end he adduces the experiments of M. Huber†, which, indeed, prove that the air in which bees have been confined contains carbon in some form besides that in union with oxygen. M. Huber conjectures that this carbon exists in union with the azotic gas of the air; but his experiments do not warrant such a conclusion, and it is opposed by all we know of the habitudes of carbon and azote. The experiments of Mr. Ellis ‡ on this point are still less conclusive: all that is proved by them is, that when pieces of flesh have been kept for many days in a vessel of atmospheric air, the air is found to contain carbonic acid gas and carbon in some other form. In both these instances I think it most probable that the carbon existed in union with hydrogen, and that carburetted hydrogen was thus evolved. That this was the case in the latter experiment can scarcely be doubted; for it is well known that animal matters, in the course of their decomposition, often exhale carburetted hydrogen; and the fœtid odour emitted by bees when confined, countenances the same opinion with regard to the other case.

Although the arguments which I have here adduced strongly oppose the opinion that carbon combines with oxygen in the lungs, it is yet abundantly manifest that a union of these elements must take place in some part of the living system to produce the carbonic acid in the expired air. That such union, in this as in all other instances, is attended with an evolution of heat, appears to me equally certain. The experiments of Delaroche and Berard *, which some have held as evidence against such conclusion, shew, indeed, that Dr. Crawford's theory of diminution of capacity is inadequate to explain *how* the union proves a source of heat; but they no more contradict the actual occurrence of this phenomenon, than they do in examples of ordinary combustion. For several reasons we cannot admit Dr. Crawford's theory of the distribution of animal heat. This theory, by reasoning on its own data, as Le Gallois † has observed, may be proved to be erroneous; for by a simple calculation on the numbers given by Crawford to represent the specific caloric of venous and arterial blood, and of oxygen and carbonic acid gases, it appears that the increased capacity of arterial blood would so far overbalance the caloric evolved from the diminished capacity of carbonic acid gas, that the lungs, where these changes are supposed to take place, would be the seat of intense cold. Further: we have already noticed that the result of later investigations prove the incorrectness of the estimates on which this theory is founded. There can be little doubt, therefore, that if the union of carbon with oxygen did take place in the lungs, the simultaneous evolution of heat would be sufficient to raise the temperature of that organ much more than what ex-

* Further Inquiry, &c. chap. iv.

† Huber and Senebier, Mémoire sur la Germination.

‡ Further Inquiry, chap. v.

* Mem. de l'Acad. 1818.

† Sur la Chaleur Animale, Ann. de Chim. et Phys.

periment has found it to be—only two degrees higher than the rest of the body. [This argument may appear to forestal the question of animal heat, which will be considered hereafter; but it bears enough on the present subject to require notice here, as furnishing an additional objection to the assumed union of carbon and oxygen in the lungs.]

3. *In the course of the circulation, the arterial blood is converted into venous by the acquisition of carbon; and its capacity for caloric diminishing simultaneously with this conversion, causes an evolution of sensible heat.*

In this part of the theory we meet with the same ambiguity of expression which we have before noticed. The blood is said to acquire carbon; but scarcely any definite view has been given as to the manner in which this is effected, or the state in which it exists. The late Dr. Murray, indeed, with his characteristic ingenuity, suggested, from general principles, an explanation of the source of the carbon; but since this view is intimately connected with another function, it will more properly engage our attention in a subsequent part of this paper.

A change like the conversion of arterial into venous blood may take place independently of any internal agency. Thus Hunter shewed that arterial blood assumes a dark colour when stagnating in a portion of an artery included between two ligatures *. The same phenomenon may be observed out of the body, when arterial blood is placed in a vacuum, or in media which exert no action on it, as azotic gas †. Now the theory under examination will furnish no explanation of this fact, unless by attributing to carbon, in relation to the other constituents of the blood, a power *actively repulsive*, by whose operation it may be isolated, or brought into a state of less intimate combination. But since such an assumption is uniformly opposed by analogy, it must be considered erroneous; and we must view the fact as furnishing another argument against the theory.

It is obvious that what relates to the change in capacity for caloric by the conversion of arterial into venous blood must fall to the ground, if the estimates on which it is founded are proved to be incorrect; consequently the subject of animal heat can derive no elucidation from this view of the function of respiration.

If, in the foregoing discussion, I have argued the question with full and fair reference to ascertained facts, which it has been my constant aim to do, we must arrive at the conclusion, that this theory of respiration, being supported by not one unequivocal fact—being opposed by some facts, and being incapable of affording explanation for many—must be abandoned, as not representing the true relations of the blood and the air.

[This theory was, when this paper was written (in 1823), generally received, especially in this country. The work of Mr. Ellis, on Respiration, more remarkable for ingenuity and labour than for accurate investigation or logical reasoning, at that time held the sway; and it was against it that I had chiefly to contend. Such was its hold, or such the indisposition to enter fully into the merits of the question, that even up to a late date this view was entertained by several distinguished authorities as at least equally probable with any other. It is only now, when, besides the scientific and masterly experiments of Edwards, a few simple and striking facts pointed out by Drs. Stevens, Mitchell, Faust, and others, have brought abundant evidence against the theory, that all have followed my example in rejecting it.]

The other theory which it remains for us to notice, is in general principle similar to that originally proposed by La Grange, and subsequently advocated by Hassenfratz; but in detail, I shall find it necessary considerably to modify it, in order to give a consistent and more definite character to its several applications. Although the method may not set the theory in the most advantageous point of view, yet, for the sake of facility of comparison, I shall expose it in the same way as the preceding theory, rendering it in propositions as nearly corresponding as the difference of the views will permit.

1. The difference in composition between arterial and venous blood consists in this—that the former contains an additional quantity of oxygen, and the latter of carbonic acid chemically united with it; the affinity subsisting between the blood and oxygen being stronger

* On the Blood, &c.

† Priestley on Air, vol. iii.

than that between this fluid and carbonic acid.

2. The oxygen of the air in respiration, pervading the parietes of the pulmonary vessels, displaces, by virtue of its stronger affinity, an equal bulk of carbonic acid gas, and thus converts venous into arterial blood.

3. In the course of the circulation, the oxygen thus absorbed gradually attracts carbon from the proximate principles of the blood, and uniting with it, evolves heat; and by the formation of carbonic acid, arterial is converted into venous blood.

We proceed to discuss these propositions *seriatim*.

1. Of the existence of carbonic acid in venous blood, we have direct proof in the experiments of Vogel and Sir E. Home, who, by means of the air-pump, extricated a considerable quantity of carbonic acid from this fluid. The manner in which the gas is combined, is rather obscure. I think it most probable that it is diffused uniformly through the blood in the same way as it and other gases are when absorbed by water. I have been led to form this opinion from having observed that the blood, as well as milk, white of egg, and other animal fluids, readily absorb carbonic acid gas, and again yield it on the removal of the atmospheric pressure. Sir E. Home has observed, that during the coagulation of the blood, a gas is evolved, and diffused through the coagulum. There can be little doubt that this is also carbonic acid gas, but this has not been proved. It has been supposed that carbonic acid, did it exist in the blood, would immediately unite with the excess of alkali present in the serum; but from the view which we have given of the constitution of the serum, we should consider the alkali as already combined with the albumen; and even did no such combination exist, the alkali of the serum may already be saturated with carbonic acid,—for vegetable colours, the only proof of the existence of alkali in the blood, are equally affected by it in a carbonated state.

[The preceding paragraph contains a view, as it has been adduced, objected to, and defended by others repeatedly since it was first written. On the condition of carbonic acid in the blood, I have before stated that there are different opinions; and the conclusion to which my experiments long ago led me, appears to be confirmed by the latest and best authorities*. This was, that the removal of atmospheric pressure is not generally sufficient to disengage carbonic acid from blood just drawn; but that after a short exposure to air, or even to hydrogen gas, blood will yield in the air-pump a notable quantity of carbonic acid gas. Dr. J. Davy, Müller, and others, have also proved that the serum of blood is capable of absorbing more than its bulk of carbonic acid gas, which it does not entirely lose again in the air-pump; and Dr. Christison has sagaciously pointed out this absorption as a cause of the deficient quantity of carbonic acid produced, compared with the oxygen consumed, as found by him in the action of blood on the air, and by Edwards and others in the respiration of animals †. The power by which carbonic acid is thus retained by the blood is certainly stronger than that by which it is held in water; but I am still disposed to view it as of a similar kind. The alkali of the serum, although in a carbonated state, may doubtless have additional chemical hold on the carbonic acid; but, as in the case of aerated alkaline solutions, this hold is insufficient to countervail the elasticity of the gas, when a gas of another kind comes in contact with it, and when the two gases, mutually non-elastic, penetrate and displace each other. I shall have occasion to recur to this subject of displacement in the second clause of this theory.]

We are next to inquire what facts warrant the opinion that arterial blood contains oxygen united with it. The air-pump does not, as in the preceding case, furnish a decisive proof of its existence; but such a result is in perfect accordance with this theory; for the affinity of the oxygen is greater than that of carbonic acid, and therefore the union is more intimate. Analogy furnishes us with strong arguments in favour of this view: saliva and mucus are capable of absorbing oxygen, and again yielding it to other substances, as

* See especially Gmelin and Tiedemann—*Poggendorf Ann.* xxxi.
Dr. Stevens has also lately found that blood received from a vein into hydrogen gas, will, after a short interval, yield carbonic acid. Dr. Edwards stated many years ago, that Vauquelin for a number of years shewed in his lectures that blood placed in hydrogen, disengages carbonic acid.—De l'Influence, &c. p. 465.

† Edin. Med. and Surg. Journ. 1831.

metals *. We have before noticed a similar property in the albumen of the egg, which bears closely on the present subject; and the proximate constitution of these fluids is not immediately altered by the absorption. It appears probable that the condition in which oxygen exists in arterial blood is not exactly similar to that of carbonic acid in venous blood; but the present state of our knowledge does not enable us to speak decidedly on this point.

[The analysis of the animal matter of venous and arterial blood given by Michaëlis seems directly to confirm this view†; but I confess, that for differences so inconsiderable between compound and not very definite bodies, I do not much rely on ultimate analysis. A stronger support of this opinion is that furnished by analogy, as stated above; and I may add here, that there are few fluid or moist animal matters which have not more or less of this faculty of absorbing oxygen. Some vegetable products, as fixed oils and extractives, possess it also. That an agent of such energetic properties as oxygen should be more intimately combined with some or other of the animal principles of arterial blood than carbonic acid is in venous, is only consistent with many other analogies; and a variety of speculations might be formed as to the precise nature of this temporary oxidation. On these I shall not enter; but, as I shall presently shew, although the penetrative properties of gases is the first step in giving oxygen to the blood, it is a further force of chemical affinity which retains it there; whilst a still stronger one, gradually acting, at length converts it into carbonic acid.]

We shall find that this view of the composition of arterial and venous blood fully accounts for the products of respiration, as well as for those changes induced in the air by venous blood out of the body; but it will be more proper to arrange these maters under the second proposition.

2. *The oxygen gas of the respired air, pervading the parietes of the pulmonary vessels, displaces, by virtue of its superior affinity, an equal bulk of carbonic acid gas, and thus converts venous into arterial blood.*

This part of the theory being the one which contains many of its characteristic doctrines, and having been thought in several points objectionable, I shall examine at some length the objections which may bear against it, after having in the first place fully developed its application. I shall first apply the theory to explain the changes which the blood sustains when submitted to the action of the air out of the body.

When venous blood is placed in contact with oxygen gas, a portion of gas is absorbed, and an equal bulk of carbonic acid gas is thereby displaced. Many circumstances render it probable that the red globule is principally concerned in this process; but the serum must also have a share; for the crassamentum, when completely deprived of it, does not exhibit the usual phenomena of change on exposure to the air. Again, we find that some animal fluids are capable of supplying the place of serum in this particular. Thus Priestley observed that the crassamentum sustains the usual changes from the air when covered with milk‡; and Dr. Wells found the same to be the ca e with the albumen ovi§. The most probable view of the operation of these fluids in facilitating the reciprocal action of the red globule and the air, appears to be, that oxygen is absorbed by the fluids, and whilst thus held in solution, it exerts its influence on the red globule. Albumen, the principal ingredient in all these fluids, is capable of producing on the air the same changes as blood; but its operation is much more slow, to a smaller extent, and its own composition is but little altered in consequence.

