AN

INQUIRY

INTO THE

CHANGES INDUCED

ON

ATMOSPHERIC AIR,

&c.

Abernethy & Walker, Printers,
Old Bank Close.

AN

INQUIRY

INTO

THE CHANGES INDUCED

ON

ATMOSPHERIC AIR,

BY

THE GERMINATION OF SEEDS,
THE VEGETATION OF PLANTS, AND
THE RESPIRATION OF ANIMALS.

By DANIEL ELLIS.

EDINBURGH:
PRINTED FOR WILLIAM CREECH;
AND J. MURRAY, FLEET STREET, LONDON.
1807.

TO

NICHOLAS NUGENT, M. D.

THIS TREATISE IS INSCRIBED,

IN TESTIMONY

OF RESPECT AND AFFECTION,

BY

THE AUTHOR.

PREFACE.

THE investigation of the subject of the following Treatise was suggested to the author by accidentally observing the spontaneous recovery of an animal in whom all the appearances of life had been suspended by drowning. Reflecting on this part of the pathology of respiration, and on the theories which have been proposed to explain it, he was led to consider with particular attention the physiology of that function. The result of his inquiry terminated in a conviction, that although many great and important steps had been made, yet much hypothetical conjecture was blended with established fact, and many suppositions were admitted into our theories which but ill accorded with the structure and economy of the animal system.

By comparing attentively the chemical facts which relate to this function with certain pathological appearances, and considering both in connection with the actual structure of the respiratory organs, he was induced, not only to reject the sufficiency of the explanations which have been hitherto proposed, but to form some opinions on the subject more consonant, as it appeared to him, with the real designs of nature. In the course of this investigation, analogy readily suggested to him a comparison of the facts ascertained in human respiration with those which have been observed in the respiration of the inferior animals; and, from the lowest order of animal beings, the transition to the analogous phenomena which occur in the vegetable kingdom, was natural and obvious. Thus, in a descending series, all the great classes of animated nature were successively brought under his review; and, arriving ultimately at the most simple form of existence, he was led to make it the first subject of investigation, and then to retrace his steps through the more complex and perfect forms of vegetable and animal life.

It has been the constant aim of the author to make observation and experiment the basis of all his reasonings, and to deduce his conclusions from a full and distinct consideration of all the circumstances which seemed necessarily to affect them. The facts which he has introduced in support of these conclusions have been collected from the best sources: and where they appeared to him to be either erroneous or defective, he has, in many cases, attempted to correct or supply them by experiments of his own. On every occasion of importance, where his results differed materially from those of preceding authors, he has endeavoured to compensate for the want of experience by a frequent repetition of his experiments: and, without regard to any particular opinions, he has detailed their results with all the accuracy in his power. These results, it is hoped, will, in general, authorise the conclusions which have been drawn from them; but, he is well aware, that many points still remain which require farther elucidation, and are susceptible of more accurate proof. In the present state of science, however, the physiologist must often be content with probability, nor expect to attain to demonstration but by very gradual approaches to truth.

Much attention has likewise been bestowed to present the several facts, connected with the subject, in the order in which they have been successively made known. By thus blending with the recital of facts, the history of their gradual development, an additional degree of interest is imparted to them, and an opportunity is, at the same time, afforded of doing justice to those eminent persons who have preceded him in this branch of inquiry. To those also who feel a pleasure in tracing the progress of the mind through the details of an extensive and complicated investigation, it may be interesting to observe, in what a slow and successive manner the knowledge which we at present possess has been acquired: how many individuals, often at very distant periods, have been employed in adding to it: and how many observations, apparently, at first, trivial and insulated, have, in the progress of science, assumed an unexpected importance, serving to connect together a long series of facts, which, but for such casual aid, might still have remained disjointed and broken. Reflections such as these, while they encourage every one to contribute his share, however small, to the general stock of knowledge, may, at the same time, serve to abate somewhat of that unpardonable vanity which has led some philosophers to assume the sole merit of having raised the edifice, when, in fact, the foundation was entirely laid by others, and they themselves have only assisted in arranging and giving form to the materials of which the superstructure may hereafter be composed.

Without entering into formal and minute description, it has been a principal object through the whole of this inquiry, to combine anatomical fact with those reasonings which relate immediately to the living system. Unfortunately for physiology, this circumstance has not always been sufficiently kept in view, and the laws which govern the movements of inanimate matter have too frequently been applied, without reserve or discrimination, to explain the functions of organized and living beings. To a certain extent, indeed, animated bodies are subject to the same laws as all other material substances; but the peculiar properties which they derive from the principle of life can never be explained on mechanical or chemical principles. Although, therefore, it will be found, that chemical phenomena have received a due share of attention, yet they have at all times been held in subservience to the established truths of anatomy, and to those laws which peculiarly belong to bodies possessed of the principle of life.

In the present Treatise, no reference whatever has been made to the theories which have been proposed to explain the phenomena of vegetation and respiration. Such theories, it is evident, must rest entirely on a knowledge of the changes produced by living bodies on the air; and it is the professed object of this inquiry to examine, with more attention than has yet been done, into the nature and extent of these changes. It will be proper, therefore, in the reader to abstract as much as possible from any preconceived opinions on these subjects; and to consider, that it is only by a scrupulous examination of the facts adduced, and of the legitimacy of the conclusions drawn from them, that the views of the author can be finally established or overthrown. To try the merits of these views, not by the evidence on which they rest, but by anticipating difficulties which they may be supposed hereafter to create, would be an open departure from the only mode in which we can ever hope faithfully to interpret the laws of nature.

Whatever may be the ultimate judgment passed upon the general merits of his performance, the author indulges the hope, that some advantage at least may indirectly result from it. The extensive series of facts which he has brought together, and the analogies which, from the evidence of experiment, he has endeavoured to trace among them, may direct the attention of future inquirers to a more comprehensive view of the subject than has yet been taken, and impart a new degree of interest and utility to the research. The attempt also to combine, in every instance, the demonstrations of anatomy with the chemical phenomena which we observe, and to consider both in connection and subservience to the laws which characterise living beings, will, he trusts, meet with the approbation of physiologists; and tend to reduce within proper limits the application of chemistry to this science. At any rate, the present inquiry may serve to recall to a new and more accurate examination, the facts on which our knowledge of these subjects is founded: and in the present state of science, such an examination cannot fail to dispel much of the error and obscurity in which they are still involved.

EDINBURGH,
MARCH 20. 1807.

CHAP. I.

OF THE CHANGES INDUCED ON THE AIR BY THE GERMINATION OF SEEDS.

1. It is a well-known fact, that dried seeds, although exposed to heat and air, may be kept for a great length of time without undergoing any sensible change; nor do they in that state, as we found by experiment, in the least degree affect the air in contact with them. If, however, moisture have access to them, they presently begin to swell, which is the first step towards their evolution. This happens whether they be placed in water containing air, or in that which has been previously deprived of its air by long boiling; for equal numbers of the same peas, at the temperature of 60° Fahrenheit, being put into separate phials of fresh and of boiled water, and then corked and sealed, exhibited the same appearances, and made a like progress. In a few hours the seeds in both phials were much enlarged, and by the next day greatly swollen: the radicles of many

began to unfold, and they afterwards grew well, if duly exposed to the air; but when retained in this situation for a few days, their outer coats began to separate; the water in each phial emitted a putrid smell, and, on being added to lime-water, threw down a copious precipitate, after which germination did not take place. Mr Gough ascertained, that after five days steeping in water at 46°, and even in forty-eight hours, at temperatures from 60° to 66°, putrefaction in seeds came on, under which carbonic acid and carburetted hydrogen gases were produced, and the faculty of germinating was then destroyed *. M. Huber found also, that both in boiled and distilled water, a small degree of germination might be made to take place in peas; but during their submersion, the radicles never increased beyond three or four lines, after which they began to decompose, carbonic acid being first produced, and afterwards carburetted hydrogen gas. The whole substance of the pea, with the exception of its membranes, is, according to Saussure, reduced into these two gases †. It is evident from these facts, that the presence of water alone, is essential to the commencement of germination; and that if its application be too long and exclusively continued, it disposes to putrefaction.

2. Besides water, a certain degree of heat is necessary also to the germinating process; for no seed

* Manchester Memoirs, vol. iv.

† Mém. sur la Germination, par M. M. Huber et Sennebier, p. 128. 157.

can be made to grow at or below the freezing point, and above that point, the degree of growth will be always more or less influenced by that of temperature. Many seeds, however, will germinate as well as ever after having been frozen, or having been kept in frozen water. Although cold, therefore, does not destroy the germinating faculty of seeds, yet a certain degree of heat is essential to the display of it, and almost every species of seed seems to require a degree peculiar to itself; for each has its peculiar season of germination, which season varies with the temperature of the air.

3. Light is another agent which has been supposed to possess a considerable influence in the germination of seeds. The experiments of Hooke, Scheele, and Herschell, have established the distinction between the heating and illuminating powers of the rays of light: and a third portion of these rays has been supposed to enter more peculiarly into combination with various substances, and thence have acquired the denomination of the Chemical Rays. Dr Ingenhousz and M. Sennebier considered the presence of light to impede the growth of seeds; but the Abbé Bertholin affirmed, that the difference in the germination of seeds in the shade and in the light was owing not to the light itself, but to the greater evaporation of moisture which seeds exposed to the light suffered: and he added, that if the seeds in both cases were kept equally moist, those in the sun germinated sooner than those in the shade. M. Sennebier, however, repeated his experiments, and employed every precaution to ensure an equality of moisture in both situations, and he constantly found

the seeds in the shade to germinate sooner than those in the light *. The more equable warmth which seeds preserve, when excluded from the direct contact of light, may be a principal reason why they grow best in the dark; and the evaporation which takes place on exposure, to which the Abbé alludes, would not only deprive the seed of the water essential to its growth, but be at the same time a considerable source of cold. In many trials we found, that steeped seeds, when exposed in the open air, rapidly lost their moisture, and made no effort to germinate; but the same seeds, when confined in a glass-vessel, so as to check the progress of evaporation, germinated freely, though exposed to the same temperature, and the same degree of light. Hence we infer, that light, simply, has little influence either in promoting or retarding the germination of seeds.

4. But although water and heat appear to be the only agents essential to the beginning of germination, yet after a certain period air becomes equally necessary. Mr Boyle, Ray, and many other philosophers, ascertained that seeds would not grow in the *vacuum* of an air-pump. Some lettuce-seeds were sown upon earth in the open air, and some of the same seeds, at the same time, on another portion of earth placed in a glass-receiver, which was afterwards exhausted of air. The seeds exposed to the air grew nearly an inch and a half high in eight days; but those in the exhausted receiver not at all. Air, however, being now admitted into the receiver,

* Thomson's System of Chemistry, vol. iv. p. 236. 1st Edit.

it was found, that in the space of one week the seeds had grown to the height of two or three inches *. Mr Gough observed, that steeped seeds put into a close vessel so as to fill it, did not grow, but that when a smaller number was put into the vessel, and its mouth covered loosely with a piece of glass, so that the air had free access to the seeds, they germinated very freely; experiments which we have often repeated, and always with the same results. Since, then, air is essential to the progress of germination, and it is known that this elastic fluid consists of two elementary parts, we may next inquire what changes this air undergoes, and whether one only, or both its parts, be required in this process.

5. It appears from the experiments of Mr Achard and others, that no seed will germinate in nitrogen gas, which gas forms nearly four-fifths of the air of the atmosphere. Mr Gough placed some barley-seeds, previously steeped in water, in a jar of this gas. At the end of three days they had suffered no change; the bulk of gas was not perceptibly altered, nor did it diminish by agitation in water, which showed that no carbonic acid was formed: but when the place of the nitrogen gas was supplied by atmospheric air, the same seeds germinated freely, and formed carbonic acid, just as if they had never been confined in nitrogen gas at all. If, however, they remained three or four days in this gas when the weather was warm, they began to putrefy in the same manner as when retained too long in

* Lowthorpe's Abrid. Phil. Trans. vol. ii. p. 206.

water, carbonic acid and carburetted hydrogen gases being formed, after which they could not be brought to germinate *. Mr Cruickshank introduced some soaked barley-seeds into a jar containing nitrogen gas, inverted over mercury. At the end of twelve or fourteen days there was not the least appearance of germination, but the gas had increased in bulk about one-fifth, containing from one-third to one-fourth of its bulk of carbonic acid; but neither the original nitrogen gas nor the barley had undergone any sensible change †. M. Huber found also, that a quantity of carbonic acid was produced when seeds were placed in a jar of nitrogen gas, and duly supplied with water; and that in a few days the radicle in a small degree was protruded, which he considers as a proof of germination ‡. But it appears that the same protrusion of the radicle takes place when seeds are submerged in water; and M. de Saussure, who repeated these experiments in pure nitrogen gas inverted over mercury, observes, that the radicle elongated only in a small degree, which is the consequence of the swelling of all the parts of the seed from the imbibition of water, and must not be considered as a true germination §. From these facts, therefore, it may be concluded, that although seeds, after being steeped in water, yield carbonic acid when confined in nitrogen gas, yet that they are in no respect affected by it; neither does that gas itself undergo any sensible alteration.

---

* Manchester Memoirs, vol. iv. p. 322.

† Experiments on Sugar, in Rollo on Diabetes, vol. ii. p. 218.

‡ Sur la Germination, p. 175.

§ Ibid. p. 184.

6. As thus the nitrogenous portion of the air seems neither to produce nor to suffer any change in the process of germination, let us next inquire into the changes which the oxygenous part, the other elementary portion of our atmosphere, undergoes. Mr Scheele and Mr Achard proved that oxygen gas was absolutely necessary for the germination of all seeds. Mr Gough likewise observed, that steeped barley-seeds, supported by means of a small hoop in an inverted jar of atmospheric air, germinated freely: the residual air, on the fifth day, being passed through lime-water, precipitated the lime, and, when thus freed from carbonic acid, was found to have lost nearly one-sixth of its original bulk, and the remaining air repeatedly extinguished a taper *. Some grains of barley, which had been soaked in water twenty-four hours, were introduced by Mr Cruickshank, on the 1st of December, into a jar of common air inverted over water, and preserved in a temperature between 60° and 70°. At the end of five days they began to grow, and on the 28th day, the greatest part of them had thrown out shoots half an inch in length. Germination continued to proceed on the 7th of February, when the barley being withdrawn, was found to be very sweet, and nearly converted into the state of malt. The air in the jar was found to consist only of nitrogen and carbonic acid gases, the whole of the oxygen having entirely disappeared. When the experiment was made in a jar containing forty-six measures of very pure oxygen gas inverted over mercury, the process of ger-

* Manchester Memoirs, vol. iv. p. 319.

mination was considerably hastened: the column of gas on the 10th day had suffered no apparent change; and the residual air then consisted of carbonic acid, mixed with only 1-50th of its bulk of oxygen. The barley was partly converted into malt*. These experiments teach us, that oxygen gas is essential to the process of germination; that it gradually and completely disappears during the continuance of that process; and that a large quantity of carbonic acid supplies its place.

7. M. de Saussure *junior*, instituted some experiments to determine the proportion which the carbonic acid, produced in germination, bore to that of the oxygen gas, which disappeared in that process. He conveyed eighteen peas into a glass-vessel, the mouth of which was plunged in mercury, and which contained 11.5 cubic inches of atmospheric air, that had been previously well washed in lime-water. About one-fourth of a cubic inch of water was then passed into the jar, and floated on the mercury, so that the peas were about half immersed in it. In ten days the radicles had sprouted about one-third of an inch; and the air in the jar, after making the necessary corrections for pressure and temperature, had undergone no sensible diminution of volume. The residual air being submitted to analysis, lost $\frac{9}{100}$ by agitation with lime-water, and the remainder afterwards suffered a further loss of $\frac{12}{100}$ by the eudiometrical test of phosphorus. If the air he employed be held to contain $\frac{21}{100}$ of oxygen gas, (which

* Experiments on Sugar, p. 218. 213.

that of his laboratory was found to do), the 11.5 cubic inches of it which he used in this experiment, will, he says, consist, before the introduction of the peas, of 3.105 of oxygen, and 8.395 of nitrogen: and at the close of the experiment he states the composition of the air at 1.88 oxygen, 8.395 nitrogen, and 1.035 carbonic acid; consequently 1.255 of oxygen were employed to form 1.035 cubic inches of carbonic acid, a result as near to truth as can be expected in observations on small volumes of air. In other experiments made over water, nearly the same results are said to have been obtained *. But some error seems to have crept into the calculations of M. de Saussure; for, if with him we consider the atmosphere as containing $\frac{21}{100}$ of oxygen gas, 11.5 cubic inches of air will contain 2.415 of oxygen, and 9.085 of nitrogen; proportions very different from those which he has assigned, and which necessarily affect all the subsequent calculations, and the conclusions he has drawn from them.

8. With the view, therefore, of ascertaining this point with more precision, we placed one dozen of peas, which had been steeped in water thirty-six hours, and whose surfaces were dried afterwards by blotting paper, on a small whalebone hoop covered with gauze, which was pushed half way up into a glass-jar well dried, and of the capacity of 18.2 cubic inches. This jar, filled with atmospheric air, was then inverted into a deep saucer, where it stood over a small glass-cup, containing half a cubic inch

* Journal de Physique, tom. xlix. p. 92.

of the water of potassa, which is well known to have a strong affinity for carbonic acid. A quantity of mercury was now poured into the saucer, and the jar was kept steady by a weight laid upon it. The whole was then set aside in a room, where the barometer stood at 29.3 inches, and the thermometer at 62.5°. By the following day, the peas had sprouted about 1-4th of an inch, the mercury in the saucer had risen 3-10ths of an inch into the jar; and as the small cup with its solution floated on the mercury, it was necessarily raised into the jar with it. By the end of the second day, the radicles were 3-4ths of an inch long, and the mercury had risen nearly 7-10ths of an inch into the jar, as appeared by a small paper scale, graduated to inches and tenths, and pasted on the outside of the jar. At the close of the third day, the radicles were to appearance about an inch long, and the mercury in the jar was nearly an inch in height. And by the fourth day, the radicles had made little additional progress, but the mercury stood nearly 1.1 inch in the jar. During the following day, no apparent changes occurred, and the mercury in the jar was now exactly 1.1 inch in height, when that in the saucer was brought to a level with it. The barometer at this period was 29.95, and the thermometer 65°.

9. Having by other experiments ascertained, that when the mercury had reached this elevation, and became stationary, the oxygen gas of the air employed had completely disappeared, we proceeded to determine the proportion which this diminution in the bulk of air, evinced by the rising of the mercury, bore to that of the oxygen gas which the air

originally contained. The jar, therefore, was cautiously raised out of the mercury, and the attraction of carbonic acid by the alkaline solution was proved by the brisk effervescence which the acetous and diluted sulphuric acids excited in it. The glass-cup, containing a bulk of water equal to that of the solution employed, was now again put into the jar, previously replaced on its bottom, together with the hoop and peas. Water was next poured in till it reached that part of the jar to which the mercury had risen, and to raise it thereto required 12.5 cubic inches, which, therefore, was the bulk of residual air at the close of the experiment; and afterwards, to fill the jar completely, it required four cubic inches more, which consequently was the measure of the volume of air that had disappeared. But these two quantities amount only to 16.5, and the capacity of the jar has been stated to be 18.2 cubic inches: the difference, therefore, equal to 1.7, will be the space which the several substances occupied within the jar, and which reduces the actual volume of air employed in the experiment to 16.5 cubic inches, of which a portion equal to four, or $\frac{1}{4.125}$ of the whole, had disappeared.

10. But the volumes of elastic fluids are inversely as the weights by which they are compressed; and for every degree of Fahrenheit's thermometer, Mr Dalton has found that atmospheric air is dilated $\frac{1}{483}$ part of its bulk under common atmospheric pressure *. Making therefore, on these accounts, the

* Manchester Memoirs, vol. v.

necessary reductions, we have $\frac{29.95 \times 12.5}{29.3} = 12.777$; but $\frac{2.5 \times 12.777}{483} = .0661$ and $12.777 - .0661 = 12.7109$, which is the actual volume of air at the end of the experiment when reduced to the same pressure and temperature as it possessed at the beginning. To find, farther, the proportion which this loss of bulk bears to that of the whole air employed, we have $16.5 - 12.7109 = 3.7891$ and $\frac{3.7891}{16.5} = \frac{1}{4.354}$. But atmospheric air contains $\frac{22}{100} = \frac{1}{4.54}$ of oxygen gas, so that the loss in the bulk of air by this experiment is rather greater than the proportional quantity of oxygen gas which the atmosphere contains. In another trial, made in the same jar, and under similar circumstances, the loss of bulk which the air was found to have suffered, after the necessary reductions were made, was $\frac{1}{4.43}$ of the whole. Hence, therefore, if it be granted that the loss of bulk which the air suffered arose from the attraction of the carbonic acid by the water of potassa; and if it be also allowed, that all the oxygen gas of the air employed had completely disappeared, it must be concluded, that the bulk of carbonic acid produced nearly corresponded to that of the oxygen gas lost. This, too, will appear more directly, by comparing the diminution which a given bulk of air suffers in germination, with the proportion of oxygen gas which the atmosphere contains: Thus, 100 : 22 :: 16.5 : 3.63, which comes near to 3.78, the actual loss of bulk which the air in the foregoing experiment underwent.

11. That the whole loss of bulk which the air suffered in the foregoing experiments, did actually arise from the formation of carbonic acid, and its subsequent attraction by the alkaline solution, appears almost certain, from the opposite results obtained when no such solution was employed. In the experiment of Mr Cruickshank, (6.), made in pure oxygen gas, the original volume of gas suffered neither increase nor diminution, although it was almost entirely converted into carbonic acid; and the same observation was made in all the experiments of Saussure. In a great number of experiments also, we observed, that where no water of potassa was placed within the jar, the seeds equally grew; the oxygen gas was completely destroyed, and the residual air lost about 1-5th of its bulk by agitation in lime-water, although the volume of air was only in a small degree diminished. This trifling diminution was probably owing to the necessary condensation which oxygen gas experiences when it combines with carbon, and which, according to Crawford and Guyton, amounts to about 1-7th of the bulk of gas employed *, although it is probable that this estimate is considerably overrated.

12. We endeavoured to ascertain the amount of this diminution in germination, by causing some soaked peas to grow, supported on a whalebone hoop, in a small flask with a narrow neck, which was inverted into an ale-glass containing mercury. When the peas had ceased to grow, the rise of the

* Murray's System of Chemistry, vol. ii. p. 352.

mercury into the neck of the flask was accurately marked, and the air was then analyzed. The destruction of its oxygen gas was found to be complete, and the usual quantity of carbonic acid to be formed. After deducting the space occupied by the seeds and the hoop which supported them, the volume of air, at the close of the experiment, was compared with that which it possessed at the beginning, and the difference noted. An estimate was then made of the quantity of oxygen gas originally contained in the air employed, and the loss of bulk was considered to arise from the condensation attending its conversion into carbonic acid, due attention being paid, at the same time, to corrections for pressure and temperature. The average amount of the diminution varied from 1-8th to 1-12th; or the volume of carbonic acid produced was about 1-10th less than that of oxygen gas which had disappeared. In some trials the amount of the diminution was much greater, but this was found to depend on the quantity of moisture given out by the seeds, and the more or less vaporific state in which it existed, and which source of error can be obviated only by reducing the air to the same state with regard to moisture at the end, as at the commencement of the experiment. From the whole, however, we may conclude, that a certain condensation attends the conversion of oxygen gas into carbonic acid in the process of germination, as well as in combustion; and consequently that a part of the diminution which the air suffers, is to be attributed to this cause. If this were not the case, the bulk of carbonic acid produced would appear greater than that of oxygen gas lost, which the experiments both of Cruickshank and Saussure forbid us to believe, and which, in all our own experiments, appeared to be somewhat less.

13. If, then, we may be permitted to suppose, that in the process of germination the oxygen gas of the air loses about 1-10th of its bulk by passing into carbonic acid, it follows, that the 3.78 cubic inches of this acid, which appeared to be attracted in the foregoing experiment, (10.), will actually consist only of 3.402 cubic inches. Now, one cubic inch of carbonic acid weighs 0.467 of a grain, and therefore 3.402 cubic inches will weigh 1.589 grains, which may be taken as the weight of the carbonic acid actually produced. But farther, a cubic inch of oxygen gas weighs 0.3474 of a grain; and therefore 3.63 cubic inches (the bulk of that gas present in the experiment) will weigh 1.261 grains, which is less by 0.328 of a grain than the weight of the carbonic acid produced. This greater weight of the acid, therefore, must be attributed to the carbon which united with the oxygen gas to form it, and which makes the carbon to constitute $\frac{1}{4.84}$ of the compound. According to Lavoisier, carbonic acid contains $\frac{28}{100} = \frac{1}{3.57}$ of carbon; and according to Dr Priestley, the weight of the carbon is about 1-4th of the compound *: but Guyton, who formed this acid directly by the combustion of diamond, found that one part of diamond combined

* Experiments on Air Abridged, vol. iii. p. 377.

with a little more than four of oxygen, and consequently that the proportion of carbon was only 1-5th of the compound *. Hence, therefore, if these experiments and calculations be correct, the quantity of carbon, which enters into the carbonic acid formed by germination, is rather greater than the experiments of Guyton assign as the proportion of that substance which forms this acid in combustion.

14. M. Huber made experiments with the view of ascertaining the composition of the atmosphere best suited to carry on the germinating process. He found, that an artificial atmosphere, composed of the same constituent parts as common air, favoured it precisely in the same manner; that germination was retarded in proportion as the nitrogen gas exceeded the usual quantity of that gas contained in atmospheric air, but was little affected when the variation was small. A superabundance of oxygen gas also, although not destructive to germination, nevertheless retarded the process; for seeds were found to germinate better in a mixture formed of three parts nitrogen and one of oxygen, than in one composed of three of oxygen mixed with one of nitrogen †. Hydrogen gas, in the course of these experiments, was often substituted for nitrogen, and answered precisely the same uses, germination being more vigorous in proportion as the hydrogen came near to that of the nitrogen existing in the at-

* Annales de Chimie, tom. xxxi. p. 108.

† Sur la Germination, p. 31. et seq.

mosphere; but neither in pure hydrogen nor nitrogen did the process at all go on *.

15. M. Huber proceeded next to ascertain the smallest quantity of oxygen that would suffice for germination. He found, that seeds did not grow in very small volumes of air; and that the greater the number of seeds confined in a given volume of air, the more was their growth retarded; that where the oxygen gas constituted only one-eighth of the atmosphere, no effect was produced on lettuce-seeds, but that one-sixth of it began to promote their growth. Lefevre, however, is said to have found, that an atmosphere containing only $\frac{1}{32}$ part of oxygen, favoured the evolution of raddish-seeds, which renders it probable that different seeds vary much in the proportion of oxygen necessary to carry on their germination †. When the atmosphere was impregnated with the vapour of sulphuric ether, of camphor, spirits of turpentine, assa-fœtida, vinegar and ammonia, the process of germination was more or less retarded. From the facts stated in this and the preceding paragraph, we learn, not only that air is necessary to the growth of seeds, but that the ordinary state of its composition in our atmosphere is, as M. Sennebier observes, the best adapted to produce a vigorous germination. This state, however, is not determined with such rigour that a small variation in the proportions or qualities essentially injure the process: it admits, on the contrary, of great latitude without much disturbance, which provision

* Sur la Germination, p. 58. † Ibid. p. 64. et seq.

affords a correction for those occasional variations which our atmosphere experiences, and insures the preservation of vegetables in all circumstances in which they may naturally be placed *.

16. Such being the principal facts in germination which bear a relation to the changes which the air suffers, let us next inquire in what manner they are to be accounted for or explained; and first of all, what becomes of the oxygen gas that disappears? Mr Gough believes that seeds, during their germination, *absorb* oxygen gas from the air, retain a part of it, and reject the remainder, charged with carbon †. Now although seeds have a power of imbibing and exhaling water, there is no evidence of their possessing a structure fitted to absorb and expel aëriform fluids; neither is there any proof of such fluids at any time existing in them. If the air be absorbed in its entire form, some receptacles ought to be shown in the seed for retaining and decomposing it, and from which the nitrogen gas could be afterwards expelled: or, if the oxygen gas alone be conceived to enter, then it may be asked, by what power of *absorption* it could be separated from the nitrogen gas with which it was previously combined? Mr Cruickshank inclines also to the belief, that the oxygen gas is chiefly absorbed by the seed, although part may, he says, be consumed in the formation of the carbonic acid ‡; and we learn from his experi-

* Sur la Germination, p. 119. et seq.

† Manchester Memoirs, vol. iv. p. 319.

‡ Experiments on Sugar, in Rollo on Diabetes.

ments, (6.), that when seeds are made to grow in oxygen gas, the bulk of carbonic acid produced is equal to that of the oxygen gas which disappears. Even granting, therefore, that a portion of this gas is absorbed by the seed, this equality of proportion in the carbonic acid produced forbids us to consider any part of it to be retained; and if it be admitted, that the formation of this acid is in part effected without a previous absorption of oxygen, why should we not allow it to proceed to the fullest extent in which it takes place, rather than have recourse to two such opposite suppositions to account for the same phenomenon?

17. To suppose this oxygen gas to be taken up by the seed by the operation of chemical affinity, necessarily implies its previous separation from the nitrogen gas with which it was united; but how could this be done, unless the seed presented something to the air which had a stronger affinity for its oxygen than the nitrogenous portion has? And what could it offer but moisture and carbon? Moisture, however, does not decompose air; and if carbon be the agent, must not carbonic acid be at once formed? And if this acid be thus formed, exterior to the seed, and out of the oxygen gas in contact with it, how can we hold that gas to be first singly taken in by the seed, and expelled afterwards in the form of carbonic acid? To say that the air is attracted in its undecomposed state, necessarily requires proof of the existence of certain cavities in the seed where it can be retained; for as the nitrogen gas neither suffers (5.) nor produces change, it must be completely expelled after the oxygen is abstracted from

it. Lastly, M. de Saussure has endeavoured to show, that the carbonic acid formed in germination contains in it precisely the quantity of oxygen gas that has disappeared: And although, from the difference of opinion which prevails concerning the actual proportions of the elements which constitute that substance, this cannot be positively assumed; yet the near proportions which, in our own experiments, as well as in those of Saussure and Cruickshank, the two gases bear to each other at the beginning and end of the process, renders it extremely probable. If this opinion be well founded, no part of the oxygen can be retained by the seed; and we may conclude, therefore, with M. de Saussure, that none of it is either attracted or absorbed *.

18. If indeed oxygen gas were in part retained by seeds, we might expect, since they deteriorate so large a portion of it, that they would acquire weight as well as other substances with which the base of that gas combines. But Mr Gough found, that steeped peas germinating in air, and sprouting to the length even of two inches, did not increase in weight †. We found, that if peas be steeped a sufficient time in water, they almost exactly double their weight, which increase of weight rapidly diminishes if they be afterwards exposed freely to the air. Six peas, weighing nineteen grains, were, after forty-eight hours steeping in water, increased in weight to thirty-eight grains, and in this state were

---

* Journal de Physique, loc. cit.

† Manchester Memoirs, vol. iv. p. 316.

exposed on a table in a room at temperature 66°: they gradually decreased in weight, and by the fifth day had fallen back to their original weight and appearance. If, however, the direct effects of evaporation be prevented, by inclosing them in a jar, they still exhale moisture and diminish in weight, but much more slowly; for the peas in the foregoing experiment (8.) were by their germination much diminished in bulk, and lost rather more than seven grains in weight;—from all which it follows, that no supposed absorption of oxygen adds, during their growth, to the weight of seeds.

19. If then there be no proof that the oxygen gas which disappears in germination enters, either by absorption or by chemical affinity, into the seed, so as to combine with it, it follows, that this gas must be at once converted into the carbonic acid produced in that process, without such previous combination; and the formation of the acid proceeding always in proportion (7. 8.) to the disappearance of the oxygen gas, sufficiently shows that it is derived from that source alone. If the acid were formed independent of the oxygen gas employed, the whole bulk of air ought to be increased by germination: and accordingly, Mr Cruickshank found, that when carbonic acid was produced by steeped seeds, confined either in nitrogen (5.) or hydrogen gas, the bulk of air was increased one-fifth, but nothing like germination then took place. He even found that seeds, after being soaked in water, and passed up into a tube of mercury, formed carbonic acid in large quantity, but without undergoing any sensible change in their appearance *. This experiment we repeated by passing up half a dozen soaked peas into a graduated tube filled with mercury, and immersed in a jar of the same fluid. By the following day two-tenths of a cubic inch of air were collected in the upper part of the tube, and the experiment was continued till the bulk of air was increased to 1.6 cubic inch, beyond which it did not proceed. Lime water was then passed into the tube, and with the aid of a little agitation, all the air except a small bubble was rapidly attracted by it. The peas did not exhibit the slightest sign of germination. These facts show, that the production of this acid during the growth of seeds is altogether different, both in its formation and the consequences which attend it, from that production of it which takes place where no living action exists. Where this acid is formed independent of germination, no change is effected in the seed, and the bulk of air is increased: where it is produced by that process, a part of the air disappears; the evolution of the seed goes on; and the quantity of the air is somewhat diminished. Hence, therefore, we conclude with De Saussure, that in germination the seed does not form carbonic acid from its own substance, but furnishes only one of the constituent parts of it, namely, the carbon †; and farther, that when it does form this acid, independent of oxygen gas, it is only under a state of decomposition, (5.),

* Experiments on Sugar, loc. cit.

† Journal de Physique, tom. xlv. p. 98.

or in circumstances where no living action is going on.

20. As therefore the carbonic acid met with in germination is not emitted ready formed by the seed, it must be produced exterior to it; and for this purpose carbon must be in some way furnished. Dr Woodhouse supposes this carbon to proceed either from the earth in which the seeds are planted, from some decayed portion of the living leaves, or from the cotyledons of the seed itself *; but this acid has been shown to be formed where no earth is present, and before any leaves appear, (7. 8.), and where the seed itself exhibits no sign of decay. It is plain, therefore, that the living seed, during its germination, must possess a power of supplying carbon; and by the union of this carbon with the oxygen gas of the air, the carbonic acid that is met with must be formed. We find accordingly, that the carbonic acid produced does actually exceed in weight (13.) the oxygen gas that disappears; and the carbon that occasions this excess can be derived only from the living seed, which, if the experiments of Hassenfratz are to be trusted, contains less carbon after germination than it did before it was submitted to that process †.

21. But it has been said, that when the air used in this process contains no oxygen gas, or when all which it contained has been converted into carbonic acid, germination no longer goes on. This can-

* Nicholson's Journal, July 1802, p. 151.

† Annales de Chimie, tom. xiii. p. 188.

not arise from the presence of nitrogen gas, for that gas exerts (*5.*) no injurious operation. Neither, although Mr Achard found, that germination did not take place in carbonic acid, can the presence of that substance be considered as positively destructive to the process; for the same effect occurs when nitrogen gas only is present, and even *in vacuo* without the direct agency of any gas at all. M. Huber too found, that when oxygen gas or common air, in certain proportions, was mixed with carbonic acid, seeds grew in it very well *: and in all our experiments, where the carbonic acid produced by germination was suffered to remain, the seeds ceased to grow, not from the presence or abundance of that acid, but from the diminished quantity or total absence of the oxygen gas out of which it was formed. Since, indeed, the absence of that gas is sufficient to account for the cessation of the process, it is unphilosophical, because unnecessary, to resort to the agency of any secondary cause; and as both carbonic acid and nitrogen gas must always of necessity be present in germination, it is not to be expected that either the one or the other should exert any injurious operation.

22. Neither, because carbonic acid is always present, is it, on that account, to be held as at all essential to germination; for the same might be urged in favour of nitrogen gas, which neither produces (*5.*) nor suffers change. The constant existence of this acid in the atmosphere is no proof of its aiding ger-

* Sur la Germination, p. 56.

mination. By many its proportion has been rated at $\frac{1}{100}$; but Berthollet considers this estimate as much too high *, and Dalton makes it amount only to $\frac{1}{1000}$ †,—a quantity much too inconsiderable to exert any sensible effect. When also M. de Saussure abstracted the carbonic acid from the air, previous to placing the seeds (7.) in it, germination went on as well as where this had not been done; and, on the other hand, this process continued to proceed when the acid was removed, as soon as formed (8.) by an alkaline solution, as when it was suffered to remain. It has been said, however, that when it is abstracted, by means of lime, from the air in which seeds are growing, the progress of their growth is retarded. The lime, indeed, does abstract the carbonic acid in the same manner as we have seen the alkaline solution to do: and, *à priori*, there seems no reason why its abstraction should in one case be more prejudicial than in the other. But this abstraction of the acid by the lime is carried on only after it has imbibed water; and we found, that when a quantity of powdered lime was placed underneath some peas growing in a jar inverted over mercury, it presently began to swell, the jar continued quite dry, and the peas germinated very imperfectly, being reduced nearly to half their bulk. Thus, they were deprived too rapidly of their moisture to enable them to grow, in the same manner as when they are too freely exposed (18.) to the air. In

---

* Chemical Statics, vol. i. p. 375.

† Manchester Memoirs, vol. i. new series.

fact, since carbonic acid is necessarily a product and consequence of germination, it seems absurd to consider it at the same time as an exciting principle and a cause.

CHAP. II.

OF THE CHANGES INDUCED ON THE AIR BY THE VEGETATION OF PLANTS.