Supposing, then, that the red globule is acted on by the oxygen of the air, through the medium of the serum, it remains for us to inquire, what is the peculiar change which accompanies this action. On this point, however, our

* Fourcroy, Ann. de Chimie, xxviii.

	Nitrogen	*Carbon. Acid.*	*Hydrogen.*	*Oxygen.*
† Arterial blood........	16·800	51·920	7·534	23·746
Venous blood	16·720	52·107	7·765	23·408

Poggendorf Ann. 1832.

‡ Priestley on Air, vol. iii.

§ Phil. Trans. 1797.

uncertainty as to the precise constitution of the red particles will not permit us to entertain any decided opinion. The change in colour in the red globule may proceed either from a change in the colouring matter itself, or from some alteration in the central globule, causing it to reflect light more copiously, which, transmitted through the red colouring matter, gives a crimson of a brighter hue. Although the first of these has probably the greatest share in producing this effect, I think it likely that the other may co-operate; for a thin film of venous blood, when viewed on a white surface, appears to be of the same colour as arterial blood. The change in the central globule capable of answering this end, is the acquisition of a degree of solidity and white opacity; and that this really occurs seems probable from the fact that arterial blood is less transparent than venous.

Whilst the *presence* of *oxygen* in the blood is enough to give it the arterial hue, the true venous colour is produced by the *absence* of oxygen, and the *presence* of carbonic acid. From some experiments I have been led to conclude that the venous blood from which carbonic acid has been abstracted is of a deep lake colour, and that the brownish tint present in venous blood in its natural state is to be attributed to the presence of this gas. Synthetic experiments likewise confirm this notion.

[On the subjects of the foregoing passages some new facts have been presented in the work of Dr. Stevens*; but although these possess much practical interest, they do not appear to me to elucidate the theory of respiration further than the views given above. We may well devote a little attention to the leading points brought forward by Dr. Stevens, and to the views which he founds upon them. I have stated it as well known that the colouring matter of the blood, when quite deprived of serum, does not become florid as usual on exposure to the air. Dr. Stevens finding that various saline solutions, as muriates of soda or potash, and carbonates of soda and potash, nitrate of potash, &c. have the property of giving a most florid arterial colour to dark crassamentum, concludes that the usual change on exposure to air depends more immediately on the saline matter of the serum than on the oxygen of the air. It has been long known that acids have the effect of darkening the colour of the blood; and Dr. Stevens ascribes the dark colour of venous blood solely to the carbonic acid which it contains, and which counteracts the reddening property of the salts of the serum. He supposes that the oxygen of the air produces the reddening effect generally referred to it, by drawing away this carbonic acid by some unknown power of attraction, and thus restoring to the salts of serum their wonted reddening property. In illustration of these points he mixed blood with Saratoga water (a saline mineral water strongly charged with carbonic acid): the immediate effect was to darken the blood; but after a short exposure to the air, it rendered it bright and scarlet.

It will occur, however, to those familiar with the chemistry of this subject, that Dr. Stevens's conclusions, assuming as they do the existence of an unknown and novel power of attraction, are by no means warranted by the facts in question. I am not prepared to deny that the saline matter of serum may be concerned in giving brightness to the colouring matter; but in the case of the action of the air on the blood, this is by no means proved to be the sole cause of the change. Saline solutions of various kinds will certainly redden dark blood in a very remarkable manner; and this quite independently of the contact of the air: but oxygen seems also to have a similar effect, independently of its power of removing carbonic acid. We know that other gases can disengage carbonic acid from venous blood; yet neither these nor the air-pump can give to it the arterial hue. Again, if free carbonic acid alone caused the darkness of venous blood, its operation should be suspended, and the reddening power restored to the salts of serum, by adding a small quantity of a pure alkali, just enough to neutralize it. I have many times made this experiment with great care, beginning with the smallest quantities of liquor potassæ, and trying it in various proportions; but I have never been able by this means at all to brighten the colour of venous blood. When the quantity of alkaline solution exceeds a few drops, it gives the blood a browner and duskier hue, which is obviously from the free alkali effecting a permanent change in the colouring mat-

* On the Blood, 1833.

ter. If added in very small quantity, the alkali must become carbonated, and should thus add to the reddening effect of the salts of serum. But this reddening effect does not ensue, and therefore we cannot admit that the dark colour is owing exclusively to free carbonic acid, nor the red colour solely to the salts in the serum. Again, as I have stated above, milk or white of egg may be substituted for serum, as a medium through which the air will exert its influence on the crassamentum; yet the saline matter of these liquids is extremely scanty.

It is scarcely necessary to examine here Dr. Stevens's notion of an attraction subsisting between oxygen and carbonic acid, for it is at variance with the generally received opinions on the subject, and it is by no means warranted by the facts which he has detailed. That the attraction is not chemical is obvious from its not leading to union; if, on the other hand, it is mechanical, it may be fairly referred to those phenomena long since classed by Dalton, Graham, and others, as illustrating the mechanical mixture of gases. Thus it is found that different gases, which have no chemical affinity, mix intimately by virtue of their elasticity, which makes particles of one gas repel each other, expand, and penetrate freely between the particles of another gas. This important law was not fully developed when I wrote the original remarks given above; but the class of phenomena to which I there compared the absorption and disengagement of gas by the blood, namely, the diffusion of gases through liquids, is comprehended under it. This, I repeat, still appears to me to be the first step in the action of air on the blood. This fluid being in contact with air, part of its carbonic acid penetrates and diffuses itself through the body of air in contact with it; whilst oxygen, the most absorbable gas of the air, enters and diffuses itself in like manner through the blood. But no sooner has the oxygen, by this mechanical property, penetrated this fluid, than it finds there the chemical affinities of animal matter, which hold it, and give space in the liquid of the serum for more gas to be absorbed, if the exposure continues. The attraction by which this interchange, or displacement of gases (as I originally called it), takes place, is as much on the part of oxygen to enter the blood, as of carbonic acid to escape and diffuse itself through the air: hence the term used by Dr. Stevens, that the oxygen attracts the carbonic acid, is not exact.

The correspondence in bulk between the carbonic acid gas evolved, and the oxygen gas deficient in air to which blood has been exposed, was formerly considered an objection to this theory; but I shewed that it was consonant with the phenomena of displacement of gases in general,—the interchange under similar circumstances being bulk for bulk. The most accurate researches do not, however, give that exact correspondence which is here noticed; and considering that the condition of the two gases in the blood does not appear to be exactly similar, I think it more probable that the carbonic acid disengaged will be rather proportioned to the quantity of oxygen which has been absorbed to form it, than to that which takes its place. This will still leave a general though not a necessary correspondence between the oxygen absorbed and the carbonic acid given out; for as the formation of the latter depends on the former, the bulk of the carbonic acid produced will bear a general proportion to the capacity of the blood for absorbing oxygen. These views, it is to be observed, are not essential parts of the theory under review; but, as an extension and further application of it, they indicate the bearings of the subject, and point out a road for further investigation.

It may be seen, in my original observations, that I suggested the possible physical causes capable of making the change in the colouring matter which presents itself to our view on the action of air on the blood. I have lately been making further researches on this and other points connected with this subject, and have obtained some very striking and important results. These are not sufficiently complete to be introduced here, but I may state the following observations, as verifying in a remarkable manner some of my conjectures before alluded to.

When a grain of common salt, or of carbonate of soda, is dropped on a portion of black crassamentum, it first whitens the point of contact, but immediately after the spot becomes of a florid red. When a portion of the same crassamentum is dropped into a solution of salt, the margins and edges are seen to turn white before the vermillion hue appears. This coagulum remained compact in the solution, and on being stirred, filled it with an opaque light red cloud. The same sized clot in water did not preserve its shape, but gave to the water a deep red colour, without impairing its transparency. It appears, therefore, that the production of a *white opacity does* accompany the reddening effect of saline matter on the colouring matter.

The same change, watched through a microscope, presents similar results in a new and interesting form. A little dark colouring matter, mixed with a drop of water, was brought into contact with a grain of salt; currents of liquid were immediately seen running to and from the salt, and in them appeared myriads of minute globules, which were not discernible before, considerably smaller than the ordinary particles of the blood. The assemblages of these particles presented to the naked eye the vermillion hue, accompanied by opacity, which characterizes florid blood; and it was obvious that the lightness of colour was caused by these freely reflecting light through the colouring matter. When a mixture of recent colouring matter with serum is examined, the effect is not so obvious, because the particles are, independently of the salt, so numerous, that it is difficult to perceive whether they increase much or not on the addition of the salt; but this always renders them more distinct, and in a remarkable degree augments their mobility and apparent solidity. Thus, in a drop of bloody serum, where the particles are indistinct, and from the little difference in their refractive power are scarcely to be discerned from the fluid, the addition of a drop of saline solution renders them conspicuous, defined, and presenting all those optical effects that distinct solid pellucid bodies, suspended in a fluid, produce. In this state, each particle, viewed by reflected light, is a minute convex mirror, giving its ray; and an assemblage of these, shining through a thin film of red hæmatine, give the bright vermillion tint which is seen in florid or arterial blood. By transmitted light the assemblage of particles is more or less opaque, in consequence of their refractive and dispersive power. As far as I have been able to determine, the new smaller particles before named are minute spherules, presenting some variety as to size, and resemble those that are seen in serum: they are probably like the central colourless globule before described, and of the same character as the small globules in lymph and chyle *.

The effects, then, of saline matter on the blood, are to give density and stability to the distinct organic particles suspended in it, and to counteract that process of dissolution to which these particles tend in water, in acids, and in the course of time in the serum itself; and it is on these effects that the reddening property of saline matter depends. Connected with the same qualities, no doubt, is that which enables salt to preserve meat; and it becomes an interesting inquiry, what share these properties may have in the physiological effects of various salts on the different functions.

I have before stated reasons for adhering to the opinion that oxygen as well as saline matter is immediately concerned in producing the florid colour of arterial blood; and although I cannot here enter into particulars, I may add that my examinations lead me to conclude that it does this in the same mode which I have described with regard to saline matter—namely, by increasing the density and distinctness of the suspended white particles, which makes them reflect light more copiously through the colouring matter.

The above full consideration of the relations of the blood and the air out of the body, will enable us much more readily to understand the chemical function of respiration, in which these relations are brought most effectually to bear on each other in the body. These points we shall next consider.]

Before we can apply the theory of oxygenation of the blood to respiration, it will be necessary to determine the important question, whether an action can take place between the blood and the air through the interposed tunics of the pulmonary vessels.

Before Mr. Ellis published his experiments on this point, the facts mentioned by Priestley, John Hunter, and Goodwyn, were generally considered sufficient to answer this question in the affirmative. Priestley observed, that when a bladder containing venous

* Müller; Poggendorf Ann. xxv.

blood is placed in contact with oxygen gas, the blood assumes an arterial hue *. J. Hunter confirmed this statement, by proving that the blood is similarly changed in colour when separated from the air by gold-beater's skin †. Goodwyn observed the same change to take place, but less distinctly, when a stream of oxygen gas was directed on the jugular vein of a living rabbit ‡. Mr. Ellis, prosecuting this inquiry, found that the change in the colour of the blood is attended likewise by a change in the composition of the air §. I must remark, however, that his experiment, although it proves that oxygen disappears, and that some carbonic acid is evolved, does not warrant the conclusions which he has drawn from it. The experiment was this:—A bladder, containing black blood, was suspended in a jar of atmospheric air standing over mercury, and having a small cup of solution of potass under it. In four days the whole of the oxygenous portion of the air had disappeared, and the mercury had risen to supply its place: hence he inferred "that all the oxygen gas that disappeared was converted into carbonic acid; and consequently," says he, "we deny that any oxygen penetrated into the bladder, in order to combine with the blood ||." In the first inference, Mr. Ellis assumes that the alkaline solution had absorbed all the air which had disappeared. The air had diminished, but it was not proved that this diminution was not caused by the absorption and condensation of some of its oxygen by the blood. But even granting that the deficiency of air at the end of the experiment was solely attributable to the absorption of carbonic acid by the alkali, it appears no more probable that this carbonic acid was then formed by the union of its elements, than that it was displaced, according to our theory, by the oxygen of the air penetrating the membrane of the bladder.

Mr. Ellis next labours to prove that the air in these experiments derived no ponderable matter from the blood; but here again his arguments are inconclusive. Finding that bladders filled with water, and placed in jars containing atmospheric air, were capable of converting the oxygen into carbonic acid, he concludes it was the bladder, and not the blood, that acted on the air in the preceding experiments. To prove this, he ought at least to have shewn that the effect was produced in the one case as rapidly as in the other, which he has not done.

Such being the doubtful state in which I found the subject, I have thought that it required a more rigid experimental examination than it had yet undergone. With a view to determine whether a reciprocal action can take place between the blood and air through an intervening membrane, I performed the following experiments:—

Therm. 62°; Barom. 29 inches. 580 grains of fresh venous blood were introduced into a rabbit's bladder weighing exactly three grains; the neck being well tied, the bladder was then suspended in a receiver containing 100 cubic inches of atmospheric air, standing over mercury, and was kept in that situation two days. The air did not appear to have undergone any sensible change of bulk, but a considerable quantity of moisture had been deposited on the sides of the jar, and a few drops of serum had exuded through the bladder. (Temperature being the same, barom. 29·15 inches): One cubic inch of this air was found to contain ·065 carbonic acid gas. Deducting the ·005 which atmospheric air usually contains, we may calculate the quantity formed in the experiment thus: ·065—·005 + 100=6 cubic inches. Now 6 cubic inches of carbonic acid gas at this temperature and pressure contain very nearly 0·79 of a grain of carbon *; therefore the 100 cubic inches of atmospheric air had acquired 0·79 of a grain of carbon. The bladder, after being carefully washed and dried, was found to weigh 2·75 grains, having lost only ¼ of a grain. Here we find, then, that more than three times the quantity of carbonic acid was formed than could be accounted for by the loss of weight in the bladder.

The experiment was repeated with 1200 grains of venous blood in a rabbit's bladder, weighing 5·5 grains, and 100 cubic inches of atmospheric air. After two days, analysis showed that

* On Air, vol. iii.
† On the Blood, &c.
‡ Connexion of Life with Respiration.
§ Further Inquiry, &c., chap. 4.
|| Op. cit. p. 293.

* Thomson's System of Chemistry, vol. i. p. 229.

the air had acquired ·075 carbonic acid gas, which in 100 cubic inches contains ·9875 of a grain of carbon. The bladder, washed and dried, weighed 5·1 grains; therefore, at least ·5875 of a grain of carbon *must have been derived from the blood* having in some form pervaded the bladder.

Considering it, then, established, that the intervention of a *dead* membrane at least does not impede the action of the air on the blood, we are next to inquire whether there be sufficient reason to attribute an opposite property to the same structure when endowed with life. The permeability of living animal membranes to liquids, may be considered as decisively established by the numerous experiments of modern physiologists, particularly Marcet and Wollaston *, Everard Home †, Magendie ‡, Fodera §, and Segalas. It is likewise proved by the constant perspiration which takes place through the cuticle, in which the most powerful microscopes have failed to discover pores. Now the fact being proved that living as well as dead membranes admit the transudation of *liquids*, we must also conclude that there can be nothing peculiar in a living structure which could prevent gaseous bodies from pervading them as easily as dead structures. We may consider the experiment of Goodwyn, before described, as furnishing a direct proof of the fact which we are endeavouring to establish; and if this has shewn that an action was to any extent exerted between the blood and the air through the comparatively thick coats of the jugular vein, we cannot have the least difficulty in conceiving the same to proceed without any impediment through the coats of the pulmonary vessels, which are of an extreme minuteness. Besides this tenuity of the tunics of the vessels and bronchial cells, I must not omit to notice another circumstance which may greatly facilitate the reciprocal action of the air and blood in the lungs. The bronchial cells are lined with a membrane which is constantly covered with the peculiar animal fluid called mucus. This liquid possesses a remarkable property (to which we have before adverted) of absorbing oxygen, and yielding it again to other substances. Now it appears highly probable that an important office of this secretion on the pulmonary surface is to form a medium through which the oxygen of the air is transferred to the blood, thereby greatly facilitating its pervasion. The changes in the composition of the air of cavities lined by serous and mucous membranes, recently examined by Dr. J. Davy *, confirms these opinions; for they are plainly referable to a similar cause. In Priestley's experiments, as well as in my own just described, the office which on the pulmonary surface is performed by mucus, was to a certain degree supplied by the serum of the blood which transuded through the bladder.

* Phil. Trans. 1811.

† Ibid.

‡ Journ. de Physiologie, t. i.

§ Ibid. Jan. 1823.—To these may now be added the remarkable researches of Dutrochet, "Sur l'Endosmose et l'Exosmose."

If I have succeeded in removing all objections to the opinion that oxygen is absorbed by the blood through the coats of the pulmonary vessels, I cannot perceive the difficulty that has occurred to some, in supposing also that carbonic acid is simultaneously evolved. If, indeed, we were advocating the opinion of Dr. Murray, and some others, that the oxygen of the air enters the blood, combines with some of its carbon, and thereupon makes its exit in the form of carbonic acid gas, then would our view assume an anomalous operation, of which (as Mr. Ellis observes) the science of chemistry does not furnish a parallel example. But if we admit the principle which I have advanced, that oxygen displaces an equal bulk of carbonic acid from the blood, then we view the case as a common example of chemical decomposition [or mechanical displacement], affording ample explanation of all the phenomena.

[No part of these observations has been so amply confirmed by researches subsequently published, as these, in which I have endeavoured to establish the permeability of animal membranes to gases. In fact, this gaseous pervasion, which I had here proved to take place with regard to the cases bearing immediately on this subject, has been shewn by Dr. Mitchell, of Philadelphia, to be the result of a general and important power of penetration, which both gases and liquids possess towards solids,

* Phil. Trans. 1823.

and towards themselves, and which seems to depend on some of the simplest and most essential properties of matter.

Dr. Priestley had long since found that hydrogen gas confined in bladders acquired the property of detonating, from atmospheric air penetrating through the bladder, and that in the course of time all the hydrogen would escape in the same manner. Mr. Graham noticed a similar phenomenon with coal gas and carbonic acid *; and it appears that Dr. Stevens had also found that carbonic acid was capable of passing through a membrane, so as to mix with air on the other side †. But it is to Dr. Mitchell that we owe the knowledge of the general law to which these examples may be referred, and the consequent removal of their apparent anomaly ‡.

The diffusion, or spontaneous intermixture, of two gases in contact, is shewn by Mr. Graham, of Glasgow, to be effected by an interchange in position of indefinitely small volumes of the gases, which volumes are not necessarily of the same magnitude, being, in the case of each gas, inversely proportional to the square root of the density of that gas §. But the transmission of gases through membranes is a more complex phenomenon, having reference not only to the law just stated, but also to the weight and elasticity of each gas, and in case of wet membranes to the affinity of the moistening liquid for the gas. Thus, although the diffusive power of carbonic acid gas is lower than that of oxygen, its weight is greater and its elasticity less, as is shewn in the power of ~~carbonic acid~~ to condense it: hence it more readily passes into the pores of a membrane; and if this membrane be moist, the absorption will be still further promoted by the affinity of water. When, however, the gas in the pores comes in contact with another gas on the other side of the membrane, none of these powers will be sufficient to countervail its tendency to diffuse itself according to the law given above; and the result will be a constant and speedy passage of carbonic acid gas through the membrane, until there is an equal proportion of it on either side. In this way, I conceive, is explained the more ready transmission of carbonic acid than of oxygen, and of the latter than of nitrogen; and thus is superseded the anomalous supposition of Dr. Stevens, that oxygen has a specific power of drawing carbonic acid to it.

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* Mr. Graham referred the phenomenon to the absorption of carbonic acid gas by water in the capillary canals of the membrane, and its subsequent diffusion on the other side. This, although not the whole explanation, certainly approaches to it; for the principle of penetrativeness, as well as of endosmose and exosmose, is more closely allied with capillary attraction than Dr. Mitchell or Dutrochet seem to admit.

† On the Blood, p. 90.

‡ Philadelphia Journ. of Medical Sciences, xiii.; also Journal of the Royal Institution, Aug. and Dec. 1831.

§ Trans. Royal Soc. Edin. 1831.

The force with which gases pass through membranes is very great, amounting to a pressure of several atmospheres. This is signally shewn where the rate of transmission is unequal, as in the cases just noticed. Thus, if a bladder, or thin caoutchouc bag, full of oxygen or hydrogen, and firmly tied, be placed in carbonic acid gas, the latter will pass into the bladder so much faster than the oxygen can pass out, that the membrane will soon become distended, and will eventually burst. If the experiment is reversed, the carbonic acid being within, the bladder will speedily become flaccid, and lose the bulk of its contents. These phenomena are most remarkable with recent animal membranes; and if tried on a living membrane, the transition is very rapid*.

Since Dr. Mitchell made known the general fact of the penetrativeness of fluids, he and several other writers have adverted to its applicability to the phenomena of respiration; but no one has yet accurately examined what share it has in the process and changes dependent on this function. Dr. Mitchell himself has, indeed, taken a view of the change very similar to that of Dr. Murray, noticed above; that oxygen penetrates the membranes of the pulmonary vessels, unites with some carbon of the blood, and again immediately pervades the membrane as carbonic acid gas. But this is not only inconsistent with the rapidity of the process, and several collateral circumstances, but is signally opposed by the fact, established by Contanceau, Edwards, and others, that animals breathing hydrogen or nitrogen continue to throw out carbonic from

* Mitchell, Journ. of Royal Institution, No. p. 108.

their lungs, sometimes to an extent exceeding the bulk of the whole animal. A similar fact is observed with blood out of the body. Again, Dr. Stevens's view, besides that it involves the hypothesis of a new attraction, before objected to, does not account for the absorption of oxygen, which is assumed, without any adequate explanation, to succeed to the removal of the carbonic acid.

I have already given an account of the displacement of carbonic acid from the blood by oxygen out of the body. It is not certain whether the oxygen enters, or the carbonic acid escapes, first; but the former is the more probable case, as it accords with the fact that blood will yield carbonic acid only on exposure to another gas. The membrane of the air-cells here acts an important part; it absorbs oxygen into its pores, and this absorption is promoted by the albuminous fluid which moistens it. Then we may bring to bear an important law established by Dr. Mitchell:—"If on the opposite side there exist a substance or power capable of occupying or removing a gas as fast as, or faster than the membrane delivers it, the actual rate of transmission will be as high as is possible*." Now from various analogies we have been led to assign to the blood such a power of "occupying" the oxygen; and thus the transfer of this element to the blood will be facilitated so as to equal the passage of carbonic acid outwards, which we have seen to be so easy and speedy a process even on the coarse scale of experiments with bladders. I have already glimpsed at the principle here developed, in the use which, in the original observations, I had assigned to the mucus of the bronchial membrane; and I still consider this to have its share in the process of transfer of oxygen from the air to the blood. For this process, as well as for the *exosmose*, or passage outwards of carbonic acid, the air in the bronchial cells must have a certain degree of purity; for the passage in both ways will be proportioned to the quantity of oxygen, and the absence of carbonic acid in the air tubes and cells. The size and form of these, capable of holding a much greater bulk of air than the vessels which ramify on them do of blood, are eminently adapted to bring a sufficient volume of pure air in free relation with the moving blood.]

The instantaneous character of the process of arterialization, so clearly illustrated in the experiments of Bichat, accords well with this theory, which supposes the action of the air to be exerted directly through the pulmonary membranes*. It would appear that the oxygen absorbed in the lungs during respiration becomes diffused through the blood of the adjoining vessels to a certain extent after the heart's action has ceased, which may explain the fact described by Le Gallois (of the reddening of the blood by artificial respiration as far as the carotids); and on this probably the resuscitation from asphyxia by insufflation of the lungs partly depends.

The experiments of Nysten on the injection of different gases into the veins of living animals, in a remarkable manner corroborate this theory. Oxygen gas was gradually injected, in large quantities, without any injury resulting. Carbonic acid gas, when injected, was also absorbed; but when in considerable quantity, it produced symptoms of asphyxia. Many other facts in accordance with this view may be found in M. Nysten's work†.