23. In the former chapter it was shewn, that water was a first and essential requisite to the germination of the seed, and numerous facts prove it to be equally necessary to the future progress of the plant; for if completely deprived of water, the plant withers and dies, although every other requisite be present; nor does it afterwards revive, even although water be supplied. The quantity of this necessary fluid that is daily absorbed by plants, varies greatly according to their structure and habits, to the temperature of the season, the supply of water, and the state in which vegetation is going on. After its absorption, it is again given out in great part by transpiration; and the degree in which this latter process proceeds, must depend on the quantity previously absorbed: and conversely, whatever restrains the freedom of transpiration, must abridge

the power of absorption; for when the vessels are once filled with fluid, if none be carried off, no more can enter. The absorption is carried on chiefly by the extremities of the roots, and the transpiration by the surfaces of the leaves. Dr Hales found, that a sunflower, three and a half feet high, lost by perspiration, on an average, 1 lb. 14 oz. of fluid *per* day. During a warm night, and without dew, it lost only 3 oz.: when the dew was sensible, it suffered no perceptible loss; and if the dew was great, or rain fell, it gained a few ounces in weight. The surface of all the leaves of this plant was equal to about 39 square feet, and that of its roots to 15.8 square feet. The depth of water absorbed by the whole surface of the roots was $\frac{1}{67}$ part of an inch, and the depth perspired by the surface above ground $\frac{1}{165}$ part. He found likewise, that different plants varied greatly in the quantity of fluid which they perspired; that evergreens perspired much less than other plants; and that the perspiration was proportional to the extent of surface of the leaves, and ceased nearly if the leaves were removed *. The experiments of M. Guichard shew, that it is chiefly by the upper surfaces of the leaves that this transpiration of fluid is carried on; and those of the same author, of Du Hamel and Bonnet, demonstrate likewise that absorption, as far as the leaves are concerned, is performed by their under surface. To prove this, the superior surface of one leaf and the inferior surface of another were covered with var-

* Statical Essays, vol. i. pp. 4. 6. 21. & 28.

nish, and the former, in a given time, suffered little diminution of weight, but the latter became much lighter. Similar leaves also, were laid on the surface of water, and those which had their superior surface inverted gained little weight, and for the most part died in a few days; while such as had their inferior surface applied to the water, became much heavier, and flourished many months. These facts make it evident, that perspiration and absorption are not performed by the same vessels, but that each has its peculiar organs *. The office of absorption is performed by fine hairs or points, placed on the under surface of the leaves, which in many plants are hollow tubes constructed for that purpose. When leaves have no such points, small apertures are found in their place †. The leaves of plants become gradually less and less fit for carrying on these functions; not so much, however, from any decay or inaptitude in their structure, as from the declining influence of another agent, whose operation we are next to consider.

24. The fluids of plants which are taken up by the roots, and afterwards perspired by the leaves, require, for their due transmission through the stem, a certain degree of heat: hence they flow most freely in the day and in sunshine, and are checked during the night and in frost. Dr Hales observes, that during moisture and warmth, the flow of the sap is most vigorous, but that a cold easterly wind imme-

---

* Dr Bell, Manchester Memoirs, vol. ii. p. 411.

† Wildenow's Principles of Botany, p. 298.

diately abates it. When the sun was clouded during the rising of the sap, it would visibly subside with great rapidity; but when the sun-beams again broke out, it would immediately return to its rising state, just like the fluid in a thermometer *. A plant of spearmint, placed in a syphon one-fourth of an inch in diameter, absorbed, during the day, so much water, that it fell into the opposite end to the depth of an inch and a half; in the night only to one-fourth of an inch; during a rainy day still less; and, when the thermometer stood at the freezing point, the plant absorbed no moisture at all †. That it is to the influence of heat alone, and not to any other peculiarity in the season, or in the plant itself, that, the other requisites being duly supplied, its vegetation is to be ascribed, we learn from the fact, that trees in a hot-house continue to grow during the winter, while others of the same species in the open air cease to do so. Even the branch of a tree, planted in the open air, will, during winter, when led into the hot-house, undergo the usual vegetative changes, provided its stem be guarded from severe frost; but, on the contrary, a branch brought into the open air from a tree that is flourishing in the hot-house, will at the same season cease to grow ‡;—affording sufficient evidence of the necessity and the influence of heat.

---

* Statical Essays, vol. i. p. 122. † Ibid. p. 27.

‡ Philosophical Transactions, vol. lxiii. part 1.

25. With regard to light, we may observe, that whatever doubts may exist concerning its effects on the germination of seeds, there can be no question but that it exerts a peculiar agency on the appearance and qualities of plants. M. du Fay is said to have been the first philosopher who remarked how much the green colour of plants depended on the agency of light; and from an observation made about the year 1760, the late Professor Robison of Edinburgh was led to infer, that it was very actively employed in giving rise, not only to the colour, but to the odour and combustibility of vegetables. Having gone down into a coal-pit in the neighbourhood of Glasgow, he brought up some whitish-looking plants, but no one knew what they were. After being exposed to the light, however, the white leaves died away, and were succeeded by green buds; and the plant acquired the smell of tansy. On further inquiry, he found, that the sods on which the plant grew had been taken down into the pit from the part of an adjoining garden; and although the plant had continued to grow in its new situation, yet neither in colour, in odour, nor combustibility, did it at all resemble plants of the same species which had vegetated under an exposure to light. The etiolation or blanching of the roots of celery, and of the inner parts of cabbages and lettuce, are familiar examples of the same kind; and Mr Davy ascertained, that after the green colour is formed, it will again disappear, if the plant be excluded from light. The leaves of the common lettuce were in six days rendered very pale, by being deprived of light, and at the end of another week they were quite white: the

growth of the plant was checked; and its leaves, on being analyzed, yielded more carbonic acid and water, and less hydrogen and residual carbon, than an equal weight of green leaves,—results similar to those obtained by Chaptal, Hassenfratz, and Sennebier. The various colours of flowers are ascribed also to the combination of light in different proportions. Red rose trees, if carefully excluded from light before their flowers begin to appear, will yield flowers almost white: and marine plants are governed by the same laws with regard to colour as land vegetables *. Scheele observed, that it is the violet ray which acts most powerfully in reducing the oxide of silver †: and Sennebier ascertained, that the same ray has the greatest effect in producing the green colour of plants ‡; but, according to Mr Gough, the presence of oxygen gas is required to aid its operation §. The late Dr Hope found, that the light produced by art effected changes in degree similar to that which issues from the sun, which has since been confirmed by others: and the light of the moon has a similar effect. It follows, therefore, from these facts, that although light is not immediately necessary to the growth of plants, yet that many of their more peculiar and characteristic qualities depend essentially upon its action.

---

* Davy in Beddoes' Contributions to Science, p. 186. et seq.

† On Fire, p. 98.

‡ Mem. Phys. Chim. vol. iv. p. 72.

§ Manchester Memoirs, vol. iv. p. 501.

26. But although water and heat alone be the first requisites to the commencement of vegetation, yet they are not of themselves sufficient to carry on that process. Air likewise is required. Mr Papin put an entire plant into the exhausted receiver of an air-pump, and it soon perished; but on keeping the whole plant, except the leaves, which were exposed to the air, in this *vacuum*, it continued to live a long time *. Dr Hales, Du Hamel, and Mr Knight, found that plants do not vegetate when deprived of their leaves: and it is well known, that when the leaves of a plant or tree are destroyed by insects, its vegetation for that season is suspended, and the fruit gradually decays. We have already seen, that when the upper surface of the leaf was covered with varnish or laid on water (23.) it died in a few days; and Dr Darwin smeared the upper surfaces of the leaves over with oil, and the plant died in a day or two after †. As thus without air plants do not vegetate, and as they cease also to grow when deprived of their leaves, even although air be supplied, we must conclude, not only that air is essential to the growth of plants, but that their leaves are the organs which act upon this air. And farther, as their death takes place if the upper surfaces of their leaves be smeared over with varnish or oil, we must also conclude, that it is to some action going on between those surfaces and the air that the continuance of their life is to be ascribed. Let us next then inquire into the

* Darwin's Phytologia, p. 51. † Ibid. p. 45.

nature of this action, or the changes which the air undergoes in the process of vegetation.

27. Dr Priestley, whose experiments and discoveries first opened the way to an investigation of the reciprocal changes which take place between living vegetables and the air, was led to conclude, that plants not only lived in nitrogen gas, or what he called phlogisticated air, but that, during their growth, they restored to the air the pure part of which it had been previously deprived by the processes of combustion and respiration, and by the decomposition of organized bodies. He found, that if a sprig of mint was put into a jar of air inverted over water, and in which a candle had previously burned out, the air was in a few days so far restored that another candle would burn in it perfectly well. He also put a sprig of mint to grow in air wherein mice had died, and in eight or nine days a mouse would again live very well in this air, though he died in another portion of the same air in which no mint had grown. In air strongly tainted by putrefaction, sprigs of mint have sometimes presently died; but if this do not soon happen, they thrive in a surprising manner. These facts led him to conclude, that plants, by their vegetation, reverse the effects produced on the air by the several processes above described, and thus become the chief means by which the purity of the atmosphere is maintained *.

28. The experiments from which this conclusion was drawn, were made so early as the year 1771,

* Observations on Air abridged, vol. iii. p. 251. et seq.

before Dr Priestley had discovered any correct means of analyzing the air; nor does he seem to have been aware, that the effects produced on the air by the living functions of animals are very different from those which arise from the decomposition of organized bodies. However much, therefore, we may be disposed to admire the beautiful provision which this supposed reciprocity of action would establish between the functions of animal and vegetable life, we must suspend our belief in its truth until better proofs are brought forward than these experiments afford. Even the fluctuation of opinion on this point which Dr Priestley himself experienced, may well suggest doubts of the correctness of his views; for towards the end of the same year, some experiments of the same kind, he observes, did not answer so well, and the restored air relapsed to its former noxious state, which led him to suspend his judgment concerning this power in plants *. In 1772, on resuming his experiments, he had the most indisputable proof of the restoration of putrid air by vegetation; but hearing that Mr Scheele and others had obtained different results, he again repeated them in 1778. The experiments of this year were unfavourable to his former hypothesis; for whether the air had been injured by respiration, the burning of candles, or other means, it was not rendered better by vegetation, but worse; and the longer the plants continued in the air, the worse it became. He tried a great variety of plants, as sprigs of mint, spinach,

* Observations on Air abridged, vol. iii. p. 251. to 263.

lettuce, and some others; but with no better success. His method was to put the roots of the plant into a phial filled with earth and water, and then to introduce it through water into the jar containing the air on which he was making the experiment. In some cases, the air was certainly meliorated, but Mr Scheele, he adds, always found it injured: and he concludes, that, upon the whole, it is *probable* that the vegetation of healthy plants, growing in natural situations, has a salutary effect on the air in which they grow *. Thus we see, from Dr Priestley's own representation of facts, related with his usual candour, that nothing certain can be inferred from his experiments in favour of this supposed purifying power in plants: and that his conclusion was, even at that period, in direct opposition to the experience of the celebrated Scheele.

29. Notwithstanding, however, the uncertain, and, in many respects, contradictory evidence on which this conclusion has been shewn to rest, few opinions in modern science have obtained a more general belief: and both physiologists and chemists seem, in this instance, to have satisfied themselves with contemplating at a distance the beauty of the *final cause*, instead of approaching to a nearer examination of the facts on which the opinion has been maintained. Accordingly, this opinion still keeps its ground, and, in no publication that we have seen, has its accuracy been even questioned. In the experiments, however, which we are about to detail, it will be

* Observations on Air abridged, vol. iii. p. 273. et seq.

seen that their results are altogether opposed to the notion that plants, by their vegetation, purify the air; and these experiments have been so often repeated, and with such uniformity of result, as to impress us with an entire confidence in their truth. We proceed, therefore, to investigate the actual changes which atmospheric air undergoes in vegetation; and, first of all, the effects produced on its nitrogenous portion. It has been already shewn, that seeds (5.) do not in the smallest degree germinate in nitrogen gas: and Dr Ingenhousz proved, also, by experiment, that, in pure nitrogen gas, plants do not grow. This conclusion has been more lately confirmed by the experiments of Mr Gough. He confined several succulent plants in jars of nitrogen gas when their flowers were just ready to expand; but they died away without putting forth any of their blossoms. Others were placed in similar situations before the flower-buds formed upon them, but made not the least effort towards vegetation; and many more, which grew very well in inverted jars of atmospheric air, ceased to vegetate when transferred into jars of nitrogen gas. But this gas, although it does not aid vegetation, appears in no degree to injure the faculty of growth in plants any more than in seeds (5.); for a slip of spearmint, which had remained twelve days in nitrogen gas, recovered upon being restored to the air; and an offset of *sempervivum* vegetated freely after being removed from a jar of the same gas, in which it had been kept from the 2d of April to the 2d of May *. As

* Nicholson's Journal, November 1804, p. 218.

thus it appears, that nitrogen gas exerts no direct influence on plants, so it is probable that they produce no change upon it; and we may conclude, therefore, that this gas, in its simple and uncombined form, is altogether inactive in the process of vegetation.

30. We have next to inquire into the necessity and use of oxygen gas, and the changes which it undergoes in vegetation. It has been shewn by Dr Ingenhousz, that plants do not, any more than seeds, grow in any species of air unless it contain a portion of oxygen gas, and this gas have access to their leaves: that they vegetate very well in atmospheric air and in oxygen gas; and, therefore, that this latter gas is necessary to vegetation *. What then are the changes which it suffers in that process? By the experiments of Dr Woodhouse, we are taught, that plants growing in earth and confined in a glass vessel of atmospheric air render it impure, and that carbonic acid is formed: that when confined in oxygen gas, previously well washed in lime-water, carbonic acid is, in like manner, produced: and that the quicker this acid is generated, the sooner does the plant die †. These facts derive confirmation from the following experiment. Some peas, growing in a small pot of mould to the height of several inches, were placed under a jar of atmospheric air inverted over water; and a common wine-glass, the foot of which was broken off, was filled with lime-water,

---

* Thomson's System of Chemistry, vol. iv. p. 290.

† Nicholson's Journal, July 1802, p. 152.

and inserted into the mould in the midst of the growing peas. The lime-water soon acquired a pellicle on its surface, which, by the fourth day, was very thick, and, by the sixth day, had sunk down to the bottom of the glass; and the water into which the jar was inverted, had risen in it to a considerable height,—proving that carbonic acid was formed, and that a diminution in the bulk of air had taken place.

31. As the mould, in which these plants grew, might by some be supposed to influence the production of the carbonic acid, the experiment was next varied in the manner following. Some mustard seeds, which were raised on moistened flannel, and had grown to more than an inch in height, were introduced into a jar of atmospheric air, and supported about half way up it by a small whalebone hoop, covered with netting. In a deep dish, filled with water, and appointed to receive the inverted jar, was placed a glass of lime-water, which stood directly under the hoop. In four hours, a thickish pellicle overspread the lime-water, and the inside of the jar was bedewed with moisture. At the end of twenty-four hours, the lime-water was very turbid, and the water, into which the jar was inverted, had risen nearly half an inch. These appearances increased during the two succeeding days, at which time the water stood nearly an inch high in the jar. The plants now began to look sickly, gradually drooped, and, by the ninth day, fell down in different directions against the sides of the jar, and, when withdrawn from it, emitted a putrid smell. The residual air extinguished a taper, rendered lime-water

turbid, and suffered thereby a farther loss of bulk. This experiment was repeated, by confining mustard plants in the same manner in jars of air inverted over mercury. The lime-water was, in like manner, rendered turbid, and the mercury for two or three days continued to rise into the jars: after a certain time, however, it became stationary, and then began to fall, and the plants when withdrawn yielded a putrid smell.

32. As by the preceding experiments it is proved, that the pure part of the air disappears in vegetation, and that carbonic acid is produced, it is desirable to know the extent to which this destruction of the oxygen gas proceeds. Some mustard seeds, therefore, which had grown on moistened flannel to the height of an inch, were conveyed into a jar, and supported about half way up it by a small hoop in the manner already described. The jar held 36.5 cubic inches of air, and was inverted into a saucer, in which was placed a small glass-cup, containing one cubic inch of the water of potassa: mercury was then poured into the saucer, so as to surround the mouth of the jar. Three slips of paper, graduated to inches and tenths, were pasted at equal distances on the outside of the jar, to mark the ascent of the mercury: and the whole was set aside in a room where the barometer was at 29.2 inches, and the thermometer at 61.5°. The inside of the jar was presently bedewed with moisture, and, by the close of the twenty-fourth hour, the mercury had risen in it $\frac{6}{10}$ of an inch, bearing up the alkaline solution with it. By the forty-eighth hour, the mercury stood at one inch, when that in the saucer was brought to a level with

it; and at the end of the third day, it had undergone no variation. At this period, the barometer was 29.75, and the thermometer 68°.

33. In order to examine the air, the saucer and jar were now immersed in the pneumatic trough, and water being admitted into the jar, the mercury fell, and the cup with its solution was withdrawn through the water: the solution effervesced briskly on the addition of diluted sulphuric acid. A portion of the residual air being then transferred into a small jar, was found to extinguish a lighted taper that was plunged in it: and another portion was shaken with lime-water without producing or suffering any change; neither did it afterwards experience any perceptible loss by agitation with the liquid sulphuret of potassa, nor by being placed in contact with a stick of phosphorus. The result, therefore, of this analysis teaches us, that, during the growth of the plants, the oxygenous portion of the air gradually and completely disappeared; and that the carbonic acid that was formed was entirely attracted by the water of potassa employed.

34. In the process of germination it has been shewn, that the bulk of carbonic acid produced, bears a constant proportion (7, 8.) to that of the oxygen gas which disappears: and to ascertain whether the same did not hold in vegetation, a similar method was adopted. After the foregoing analysis of the air was completed, the jar was allowed to run dry, and being then re-inverted, it was set on its bottom: and the plants, together with the glass-cup, and a bulk of water equal to the solution, were re-placed in it. Water, to the quantity of 25.4 cubic inches,

was then poured in till it reached the height to which the mercury had risen, which, therefore, was the bulk of residual air; and afterwards to fill the jar completely, it required 7.6 cubic inches more, which was the bulk of air that had disappeared. These quantities together amount only to 33.0, and the 3.5 required to make up the 36.5 cubic inches of air which the jar was capable of containing, must, therefore, be allowed as the space occupied by the several substances used in the experiment, which reduces the volume of air actually employed to 33.0 cubic inches, of which a portion equal to 7.6 had disappeared. The barometer, at the end of the experiment, was, as already stated, 29.75, and the thermometer 68°. Hence, therefore, to make the necessary reductions, we have $\frac{25.4 \times 29.75}{29.2} = 25.8784$, but $\frac{6.5 \times 25.8784}{483} = .34826$ and $25.8784 - .34826 = 25.53014$, the corrected volume of air at the close of the experiment. But atmospheric air contains $\frac{22}{100} = \frac{1}{4.54}$ of oxygen gas, and $33 - 25.53014 = 7.46986$ and $\frac{7.46986}{33} = \frac{1}{4.417}$. Or if we compare the diminution which the air suffered, with the quantity of oxygen gas which it originally contained, we have 100 : 22 : : 33 : 7.26, which comes near to 7.46, the estimated amount of the diminution that actually took place. In a second experiment, conducted in a similar manner, the loss of bulk which the air suffered, after the necessary reductions were made, was $\frac{1}{4.1}$. From these facts it is inferred, that the loss of bulk, which a given volume of air suffers in vegetation, corresponds very nearly with the proportion of oxygen gas which that air contained: and if this loss be attributed entirely to the abstraction of carbonic acid, the quantity of acid produced will rather exceed that of oxygen gas lost. But on passing into carbonic acid, this gas suffers (11.) a small degree of condensation; and whatever future experience may determine this to be, it must of course be deducted from the whole diminution which the air suffers, and consequently will diminish somewhat the quantity of carbonic acid formed.

35. Having thus endeavoured to trace the nature and extent of the changes which the air suffers in vegetation, let us next direct our attention to the manner in which they take place. Through the whole of the preceding experiments (30. et seq.) we have seen that oxygen gas is essential to vegetation, and that it gradually and completely disappears, when, in a given bulk of air, that process is made to take place. What then becomes of it? By many it is supposed to be *absorbed* by a necessary function of the plant. It may be worth while, therefore, to inquire what is that structure of the plant by which such an absorption can be effected? Grew and Malpighi speak of *spiral vessels* which they discovered in the wood, and which later inquirers have found to extend to the minutest branches, and to spread through every leaf. From these vessels being always found empty, they supposed them to be air-vessels, and called them *tracheæ*; but Dr Darwin observes, that if the end of a vine-stalk, two or three years old, be cut horizontally, these vessels may, by the help of a common lens, be seen to be full of juice, which, in a minute

or less, passes on or exhales, and then the vessels appear empty, that is, filled with air. The same author, however, speaks of *horizontal vessels*, first noticed by Malpighi, which pass through the bark of trees to the alburnum, and, as he supposes, admit oxygen gas * ; but, besides that these vessels are not proved to extend to the leaves, which alone operate changes on the air, we have seen that the leaves of themselves are able to preserve the life of the plant when the stem is confined *in vacuo* (26.); and, on the contrary, that the stem is not sufficient, when deprived of its leaves, or these are smeared over with oil. Lastly, Dr Hales readily caused air and water to pass through a piece of recent stick, by setting one end of it in a cup of water placed under a receiver, and then exhausting the receiver of its air; and these vessels he imagined to perform a respiratory function ; but the same vessels were found by Malpighi to be present in the roots, where they are not exposed to the air, and cannot, therefore, serve the purpose of respiration. Neither, as before remarked, have we any proof that any other part than the leaves of the living plant act upon the air in contact with it to an extent necessary to maintain its life.

36. Have then these leaves any organization by which an absorption of air can be effected ? It is ascertained that the sap of plants ascends to the leaves, and after undergoing certain changes in them, is again returned to the branches and stem. Dr Dar-

* Phytologia, p. 12, 13.

win placed a plant of spurge (*Euphorbia Helioscopia*) in a decoction of madder, and in a few days the coloured fluid passed along the middle rib of the leaf, and ran by several branches to the extremity of it upon the upper surface; while, on the under side, a system of vessels was seen coming from the end of the leaf, and conveying a milky fluid, which descended finally into the foot stalk*: and Mr Knight has traced the vessels conveying this peculiar juice from the leaves into the cortical layers of the inner bark †. Now the difference in the appearance and qualities of this returning fluid was probably effected in the leaves by the agency of the air, and in this way the *succus proprius* of plants seems to be prepared. But can we suppose, either the air or its oxygenous portion to be received into these minute vessels, already filled with fluids, to be there decomposed, then transformed into, and at length emitted as carbonic acid? If so, by what passages can it enter, and how afterwards can it be expelled? It cannot enter by the transpiring or exhaling pores, because the fluids constantly escaping from them must obstruct its admission; nor have we any instance of any organ performing two contrary actions at one and the same time,—a condition which this supposition would involve. If, again, it enter by peculiar vessels fitted for the absorption of air, (whose existence however is not proved), by what vessels can it afterwards be emitted as carbonic acid? Not surely

* Phytologia, p. 43.

† Philosophical Transactions, 1801, p. 337.

by the same vessels which absorbed it, for the reason above given: and, therefore, the existence of other peculiar vessels for the emission of air, ought likewise to be proved. By the common absorbents of the leaf, it cannot be supposed to enter; for they are employed in taking up fluids, and are besides situated on the inferior surface (23.), while the changes on the air are effected by the superior only. To suppose that it gains admission through inorganic pores, is contrary to all sound physiology; for we have no example of any healthy function being carried on by animated beings, otherwise than by a living action, and by a peculiar organization destined to perform that action.

37. Rejecting, therefore, the opinion that any aëriform fluid obtains admission into the vascular system of plants by any process resembling the living function of *absorption*, let us next inquire how far the supposition of *chemical affinity* will help to remove the difficulty. The juice of the leaves, which is supposed so powerfully to attract the oxygen gas of the air, is contained within its peculiar vessels, and these vessels are likewise covered by the green colouring matter, over all which is spread the *epidermis*. The minute structure of these parts has been lately described by M. Jurine, as it appeared on microscopic examination *; but the appearances observed by him are so different from those represented by other authors, as to justify, in his opinion, the adoption of new names, in almost every instance, to

* Phil. Mag. vol. xvi.

express them,—a circumstance which, while it begets a doubt of their accuracy, tends greatly to puzzle the student, without proportionally advancing the science. It is sufficient for our purpose to observe, that no direct communication is demonstrated to exist between any pores on the surface of the epidermis, and the vessels in which the vegetable fluids move. If, therefore, chemical affinity operate here, it must be between substances, not, as usual, in actual contact, but at a certain assignable distance; and that distance intercepted by a very compounded organized structure. Let us, however, suppose for a moment the oxygen gas of the air to obtain admission into the juices of the plant, under all these circumstances, by the operation of chemical affinity;—by what power will the carbonic acid, which it is there supposed to form, be afterwards emitted? Not by chemical affinity, surely, for where is the external agent to effect it? Not through a living action, for where is the structure to perform it?—Or how can the reception of air be thus carried on by one law, and its expulsion by another so widely different? How, also, is this oxygen gas, previous to its supposed attraction into the plant, to be separated from the nitrogen gas with which it is naturally united? If, as is probable, the separation be brought about only by the superior affinity of carbon furnished by the leaf, then the carbonic acid, instead of being emitted from the leaf, is formed exterior to it, and out of the very oxygen gas which is supposed first to enter into the leaf to form it. Those, also, who favour this opinion of the attraction of oxygen gas, cannot well suppose that plants likewise attract carbonic acid, retaining the carbon, and giving out the oxygen gas; for this would not only be making an attraction and expulsion of oxygen gas to go on at the same time, but would be doing the same by the carbonic acid also: and how could that acid be ever formed and emitted at all, if the oxygen gas which enters into it is first singly expelled, and the carbon, which is its other constituent, is permanently retained? And if we suppose, that, during the day, carbonic acid is received and oxygen gas expelled by the leaves, and that during the night the exact contrary takes place *; it seems very difficult to account for the death of plants (30.) confined in a given bulk of air at all, since they would, in fact, be weaving Penelope's web, undoing by night what they had done by day, and *vice versâ*. All these difficulties are greatly increased by the supposition, that the air enters into the vessels of plants in its *undecomposed* state; for since the nitrogen gas remains unaffected (29.), what useful purpose could its reception answer? Or by what means, when separated from its oxygen gas, could it again be expelled from the plant in the exact bulk which it before occupied? On these grounds, we deny the entrance of aëriform fluids into the vascular system of plants, by any power analogous to chemical affinity.

38. If from the experiments already related (32.), it be granted, that, during vegetation, the oxygen gas of the air gradually and completely disappears, and that a bulk of carbonic acid, nearly

* Thomson's System of Chemistry, vol. iv. p. 291.

equal thereto (33.) is formed; and if it be admitted, that none of this gas is received into the vessels of the plant (35, 6, 7.), we must embrace the belief, not only that the acid is formed out of the oxygen gas originally present in the air, but that it is not emitted ready formed by the plant. If the acid were formed independent of this oxygen gas, the whole bulk of air ought to be increased, instead of being diminished, by vegetation: if it be not so formed, then as no oxygen gas is received into the plant, no carbonic acid can be emitted from it. Plants also, in nitrogen gas, do not vegetate, and cannot, therefore, by a living process, form carbonic acid; and when, in atmospheric air or in oxygen gas, this acid is formed by vegetation, that gas disappears, and the plant for a time lives: whence it follows, that without the presence of oxygen gas, the living plant is unable to form carbonic acid, and that this acid, therefore, is formed by the reciprocal changes going on between this oxygen gas and the plant. If we do not, indeed, admit the conversion of oxygen gas into carbonic acid, there is no way in which its loss can be accounted for, since there is no other new product formed, and none of the gas, as we maintain, enters into the vascular system, and combines with the fluids of the plant. In all the foregoing experiments also, the deterioration of the air increased in proportion to the continuance of the vegetative process, until the whole of its oxygen gas completely disappeared; and the gradual rising of the mercury into the jar (32.), indicated a corresponding production of carbonic acid, until the bulk of the latter nearly equalled that of the former;

which reciprocal action can in no way be accounted for, but by that conversion of the one gas into the other, which is here supposed actually to have place.

39. To effect this conversion, however, of the oxygen gas, carbon must be from some source supplied. This Dr Woodhouse supposes, as in the case of seeds, to proceed either from the organized remains of the soil in which the plants grow, or from the carbon of the decayed leaves, when confined a considerable time in a given bulk of air *: but it has been shewn, that carbonic acid is equally produced (31.), where there is no soil present to afford carbon; and that it appears also in a few hours (31.), and before the leaves, therefore, exhibit any sign of decay. Indeed, there does not appear any good reason why oxygen gas should be so essential to the life of vegetables, if it could be acted on or changed by those parts of them only which are already dead. To the living plant, therefore, as well as to the seed (20.), we must look as affording carbon, and by the union of this carbon with the oxygen gas of the air, must we consider the carbonic acid met with in vegetation to be formed.

40. But as this carbonic acid is produced in vegetation, the oxygen gas proportionally disappears, (30. et seq.), and when this substitution is complete, the plant gradually declines and dies. To what cause is this effect to be immediately ascribed? It must arise from the superabundance either of nitrogen gas, or of carbonic acid, or from the defi-

---

* Nicholson's Journal, July 1802, p. 152.

ficiency of oxygen gas. Now, it has been shewn (29.), that although plants do not grow in nitrogen gas, yet that gas suspends only, but does not destroy the vegetating faculty: and precisely the same occurs when they are placed *in vacuo*, where no direct effect can be attributed to any air at all. In all the preceding experiments also, vegetation did not cease till all the oxygen gas had disappeared; and, therefore, the superabundance of nitrogen gas did not prove fatal so long as any oxygen gas remained. Mr Gough, indeed, has shewn (29.), that certain plants retain the faculty of growth after being confined for weeks in nitrogen gas. To the want or absence, therefore, of oxygen gas, and not to the presence of nitrogen gas in any proportion, is the death of plants in a given bulk of air, as well as that of seeds (21.), to be entirely attributed. How far the proportions of nitrogen and oxygen gas, as they exist in atmospheric air, may be the best adapted to the actual structure and constitutional habits of plants, we have not experimentally attempted to determine: but the general analogy to be derived from the case of seeds (14, 15.), and the fitness of means to the perfection of an end observed throughout all the operations of nature, lead irresistibly to the belief, that these proportions are in reality the best.

41. The only other gas present in the preceding experiments, by which the growth of plants can be affected, is carbonic acid: and the experiments of Priestley*, Ingenhousz, and others, clearly evince,

* On Air, vol. i. p. 36. and iii. p. 310.

that when confined in this acid, plants will die. This, however, is no more than what we have seen to take place in nitrogen gas (29.), and *in vacuo*, and cannot therefore be received as proof of the positive operation of carbonic acid. From the experiments of the late Dr Percival, and Mr Henry, we learn also, that although plants, when wholly confined in carbonic acid, certainly died, yet that where a small portion of oxygen gas was admitted, they as certainly lived and flourished *. Neither do plants, confined in a given bulk of atmospheric air, die till all the oxygen gas disappears (30.); nor, consequently, does the quantity of carbonic acid formed prove fatal so long as any oxygen gas remains. So far, indeed, is carbonic acid from being fatal to vegetation, that many have deemed it highly *salutary* †, and some even contend that it is *essential* to that process,—alleging that it is absorbed by the leaves of plants during the day, decomposed within their vessels, its oxygen being afterwards emitted, while its carbon is retained as food ‡.

42. Against this opinion of the absorption and emission of gases by the leaves of plants, when growing naturally in air, we have already, both on physiological and on chemical grounds, been induced to enter our protest: but the importance of the question, together with the high authorities by which it has been supported, will, we trust, plead in excuse

* Manchester Memoirs, vol. ii. p. 341. et seq.

† Ibid. vol. ii. p. 331.

‡ Thomson's Chemistry, vol. iv. p. 283. et seq.

for the additional remarks which the present occasion enables us to offer. Passing over, then, for the present, the anatomical and chemical difficulties which beset this opinion, let us, for a moment, admit the capability of the leaves to absorb or attract carbonic acid; and then examine how far such a supposition is consistent with reason and with fact. That the same substance, carbonic acid, should, during the day, be absorbed by the leaf, and decomposed within it as salutary; and, during the night, should be formed within the same leaf, and emitted from it as noxious *, seems to be not only inconsistent, but absurd. Where would be the advantage in the carbon of the acid being retained for twelve hours as food, if, for the next twelve, it must again be given out as excrementitious? Or where is there an instance, in the whole circle of existence, of a living agent not only first forming its own food, but feeding on its own excretions? If this carbon were, during the day, retained as food, whence comes *that* composing the acid which plants, when confined in a given bulk of air (31.), are constantly forming? If oxygen gas, as these chemists suppose, be during the day constantly emitted, why does that gas gradually disappear as the process of vegetation proceeds (32, 3.)? And why at last is none to be met with, although there is present an abundance of carbonic acid, out of which it is supposed to be formed? It has been proved, that during the day carbonic acid, by the act of vegetation (32, 3.), is constantly forming;

* Thomson's Chemistry, vol. iv. p. 283.

but if, at the same time, it be as constantly absorbed by the leaves, how can its presence be manifested in such quantity, and in such progression, as experiment evinces that it is? If plants do die in a given bulk of air, and that, too, more or less rapidly as carbonic acid is produced (30.), with what propriety can we hold that substance to be essential to their life? If they do live and flourish in air deprived of carbonic acid (30.), and this acid be a consequence of their growth, on what principle can we at the same time assign it as a cause; or how can we consider that substance to be essential to the existence of this process, when it did not itself exist until produced by the very process to which its existence was essential.

43. But it is said, that M. Saussure, by putting a quantity of lime into the glass-vessel in which plants were vegetating, found that they no longer continued to grow, and that the leaves in a few days fell off *. Admitting the correctness of this experiment, is it any proof that the process ceased from the want of carbonic acid? Would not the lime equally abstract the moisture of the plant, and would not this be more likely to occasion the fall of the leaf? The leaf, indeed, might die from the absence of any elastic fluid essential to its life; but, if it did, it would still adhere to the stem. The following experiment is offered in support of this opinion. Two equally sized pots of earth, containing four growing peas each, were placed under two equal jars of atmosphe-

* Thomson's Chemistry, vol. iv. p. 283.

ric air, one of which was inverted over mercury, and the other over water. Each of the pots was surrounded by a quantity of powdered lime, spread on the board by which it was supported. The leaves of the plants in the mercurial jar continued green for several days, and became at length very much curled, but the plants themselves had not increased in height; and when withdrawn from the jar on the sixth day, their leaves were dry to the feel, crumbled between the fingers, and were still very green. The jar, during the greater part of the time, remained dry and transparent; the lime was much increased in bulk; and the mercury had risen considerably into the jar. In the other jar, the water, by the second day, had risen so as to moisten the lime, and its whole inside was quite bedewed with moisture, which appearance it continued to exhibit through the whole period of the experiment. The leaves of the plants did not continue dry or green as in the former case, but assumed a yellowish appearance, and, by the fifth day, the plant had increased an inch and a half in length, the leaves becoming still paler and more yellow; and when withdrawn from the jar, on the sixth day, they were quite moist to the feel. From these experiments, it appears, that lime, placed in jars with growing plants, abstracts their moisture, and thereby checks their growth; but that if the lime be previously moistened, the growth of the plants is not checked, and their moisture is as great as usual. But water is as necessary to vegetation (23.) as air; and, therefore, granting unto lime the power of attracting carbonic acid as well as water, the death of the plants in M, Saussure's experiments, may with as much justice be attributed to this abstraction of water, as to that of the carbonic acid.

44. That this event did not arise from the abstraction of the carbonic acid, is farther proved by the experiments which follow. Two glass-jars, each containing about 40 cubic inches of atmospheric air, were inverted over water, and into each of them was introduced some mustard plants, growing on flannel, which were supported, as before, by a small hoop fixed half way up the jar. Under the hoop in one jar was placed a small cup, containing about an ounce of water of potassa : the other jar contained the growing plants only. In 24 hours, the water in the jar with the alkaline solution had risen 7-10ths of an inch, and in 48 hours 1½ inch ; but in the other jar, in the same time, it had risen only 6-10ths of an inch. The plants and alkaline solution were now withdrawn under water from the first jar, and five cubic inches of the residual air being then passed into the eudiometer filled with lime-water, produced not the smallest discoloration, nor suffered the least diminution in bulk ; but the same quantity of the air of the other jar, being afterwards treated in like manner, rapidly made the lime-water milky, and suffered a loss of nearly 1-5th of its bulk. The air of neither jar, after washing with lime-water, experienced any loss of bulk by agitation with the sulphuretted solution of potassa, and in all respects the plants in both jars presented the same appearance, a few only reviving on exposure to fresh air. In both these jars, therefore, carbonic acid was formed by the act of vegetation, but in one case it

was abstracted as soon as formed by the alkaline solution, while in the other it remained in contact with the plants; but neither the presence nor the absence of this acid caused any difference in the growth or appearance of the plants; and their decline is referable only to that complete abstraction of oxygen gas, which has been before shewn to be fatal to the vegetative process: hence then carbonic acid, applied to the leaves of plants, is not essential to vegetation, neither is it destructive to the continuance of that process.

45. All that has been hitherto said, applies to the circumstances of plants growing naturally in air: when they are placed in water, other phenomena arise, from which have been drawn arguments in favour of an absorption and emission of gases by leaves. Dr Priestley first advanced the opinion, that plants, in certain circumstances, emitted oxygen gas; and Ingenhousz soon after discovered, that the leaves of plants, when immersed in water, and exposed to the light of day, produced an air which he announced as oxygen gas. But whatever may be the results of these experiments on plants immersed in water, they are not necessarily to be received as proof of the same actions being performed by the leaves when growing naturally in the air: on the contrary, it has been shewn by direct experiment (33.), that when plants are confined in a given bulk of atmospheric air, they gradually and completely destroy its oxygenous portion, which could not possibly happen if they possessed the power of emitting oxygen gas. Moreover, the experiments of Dr Ingenhousz himself teach us, that this supposed emission of oxygen

gas does not depend so much on the power of the leaves, as on the quality of the water in which they are immersed; for if the water be previously boiled, little or no oxygen gas is collected. River water affords very little gas, but pump water is the most productive of all: and Sennebier also proved, that if the air be previously deprived of all its air by boiling, the leaves of plants immersed in it do not emit a particle of air *. But not only must the water previously contain air, but it must contain carbonic acid; for Sennebier has shewn, that no oxygen gas is yielded by leaves when plunged in water destitute of carbonic acid: that the quantity of oxygen afforded is proportional to the quantity of carbonic acid which the water contains; and that when the water loses the power of affording oxygen gas, all the carbonic acid which it contained has disappeared. These experiments prove, that the oxygen gas, which is separated when the leaves of plants are immersed in water, depends altogether upon the presence of carbonic acid †.

46. But surely it cannot be maintained by any one, who for a moment considers the structure and livingfunctions of vegetables, that the leaves of plants, when, as in these experiments, they are separated from the branches, and wholly immersed in water, absorb and decompose this carbonic acid as a natural and healthy function, and afterwards emit its oxygen and retain its carbon. Not only, in these circumstances, must the circulation of their fluids be

* Thomson's Chemistry, vol. iv. p. 284. † Ibid.

completely suspended, but the plants must be entirely cut off from the contact of atmospheric air, which has been shewn to be essential to their life; and if the separated leaf speedily dies when its upper surface only (23.) is laid upon water, what reason have we to conclude that its life can be preserved by immersing it completely in that fluid. Even granting that the production of oxygen gas, in these experiments, was effected by a natural function of the leaves, no sensible advantage, depending on this power, can be supposed to arise to those plants which flourish in the open air; for the carbonic acid, from the decomposition of which alone the oxygen gas is derived, exists in the atmosphere in a quantity infinitely too small to be productive of any good effect: and we have shewn, that instead of absorbing carbonic acid from the atmosphere, plants, by their vegetation, are constantly producing it; and instead of emitting oxygen gas, they are at all times converting it into this very acid. Neither, after all, is it clear that the oxygen gas, which is thus obtained by the decomposition of carbonic acid, depends on any peculiar operation of the leaves; "for light is an essential agent in the decomposition, and it is probably by its agency, or by its entering into combination with the oxygen, that this substance is enabled to assume the gaseous form, and to separate from the carbon *."

47. That the organized structure of the leaves is not at all necessary to effect the separation of air from

* Thomson's Chemistry, vol. iv. p. 285.

the water in which they are immersed, may be inferred also from the experiments of Count Rumford, who found that dried leaves, fibres of raw silk, and even of glass, when placed in similar circumstances, produced a like separation of air. This air varied in quality, being in many cases less pure than that of the atmosphere *; and Dr Woodhouse found river water to yield chiefly nitrogen gas †. This variation arises no doubt from the nature of the air which the water contains; for Mr Dalton remarks, that although atmospheric air, expelled from pure water, contains 38 *per cent.* of oxygen, yet that, by stagnation, it loses a part or all of it, notwithstanding its constant exposition to the atmosphere: and this must arise from some impurities in the water which combine with the oxygen, since pure rain water lost none of its oxygen after standing in a bottle more than a year ‡. Hence, then, we see, that to effect the separation of air from water, the organized structure of the leaf is not only not necessary, but that the quality of the separated air is altogether different from what this supposed function of the leaves ought to supply. No proof, therefore, of the absorption and emission of gases, much less of oxygen gas, by the natural functions of leaves, can be derived from these experiments on plants immersed in water: and were the experiments even more precise, they would not in the least apply to the case of vegetables which flourish in the open air.

---

* Philosophical Transactions, 1787.

† Nicholson's Journal, July 1802.

‡ Manchester Memoirs, vol. i. new series.

CHAP. III.

OF THE CHANGES INDUCED ON THE AIR BY THE RESPIRATION OF INSECTS, WORMS, FISHES, AND AMPHIBIOUS ANIMALS.

48. To the commencement of living action in vegetables, the presence of water (1. 23.) has been shewn to be essentially necessary, and among several inferior animal beings, its operation is equally striking and apparent. The ova of innumerable tribes of animals, some of which afterwards inhabit the air, are deposited in water, and undergo their various stages of evolution only while exposed to the influence of that necessary fluid. Neither is its agency confined to the earliest states of existence, nor to those animals which may be properly called aquatic; for examples abound, where its operation extends through every period of life, and among animals which reside wholly in the air. Snails in their shells have been thrown into a drawer, and lain by for fifteen years; but reco-

vered action on being immersed in a bason of water *. The gordius, or horse-hair eel, while in water, is in incessant motion, but, if the water dry up; its movements cease, it shrivels up, and may be kept in this state for an indefinite length of time; but place it again in water, it begins to move, and, in a few minutes, is as brisk and lively as ever. There is an animalcule that sometimes resides in wheat, which, after lying dormant for nearly thirty years, has recovered its vital functions, merely by moistening the grain with water. The rotifer (*vorticella rotatoria*), which lives in small puddles of water, often on the tops of houses, shrivels up as the water evaporates, till it becomes like a piece of dried parchment, in which state it may be preserved for years without suffering the smallest change: but on moistening it with water, it resumes its pristine form, and soon becomes as lively as ever. Suffer the water again to evaporate, and the animal dries up as before: but restore to it the moisture, and again it is brought to life. In this way it has been alternately deadened and revived eleven times, without any apparent exhaustion of its vital powers: and although subjected, during its torpid state, to a heat of 56° Reaumur, and a cold of 19°, it was equally susceptible of revivification as at first †. These facts sufficiently demonstrate the necessity of water to the commencement of animal action.

---

* Philosophical Transactions, 1774.

† Anderson's Recreations in Agriculture, No. x. p. 255. et seq.

49. Equally essential to this action is the presence and operation of heat. The ova of myriads of insects are evolved by its immediate influence; and its power is not less necessary to the maintenance, than to the beginning of living action. When abstracted to a certain degree, numerous tribes of animals pass into a torpid state, and again recover action as the heat is restored. Caterpillars, spiders, and ants, were many times in succession rendered torpid, and again restored to action, in the experiments of Dr Michelotti, by the alternate abstraction and communication of heat *. In a temperature,—1° Reaum. Spallanzani found living action in snails to cease; and the same result was obtained by exposing the marmot to a similar abstraction of heat †. The heart moves quicker in hot than in cold animals, says Dr Irvine; and in many animals, during the severe cold of winter, it does not move at all. The heart of snails, which beats manifestly in summer, was found to be perfectly at rest in winter; and the same thing is observed in many of the fly tribe. Of the same description too, are the serpent and viper tribe, frogs, toads, and tortoises: even the bat, which is naturally a hot animal, becomes, during the winter, as cold as the surrounding medium, and its heart is perfectly at rest ‡. If an egg be opened some days after incubation, so that the *punctum saliens* may come into view, according as it is exposed to heat or cold,

* Phil. Mag. June 1804.

† Memoirs on Respiration, p. 154. 334.

‡ Irvine's Chemical Essays, p. 201.

says Dr Mayhow, you will perceive the *corculum* or heart to pulsate, or to languish and cease from motion *. The hearts of frogs, reduced to torpidity, were removed by Spallanzani, and living action could still be excited in them, by the re-application of a proper degree of heat: and the heart of a turtle, on being put into milk-warm water, was repeatedly observed by Dr Gardiner to yield a tremulous motion, six or seven hours after it was removed from the body, and had become much shrivelled and dried. If suffered to become cold, it was insensible to every stimulus; but when again warmed in water, it repeated its palpitations on being pricked with a needle †. These and many other facts which might be adduced, sufficiently establish the necessary concurrence of heat to the production of action in animals: and prove likewise, that its abstraction, although causing a suspension of the animal functions, does not necessarily destroy the capacity of their renewal.

50. The direct effects of light in producing the various colours of vegetables (25.) have been distinctly proved: and the experiments and observations of Mr Davy, go to shew that this agent exerts a similar operation on the various classes of animal beings. He has observed, that the zoophyta exposed to light, are uniformly brighter coloured than those which have by any means been secluded from it; and he succeeded in altering the colour of two sea anemones

* Tractat. Quinq. p. 325. An. 1674.

† On the Animal Œconomy, p. 46.

from a dark red, to a pale pink, by secluding them from light. The parts of fish, which are exposed to light, exhibit various colours; but the belly, which is deprived of light, is uniformly found white in all of them. The birds that inhabit the tropical countries, are much brighter coloured than those of the north: and those parts of the feathers, which are exposed to light, are almost always coloured, while the parts secluded from light are generally pale or white. The same observations apply to the hairs of quadrupeds: and, not only are the beasts of the equatorial uniformly brighter coloured than those of the polar countries, but the northern animals are dark coloured in summer, and white, or pale, in winter *. These observations point out the influence of light, in giving rise to the colour of animals; but they do not, any more than in vegetables, shew the immediate necessity of it to the existence of animal action.

51. But when, by the combined agency of water and heat, the animal structure is brought into a condition fitted for exhibiting living action, the presence of atmospheric air is required to enable it to continue this action; for, as far as observation has extended, no living being can long subsist without a due supply of fresh air. "In insects," says Mr Ray, "there are many orifices on each side of their bodies for the admission of air, which, if you stop with oil or honey, the insect presently dies, and revives no more. This, he adds, was an observation

* Contributions to Science by Beddoes, p. 192.

of the ancients, though the reason of it they did not understand, (*oleo illito, insecta omnia exanimantur*, Plin.), which was nothing but the intercluding of the air; for, though you put oil upon them, if you put it not upon or obstruct those orifices therewith, whereby they draw the air, they suffer nothing *." Mayhow observes, that if the oil be applied only on some of these orifices, the neighbouring parts immediately become paralytic, by being deprived of the nitro-aërial particles of the air, while the other parts, in the meanwhile, continue sound †. Mr Derham found, that wasps, bees, hornets, and grasshoppers, seemed dead in two minutes, when placed under the exhausted receiver; but revived in two or three hours on being restored to the air, even though they had remained *in vacuo* twenty-four hours ‡. Of the *vermes* class, snails survive several hours in the exhausted receiver: efts, or slow worms, two or three days; and leeches, five or six ||.

52. The same necessity of fresh air in the water in which they live, is required by aquatic animals. Zoophytes, according to Mr Davy, require the presence of air in the water in which they grow, and they act upon it like fishes §. The animalcules in pepper water remained *in vacuo* twenty-four hours: and, being afterwards exposed a day or two to the

* Wisdom of God in the Creation, p. 82.

† Tractat. Quartus, cap. iv. p. 39.

‡ Physico-Theology, p. 8. 7th Edition.

|| Hutton's Mathematical Dictionary, article Air Pump.

§ Beddoes's Contributions to Science, p. 138.

open air, Mr Derham found some of them dead and some alive *. Mr Ray remarked, that fishes cannot live in water without air: they will live in a vessel of water with a narrow mouth for months or years; but if the vessel be stopped, so as wholly to exclude the air, or interrupt its communication with the water, they will be suddenly suffocated †. Dr Priestley confined several small fishes in a vessel, containing three pints of rain water, that had been previously well boiled to deprive it of its air, and they lived only between three and four hours ‡. Mr Davy introduced a large thornback into a jar containing three cubic inches of water, which had been deprived of its air by distillation through mercury: he was very quiet for four minutes and a half, but then began to move about, and, in seven minutes, had fallen on his back, but still continued to move his gills. In eleven minutes, he was motionless, and when taken out, after thirteen minutes, he did not recover ‖. Amphibious animals, likewise, cannot live without air, but its deprivation is not immediately fatal to them. Frogs and toads bear the pump for two or three hours, and a frog recovered on exposure to the air, after remaining *in vacuo* seemingly dead for eleven hours §. Hence we see, that, to all these animals, whether inhabiting the air

---

* Physico-Theology, p. 8.

† Wisdom of God in the Creation, p. 81.

‡ Observations on Air, vol. v. p. 139.

‖ Researches concerning Nitrous Oxide, p. 367.

§ Physico-Theology, loc. cit.

or the water, a constant renewal of fresh air is required, while the actions of life continue. What then are the changes induced on atmospheric air by these several classes of animal beings, whereby it is rendered so essential to the maintenance of vital action?

53. For the first, and most accurate knowledge we possess concerning the changes which the air suffers by the respiration of insects, we are indebted to the labours of the celebrated M. Vauquelin. The experiments of this excellent chemist were made on the grasshopper (*gryllus viridissimus*), which is described as having twenty-four stigmata, or breathing pores, ranged parallel with, but exterior to, two white lines, extending longitudinally on the middle of the belly. In this insect they are of an oval form, but they vary in shape in different insects: and it is chiefly by their mediation, that the changes on the air are effected. A female grasshopper was placed in eight cubic inches of atmospheric air: it breathed from fifty to fifty-five times in a minute, and lived thirty-six hours. The air had not sensibly diminished in volume, but, when examined by the test of lime-water, carbonic acid was detected, and after this was removed, the remaining air still extinguished a taper. When many grasshoppers were put at the same time into a given bulk of air, and left till they died, the oxygen gas was nearly, but not entirely, consumed: and phosphorus melted in the residual air when heat was applied, but burned very little. A male grasshopper lived eighteen hours in six cubic inches of oxygen gas: its respiration was oppressive, and it breathed from sixty to sixty-five

times in a minute. The volume of air was not sensibly diminished, but it lost $\frac{5}{100}$ of its bulk by being washed in an alkaline solution *. From these facts, we learn, that insects, by their respiration, consume the oxygenous portion of the air : that carbonic acid is, at the same time, produced ; and that, when all the oxygen gas has disappeared, the animal no longer survives.

54. M. Huber found, that bees very speedily die when put into nitrogen gas, but that they survive in a close vessel of atmospheric air, until almost the last atom of its oxygen gas is consumed †. We likewise confined a number of flies in a flask, containing nine cubic inches of air, and then inverted it into a tall glass of mercury. By the third day, the flies were all dead, and the mercury had risen considerably into the neck of the flask. The residual air lost about $\frac{18}{100}$ by agitation with lime-water, and the remainder did not suffer the smallest diminution by being placed in contact for two days with phosphorus. These results, therefore, agree with those obtained by Vauquelin, and prove farther, that, by the respiration of flies, the whole of the oxygen gas of the air disappears, and that a bulk of carbonic acid nearly equal thereto is formed. The small diminution of bulk also which the air suffered, is to be regarded as a necessary consequence attending the conversion of oxygen gas into carbonic acid, and which, as it accounts for the whole loss the air experienced,

* Ann. de Chimie, tom. xii.

† Mem. sur la Germination, &c. par M. M. Huber et Sennebier.

seems to authorise the conclusion, that, while the oxygen gas had, in this case, completely disappeared, the nitrogenous portion of the air continued undiminished, and probably unaltered.

55. M. Vauquelin proceeded next to investigate the changes produced on the air by the respiration of the *vermes* class of animals. He confined a red slug in twelve cubic inches of atmospheric air, and it lived forty-eight hours. He thinks, that in this animal the breathing pores are situated chiefly behind the head. The air was not sensibly diminished in volume, but it extinguished candles, and copiously precipitated lime from water. Phosphorus was melted in this air, but did not suffer any combustion or change of colour. A snail (*helix pomatia*) was next put into twelve cubic inches of atmospheric air, and lived four days. The oxygen gas entirely disappeared; for the residual nitrogen gas contained not an atom of vital air, and, consequently, phosphorus did not burn in it at all: it contained, however, carbonic acid. Slugs and snails, therefore, require fresh air while in an active state, the oxygen gas of which, by the function of their respiratory organs, is made completely to disappear, and a quantity of carbonic acid is produced, while the nitrogenous portion of the air remains unaltered: and when these changes are effected, living action speedily comes to an end. So exactly do these animals separate the oxygenous from the nitrogenous portion of the atmosphere, that M. Vauquelin suggests the employment of them for eudiometrical purposes *.

* Ann. de Chimie, loc. cit.

56. These experiments of M. Vauquelin were repeated by the late Abbé Spallanzani, and nearly with the same results. That industrious philosopher confined a slug (*limax flavus*) in a given quantity of atmospheric air; and the whole of its oxygen gas entirely disappeared, for the residual air was not in the least diminished by the introduction of phosphorus *. Other living slugs also entirely consumed the oxygen gas of the air, and produced carbonic acid, while the nitrogen gas remained unaltered †. In other instances, however, the whole of the oxygen gas did not disappear during the life of the animal: but, whether this happened or not, the nitrogen gas was, in all cases, left undiminished ‡. When placed in pure oxygen, a portion of that gas likewise disappeared, and carbonic acid was, in like manner, produced ||. Different species of worms were shut up in a given quantity of air, and they all consumed the whole of the oxygen gas it contained, and carbonic acid was always produced: and when pure oxygen gas was employed, more of it disappeared, and carbonic acid was in proportion produced §. The results of all these experiments coincide completely with those of M. Vauquelin, related in the preceding paragraph: they prove that the oxygenous portion of the air entirely disappears, that carbonic acid is produced, and that the nitrogen gas continues unaltered.

57. Different species of snails were next submitted to experiment by the same author. One of

* Memoirs on Respiration, p. 241. † Ibid. p. 253.
‡ Ibid. p. 258. || Ibid. p. 253. § Ibid. p. 68—70.

these animals was confined in seven cubic inches of air, inverted over mercury, for the space of six days, in a temperature varying from 7° to 8° Reaum.: and the bulk of air was sensibly diminished. A quantity of the residual air was then introduced into an eudiometrical tube, filled with mercury, so as to occupy one hundred parts: it was afterwards washed in lime-water, and re-introduced into the eudiometer, on which the mercury rose to 11°. Phosphorus was next inflamed in the remaining air; and when every thing was again brought back to the former temperature, the mercury had risen to $11\frac{1}{7}$° *. This elevation of the mercury, he adds, is not very sensible when phosphorus is employed, but when equal quantities of the residual air, and of nitrous gas were mixed together, so as to occupy two hundred parts, the diminution was four, five, or six of these parts, which indicated the quantity of oxygen gas the air contained †. From these results, he is led to conclude, in opposition to M. Vauquelin (*55.*), that the consumption of oxygen gas, by the respiration of snails, is not complete.

58. With regard to this partial consumption of oxygen gas by snails, it may be observed, that the author found the same thing sometimes to happen with slugs, which he, nevertheless, concludes generally to consume the whole of this portion of the atmosphere. The portion of oxygen gas unconsumed, is likewise so very small, and the result, by the test of phosphorus, so nearly approximates to that ob-

---

* Memoirs on Respiration, p. 146. † Ibid. p. 150.

59. But not only does Spallanzani dissent from the conclusion of Vauquelin, as to the complete destruction of oxygen gas by the respiration of snails: he contends likewise, that a portion of the nitrogen gas at the same time disappears. He placed different single snails in several equal bulks of atmospheric air, where they remained till they ceased to exhibit any signs of life. To ascertain the consumption of nitrogen gas, he passed the residual air into the eudiometrical tube through mercury, and compared its bulk with that which it possessed before the snails were placed in it. Having then ascertained the complete destruction of the oxygen gas, by the test of phosphorus, he next withdrew the carbonic acid by means of lime-water; and every degree of diminution beyond $\frac{20}{100}$, which he considers as the proportion of oxygen gas contained in atmospheric air, was referred to the destruction of the nitrogenous portion of the air *.

60. But, in these experiments, he has overlooked many circumstances which ought to have been attended to: and hence the results differ so much as to render the conclusion quite unsatisfactory. In two instances, $\frac{20}{100}$ of the oxygen gas of the air disappeared, and from five to $\frac{8}{100}$ of the nitrogen: but, in two others, only $\frac{16}{100}$ or $\frac{18}{100}$ of the former were lost, and from three to $\frac{4}{100}$ of the latter †. Where, in one experiment, two snails were confined together, the air lost $\frac{20}{100}$ of its oxygen, but only $\frac{2}{100}$ of its ni-

* Memoirs on Respiration, p. 162. † Ibid. p. 163.

trogen; so that much less nitrogen gas was destroyed in this case by two snails, than in the former experiments by one. In other experiments with snails, he has more than once found the whole eighty parts of nitrogen gas remaining; and the *helix vivipara* consumes all the oxygen gas of the air without producing any increase or diminution of the nitrogenous portion *. These facts, taken in connection with the apparently decisive experiments of Vauquelin, and those of the author himself with regard to slugs, lead us to the direct conclusion, that in the respiration of slugs, worms, and snails, the whole of the oxygenous portion of the atmosphere completely disappears; that carbonic acid is, in all cases, produced, and that the nitrogen gas remains unaltered.

61. Nearly the same results were obtained by experiments on the respiration of another species of snails, whose residence is wholly in the water. Spallanzani found, that the *helix vivipara*, which inhabits still rivers and pools, consumed, by its respiration, the oxygen gas of the atmosphere, like snails which live on the land: that this action did not go on in temperatures a few degrees only above the freezing point, but was very considerable in higher temperatures: that water, deprived of its air by boiling, did not support life, neither did it, when freed from its oxygen gas, and standing in contact with nitrogen gas: that snails, confined to the bottom of a jar of water, consumed only half the quantity of air that those did which were allowed to come to the surface:

* Memoirs on Respiration, p. 214. 301.

that the water attracts oxygen gas to supply the place of that which is consumed; and that, when confined in air only, these snails, by means of their skins, consume all the oxygen gas, and produce carbonic acid, without changing the quantity of nitrogen gas*.

62. Other aquatic animals, as muscles (*mytilus edulis*) were next confined in a jar, and water was poured over them to the depth of an inch. They soon opened the thinnest part of their shell, and, in ten or fifteen minutes more, threw out a small quantity of water: the shells were then shut again for a few minutes, and this action was alternately repeated. When two of these muscles were placed in a tube, half filled with water and half with air, in a temperature of about 66° Fahrenheit, they continued to live seven days, and, on the eighth day, the oxygen gas of the air was considerably diminished. With a portion of oxygen gas occupying the superior part of the vessels, these muscles would live nine days, but when nitrogen gas was in the same way employed, they died in three days. In every case, whatever quantity of nitrogen gas was present, it always remained unchanged †. On several species of marine *testacea*, he likewise made experiments, and found that they consumed the oxygen gas of sea-water, which attracts more to supply the waste; that when confined in air only, they consume all its oxygen gas, and that they soon perish when nitrogen gas only is made to rest on the water, producing no change upon it ‡.

* Memoirs on Respiration, p 288. 296. 301.

† Ibid. p. 304. 306. 309.

‡ Ibid. p. 311. et seq.

63. It has been already shewn (52.), that fishes cannot live in water deprived of air, nor unless this air be constantly renewed: and Dr Priestley proved, by experiment, that they deteriorate the air which water contains. Several minnows were confined by him in a large phial of water till they died: and the air being afterwards expelled from the water, was examined by the test of nitrous gas, and found to contain less oxygen than that in which a candle goes out. He likewise impregnated water, previously deprived of its oxygen, with nitrogen and with hydrogen gases, and found, that in such water, the fishes died in about an hour *. Mr Davy ascertained that fishes die in a few minutes in water containing nitrogen gas, but live in that which is impregnated with oxygen gas: that the proportion of this latter gas is diminished by them, and carbonic acid is produced; for, on adding lime-water to that in which the fishes had been confined, a cloudiness was very perceptible, indicating the formation of carbonate of lime †. Dr Carradori has observed, that fishes are able to exhaust water entirely of its oxygen, which ebullition is unable to effect: and that they die instantly in water wholly deprived of oxygen ‡. To supply the waste of oxygen gas, occasioned by the respiration of fishes, water, as Scheele first remarked, is endued with the power of attracting it in pre-

* On Air, vol. v. p. 137. et seq.

† Beddoes's Contributions to Physical Knowledge, p. 137.

‡ Nicholson's Journal, vol. xvi. p. 76.

ference to common air, and nearly in the same proportion *. Dr Crawford observes, that if a portion of atmospheric air be exposed in an inverted jar to water which has had its air separated by boiling, the purer part will be attracted by the water, and the noxious portion will remain distinct in the vessel †. The experiments of Mr Dalton, however, do not allow us to suppose that water thus completely analyzes the air, for, according to him, if a quantity of water, deprived of air, be agitated with atmospheric air, the water will attract portions of each of its constituent parts the same as if they were presented to it separately in their proper density ‡. From the operation of this distinct attractive power of water for the two gases of which the atmosphere is composed, it follows, that the oxygen gas, contained in water, will bear a considerably larger proportion to the nitrogen gas than exists in the air of our atmosphere; and that as this oxygen gas is consumed by the respiration of aquatic animals, more will be attracted to supply its place, and to maintain the due respirability of the air.

64. The changes induced on the air by fishes are effected by the *branchiæ* or gills, which in form and structure vary greatly, according to the mode of life of the fish. In eels, and those fishes which live in impure water, the gills are supported by boney arches, and are very large in proportion to the size

* Thomson's Chemistry, vol. iv. p. 490.

† On Animal Heat, p. 145.

‡ Manchester Memoirs, vol. i. new series.

of the fish: others, as the lamprey, receive the water into their gills by the mouth, and expel it through several holes in their sides, while those fishes that move rapidly, and make long migrations, take in the water largely by the mouth, and reject it very often by the gills. The gills were by the ancients considered to perform an office for fishes similar to that which the lungs perform for land-animals; and Mayhow conceived them to be especially constituted for separating the air from water, whereby some vital aërial property was conveyed into the mass of blood. Hence it is, he adds, that fishes alternately draw in and expel water, as land-animals receive and expire air *.

65. But a somewhat more particular description of the structure of the gills of fishes will much assist our conception of the manner in which the respiratory function is performed by them. In fishes, the heart consists only of one auricle and one ventricle; and from the latter, one artery is sent off, which is spent entirely on the gills. This artery conveys venal blood from the heart, which, in its passage through the gills, assumes a florid hue, and being afterwards collected by the branchial veins into one large trunk, is distributed, without the intervention of a second heart, to all parts of the body; from which it is again brought back to the heart in a venal state, to undergo the same circulation. The gills, upon which the branchial artery ramifies, are of great extent. In each side of the body of a skate,

* Tractat. Quinq. p. 259.

says Dr Monro, there are four double gills, or gills with two sides each, and one single gill; or there are in all eighteen sides or surfaces on which the branchial artery is spread out. On each of these sides, there are about fifty divisions or doublings of the membrane of the gills; and each division has on each side of it one hundred and sixty subdivisions or folds of its membrane, the length of which, in a very large skate, is about one-eighth part of an inch, and its breadth about one-sixteenth part: so that in the whole gills there are about 144,000 subdivisions or folds, the two sides of each of which are equal to the sixty-fourth part of a square inch; or the surface of the whole gills in a large skate is equal to 2.250 square inches, that is, to more than fifteen square feet, which have been supposed equal to the whole external surface of the human body. When, after a good injection of the branchial artery, a microscope is applied, the whole extent of the membrane of the gills is seen covered with a beautiful network of exceedingly minute vessels; and if distilled oil of turpentine, coloured with vermilion, has been injected with moderate force in a living or recently dead skate, some of the colourless parts of the oil exude upon the surface of the gills *. From all these facts, concerning the respiration of fishes, we learn, that the oxygen gas of the air contained in water, is changed (63.) into carbonic acid by the medium of their gills; and that their blood, like that in the lungs of breathing animals, loses at the

* Monro on the Structure, &c. of Fishes, p. 15.

same time its venal characters, and becomes arterialized. Since also, it has been shewn (63.), that fishes do not live in water which contains only nitrogen gas, it is reasonable to infer, that, like water-snails and muscles, they produce no change upon it.

66. The amphibious class of animals, which live partly in air and partly in water, exhibits great variety in the structure of their circulating and respiratory organs. In the frog and toad, the heart consists of one auricle and one ventricle, as in fishes. The auricle receives the venal blood from the body, which passes into the ventricle, and from thence into the aorta; but this aorta soon divides into two branches, one for the body and one for the lungs; and hence but half of their blood is, in each circulation, exposed to the action of the air received into the lungs. In the turtle, the same intention is effected by a different mechanism; for, though the heart of that animal consists of four cavities, yet the ventricles freely communicate, and therefore the pulmonary artery and aorta arise in fact from one cavity; so that only half of the blood, thrown out at each contraction of the ventricles, will pass through the lungs, provided the areas of the two arteries correspond. As a part only of the blood is thus sent through the lungs in each circulation, it is plain that a cessation of the respiratory function does not necessarily put a stop to the circulation of the blood in these animals, as it does in fishes, in the mammalia and in birds; and such animals are said to possess a *pulmo arbitrarius*, or are able to live either in water or in air. The length of time which they can live without respiration, has been supposed to depend on the

structure of their lungs, and the capacity of these to receive and contain air. In the frog and toad, the lungs consist of two large membranous bags, divided into a great number of vesicles, over which the blood-vessels are minutely distributed; and, in the snake, viper, and many others, the lungs are continued down through the whole belly, in form of two bags. Many animals also of this class, as the otter and porpoise, whose lungs are constituted like those of man, can live a considerable time under water without breathing: and this power is much improved by habit. This is the case even in the human subject; for, while in ordinary persons, suffocation begins to take place in about half a minute when the body is submersed in water, those who dive for pearls, corals, &c. are said, by long practice, to be able to prolong this period to several minutes, being able to keep under water as long as the seal, porpoise, and the amphibia.

67. Of all the foregoing animals, therefore, which by naturalists have been placed in the class *amphibia*, none can be said to be truly amphibious, or to possess the faculty of supporting life, for an indefinite length of time, either in water or in air. This faculty belongs only to the syren, an animal said to be furnished both with lungs and gills. Something of the same sort may, indeed, be attributed to the frog at different periods of its existence; for we are told, that, during the first fourteen days of its life as a tadpole, it has only gills projecting like fins: that, by the thirty-sixth day, these are taken into the jaws, and form four rows of gills on each side, like those of fishes: and that, during this time, the lungs, as in the

fœtus of the mammalia, are inert, and not called into use until the animal exchanges his watery habitation for an aërial one, when the gills gradually shrink *. Hence, then, it appears, that however various the structure of the lungs in this class of animals may be, a more or less constant supply of fresh air is required to enable them to support the functions of animal life.

68. To obtain a knowledge of the specific changes which the air suffers by the respiration of the amphibia, the following experiments were instituted. A toad, supported on a small hoop, was inclosed in one hundred and eight cubic inches of atmospheric air contained in a jar inverted over water, and standing in a room varying from 55° to 60° Fahrenheit. He died on the fifth day. The water had risen considerably into the jar, and the residual air was still farther diminished by agitation with lime-water, which it rendered turbid. Fifty parts, after being washed in lime-water, were next shaken in the eudiometer with the liquid sulphuret of potassa, and lost only one part of its bulk. The experiment was repeated by confining another toad, in the same manner, in another jar containing forty cubic inches of atmospheric air, inverted over mercury. Under the hoop which supported the animal, was placed a small cup containing 1.5 cubic inch of the water of potassa, which floated on the mercury. The whole was then set aside in a room, of the temperature of 64°. By the twelfth hour, the mercury had

---

* John Bell's Anatomy, vol. ii. p. 152.

risen nearly half an inch into the jar, which was thickly moistened with vapour, and the breathing of the animal seemed rather languid: by the twenty-first hour, he breathed very faintly; and, by the twenty-fourth hour, he had ceased to breathe. The jar was allowed to stand some hours, at the end of which time, the mercury stood about eight-tenths of an inch high, and one-tenth of an inch of fluid was deposited on its surface. The jar was now raised, and diluted sulphuric acid being poured into the alkaline solution, excited in it a verybrisk effervescence. It is inferred, therefore, from these experiments, that the oxygenous portion of the air almost entirely disappears during the respiration of these animals, after which they cease to breathe; and that a large portion of carbonic acid is at the same time produced.

69. Proceeding on the supposition, that the loss in the bulk of air, evinced by the ascent of the mercury, in the last of the foregoing experiments, arose from the attraction of the carbonic acid by the alkaline solution, we endeavoured to ascertain the proportion which this loss of bulk bore to that of the whole air originally employed. With this view, a frog was procured, and placed in a jar of the capacity of forty cubic inches. Under the hoop which supported him, about half way up the jar, was placed a small cup, containing one cubic inch of the water of potassa, and the jar being then filled with atmospheric air, was inverted into a dish of mercury, and kept steady by a weight pressing upon it. In the room in which the animal was placed, the barometer stood at 29.2 inches, and the thermometer at 61°. At the end of twenty-nine hours, the animal was

resting quietly on the hoop, with no appearance of distress, and the mercury in the jar, when that in the dish was brought to a level with it, had risen six-tenths of an inch. In twenty-four hours more, the frog was still alive: his respiration seemed now to labour, and he rose often to the top of the jar as if desirous of escaping, or of obtaining fresh air: the mercury had now risen to 1.15 of an inch. From this time, the difficulty of breathing continued to increase, and, at the close of the fifty-ninth hour from the commencement of the experiment, after having lain quiet for a considerable time, he gave a convulsive struggle, and moved no more. The mercury in the dish was now brought to a level with that in the jar, and its height was 1.2 of an inch. The barometer, at this period, was 29.8, and the thermometer 65°.

70. In order to examine the residual air, we plunged the dish under water, which, rising into the jar, displaced the mercury, and the cup, with its solution, was then withdrawn under water. The residual air suffered no diminution by being shaken with lime-water, nor by contact with phosphorus, but it lost rather more than $\frac{1}{100}$ by agitation with the liquid sulphuret of potassa. The jar originally held forty cubic inches, but the animal, with the hoop, cup, and solution, occupied a space equal to four, so that the actual bulk of air employed was thirty-six cubic inches. Having placed the jar on its bottom, water, to the quantity of twenty-seven cubic inches, was poured in, till it reached the point to which the mercury, during the experiment, had risen; and this, therefore, indicated the volume of residual air: it

then required nine cubic inches more of water to fill the jar completely, which, consequently, was the bulk of air that had disappeared. Hence, therefore, we have $\frac{27 \times 29.8}{29.2} = 27.5547$, but $\frac{4 \times 27.554}{483} = .22819$ and $27.554 - .22819 = 27.32651$, the corrected volume of air at the close of the experiment. But farther, $36 - 27.32651 = 8.67349$, and $\frac{8.67349}{36} = \frac{1}{4.15}$; so that the diminution of bulk which the air suffered in this experiment is rather greater than $\frac{1}{4.54}$ the proportion of oxygen gas which the atmosphere contains. In a second experiment, another frog lived in the same volume of air about sixty hours, and the diminution which it suffered, after making the necessary reductions, amounted to $\frac{1}{4.868}$ of the whole. Where the carbonic acid, formed by the respiration of another frog, was suffered to remain, the jar, after the death of the animal, adhered firmly to the saucer in which it was inverted, and, when cautiously elevated, the surrounding mercury rushed in, and occupied only about one-tenth of the space which it filled in the above-mentioned cases. The inferences deducible from these facts, instruct us, that the diminution which atmospheric air suffers by the respiration of these animals, bears a near proportion to the oxygen gas which it contains, when all the carbonic acid is removed : and, as a small loss of bulk likewise takes place when this acid is allowed to remain, we must ascribe a part of the observed diminution to the necessary loss which always accompanies the conversion of oxygen gas into carbonic acid.

71. It follows from the preceding series of experiments, that the oxygenous portion of the air is changed by the respiration of amphibious animals in the same manner as by that of the other classes, carbonic acid, in proportion thereto, being, in all cases, produced: and that when the whole, or nearly the whole, of that gas is so changed, the animal no longer survives. But if the animal die when all the oxygen gas is changed, and all the air that has disappeared, when the carbonic acid is removed, be oxygen gas, then the bulk of air that remains, and is unchanged, must consist wholly of nitrogen gas; and, as this nitrogen gas, joined with the oxygen gas that has disappeared, makes up the whole bulk of air originally employed, it follows also, that, while the oxygen gas of the air has diminished and suffered change, the nitrogenous portion has continued undiminished and unaltered.

72. During all these changes operated on the air contained in water by the respiratory functions of aquatic animals, the water itself seems to suffer little or no alteration. Mr Carlisle took separate glasses, each containing one pound of distilled water, which was previously boiled to expel all its air, and then, inverting them over mercury, he put into them one gold fish, one frog, two lēeches, and one fresh water muscle. The animals were confined several days in these situations, and exposed to the sun during January in temperature 43° and 48° Fahrenheit: but no air-bubbles were produced in the vessels, nor was there any sensible diminution of the water. The frog died on the third day, the fish on the fifth, the leeches on the eighth, and the muscle on

the thirteenth day. This experiment was made to ascertain the changes produced in water by the respiration of aquatic animals; but the water had not undergone any chemical alteration *.

73. From this enumeration of the principal facts, concerning the changes induced on atmospheric air by the respiration of all these several classes of animals, we obtain positive evidence, that, in most cases (53. et seq.), its nitrogenous portion, as in the growth of vegetables (5. 29.), continues unaltered: and since, in the remaining cases, the air in degree is proved to suffer the same change, and the ultimate result, viz. the display of living action, is, in all respects, the same; it is not, we hope, exposing ourselves to the charge of too hasty generalization, or of resting too much on analogy, if we conclude, that, in the whole view which we have hitherto taken of animal respiration, the nitrogen gas of the atmosphere remains unchanged. Moreover, as this gas itself suffers no change, so neither does it seem to exert any influence on the animal in contact with it; for Spallanzani found, that snails could live in nitrogen gas twelve hours †, which is as long as they live *in vacuo* (51.): and, in all the experiments we have made in atmospheric air, the animals did not appear to die from the superabundance of nitrogen gas, but from the small proportion, or total absence, of oxygen gas.