With respect to the relations of azote in respiration, experiments have proved so various, that I have found it impossible to arrive at conclusions in any respect approaching to certainty. Some say it is exhaled; others that it is absorbed; others, again, that it suffers no change; and all that can be asserted is, that if nitrogen be absorbed or exhaled during respiration, it must be to an extent very small in comparison with the other changes. [This neutral character of azote accords well with its remarkable inertness with regard to the penetration of membranes, for it is more sluggish in this respect than any other gas. It has also few chemical relations; and probably its absorption may sometimes be the result rather of accident affecting its mechanical penetrativeness, than of any essential relation to the function of respiration. The results of the experiments of Edwards are, however, too regular to be viewed in this light; and they seem to

* Loc. cit. p. 116.

* Recherches, &c. referred to at p. 13.

† Recherches de Physiologie et de Chimie Pathologiques, 1811.

establish, that seasons especially influence the relations of nitrogen to respiration, there being always an exhalation of this gas in the summer, and an absorption in the winter. I do not consider that those experiments in which animals are made to breathe pure oxygen, or oxygen and hydrogen, are at all conclusive as to the proportion of azote that may be exhaled; for under these circumstances the increase of this element may in great measure be the result of a mechanical displacement from the various tissues, fluids, and cavities of the body, which are all more or less penetrable, and capable of receiving certain portions of the air which is breathed. This objection will especially apply to experiments with birds in the gases just named; for their plumage must necessarily contain a considerable quantity of common air, more or less condensed, according to its porosity.

An opinion recently advanced by Gmelin and Tiedemann requires notice here. They believe that arterial blood contains a free acid, and that this is the cause of the evolution of carbonic acid in the lungs. They could not, by means of the air-pump, extricate any carbonic acid from recent blood, but did so without difficulty on adding a little vinegar to the blood. They thus disengaged from venous blood more of this gas by a third than from arterial. Tempted by this fact into the conjecture that exposure to air, or oxygen, causes the formation of a little acetic acid, which throws off the carbonic acid, they tried to detect this acetic acid in the blood; and at last, by a variety of agents, they did succeed in evolving something that *smelt like it* *. I am by no means disposed to deny that acetic acid may be occasionally present in the blood. Its formation is one of the general steps towards decomposition, or simplification, to which most complex animal and vegetable fluids naturally tend; but that it is a necessary result of the oxygenation of the blood, seems to me inconsistent with its extreme paucity in arterial blood, if it be really present there at all; and that the evolution of carbonic acid is necessarily dependent on its formation is disproved by the fact that hydrogen and nitrogen gases, which can in no way promote the formation of acetic acid, can disengage carbonic acid gas from venous blood.

It may appear that in the foregoing explanation of the changes of the blood from respiration, I have dwelt too exclusively on *chemical and physical* laws, and have paid too little regard to the *life* of the parts in which these changes take place. I readily admit that influences peculiarly vital may be exercised on all functions, however chemical they may be; but when we see that chemical changes of the blood and the air, similar to those of arterialization, may be effected out of the body, beyond the influence of vitality, and that the same changes may be kept up by artificially supplying air in animals whose various functions of secretion, and in some cases whose heart's action, have been paralysed by poisons or by injuries to the nervous system, we must view these changes as essentially a physico-chemical process. For these reasons I do not admit the view of some modern physiologists, who ascribe these changes to vital exhalation and absorption. In a subsequent part of this essay I shall endeavour to shew where the vital powers may influence the products of respiration, by operating through other functions on the composition of the blood.]

3. *In the course of the circulation, the oxygen thus absorbed gradually attracts carbon from the proximate principles of the blood, and, uniting with it, evolves heat; whilst, by this formation of carbonic acid, arterial is converted into venous blood.*

In this division we find an explanation of the changes which especially constitute the conversion of arterial into venous blood in the living system. The data by which we judge of this process are much less certain than those which we possess relating to the function of arterialization. By reasoning on the supposed differences between arterial and venous blood, we may get an abstract view of the most important feature of the change; and hoping to be able to elucidate the minuter points by a comparative examination of other functions hereafter, I shall at present confine myself to this general view.

The oxygen absorbed by the blood in the lungs becomes combined with carbon in the course of the greater circulation, but *particularly in the capillary*

* Poggendorf Ann. xxxi.

vessels. It is by no means necessary to suppose that this carbon is supplied in a free state, as some have imagined. It may be derived directly from the decomposition of some of the proximate constituents of the blood; to effect which, two chemical affinities would assist—one between the oxygen and a portion of the carbon of one of the animal principles of the blood, and the other between the remaining constituents of this principle to form a new one, containing a smaller proportion of carbon. The union of the oxygen and carbon is thus favoured by every circumstance: they are exposed to each other's action in proximate contact, with no influence, mechanical or chemical, to prevent their union.

We have before noticed that arterial blood, or that oxygenated by exposure to air, will, if excluded from external influence, in time again become dark. This fact illustrates the natural process, and may be explained in like manner. The oxygen in union with the red particles gradually acts on their ultimate composition, by entering into direct union with a part of their carbon. This change, the substitution of carbonic acid for oxygen, darkens the blood. This phenomenon, which we found inexplicable by the former theory, appears here in accordance with the laws which regulate the affinities of complex matter. Some part of the blood (we have supposed the red globule, perhaps the central globule only) sustains a change of composition from this process. Without speculating here on the exact nature of this change, I shall merely observe that the conversion of fibrine into albumen, or, if we consider these principles as the same, the variation which is found to exist in its constitution, would easily supply carbon to be united to the oxygen.

I am aware of only one objection that has been raised against this view of the conversion of arterial into venous blood. It has been asserted, on the authority of a single experiment of Priestley, that arterial blood which has absorbed carbonic acid, and has been darkened thereby, does not again become florid on exposure to oxygen: hence it is concluded that venous blood does not owe its characters to its containing carbonic acid. [An argument to prove that if the experiment were exact, this inference is not logical, I omit, because it is unnecessary.] Although the result of this experiment would not disprove the explanation just given, it may be considered as rendering it doubtful; and I was therefore induced to repeat the experiment. When arterial blood, or blood reddened by exposure to the air, is placed in contact with carbonic acid gas, it soon assumes a dark hue (which differs from that of venous blood only in being somewhat browner), whilst a portion of gas is absorbed. I have uniformly found (and my trials have been so numerous that I can state their results with confidence) that blood thus darkened, on exposure to oxygen gas or atmospheric air, becomes *scarcely* less florid than before its exposure to the carbonic acid. I say *scarcely*, because a slight tinge of brown sometimes remains, probably caused by the retention of a portion of the acid gas. The experiment is so simple and easy, that I feel no hesitation in stating its result, although at variance with the authority of Priestley.

[This part of the subject cannot be fully developed until the various changes which the blood sustains in the greater circulation shall have been considered. The imitation which we have out of the body, of the process of *venosation* of the blood, by leaving arterial or oxygenated blood to act upon itself, is well worthy of notice. It is true that this is a slow process, in comparison with that of the circulation; but it illustrates well the tendency which all complex organic fluids have to change into simpler and more permanent compounds. The change out of the body is promoted by a moderate heat; and in the body of living animals, Sir Astley Cooper found that it could be retarded by severe cold, the blood remaining florid throughout the system after the death of the animal *. This change in the living body is, as I have before said, much under the influence of the vital powers in the various organs through which the circulation carries it; but as we have it imitated by chemical affinity out of the body, it becomes a fair subject of chemical inquiry how these several functions may act to promote it. This we shall proceed next to consider.

* Dr. Hodgkin's notes to the Translation of Edwards on Physical Agents, &c.

On the Changes produced in the Blood by the processes of Assimilation and Secretion.

[Before entering on this subject, I think it necessary to say a few words on the mode of inquiry which I have adopted in examining it. Taking the blood as a whole, we find that various matters are added to it, and separated from it in different parts of the system; and that no one of these matters resembles, in composition, the blood itself. The matter separated from the blood in the lungs we have seen to be very simple in character, being, chiefly at least, carbonic acid; and as about an equivalent of oxygen is at the same time absorbed, the only ultimate change is in the loss of carbon. The simplicity of the fact, that the blood loses carbon by respiration, has occasioned the question among chemists, whence is this carbon supplied? But I shall have occasion to shew that there is no more reason for this question, than for asking whence are the materials of other secretions or processes derived. These, although less simple than the product of respiration, differ yet materially from the blood; and taken separately, we must perceive that each ought equally to have a source of supply, without altering the mass of the blood itself, which we know must retain a certain identity of qualitative composition, in order to fit it for its various offices. And here we shall find that nature will answer the above inquiries, by displaying one of those comprehensive and harmonious relations that unite and simplify her apparently complex operations. A partial inquirer into the œconomy of the animal system can never attain to these truths: there must be a comprehensive study of all the functions, before the relations of each can be truly or fully understood.

The following view of the relation of the several functions to the mass of blood can only be approximative, inasmuch as the information which we have of the composition of the various animal matters is not in all cases certain or constantly applicable; but we shall find enough to guide us to more positive conclusions than have been hitherto attained.]

On the Changes produced on the Blood by the Assimilation of the Chyle.

The addition of the chyle to the blood necessarily alters the composition of the latter fluid; but as it is soon assimilated to it, it becomes a question what effect this change has on the proportion of the ultimate constituents of the blood. I cannot enter into the question respecting the particular seat of the process of sanguification; for what we know on the subject does not lead to any positive conclusions. The most probable hypothesis that can be formed at present must regard this function as not confined to one or two organs, but to be the result of the combined action of many. The chief feature of sanguification is the formation of the colouring matter which is to envelope the white globules found in the chyle. The seat of this process, and the precise sources from which materials are derived, are yet wholly unknown; for although several conjectures have been made in relation to them, the vague and inconclusive nature of the arguments adduced in their support, sufficiently point out the perplexity of the subject. [The coagulum of chyle assumes a pink colour on exposure to the air; and this has been supposed to be caused by hæmatine in an incipient state. But Tiedemann and Gmelin state that this colour is not seen in the chyle of the lacteals before it has passed through the mesenteric glands; and they suppose, therefore, that the colour is derived from the blood. Neither the opinion nor the objection to it is conclusive.]

Equally vague is the hypothesis advanced by the late Dr. Murray and Dr. Thomson respecting the general nature of the change in the composition of the blood from the assimilation of the chyle to it. These authors have endeavoured to explain how carbon is continually furnished for excretion by the lungs, as follows:—Holding the chyle to be less completely animalized than the blood, and that it in some measure partakes of a vegetable nature, they suppose that vegetable substances, from their having no azote, must necessarily contain a larger proportion of carbon than animal matters; whence they infer that the conversion of chyle into blood must be attended with the removal of this excess of carbon. This reasoning is really so loose and unsatisfactory, that it cannot surprise us that the theory is altogether erroneous. Analysis has shewn, it is true, that the chyle of herbivorous animals in some measure does partake of a vegetable character; but if the same obtains in omnivorous species, it can scarcely be applied to those which live entirely on animal food. The first position, therefore, on which this hypothesis rests, has not been proved to be true, and probably does apply to only a limited part of the animal creation. The next position, that animal matters contain less carbon than vegetable, is assumed on hypothetical grounds, and in the generality of instances is not true, for the quantity of oxygen is so much greater in the latter than in the former, as to supply the whole deficiency of azote; so that many vegetable substances, as sugar, gum, starch, &c., actually contain less carbon than albumen and fibrine. As the reasoning thus proves to be defective, so, if we resort to analysis, a much surer criterion, we shall find the conclusions in this particular instance contrary to fact.