---

* Philosophical Transactions, 1805.

† Memoirs on Respiration, p. 347.

74. Assuming then, as a fact, that the nitrogen gas of the air neither produces nor suffers change, we have next to inquire, what becomes of the oxygen gas, which has been shewn, more or less, in all cases, to disappear. Is it *absorbed* by any organized structure of the animal adapted to the performance of such an office? No vessels fitted for such a purpose have been yet demonstrated in the animal system. The small size of the stigmata, or breathing pores, in insects, renders them but little suited to be receptacles for containing and decomposing air: and, in many of the *vermes* class, the mucous matter with which their bodies are constantly smeared over, must oppose great difficulties to such an absorption. In the case of aquatic animals, these difficulties are still farther increased; for the air must be first separated from the water before it can be taken up by absorption; and, after this is effected, it is not easy to conceive how the gills of fishes can be rendered capable of absorbing and emitting air. It is not probable, that this air is taken up in its entire state, for as the nitrogen gas undergoes no change (73.), its absorption can answer no obvious use, but would tend rather to impede the decomposition supposed to go on within the vessels, and the subsequent formation and emission of the carbonic acid. If, on the other hand, the air be considered to be decomposed previous to its absorption, then a new compound must be at once formed; and, if this be brought about by the union of some substance with the oxygen gas, then that gas, simply as such, cannot be held to enter into the animal system. No one has ever yet detected air in the animal fluids

while in a healthy state; and if we consider the great extent of surface, and extreme minuteness of the vessels in the gills of fishes (65.), we cannot but consider them as well adapted to produce an extensive contact of surfaces, and but little fitted to absorb, decompose, and again emit aëriform fluids.

75. If to account for this supposed entrance of air into the vascular system, the agency of *chemical affinity* be had recourse to, by what means, we would ask, can its operation be in this case explained? No sensible or obvious principle, equal to such an effect, can be held to reside in the blood, since the changes go on equally in all these animals, though the blood be of various colours, and, in many instances, where it is totally devoid of colour. During a torpid state also, Spallanzani has shewn, that no change is effected by the animal on the air *; and consequently, no oxygen gas is then attracted by the blood, although, if the supposed carbon of that fluid be considered to attract this gas, the union ought still to proceed, because, according to the received opinions, the animal system is at this period surcharged with carbon. Neither can the conditions, indispensable to the operation of chemical affinity, be in these cases fulfilled; for the interposition of organized substance between the air and the blood, altogether precludes that degree of absolute contact, which is held to be essential to chemical action. Even if the oxygen gas were attracted into the blood by the operation of chemical affinity, by

* Memoirs on Respiration, p. 334.

what power would the carbonic acid that is formed be again given out by that fluid? No chemical agent either in the air or the water can be imagined equal to the re-attraction of it through the organized structure of the animal: nor is it conceivable, how the blood, by any power of its own, should be able to emit it, independent of such agent. And to suppose, that, by a power of chemical affinity, this gas should enter into the blood, and be afterwards expelled from it, as carbonic acid, by any method analogous to the ordinary animal excretions, is too inconsistent to be entertained for a single moment.

76. On the grounds, therefore, that the oxygen gas of the air does not obtain admission into the blood-vessels, either by the function of absorption, or by the operation of chemical affinity, we must reject the belief of its union with the supposed carbon of the blood, to form the carbonic acid that is produced. Still, however, the gradual disappearance of that gas, and the production of carbonic acid which ensues, justify the conclusion, that in the animal, as well as in the vegetable, kingdom, they observe always a regular and progressive ratio, and are, in fact, proportional to each other, which admits of no other solution than that of their being converted into one another. To effect this conversion, however, no other substance but the animal was present, in these experiments, from which the carbon could be derived; consequently, the acid must be formed by the union of carbon furnished by the animal with the oxygen gas of the air, and this, too, exterior to the vascular structure of the animal.

77. Those who maintain, that the carbonic acid is not directly formed by the union of the oxygen gas of the air with the animal carbon, but that it escapes ready formed from the animal system, ought to point out some other source from whence, in sufficient quantity, the oxygen gas can be derived: to tell us at the same time what becomes of the oxygenous portion of the air that actually disappears: and why the production of carbonic acid bears always so constant a proportion to the loss of this oxygen gas. To suppose with Spallanzani, that this acid is yielded by the process of digestion, because some snails which had been well-fed, furnished more of it than others which had suffered a long abstinence, is by no means proving the point; for a snail which had long fasted, yielded as much, in one instance, as those which had been recently fed, and, in the other examples, the starved snails fell short only in a small degree. Every animal function, also, is, *cæteris paribus*, carried on best in a state of health and vigour, which again depends altogether on a due supply of food: hence, therefore, the debility succeeding to abstinence, must affect the organs of respiration, in common with the other organs, and consequently their power of acting so completely on the air. Whatever substances, moreover, are received into the animal system, suffer or produce some change: but to suppose, that carbonic acid should be first formed in the stomach, then taken up by the lacteal vessels, and carried through the mass of blood to be again thrown out by the respiratory function, simply as carbonic acid, is not only without proof, but against all probability. It is also to the quantity of air

changed, and not to that of food taken in, that this acid bears a proportion; and, provided living action be equally well maintained, as much air seems to be required by an animal when he abstains from food as when he takes it, and as much carbonic acid to be produced. In as much, however, as a long abstinence from food, debilitates the system, and affects the production of carbon, in so much will it diminish the quantity of carbonic acid, which the animal is accustomed to form.

78. But the quantity of this acid, when formed by the respiration of a given volume of air, does not seem to exert any noxious operation on the animal powers; for Spallanzani found, that animals placed in a given bulk of air, did not live longer when the carbonic acid was abstracted by an alkaline solution as soon as formed, than when it was suffered to remain *; and, in all our experiments, where the carbonic acid was allowed to remain, the death of the animals seems to have arisen, not from the over-proportion of that acid, but from the diminished quantity, or total absence, of the oxygen gas. Since, indeed, air is necessary to the continuance of living action in all animals, and its nitrogenous portion appears to suffer no alteration, this necessity must arise from its containing oxygen gas, and from the requisite changes which, in respiration, that gas is made to undergo. When, therefore, the greater part, or the whole of the oxygen gas of the air is so changed, death ought to happen; and then accordingly,

* Memoirs on Respiration, p. 317.

and not till then, does this event take place. It is therefore to the small proportion or total absence of oxygen gas, and not to the presence of carbonic acid formed out of that gas, that the cessation of the animal functions, in all the foregoing examples, is to be immediately ascribed.

CHAP. IV.

OF THE CHANGES INDUCED ON THE AIR BY THE RESPIRATION OF BIRDS, OF QUADRUPEDS, AND OF MAN.

SECTION I.

79. Not only, as we have seen, is water necessary to prepare the organization of vegetables, and of the inferior animals, for exhibiting living action, but it is required also by those which belong to the superior orders. " The whole material world," says Mr Hunter, " has been very properly divided into solids and fluids, these being the only essentially different states of matter which we are able to observe. From one of these states into the other, matter appears to be continually passing; but no species of matter can assume a solid form without having first been in a fluid state; neither can any change take place in a solid, till it be first reduced to, or suspended in, a fluid. The living animal

body is obedient to these general laws; for all the solid matter of animals has been once in a fluid state, and, having passed into the solid form, becomes a recipient for other fluids, out of which the solids themselves may, in turn, be increased and renovated *."

80. This conversion of fluid into solid matter, cannot, however, go on in the animal body without the constant presence of heat, the agency of which is essentially necessary to carry forward these transformations. When, to a certain degree, heat is abstracted from the body, its vital functions gradually decline, and at length finally cease. Many animals experience these effects periodically, without injury to the vital organs, and the actions of life re-appear as the temperature of the season returns. In our own climate, the hedge-hog, the bat, the dormouse, and several birds, pass into a state of torpidity during the winter season; and, in the more northern parts of Europe and America, the bear and alligator do the same: from which we may conclude, that a certain degree of heat is necessary to sustain the actions of life in the superior, as well as in the inferior animals.

81. The operation of light on the colour of animal bodies (50.), and probably on some of their other properties, has been already noticed. In the human subject, the colour of the skin depends on that of the reticular membrane placed beneath the cuticle, which assumes various colours in different parts of

* Treatise on the Blood, p. 12.

the earth. In the native American, the inhabitant of Asia, and the southern European, the colour varies from dark copper to pale tawny; and in the negro it is quite black. The inhabitants of the northern countries, on the other hand, are white: and, not only are those parts of the body, which are most covered, the whitest, but a sensible difference in colour exists in the same person at different seasons of the year. These variations in colour have been held to depend very much on the agency of light.

82. The necessity of atmospheric air to the continuance of living action in all the superior animals, was long ago proved by the experiments of Mr Boyle, soon after the discovery of the air-pump; and he observed farther, that the function of respiration is quickly suspended, unless the lungs are furnished with a regular supply of fresh air *. "Animals, whose hearts have two ventricles and no *foramen ovale*, says Mr Derham, as birds, dogs, cats, and mice, die under the action of the air-pump in less than half a minute, counting from the very first exsuction, especially in a mall receiver †." In corroboration of these facts, we may mention an experiment, exhibited before the Royal Society, by the celebrated Dr Hooke. He cut away the ribs, diaphragm, and pericardium of a dog, whereby the lungs and heart were brought into view; and then, dividing the windpipe, he introduced into it the nozzle of a pair of double bellows, and made, at the

* Boyle's Works, vol. i. p. 99.

† Physico-Theology, p. 8.

same time, several small punctures through the outer coat of the lungs. By blowing in a stream of fresh air, which continued to escape through the small apertures made in the lungs, he was enabled to keep those organs fully distended. As long as he supplied the lungs with air, the actions of life continued, and the heart beat very regularly; but, on intermitting the supply, the dog would immediately fall into dying convulsive fits, and revive again as soon as the lungs were filled with a stream of fresh air. The circulation through the lungs continued both during their distended and collapsed state, and as well when they were kept at rest, as during a state of motion: whence he concluded, that neither the motion of the lungs, nor the cessation of their motion, nor the stopping of the circulation of the blood through them, was the immediate cause of death, but the want of a sufficient supply of fresh air *. By the researches of later philosophers, it has been proved, that the air, in all animals, serves the same uses; to fit it for which, it undergoes the same changes. In our inquiry into the nature of these changes, the manner in which they are effected, and the uses which they are found to serve, we shall confine ourselves chiefly to the facts which take place in human respiration, not only on account of their greater interest, but also because they have occupied more research, and, having been more frequently submitted to experiment, are in some respects better ascertained.

---

* Lowthorpe's Abrid. Phil. Trans. vol. iii. p. 66.

83. When atmospheric air is respired by man and by other animals, it undergoes two remarkable changes: its bulk is diminished, and its qualities are altered. This diminution of bulk was early noticed by Boyle, who estimated it at about $\frac{1}{30}$ of the air employed. Mayhow, whose genius enabled him to anticipate so many important discoveries of modern chemistry, confined an animal in a glass-vessel inverted over water, and, by the aid of a syphon, brought the water on the inside of the vessel to a level with that on its outside. Having then marked the height of the water by pieces of paper affixed to the vessel, he observed its gradual rise as the animal continued to breathe: and then comparing the space occupied by the air at the commencement of the experiment, with that which it possessed when the animal ceased to breathe, he found that it was reduced about $\frac{1}{14}$ part of its bulk *. In the experiments of Dr Hales, the degree of diminution varied from $\frac{1}{13}$ to $\frac{1}{30}$ of the whole air employed †: and in those of M. Lavoisier from $\frac{1}{31}$ to $\frac{1}{60}$ part ‡; with which the results of Dr Goodwyn's experiments on his own respiration nearly coincide ‖. Dr Priestley confined a mouse in a jar containing a given quantity of air, which was inverted over mercury: the animal was suffered to remain two or three days after he had died, in which time there was no sensible diminution of the air, but

* Tractat. Quinq. p. 104.

† Statical Essays, vol. i. p. 238.—vol. ii. p. 320.

‡ Mem. Acad. 1777 and 1780.

‖ Connexion of Life with Respiration, p. 51.

on passing lime-water into the jar, the air was diminished $\frac{1}{28}$ part of its bulk; and when, in a subsequent experiment, the residual air was agitated in water, it was reduced between one-fifth and one-sixth of the whole *. Dr Crawford found also, that when the experiment was made over mercury, the diminution was not sensible; but that, if water of potassa was added to the residual air, it became mild, and the air was diminished in the same degree as if the experiment had been made over water, or nearly one-fifth of its bulk †. These variations in the results arise, no doubt, from the more or less complete attraction of the carbonic acid by the fluids over which the experiments were made; and, from the whole of them, we may collect, that, when mercury is employed, which has no attraction for carbonic acid, the diminution is hardly sensible; but that when this acid is completely abstracted by an alkaline fluid, the loss of bulk amounts nearly to one-fifth of the whole air employed. This inference corresponds very exactly with the facts which occur in vegetation, and in the respiration of the inferior animals.

84. But experiments of this nature, although they shew the extent to which the destruction of the oxygen gas, contained in a given quantity of air, may, by the process of respiration, be made to proceed, yet they do not apply to the ordinary circumstances in which that function is carried on; for the air of

---

* Observations on Air, vol. v. p. 112. et seq.

† On Animal Heat, p. 146.

the vessels in which the animals were confined, must, by repeated breathing, have become less and less fit for respiration, and was therefore gradually declining from that state in which it is usually inspired. We have seen, that several of the inferior animals will live in a given quantity of air until its oxygen gas is completely (54. et seq.) consumed: but those of the superior orders do not bear this total privation. Birds die in air confined by lime-water, before they have consumed two-thirds of its oxygen gas: and a mouse and guinea pig expire when about three-fourths of this gas have disappeared, although the carbonic acid be withdrawn *. Spallanzani observes likewise, that birds and quadrupeds consume not more than $\frac{19}{100}$ of the oxygen gas of the air, and sometimes only 17, 16, or $\frac{15}{100}$, and then die, even although the carbonic acid be removed †. Lavoisier found, that by repeatedly withdrawing the animal from the vessel when he began to sicken, and re-introducing him after he had revived, he could be made to consume almost the whole of the oxygenous portion of the air ‡. In all the foregoing experiments, it may be doubted, whether the same actual diminution in bulk takes place, as would have occurred if the same volume of air had been submitted to successive respirations in the open atmosphere. It is only with the latter

* Higgins's Minutes of a Society, p. 158.

† Memoirs on Respiration, p. 318.

‡ Mem. Acad. 1783.

kind of diminution, viz. that which takes place in natural respiration, that we are at present concerned; and as this cannot be determined by experiments made on brutes, we must resort to the facts which have been ascertained by those instituted on the respiration of man.

85. A knowledge, however, of the diminution of bulk which the air, during respiration, suffers, implies a previous determination of the quantity ordinarily inspired. To ascertain this point, many modes of experiment have been adopted, and the conclusions which have been drawn from them very widely differ. Borelli estimated the bulk of air taken in at a single inspiration, at 15 cubic inches *; Mr Kite from 12 to 17 †; Dr Goodwyn at 14 ‡; Mr Davy from 13 to 17 §; and Drs Jurin, Hales, Haller and Sauvages, at 40 cubic inches. With the conclusion of these latter authors the experiments of Dr Menzies nearly coincide, and as the methods which he adopted seem less liable to objection than those of any other author, it may not be improper shortly to give the detail of them. He procured an allantoid, and fixed to it a machine consisting of two pretty large tubes, joined at right angles, nearly in the form of a common brass cock. One end of the horizontal tube was connected with the allantoid, and the other received into the mouth, while the upright tube, which rose from its centre, communi-

* De Motu Animal.

† Essay on Apparent Death, p. 24.

‡ Connexion of Life with Respiration, p. 28. et seq.

§ Researches, p. 410. & 433.

cated with the atmosphere. The tubes were large, and valves, made out of an allantoid, were affixed to the end of the upright tube, and to that attached to the allantoid, so that the air, when expelled from the lungs, should not escape into the atmosphere, nor return from the allantoid, after having once entered it. Precautions were taken also, by covering the mouth and nostrils, to prevent any air from passing in or out of the lungs, except by the tubes above mentioned. Things being thus prepared, he began to respire, and did not remove his mouth from the tube till he had filled the allantoid, taking care to stop his nostrils during expiration. The allantoid was filled, in repeated trials, by about 56 expirations, as natural as possible; and as its capacity was 2400 cubic inches, the average bulk of air thrown out of the lungs by each expiration, was 42.8 cubic inches. He then fixed another allantoid, whose capacity had been previously ascertained, to the end of the upright tube; and having filled it with atmospheric air, he inspired the air from one allantoid and expired it into the other, and the quantities were found to be nearly the same. Several persons of the middle size repeated this experiment with nearly the same result; the difference being scarcely ever more than one or two cubic inches. By another mode of experiment, first proposed by Boerhaave, of plunging a man into a tub of water up to his chin, and judging of the dilatation of the lungs from the ascent and descent of the water, he obtained, by several trials, nearly the same results; and when these same men were made to breathe from and into the allantoids, in the manner above described, the correspondence by the two methods was almost complete *. As there seems no obvious source of inaccuracy in the processes here employed, and their results so remarkably coincide; and as they present the average bulk deduced from 56 respirations, we may conclude, says Dr Bostock †, that 40 cubic inches is the quantity of air employed in an ordinary act of respiration ‡.

86. The difficulty in arriving, by experiment, at certain conclusions respecting the volume of air taken into the lungs in each inspiration, may arise from a difference in the state or capacity of those organs in different individuals; from the relative vigour or debility of the muscular powers carrying on the respiratory function; from the circumstances in which the animal is placed; the composition of the air itself; or the manner in which it is breathed. In many modes of experiment also, the friction between the air and apparatus employed, or the resistance which this latter may create to the ordinary process, will greatly vary the result: and considerable errors must likewise have arisen from the variation in bulk, occasioned by the change of temperature, which the air, during its respiration, suffers; from the difficulty of breathing in a natural manner when the

* Menzies on Respiration, p. 21. et seq.

† On Respiration, p. 34.

‡ Besides the respectable authorities mentioned in the text, Dr Bostock quotes the names of Blumenbach, Chaptal, Bell, Fontana, and Richerand, as estimating the bulk of a single inspiration at between 30 and 40 cubic inches of air.

mind is directing that process; and from the embarrassments opposed to the natural action of the respiratory organs by the contrivances adapted to them.

87. It will not be denied, that the size and capacity of the chest must, in a certain degree, regulate the quantity of air which is taken into or expelled from it; and since respiration is neither wholly a voluntary nor an involuntary act, but within certain limits partakes of the nature of both, and is carried on by the exertion of muscular powers, the bulk of respired air must vary also, either from an alteration in the action of these powers, or from a change in the will of the agent who exerts them. This may be illustrated by considering the different quantities of air taken into the lungs in different states of natural and forced respiration. Dr Goodwyn, supposing a person at death to make a *complete* expiration, endeavoured to ascertain the bulk of air then remaining in the lungs, which he estimated at 109 cubic inches *. This estimate he formed by measuring the capacity of the chest, in subjects who had died a natural death by disease, previous to which the expiratory powers must have been much weakened, and unable, in consequence, to expel so much air as when in a state of health and vigour; and in such cases, therefore, expiration might be final without being complete. Mr Cruickshank observes, accordingly, that the lungs in the dead body, (though expiration is the last action of life), always retain more

* Connexion of Life with Respiration, p. 27.

air than is given out at several expirations*. By a very different mode of experiment, we find Mr Davy to conclude that his lungs, after a forced expiration, contain only 32 cubic inches of air, when it is reduced to the temperature of 55°, but which, by the heat of the lungs, and saturation with moisture, are increased to 41 cubic inches; and, after a natural expiration, they contained 118 cubic inches†; so that the difference between the two states of natural and forced expiration is 77, which is somewhat more than Dr Menzies allows, who remarked that many men, after an ordinary expiration, could still expel from their lungs 70 cubic inches of air‡. Mr Davy adds, that his estimate of 118 cubic inches, as the capacity of the lungs after natural expiration, agrees very well with that of Dr Goodwyn, who makes it about 109‡; and, on the supposition that the general debility which precedes the ordinary extinction of life, so weakens the expiratory muscles, as to disable them from making so complete an expulsion of the air, as they can effect when in health and vigour, the agreement is very striking; for nearly the same quantity of air would, in that case, remain in the lungs at the period of natural death, as after that of ordinary expiration.

88. Dr Bostock conceives, that Dr Goodwyn's estimate of 109 cubic inches of air remaining in the lungs after complete expiration, is not very remote

---

* On Insensible Perspiration, p. 97.

† Researches, p. 409. & 410.

‡ Dissertation on Respiration, p. 31. § Ibid. p. 411.

from the truth: and he objects to Mr Davy's mode of ascertaining the residual air of the lungs after a forced expiration, from a supposition that the hydrogen gas which he inspired for that purpose was not, in consequence of its low specific gravity, uniformly diffused through all the cavities of the lungs: and therefore, that the proportions of the gas discharged could furnish no accurate estimate of those which were retained *. But Mr Dalton has shewn, that hydrogen gas and atmospheric air intermix, when the former is kept in a phial above the latter, and communicating only by the small tube of a tobacco-pipe; and both in a state of rest †: How much more readily then may this be expected to take place, where the gases are exposed to so large a surface, such great agitation, and increased temperature, as they must have been in the experiments of Mr Davy. Neither is the small quantity of air, which Mr Davy assigns, so incompatible, as Dr Bostock supposes, with the anatomical structure of the thorax; for if we call to mind the space which the heart and the lungs occupy, and recollect, that, under a violent exertion, the chest is made to contract in every direction, and more especially by the ascent of the diaphragm nearly to the fourth or fifth rib, there is no difficulty in imagining the quantity of air in the lungs, in such circumstances, to be nearly that which Mr Davy's experiments assign.

---

* Essay on Respiration, pp. 17. 25.

† Manchester Memoirs, vol. i. new series.

89. From a review, therefore, of all the facts and experiments above stated, we venture to draw the following conclusions, as approaching nearest to the truth. First, then, according to Mr Davy, the lungs contain, after a forced expiration, a bulk of air equal to about 41 cubic inches; and according to the same author and Dr Goodwyn, they contain, after a natural expiration, from 109 to 118 cubic inches: therefore the state of forced is to that of natural expiration as 41 to 118. Secondly, according to Dr Menzies, 40 cubic inches of air are received into the lungs at each ordinary inspiration: therefore the state of natural expiration to that of natural inspiration will be as 118 to 158. Mr Davy found likewise, that by a forced expiration after a forced inspiration, he could expel from his lungs 190 cubic inches of air, and Dr Menzies often found it to amount to 200 inches: therefore the state of greatest exhaustion of the lungs is to that of greatest repletion, as 41 to 231. But the 41 cubic inches of air, when inspired at temperature 55°, occupied a bulk equal only to 32; and therefore, by the same rule of proportion, 190 cubic inches, inspired at the same temperature, will be increased to 241.5: consequently, the greatest diminution of the capacity of the chest to its greatest expansion will be as 41 to 241, in the case of Mr Davy. But these numbers must be considered as indicating proportions only, the absolute quantities being different in different persons *. These facts decidedly shew how much

* At the time of making these experiments, Mr Davy states

the volume of air in the lungs will at all times depend on the relative capacity of those organs, on the more or less vigorous state of the expiratory powers, and on the degree of voluntary exertion with which the function may be performed.

90. The circumstances in which the animal may happen to be placed, will render this variation still more striking. Thus, from the experiments of Mr Kite and Mr Coleman, we learn, that in the act of drowning, animals are able to expel almost all the air which their lungs contain, by which those organs are brought into a state of collapse *. Dr Goodwyn, on the other hand, found, that in three executed persons, the lungs were expanded almost to their utmost extent, containing 250, 262, and 272 cubic inches of air †: and Mr Coleman observes, that when, previous to their suspension, he secured the trachea of animals by a ligature at the instant an inspiration was made, in less than four minutes they ceased to struggle, though the whole of the air was confined within the lungs, and no obstruction to the passage of the blood existed from their collapse ‡. Dr Baillie also has often observed the lungs filling the chest, and distended with air and mucus, in persons who have died asthmatic; so that to die and to

---

his chest to have been narrow, not exceeding in circumference 29 inches. (Researches, p. 410.).

* Kite on Apparent Death, p. 27. 29.

Coleman on Suspended Respiration, p. 7. et seq.

† Goodwyn's Essay, p. 25.

‡ On Suspended Respiration, p. 111—138.

expire are by no means synonymous terms,—an observation long since made by Mayhow, who remarked, that if air be drawn into the lungs, and the mouth and nostrils afterwards closed, " quamvis inflati maneant pulmones, mori tamen necesse erit, quia non licet expirare *. If indeed we reflect, that during submersion in water no fresh air can enter into the lungs, but that all which they contain may freely escape; and if we consider, that before suspension by the neck in the human subject, a deep inspiration, under the influence of fear, as Dr Goodwyn observes, is made, and that no air can afterwards pass out, if the cord completely close up the trachea; it is reasonable to expect, that this variation in the bulk of air contained in the lungs should obtain, under the very different circumstances in which respiration is brought to a stand.

91. How much the composition of the air itself, and the manner in which it is breathed, will vary the bulk of residual air in the lungs, we may collect from the experiments of various authors. Dr Hales moistened a bladder, and fixed to it a fosset, both of which would contain 74 cubic inches of air. Having blown up the bladder, he put the small end of the fosset into his mouth, and, at the same time, pinched his nostrils close, that no air might escape through them, and he then breathed to and fro the air contained in the bladder. In less than half a minute, he found a considerable difficulty of breathing, and was forced after that to draw his breath very

* Tractat. Quinque, p. 300.

fast: and at the end of the experiment, the suffocating uneasiness was so great as to oblige him to take away the bladder from his mouth. Towards the end of the minute, the bladder was become so flaccid that he could not blow it above half full, with the greatest expiration that he could make *. When also Mr Davy respired atmospheric air in a natural manner, he took in, he says, only 13 cubic inches and expelled 12.7, so that only about $\frac{1}{43}$ part of the original bulk was retained: when he made one respiration of 100 cubic inches of air, the diminution was to 99, or $\frac{1}{100}$: when, after a complete exhaustion of his lungs, he respired 141 cubic inches of air, once only for one-fourth of a minute, they were reduced to 139, or $\frac{1}{70}$ nearly: and when 161 cubic inches were breathed for about a minute, their bulk was diminished to 152, or $\frac{1}{18}$ †;—in every case, the diminution augmenting with the repetition of the respiration, and consequent impurity of the air, and distress of the respiratory organs. So likewise, when Dr Henderson breathed from and into the gasometer 600 cubic inches of air for four minutes, they were reduced to 570, or lost $\frac{1}{20}$ of their bulk; and he adds, that he held on respiring until the sense of oppression about the chest obliged him to desist ‡. These distressing symptoms, brought on by the repeated breathing of the same quantity

* Statical Essays, vol. i. p. 238.
† Researches, p. 431, 432, 433. 435.
‡ Nicholson's Journal, May 1804.

of air, were felt in a still greater degree by Mr Kite; for on respiring 591 cubic inches of atmospheric air from and into a bladder, he experienced, in one minute, great anxiety at the breast, which in half a minute more became intolerable: his face swelled, became black, and felt excessively hot, and sparks of fire danced before him: loss of sight, giddiness and confusion of the senses succeeded, and at the end of little more than two minutes, he fell back into a chair. He was relieved by fresh air, but remained confused and giddy *. The amount of the diminution of respired air, says Professor Pfaff, depends not only on the *time* during which a given volume of air is respired, but principally on the magnitude of the volume of air itself: it must be proportionally less the greater the quantity inspired. He breathed 144 cubic inches of air once only in the time of ten or twelve seconds, and the diminution was four cubic inches, or $\frac{1}{36}$ of the primitive volume: when he respired the same volume of air twice, during twenty seconds, it lost eight cubic inches, or $\frac{1}{18}$: and when it was thrice respired, during thirty seconds, the diminution amounted to twelve cubic inches, or $\frac{1}{12}$ of the primitive volume †. Now, in all these cases, the volume of air respired was precisely the same, and could not, therefore, affect the ratio of diminution: but as the times were doubled and tripled, so nearly were the degrees of diminution. But the more frequently the same air

* Essay on Apparent Death, p. 25.

† Nicholson's Journal, December 1805.

is breathed, the more unfit does it become for respiration: and to this change of composition, more than to the time, or the magnitude of the volume of air, is the increased degree of diminution to be ascribed.

92. This will perhaps appear more striking, if we attend to what happens in respiring nitrous oxide, which is composed of the same elements as atmospheric air, but contains a much larger proportion of oxygen. After exhausting his lungs, Mr Davy inspired 108 cubic inches of this gas, which, when expired, were reduced to 99, or had lost $\frac{1}{12}$ of their bulk. When he made two respirations of the same quantity of the oxide, the diminution was to 95, or about one-eighth: and when he respired 102 cubic inches of nitrous oxide, mixed with $\frac{1}{50}$ of common air, for half a minute, the volume of air, after the seventh expiration, was reduced to 62, or had suffered a loss equal to $\frac{1}{2.55}$ *. Hence it appears, that in the natural respiration of atmospheric air, only a small diminution (85.) of its bulk takes place: that this diminution increases as the air becomes vitiated (91.) by repeated respirations, or is breathed in a preternatural manner: and that when a gas of the same elementary materials, but combined in very different proportions, is substituted into the place of pure atmospheric air, the diminution increases in a tenfold degree. Now, the repeated breathing of the same atmospheric air, has been shewn to bring on

---

* Researches, p. 394. 416.

the most distressful symptoms, and at length an utter inability to continue respiration; and Mr Davy tells us, that, after a voluntary exhaustion of his lungs, he could respire the nitrous oxide with accuracy, when stooping, for about half a minute, but, even then, strong sensations were produced, with fulness about the head rather alarming: that if the respiration extended to three-fourths of a minute, he could not rely on the accuracy of any experiment; and that the determination of blood to the head became, in less than a minute, so great, as often to deprive him of voluntary power over the muscles of his mouth *. But respiration is a function carried on by the exertion of muscular powers, in a great degree obedient to the will (87.); and the quantity of residual air in the lungs in preternatural respiration, will, at all times, be much influenced by the manner in which the will exerts itself, and the degree in which the muscles are able to act. When, therefore, the power of the will over the muscles is in any degree diminished, or is wholly lost, or the muscles themselves are much weakened, a proportional derangement will take place in the respiratory function; and as, in the natural condition of the body, expiration is subsequent to inspiration, the ability to inspire will last longer than the ability to expire: consequently, the cessation of the process is brought about by a failure in the expiratory powers. But if the expiratory powers are unable to expel the air from the lungs, it must remain in

* Researches, p. 392.

those organs; and hence we see, in all the foregoing examples, that the diminution in the volume of expired air was greater in proportion as the respiratory organs suffered distress or oppression, and amounted even to more than one-third of the air inspired when all voluntary powers ceased.

93. By Mr Davy, however, and many others, the difference in bulk between the volume of nitrous oxide inspired, and that which is expired, is considered to arise in every case from a " rapid absorption of this elastic fluid by venal blood through the moist coats of the pulmonary veins *:" and he also thinks it " reasonable to suppose, that the whole compound atmospheric air, passing through the moist coats of the vessels, is first dissolved by the serum of the venal blood, and, in its condensed state, decomposed by the affinity of the red particles for its oxygen; the greater part of the nitrogen being liberated unaltered, but a minute portion of it possibly remaining condensed in the serum and coagulable lymph, and passing with them into the left chamber of the heart †." It happens, rather unfortunately for this opinion, that, in the natural respiration of atmospheric air, a very small difference exists between the inspired and expired volumes, though the powers of absorption, if such there be, must then be acting in their greatest vigour; while under an almost total exhaustion of muscular and vital power, this absorption is considered to take place in an extraordinary degree. But the subject

* Researches, p. 396. † Ibid. p. 447.

of the absorption of elastic fluids in the human lungs is of so much importance in itself, and has so much divided the opinions of physiologists, as to demand from us a more distinct and detailed discussion.

94. When then the air, received into our lungs, is said to obtain admission into the blood-vessels by a process of absorption, it may be proper, in the first place, to inquire into the structure of those organs, at least so far as to ascertain by what means such a process takes place. The lungs, one of which occupies each cavity of the chest, are composed principally of air-cells and blood-vessels, connected through their whole extent by intervening cellular membrane. The *trachea*, or windpipe, on its arrival in the chest, divides into a right and a left branch, which branches again subdivide into smaller ones called *bronchia*, and these into others still more minute, until at length they lose their cartilaginous texture, become membranous, and expand into a cellular structure, which fills at all times the cavity of the chest. "The cells composing this structure, are purely membranous, of an irregular figure, compressed and closely connected, and have a free communication with each other. Between the different lobes, lobules, and cells of the lungs, a large quantity of common cellular substance, destitute of fat, is interposed, which unites and strengthens them; but the cells have no communication with this substance; for, when air is blown into it, the lobules are compressed, but when the air is blown in through a branch of the *trachea*, the cells are again distended, and the lobules recover their former dimen-

sions *." The pulmonary artery, which conveys the venal blood from the right side of the heart, divides into two branches, which are dispersed through the substance of the lungs; and its smaller branches, running in the common cellular substance, become at length inconceivably minute, forming at last a *plexus*, or fine net-work, upon the proper cells: and they terminate afterwards, partly into exhalent vessels, and partly into corresponding branches of the pulmonary veins. These veins, by frequent anastomosis, diminishing in number and increasing in size, form at last four large trunks, which finally deliver the blood into the left side of the heart. From this description, it is manifest, that, between the air contained in the cells, and the blood flowing through the vessels, are interposed the coats both of the cells and vessels. When, therefore, air is said to enter into the blood from the cells of the lungs, it must, in some way, be conveyed through the coats of these cells and blood-vessels. After what manner, therefore, is it able to effect a passage?

95. Every anatomist will allow, that the surface of the cells of the lungs, like every other surface of the body, is duly furnished with absorbent vessels, of which not only the ordinary absorption of fluids carried on by this surface, but the frequent removals of morbid matter from the bronchial cells, supply abundant proof. Mr Cruickshank has frequently seen the absorbents of the lungs turgid with blood in cases of hæmoptoë, which blood they had ab-

* Fyfe's Compendium of Anatomy, vol. ii. p. 148.

sorbed from the air-cells instead of their transparent fluid *. Does the air also, which is supposed to pass out of the cells of the lungs into the blood-vessels by a process of *absorption*, take the route of these absorbent vessels? To this question we reply, in the language of Haller, that the fineness of those vessels, the mucus perpetually smearing the surface of the cells, the elastic nature of air itself, and its repulsion by water, so that it neither penetrates moist paper, cloth, nor skin,—all demonstrate that no air by this route gets into the blood †. If, indeed, air were taken in by the absorbents, it must, as Dr Goodwyn observes, take the route of those vessels, and, by passing directly to the right side of the heart, change the colour of the blood there; which, however, does not happen ‡: nor, when air was forced down the windpipe of a dog, in the experiments of Dr Hales, was it able to pass into the pulmonary artery or veins ||.

96. If, then, no proof exist of the passage of air into the blood by the ordinary course of the absorbent vessels, the only other mode of effecting this purpose that has been hitherto suggested, is the power of *chemical affinity*. What then are the chemical affinities subsisting between venal blood and atmospheric air? About the middle of the 17th century, Dr Lower observed, that the upper surface of

* On the Absorbents, p. 42.
† Prim. Lineæ, par. 306.
‡ Connexion of Life with Respiration, p. 62.
|| Statical Essays, vol. ii. p. 276.

venal blood, received into a vessel, acquired a scarlet colour by exposure to the air: that if this surface was removed, the subjacent one was soon changed to the same colour: that if the cake of blood, after being allowed to settle in the vessel, was inverted, its exterior and upper surface speedily also assumed a florid hue: and, lastly, that if venal blood was shaken in a vessel, so that the air thoroughly intermixed with it, it became entirely florid *. These opinions were afterwards held by Sig. Fracassati and Dr Slare, the latter of whom observes, that the blood thrown up by a rupture of the capillary vessels of the lungs, is frothy and of a scarlet colour; the first of which effects he attributes to the intermixture of air, and the latter to its tinging power †. Mr Hewson employed similar arguments to prove, that the florid colour, acquired by venal blood on exposure, was produced by the contact of the air: and, by injecting air into the jugular vein of a rabbit, he found that it there also rendered the blood florid ‡. M. Cigna not only confirmed the foregoing facts, but proved also that the change of colour in this fluid did not take place when the blood was covered with oil or placed *in vacuo*; and Dr Priestley ascertained, that not only by common air, but more especially by oxygen gas, this florid colour was produced on the black crassamentum of blood ||.

---

* Tract. de Corde, p. 178.—An. 1669.

† Lowthorpe's Abrid. Phil. Trans. vol. iii. p. 235.

‡ Hewson on the Blood, p. 9.

|| Priestley on Air, vol. iii. p. 66.