The chyle is a fluid, holding colourless globules in suspension, and coagulates in the same manner as the blood. The coagulum appears, from the experiments of Vauquelin *, to be a variety of albumen or fibrine. The serum contains albumen in solution, and the same neutral salts that are found in the blood. In these respects, therefore, chyle does not differ from the blood; and no change will be required in these constituents. The chief peculiarity of chyle is a quantity of saccharine matter, like sugar of milk, which it holds in solution†. Now, as this is not found in the blood, the conversion of this sugar into albumen and fibrine, the animal principles of the blood, must constitute the most important change in the assimilation of chyle. To ascertain the effect of this change on the proportions of the ultimate elements of the blood, we need only compare the composition of the saccharine matter with that of albumen and fibrine. Taking the analysis of sugar of milk as representing that of the saccharine matter in question (omitting fractions), 53 oxygen, 40 carbon, and 7 hydrogen, are its proportions, as found by Berzelius ‡. The composition of the animal principles of the blood will be represented by the mean numbers of albumen and fibrine; and following the analysis of Gay-Lussac and Thenard*, we obtain the following numbers, as nearly representing these aggregate proportions: 21 oxygen, 53 carbon, 7 hydrogen, and 19 azote. Hence it appears, that in order to convert the sugar of chyle into the principles of the blood, 32 oxygen must be abstracted, and 13 carbon and 19 azote must be added in the hundred parts. According to this statement, the assimilation of chyle, instead of proving a source of carbon in the system, actually causes a demand for this principle: therefore Dr. Murray's hypothesis must fall to the ground.

The comparative view which I have just given conveys some idea of the nature of the change effected in the composition of the blood by the function in question. The abstraction of oxygen, and the addition of carbon, may easily be effected by other functions of the living system; but whence the azote is derived, particularly in herbivorous animals, is yet entirely unknown, and will probably remain a mystery until the real nature of its composition shall be revealed.

[Our knowledge on the subject of the chyle and its assimilation is still scanty and unsatisfactory. There have been but few analyses of it; whereas, from its probable variability, many would be required to give us accurate notions of its general composition. Dr. Prout considers the animal principle to be chiefly a modification of albumen, which is scarcely coagulable by heat, and which he therefore calls *incipient albumen*. I have formerly noticed that the white of an egg just laid will not coagulate so firmly by heat as after it has absorbed oxygen; probably a similar reason may be given for the imperfect coagulation of chyle. Mr. Brande thinks the albuminous matter of chyle rather to resemble the curd of milk, or caseum; a substance which the researches of Bracconot have made particularly interesting. The proportion of sugar of milk found by Dr. Prout in the chyle of a dog fed on vegetable food, appears to have been but trifling, and scarcely to warrant the numerical calculation which I have given above. Berzelius describes a fatty matter as a constant ingredient of chyle, especially

* Ann. de Chimie, lxxxi.
† Brande, Phil. Trans. 1812.
‡ Annals of Phil. vol. v.

* Recherches Physico-Chim. t. ii.

in that obtained during the digestion of food; but he does not state its proportion. If it exceeds in quantity that found in the blood, it may prove a source of carbon, for animal oils contain from 20 to 30 per cent. more carbon than albumen does. Berzelius supposes the white colour of chyle to depend, as in the case of milk, on the oil in suspension, and Raspail seems to hold the same opinion; but authors have generally ascribed the white opacity to the organic globules which resemble the central globules of the blood.

The source from which vegetable nutriment becomes azotized in the body, is less a matter of mystery now that it is well ascertained that azote is occasionally absorbed in respiration; but we can go no further at present than state this as its possible source.]

On the Changes produced in the Blood by the processes of Secretion.

I now proceed to inquire into the changes produced by the several secretions; a subject which, notwithstanding its obvious importance, has scarcely received any attention, and, consequently, on which I have been unable to derive satisfactory information from the writings of others. The mode of inquiry which I have applied to determine the changes in the blood from the assimilation of chyle, is so simple, and comparatively so free from fallacy, that I shall not hesitate to adopt it on the present subject. It would be incompatible with the limits of these observations to enter into a minute account of the composition of every secretion; nor do I apprehend that the inquiry would be useful. We shall therefore examine those only which, from their quantity, produce considerable effect on the composition of the blood; and in these instances confining our attention to their most prominent and characteristic features.

Changes from the Secretion of the Urine.

The secretion of urine must be considered as by far the most important in relation to the present subject; both from the peculiarities in its composition, and from its being altogether eliminated from the system, by which we are assured that it must absolutely affect the composition of the blood.

The animal principle called urea is that which gives to urine most of its characteristic qualities; and since it constitutes by far the greater portion of the solid organic contents of this secretion, we shall arrive at pretty accurate conclusions respecting the effect of the whole secretion on the blood, if we assume this principle as its representative. If, then, we compare its composition, as deduced from Dr. Prout's analysis, with the mean of the numbers of albumen and fibrine, which represent the blood, an estimate will be formed of the conversion of the latter into the former. The smaller fractions are omitted.

Elements.	In the Blood.	In Urea.	Excess in Blood.	Excess in Urea.
Carbon	53	20	33	0
Hydrogen	7	7	0	0
Oxygen	21	26·5	0	5·5
Azote	19	46·5	0	27·5

We are thus led to an important conclusion respecting the relations of the urinary excretion in the animal economy. From the large proportion of azote which urea contains, exceeding that in the constituents of the blood to the amount of 27·5 per cent., we find the conjecture confirmed which has been thrown out by some authors, that one office of the kidneys is to eliminate azote from the system. It might naturally be questioned what end this elimination can answer, for it should be expected, *à priori*, that there will ever be a deficiency instead of a superfluity of azote, which element is required to convert the vegetable part of our aliment into animal matter. The question, as I have before remarked, presents much difficulty; but this lies not so much in accounting for the elimination of azote from the system, as in the mode by which vegetable nutriment is animalized. The several fluids of the body are secretions from the blood, the nature of which is fixed, being always constituted of proximate principles, into whose composition azote largely enters. Now we shall find that the other secretions of the body contain less azote than the blood; and it must therefore be that their formation from that fluid will leave in it an excess of azote: it is by removing this and preserving the uniform composition of the blood, that the elimination of azote by the kidneys proves useful.

But the most striking point of difference between urea and the constituents of the blood, is the great deficiency of carbon in the former, amounting to 33 per cent. This fact at once suggests the conclusion that another important use of the excretion of urine is to counterbalance the removal of this element by some other function. The intimate relations which this view discovers between the function of respiration and the secretion of urine, must be viewed with the more interest, as it serves to explain, what I have shewn inexplicable on former hypotheses, whence proceeds the carbon so copiously eliminated through the former of these functions.

Of the other constituents of urine, whose composition is known, ammonia alone can be derived from the animal principles of the blood; and the formation of this alkali, composed of hydrogen and azote, would contribute to the same effects that we have pointed out in the case of urea. [The analysis of urine by Berzelius *, gives uric and lactic acids, and lactate of ammonia, as amounting to nearly 2 per cent. Uric acid contains 17 per cent. less carbon, 5 less hydrogen, 15 more nitrogen, and 7 more oxygen, than albumen. Lactic acid has no azote and less carbon, but more oxygen. Hence these animal matters would all leave an excess of carbon in the blood, and all, except lactic acid, would draw to excess on its azote.]

Changes from the Secretion of the Bile.

Although the bile has attracted in an especial degree the attention of chemists, the discordant results obtained by different experimenters shew that we have not yet attained a precise knowledge of its composition. If we confide in the experiments of Berzelius, which, from his acknowledged accuracy, may be supposed nearest the truth, we find that the peculiar principle termed *picromel* is almost the only animal matter entering into the composition of the bile. The analysis of this matter having been ascertained, we make the comparison as follows, omitting the least fractional numbers:—

	In Blood.	In Picromel.	Excess in Blood.	Excess in Bile.
Carbon	53	54	0	1
Hydrogen	7	2	5	0
Oxygen	21	44	0	23
Azote	19	0	19	0

We find the following results from this comparison. 1. That the secretion of bile does not, as we found with the urine, leave an excess of carbon in the blood, but even proves a means of removing it from the system; this, however, to an extent so trifling as scarcely to require notice. 2. That the liver is not, as some have conjectured, an emunctory for the superfluous hydrogen of the body; since picromel appears to contain much less hydrogen than the principles of the blood. It is useless to hold in view the proportions of hydrogen and oxygen in these comparisons, since they may so readily escape or enter the system in the form of water, that our attempts to determine the share or office of the different secretions, with regard to them, would be fruitless. 3. From the complete absence of azote in picromel, compared with its abundance in urea, we are led to conclude, with regard to the secretions of bile and urine, that one assists to counterbalance the effects produced by the other on the composition of the mass of the blood.

[More recent analyses have proved the bile to be an exceedingly complex fluid; if, indeed, the numerous matters found in it were not the products of the process. The chief organic matters, however, besides the picromel, are a peculiar resinous substance and cholesterine. Now both these abound in hydrogen and carbon, without azote, and with very little oxygen; and this fact, besides confirming the conclusions just expressed, countenances an opinion that has been held, that the liver may assist the lungs in removing superfluous carbon from the system.]

Changes from other Processes of Secretion.

The only other fluid secretions which require notice are that numerous class which have the peculiar matter called

* Traité, tom. vii.

mucus as their chief ingredient, including *saliva*, *the secretion of mucous membranes*, *tears*, *pancreatic juice*, &c. We cannot satisfactorily conclude respecting the changes which these secretions produce on the blood, because it is difficult to determine how far they are really and entirely excreted from the system, and particularly because the precise composition of mucus is not known. It appears to resemble gelatine more than any other principle, containing more oxygen and less carbon than the constituents of the blood. It may therefore be conjectured that the separation of the fluids containing it will slightly contribute to leave an excess of carbon in the blood, helping to account for the large quantity of this element removed from the system through the function of respiration. [Much further examination is necessary to determine the chemical nature of many of the secretions of this class. The matter secreted by the lower parts of the alimentary canal is quite peculiar, and probably consists of much of those effete materials to which many of the preceding remarks would apply. It has a fœtid odour, and appears to run readily into putrefaction, exhaling the fœtor of putrid urine. It can scarcely be doubted that it is an excrementitious matter, tending progressively towards ultimate decomposition.

A substance present in many of the fluids of this class is the lactic acid, in union with one of the alkalies. It is also a product of the spontaneous decomposition of milk and several other animal fluids. It contains less hydrogen and carbon, and more oxygen, than albumen, and no azote; it may therefore tend to balance the urea, carbonic acid, &c., which are excreted by the kidneys and the lungs.

The excretion from the skin does not require a detailed notice, inasmuch as it is the same as that from the lungs, carbonic acid and watery vapour, together with a minute quantity of animal matter, consisting of gelatine with some free acid, which Berzelius considers to be the lactic. The effects of its separation from the blood may therefore be inferred from the preceding remarks.

There is one other process which should be noticed before we conclude this subject, namely, the formation of the *animal solids*. Many of these contain the principles which exist in the blood: thus fibrine enters largely into the composition of muscle; albumen exists in cartilage, membranes, and other tissues; and fat is found in the blood as well as deposited among the solids. But a large proportion of the solids consists of *gelatine*, a principle not found in the blood or other animal fluids. It constitutes a chief part of tendons, ligaments, skin, membranes, bones, cellular tissue, horn, &c., and when thus deposited from the blood, either during growth or in the supposed change of the tissues, its formation must modify the composition of this fluid. As, however, the elements of gelatine do not differ much from those of the principles of the blood, the change is not very important. It may be pretty nearly represented by the abstraction from albumen of a little carbon; and thus the process of nutrition proves to be another source of carbon in the system.]

Such, then, appear to be the harmonious relations which subsist between the several important functions of secretion and respiration in the state of health; and if we extend the comparison, we shall find a uniform ratio preserved when one or more of these functions are variously modified. Thus an increased secretion of urine results from the external application of cold to the body: the exhalation of carbonic acid from the lungs is also simultaneously augmented*. During the digestion of food there is a general increase in all the secretions; the carbonic acid emitted from the lungs is likewise proportionally great†. The same remarks may be applied to exercise and other circumstances producing variations which do not injure the health; and it would be possible to extend these correspondences to cases of disease also.

The views which I have given of the effects of the assimilation of the chyle on the blood, receive support from the observations of Dr. Fyfe‡, that a vegetable diet diminishes the proportion of carbonic acid emitted by respiration.

* Crawford on Animal Heat.
† Lavoisier and Seguin, Mém de l'Acad.
‡ Ann. of Philos. xiv.