97. In effecting these remarkable alterations in the colour of the blood, the air itself, at the same time, suffers material changes. Dr Priestley found, that in twenty-four hours oxygen gas was so far depraved by being in contact with venal blood, that one measure of it and two of nitrous gas occupied the space of a measure and a half, whereas, at the beginning of the experiment, they occupied the space of no more than half a measure *. Dr Goodwyn confined venal blood under a jar of oxygen gas inverted in mercury, and repeatedly observed that the change of colour was always very sudden, and, after several minutes, the mercury ascended two or three lines; from which he concluded that a small portion of the air had disappeared †. The precise change, however, which the air underwent, seems first to have been observed by Dr Girtanner, who placed six ounces of venal blood in a jar of oxygen gas inverted in mercury: the blood presently assumed a florid colour: the air was somewhat diminished in bulk, and contained a portion of carbonic acid, which was attracted by lime-water ‡. Dr Bostock observes also, that a diminution of oxygen and production of carbonic acid take place when a piece of crassamentum is placed in a jar filled with oxygen gas ‖. The same production of carbonic acid we found to occur when blood is placed in contact

* Priestley on Air, vol. iii. p. 75.

† Goodwyn's Essay, p. 61.

‡ Memoirs on Irritability in Beddoes' Obs. on Calculus, &c. p. 219.

‖ On Respiration, p. 227.

with atmospheric air. A quantity of this fluid was received into a cup, and confined in a jar of air inverted in water, a glass of lime-water having been previously placed in the cup. The internal surface of the jar was soon bedewed with moisture, and a pellicle began to form on the lime-water, which in a few hours was increased to a thick crust of carbonate of lime. The crassamentum was then removed, and a fresh glass of lime-water was placed in the serum, which in thirty-six hours had acquired a crust like the former, and the water had risen considerably into the jar. In another experiment, where the serum was placed for twenty-four hours in a jar of air inverted in mercury, the residual air rendered lime-water milky, and the remainder had lost a part of its oxygen. A similar production of carbonic acid seems to have occurred, when, with a small diminution of the gas, a slight change of colour was produced on venal blood by placing it in contact with nitrous oxide, in the experiments of Mr Davy: for when a solution of strontian was admitted to the oxide, it became slightly clouded, and, with the diminution of bulk that followed, minute portions of carbonic acid and nitrogen gas were produced *. Hence then we learn, that when venal blood is exposed to the contact of atmospheric air, of oxygen gas, or of nitrous oxide, it presently assumes a florid colour, and, at the same time, the volume of air is somewhat diminished, and a portion of carbonic acid is produced.

---

* Researches, p. 377, 380, 387.

98. Does then the carbonic acid, which is here met with, proceed ready formed from the blood, or is it in part formed by the decomposition of the air? No one has yet proved that any aëriform fluid, much less that carbonic acid, exists naturally in the blood; and if this be true, no such aërial acid can be expected to issue from it. The carbonic acid also, is not formed by blood when it is confined in nitrogen gas; neither does the colour of the blood, in that case, undergo any sensible change: but this acid is formed by blood, either in oxygen gas, in nitrous oxide, or in atmospheric air, all of which are deteriorated thereby; whence it follows, that without the presence of oxygen gas, the blood is unable to form carbonic acid, and that this acid, therefore, is, in part, formed out of that gas. If the oxygen gas that disappears do not contribute to form the carbonic acid that is produced, in what other manner can its loss be accounted for? or from what other source than the oxygen gas of the air, in contact with the blood, can that ingredient of the acid be derived? Those who suppose the carbonic acid to be furnished by the blood, independent of the air employed, must likewise suppose that the nitrogen gas is furnished by it also; for the experiments of Mr Davy teach us, that a portion of that gas, as well as of carbonic acid, is always present when nitrous oxide is decomposed, which renders it probable that the same thing likewise occurs when air is changed by venal blood. But in what manner the blood should be able to furnish nitrogen gas, it is not easy to conceive, since no affinity exists between that gas and

venal blood *. We infer, therefore, from these facts, that atmospheric air is decomposed by being placed in contact with venal blood, its oxygenous portion being in part converted into carbonic acid, and a quantity of its nitrogen being, in consequence, left free.

99. But, supposing the air to be thus decomposed by the blood, it still remains a question, whether it has been first attracted by that fluid, then decomposed, and afterwards in part expelled ; or, whether the decomposition has been effected without such previous attraction and intermixture of air. The only evidence of this supposed attraction seems to be the small diminution of bulk which the air in all cases suffers ; but this cannot be considered as a proof of the attraction of the air ; for it is a necessary consequence of that conversion of oxygen gas into carbonic acid which has been shewn (11.) to take place when these substances are brought into contact. Even granting to the blood this power of attracting air, or its oxygenous portion, it is not easy to conceive why it should so readily lose it, and again give out this air in the form of carbonic acid. No change of quality in the blood, nor any variation of temperature, can have taken place sufficient to alter so rapidly its affinity for these substances : and it cannot proceed from a want of affinity between the blood and the carbonic acid that is formed ; for that acid suffers a greater diminution, either than oxygen gas or atmospheric air, by being placed in contact with

* Davy's Researches, p. 375.

blood. We incline, therefore, to the opinion, that neither the air nor its oxygen gas is attracted by, and diffused through the blood, as happens with several gases when placed in contact with certain fluids: but that the air is decomposed, and its oxygen gas changed into carbonic acid, without entering into the substance of that fluid.

100. But, for the formation of this acid, the blood must supply carbon, since no other substance was present from which it could be derived : and it is well known also, that carbon enters largely into the composition of that fluid ; and our experiments (97.) prove, that it exists as well in the serous as in the more solid parts. By some it may be objected, that because carbonic acid is formed directly by the combustion of charcoal, it cannot be produced at so low a temperature as exists in these experiments. To this we can reply only by an appeal to the general facts exhibited through the whole course of our inquiry, by which it appears, that both by the living functions of vegetables and animals, and by the decomposition of animal and vegetable matter, this acid is, in like manner, formed at temperatures equally low. Even those who consider this acid to have proceeded ready formed from the blood, cannot attribute its production to the operation of heat ; for in the animal body, the temperature of the blood seldom exceeds 100°—a degree of heat incompetent to form carbonic acid by any process analogous to combustion. The combination of many bodies is, indeed, greatly accelerated by being exposed to very high temperatures ; but this surely does not set aside the fact of their spontaneous union at temperatures

much more low. From this review of the effects which take place between the blood and air, we conclude, that the chemical phenomena which arise when these substances are placed in contact, do not prove an attraction and diffusion of air through the blood; but shew only that a reciprocal action takes place, by which a new product is formed: no inference, therefore, in favour of an attraction of air by the blood in the lungs, can be drawn from the reciprocal action which they exert on each other out of the body.

101. To the operation of chemical affinity also, a degree of absolute contact is required, which may, and does exist between air and venal blood out of the body; but the intervention of the coats of the cells and blood-vessels altogether forbids this necessary condition in the lungs. The supposition, that the coats of these vessels and cells are so thin, that, when moist, they allow the air, or its oxygen gas, to pervade them, is wholly gratuitous, and in opposition to the results (94.) of direct experiment: and the belief, that certain pores exist, through which elastic fluids may permeate, is equally unsupported by anatomical fact; for the terminations of the absorbent and exhalent vessels are the only orifices which are known to open on the surface of the bronchial cells. If, indeed, air did permeate the bronchial cells through any supposed pores, it would more readily pass into the cellular substance which connects them together than into the pulmonary vessels, and thus would create, at all times, an emphysematous state of those organs; but this is never known to be the case, for these cells are impermeable by air. It is, lastly, ex-

tremely difficult to conceive how the same air, which is so readily confined in membranous substances out of the body, should, with such perfect freedom, pass to and fro through a much more complicated structure within it. It has indeed been said, that, when any gas is confined in a bladder, it will permeate its coats and escape, while atmospheric air, passing at the same time through these coats, will supply its place. Allowing this to be the case, it will not be denied, that many days, or even weeks, are required to accomplish this operation : and it bears, therefore, no sort of analogy to that rapid attraction and expulsion of air which is supposed to go on through the cells and blood-vessels of the lungs. It should be remembered also, that the bladder, when removed from the body, soon loses its living properties, by which its power of resisting the passage of fluids may be diminished ; for we know that the bile, a much denser fluid than air, is during life perfectly retained within the gall bladder ; but a short time after death, its colouring matter often escapes, and gives to the surrounding viscera a yellow tinge. Neither can any thing, necessarily residing in the venal blood, be held sufficient to account for this supposed attraction of air ; for Girtanner found, that arterial blood produced the same changes *, and the like occur (97.) when serum only is employed ; and even if, by superior affinity, the blood did attract air through the coats of the cells and vessels of the lungs, in what way shall we account for its so ra-

---

* Memoirs on Irritability, p. 228. 231.

pidly losing this superiority, and again giving out nearly the whole of this air, through these same blood-vessels and cells ?

102. But if, either by the function of absorption, or by the operation of chemical affinity, air did enter into the blood, we may surely with reason demand some proof of its presence; yet, says Haller, "Nulla unquam in vivo calido animali bulla aëris in sanguine visa est *." This opinion is confirmed by the direct experiments of Dr D. Darwin; for having inclosed a portion of the jugular vein of a sheep between two ligatures, it was cut out, stripped of its adhering cellular membrane, and then thrown into a glass of water of temperature 100°, standing under the receiver of an air-pump. It at once sank to the bottom, and did not rise when the air was exhausted; nor, when afterwards taken out, wiped dry, and laid on the floor of the receiver, did it exhibit any swelling under the exhaustion of the vessel. The experiment was repeated with a similar result on a portion of the vena cava of a swine †.

103. Neither do the effects resulting from the admixture of aëriform fluids with the blood, favour the notion of the entrance of air into that fluid. "Animal, cui aër in sanguinem inflatur," says Haller, "perit certo et velociter; neque quidquam satis certi est in sanguinis venarum pulmonalium aucto rubore ‡." This assertion is confirmed likewise by

* Prim. Lin. par. 306.

† Philosophical Transactions, vol. lxiv. p. 345.

‡ Prim. Lineæ, loc. cit.

direct experiment. When Dr Girtanner injected oxygen gas into the jugular vein of a dog, he cried dreadfully, breathed quick, and died in three minutes: when nitrogen gas was thrown in, death happened in 20 seconds *. Air, says M. Bichat, thrown into the vascular system, quickly brings on agitation, convulsions, and death †. By forcing air through the windpipe into the lungs with a syringe, and confining it there, he has made it to enter into the blood-vessels, which immediately brings on agitation and exertion in the animal; and if an artery in the leg or foot be now opened, the blood will spring out frothy, and full of bubbles of air. If hydrogen gas has been used, the bubbles may be inflamed; and when this frothy blood has flowed 30 seconds, the actions of life cease, and cannot again be restored, even although fresh air be supplied ‡.

104. As the chief arguments in favour of the entrance of air into the blood, have been drawn from the experiments of Mr Davy in his excellent "Researches" into the nature and respiration of nitrous oxide, it may, perhaps, be required that we should notice the leading proofs which he adduces in favour of it. Mr D. had found that hydrogen gas, when inspired, only mingles with the air present in the lungs, and is again thrown out unaltered with a portion of the residual air ‖. The bulk of this residual air, reduced to the temperature of 55°, he es-

* Memoirs on Irritability, p. 221. 223.

† Recherches sur la Vie et la Mort, p. 179. ‡ Ibid. p. 303.

‖ Researches, p. 399.

timates at 32 cubic inches, being composed of nitrogen 23, carbonic acid 4.1, and oxygen gas 4.9 *. When he breathed the nitrous oxide, the proportions of these residual gases appeared nearly the same as when hydrogen gas was respired; which led him to conclude, that no portion of the nitrogenous or oxygenous gases was produced by the decomposition of the oxide in the lungs †. But a reference to the results of Mr Davy's experiments will shew, that the carbonic acid in the lungs and air-holder, which, before breathing the oxide, was estimated at 4.1, was afterwards increased to 5.2 cubic inches; and that the oxygen gas also was increased from 5.6 to 6.1. In another experiment, the carbonic acid was increased, after breathing the oxide, from 4.1 to 6.8, and the oxygen gas, at the same time, from 5.5 to 6.3; and Mr Davy admits, "that the quantity of carbonic acid and oxygen is rather greater than that which existed in the experiments on hydrogen ‡." Finding, indeed, the bulk of these two gases in the lungs and air-holder to be only 9 cubic inches when hydrogen was respired, and that it amounted to 13.1 when nitrous oxide was breathed, and, denying that the acid is formed by the immediate decomposition of the nitrous oxide itself, Mr Davy is led to believe, that "it is wholly or partially liberated from the venal blood through the moist coats of the vessels §;"—a supposition, against which it has, we trust, been already sufficiently argued.

---

* Researches, p. 409.
† Ibid. p. 414.
‡ Ibid. p. 413. 415.
§ Ibid. p. 420. 422.

105. Together with this increase of oxygen gas and carbonic acid, beyond what the residual air in the lungs would supply, there was a considerable increase also of nitrogen gas, which corresponds with what takes place when this oxide is exposed to blood out of the body; for, besides the carbonic acid produced, there are always present small portions of nitrogen gas *. Since, therefore, the same products are obtained when this oxide is breathed, as when it is exposed to blood out of the body, it may be inferred that they are effected by similar means. These means cannot, in the latter case, be absorption in the sense we apply that term to the living human body; because the blood speedily becomes an inert mass, bearing no analogy in its properties to the absorbent function in the lungs: and, even if chemical attraction were allowed to operate between this oxide and blood out of the body, this will not apply to the circumstances in which they are respectively present in the lungs; because the intervention of cells and blood-vessels wholly forbids that degree of actual contact, which is essential to chemical action. It is therefore only on the supposition of a *decomposition* taking place, that these contradictory results can be reconciled.

106. Against the idea of such decomposition in the lungs, Mr Davy urges, that " it is difficult to suppose how nitrous oxide, which requires the temperature of ignition for its decomposition by the most inflammable bodies, should be partially ab-

---

* Researches, p. 387.

sorbed, and partially decompounded at 98°, by a fluid possessed apparently of uniform attractions *." To this it may be replied, that the difficulty of decomposing a substance by some bodies, in certain circumstances, at a high temperature, is no proof that it cannot be decomposed, by other bodies, in other circumstances, at a low temperature. This oxide, Mr Davy believes, to be in some manner decomposed by the blood during its circulation with that fluid: but, does the temperature of the blood exceed that of the lungs? Even out of the body, and in the ordinary temperature of the atmosphere, the experiments of Mr Davy (97.) evince, that this oxide can be partially decomposed by venal blood.

107. According to the earliest and the latest experiments of M. Lavoisier on the respiration of atmospheric air, its nitrogen gas was considered to be in no respect affected by that function; in which conclusion Goodwyn, Menzies, and most other authors, have acquiesced: and we have seen that this gas is not affected by the growth of vegetables, (5. 29.), nor by the respiration (73.) of the inferior animals. Dr Priestley, however, at one time supposed, that the oxygen gas passing the membrane of the lungs, carried with it some part of the nitrogen with which it was previously combined; but, on the suggestion of Sir Charles Blagden, he afterwards thought it more probable that the deficiency of nitrogen gas was owing to the greater proportion of it existing in the lungs after the process than before †.

* Researches, p. 415.

† Experiments on Air abridged, vol. iii. p. 380.

Mr Davy, from the results of his experiments, has been led to revive this opinion concerning the entrance of nitrogen into the blood. In the respiration of atmospheric air, he calculates that 5.2 cubic inches of nitrogen disappear every minute *: and when nitrous oxide is breathed, he supposes that "an immense quantity of this substance is taken into the blood; and that the part of it not expended in new combinations, during living action, is liberated in the aëriform state by the exhalents, or through the moist coats of the veins †." But why the blood should so much more powerfully attract nitrogen from nitrous oxide than from atmospheric air, is enough of itself to beget doubts of the truth of Mr Davy's conclusion, more especially if we consider, that "nitrogen and oxygen exist, perhaps, in this oxide in the most intimate union which those substances are capable of assuming ‡." The only direct evidence in favour of this opinion, is the loss in bulk which nitrous oxide suffers in respiration. For this we have endeavoured to assign an adequate cause (91. et seq.); and a reference to Mr Davy's experiments (92.) will shew, that when he breathed this oxide once only, the loss in bulk was one-twelfth; when twice, the loss was about one-eighth; and when seven times, the loss amounted to more than one-third; thus regularly increasing with the repeated respiration of a noxious gas, and consequent debility of the expiratory powers, just as takes place in the repeated breathing of the same volume of atmospheric air.

---

* Researches, p. 434. † Ibid. p. 415. ‡ Ibid. p. 328.

108. Mr Davy, however, taking for granted that immense quantities of nitrogen gas enter into the blood, observes, that "this being true, the quantity of nitrogen produced in respiration, ought to be increased in proportion as a greater quantity of nitrous oxide enters into combination with the blood *." This he attempts to prove by experiment. Having exhausted his lungs, he inspired the oxide out of a silk bag, containing eight quarts of that gas, and thus made nine respirations. The gas of the first expiration was not preserved, but that of the second gave 29 of the oxide, and 17 parts nitrogen gas; the third was as 22 to 8; the fifth, as 27 to 6; the seventh, as 23 to 7; and the ninth was as 26 to 11. So far, therefore, from the nitrogen gas increasing in proportion to the supposed combination of the oxide with the blood, it observes nearly a decreasing ratio; and Mr Davy accordingly admits, that the "results of these experiments are not so conclusive as could be wished †." He nevertheless goes on to say, that "if any portion of nitrous oxide were decomposed immediately by the red particles of the blood, one should conjecture that the quantity of nitrogen produced, ought to be greater during the first inspirations, before these particles became fully combined with condensed oxygen ‡." And what is the fact from the experiments just related? Why, that in the second expiration, the nitrogen gas was 17 parts; in the third only 8; and in the fifth but 6; evidently showing that the greater

* Researches, p. 416. † Ibid. p. 418. ‡ Ibid. p. 419.

quantity of this gas did appear during the first inspirations; and not after the supposed combination of oxygen with the red particles of the blood.

109. Mr Davy has breathed the nitrous oxide, in a state of purity, for four minutes and a half, and some have respired it five minutes; and he states the proportion of oxide absorbed to be as great in the last, as in the first inspirations, the consumption, by the same individual, being nearly in the ratio of the time of respiration *. He thinks about a pint, or 30 cubic inches, to be the ordinary range of consumption in different individuals, which, he says, is not far from two cubic inches, or about one grain every second; or in one minute 120 cubic inches, or 60 grains †. This quantity amounts, in five minutes, to 600 cubic inches, or 300 grains; and in one hour, to 7200 cubic inches, or 3600 grains; equal to more than 31 gallons measure, or seven ounces and a half troy weight, if it were possible so long to continue respiring, and the consumption were nearly in the ratio of the time of respiration. But taking the proportion only for five minutes, and allowing all that Mr Davy would deduct as liberated again from the blood in the same space of time, it would amount to a bulk and weight altogether inconsistent with what we know to be the state and condition of the sanguiferous system; and the inference, if followed out, might therefore, in the form of a *reductio ad absurdum*, be employed against Mr Davy's opinion.

---

* Researches, p. 425. † Ibid. p. 427.

110. The idea, that "the absorption of nitrous oxide by venal blood, is owing to a simple solution of the gas in that fluid, analogous to its solution in water or alkohol *, will by no means obviate the objection to such immense quantities of it entering into the blood; for it appears, that but a very small diminution of bulk takes place (96.) when this oxide is placed in actual contact with venal blood; and when again it "is carried through the pulmonary veins and left chamber of the heart to the arteries," and made to undergo decomposition, in order that "its oxygen may be chiefly expended in living action," and its "nitrogen be partially consumed in new combinations †, will not then the oxygen gas be in contact with a fluid, over which, out of the body, it does not "perceptibly diminish ‡," and the nitrogen gas exist in one over which it "possesses no power of action, and with which it is incapable of combining ‖?" The effects resulting from such admixtures of aëriform fluids with the blood in the living body, have already (103.) been distinctly noticed. If it be said, that this gas and atmospheric air are received into the blood in a peculiarly modified state as to their gaseous form, then they cannot be supposed to pass entire (93.) through the moist coats of the cells and vessels, but must suffer a previous decomposition in the lungs. If, on the other hand, they do pass entire into that fluid, and if, from its incapacity of combining with the blood,

* Researches, p. 378.
† Ibid. p. 419.
‡ Ibid. p. 381.
‖ Ibid. p. 375.

"the greater part of the nitrogen gas (93.) be again liberated unaltered," why cannot we discover some traces (102.) of its existence in that fluid? or why should its presence be so rapidly fatal, as experiment (103.) proves it to be?

111. But the supposed stimulant effects of this oxide have been assigned as an argument for its being in some way contained in the blood. In many cases, however, these effects do not follow the inhalation of that gas: and, in other instances, common air, breathed under the impression of its being this oxide, has produced them. Similar effects likewise are attributed by some authors to the inhalation of carbonic acid; and Dr Percival quotes M. Beaumé as relating the history of a man who was recovered from apparent death, produced by exposure to the foul air of a cellar, who asserted that he had felt neither pain nor oppression, but that, at the point of time when he was losing his senses, he experienced a delightful kind of delirium. This account, adds Dr Percival, receives confirmation from the testimony of Dr Heberden, who says, that he had seen an instance in which the fumes of charcoal brought on the same delirium as intoxicating vegetables produce. The Abbé Fontana likewise breathed a certain portion of hydrogen gas, not only without inconvenience, but with unusual pleasure: he had a facility in dilating the breast, and never felt an equally agreeable sensation, even when he inhaled the purest oxygen gas *. From these facts, it ap-

* Manchester Memoirs, vol. ii. p. 491, 492.

pears, that the inhalation of any gas, unfit to carry on respiration, will, in some persons, produce effects precisely similar to those which have been ascribed to an absorption of nitrous oxide ; and, on the other hand, that no such stimulant effects are produced in other persons by the inhalation and supposed absorption of that gas. In no respect, therefore, can we trust to the effects, which succeed to the respiration of this oxide, as affording any decisive proof of its absorption and commixture with the blood.

112. It is, as we have seen, the opinion of Mr Davy, and of some other authors, that air passes entire into the blood-vessels, and is decomposed during the circulation of that fluid. Now, the gases expired are, and must be admitted to be, formed out of those previously inspired, and, in natural breathing, the air suffers but a small diminution (85.) in bulk ; and the time of an ordinary inspiration occupies from $\frac{1}{15}$ to $\frac{1}{25}$ part of a minute. To suppose the inspired air to enter through the moist coats of the cells and blood-vessels, suffer decomposition in the blood, and be again returned through these vessels and cells, with so small a difference in bulk, and in so short a time, seems not only without proof, but against all probability, especially if we consider the extreme minuteness of the pulmonary vessels, and the great rapidity with which the blood is transmitted through them. If indeed, the reciprocal changes which take place between the blood and the air were effected during the circulation of that fluid, and it were only after that period the expired airs could be rendered, we ought not to expect them for nearly three minutes ; for suppose one ounce and a

half of blood to be propelled at each contraction of the heart, and each contraction to occupy $\frac{1}{72}$ of a minute, and the whole circulating mass to be about 324 ounces, then a complete circulation is performed in the time of 216 pulsations, that is, in three minutes. The almost instantaneous return of the inspired airs, however, is totally adverse to this fact: and if this be true of one respiration, it must be true of all, since the power acting, and the substances acted upon, preserve always the same relative state and circumstances, and the actions are performed not in time only, but in succession also; and thus, as one cannot begin until the other is ended, each is only a repetition of the same event, which, being operated by like powers, and in like circumstances, must afford invariably a like result.

113. If then there be no proof that air, either by absorption, or by chemical affinity, enters into the blood-vessels of the lungs, the sensible effects which it is there known to produce on the blood, must, in some other way, be accounted for. The celebrated Lower, not only first remarked the scarlet colour which black blood acquired by exposure to the air, but he first proved likewise, that the same change was produced by the air received into the lungs. In combating the opinion of Willis, that the florid colour of the blood was derived from an innate fire kept up in the heart, he asserted, that, if this were the case, it should, from their similarity of structure, be effected by the right, as well as by the left, side of the heart, whereas the blood sent into the lungs from the right heart was dark and venal. He passed a ligature also round the *trachea* of an animal, so

as effectually to exclude the air, and then opening an artery in the neck, he found that the blood which issued from it was black: and he farther remarked, that in a strangulated animal, where the entrance of air was precluded, the blood in the left side of the heart was like to venal blood; but if, before it coagulated, it was forced through the lungs, and air was at the same time blown into those organs, it again became florid, after the death of the animal, in the same manner as when he was alive*. Mr Hunter, by means of a double bellows, was enabled by one action to exhaust the lungs of their air, and by another to pass, at the same time, fresh air into those organs, without mixing the two together; and having introduced the nozzle of his bellows into the *trachea* of a dog, whose heart and lungs had been previously brought into view, he found, that, while he continued the artificial breathing, the heart continued to act as before, but more frequently: if, however, he stopped the motion of the bellows, the contractions of the heart gradually became weaker, and at length ceased. On recurring to the artificial breathing, the heart again began to move, and became at length as strong as at first. This process he repeated ten times, stopping from five to ten minutes between each; and he observed, that every time he left off working the bellows, the heart became extremely turgid with blood, and that the blood in the left side was as dark as that in the right, which was not the case when the bellows were

* Tractat. de Corde, p. 184.

working *. Dr Goodwyn repeated these experiments, both with common air and with oxygen gas, on dogs, and on lizards and toads, whose lungs consist only of a transparent bladder; and he observed the same changes of colour, according as the lungs were supplied with or deprived of fresh air †. This suddenness of effect seems wholly inconsistent with the supposed entrance of air into the blood, and its subsequent decomposition during the circulation of that fluid; and favours the probability of its being actually decomposed in the lungs.

114. Not only, however, in the lungs, but in the bronchial cells of those organs does this decomposition appear to happen, of which the vast extent of their surface is a strong presumptive proof. Dr Hales estimates the capacity of each cell, in the lungs of a calf, at the $\frac{1}{100}$ part of an inch diameter, and the surface of all the cells together at 40,000 square inches, to which, if 1635 square inches be added as the estimated surface of the bronchia, the sum of the surface of the whole lungs in that animal will be 41635 square inches, equal to nineteen times the surface of a man's body, which is computed at fifteen square feet ‡. Dr Keil estimates the diameter of each cell in the human lungs at $\frac{1}{50}$ part of an inch, and its superficies at 0.01256: the whole number of cells he calculates at 17,441,860, which, multiplied by the superficies of each, makes the sum

* Observations on the Animal Œconomy, 2d edit. p. 134.

† Goodwyn's Essay, p. 58. 62.

‡ Statical Essays, vol. i. p. 241.

of the superficies of all the cells, independent of that of the bronchia, equal to 21907 square inches, or more than ten times greater than that of the whole body *. The irregularity in the shape and size of the cells, and the want of adequate means correctly to determine their number, render an accurate estimate of their entire surface extremely difficult, if not altogether impossible: it is sufficient for our purpose to have shewn, that they exceed, in extent, by many times, the surface of the human body. The ramifications of the pulmonary artery and veins, running over these cells, must be minute and extensive, in proportion: and "by this admirable contrivance," says Dr Hales, "the blood is spread out into a vast expanse, commensurate to a very large surface of air, from which it is divided by thin partitions, so very thin as thereby to bring the blood and air within the sphere of each other's attraction †." Such a structure, how favourable soever it may be to an extensive contact of surfaces, is but little fitted to promote a mixture of substances by absorption.

SECTION II.

115. In the former section, we have endeavoured to shew the necessity of atmospheric air to the continuance of living action in the superior animals,

---

* Tentam. Med. Phys. p. 80.

† Statical Essays, vol. i. p. 247.

and have noticed the diminution of bulk which it suffers in respiration: we have attempted to ascertain the volume of air ordinarily inspired by man, and to appreciate the relative proportions which exist in the lungs in the different states of those organs: we have laboured to prove, that the actual bulk of air in the lungs must, and does vary, according to the various conditions of the system, to the circumstances in which respiration is carried on, the composition of the air itself, and the mode in which it is breathed; deducing from the whole, that the bulk of residual air in the lungs, at any period of life or of death, must be calculated always with referefice to these considerations. We have spoken generally against the entrance of any part of the inspired air into the blood-vessels of the lungs, by the function of *absorption*, as unsupported by anatomical fact; and have held its supposed *attraction* by the blood through the coats of those vessels as inconsistent with the acknowledged laws of chemical affinity: we have denied that any proof exists of air being, at any time, present in healthy blood, and have demonstrated its fatal effects whenever it gains admission into that fluid: we have examined the experiments and arguments of Mr Davy, in favour of an absorption of aëriform fluids by the blood, and have found them wholly insufficient, and leading to conclusions which are completely at variance with the established laws of the animal system. Lastly, we have adduced arguments against the possibility of the inspired air being decomposed during the circulation of the blood; and have assigned reasons in proof of its actual decomposition in the lungs, and even in the bronchial cells of those organs. This leads us, therefore, in the next place, to speak of the nature and extent of this decomposition; in other words, of the changes in quality which the air of our atmosphere experiences in the lungs during its respiration.

116. It has been already shewn (96. et seq.), that when the dark crassamentum of venal blood is placed in contact with atmospheric air, or with oxygen gas, it speedily assumes a florid colour: that the bulk of air is, in a small degree, diminished: that a portion of the oxygen disappears, and a quantity of carbonic acid is produced: and these effects have all been considered to arise from the combination of the oxygenous portion of the air with carbon supplied by the blood. In the living body, the venal blood experiences the same change of colour (113.) when atmospheric air, or oxygen gas, is received into the lungs: the air itself also is somewhat diminished in bulk: a portion of its oxygen gas, as we shall hereafter see, disappears: and that carbonic acid is, in like manner, produced, was first proved by the experiments of Dr Black. " So early as the year 1757," says this distinguished philosopher, " I convinced myself that the change produced on wholesome air by breathing it, consists chiefly, if not solely, in the *conversion of part of it into fixed air:* for I found, that by blowing through a pipe into lime-water, or a solution of caustic alkali, the lime was precipitated, and the alkali was rendered mild *." At a later period, Mr Bewley detect-

* Black's Lectures by Robison, vol. ii. p. 87.

ed the formation of carbonic acid in respiration by a method somewhat similar: he found, that on breathing through an infusion of litmus, the same change to a red colour was produced in it, as when it was exposed to the action of fixed air; and when, by adding a few drops of the water of potassa, the blue colour was restored to the infusion, it could again be made to dissappear by supersaturating it with the acid expired from the lungs *.

117. The particular substance which constituted the wholesome part of atmospheric air, was not, however, known to Dr Black at the time his experiments were made: and long before the compound nature of the atmosphere was ascertained, it had been supposed by many philosophers, that, to use the language of Bishop Berkeley, " there was no such thing as a pure simple element of air. There is," he adds, " some one quality or ingredient in the air on which life more immediately and principally depends. What that is, though men are not agreed, yet it is agreed it must be the same thing that supports the vital and the common flame; it being found, that when air, by often breathing in it, is become unfit for the one, it will no longer serve for the other. This quality of the air is necessary both to vegetables and animals, whether terrestrial or aquatic; neither beasts, insects, birds, nor fishes, being able to subsist without air: and when air is deprived of this ingredient, it becometh unfit to maintain either life or flame, even

* Priestley on Air, vol. v. p. 383.

though it should retain its elasticity *." Dr Hooke asserted, that this ingredient or substance inherent in, and mixed with the air, is like, if not the very same with that which is fixed in saltpetre, by which, during combustion, inflammable bodies are dissolved †. The same opinion was afterwards held by Willis, Lo er, and Mayhow, all of whom likewise considered the nitrous quality of the air to act an important part in respiration. The last author, in particular, made experiments precisely similar to those which have lately been brought forward, to prove, that both by the burning of a candle and other combustible bodies, and by the respiration of animals, the nitro-aërial particles of the air were exhausted, whereby the volume of air was diminished, and the residual air was unable afterwards to support either life or flame ‡. The exhibition, however, of this peculiar, or nitro-aërial, part of the air in a distinct and separate form, we owe to the genius of Dr Priestley, who, by the discovery of pure or dephlogisticated air, in the month of August 1774, first made us acquainted with the true composition of the atmosphere. This great discovery, and the use which he made of it, enabled this celebrated philosopher to propose the first consistent explanation of the phenomena of respiration that had ever been offered to the public; and, although the theoretical opinions on which that explanation

* Siris, par. 143. et seq.—2d Edition.

† Micrographia, p. 103.—An. 1665.

‡ Tractat. Quinque, p. 98. et seq.

was partly founded, no longer exist, yet it should never be forgotten that his experiments and discoveries first pointed out the true path of investigation; and have contributed, in a pre-eminent degree, to advance our knowledge of this most important function. The cause of the unfitness of air, beyond a certain extent, to support life and flame, he proved to arise from the destruction of its pure part, or what has since been called its oxygen gas; and he concluded, that, by the several processes of respiration, combustion, and calcination, it underwent precisely the same changes *. M. Lavoisier, in the following year, investigated these changes with greater accuracy. He confirmed the fact of the disappearance of oxygen gas during respiration; and added, that the residual air of this process differed from that left after the calcination of metals in containing a quantity of carbonic acid †; thus verifying also the fact discovered more than twenty years before by Dr Black.

118. Whence then is this carbonic acid, which is formed in respiration, derived? In the several processes of germination, of vegetation, and of respiration in the inferior classes of animals, it has been shewn, that, as carbonic acid is formed, the oxygenous portion of the air gradually and completely disappears; and that the acid produced bears always a constant proportion to the oxygen gas lost. We have now to shew, that

---

* Philosophical Transactions, 1776.

† Mem. Acad. 1777.

the same things take place in the respiration of the superior animals, and of man. Mr Davy confined a small mouse in a jar, inverted over mercury, and containing fifteen cubic inches of atmospheric air, previously deprived of its carbonic acid by long exposure to the water of potassa. In 55 minutes, the animal was taken out apparently dying, and of the quantity of oxygen gas originally present in the jar, 2.6 cubic inches had disappeared, and two of carbonic acid were produced. In another experiment, where a mouse was confined in 15.5 cubic inches of the same air, 2.7 of its oxygen gas, in nearly the same time, had disappeared; and 2.1 of carbonic acid were formed *. Mr Davy himself breathed 141 cubic inches of air one-fourth of a minute, making one inspiration and one expiration, seven or eight different times: and he found, by analysing the respired air, that the quantity of oxygen gas lost in each respiration, was from five to six cubic inches, and that of carbonic acid produced from five to 5.5 cubic inches †. Dr Henderson, on examining 100 parts of the air which he had respired four minutes, found, in one experiment, that of the $\frac{22}{100}$ of oxygen gas originally present, $\frac{8}{100}$ had disappeared, and $\frac{7}{100}$ of carbonic acid were produced: in another, $\frac{10}{100}$ of the oxygen gas were lost, and its place was supplied by $\frac{8}{100}$ of carbonic acid: and when, in a third experiment, the loss of oxygen gas fell between the foregoing numbers, or was $\frac{9}{100}$, so

* Researches, p. 437. 443.

† Ibid. p. 431.

likewise did the production of carbonic acid, being in that case $\frac{7.5}{100}$ *. This reciprocal increase and diminution of these two gases could spring only out of some necessary connexion betwixt them: and the variations occurring so regularly, and in such small quantities, are inconsistent with the supposition that the oxygen gas, which forms the carbonic acid of respiration, is derived from any other source than that of the inspired air. If there were any such source, why should not this acid be expired, in a quantity beyond what the residual air of the lungs will supply (104.), when hydrogen gas is breathed; or why should it appear only when air containing oxygen gas is respired, and in proportion always to the disappearance of that gas? Why also should this acid be produced, and life for a time be sustained, when animals are confined in pure oxygen gas: and why should death speedily take place and no acid be formed (beyond what the residual air of the lungs will supply), when they are placed wholly in nitrogen gas? These facts decisively shew, that the oxygen gas, which composes, in part, the carbonic acid formed in respiration, is derived from the inspired air alone: and that the production of this acid bears always a constant proportion to the loss of oxygen gas.

119. But, to constitute this acid, carbon, its other ingredient, must be supplied, which, in the inferior animals, has been shewn (76.) necessarily to proceed from the animal system. In the forego-

* Nicholson's Journal, May 1804.

ing experiments, also, where carbonic acid was formed when the animals were confined in jars of pure atmospheric air inverted over mercury, the animals must have furnished the carbon, since no other substance was present which could afford it. This carbon is likewise given out by the blood of animals (100.) after it is withdrawn from the body, in a state capable of uniting with oxygen gas: and, it is generally admitted, that the animal fluids possess the same power of affording carbon, while retained in the living system; to the union of which substance, with the oxygen gas of the inspired air, the formation of the carbonic acid of respiration has been ascribed.

120. Concerning the manner, however, and the place in which the union of these two substances is brought about in the living body, opinions have greatly varied. By some, the carbon of the venal blood has been supposed to attract the oxygen gas of the air through the coats of the pulmonary cells and vessels, and, by uniting with a portion of it, to form the carbonic acid, which is again returned immediately through the coats of these same vessels and cells. According to others, the oxygen gas enters into the blood-vessels, where it loosely combines with the blood in the capillaries of the lungs, and performs a circulation with it: during this circulation, a part of the oxygen unites with the carbon of the blood, so as to form an oxide of that substance, which, on the return of this fluid to the lungs in a venal state, is, by the acquisition of more oxygen, transformed into carbonic acid, and afterwards expelled through the coats of these same capillary vessels. Others, again, imagine the air to enter in-

to the blood-vessels in its entire state, and to be dissolved and afterwards suffer decomposition during the circulation of that fluid : while these changes are going on, a part of the oxygen combines with the carbon of the blood, and forms carbonic acid, which is liberated through the moist coats of the cells and vessels, when the blood returns to the lungs. In all these hypotheses, it is taken for granted, that the carbonic acid is in some way formed by the union of the carbon of the blood with the oxygen gas of the inspired air ; and the chief difference arises from this union being held by some to take place only in the capillaries of the lungs, and by others, through the whole course of the vascular system. But if, as we contend, no part of the inspired air gains admission into the blood-vessels of the lungs, neither of these opinions can be any longer maintained ; for no oxygen gas can, on such grounds, be held to enter into the blood to unite with its carbon, neither could the acid, which it is supposed to form, be afterwards expelled from that fluid.