Comparing these correspondences with our view of respiration, we are led to speculate further respecting the formation of those principles which characterize secreted fluids. Without entering here into any disquisitions as to the manner in which these principles are formed by the operation of the vital powers, I would point out that, as in the generality of instances, the abstraction of carbon is one of the changes requisite to produce them from the constituents of the blood, the oxygen which is absorbed in respiration, by promoting this change by its chemical affinity, must co-operate with the vital powers in elaborating the secretions. I can extend this remark further in no instance but in that of the secretion of urea. The experiments of Prevost and Dumas have shewn that this principle may be found in the blood of animals whose kidneys have been removed. [Dr. Christison and others have since proved the same thing to occur in persons suffering from disease of the kidneys, with albuminous urine.] This renders it probable that the whole sanguiferous system is the seat of its formation, and that the function of the kidneys is merely to separate it, by virtue of what Bichat terms the insensible organic contractility of their vessels. [This term of Bichat is hypothetical, and probably incorrect.] From the views which I have given, urea is to be regarded as the useless remains of the animal principles of the blood, after the other secretions and the carbon of respiration have been elaborated from them. Hence we are not authorized to infer that matters which answer some ulterior purpose in the animal economy have a similar seat of formation. But, on the other hand, we at present know nothing which militates against the idea that picromel, mucus, &c. may exist in a nascent state in the blood, whilst their complete separation into a distinct form may be the real office of their respective secretory organs. Of the opinion of Berzelius, which supposes the sulphuric and phosphoric acids, together with lime, found in the urine and not in the blood, to be formed by the union of their elements at the moment of separation from the kidneys, I would remark, that it appears by no means well grounded; for they may exist ready formed in the blood in such minute quantities as to escape detection, and yet be accumulated in sensible proportions in the urine, which is intended to convey out of the system all such superfluous saline matters.

[The subject of the formation of the secretions, their relation to each other, and to the processes of nutrition and decay, present several other points of interesting inquiry; and as I think that some light may be thrown on them by extending the preceding observations, I shall again recur to them. The next part of this essay, *the origin of animal heat*, as it is intimately connected with them, will properly precede the discussion of these matters.]

On the Origin of Animal Heat.

It is unnecessary to advert to the theory of animal heat proposed by Dr. Crawford; for besides the experiments of Dr. John Davy, which overthrow its fundamental positions, the objections which I have opposed to the theory of respiration with which it is connected, may be considered conclusive against it. The limits of this essay do not permit me to notice many other theories that have been or are now held; and I must be brief in stating and illustrating my own views. These I shall first give in a general outline, and afterwards examine how they accord with the experiments related by others and made by myself on this subject.

It will not be denied, that when oxygen and carbon unite, heat is constantly and uniformly generated. We know, from the phenomena of respiration, that such a union is perpetually taking place in the living body; and by a variety of arguments, I have endeavoured to prove that the seat of this union is in every part of the sanguiferous system; perhaps more in the capillaries and secretory organs than elsewhere. In this case, as in all others, the formation of carbonic acid must be attended with the evolution of heat. Unless, therefore, any process simultaneously goes on which shall cause the absorption of heat thus generated, the same change must prove a source of animal heat. Now we know of no such cause of absorption of heat in the living system, nor can we admit its existence, unless it be unequivocally proved that heat is not actually evolved by the union of carbon and oxygen in this instance. Although an opinion to this effect has been entertained, I shall not at present discuss it, but for the present assume that the formation of carbonic acid does prove a source of heat in the animal system. Whether this chemical union is the *sufficient and only source* of animal heat, is a distinct question, and one that I cannot consider yet decided. Dr. Crawford found, that when equal quantities of oxygen gas were converted into carbonic acid gas by the combustion of charcoal, and the respiration of an animal, as much, or even more heat was given out by the burning charcoal than by the breathing animal *. The results obtained by Lavoisier were, that the heat given out by the combustion of charcoal was to that produced by the animal as 10·3 to 13 †. In regard to the comparative accuracy of these experiments, a consideration of the modes in which they were respectively performed, would lead me to place more confidence in those of Crawford. Lavoisier judged of the quantity of heat evolved by surrounding the subjects of his experiments with ice, and observing the quantity melted. Now it cannot be doubted that the absolute temperature of an animal would be lowered by being surrounded by such a cold body: consequently the heat acquired by the ice would be derived not only from the conversion of oxygen into carbonic acid, and any other calorific process in the animal, but also from the warm body of the animal at the beginning of the experiment. A set of experiments, very recently performed at Paris, have led to the conclusion that the carbonic acid given out by respiration is not a sufficient source of animal heat. [These were the experiments of Dulong ‡. In those of Despretz, which followed soon after §, the carbonic acid produced by a breathing animal would account for about six-tenths of its heat, while the remaining four-tenths were unexplained. Great care seems to have been bestowed on these researches, and I think they may be taken as approximating the truth.]

Without attempting to decide between these discordant accounts, I have long been, and am still, impressed with a belief that the union of carbon and oxygen, to the extent occurring in the body, is insufficient in itself to explain the constant and uniform temperature of so large a mass of matter, prepetually exposed to the cooling agency of evaporation, radiation, and conduction. I am at the same time convinced that this chemical combination is a principal source of heat; but as it is only *one* of many changes by which complex animal matters are resolved into simpler principles, so likewise it probably forms a part only of the means by which animal temperature is supported. To speak with more precision: all the animal principles whose constitution is known, entering into the composition of the fluid secretions, as urea, picromel, &c., are of a simpler nature, and consist of fewer atoms, than the albumen and fibrine from which they are formed. Now, as we know that the resolution of complex matter into simpler bodies is always attended with the evolution of heat in the case of fermentation and putrefaction, so it is probable that the same phenomena will result from analogous changes proceeding under the influence of the vital powers. It will perhaps be said that this view is too hypothetical to merit attention; but I can truly declare, that although, for facility of comparison, and to increase the interest of the subject, I present the general inferences before the facts, it is from reflection on the various experiments which I shall now notice that I have been led to take the view which I have just developed.

I have tried repeatedly to ascertain whether the action between oxygen and the blood out of the body is attended with an evolution of heat; and the general result has been that heat is to a trifling degree generated by the oxygenation of the blood, and from the greater slowness with which it cools, that the blood seems to evolve a slight degree of heat for some time after. In some of these experiments I found that the temperature did not rise in serum agitated with air, but on the addition of coagulum a perceptible rise took place. So Prevost and Dumas have shown that the parts contained in the coagulum are most indispensable to the functions of life *. The power of arterial blood to generate heat, by the gradual union of the oxygen with its carbon, is perhaps

* On Animal Heat.
† Mém. de l'Acad. 1783.
‡ Journ. de Physiologie, 1823.
§ Ibid. Avril 1824.

* Examen du Sang, &c.; Biblioth. Univ. 1821.

what led Dr. Crawford into a mistake respecting its capacity for caloric.

On the influence of the Nervous System in causing Animal Heat.

Of the experiments which have been instituted to demonstrate the dependence of animal heat on the brain and nervous system, none for originality and importance claims our attention so much as those of Mr. Brodie *. These are so familiarly known, that it will be unnecessary for me to cite more than their general results, which were, that when the nervous influence is destroyed, either by decapitation, or by a poison which acts immediately only on the animal life, artificial respiration cannot in any degree preserve the heat of the body, although the expired air sustains the same changes as in natural respiration: and moreover, that the secretion of urine is at the same time suppressed. From these circumstances Mr. Brodie concludes that the function of respiration has no share in the production of animal heat.

Against the validity of the objections thus opposed to the chemical theory of animal heat, it has been argued that it is impossible so exactly to imitate natural respiration as to introduce into the lungs the proper quantity of air: hence, from the unnatural manner in which respiration is conducted, its calorific effects may be counteracted. Although such an objection, were it well founded, could only in a trifling degree affect Mr. Brodie's conclusions, I endeavoured to ascertain how far it is valid, and accordingly made the following experiments:—

Exp. I.—The subject of this experiment was a small rabbit. Heat in recto, $102\frac{1}{2}°$; temperature of the room, $58°$. The trachea was laid bare, and an opening made into it, through which artificial respiration was established by means of an inflating bellows. At the commencement the heat in recto was $101\frac{1}{2}°$; in five minutes, $100°$; in eight minutes, $99\frac{1}{2}°$; in ten minutes, $101°$; in twelve minutes, $99°$; in fifteen minutes, $99\frac{1}{2}°$.

Exp. II.—Performed on a large rabbit. Heat in recto, $98°$; temperature of room, $56°$. Artificial respiration being made as nearly as possible to imitate the natural function in extent and number, the heat was noted as follows. At commencement, $97\frac{1}{2}°$; in five minutes, $97°$; in six minutes, $97\frac{1}{2}°$; in eight minutes, $97\frac{1}{2}°$; in ten minutes, $97\frac{3}{4}°$; in twelve minutes, $98°$; in fifteen minutes, $98°$. Artificial inflation being then discontinued, and the animal left to breathe of itself, the heat rose no higher. Similar results were obtained on repeating these trials.

In regard to the varieties exhibited in the heat, it was remarkable that it always became lower during the operation of tracheotomy, and, as I afterwards found, during every painful operation. This corresponds well with the observation of Bichat, of the imperfect manner in which arterialization is effected in persons undergoing surgical operations. The other variations were plainly referable to the manner in which the lungs were inflated; for when this was assiduously and uniformly done, the heat remained stationary, or rose nearly to the natural standard; but when less steadily attended to, the temperature fell. Hence it follows that artificial respiration may be conducted without causing a diminution of animal heat.

[These experiments, although interesting as shewing the influence of pain in depressing animal temperature, by no means establish what they were intended to prove; for although the lungs could receive air only through the tube of the bellows, the respiratory muscles, external and internal, were still in full force, and as their action must have constituted part of the efforts of the animal, the respiration could not be called entirely artificial. It is only when sensation is destroyed that the respiratory machine becomes passive, and can be worked artificially. In doing this, great care and steadiness are required to inflate regularly, and not too fully or too often, otherwise, besides the over-cooling of the lungs by an excess of cold air, they may become emphysematous, and unfit for their office.]

Another objection to Mr. Brodie's experiments has been urged by Le Gallois, who found that healthy rabbits, when kept for a long time in particular positions, suffer refrigeration to a fatal

* Phil. Trans. 1811 and 1812.

extent*. Hence he concludes that the delicacy of these animals renders them improper subjects for these experiments.

Besides these objections to the mode in which Mr. Brodie's experiments were performed (which objections certainly do not carry with them much weight), the experiments themselves have been repeated by several individuals, and in no instance with precisely similar results. Those of Le Gallois, at Paris, and of Dr. Hale, in America†, led them to conclude that animals in which the nervous influence has been destroyed, cool less rapidly when artificial respiration is performed than where it is omitted; and that the loss of temperature that does occur is solely attributable to the diminution in the force of the circulation. The latter conclusion has, however, been completely refuted by the accurate researches of Chossat, in which it was proved that the rate of cooling, after the destruction of the nervous influence, bears no relation to the state of the circulation ‡. The results of the experiments, as far as they relate to the absolute cooling of decapitated animals, have been confirmed by those of Drs. Wilson Philip and Hastings, who also found that artificial respiration retards the cooling of a decapitated animal, although the former, with Mr. Brodie, considers the nervous influence necessary for the generation of animal heat.

The latest published experiments on this subject are those of M. Chossat. I must refer to his excellent memoir itself for an account of the truly philosophical manner in which the author was led to trace the connexion of animal heat and the nervous system especially to the great sympathetic nerve. He then ascertained experimentally, that whatever destroyed the functions of this nerve, such as contusion of the semilunar ganglion, or more completely tying the thoracic aorta, caused as rapid a diminution of animal heat in every part of the body, equally in the œsophagus and in the rectum, as is produced by the destruction of the brain itself. Throughout his experiments M. Chossat had frequent opportunities of confirming Mr. Brodie's conclusions respecting the effect of injuries of the nervous system in suppressing the secretions, and observes further: "Toutes les lésions du système nerveux qui affaiblissent le dégagement de la chaleur animale, agissent sur les secrétions d'une manière analogue."

Such are the most important facts which I have been able to collect from recent works on this subject. Before I undertake to compare them with the views which I have given respecting the sources of animal heat, I shall concisely relate the results of my own observation, selecting those of my experiments which were most carefully conducted, and which were attended with the most positive results.