121. The objection stated above, applies, however, in part only to the opinion of Mr Abernethy, who considers the carbonic acid of respiration to be derived, not from the oxygenous part of the inspired air, but to be simply exhaled from the pulmonary vessels *. As this opinion rests on the belief that the quantity of oxygen gas that disappears in respiration is not sufficient to account for the bulk of carbonic acid produced, it can no longer be maintained,

---

* Essays, Surgical, &c. p. 146.

if it be shewn that the volume of that gas which is lost, actually exceeds that of the carbonic acid which is formed. Now, this has been amply done in the experiments (118.) already given: and, indeed, the excess of oxygen lost, was so apparent and so constant, as to lead Lavoisier, and others after him, to conclude, that it was employed to form a part of the water expelled from the lungs, by uniting with hydrogen supposed to reside in the blood, just as water and carbonic acid are formed by the combustion of wax and many other substances. It is, moreover, incumbent on those who hold this opinion concerning the production of carbonic acid, to find out some adequate source from whence the oxygen gas, forming the expired carbonic acid, can be derived; to explain to us, at the same time, what becomes of the inspired oxygen gas which actually disappears; and why the acid produced bears always so constant a proportion to the disappearance of that gas.

122. Taking for granted, then, that the carbonic acid expired, is formed by the union of the inspired oxygen gas with carbon furnished by the animal system, and that the gas lost exceeds in volume the quantity of acid produced, we proceed next to investigate the amount of this difference and its cause. From the experiments of Doctors Priestley and Crawford (83.), it appears, that when animals are confined in air inverted over mercury till they die, little or no diminution of the bulk of gas takes place: those of M. Lavoisier, however, furnish a different result. He confined a guinea pig in a jar, containing 248 cubic inches of gas, consisting prin-

cipally of oxygen, which was inverted over mercury: in an hour and a quarter, the animal breathed with much difficulty, and, being removed, the air was examined, and found to be diminished in bulk by eight cubic inches: 40 cubic inches more were attracted by the water of potassa, and consequently consisted of carbonic acid; from which it appears, that the volume of acid produced, was one-sixth less than that of oxygen gas which had disappeared *. In a second experiment, another animal of the same species was confined an hour and a half in a cubic foot, or 1728 cubic inches, of oxygen gas, inverted over mercury: the gas suffered a diminution of 55 cubic inches, and 229.5 cubic inches more were attracted by the water of potassa, leaving a residue of pure oxygen †: hence, therefore, the whole loss which the oxygen gas experienced, amounted to 284.5, while the quantity of acid produced was only 229.5, or was $\frac{1}{5.17}$ less in volume than the oxygen which had disappeared. In these experiments, however, the air breathed was not natural to the lungs, and the confinement of the animal was prolonged until he breathed with difficulty, which, on the principles already stated (92.), would necessarily increase the volume of residual air in the lungs, and render the apparent diminution greater than it ought to be. In the experiments made by Mr Davy on his own respiration, the oxygen gas that disappears every minute, is estimated at 31.6 cubic inches, and the

* Mem. Acad. 1780.

† Ann. de Chimie, t. v. p. 261. et seq.

carbonic acid produced at 26.6,—a difference in volume equal to $\frac{1}{6.32}$ *: and Dr Bostock concludes, that in 24 hours, a man consumes somewhat more than 26 cubic feet of oxygen gas, and produces about 22 cubic feet of carbonic acid †, so that the bulk of the acid falls short of that of the oxygen gas by $\frac{1}{6.5}$ of the whole. It follows from all the foregoing facts, that the volume of carbonic acid, produced by the respiration of animals, is, in all cases, from one-fifth to $\frac{1}{6.5}$ less than that of the oxygen gas which has disappeared during that process.

123. From what cause then does this loss of bulk proceed? It cannot arise from an absorption or attraction of the oxygen gas, and subsequent combination of it with the blood: for both of these circumstances are forbidden by the actual structure of the lungs. But we have seen, that, by the conversion of oxygen gas into carbonic acid, in the process of combustion, a diminution of its bulk (11.), amounting nearly to one-seventh, takes place: and we have traced this diminution, in a less degree, when the same change is produced on oxygen gas by the growth of seeds and of plants, by the respiration of the inferior animals, and also by its being placed in contact with blood. It is true, that the degree of this diminution varies, in these several examples, from $\frac{1}{5.17}$ to $\frac{1}{10}$ of the volume of oxygen

* Researches, p. 434.

† Essay on Respiration, p. 99.

gas employed, but this variation does not destroy our belief in the general fact: and if we consider that these calculations have been formed from experiments where the authors had no such circumstance in their contemplation; that the correct observation of the fact itself requires an attention to many circumstances of much delicacy and difficulty; and that the composition of carbonic acid is not yet determined with such rigour as to leave no room for doubt, we must be satisfied with such an approximation towards the truth, as our present knowledge entitles us to make. In conformity, therefore, with these views, we venture to conclude, that oxygen gas, by its conversion into carbonic acid, suffers in respiration, as well as in combustion, a diminution of about one-seventh of its bulk: and since this necessary diminution accounts nearly (122.) for the whole difference in volume between the oxygen gas lost, and the carbonic acid produced in respiration, we may farther conclude, that the whole of this oxygen gas has been employed to form the carbonic acid in question.

124. But, notwithstanding this diminution of volume, which the oxygen gas thus experiences by uniting with carbon, the weight of the compound that is formed, is, at the same time, increased. MM. Lavoisier and Seguin estimated the weight of oxygen gas, consumed by a man in 24 hours, at 15661.66 grains: and that of carbonic acid produced in the same space of time, at 17720.89 grains *:

* Bostock on Respiration, p. 83. 86.

so that the weight of the carbonic acid exceeded that of the oxygen gas by 2059.23 grains. This excess must be attributed to the addition of carbon (119.) derived from the animal system; and which makes it to constitute $\frac{1}{8.6}$ of the weight of the acid that is formed. In the experiments of Mr Davy (122.), the volume of oxygen gas that disappeared every minute, was 31.6 cubic inches, and that of carbonic acid produced 26.6 inches. But one cubic inch of oxygen gas weighs 0.3474 of a grain, and therefore 31.6 cubic inches will weigh 10.97784 grains: and again, one cubic inch of carbonic acid weighs 0.467 of a grain, and therefore 26.6 inches will weigh 12.4222 grains: so that the weight of the carbonic acid exceeds that of the oxygen gas by 1.4444 grains, or the carbon, as before, constitutes $\frac{1}{8.6}$ of the compound. From these facts, it would seem, that the proportion of carbon, which composes the carbonic acid of respiration, is much less than that which forms the same acid in combustion, which, as we before remarked (13.), was estimated by Guyton at one-fifth. But the whole subject is, at present, surrounded with so many difficulties, that nothing more can be expected than an approximation to the truth; and we must content ourselves, therefore, with stating generally, that, although the carbonic acid formed in respiration be less in bulk than the oxygen gas which disappears, yet that it much exceeds it in weight; and that this excess is derived from the carbon supplied by the animal system.

125. As the conversion of oxygen gas into carbonic acid is at all times going on in the lungs, du-

ring the continuance of the respiratory process, it is next in order to examine how far the necessary diminution of bulk (123.) which attends that conversion, will go to explain the absolute loss which the air, during its respiration, suffers. The amount of this loss has been already stated to have been very variously estimated : and the cause of this variation in experiments made on animals (83.) confined in jars of air, has been explained. It is our present object to inquire into that degree of diminution which takes place in air that has only once passed through the lungs, and where all the circumstances of the experiment resemble those which occur in the ordinary process of respiration. Dr Goodwyn states, that the volume of air, taken into the lungs at a single inspiration, loses $\frac{1}{50}$, or sometimes only $\frac{1}{60}$ of its bulk, when expelled from those organs by the next succeeding expiration *. Dr Menzies, taking the average amount of the loss which the air suffered by 56 successive respirations, observes, that the volumes of air, received into and expelled from the lungs, were nearly the same †. Several attempts were made by Mr Davy to estimate the degree of diminution experienced by the air in a single respiration ; and it has been shewn that his determinations greatly vary (91.) according to the manner in which the experiment was made. In the only instance where all the circumstances were perfectly natural, the 13 cubic inches of air which he inspired, lost 0.3 of a

* Connexion of Life with Respiration, p. 51.

† Essay on Respiration, p. 22.

cubic inch, or $\frac{1}{43}$ part of their bulk *. Dr Bostock, on the other hand, concludes, that the air, by a single respiration, loses only $\frac{1}{80}$ part of its bulk †. Amid such contradictory results, it is not to be expected that a conclusion can be drawn which shall truly express the amount of the diminution in question: and indeed, from a consideration of the powers which govern respiration, and the various circumstances which sensibly affect that process, we cannot but consider the actual loss of bulk which the air suffers by a single respiration, as in its nature extremely difficult, if not impossible, to determine. All, therefore, that we can at present venture to assert, is, that the difference between the volumes of air inspired and expired, in natural respiration, is extremely small: and to this we may add our belief, that the necessary loss of bulk which oxygen gas suffers, by its conversion into carbonic acid, in that process, may be made sufficient to account for it.

126. Several attempts have been made to ascertain the absolute quantity of oxygen gas consumed in a given time by respiration: but many circumstances concur to render this a matter of very difficult determination. Dr Menzies, from his experiments, was led to conclude, that, in respiration, 36 cubic inches of oxygen gas were, every minute, converted into carbonic acid; and that thus 51840 cubic inches of that gas, equal to 17625.6 grains, were

* Researches, p. 433.

† Essay on Respiration, p. 99—210.

daily consumed. MM. Lavoisier and Seguin estimated the mean consumption of oxygen gas, every 24 hours, at 46037.38 cubic inches, or 15661.66 grains: and the same illustrious philosopher, on repeating his experiments in the following year, obtained nearly the same results, an account of which he was employed in drawing up, when cut off by the murderous tyranny of the French government. Upon the estimate of Mr Davy, that 31.6 cubic inches of oxygen gas are consumed every minute, the amount, in 24 hours, will be 45504 inches, a quantity equal to 15471.36 grains. This estimate, says Dr Bostock, coincides nearly with that of Lavoisier, though it was obtained by a different process, and by the use of a different apparatus: we may therefore conclude, he adds, that between 45 and 46,000 cubic inches, or about 15500 grains, 2 lbs. 8 oz. troy, is the average quantity of oxygen consumed in 24 hours by the respiration of an ordinary man *.

127. Notwithstanding, however, the necessity of oxygen gas to the continuance of respiration, and the great quantity of it that is thus daily consumed, many facts tend to prove, that, by the very constitution of that function, a necessary limit is placed to its consumption: and that this limit is determined, not by the purity of the air employed, but by some circumstances inherent in the animal system. It has been already observed (14. 40.), that the growth of vegetables is retarded by a great superabundance of

* Essay on Respiration, p. 84.

oxygen; and that, although insects will live a considerable time in this gas, yet their breathing becomes oppressive, and they die (53.) long before the whole of it is consumed. There can be little doubt but that the other classes of inferior animals would, under the same circumstances, suffer in the same manner. In the experiment also made by Lavoisier on the guinea pig (122.), the animal is said to have breathed with much difficulty, although not more than one-fifth of the oxygen gas was consumed: but some experiments of the same author, at a later period, seem in opposition to this fact. In comparing together the phenomena of combustion and respiration, he observes, that much more combustible matter is consumed in a given time in vital air, than in that of the atmosphere, but that the same circumstance does not hold in respiration: for whether animals respire oxygen gas in its pure state, or mixed with a proportion, more or less considerable, of nitrogen gas, the quantity of oxygen which they consume is always the same. If a guinea pig, he adds, be kept for several days in oxygen gas, or in a mixture composed of fifteen parts nitrogen and one of oxygen, preserving constantly these proportions, the animal in both cases continues in his natural state: his respiration and circulation do not sensibly appear to be either accelerated or retarded: his temperature remains the same, and he has only, when the proportion of nitrogen gas is too great, a slight disposition to drowsiness *.

* Mem. Acad. 1789.

128. The results of Mr Davy's experience, however, do not correspond with these conclusions of Lavoisier. He introduced a mouse into a jar containing an atmosphere composed of 10.5 cubic inches of oxygen, and three inches of nitrogen gas. In half an hour, the animal appeared to suffer much, and, in about an hour, lay down on his side, as if dying: in an hour and a quarter he was withdrawn from the jar alive, but motionless. The residual air, on being analysed, was found to have lost only 2.1 cubic inches of its oxygen gas, and consequently 8.4 inches of that gas still remained. Another mouse, which was put at the same time into a jar containing 15.5 cubic inches of atmospheric air, was taken out through the mercury alive, but unable to stand, in 50 minutes: and on analysing the residual air, 2.7 cubic inches of its oxygen were consumed. Hence it appears, that the mouse in atmospheric air consumed nearly one-third more of oxygen in 50 minutes, than the other mouse did in an hour and a quarter, when placed in a jar containing so large a portion of oxygen *. The results of these experiments on mice are corroborated by those made by Mr Davy on his own respiration; for he found, that he consumed much less oxygen gas when he respired it pure, than when, for the same length of time, he breathed atmospheric air; and the quantity of carbonic acid formed in the first case, was but little more than half that obtained by the respiration, for the same time, of atmospheric air †. These ex-

* Davy's Researches, p. 443. † Ibid. p. 442.

periments differ greatly therefore from those of Lavoisier as to the effects produced by the respiration of oxygen on the animal system; for, while the latter philosopher informs us, that this gas may be respired for many days without inconvenience, Mr Davy has shewn that the animal dies long before the whole of it is consumed. Trusting, therefore, to the accuracy of Mr Davy's experiments, as in all respects supported by analogy, we infer, that an excess of oxygen gas in the air that is breathed, is not suited to the due maintenance of the respiratory function: and, on the other hand, the oppressive symptoms which the respiration of impure air occasions, as well as the results of Lavoisier's experiments (127.), in which nitrogen superabounded, equally instruct us, that a deficiency of this gas is alike unsuited to it. Consequently, we may conclude, that the atmosphere, as it is naturally composed, is best adapted to the economy of the animal system; but, that this system is, at the same time, so constituted as to be able to bear great variations in the composition of the air without immediate injury to the powers of animal life.

129. When, however, this variation proceeds to a certain extent, the air is no longer capable of supporting vital action; but different animals, when confined in given volumes of air, possess the power of prolonging this action in very different degrees. Thus we have seen (53. et seq.), that insects, worms, fishes, and the amphibia, live until all the oxygen gas of the air is nearly or entirely consumed; while birds die in a given quantity of air before they have consumed two-thirds of its oxygen (84.), and a

mouse and guinea pig expire when about three-fourths of this gas have disappeared. Dr Priestley observed, that if a mouse can stand the first shock of being put into impure air, or has been habituated to it by degrees, he will live a considerable time in air in which other mice will instantaneously die *. The following case, related by the late Dr Percival, seems to prove that a similar observation may be extended to the human subject. An unfortunate man descended into a coal-pit 90 feet in depth, soon after which the sides of the pit fell in. No assistance could be rendered him till the afternoon of the seventh day, at which time he was found sensible and spoke to his companions: his hands and feet were extremely cold, and no pulse could be felt at the wrist: he slept when not aroused to take nourishment, and lived a few hours after being taken out of the pit. The compass of the cavity which he had dug, and in which he was laid on his belly when his companions reached him, was three yards in length and two in breadth, and the stratum of coal was two feet thick. No circulation of air could possibly have taken place through this cavity; and the foulness of the vapours which issued from it, prevented any one, for some time, from venturing to his assistance †. Now, the capacity of a cavity, nine feet long, by six wide and two deep, will be equal to 108 cubic feet; from which, if we deduct three feet, as the space occupied by his body, the actual

* Experiments on Air abridged, vol. iii. p. 257.

† Manchester Memoirs, vol. ii. p. 467.

quantity of air which the cavity contained will be equal to 105 cubic feet. If we suppose this air to have been at first similar in composition to that of the atmosphere, containing $\frac{22}{100}$ of oxygen gas, the quantity of that gas may be taken at 23 cubic feet, by which the function of respiration was supported for more than seven days. But, in ordinary respiration, the daily consumption of oxygen (122.) amounts to 26 cubic feet; so that, in this case, animal life was protracted for seven days on much less than one-seventh of the usual quantity of that gas. When, however, death does happen to animals confined in a given volume of air, it must arise either from the noxious operation of the nitrogen gas that is always present, or from that of the carbonic acid, which is formed; or it must proceed from the deficiency, or total absence, of oxygen gas. Now, although nitrogen gas do not of itself support life, yet we have no evidence that it exerts any injurious effect on the animal system. In vegetation (5. 29.), and in the respiration of the inferior animals (73.), it has been shewn to be wholly inactive; and when, in the experiment of Lavoisier (127.), it constituted $\frac{15}{16}$ of the air employed, a degree of drowsiness only seems to have been induced by it. That it is entirely passive, is still farther confirmed by an experiment of Lavoisier, who found that hydrogen gas, mixed in due proportion with oxygen, would serve the purposes of respiration as well as the air of the atmosphere. We have no proof that nitrogen is able to enter the vessels so as to produce any direct operation on the blood,—an effect which is still farther forbidden by its incapacity of uniting with that fluid. We may therefore conclude, that nitrogen gas, when respired, neither suffers any change itself, nor produces any direct operation on the animal system.

130. The only other gas to which the death of animals, in these circumstances, can be ascribed, is carbonic acid, which, however, when formed by respiration, does not seem destructive to animal life. Dr Goodwyn observes, that when the same air is breathed several times, so as to increase the quantity of carbonic acid, its noxious operation is to be attributed not to the presence of this acid, but to the deficiency or absence of oxygen gas *; and when Spallanzani, by means of an alkaline substance, abstracted this acid as soon as formed by the respiration of birds and quadrupeds, he did not find that they lived longer in a given bulk of air than when it was suffered to remain †. In the foregoing case also, related by Dr Percival, the carbonic acid formed by respiration must have been retained in the cavity, and yet no destructive effect seems to have followed from it. Dr Higgins observes likewise, that debility, convulsions, and death, follow the successive diminution of the oxygen gas of the air in respiration, long before the whole of that gas is consumed, although the carbonic acid that is generated be, in the mean time, carefully withdrawn ‡. Indeed, we might in this, as in former examples, be

* Connexion of Life with Respiration, p. 66.

† Memoirs on Respiration, p. 318.

‡ Higgins's Minutes of a Society, p. 160.

led to suppose, that neither the carbonic acid formed in respiration, nor the nitrogen gas employed in that process, would exert any positively destructive operation on the animal powers, since both of them must, at all times, necessarily be present in the system; and seeing, moreover, that the abstraction of oxygen gas alone is sufficient to account for the fatal effects which ensue, it must be deemed unnecessary to resort to the supposed agency of any subordinate cause.

CHAP. V.

OF THE SOURCE OF THE CARBON IN VEGETABLES AND ANIMALS BY WHICH THE CHANGES IN THE AIR ARE EFFECTED.

131. In the preceding chapters, it has been generally concluded, that the oxygenous portion of the air is converted into carbonic acid by the processes of germination and vegetation, and by the respiration of animals: we must therefore presume that seeds, and plants, and animals, in some way, furnish the carbon whereby the acid in question can be formed. To inquire into the immediate source of this carbon is the object of our present investigation.

132. It appears, from the results of experiments already related (1.), that if seeds, in a dry state, be exposed to atmospheric air, they neither suffer any change themselves, nor produce any on the air in contact with them: but that if they be confined in water,

they gradually suffer decomposition, forming carbonic acid and carburetted hydrogen gases; and the faculty of germinating is then destroyed. If, farther, they be first steeped in water, and then placed in nitrogen gas (5.), carbonic acid is, in like manner, formed; and the same acid is likewise produced when they are simply plunged into a tube of mercury without the aid of any elastic fluid at all; but, in neither of these cases, do they exhibit any sign of germination. Whatever, in these experiments, may be the source from whence the oxygen gas forming the acid is derived, it must be granted that the seed, in every case, furnished the carbon. But this acid is likewise produced when steeped seeds are made to germinate in atmospheric air (6.), or in oxygen gas, where nothing also but the seed is present from which the carbon can be derived: hence therefore, it follows, that the seed, during its germination, as well as under decomposition, must be able to supply carbon.

133. Does then the carbon, which thus contributes to the formation of the acid, issue directly from the seed, and unite with the oxygen gas of the air exterior to its substance? or does this oxygen gas previously enter into the seed, and, combining with its carbon, again escape from it in the form of carbonic acid? If the view which we have taken of the structure of seeds, and of the manner in which the air is affected by their growth, be just, this latter supposition cannot be entertained: and, from the facts stated in the foregoing paragraph, it is sufficiently clear, that, in many cases, the entrance of oxygen gas into the seed is by no means necessary

to abstract its carbon; for carbonic acid was formed by seeds where none of that gas previously existed. From the following facts, it will be seen also, that the seed, during its germination, is able to yield carbon, without the supposed agency of oxygen gas to abstract it. M. Huber observed that seeds did not germinate in air, which had been previously injured by the respiration of bees, although a quantity of oxygen gas was added to it sufficient to carry on germination, if mixed with pure nitrogen gas: at first, he attributed this failure to the presence of the carbonic acid which remained in the residual air, but when he carefully removed this acid by first washing the residual air in lime-water, he still found, that although fresh portions of oxygen were added, no germination took place, until after a very considerable quantity of that gas had been supplied. He found precisely the same thing to happen when he employed the residual air that had served for successive germinations, since fresh seeds would not grow in such air until it was mixed with a quantity of oxygen much more than sufficient to carry on germination in pure nitrogen gas. Reflecting on these circumstances, he conceived, that as seeds yield carbon to unite with the oxygen gas of the air during their germination, that substance might also combine with its nitrogenous portion: and when thus saturated with carbon, the nitrogen might not favour germination if mixed only with its usual quantity of oxygen, because the oxygen gas, having a greater affinity for carbon than the nitrogen, would carry it off from the latter to form carbonic acid, and consequently not leave sufficient oxygen to support germination. This supposition was rendered the more probable, because he found, that when oxygen gas was mixed with nitrogen thus saturated with carbon, carbonic acid was formed, although the two gases had, previous to their mixture, been carefully washed in lime-water *.

134. That the carbon of the seed had really escaped, and combined with the nitrogen gas in the experiments above stated, is farther evinced by what follows. M. Huber prepared an artificial atmosphere in proportions similar to that of common air, but in which hydrogen was substituted for nitrogen gas: and in this atmosphere he caused successive quantities of lettuce-seeds to germinate, till they ceased any longer to grow. He then carefully washed this mixture of gases in lime-water, until it produced not the slightest discolouration; and, when all the carbonic acid was by this means removed, he added to the residue a portion of oxygen, previously deprived of every particle of carbonic acid, and inflamed the mixture in Volta's eudiometer over lime-water. At the moment of inflammation, the lime-water was rendered very turbid, and a great precipitation of carbonate of lime took place. As no carbonic acid existed in the two gases previous to their inflammation, its production must have arisen from that process; and we must therefore admit, that the carbon which had combined with the hydrogen during germination was again separated from it, and had united with the oxygen gas to form the carbonic acid

* Mém. sur la Germination, p. 41. et seq.

in question. If, after being thus prepared, the two gases, instead of being inflamed, were placed together over lime-water, carbonic acid was, in like manner, formed; but the same result, which in one case was effected in an instant, required, in the other, a much longer time to produce. It is farther remarked, that pure hydrogen gas burns with a lively white flame, but that the gas which has been employed as an atmosphere in germination, yields a tinge more or less blue, although it may have been washed in lime-water. From these facts, says Sennebier, it must be concluded, that hydrogen gas, which has been used in germination, becomes charged with carbon, and that this carbon can have been furnished only by the operation of germination or vegetation, which produce upon this gas the same effect. It is farther proved by these facts, he adds, that growing seeds and plants are not the direct sources of the carbonic acid found in the atmosphere in which they have lived; but that this acid is a combination of the carbon which escapes from these bodies with the oxygen gas of the air *. The decomposition of carburetted hydrogen in the foregoing experiments of M. Huber, accords with the phenomena which take place when it is placed in contact with oxymuriatic acid gas: for all the varieties of carburetted hydrogen gases, if mixed in due proportion with this acid, are converted into water, carbonic acid, and carbonic oxide gas; and if, instead of allowing the mixture to remain at rest, the electric spark

* Sur la Germination, p. 86. et seq.

be taken through it, a slight explosion follows, with a production of carbonic acid and water *.

135. To these facts, tending to prove the emission of carbon from seeds, it may perhaps be objected, that this substance exists in so fixed a state, that it cannot pass off in the attenuated form, which, in these cases, is required; but this objection applies equally to every case where carbonic acid is spontaneously formed. Those who suppose the oxygen gas to enter into the seed, and, by the power of chemical affinity, to abstract its carbon, must likewise admit, that this carbon is raised into a gaseous out of a fixed state, or no carbonic acid could be formed. This power of assuming the gaseous form, differs not only according to the circumstances in which the carbon may be placed, but also according to the state of combination in which it exists. Thus, in its pure state of diamond, a very intense heat is required to effect the union of carbon with oxygen gas: in the example of plumbago, a lower heat will suffice; and in that of charcoal, a much lower still. Not only, however, by these high degrees of heat, but at the ordinary temperature of the atmosphere, may this carbon be separated from its combination with various substances. Mr Cruickshank observed, that the gas which arises from the slow decomposition of humid vegetable matter, is similar to that obtained by the decomposition of camphor or æther by heat †: and many examples have

* Murray's System of Chemistry, vol. ii. p. 385.

† Ibid. vol. ii. p. 384.

been given, in which carbonic acid and carburetted hydrogen gases were formed by moistened seeds, at the ordinary temperature of the atmosphere. Nor is even the decomposition of the seed necessary to extricate its carbon; for M. Vauquelin found, that moistened seeds, which had yielded carbonic acid in hydrogen gas, would afterwards grow when placed in the open air *. From these facts it follows, that the carbon of vegetable bodies may be made to unite with oxygen gas at a low temperature, as well as at a high one; and that this matter may be given out by seeds antecedent to their decomposition, as well as under that process.

136. It is more difficult to ascertain the form which carbon assumes, and the means by which, in these various examples, it is brought into that form in which it exists at the moment of its union with oxygen gas. In the processes where heat is employed, its particles are perhaps so changed or attenuated by the operation of that subtile fluid, as to be rendered capable of combining with the oxygen gas presented to them; but no such operation of heat can be going on where the carbon is made to unite with that gas at the ordinary temperature of the atmosphere. Some other means, therefore, must be sought, which shall be sufficient to reduce the carbon to the state required for such a combination. Now, we have seen, that the carbon of the seed does not combine with the oxygen gas of the air, so long as the seed continues in a dry state; but when

* Phil. Mag. vol. xxv. p. 223.

it is duly moistened, this carbon, in the same temperature, and under the same circumstances, is yielded by the seed, and combines either with oxygen, nitrogen, or hydrogen gas. No other agent but humidity, therefore, being introduced, to which this variation in effect can be ascribed, we are led to attribute it to that cause; and to believe that carbon, as it exists in seeds, may be so acted on or dissolved by the imbibed water, as to be capable of passing off from the seed with its exhaled moisture, which thus becomes the proper vehicle of it. It is well known, that both seeds and plants contain a large portion of carbon, and it is extremely probable that they derive it from a state of solution in water; for M. Giobert found, that plants did not grow well in a mixture of simple earths until he moistened his soil with water from a dunghill. Now it is certain, from the experiments of Hassenfratz, says Dr Thomson, that this water contains carbon; for when evaporated, it constantly leaves behind a residuum of charcoal. All those manures likewise which act with efficacy and celerity, contain carbon in such a state of combination, that it is soluble in water; and the efficacy of the manure is proportional to the quantity of carbon so soluble *.

137. If then it be granted, that the carbon of decomposing vegetables is largely soluble in water, there seems no reason why the carbon of the seed, which gradually diminishes (20.) during germination, should not be soluble in it also: and since the wa-

* Thomson's Chemistry, vol. iv. p. 253. 256.

ter imbibed is constantly escaping from the seed, there is reason to believe, that it is the proper menstruum and vehicle of this carbon. Hence, in the foregoing experiments of M. Huber (133-4.), the carbon furnished by the seed united first with the oxygen gas, until that gas became saturated, and afterwards combined with the nitrogen or hydrogen, whose affinity for it is not so strong as that of oxygen *. Even after combining with hydrogen, carbon will again leave it to unite with oxygen, either when the two gases are inflamed together by electricity, or are suffered for a few hours to remain at rest. If, however, fresh portions of oxygen be successively supplied, the carbon, afforded by the seed, seems to unite with it alone, and germination regularly proceeds. There does not, in these examples, appear to be any ground for supposing the oxygen gas to be first received into the seed in order to combine with its carbon, and to be afterwards expelled from it in the form of carbonic acid; for no known powers of the seed are capable of performing such an office, and such a supposition is the less necessary, since the seed is able to emit carbon without it. If also, from the mere fact of their combination, the oxygen gas be considered to enter into the seed, and abstract its carbon, the hydrogen gas must be held to do the same; and the same thing may likewise be affirmed of nitrogen gas, with which (133.) the carbon of the seed will combine. Moreover, the seed affords carbon through every

* Murray's System of Chemistry, vol. ii. p. 311.

stage of its decomposition, where we must suppose it to escape, like the other elements of the body, from the dissolution of those affinities by which they were held together. From the whole, therefore, we are led to conclude, that the carbon of the seed, by the action of the imbibed water, is reduced to a state capable of combining with oxygen gas at the ordinary temperature of the atmosphere, and that it passes off from the seed with the moisture exhaled from it.

138. If any cause obstruct the emission of this water and carbon, germination is either partially or wholly prevented, as the following experiments will prove. Equal numbers of steeped peas were supported by two hoops in two equal jars of atmospheric air; but the peas of one jar were previously dipped in oil. Both jars were then inverted over mercury, and placed in a room beside each other. The oiled peas in a few days had sprouted about one-third of an inch, while the radicles of the others were more than an inch in length. The air of the jar in which the oiled peas had been confined was diminished but little by agitation with lime water; while that of the other jar rendered the lime water quite milky, and lost nearly one-fifth of its bulk. The same results were obtained when varnish was applied over the peas instead of oil, very little carbonic acid being formed, and the residual air still retaining $\frac{15}{100}$ of oxygen; while the residual air of the jar in which other peas had for the same time been growing, when freed from carbonic acid, contained no oxygen gas at all. In like manner, if the imbibed water be too rapidly abstracted, the growth of

the peas is checked, and the air in proportion suffers but little change. A jar containing some steeped peas, supported on a hoop, was, as before, inverted over mercury; but within it was previously placed a small glass cup, filled with powdered lime, which floated on the mercury. The jar, during the whole experiment, continued dry; the peas sprouted but little, and shrunk nearly to the same size as when in their dry state; and the lime greatly increased in bulk. The residual air, when examined, suffered no diminution by being shaken with lime water; but it still contained a large portion of oxygen gas; while the air of another jar, placed in similar circumstances, except that it held no lime, had in the same time lost the whole of its oxygenous portion: the peas also were nearly double their original size, and their radicles 1.2 inch long. Hence, therefore, it is clear, that whatever obstructs the emission of moisture and carbon from the seed, or too rapidly draws off the moisture, necessarily puts a stop to those reciprocal changes between the seed and the air, so essential to its future growth.

139. That the carbon which issues from the seed during its germination, is given out in union with the moisture previously imbibed, will, perhaps, appear more evident from what we have next to offer concerning the carbon furnished by plants during their vegetation. It has been shewn (30.), that the presence of oxygen gas is essential to the growth of plants, and that, by the upper surfaces of their leaves (26.), it is converted into carbonic acid: it has been also shewn (39.), that the carbon, which effects this conversion, proceeds neither from the

soil, nor from decayed leaves, but from the living leaf itself. In what manner, then, is the living leaf enabled to exercise this important function? The sap of plants, which is chiefly taken up by the roots, passes through the vessels of the stem to the surface and extremities of the leaf, where it suffers considerable changes (36.), and is then again returned by the vessels of the footstalk to the branches and stem. By this flow of the sap, the function of perspiration in the leaf is supported, and when, from any cause, it diminishes, vegetation seems, in a great degree, to be suspended. This happens to many plants during a cold rain, as well as through the night; and the leaves, in consequence, assume a change of form and condition, which some have supposed to resemble a state of sleep; and in winter, we know, with the cessation of the flow of the sap, vegetation wholly ceases. But the plant, during all these times, is in contact with the atmosphere, and the cessation of the usual vegetative changes cannot, therefore, be attributed to the want of oxygen gas: they must, consequently, proceed from some alteration in the state of the plant itself, which alteration consists in the diminished or suppressed flow of the sap, by which all the vegetable secretions are suspended.

140. If, farther, the leaves of a plant be separated from the stem, they retain life for a considerable time: but the circulation of fluids through them must then of necessity be cut off. Dr Woodhouse exposed handfulls of the leaves of twelve different plants separately to the light of the sun in 40 ounce measures of air for four hours, and its purity was found to be neither increased nor diminished. Af-

ter they had remained sixteen hours in the air, no effect was produced on it. The leaves were fresh gathered, and no decay could be observed in any part of them *. Now, as the only difference in the state of these leaves consisted in their removal from the plant, and consequent cessation of the flow of the sap, by which the usual changes on the air were prevented, it is to that cessation arising from such removal, and not to any other alteration in the structure or condition of the leaves, that this prevention of change is to be ascribed. But this flow of the sap is a proof of the existence of living action in plants, and by its means all the vegetable secretions are carried on : whence it follows, that as the leaves act upon the air, and consequently emit carbon only while this flow continues, its emission is the result thereof, and consequently of a living action also.

141. By smearing over the upper surface of the leaves with varnish or oil, a mechanical obstruction is furnished to the emission of carbon, and the plant dies (26.) just as when the circulation itself is cut off, little or no change, it is believed, being then produced on the oxygen gas of the air. Since then the leaves alone effect changes on the air, and these only while attached to the stem ; and since these changes cease, either when the circulation of the sap is suspended or cut off, or a mechanical obstruction is furnished to the escape of the carbon ; we must conclude that the carbon, thus emitted by the leaves, is in truth a vegetable excretion, dependent, like

* Nicholson's Journal, July 1802, p. 159.

others, on the due circulation of the fluids of the plant, and varying as that circulation varies. As we observe this carbon, however, only in combination with oxygen gas, its precise form or mode of existence cannot as yet be ascertained; but the fact of its always accompanying the exhalation of moisture from the leaf, leads to the opinion, that it is not only coincident with that excretion, but is carried on likewise by the same structure; the exhaled moisture, as in the case of the seed, serving as the proper vehicle of it. It appears also from the facts already stated (134.), that carbon is afforded by the plant, as well as by the seed, where no oxygen gas was present to unite with it; which shews that its emission depends immediately on the power of the plant, and not on any supposed chemical agency of the air. It may be added, that the experiments of Dr Woodhouse demonstrate likewise, that leaves as well as seeds, under decomposition, form carbonic acid *: but this we notice for the sake of discrimination only, since we are now considering the changes which atmospheric air undergoes from the agency of certain living processes alone.

142. Ascending next, in the order of our inquiry, into the animal kingdom, we find, that, to insects and worms, the presence of oxygen gas is essential to the continuance of living action (53. et seq.), and that they gradually, and, in many instances, completely convert this gas into carbonic acid. No other substance but the animal being present in the

* Nicholson's Journal, June 1802, p. 159.

experiment by which the carbon could be supplied, we must refer its production to that source. The formation of this acid was found also by Spallanzani to depend very much on temperature; for snails consumed more oxygen, and died in equal bulks of air, much sooner when the temperature was mild, than when it was severe *; and, by gradually reducing the temperature, he at length reached that point where the air in which the snails were confined, underwent no change at all; which point he found to be at zero †. The circulation all this time was gradually declining, and at —1° Reaum. the pulsations altogether ceased, the heart remained perfectly at rest, and the whitish fluid usually flowing in the vessels was in a state of stagnation ‡. Nearly similar results were obtained from another species of snails in a hybernating state. In these, the covering or opercle with which they close up their shell in winter, effectually excludes the air on that side, neither does it penetrate their shells elsewhere; for a small glass tube, filled with mercury, being inserted into this covering, and then inverted, sustained a column of 28 inches of that fluid, which corresponded to the height of the mercury in an adjacent barometer. When, however, the covering was punctured so as to admit the air, it instantly forced down the mercury. The small portion of air contained in these shells for several months during the torpid state of the animals, was found in several trials to be of like

* Memoirs on Respiration, p. 150.

† Ibid. p. 152. ‡ Ibid. p. 154.

purity with that of the atmosphere *; so that when living action had ceased in these animals, no change was produced on the air in contact with them.

143. Nor are these effects peculiar to those animals which change the air by the mediation of their skin: they are common also to those which breathe by lungs. A marmot that was cold to the touch, and so closely rolled up as to be tossed about and irritated in every shape without exhibiting the slightest indication of life, was exposed in a recipient of atmospheric air, inverted over mercury, to a temperature —12° Reaumur. He continued motionless three hours and a half, during which time the mercury in the jar remained stationary: and the air, on being analysed, was found in every respect similar to that of the room in which the experiment was made †. The same animal was then transferred into a jar of carbonic acid at temperature —12.5° Reaumur, where he remained four hours without shewing the least sign of motion, or suffering the smallest injury; but a rat, put at the same time into the jar, died almost instantly; and when, in a second experiment, the temperature was raised to zero, the marmot also began to breathe, and then speedily died in a jar of this gas. These facts prove, not only that the carbon, which decomposes the air, is in every case furnished by the animal, but that its emission depends wholly on the state of the circulating fluids; for, as the circulation increased, declined, or ceased, so likewise did the emission of carbon, and consequent pro-

* Memoirs, p. 197. 203.

† Ibid. p. 333. et seq.

duction of carbonic acid : the oxygen gas could not vary the result, being in all cases equally abundant.

144. Does then this emission of carbon proceed from the fluids circulating in the larger vessels of the animal ? Or is it given out from the exhalent structure of the body ? That these fluids may contain carbon as a constituent element, we do not mean to deny : but, that this substance can be emitted through that organized structure which carries on the respiratory function, may, we think, be justly disputed. This structure has been shewn to embrace a large portion of the surface of the bodies of insects, which again is occupied only by the terminations of exhalent vessels, whose office it is to emit fluids, previously separated from the blood, while the blood itself usually circulates in the deeper seated vessels, or even at the centre of the animal. When also the usual changes on the air are suspended, by placing the animal in a cold medium, the carbon might be expected to accumulate in the blood, if that were the source from which it is directly derived, and carbonic acid to be at that time most abundantly produced : but the preceding facts evince, that it is not to the supposed accumulation of this substance in the blood, but to the state and vigour of the circulating fluids, that the production of this acid is to be ascribed. We have therefore no proof that carbon exists in the mass of blood in a state capable of attracting air, or of being attracted by it through the organized structure of the animal : nor even if these attractions were allowed to take place, do the phenomena at all correspond with what, under such a supposition, ought to happen.

145. If, then, the carbon in question be not emitted from the blood while yet in the larger vessels, nor after it has ceased to be in motion, it must be given out by that fluid while in circulation, and after it has entered into the minuter vessels: and thus it becomes an animal excretion, derived, like other excretions, from the blood, and emitted, like them, by some appropriate structure from the surface of the body. Hence any cause, as cold, checking the circulation, restrains the production of this carbon; or, although the circulation be not checked, the emission of this substance is prevented by smearing the bodies of insects (51.) over with unctuous matter, which in consequence causes their death. All that has now been said of insects and snails, applies equally to the marmot and other torpid animals; for the emission of carbon in all, is obedient to the same laws, except that, in the former case, it is given out by the exhalent surface of the body, while, in the latter, it proceeds from the exhalent structure of the lungs. For these reasons, we consider the emission of carbon in these animals to be truly an excretion dependent on the due circulation of their blood, and partaking of all its variations. The experiments of Spallanzani, it may be observed, prove likewise, that under decomposition, animal as well as vegetable substances form carbonic acid in air where no oxygen gas was originally present *,—a fact with which the experiments of Priestley had long before made us acquainted †.

* Memoirs on Respiration, p. 346.

† Priestley on Air, vol. iii. p. 340.

146. To the life of water-snails, muscles, fishes, and the amphibia, a supply of fresh air has been shewn to be equally necessary, its oxygenous portion being, in every instance, more or less completely consumed and changed into carbonic acid; the carbon for which purpose could be derived only from the animal system. We have not, in this instance, the evidence of experiment to prove that the carbon was emitted by the living power of the animal, that is, only while its circulation continued: nor, although some of these animals are known to suffer torpidity, can we experimentally shew that they then produce no change on the air contained in the water in which they are placed. In every view of the subject, however, analogy is so strongly in our favour, that we cannot hesitate to admit both suppositions to be true: and the termination of the branchial artery in exhalent orifices (65.) on the gills of fishes, would seem to point it out as the structure by which, in that class of animals, the carbon, in combination with the exhaled fluid, escapes. At all events, the blood of a very large number of these animals being devoid of colour, is at least a proof that carbon does not cause the blackness of that fluid; or, if it still be contended that it does, then blood, which is white, and may be presumed therefore to contain no carbon, can effect the same changes on the air, as that which owes its blackness to this substance.

147. Analogous to the emission of carbonic matter from the surface of the bodies of snails (145.), seems to be that which is carried on by the external and internal surfaces of the human body. The Count de Milly first observed small bubbles of air

to form on the surface of the body when it was immersed in the warm bath: and, by means of a funnel and inverted glass jar, he collected, in a few hours, a considerable quantity of it, which, being examined by M. Lavoisier, was found to precipitate lime-water, and to suffer no diminution from the admixture of nitrous gas *. Mr Cruickshank observed, in repeated trials, that the air of the glass-vessel in which his hand and foot had been some time confined, rendered lime-water turbid, and that a lighted taper burned very dimly in it †. Mr Abernethy exposed his hand for five hours to seven ounce measures of atmospheric air, contained in a jar inverted over mercury: the quantity of air diminished half an ounce, and an ounce more of it rapidly disappeared by agitation with lime-water, and the lime was precipitated: the remaining air being examined by the test of nitrous gas, was found to contain nearly one-sixth less of oxygenous gas, than it did before the experiment. In another similar experiment, continued for nine hours, more than an ounce measure of carbonic acid was produced, and the remaining air contained one-fourth less of oxygen than before ‡. M. Jurine fixed well-dried bottles under his arm-pits and round his waist, and, after they had remained an hour in these situations, the air in them was much vitiated: and on different occasions was found to be diminished in bulk, and to contain from

---

* Berlin Transactions for 1777, p. 35.

† On Insensible Perspiration, p. 81. & 82.

‡ Essays, Surgical and Physiological, p. 118.

from four and a half to $\frac{7\frac{1}{2}}{100}$ of carbonic acid. The air confined under the bed-clothes where different persons had slept, he found likewise to contain from four to $\frac{8}{100}$ of carbonic acid. In the stomach also, carbonic acid is formed: and from that organ the proportion of oxygen gas diminishes progressively downwards, the carbonic acid varies in quantity, and the nitrogen gas is uniformly increased *. These experiments prove, that carbonic acid is produced wheresoever the surfaces of the living body are brought into contact with atmospheric air.

148. Whence, then, does this carbonic acid proceed? Dr Ingenhousz, finding that small bubbles of air still escaped from the surface of the body, even although he first carefully removed those which naturally adhered to it, was led to consider them as an aërial transpiration from the skin †. But when Dr Priestley employed rain water, from which the air had been completely expelled by boiling, and detached also the adhering bubbles from his arm, he was never able to collect a single bubble of air ‡. Dr Pearson also found, that there was no appearance of aërial bubbles on the surface of the cuticle during bathing in warm water that had been previously boiled, so as to expel the air usually mixed with it §. And M. Jurine, after remaining in the bath for hours together, in temperatures varying

---

* Mém. de la Societé de Med. tom. x.

† Exper. sur les Vegetaux, tom. i. p. 152.

‡ Experiments and Observations, vol. v. p. 103.

§ Hunter's Observations on Animal Œconomy, p. 168.

from 70° to 100°, never could obtain a single bubble of air from his skin. These facts correspond precisely with those already adduced concerning the separation of air from water (45. 47.) by vegetable bodies and other fibrous substances. They prove decisively, that the human body, like other solid bodies, has the power of separating the air naturally contained in water: but that when the water contains no air, none is separated by immersing the body in it. It matters not what the substance is, says Mr Hunter, if it be but warmer than the water; for a piece of iron heated to 150°, and immersed in water at 70°, will cause the water to part with its air; but if the iron be ten degrees colder than the water, little or no air will be separated *. The small hairs distributed over the body facilitate, in all probability, this separation of air; for Mr Abernethy observed, that, on introducing his hand and arm into a jar of water at the temperature of 60°, every pore seemed covered with a little spherule of air, which, on agitating the water, was detached, and quickly rose to the top of the vessel; and when the water was changed at each repetition of the experiment, a much greater quantity of air was collected, but in moderately warm water he scarcely procured any air †. The air separated in experiments of this nature was, according to Dr Priestley, just that mixture of carbonic acid and partially vitiated air which pump-water generally abounds with: and Dr Klapp,

* On the Animal Œconomy, p. 168.

† Essays, Surgical, &c. p. 115.

who has lately repeated these experiments in America, found, that the gases collected by holding the arm in an inverted glass-vessel filled with water, differed in no considerable degree from those which had been previously obtained from the water. These facts altogether forbid our drawing any conclusion in favour of an aërial transpiration by the skin, from the mere circumstance of air being separated by the body when it is immersed in certain kinds of water.

149. But Mr Abernethy supports the opinion of an aëriform perspiration by the skin on other grounds, maintaining, that carbonic acid is given out as well when the hand is plunged in a jar of mercury, or is immersed in any kind of air, as when it is placed in contact with oxygen gas *. After detaching the adhering air from the surface of his hand, by moving it for ten minutes in mercury, he introduced it within a jar filled with that fluid, the thermometer standing at about 60°. Minute air bubbles rose at first to the top of the fluid, but more slowly afterwards; and in an hour, when the hand was withdrawn, the air collected was equal in bulk to one scruple of water. In sixteen hours, it equalled half an ounce; and of this, two-thirds were attracted by lime water, and the remainder did not diminish on mixture with nitrous gas: whence he concludes, that it consisted of carbonic acid and nitrogen gas, and that these gases were perspired by the skin. The small quantity of air separated in this experiment, during sixteen hours, is itself a proof that this

* Essays, Surgical, &c. p. 121, 122.

supposed aërial perspiration is very inconsiderable; and the air obtained is to be considered not as the result of one, but of many experiments; for we are told, that after the first hour the hand was withdrawn; and there can be little doubt but that this was several times repeated before the experiment was closed. By so frequent an introduction of the hand, it is very probable that small quantities of adhering air would be repeatedly carried in, which is confirmed by the observation of Mr Abernethy, that the separation of the air was greatest on first introducing the hand *. He farther says, that when in temperatures between 60° and 70°, he kept his hand five hours in seven ounce measures of nitrogen gas, confined in a jar over mercury, there appeared no difference in the quantity of the air; and on passing lime-water into the jar, rather more than one ounce of carbonic acid was produced, when no oxygen was present. The same results, nearly, were obtained, when hydrogen and other gases were employed †.

150. These experiments of Mr Abernethy have lately been repeated in Philadelphia by Dr Klapp, in the presence of Dr Woodhouse, Professor of Chemistry in that university; and with completely different results. This gentleman carefully separated all the adhering air from his hand and wrist, by moving them in different directions under the surface of the mercury, for ten minutes, as Mr Abernethy had done: they were then introduced into the inverted glass-vessel, filled with the same fluid, at tem-

* Essays, Surgical, &c. p. 112. † Ibid. p. 121.

perature 73°, and retained for more than three hours. During all this time, not a bubble, either of carbonic acid or nitrogen gas, was seen to emanate from the skin; and the experiment was a second time repeated with precisely the same result. He next immersed his hand and wrist in very pure lime-water, of the temperature of 70°, and held them in that situation, in different experiments, for one and two hours, during which times no carbonate of lime was formed; but, on the contrary, the lime-water, when the hand was withdrawn, was as transparent as before it was introduced. He next placed his hand and wrist, after separating all adhering air from their surface, in four ounce measures of pure hydrogen gas, confined in a vessel inverted over mercury, and kept them in this situation for three hours. The air in the vessel was then examined in the presence of Professor Woodhouse: its volume was not in any degree diminished; neither, on the addition of lime water, could any trace of carbonic acid be discerned in it *. In addition to these experiments, disproving the existence of an aëriform function in the skin, it may be observed, that if such a function did really exist, the bulk of air ought to be increased, when carbonic acid is formed by placing the arm in a jar of atmospheric air; but, according to Mr Abernethy himself, the air was considerably diminished. All the arguments already urged against the absorption of aëriform fluids, forbid us to suppose that the oxygen gas which disappears, is taken up

* Inaugural Dissertation, Philadelphia, 1805, p. 21. et seq.

by a natural function of the skin: and the apparently decisive experiments of Dr Klapp, prove, that no such absorption of oxygen is necessary, because no emission of carbonic acid, or of any other aëriform fluid, can take place from that organ. The production of carbonic acid in the stomach, with the diminution of oxygen, and progressive increase of nitrogen gas in the intestines, observed by M. Jurine, appear necessarily to arise from the longer time in which the air remained in contact with the body, and the greater degree of decomposition which it in consequence underwent.

151. If, then, there be no proof that oxygen gas enters into the body through the skin, and if it be also allowed that no carbonic acid can be emitted from that organ, it must be granted, that the acid actually produced (147.), is formed exterior to the skin itself; and the disappearance of the oxygen gas, in proportion to the production of this acid, leads at once to the opinion that it is formed in part out of that gas. M. Jurine found accordingly, that as much acid was formed out of a given bulk of air, when his arm was confined in it for one hour, as when the experiment was continued for three or four hours, which, it appears to us, could have arisen only from all the oxygen gas having within the first hour been changed. For the formation of this acid, however, carbon, its other ingredient, must be in some way supplied; and as, in these experiments, no other substance but the skin was in contact with the air, from which it could be derived, we must ascribe the production of the acid to the spontaneous union of carbon, furnished by the skin, with

the oxygen gas of the air ; and this carbon we conceive to be given out by its exhalent function.

152. In vegetables, and in the inferior animals, it has been shewn, that carbonic matter is yielded only while the circulation of their fluids continues: and there are some facts related by M. Jurine which render it very probable, that the same thing holds true concerning the carbon which is furnished by the human skin. He found, that in given times, the greatest quantity of carbonic acid was formed by the skin of a person in the vigour of life, next by a boy of ten years of age, and least of all by a man of 70; and that its production depended much, likewise, on the season of the year, and the vigour of the cutaneous organs. All these facts seem necessarily to follow from the opinion that this carbonic matter is truly an excretion, depending altogether on the distribution of the animal fluids, and subject to all their variations. Whether, as in other animals, the air is no longer decomposed by the skin when the circulation has wholly ceased, we have not the means of deciding; but the results of the foregoing experiments of Jurine, lead us to suppose that such is actually the fact. Even at the period in which Mr Cruickshank wrote, it is probable, that, had the composition of carbonic acid been then determined, he would have formed a similar opinion concerning the source of the carbon: for, in referring to his experiments, he observes, "that (admitting the common theory of fixed air and phlogiston) something passed off with the vapour of insensible perspiration by the skin, which rendered air fixed, and

heavier than atmospheric air *." By wearing a fleecy hosiery vest also next the skin for a month at a time in the hottest parts of summer, he collected an oily substance which accumulated on the nap: this substance he exposed to a red heat in a silver spoon, and, when burnt, it left behind a black powder resembling in every thing the powder of charcoal. "This all seems to prove," he adds, "that phlogiston" (by which he can here mean nothing else than carbon) "is emitted from the pores of the skin †." From the establishment of this power in the skin, we obtain, besides, this additional and important fact,—that carbonic acid may be formed out of the oxygen gas of the atmosphere, when brought into contact with the human body, without the necessity or even the possibility of believing that the carbon entering into its composition, is derived from the venal blood; for the exhalent vessels of the skin are all of arterial structure, and the fluids which they contain and emit, exhibit nothing of the colour or the properties of venal blood.

153. The changes effected on the air by the eggs of birds, may still farther elucidate this subject. The shell of the egg is formed of carbonate of lime, with a small portion of phosphate, united to an organic tissue. Spallanzani found it to be full of holes, through which pass the extremities of very minute vessels, coming off from the chorion, or membrane with which the shell is internally lined: these vessels terminate by small orifices on the surface of the

* On Insensible Perspiration, p. 82. † Ibid. p. 94.

shell, forming a fine and transparent net-work. A hen's egg was placed in three cubic inches of atmospheric air, inverted over mercury: on examining the air at the end of the fourth day, it was somewhat diminished in bulk, its nitrogenous portion remained unaltered, a large quantity of its oxygen had disappeared, and a portion of carbonic acid was produced. This diminution of the oxygen could not be owing to any supposed attraction of it by the substance of the egg; for the shell, when deprived of its contents, was found to produce a similar effect on the air *. As the carbonic acid in this, as well as in the former instances, was formed when the air and the egg were in contact, and neither of them separately could produce it, it is evident that the air must have furnished the gas, while the egg supplied the carbon. It is known that an exhalation is constantly going on from the egg, by which it is daily becoming lighter; and the more rapidly this exhalation proceeds, the sooner does the egg decay. The egg of a sparrow, weighing 29 grains, was exposed on a table in a room varying from 60° to 66° Fahrenheit. By the third day, it had lost one grain: by the fifth day, two grains: by the tenth, three grains: by the seventeenth, four and a half grains: and by the twenty-sixth day, six grains: when broken, its contents were very putrid. A fresh hen's egg, weighing 13 drachms, and exposed in the same room from June 15. to July the 30th, lost in weight 2 drachms 20 grains: while another egg, equally exposed, but

* Memoirs on Respiration, p. 237. et seq.

smeared over with oil, lost, in the same time, only 45 grains. If also eggs be varnished over, or placed under the exhausted receiver, they do not corrupt: and Bomàre mentions the circumstance of three eggs being found in the wall of a church, which, by being protected from the air, were quite fresh after the lapse of 300 years *. These facts prove, that a reciprocal action is going on at all times between the carbon of the egg and the oxygen gas of the air, by which carbonic acid is produced; but which action may, at any time, be interrupted, either by closing the pores of the shell, or by abstracting the pure part of the air. The loss of weight which eggs suffer, arises, in all probability, from the escape of their more watery parts, which pass off through the processes of the chorion described above; and, as the same causes which obstruct the exhalation of moisture, seem also to check the emission of carbon, it is reasonable to suppose, that they both proceed from the same individual structure.

154. We have, in the next and last place, to inquire whence issues the carbon which unites with the oxygen gas of the air to form carbonic acid, in the respiration of man and other warm blooded animals. For the most part, this carbon has been supposed to be derived from the venal blood, either in the course of its transmission through the pulmonary vessels of the lungs, or during its circulation through the capillaries of the system: and to the

* Encyclop. Brit. art. Physiology.

presence of this substance in the blood, the black colour of that fluid has been attributed. Formerly, indeed, this blackness of colour was ascribed to the agency of phlogiston, or of hydrogen: but as the existence of the former principle is now altogether denied, and the latter substance can be shewn to reside in the blood only as a constituent part, as it does in the other fluids and solids which are of various colours, we must reject the opinion of its sufficiency to account for the black colour of the blood. Because also red blood became black when placed in nitrogen gas, by some, that gas has been held to be the cause of its blackness; but, according to Mr Davy, no affinity subsists between nitrogen gas and blood *. The same effect takes place also, when blood is exposed to the contact of other gases, and even *in vacuo* without the agency of any gas at all. Dismissing then these opinions concerning the cause of the black colour of the blood, let us next inquire how far it may, with justice, be attributed to the presence of carbon.

155. That carbon is a constituent element of animal substances, is generally allowed; and that it exists in the blood, and is yielded by it to form carbonic acid, have likewise been admitted: but that, to this carbon, the black colour of the blood is, or can be, owing, has not hitherto been distinctly proved. It remains to be shewn in what state this carbon exists, and by what agency it can produce this colour; for in its pure state of diamond, nothing is farther

* Researches, p. 375.

removed from blackness than carbon. The disappearance of the black colour of the blood, as carbonic acid is formed in respiration, is no proof of such blood losing its colour, in consequence of yielding carbon ; for we have seen, that, by living vegetables, by animals, and by the human skin itself, this acid is equally formed where no black blood is present to supply carbon. Dr Lower also relates the case of a young woman bled in the foot, whose blood was as white as milk, and yet she was in good health : and, in two other cases, the blood drawn from a vein in the arm, was of the same complexion *. The loss of the black colour of the blood too may be owing to the acquisition, rather than to the escape, of some colouring matter ; or it may arise from some change in the properties of the blood, which shall vary its power of reflecting the rays of light. Neither because carbonic acid is formed by exposing black blood to the air, can we admit this as a proof, that its blackness arose from the presence of carbon ; for Dr Girtanner found, that this acid was formed equally by arterial blood (101.), which is red : and we have seen it to be produced by the serum (97.), as well as by the crassamentum of the blood. Other agents also, which are totally independent of carbon, are capable of producing this black colour in the blood. Every one knows, that a part exposed to great cold, speedily becomes black ; and Dr Crawford found, that, on immersing a dog, whose temperature was 100°, in water at 45°, the

* Lowthorpe's Abrid. Phil. Trans. vol. iii. p. 239.

blood drawn from his jugular vein, was the darkest he had ever seen *. Lastly, we have endeavoured to prove, that no gases either exist in the blood, or can be transmitted through the cellular and vascular structure interposed between the air and that fluid in the lungs; consequently, no oxygen gas can enter into the blood to unite with its supposed carbon, nor, if such an union did take place, could the carbonic acid be afterwards expelled from that fluid.

156. If, then, the carbon, which unites with the oxygen gas of the inspired air during respiration, come not directly from the blood flowing through the pulmonary vessels, it must in some other way be supplied by the animal system. It is generally admitted, that the cellular surface of the lungs is furnished with exhalent vessels, like every other surface of the body: and indeed, the excessive quantity of fluid which is sometimes poured out by these vessels into the bronchial cells, is not unfrequently the cause of severe disease. This exhalation of fluid from the lungs, contrary to what is the case in other exhalent surfaces, seems to be chiefly supplied by the watery parts of the venal blood; for the pulmonary artery in the lungs, not only terminates into veins, but, like other arteries, and like the branchial artery in fishes (65.), has an exhalent termination also. Dr Thruston injected a coloured liquor into the pulmonary artery of a sheep, and the fluid partly passed into the veins, and in part also escaped by the trachea †:

* On Animal Heat, p. 310.

† De usu Respirationis, p. 48. an. 1671.

and Dr Hales having filled the lungs with air, after pouring water into the pulmonary artery of a calf, observed the water to flow so freely into the bronchial cells, as to run out at the windpipe; but the vessels by which it escaped, were not large enough to permit the colouring particles of the blood to pass *. In like manner, injections will pass from the pulmonary veins into the bronchia; so that the exhalent function in the lungs is carried on by the whole system of pulmonary vessels. And, indeed, if we reflect how much larger a portion of water the venal blood (from the termination of the trunk of the lymphatic system into the left subclavian vein at only a short distance from the heart) must possess in the lungs, than after it has passed those organs and circulated through the whole body, we cannot but admire this deviation from the ordinary laws of the exhalent system, directed as it is, to continue and preserve, in sufficient abundance, an excretion apparently so essential to the existence of living action.

157. These exhalent vessels of the lungs, like those of the skin and intestines, appear to be endued with a power, not only of exhaling water, but likewise of emitting carbon; for water and carbonic acid are expelled from the lungs in respiration, in the same manner as they are produced by the skin (147.) when in contact with atmospheric air. As, therefore, the products of respiration and perspira-

* Statical Essays, vol. ii. p. 72.

tion are in kind precisely similar, we are justified in ascribing their formation to similar laws: and, since it seems to have been demonstrated by direct experiment (150.), that no transpiration of aëriform fluids takes place through the skin, we may presume that none is able to be carried on through the cells and blood-vessels of the lungs. Not only is the cellular surface of the lungs furnished with absorbent and exhalent vessels like that of the skin, but it is supplied from within by the same blood, and exposed from without to the same atmospheric air. It has been shewn also, that the colourless fluids of various animals, are able to effect the same change on the air, as that which is produced by the blood : and that the serum of the blood itself, (which is especially destined to supply the exhalent function), produces on the air the same identical change as it experiences in the lungs: all which circumstances strongly incline us to suppose, that the function of the lungs resembles in kind that of the skin. The proofs likewise already adduced, that the carbon furnished by vegetables (140.), and by the inferior animals, as well those which respire by the skin (142.), as those which breathe by lungs (143.), depends wholly on the due circulation of their fluids, and is, consequently, the result of a living action, are strong presumptive evidence, that the same law obtains in the superior animals, and in man : and seem to authorise the conclusion, that the carbon supplied in human respiration, is truly an animal excretion, carried on by the exhalent vessels of the lungs; and therefore, that it primarily depends, like other excretions, on the due circulation and distribution of the blood, and is more or less affected by all its variations *.

158. In the inferior animals we have seen (142. 3.) that the production of carbonic acid, and consequently of carbon, is very much influenced by external temperature, as this more or less affects the circulation of the blood: and even in the human subject, it has been shewn (152.), that the same causes possess a decided

* Long since this opinion concerning the source of the carbon furnished in animal respiration, was entertained by the author, his friend Dr Nugent has pointed out to him a passage in a French writer, wherein a similar notion is distinctly stated. The author, M. Caron, in opposing the opinion of Dr Goodwyn concerning the entrance of oxygen gas into the blood-vessels, and the emission of carbonic acid from them, has these words:—"Je vais demander ici à nos chymistes modernes, si, pour qu'il se forme de l'air fixe dans la respiration, il ne suffit pas que l'air vital puisse se charger, se souler de l'humeur qui sort des vaisseaux exhalans des poumons: pour moi, je crois que cet effet peut s'opérer tout bonnement de cette manière, sans que l'air vital communique en quelque chose avec le sang: Je suis d'autant plus fondé à le croire, que Goodwyn, ainsi que tous les chymistes modernes, avouent qu'ils ne connoissent pas encore la route que l'oxigène peut prendre pour y parvenir, ni par quelle vertu il peut agir sur lui." In a subsequent paragraph also, he asks, whether, instead of saying with Goodwyn, that a certain quantity of oxygen gas is separated from the inspired air in the lungs by respiration, and a certain quantity of carbonic acid substituted in its place, it would not be better to say:—"Une certaine quantité de gaz oxigène constituante l'air atmosphérique, se charge dans les poumons d'une certaine quantité de l'humeur de la respiration, qui le metamorphose, et en change tellement la nature, que l'oxigène n'est plus reconnoisable, et qu'il est devenu d'acide carbonique *."

* Recherches Critiques, par J. C. F. Caron, p. 29. 51. an. 1798.

influence over the formation of this acid by the skin. M. Jurine not only proved the greater consumption of oxygen gas by the skin during a vigorous action of the cutaneous organs, but likewise shewed, that increase of temperature, muscular exertion, and digestion, had similar effects on the products of respiration. These experiments were followed out with greater accuracy by MM. Lavoisier and Seguin, who found, that a man at rest, with an empty stomach, and in a temperature of 82°, consumed in an hour 1210 cubic inches of oxygen gas; but that during digestion, the consumption amounted to 18 or 1900 inches. With an empty stomach, and during violent exercise, 3200 inches were consumed in an hour; and when the same exertion was used after taking food, the quantity was increased to 4600 cubic inches *. The difference in the results of these experiments, is so very great, as to lead to a suspicion, that some considerable error exists; but, at all events, they seem to prove, that whatever, in a certain degree, accelerates the circulation of the blood, increases the consumption of oxygen gas in the lungs, and as this gas, in natural respiration, disappears only in consequence of its union with carbon, it follows, that the carbonic matter must be proportionally increased, which is a necessary consequence of its being considered an animal excretion, immediately dependent on the motion of the blood.

159. But, granting unto the several classes of vegetables and animals which have passed under our re-

* Mém. de l'Acad. de Scien. 1789, 1790.

view, and to the surfaces of the human lungs and skin, the power of emitting carbon, it follows, since this emission is carried on through the whole period of living action, and is essential to the continuance of it, that some ulterior source must be provided, from whence its supply may be duly maintained. In seeds, this carbon forms a considerable part of their substance, and, during the whole period of germination, its proportion is constantly diminishing (20.), by uniting with the oxygen gas of the air. Before this supply is exhausted, roots are sent off from the seed into the earth, by which it draws in nutritious matter, a large portion of which is considered to be carbon; and this carbonic matter undergoing the ordinary changes in the assimilating organs, is, in part, like other excreted substances, thrown off by that expansion of the seed which now forms the plant, and flourishes in the open atmosphere. So long as water and heat are duly supplied, so long does living action continue, of which the absorption of carbonic matter by the roots of plants, and its expulsion by the leaves, are natural and necessary consequences; but the various intermediate changes which it is made to undergo, we do not pretend to explain: they belong, in fact, to another branch of inquiry, and cannot be understood until the functions of nutrition and secretion be more fully ascertained.

160. In the inferior animals, carbon is likewise a necessary constituent substance; and certain of the laws by which it is expelled from them, we have attempted to ascertain. By them too, the means of acquiring it, must be as constant as its expulsion, during living action, has been shewn to be; and

from no other source than through the organs of digestion and secretion can it be conceived to be derived. But our knowledge of the theory of these functions in animals, as in vegetables, is extremely limited and imperfect: and while it so continues, no rational explanation of this matter can be expected or obtained.

161. Nearly the same remarks may be made concerning the primary and original source of carbon in the superior animals. To the organs of digestion, assimilation, and secretion alone, are we enabled to trace it; but the mode in which it is reduced to that state in which it is afterwards expelled by the surfaces of the lungs and skin, involves a knowledge of the nature and qualities of our food, of the various and successive changes which it is made to undergo in the system, and of its distribution by the blood to the different organs of secretion, according to the several uses which it is afterwards destined to answer: concerning all of which subjects, we have of late succeeded in getting rid of much error and absurdity, but have not, in any instance, attained to complete knowledge.

CHAP. VI

OF THE PHENOMENA WHICH ARISE FROM THE CHANGES INDUCED ON THE AIR BY THE LIVING FUNCTIONS OF VEGETABLES AND ANIMALS.

162. Having, in the foregoing chapters, endeavoured to trace the immediate source of the carbon furnished by vegetables and animals in the several processes above described, and the changes which it effects in the air that surrounds them, we are prepared next to inquire into the phenomena which arise from, and always accompany these changes. Our conception of these phenomena will be much aided by premising a few fundamental facts regarding the doctrine of heat with which modern chemistry has made us acquainted.

163. The sensation of heat which we experience from certain bodies, is known to depend on the operation of a power to which chemists have applied

the term caloric ; and the same power, when it enters into bodies, not only raises their temperature, but almost universally expands them : hence the sensation of heat, temperature, and expansion, are considered as effects, of which caloric is the immediate cause *. The quantities of caloric contained in homogeneous bodies are proportioned to their temperatures and quantities of matter ; but, in other cases, it is established as a general law, that different bodies in equal quantities, whether estimated by weight or volume, contain unequal quantities of caloric †. This property or power in bodies to contain very different quantities of caloric, has, by some, been termed the *capacity* of bodies for heat, while others express the fact more simply, by employing the phrase *specific caloric* to denote the relative quantities of heat which different bodies contain.

164. The greater the quantity of caloric that enters into bodies, the more do they become expanded ; and if this expansion proceed to a certain ex-

---

* This distinction between heat, and the effects which it produces, did not escape the observation of the ancients. Theophrastus, in his book *De Igne*, says Bishop Berkeley, distinguisheth between heat and fire. The first he considers as a principle or cause, not that which appeareth to sense as a passion or accident existing in a subject, and which is in truth the effect of that unseen principle. This invisible fire, he adds, is present in all parts of the earth and firmament, though, perhaps, latent and unobserved, till some accident produceth it into act, and renders it visible in its effects *.

† Murray's System of Chemistry, vol. i. p. 350.

* Siris, par. 157, 176.

tent, solids are converted into fluids, and fluids into vapours and airs. The rarity and specific caloric of bodies, however, though intimately connected, are by no means proportional to each other. Solid bodies contain less caloric than fluids, and fluids less than airs; and, when a change of form in these bodies takes place, caloric either enters into, or is given out by them. Dr Black, whose admirable discoveries relating to heat, form so brilliant a period in the history of modern chemistry, found, that a quantity of heat, equal to 140° of Fahrenheit, was required to convert ice into water, although the water still continued at the temperature of 32°: and to raise water into vapour required 800° more, although the temperature of the steam did not exceed that of boiling water. As the caloric employed in producing these changes of form, did not sensibly raise the temperature of bodies, he called it *latent heat:* and he shewed that this latent heat was again given out when the vapour was reduced to water, or the water passed into the state of ice. The permanently elastic fluids are subject to the same laws; for when on being disengaged from a combination, they assume a gaseous form, a large quantity of caloric, in a latent state, unites with them, which is again liberated, whenever, by compression, or by chemical combination, they are reduced to a denser state.

165. Many experiments have been made to discover the comparative quantities of heat, or the specific caloric which, at a given temperature, equal weights of various solid, liquid, and aëriform bodies contain. Taking the specific caloric of water as 1.0000, Dr Crawford found that of arterial blood

1.0800; and that of venal blood 0.8928. Amóng elastic fluids, the specific heat of oxygen gas, compared with that of water, is as 4.7490 to 1.0000: of nitrogen, the specific heat is 0.7936: of atmospheric air, 1.7900: and of carbonic acid gas, 1.6454 *. This high specific heat of oxygen gas, is productive of very important effects in the operations both of nature and art. The power of this gas to supply heat, says M. Berthollet, is well known, and there is no substance which suffers so much of it to escape by the changes of its constitution. In the formation of water by the combustion of oxygen and hydrogen gases, the greater part of the heat might at first be supposed to proceed from the condensation of the latter; but solid bodies, as phosphorus, occasion the greatest emission of heat by combining with a given quantity of oxygen; and when water is decomposed by sulphuric acid and iron, much heat is given out, and nevertheless all the hydrogen of the water resumes its elastic form. This heat therefore must, he adds, have proceeded from a change in the state of the oxygen of the water, which gives only a small part of that which it yields, by combining with the iron, to enable the hydrogen to assume the gaseous form †. We now proceed to apply these facts to the illustration of our present subject.

166. Through the whole of our inquiry it has appeared, that the oxygen gas of the air is converted into carbonic acid by the living processes which have

* Crawford on Animal Heat.

† Chemical Statics, vol. ii. p. 15.

fallen under our examination; and since, from what has just been stated, the specific caloric of this acid is found to be very little more than one-third of that which the oxygen gas itself previously contained, it necessarily follows, that a large quantity of caloric is liberated whenever this conversion of gases takes place. For this office it appears, from the observations of Berthollet, that oxygen gas is peculiarly fitted, since no other substance gives out so much caloric by the changes of constitution which it suffers: and its powers, in the particular instance of change which we are now considering, may perhaps be still farther aided by the degree of condensation which we have seen it in all cases to undergo. Let us then inquire, whether the presence of caloric be manifest in the several living processes above mentioned, and how far it may be deemed to arise from the changes effected on the oxygenous portion of the air; and first of all in the instance of germination.

167. Some peas, which had been steeped in water 36 hours, were placed in a small glass-jar standing in a room at temperature 63°. In this situation they remained till signs of germination appeared, when the bulb of a very delicate thermometer was plunged into the midst of them, which had previously been compared with another standing beside it. The standard thermometer continued stationary for more than an hour; but the one that was plunged among the peas, although at first 0.4° below the former, rose in 20 minutes 0.2° above it, and, in 40 minutes, was 0.4° above it; but at the end of half an hour more, it had again fallen to 0.2° of superiority only.

In a second trial, after turning the peas out of the jar, and again replacing them with a quantity of fresh air, the difference indicated by the thermometer plunged in the peas, amounted, in half an hour, to one degree, but, at the end of another half hour, it was only 0.5°. The amount of this difference must be expected to vary according to the more or less active manner in which the process is going on; but the result seems to prove, that, during the process of germination, a production of temperature actually takes place. The process of malting affords us an example of the same thing. This is conducted by exciting germination to a certain extent in the seed; and, during the process, a considerable production of temperature takes place; so great, indeed, that in certain circumstances, grain improperly kept has even taken fire. From this fact we may conclude, that during the germination of seeds in the earth, a production of temperature likewise takes place *.

168. From what source then is this increase of temperature derived? It cannot proceed from any kind of action in the seed of a mechanical nature, for its structure is not fitted to produce it: neither can it be derived by communication from an external source, for the surrounding bodies are all colder, and are constantly drawing off the heat. Farther, it occurs only while germination is going on; but during that process, the oxygen gas of the air is constantly changing (8. 9.) into carbonic acid by uniting with carbon (20.) furnished by the seed. In

* Thomson's Chemistry, vol. iv. p. 238.

suffering this change, however, the greater portion of its specific caloric is necessarily yielded (166.) by the oxygen gas, and a rise of temperature in consequence takes place; and hence, therefore, we must ascribe the production of temperature which occurs in germination, to the decomposition of the oxygenous portion of the air, by the agency of the living seed.

169. As the same changes are produced on the oxygen gas of the air by the growth of plants (30. et seq.), as by the germination of seeds, the same extrication of its heat may reasonably be expected to follow. The sudden thaw of snow lying on grass, while that on the adjoining gravel continues unthawed; the fact of the moisture of dead sticks freezing, while growing twigs are not at all affected; and of herbaceous plants resisting often degrees of cold which freeze large bodies of water,—all, says Sir Charles Blagden, seem to shew that vegetables possess a power of generating heat *. Mr Hunter also found the leaves of plants to resist freezing longer than water. A growing fir-shoot, and the leaf of a bean, were laid on a cold mixture of the temperature of 28°, and in some minutes they had thawed the surface on which they lay. This did not arise from the greater warmth of the leaves when first applied, for, on removing the fir-shoot to another part, the same effect was produced †. By applying likewise thermometers to the internal parts of vegetables, he discovered them to possess a temperature above

* Philosophical Transactions, 1775. † Ibid.

that of the atmosphere, but less than that of cold-blooded animals. When the atmosphere was below 56°, the temperature of the tree was always above it; but when the weather was warm, the heat of the tree was several degrees lower *.