Having convinced myself by repeated observations, that comparative experiments with different animals are always liable to fallacy, from the inexplicable variety which occurs in their rate of cooling, however similar the circumstances of size and absolute temperatures, I thought that by attending with greater accuracy, and with shorter intervals, to the variations of temperature in each animal, I might obtain positive and independent results, and thus elude this source of fallacy. Mr. Brodie noted the temperature in his experiments only at intervals of fifteen or thirty minutes after decapitation. Now, from the trials of Chossat, it appears probable that a slight reaction takes place before this, causing an increase of temperature, followed by a more rapid depression. To include these variations I marked the temperature nearly every two or three minutes.

Exp. III.—Subject, a small rabbit. Heat in recto, 99°; temperature of room, 58°. The tube was inserted through an incision in the trachea, after which the heat was 98°. The neck being secured by a common ligature, decapitation was performed, and artificial respiration established. Heat in recto was noted as follows:—

Minutes.	Heat.	Minutes.	Heat.
3 ..	96½°	27 ..	96°
5 ..	96	28 ..	95½
7 ..	96½	30 ..	95
10 ..	96½	32 ..	95
17 ..	96	35 ..	95
20 ..	96	38 ..	94
23 ..	96	40 ..	93½
24 ..	95¼		

Exp. IV.—Temperature of room, 58°; subject, a full grown rabbit. Heat, 102½°; after tracheotomy, 100½°. De-

* Diss. sur la Chaleur Animale; Ann. de Chimie et Physique.

† Med. and Phys. Journal, 1814.

‡ Mém. sur l'Influence du Système Nerveux sur la Chaleur Animale. 1820.

capitation being performed, and artificial respiration established as before, the heat was as follows:—

Minutes.	Heat.	Minutes.	Heat.
5 ..	101°	20 ..	100°
7 ..	101¾	25 ..	99¼
10 ..	101¼	27 ..	99
12 ..	101	30 ..	98½
14 ..	101	35 ..	97
16 ..	100½	40 ..	97

From the perfect state of the respiratory function in birds, and from their high temperature, I was led to expect that varieties might be more easily noted. Accordingly I made a common hen the subject of the following experiment:—

Exp. V.—Temperature of room, 50°; heat in recto, 108°; and after the tube of the bellows had been adapted to the trachea, 107°; but in ten minutes returned to 108°. The neck being inclosed in a strong tight ligature, the head was cut off between the second and third cervical vertebræ. The body was strongly convulsed for five minutes. The heat was then noted as follows:—

Minutes.	Heat.	Minutes.	Heat.
1 ..	106°	24 ..	105½°
3 ..	107	27 ..	106
4 ..	107	30 ..	104
6 ..	108	31 ..	105
9 ..	108	33 ..	105
12 ..	107½	35 ..	104½
15 ..	107	38 ..	104
18 ..	106½	40 ..	103
21 ..	106¼		

Although the total result of each of these experiments was a considerable diminution of temperature after decapitation, yet in every case artificial respiration seemed to retard it, and in some degree to increase absolutely the heat of the body. In the last experiment heat was completely restored ten minutes after decapitation, having increased two degrees in that period.

From a consideration of the efficacy of galvanism in restoring some secretions suppressed by certain injuries to to the nervous system, I was induced to try whether it is also capable of maintaining animal heat*.

Exp. VI.—Temperature of room, 50°; subject, a full-grown rabbit. Heat in recto, 100°; and after separating the trachea and adapting the tube, 99°. The vessels of the neck being secured by a common ligature, the head was cut off immediately below the atlas, and inflation immediately commenced. The heat was found at 96°, at which point it remained for five minutes. Galvanism was then applied, by the negative wire inserted in the spinal marrow, and the positive over the lower part of the abdomen, which had been shaved bare, and covered with wet tinfoil. The power used was such as to produce slight twitches of the fore paws, being that excited by at first twelve, afterwards gradually increased to forty pairs of three-inch plates. The artificial respiration being regularly supported after the galvanism was begun, the heat was observed as follows:—

Minutes.	Heat.	Minutes.	Heat.
5 ..	97°	20 ..	96½°
7 ..	96½	21 ..	96
10 ..	97	23 ..	95
12 ..	97½	25 ..	94½
14 ..	97	28 ..	94
16 ..	97	30 ..	94
18 ..	97	33 ..	93½

The galvanism appeared here to have caused at first an evolution of heat, followed by an unusually rapid diminution. The latter may be in some measure ascribed to the evaporation from the wet abdominal parietes. The urinary bladder was found quite empty, although no urine had been voided during the experiment.

Exp. VII.—Temperature of room, 50°. A large rabbit. Heat in recto, 102°; and after adjustment of the tube to the trachea, 101°. Some essential oil of bitter almonds was dropped into a wound. Five minutes after the heat was 100¾°; in ten minutes, 100°. In about twenty minutes it was convulsed, with gasping and some tendency to opisthotonos, and died in twenty-five minutes. Galvanism was applied as in the last experiment, but with the poles reversed. The artificial respiration was not effectually performed, owing to the rigid state of the muscles of the chest. The heat was noted after the first application of the galvanism:—

Minutes.	Heat.	Minutes.	Heat.
1 ..	96½°	35 ..	93½°
5 ..	96	40 ..	93
10 ..	95	45 ..	94
15 ..	96	50 ..	93½
22 ..	95	55 ..	93
30 ..	94	60 ..	92½

* I believe that Dr. Beilby had made some experiments similar to these some years before, but I do not know with what result.

It seemed to me that the slight increase of temperature observed here twice, was the effect of attempts to make the artificial respiration more perfect. The bladder was found to contain some turbid urine.

Exp. VIII. was performed with the view to determine the respective shares which respiration and galvanism might have in affecting animal heat. Temp. of room, 48°; subject, a full-grown cock. Heat in recto, 107½°; and in ten minutes after adjusting the tube to the trachea, 106°. Decapitation being then performed, a galvanic current, excited by from 12 to 24 plates, weakly charged, causing slight twitchings of the wings, was applied at times, which, with the respiration and the heat, are expressed in the following table. The letters R. and G. denote the times when respiration and galvanism were continued.

	Minutes.	Heat.		Minutes.	Heat.
R.	1	105°	R. & G.	22	106°
R.	5	104	R. & G.	25	105½
R. & G.	7	105	R. & G.	27	105
R. & G.	9	106	0 0	29	104
R. & G.	10	106	R.	30	103
R.	12	106	R.	33	102½
R.	14	105½	R. & G.	35	102
R. & G.	16	105	R. & G.	37	102½
R. & G.	18	105	R. & G.	40	102
R. & G.	20	106			

As the artificial respiration was here uniformly conducted for the first twenty-nine minutes, the variations in the temperature during that time must be chiefly ascribed to the galvanism, the applications of which were distinctly and uniformly followed by a rise of temperature. The depression which succeeded to the cessation of both the respiration and galvanism at the twenty-ninth minute was such that the original heat was not recovered.

[The preceding experiments, in which galvanism was used, are highly interesting, in so far as this agent proved a means of exaggerating the calorific power which remained in the animals after their nervous energy was destroyed; but they carry with them no proof of the identity of this energy with the galvanic fluid. The experiments generally, however, do certainly prove that artificial respiration, when effectually performed, retards the cooling of animals, which it can do only by causing in some way a development of heat. How it effects this in the first instance, and why afterwards it fails to do so, and, on the contrary, becomes a cooling process, and accelerating the refrigeration of animals whose nervous energy has been destroyed, are the points which we have next to consider.]

If we compare with the general results of the experiments which have been described or noticed in the preceding observations, the views which I have advanced respecting the changes of the blood and animal heat, we shall find them to be in perfect consistency with the phenomena, and explanatory of them.

It is not impossible to reconcile these views even with the extreme results of Mr. Brodie's experiments. According to our theory, the carbonic acid exhaled by respiration is what has previously existed ready formed in the blood. The usual quantity of this gas might therefore be *emitted* for the first half hour, without a sufficient quantity being actually *formed* to preserve the heat of the body. That little carbonic acid is formed where the nervous influence has been destroyed, appears further probable, from the fact observed by Le Gallois, that the blood does not in this case assume the proper venous hue; and this is, most likely, because the secretions, which we have shown to supply carbon for this purpose, are suppressed. This suppression also removes the other process which we have supposed to be a direct source of heat—namely, the formation of the principles of the secretions.

If it is possible to explain these extreme results, there can be no difficulty in accounting for the phenomena observed by other physiologists, and by myself, in which for a short time animal heat *was* kept up by artificial respiration. Hence we must either ascribe to the functions of respiration and circulation the power of generating heat (although to small extent, unless supported by other functions), or, with those who attribute to the nervous energy a power almost omnipotent—assume that the heat generated after the removal of the brain is produced by the nervous organs still remaining in the body. This latter alternative is so entirely destitute of support, that it is unnecessary to discuss it. I will, however, remark of the only author who, to my knowledge, has advocated it, that he had a particular hypothesis involved in it. I allude to Dr. Wilson Philip, who considers animal heat to be a *secretion*, a *tertium quid*, resulting from the operation of the nervous energy, or electric fluid (for he holds them to be the same), on arterial blood*. If, by this expression, he implies that the evolution of heat is the only consequence of the supposed action, I affirm that his proposition is not in accordance with the known properties of heat, nor with the generally received opinions of its nature; but if he admits that the supposed action is attended with other changes, besides the mere evolution of heat, his term *secretion* is logically inaccurate; for the heat must be the result of these other changes (whether in composition or in condition) of the principles of the blood, and not the direct effect of the supposed nervous power on this fluid. That such was the case in Dr. W. Philip's experiments with galvanism and arterial blood, a perusal of his account of them will sufficiently prove; for the rise in temperature which took place on passing the electric current through florid blood was attended with a blackening and obvious change in the chemical condition of this fluid.

[Since the foregoing remarks were written, the opinions of philosophers respecting heat have undergone some change, the notion having gained ground that it is the undulation of an elastic medium rather than a distinct matter. This will but little affect our subject; for we have less to do with the nature of heat than with the laws of its development. We find heat continually evolved in the animal body; and if we can point out according to what physical or chemical law it is there produced, we explain its cause as far as is necessary in the science of physiology. Any notion which falls short of this is not an explanation. Such appears to me to be the opinion of Dr. Wilson Philip, above quoted. To call heat a secretion, in spite of its want of parallelism with any other secretion, is to substitute an hypothetical, and probably erroneous, term for an explanation. With more plausibility animal heat might be referred to the calorific power of a current of electricity passing through an insufficient conductor. When a large quantity of electricity is sent through very fine metallic wires, it will heat them even to incandescence and fusion. The heat is here truly a *tertium quid* (a *secretion*, if you will), resulting directly from the action of electricity on the metal; there being no chemical or physical change to account for it. But if we admit (as I think we must) that there are electric currents continually present in the animal frame, there is no trace of the conditions under which they can produce heat, as in the case just described. There is no where in the body a sign of so strong a current, or of insufficient conductors. Besides, nearly all the solids and fluids of the body are electrolytes, and chemical changes would accompany the passage of electricity through them, as in the case of Dr. Philip's experiment, which destroys the analogy with the case of conduction by a metal. If, then, electricity be a direct cause of animal heat, it must be through some property not at present known; and to assume gratuitously the existence of such a property, is contrary to all rules of philosophy.

The hypothesis that electricity is the direct cause of animal heat being unsupported by analogy or experiment, we have again to inquire whether the position which I have advanced, that the chemical changes continually going on in the system will explain it, is still tenable. This view requires further consideration than I had given it in the preceding essay, and it may be conveniently examined under the following propositions:—

1. Certain kinds of chemical change are attended with an evolution of heat.
2. Such kinds of change do take place in the living body.
3. Relations are observed to subsist between these changes and the production of heat in the living body.

1. It is well known that when bodies pass from a gaseous to a liquid state, or from a liquid to a solid state, they give out heat. The same phenomenon accompanies any change in a body from a rarer to a denser condition. The most numerous and remarkable sources of heat are, however, the changes accompanying chemical action; and at the head of these stands the familiar one of combustion.

The phenomena of combustion depend on a certain intensity and rapidity of chemical action, which, when moderate and slow, does not give out heat sufficient to be luminous. Thus a mixture of hydrogen and oxygen in the pro-

* Experimental Inquiry, &c.

portions to form water, when heated sufficiently, unite instantaneously, and by the heat, light, and explosion, exhibit the intensity of their mutual action; but if heated to a lower degree, or if much diluted with some neutral gas, and exposed to spongy platinum, they then unite slowly and silently, still giving out heat, but not with the rapidity and quantity necessary to constitute combustion and explosion. This example, although elementary, illustrates generally the principles of calorific union. We are apt to associate the generation of heat especially with combustion, forgetting that this phenomenon exhibits only the higher degrees of what occurs also extensively when similar chemical changes take place more slowly, or when circumstances prevent the accumulation of heat. Water has this latter effect in a signal degree; and as this property is materially concerned in the calorific processes which are to come under our notice, I will illustrate it by a simple example. We know how sudden and vivid is the combustion of dry gunpowder. When the mutual action of its component parts is developed, nothing interferes with the extrication of heat, which is accordingly intense and instantaneous; but if the powder be moistened, the evaporation of the water impedes the heating process, and the combustion proceeds through the mass more slowly, and less vividly. Again, expose the chief components of gunpowder to each other's action in a watery medium, by warming sulphur and charcoal in nitric acid; the chemical changes are much the same as in the former case, and heat is evolved; but the liquid impedes its extrication and the rapidity of the action, which therefore go on in a lower degree, and for a longer time. This instance of gunpowder is given, because its constituents and their mutual action are well known; but it would be easy to shew that many other chemical actions, which, at their acmé, exhibit the phenomena of combustion, are, in their lower degrees, and when retarded by the presence of a cooling mass of water, still attended with the gradual extrication of minor degrees of heat.

In a somewhat similar light may be viewed the processes of fermentation and putrefaction, which develope low degrees of heat; and their chemical nature, although different from that of common combustion, may be proved to pass into it by successive gradations. It is thus, as I have elsewhere shown, that many cases of spontaneous combustion originate. Take, for example, the spontaneous combustion of damp, or newly-made hay. This arises from a fermentation produced by the presence of moisture; but the heat thereby generated gradually dissipates the moisture, and developes the other ultimate affinities between the vegetable matter and the air. Hence the hay emits at first a fragrant steam, then an empyreumatic smoke, whilst its interior becomes charred by the increasing heat, which, under favourable circumstances, at last breaks out into open flame.

It would lead us beyond the immediate purport of this essay to pursue this part of the subject further here: the examples adduced will suffice to show how generally heat, in various degrees, accompanies certain chemical actions. *What are these actions which produce heat?* is a question still requiring a brief notice. It may be answered, *those kinds which tend to the formation of simple and permanent products.* The most efficient cause of heat by chemical change is the union of two contrary elements, with no opposing affinities to restrain their combination. Such is the union of oxygen with hydrogen, with carbon, &c. The next in calorific effect may be the open combustion of various animal and vegetable matters: the simplest products result; but the development of heat is somewhat impaired by the pre-existing slight affinities, and the presence of some oxygen, already in the combustibles. A lower degree of heat, but yet luminous, is produced in the low combustion which I have shown to affect most combustible bodies, both simple and compound, at temperatures considerably below red heat*. The products of this combustion in vegetable and animal matters are, according to the kind, chiefly water, empyreumatic acetic acid, oxalic acid, carbonic acid, and ammonia; all more simple in ultimate composition than the combustible. Lastly, the lowest cases of calorific change are those of fermentation and putrefaction. In one sugar is converted into alcohol and carbonic

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* Transactions of the British Association for the Advancement of Science: vol. iii., Chemical Section.

acid, or alcohol into acetic acid; in the other, animal and vegetable compounds are resolved into ammonia, carbonic and hydrocyanic acids, carburetted hydrogen, and the like: in both these cases the change being from the more complex organic principles to combinations simpler, and more resembling the permanent products of combustion. We may therefore reduce all the above instances to this general law—*the evolution of heat during chemical action is, cæteris paribus, in proportion to the change from isolation, or weak combination, to firm and simple union.*

2. Our next position to establish, is, that such chemical changes as have been just shown to evolve heat, do take place in the living body. We know of no processes in which the condensation of gases, or solidification of liquids, can become a source of animal heat. It is true that, in nutrition or reparation, the solids are formed of the liquid blood; but this is balanced by the opposite process of decay, in which the solid structures are again removed in a fluid state by the various absorbent vessels; and by the operation of chylification, in which a liquid is extracted from a more or less solid mass of aliment.

But the several chemical changes in the blood which we have been considering in the preceding pages, I apprehend we shall find included in the definition above given of the production of heat by chemical action. That the union of carbon and oxygen must prove in the body as elsewhere a source of some heat, is too obvious to need further argument. I shall here only consider the changes effected in the formation of the secretions. Here, from the highly perfect animal principles, albumen and fibrine, which originate only in living bodies, are susceptible of organization, and become the medium of some of the most remarkable properties of life,—we find produced, urea, uric acid, ammonia, picromel, resin of bile, lactic acid, and the like; matters not only totally insusceptible of organization, but obviously approaching in nature to inorganic substances, and capable of being formed by the decomposition of other organic matter, and in some instances even by the synthesis of inorganic compounds. Thus urea consists of precisely the same elements as the hydrated cyanate of ammonia; it may be formed artificially by the action of ammonia on cyanogen; and as it exists in urine, a boiling heat is sufficient to resolve it into the still simpler carbonate of ammonia. The uric and lactic acids also approach to ultimate compounds; the former differing but little from some of the combinations of cyanogen, and the latter being the product of fermentation of milk and other animal fluids. Of picromel and the resin of bile we can speak with less certainty; but their ultimate composition, resembling oils, varieties of hydro-carbon, bespeaks the simplicity of their nature.

As we thus find the principle of calorific change fulfilled in the formation of the excretions of the animal body, so we may have experimental illustrations of it in the further history of the excreted matter. Thus in the septic properties and progressive decomposition of the urine and dung of animals we see a continuance of the simplifying process which had begun in the body; whilst the attendant heat, so familiar in the steaming dunghill, and usefully applied in hot-beds for forcing plants, exhibits the constancy of the phenomenon, which in its earlier periods had assisted to sustain the temperature of the living animal.

3. The last position to be examined, is, that relations are actually observed to subsist between the chemical changes going on in the body, and the animal temperature.

I need not dwell on the general relation observed between the heat and the perfection of the respiratory process in healthy animals. This has been sufficiently pointed out by the various writers on the subject, from the time of Dr. Black and Lavoisier. The experiments of Edwards have added a new proof, in the fact that young animals gain the power of preserving their own temperature in proportion as respiration becomes necessary to them. Animals born blind, or without covering, partake somewhat of the fœtal state, and will live for a short time without air; in this condition they have very little power of generating heat, and depend in great measure on the warmth of the mother: but as the respiration and connected functions become more perfect and indispensable, they gain the power of sustaining their own temperature. The exceptions to this law of relation in the experiments of Sir B. Brodie, and of those who have followed him, do not disprove it; they only shew that a certain integrity of the nervous function as well as of the respiratory is necessary for the preservation of animal heat. But, as it has been already observed in this essay, these experiments afford a direct illustration of the relation which we seek to establish between certain chemical changes and animal heat. A quotation already given from the accurate Chossat comprehends the result of his and all other experiments on this point. "*All the lesions of the nervous system which diminish the production of animal heat, act in a similar way on the secretions.*" Here, then, we find the exception proving the rule. A certain integrity of the nervous function is necessary to maintain animal heat, because it is necessary to the continuance of those chemical processes on which animal heat depends. It is directly necessary to the formation of the secretions, (one cause of heat) and as these supply carbon to maintain the production of the carbonic acid of respiration, (the other cause) the nervous energy is also necessary to the continued perfection of this function. Some production, as well as exhalation, of carbonic acid, may take place after the destruction of the nervous influence; and my own and other experiments shew that some heat is at the same time evolved. But this portion of calorific power is insufficient to preserve the heat of the blood: hence, besides the absence of the agency which forms the secretions, the increasing depression of temperature diminishes even the changes which the oxygen absorbed can *chemically* effect; the cooling is therefore progressively rapid, and when it has reached a certain degree, artificial respiration accelerates instead of retarding it.

I have before had occasion to cite from Dr. Hodgkin's notes to Edwards, some experiments of Sir A. Cooper which illustrate the effect of intense cold in arresting the changes of the blood in a sound animal, that in the veins retaining the arterial hue. It at once destroys that nervous influence, and impairs those chemical affinities, which jointly work the calorific changes. Such a moderate degree of cold as the vital powers can react upon has an opposite effect; more counteracting heat is generated, the blood becomes darker, and more carbonic acid is given out from the lungs;* and it is this exhibition of a property peculiarly vital, adapting the laboratory of the body to a variety of states, that distinguishes living from dead chemistry, and makes the very laws of decomposition subserve to maintain the heat and health of the body. We are familiar with the effect of external cold in increasing the secretion of the kidneys, and are used to ascribe it to the mechanical change of the circulation of blood thrown inwardly from the constricted surface; but nature's beneficial purposes are not limited to this, for in this inward determination of blood we also see a beautiful provision for the maintenance of animal temperature by an increase of those changes in which the internal viscera are materially concerned.

* Crawford, Lavoisier, &c.

The relation of the heat and respiration of very young animals has already been noticed; but we have to add the correspondent relation of the state of the secretions. In the fœtus the secretions are scanty, and devoid of distinctive character, the power of producing heat being at the lowest; but as soon as the animal has breathed, the vital energies are excited, the chemical changes are promoted, the glands yield their peculiar secretions, and the power of maintaining heat is proportionately acquired.

These last considerations deter me from reckoning *nutrition* among the chief calorific processes of the animal body. The only way in which I see that it can contribute to produce heat is in the trifling extent to which it may (as before noticed) supply carbon for the respiratory function. But the opposite process, *decay*, which is supposed by physiologists to be continually affecting the tissues of the body, is essentially one of those simplifying changes which are always attended with an evolution of heat, and we thus find that this process, which has hitherto been deemed a result of defective composition, whether we view it as a distinct operation or as a part of the function of excretion, answers the useful and important purpose of contributing to sustain the heat of the body.

The necessity of a controling power over all the chemical changes which thus sustain animal heat is as obvious as is the need of a similar influence to predominate over the various physical properties of the animal machine; and if in the agent by which that control is, exercised we recognise some characters which approximate it to electricity, we step but little higher in the mysteries of vitality. What directs this agent, and what are its relations to the physical and chemical laws which act upon it, and which it so powerfully and beneficially controls, are matters which according to our present knowledge are entirely beyond our comprehension. We have been studying the chemical changes as they do occur under this unknown influence, and find in them a sufficient cause of the heat of the animal frame. This inquiry, far from degrading our view of the animal economy into a mere application of chemical or mechanical laws, exalts and enlarges it, in the further proofs which it has afforded, that these laws, which in dead matter tend to decompose and to destroy, are made under the influence of vitality to warm, sustain, and purify the living body. In the variations of disease, which are too numerous to admit of present notice, we may find proofs of insubordination in the chemical agencies which sometimes seem even to predominate for a while; and in the balance of these against the opposite ones of vital reaction, are comprehended many of the phenomena of morbid action. The physical condition of temperature is, however, still amenable to the laws of its production; and in the highly charged excretions and increased carbonic acid expired in inflammatory fevers, we see the causes of augmented heat; whilst the lower temperature in typhoid, cachectic, and dropsical states, is accompanied by a diminished excretion from all the important organs.* Other causes may doubtless be assigned for these variations of functions and phenomena; nor do I deny their reality: and in adducing these new relations of acknowledged facts, I do but shew further instances of the admirable economy of nature, by which the chain of causation, although consisting of many links, is at once simple, comprehensive, and harmoniously adapted to a variety of ends.

Half-moon Street, Piccadilly,
Oct. 5, 1835.

* Nysten, Recherches, &c. p. 202. Apjohn, Dublin Hospital Reports, Vol. V.

WILSON AND SON, PRINTERS, 57, SKINNER-STREET, LONDON.