170. But the most remarkable and decisive facts, relating to the temperature of vegetables, are contained in the following account of M. Hubert, concerning the heat given out by the *spadices* of the *arum cordifolium* during the process of fecundation. This plant grows in Madagascar and the Isle of France: its flowers exhale a strong and rather pleasant odour. About sun-rise, M. Hubert tied five spadices, which had unfolded during the night, round the bulb of a thermometer, and the mercury rose to 44°; while another instrument of comparison, at six o'clock in the evening, was only 19°. At eight next morning, the standard thermometer was only 21°, and that used in the experiment had fallen to 42°; and by nine at night had sunk to 28°, while the first remained at 21°. The next day, at nine in the morning, the thermometer of experiment followed the ordinary course. These trials were repeated seven or eight times with similar results. When surrounded with very fine spadices, the mercury rose to 45°, but reached only to 42° with the smallest; and, in one instance, where the thermometer was surrounded by twelve flowers, the *maximum* of heat was 49.5°. The male parts of six spadices raised the thermometer to 41°, while the same number of the female parts of the flowers raised it only to 28°

* Encyclop. Brit. art. Physiology.

or 30°. No variation in the results occurred, whether the experiments were repeated in a dry room, or under the shade of thick and humid trees.

171. M. Hubert next endeavoured to ascertain the part of the spadix to which this increase of temperature was owing. He had found that the *medulla*, or pith, raised the thermometer, when plunged into it, in the same way as the exterior surfaces had done; but reflecting that the heat of the pith might arise only from the exterior surface, he removed this latter from four spadices without touching the medulla. These medullæ were then tied round a thermometer, which, at sun-rise, was at 17°, but no sign of increased temperature occurred during 24 hours, and the uncovered spadices withered towards the middle of the day. At the same time, he tied the removed surfaces of the spadices round the bulb of another thermometer, and it rose to 39°. This he repeated several times, which convinced him, that this singular faculty is possessed by the exterior surfaces of the spadices, and within the thickness of $\frac{1}{12}$ of an inch at most. If the spadices of the plant were divided some time before the development of this heat, the fluid that escaped from the divided portions was colourless, which is not the case when the heat has been previously given out.

172. Lastly, M. Hubert made experiments to discover the circumstances necessary to the production of this great increase of temperature. He found that if the spadices were closely covered with a cloth dipped in olive oil, grease, or tallow, no increase of temperature took place: and that if, at their highest temperature, they were plunged into cold water,

their heat quickly disappeared, but again returned, in 25 or 30 minutes, on their being withdrawn. When they were covered with oil or honey, the production of heat in the spadices was suspended for about an hour; and a spadix covered with starch gave no indication of increased temperature till its covering dried and fell off in small portions. Other species of the same genus were found to possess similar properties of producing heat; and it is concluded, that this property is confined to the outer surface of the spadix, that it is independent of light, but that the contact of air is necessary to its production. It was moreover proved, that the air in which these flowers had grown, had suffered considerable changes: for it extinguished a lighted taper, and a chick was suffocated in a closed jar in which several spadices had remained five hours, but recovered on being withdrawn *. These facts, in connection with those before related, sufficiently prove that vegetables possess, during their growth, a temperature above that of the ambient air, and that the contact of this air is essential to its production; and, since it has been shewn, that the oxygen gas of the air is converted (38.) into carbonic acid, by carbon exhaled (141.) by the living plant, whereby the greater part of the specific caloric (166.) of that gas is disengaged, it is to this change of composition in the air, and consequent extrication of its heat, that the increased temperature observed in plants, during their vegetation, is to be ascribed.

* Voyage dans les Isles des mers d'Afrique, par M. Bory de St Vincent.

173. We proceed next to speak of the heat of those animals, which, from the low temperature they possess, have been denominated cold-blooded, and this we shall do in the same order in which they have been already considered. With regard to *insects*, Dr Martine observes, that the whole tribe is commonly brought under the class of cold animals. Caterpillars have but a small degree of heat, about a division or two, above that of the air they live in; but the heat of a swarm of bees raises a thermometer buried among them, above 97°,—a degree of heat nothing inferior to our own *. Some insects, as flies and wasps, can sustain a loss of heat without losing life, but a bee cannot †. Coleopterous insects become torpid at 34°: at 36°, they move slowly and with difficulty, and, at a lower temperature, their muscles cease to be irritable ‡. During their state of dormancy, the chrysalides of many insects may be frozen without destroying their power of recovering action §.

174. In the class *vermes*, Spallanzani observes, that when a snail or slug is insulated in a jar of atmospheric air, a thermometer placed in the jar will continue stationary; but when several are confined together, the mercury rises one-tenth, one-seventh, and even one-fifth of a degree, and in oxygen gas one-third of a degree; from which he concludes, that snails and slugs, in decomposing oxygen gas,

* Martine on Thermometers, p. 140, 141.

† Hunter's Observations on Animal Œconomy, p. 108.

‡ Carlisle Phil. Trans. 1805, p. 25. § Ibid. p. 18.

give out caloric enough to be sensible to the thermometer *. This experiment we repeated, by confining several snails in a pint jar of air, from the top of which a small thermometer was suspended, and at the bottom a glass of lime-water was placed. A film of carbonate of lime soon overspread the lime-water, the inside of the jar was dimmed by moisture, and the mercury in the thermometer rose at the same time nearly one degree. Dr Martine says, that from the result of several trials which he made, snails were about two degrees warmer than the air †. Mr Hunter found the lungs of snails 38°, when the atmosphere was 34°; and, in other instances, snails were six and seven degrees above the atmosphere, when it was so low as 30°. Earth worms he found 58.5°, when the atmosphere was 56°; and, in other trials, the worms exceeded by four, leeches by three, and slugs by four degrees the temperature of the ambient air ‡. The temperature of a snail, which was 44°, sank, on exposure to a cold mixture, down to 31°, and then froze; and several leeches froze likewise when reduced to 31° §. In all these experiments, the animals, when thawed, were found to be dead; but Mr Carlisle says, that the garden snail may be frozen, during its state of dormancy, without destroying its muscular irritability ‖.

---

* Memoirs on Respiration, p. 255. 258.

† On Thermometers, p. 141.

‡ Treatise on the Blood, p. 298. et seq.

§ Observations on the Animal Œconomy, p. 105.

‖ Philosophical Transactions, 1805, p. 18.

175. The class of fishes possesses also a temperature above that of the medium they inhabit. In flounders, whitings, cod-fish, and haddocks, the temperature, according to Dr Martine, was scarcely a degree more than that of the water they were swimming in, even when that was so low as 41°. Nor are the red-blooded fishes much warmer than the white ones; for trouts were but 62°, when the river water they were swimming in was 61°; and carp and eels hardly exceeded the heat of the water they inhabited *. Mr Hunter, however, found the mercury in the thermometer to stand at 69° in the stomach of a carp, when the water in the pond from which he was taken, was only 65.5°. That fishes part with this excess of heat to the colder bodies which surround them, we learn also from the experiments of the same author. He put two carp into a glass-vessel with common river water, and then placed the vessel in a freezing mixture: and, as the water in the vessel did not freeze fast enough, snow was added so as to render the whole thick. The snow round the carp melted, and more being added, it melted also, till at last he grew tired of putting it in, and left them to freeze by the joint operation of the freezing mixture, and the natural cold of the atmosphere. At length they froze, but did not, when thawed, recover life with flexibility †; but, that both snakes and fishes, after being frozen, have still retained so much of life, as

---

* On Thermometers, p. 141.

† Observations on the Animal Œconomy, p. 89.

when thawed to recover vital action, is a fact, says Mr Hunter, so well attested, that we are bound to believe it.

176. Amphibious animals exhibit a great variety in the structure of the respiratory organs, and, consequently, in the degrees of animal heat. Frogs and land tortoises possess a temperature about five degrees higher than that of the medium they inhabit, according to Dr Martine. The same may be said of sea tortoises, toads, vipers, and all the serpent kind, all of whom have lungs of the same fabric, and the same cold constitution of body *. Mr Hunter observed, that the frog and toad were about four or five degrees warmer than the atmosphere when it was at 35° or 36°: and that some hours after death, they gradually fell down to the temperature of the surrounding air †. This difference of temperature appears to increase in a warmer atmosphere: for Mr Carlisle kept three frogs for many days in an equable atmosphere of 54°, and their stomachs preserved a temperature of 62° ‡. In an atmosphere of 58°, Mr Hunter found the thermometer, introduced into the stomach of a healthy viper, to stand at 68°; but, after the animal was put into a pan, and the pan into a cold mixture of 10°, where it remained about ten minutes, the heat was reduced to 37°, and in twenty minutes more to 31°, nor did it sink lower: its tail now began to freeze,

* Essay on Thermometers, p. 142.

† Treatise on the Blood, p. 298.

‡ Philosophical Transactions, 1805.

and the animal was very weak. A frog also, whose temperature was 44°, when put into a cold mixture, soon fell down to 31°; and beyond this point it was not possible to lessen the heat without destroying the animal *. A toad being placed in cold water just deep enough not to cover his mouth, the whole was put into a cold mixture between 10° and 15°. The water froze around the toad, and, as it were, closed him in, but he did not die, and therefore was not frozen. Why the animals, mentioned in these experiments, died before they were frozen, while those which are exposed to the atmosphere in very cold climates do not die, is a point which Mr Hunter does not pretend to determine; not knowing the difference, he says, between the effects of a natural and artificial cold †.

177. The experiments of Mr Hunter farther prove, that the temperature of most of the foregoing animals not only falls rapidly in a colder medium, but that it rises more quickly in a warmer one than that of those which possess a higher standard temperature. In the stomach of a frog, the thermometer rose from 45° to 49°: the animal was then placed in an atmosphere made warm by heated water, where it remained for twenty minutes, and, upon introducing the thermometer again into the stomach, the mercury rose to 64° ‡. A healthy viper was put into an atmosphere of 108°, and, in seven minutes, the heat of the animal, both in the stomach and

* Observations on Animal Œconomy, p. 104.

† Ibid. p. 89, 90. ‡ Ibid. p. 90.

anus, was found to be 92.5°, beyond which it could not be raised in the above heat. An eel, very weak, whose heat was 44°, which was nearly that of the atmosphere, was put into water heated to 65°, for fifteen minutes; and upon examination, it was of the same degree of heat with the water. The heat of a tench was, in ten minutes, raised from 41° to 55° both in the stomach and rectum, by being put into water at 65°. He found also, that a living and a dead tench, and a living and a dead eel, put together into warm water, received heat equally fast: and when they were exposed to cold, both the living and the dead admitted the cold likewise with equal quickness *. Hence, therefore, the animal heat, in all the classes of animals hitherto mentioned, whether they inhabit the air or the water, seems to follow nearly that of the medium in which they are placed; and their standard temperature cannot, in consequence, be restricted to any fixed point, but must be considered always in relation to that of their surrounding medium.

178. Notwithstanding, however, the low degree of heat which these several classes of animals possess, hardly, in some instances, exceeding that of the medium in which they live, yet this small excess is a proof that they possess within themselves a power of producing heat. The loss of heat which insects (173.) suffer under cold, the fall of temperature which many of the vermes class (174.) undergo from the same cause, the melting of snow (175.) by

* Observations on Animal Œconomy, p. 104, 105.

the heat of fishes, and the decline of animal heat which the amphibia (176.), when exposed to great cold, experience, all demonstrate, that the surrounding medium, whether it be air or water, is constantly drawing off their heat, which renders necessary as constant a reproduction of it.

179. By what process, then, or from what source, is this superiority of temperature derived? It does not, says Mr Hunter, depend on the motion of the blood, as some have supposed, because it likewise belongs to animals which have no circulation: neither can it be said to depend on the nervous system, for it is found in animals which have neither brain nor nerves. It is probable, he adds, that it arises from some principle, so connected with life, that it can and does act independently of circulation, sensation, and volition; and is that power which preserves and regulates the internal machine *. This supposition in no degree removes the difficulty. Of the principle of life, or that power which enables organized bodies to exhibit living action, we can form some idea, although we know not its nature, just as the astronomer speaks of gravitation without pretending to define what it is; but of another principle connected with life, and producing sensible effects in the system, independently of all the animal functions, we certainly know nothing; and, fortunately for science, the admission of such imaginary principles, or agents, is now banished from all chaste philosophy. Admitting also, with Mr Hunter, the insuffi-

* Observations on the Animal Œconomy, p. 91.

ciency of the motion of the blood to account for animal heat, it does not follow that the temperature of animals is wholly independent of it. Dr Jenner, whose discovery of vaccination has conferred so much honour on himself, and such incalculable benefits on mankind, at the request of Mr Hunter, examined the temperature of a hedgehog in its torpid state. In an atmosphere of 44°, its temperature was in the pelvis but 45°, and at the diaphragm 48.5°: and when the atmosphere was reduced to 26°, the heat of the hedgehog in the abdomen did not exceed 30°; but in summer, when the air is at 78°, and the animal in an active state, its temperature rises to 95° in the pelvis, and to 97° at the diaphragm *. It is sufficiently evident from these facts, that, during a state of torpor, the temperature, even of the warm-blooded animals, exceeds only in a small degree that of the atmosphere by which their torpor is induced.

180. But on passing into this torpid state, under which the temperature so greatly falls, the motion of the blood in animals gradually declines (142. 3.), and at length, in some cases, wholly ceases. All the secretory functions of the animal must, at this period, be suspended, in consequence of which the air in contact with it (142. 3.) undergoes no change; but when heat is restored, the blood again renews its motion, the secretory functions return, and the air undergoes its accustomed changes. These changes consist in the conversion of its oxygen gas (53. et seq.) into carbonic acid, by carbon emitted (145.) by

---

* Observations on the Animal Œconomy, p. 99.

these several classes of animals through the medium of their respiratory organs. By these means, a quantity of the specific caloric of that gas (166.) is at the instant set free: and to this constant liberation of caloric by the perpetual decomposition of the air, do we ascribe that superiority of temperature above the surrounding medium which these animals, as well as vegetables (168. 172.) during the continuance of living action, are enabled to exhibit and preserve.

181. We now pass on to the consideration of animal heat in the warm-blooded animals, including the mammalia and birds. In man, the ordinary standard temperature near the surface of the body reaches to about 98°; in most of the *mammalia*, it somewhat exceeds that of man, and in birds it rises still higher than in the mammalia. Dogs, cats, sheep, oxen, and swine, raise the thermometer four or five divisions higher than man, viz. to 100°, 101°, 102°, and some to 103°, or a little more *. The heat of a puppy, at the diaphragm, was found by Mr Hunter to be 102°: that of a mouse 99°: in the right side of the heart of a dog, the temperature was 101°: in the rectum of an ox 99.5°: and the same in the rabbit †. Mr Carlisle found the temperature of the flowing blood in the horse to be 103°: in pigs, it was 106° and 107°: in the ox, 103°: in sheep, 105.5°, which temperatures, as he observes, are considerably higher than the common estimation ‡. In

---

* Martine on Thermometers, p. 145.

† Observations on the Animal Œconomy, p. 100. et seq.

‡ Philosophical Transactions, 1805, p. 22.

those quadrupeds also which mostly inhabit the waters, the temperature is quite as high. Dr Martine found the heat of the skin of a sea-calf to be near 102°, and, in the cavity of the abdomen, it was about a division higher *. Such, too, is the case in the cetaceous order of animals, the temperature of the whale and porpoise being as high as that of land animals. In all these animals, the organs of respiration and circulation, in their general structure and actions, resemble nearly those of man, with such difference only as the peculiar configuration and modes of life of the animal necessarily introduce.

182. But in birds, which have been stated to possess the highest temperature of all animals, the lungs are differently constituted, and are much larger in proportion to the animal than in other cases. Instead of lying loosely in the chest, and yielding to its alternate contractions and dilatations as in the mammalia class, they adhere to the thin transparent membrane which covers their lower surface, and performs the office of a diaphragm: besides this attachment, they are also connected to the ribs and sides of the vertebræ. In the larger cavities of the body, and in the interstices about the breast and axilla, are placed air-bags of different sizes, some of which communicate immediately with each other, and all may be said to have a communication by means of the lungs. The bones also, which, in other animals, are filled with medullary matter, are, in birds, receptacles for air. Some of these, as the sternum, ribs, and verte-

* On Thermometers, p. 146.

bræ, have their internal substance divided into innumerable cells; while others, as the shoulder and thigh bones, are hollowed out into one large canal, with sometimes a few bony columns running across at the extremities; and, at that end of the bones next to the trunk of the bird, several holes or openings are placed. There are openings in the lungs, by which the air they receive is transmitted to other parts; and the diaphragm also is perforated with holes of a considerable size, to each of which is joined a distinct membranous bag, which, being continued through the whole of the abdomen, is retained in its proper situation, by being attached to the back or sides of that cavity. Each bag receives air from the lungs through its respective opening, and such bags extend over the whole abdomen. At their superior part, the lungs communicate with the large cells of a loose net-work, situated on the anterior part of the breast; and when these are distended with air, the size of the part is considerably increased, as is plainly seen in the turkey-cock, the pouting pigeon, and in the breast of the goose when she cackles. These cells in the breast communicate with others in the axilla, which again communicate with the os humeri, by small openings near the head of that bone. The posterior edges of the lungs open into the cells of the bodies of the vertebræ, into those of the ribs, the canal of the spinal marrow, and into the cells of the bones of the pelvis, from which parts the air finds a passage into the cavity of the thigh bone. Thus, the cells, situated in the soft parts and in the bones of birds, can be furnished with air through the medium of the lungs.

183. Mr Hunter, from whom the foregoing description of the respiratory organs of birds is taken, made several experiments upon the breathing of these animals. He found, by making an opening into the belly of a cock, and introducing a silver canula, previously to tying up the trachea, that the animal breathed by this opening, and might have lived, but for an inflammation of the bowels supervening, which, by adhesion, cut off the communication with the air. He next cut the wing through the os humeri, and, tying up the trachea, found that the air passed to and from the lungs by the canal in this bone. The same experiment was made with the os femoris of a young hawk, and was attended with nearly the like success: but the difficulty of breathing was greater than in the former case, and the animal soon died *. From the great size and peculiar communications of the respiratory organs of birds, it may be presumed, that, in proportion to their bulk, they respire a very large quantity of air; and accordingly, it has been found by experiment, that, in a given time, they consume more oxygen than other animals of the same size, and therefore die sooner in a given volume of air. With regard to their temperature, Dr Martine found them to be warmer than quadrupeds by three or four degrees: for the thermometer being lodged in the groin of ducks, geese, hens, pigeons, partridges, swallows, &c. the mercury was raised to 103°, 104°, 105°, 106°, and 107°: and, in

* Observations on the Animal Œconomy, p. 81.

a hen hatching eggs to 108° *. Mr Hunter also found the heat of the hen to raise the mercury from 103° to 104, when introduced within the rectum †.

184. But the degree of heat in animals varies, not only at different times under a change of circumstances, but also in different parts of the same animal, where all the circumstances continue the same. Some examples of this fact have already been incidentally mentioned as occurring in different animals. Under the tongue of a man, Mr Hunter found the degree of heat to be 97 : at one inch within the urethra, 92° : at two inches 93° : at four inches 94° : and at the bulb 97°. The heat of the rectum in the same man was 98.5°. In the mouse, when the atmosphere was 60°, the mercury in the thermometer stood in the pelvis at 96.8°, and at the diaphragm 99°. In the rectum of a dog, the temperature was 100.5° : in the liver, 100.7° : and in the right ventricle of the heart and stomach, it was exactly 101° ‡. The temperature of a horse, killed by dividing the spinal marrow and large blood-vessels, was, in the colon, 98° : in the stomach 101° : and in the spleen 103°, according to Mr Carlisle, when the atmosphere was at 30°. The urine of the same animal was 97°, and the flowing blood 103°. The water flowing from a tapped person was 101°, but, at the surface, the temperature was 96°, the atmosphere at the time being 43° §. From these facts, it is plain, that eve-

* On Thermometers, p. 147.

† Observations on the Animal Œconomy, p. 103.

‡ Ibid. p. 94. et seq.

§ Philosophical Transactions, 1805, p. 22.

ry part of an animal is not of the same degree of heat: and that the more interior and vital parts possess the highest temperature.

185. We have seen that animals which ordinarily possess a degree of heat but little above that of their surrounding medium, very readily adapt their temperature (177.) to all the variations of that medium. In man, however, and other warm-blooded animals, the system is able to bear very great changes of temperature without a corresponding change in the degree of animal heat. Our sensations, indeed, often apprize us of very slight alterations in the temperature of surrounding bodies, when no perceptible difference, ascertainable by the thermometer, exists. These sensations again, are not only influenced by the general healthy condition of the body, but by the habits with regard to heat and cold which we have been accustomed to indulge. Thus persons who clothe themselves warmly, or live generally in uniform temperatures, are affected by slight variations, which others of hardier habits totally disregard: and those parts of the body which are commonly exposed to the irregularities of the season, are less susceptible of the sensations of heat and cold than such as are more protected from them. The sensation also of heat or of cold, when present in any part of the body, is not only in a great degree independent of the actual temperature of that part, but depends immediately on the previous state or condition of the part itself with regard to sensation; for the same substance will feel hot or cold when applied to a part, according as that part previously possessed the sensation of heat or cold relatively to

that substance. If, for example, the hands be greatly cooled, and then plunged into fresh pump-water, the water will communicate the sensation of heat, while the same water, at another time, when the hands were previously warmed, would impress the sensation of cold. In like manner, we are often not conscious of any feeling of cold in our hands or feet, but if we apply them to a less exposed and warmer part of the body, the feeling of cold is impressed on that part. The same fallacy with regard to the actual temperature of the body, as indicated by our sensations, exists, when those sensations are connected with internal causes; for a general feeling of cold not unfrequently extends over the body, where no variation in its actual temperature, or in that of the surrounding bodies, has occurred. "In the beginning of an ague fit, when I was all shivering and under a great sense of cold," says Dr Martine, "my skin was two or three degrees warmer than in a natural healthy state *."

186. But the actual temperature of the human body, is not only in a great degree independent of sensation, but also of the variations in temperature which the surrounding medium suffers. Governor Ellis, in the year 1758, observed, that a person remaining in a medium higher in temperature than that of his own body, preserved, nevertheless, his natural standard heat. Dr Fordyce went successively into three rooms heated to 90°, 110°, and 120°. He staid in the first room five minutes, which gently

* On Thermometers, p. 150.

sweated him: in that of 110°, the perspiration was more profuse, and streams of water ran down his body: in ten minutes more, he entered the room of 120°, and after staying there twenty minutes, the mercury of the thermometer which he held in his hand, and placed under his tongue, stood just at 100°, and his urine was of the same temperature. His pulse was 145, the veins on the surface of the body were much enlarged, the external circulation greatly increased, and an universal redness and strong sense of heat diffused itself over the whole body. He afterwards remained fifteen minutes in a room heated to 139°, and the temperature of his body under the tongue, in his hand, and urine, did not exceed 100°. Sir Joseph Banks, Sir Charles Blagden, and Dr Solander, went afterwards into rooms heated to 212°, the heat of boiling water, where they remained several minutes. The feeling communicated by the air was unpleasant, but easily borne: and respiration was little affected, except in the want of that refreshing coolness which the inspiration of cool air imparts. If they breathed on the thermometer, it sank several degrees; and every expiration was cool to the nostrils, previously scorched by the hot inspired air. The body, to the touch of the fingers, felt cold as a corpse, and the actual heat of the skin, and under the tongue, was 98°. Sir Joseph Banks alone sweated profusely. In a subsequent experiment, Sir Charles Blagden remained eight minutes in a room heated to 260°: the air felt hot, but did not give pain, and the sweating was not profuse: the sensible heat of the body varied but little. For seven minutes, the breathing continued perfectly natural, but anxiety and oppression then came on: the pulse was 144. When the clothes were stripped off, the air at 212° was more disagreeable for five or six minutes, until a profuse perspiration breaking out, gave instant ease: the breathing during this experiment was not oppressed, partly because the pulse was eight beats less in a minute, and partly because the experiment was made with an empty stomach, and the former with a full one *. Dr Dobson also went into a stove heated to 224°, and felt no oppressive sensation of heat, although every metal about him became speedily hot: a porter remained twenty minutes in the stove when heated to 210°, and, on leaving it, the pulse beat 164 in a minute, and the animal temperature rose only to 101.5° †. M. Tillet has observed, that girls accustomed to attend an oven, have borne, for ten minutes, a heat equal to 280° Fahrenheit ‡.

187. Other animals, when exposed to intense heats, exhibit similar phenomena. A bitch, of a moderate size, was put into a room heated to 220°; in ten minutes, she panted and held out her tongue, but shewed no other sign of distress. After remaining in the room half an hour, when the heat had risen to 236°, the basket in which she had been confined, was opened, and its bottom was found wet with saliva. The thermometer being placed in her flank, fell to 110°, only nine degrees above the natural standard heat: and when turned into the cool

* Philosophical Transactions, 1775. † Ibid.

‡ Mem. Acad. 1764.

air, she was as brisk and lively as ever, and apparently not in the least injured by the heat. That the heat of these rooms, was truly indicated by the thermometer, was proved by the effects produced on inanimate matter; for an egg, placed on a tin plate, was, in twenty minutes, roasted quite hard: in thirty-three minutes, a beef-steak was roasted dry, and when the heated air was directed in a stream through a bellows, it was roasted in about thirteen minutes *. On the other hand, the human body, and that of other animals, will bear great degrees of cold, without any actual diminution of its ordinary standard heat; and thus the temperature of man is the same nearly in winter as in summer—in an inhabitant of the frigid as in one of the torrid zone.

188. Since, then, in every climate, the human body, and that of the superior animals, support a standard temperature, which varies but little under every vicissitude of heat and cold, consistent with the due performance of the animal functions, there must exist in man, and in other animals, appropriate organs by which this temperature can be at all times sustained: but no living powers of the animal system appear to be sufficient for this purpose, independent of the concurring aid of external agents. No supposed attrition between the contiguous soft parts of the animal; no friction between the vessels and the globules of the blood; no action of the solid parts upon one another; no circumstances arising

* Philosophical Transactions, loc. cit.

out of digestion or fermentation in the living body; no imagined combustion of phosphorus in the blood; nor liberation of the phlogistic, or any other principle, through the system, can be received as sufficient to account for the uniform height and steadiness of this temperature. As, therefore, the animal system, by virtue of its own powers, is unable within itself to produce this high degree of heat, to what external agent shall we have recourse, and to what organs shall we refer the production of that elevated temperature, which, in all animals, we have seen to have place?

189. In our review of the temperature of the inferior animals, it has been observed, that insects, worms, and fishes, which have no respiratory structure similar to that of the lungs; and the amphibia, the surface of whose lungs, in proportion to that of the body, is comparatively small, and whose blood, at each circulation, is but partially exposed to the influence of the air, possess a degree of heat but little above that of the medium in which they live: while the mammalia class has a temperature considerably higher, and birds, whose lungs bear the largest proportion to their bodies, are the warmest of all animals. The observation of these facts led naturally to the opinion, that the temperature of animals was immediately connected with the function of the respiratory organs; and we have endeavoured to prove, that the small excess of temperature, which not only the inferior animals, but which vegetables also possess, is actually derived from the decomposition of the air by these several classes of beings, so long as living action continues. No explanation,

however, of the mode in which the air contributes to sustain animal heat, was attempted, till after the great discovery of latent heat by Dr Black. That excellent philosopher having already proved, that the change effected in the air by respiration, consisted in the formation of carbonic acid (116.), similar to what happens in many examples of combustion, ascribed the production of animal heat to the decomposition of the air in the lungs, by which its latent heat was rendered sensible, in the same manner as it is given out in combustion. The blood, in its passage through the lungs, had, he conceived, its temperature by this means raised; and thus was rendered capable of communicating heat to all parts of the body, in the course of its circulation through the system. To this it was objected by Dr Cullen, that, if true, the temperature of the body ought to be greatest in the lungs, and to diminish gradually as the distance from the lungs increases, which is not according to fact. This difficulty was removed by the ingenuity of Dr Crawford, who, by a happy extension of Dr Black's doctrine, maintained, that the heat, liberated by the decomposition of the air in the lungs, passed into the blood, and existed in that fluid in the form of latent, or, what is now termed, specific heat, in consequence of which its temperature was not raised; and that this heat, by other chemical changes, was given out by the blood in a sensible form during its circulation.

190. In what manner, then, does the air, breathed by the superior animals, give out its heat, to support that high degree of temperature above the surrounding medium, which they all possess? We

have seen reason to conclude, that the inspired air is decomposed in the bronchial cells (114.) of the lungs, and that all its oxygenous portion which disappears, is converted (123.) into carbonic acid, by carbon emitted from the exhalent surface (157.) of those organs. During this gradual conversion of the oxygen gas, a quantity of specific caloric (166.), much greater than what is necessary to maintain the elasticity of the carbonic acid that is formed, is necessarily set free; and to this excess of heat, thus constantly liberated in the lungs, by the decomposition of the air, do we look as the source of that superiority of temperature, above the surrounding medium, which man and other animals, under every vicissitude of climate, are enabled to exhibit and maintain.

191. But if a quantity of caloric be thus constantly disengaged in the lungs, it may be expected that the blood, in its transmission through those organs, should acquire a certain portion of it. To ascertain this point, Dr Crawford, pursuing the discoveries of Drs Black and Irvine, mixed together certain quantities of water, at the temperature of 53°, with separate portions of arterial and venal blood; and then measuring the heat of the mixture, at different successive periods, till coagulation took place, he found, that the water containing arterial blood preserved a superiority of temperature over that mixed with venal blood; and, from the results of several trials, he concluded, that the specific heat of the arterial blood of a dog, was to that of the venal as 114 to 100, and that of a sheep as 115

to 100, or as 11½ to 10 *. These results derive confirmation from the experiments of Mr Coleman, who, in order to discover the relative specific heat of arterial and venal blood, while yet retained in the system, strangled a cat, and immediately opened its chest, while the blood in the left ventricle was still florid. He then introduced a thermometer, through an opening in the pericardium on each side of the heart, and it stood at 98° : in the left ventricle, the temperature was only 97°, and in the right ventricle it was nearly 99°. In fifteen minutes, however, instead of the right ventricle possessing two degrees of heat more than the left, it was found to have four degrees less. Mr Astley Cooper repeated this experiment in different ways, and found invariably, that although the venal blood was superior in temperature at first, yet before coagulation was complete, the arterial became from three to six degrees warmer †. These facts afford clear and decisive proof, that the specific heat of the arterial blood exceeds that of the venal, and demonstrate, likewise, that this excess is obtained during the passage of that fluid through the lungs.

192. Admitting the lungs, then, to be the organs in which, by a decomposition of the air, the blood, as it passes through them, obtains its heat, it is next required to shew the sufficiency of this decomposition, to supply heat enough for the maintenance of that superiority of temperature, which the warmer

* On Animal Heat, p. 279.

† Coleman on Suspended Respiration, p. 42. et seq.

blooded animals possess. It is now very generally allowed, that the caloric given out during the combustion of bodies, is, like that obtained in respiration, derived from the changes which the oxygen gas of the air is made to undergo. When, therefore, equal quantities of oxygen gas are converted into carbonic acid by combustion and by respiration, equal quantities of caloric may be expected to be set free; and this opinion, too, Dr Crawford attempted by experiment to establish. He found, that 100 ounce measures of oxygen gas, changed into carbonic acid by the respiration of a guinea-pig, shut up in a close vessel, communicated to 31 lb. 7 oz. troy of water, surrounding that vessel, 17.3° of heat; while the same quantity of oxygen gas, converted into the same acid by the burning of charcoal, communicated to a like quantity of water 19.3° of heat; and he concluded, therefore, that somewhat more heat was produced by the combustion of charcoal, than by the respiration of an animal *. M. Lavoisier, likewise, made experiments of a similar nature, but he substituted the melting of ice, as a measure of the comparative heat given out by respiration and combustion, for the raising of the temperature of water; and the conclusion which he drew, when equal quantities of oxygen gas were changed into carbonic acid by the respiration of a guinea-pig and the combustion of charcoal, made the quantity of heat produced by the animal, exceed that by the combustion of charcoal in the propor-

* On Animal Heat, p. 351.

tion of 13 to 10.3 *. Although, therefore, the results, obtained by these philosophers, do not completely coincide, yet in experiments of so much difficulty and delicacy, the small difference that exists is not sufficient to invalidate the general conclusion, That, " when equal quantities of oxygen gas are converted into carbonic acid by animal respiration, and by the combustion of carbon, nearly equal quantities of caloric are set free †."

193. But farther, as the quantity of heat produced, when a given bulk of carbonic acid is formed by the combustion of charcoal, has been demonstrated by Lavoisier ‡; and as the quantity of air usually respired (85.) has been ascertained, it is evident, that the quantity of heat liberated in the lungs, in any given time, could also be found out, were we able to estimate the quantity of carbonic acid contained in air which has been once breathed. Thus, says Dr Menzies, if the bulk of air commonly inspired be estimated at 40 cubic inches, and the number of respirations at 18 in a minute, the volume of air inspired every minute will be 720 cubic inches; of which $\frac{22}{100}$, or 158.4 cubic inches, consist of oxygen gas, the only part of the air changed by respiration. But of the air thus inspired, $\frac{1}{20}$ only, or two cubic inches, are changed in each respiration, which gives for the amount of carbonic acid, formed every mi-

* Mém. de l'Acad. des Scien. 1783.

† Crawford on Animal Heat, p. 353.

‡ Kerr's Lavoisier's Elements of Chemistry, 4th edit. p. 113.

nute, 36 cubic inches, or 51840 cubic inches in the space of a day, equal in weight to 3.9697 lb. troy. But for every pound of carbonic acid formed by the combustion of charcoal, a quantity of heat is given out, according to Lavoisier, sufficient to melt 27.02024 lb. of ice: therefore, 3.9697 lb. of this acid, produced daily in respiration, will furnish heat enough to melt 107.2 lb. of ice; for $27.02024 \times 3.9697 = 107.2622$. Of this quantity of heat, however, a portion, adds Dr Menzies, is carried off in the form of sensible heat with the air that is expired, and another portion is rendered latent by combining with the vapour that issues from the lungs. These two portions, from experiment, he calculates to amount to heat sufficient to melt 32.9833 lb. of ice; which sum, subtracted from 107.2622, leaves a remainder of 74.2789 lb. as the quantity of ice capable of being dissolved by the heat daily set free, by the decomposition of the air, in the lungs of an ordinary man *. How admirably the blood in the lungs is disposed to receive this heat, the vast extent of the cellular surface (114.) of those organs, and the decomposition of the air, and consequent extrication of its heat, going on over every point of that surface, furnish abundant proof.

194. The inferences drawn from the foregoing experiments and calculations appear to be just, if the premises on which they are founded be in all respects correct: but neither the quantity of air ta-

* Menzies on Respiration, p. 50. et seq.

ken in at each inspiration, nor the portion of it that is changed, nor the number of respirations made in a minute, are yet finally agreed on; and, indeed, from various causes, they must be subject always to much variation. Greater accuracy, also, in the experiments of Crawford and Lavoisier, as to the comparative quantities of heat, produced by respiration and by combustion, is desirable; and in those of Dr Menzies, the abstraction of heat by the air and vapour expelled from the lungs, appears to be much overrated. In their present state, therefore, these inferences must be considered only as approximations to the truth; but the particulars on which they rest, seem to embrace all the necessary considerations, if in every instance they were accurately determined. At any rate, it may, we think, be safely held, that in the lungs of animals, sufficient air is decomposed to furnish a large quantity of heat; and the fact, that the temperature of all animals is in proportion to the relative capacity which their respiratory organs bear to the body, and to the quantity of air which they breathe in a given time, appears to justify the conclusion, that animal temperature is derived chiefly, if not entirely, from this source alone.

195. But we have seen, that the oxygen gas of the air is converted into carbonic acid by the human skin (147.) and intestines, as well as by the lungs; and in many of the inferior animals, their superiority of temperature is sustained by the changes induced on the air through the agency of the skin alone. As, therefore, the skin, by the same powers, changes the air after the same manner as the lungs, it is reasonable to infer, that phenomena similar in kind,

result therefrom ; and that as insects and worms, by means of their skins, preserve a superiority of temperature over the medium in which they live, so likewise those surfaces of the human body which communicate directly with the air, derive from this source a degree of heat also, by which means they become, in some measure, auxiliary to the lungs *.

196. Besides this production of heat, as the principal phenomenon arising from the change which the air suffers in respiration, it has been supposed, that water also is formed by an union of a portion of the inspired oxygen gas with hydrogen residing in the blood. But Mr Davy remarks, that there are no reasons for supposing any residual atmospheric oxygen to be immediately combined with fixed or nascent hydrogen, or hydro-carbonate, in the venal blood at 98° ; and, consequently, none for believing that water is immediately formed in respiration † ; and Dr Bostock observes, that the discharge of hydrogen from the blood has been admitted without

* This power in the skin to produce heat, naturally occurred to the author from the view which he had taken of the effects produced on the air by that organ. From information which he has since obtained, he learns, that the same opinion was, many years ago, taught by Mr Allen in the lectures on physiology which he delivered in Edinburgh ; and a similar doctrine has been pointed out to him in the "System of Chemical Knowledge" by M. Fourcroy, where, however, it is obscured by the language of that imperfect analogy which chemical physiologists never fail to institute between respiration and combustion.

† Researches, p. 423.

sufficient evidence *, and conceives, with Mr Davy, that the pulmonary exhalation proceeds from a secretion carried on by the vessels of the lungs. Moreover, we have endeavoured to shew, that no oxygen gas can enter into the blood-vessels, but that the whole of it that disappears is decomposed in the lungs, and is actually expended in forming the carbonic acid (123.) which is there produced: so that none remains for the formation of water by uniting with the supposed hydrogen of the blood. Even if oxygen gas did enter into the blood, and hydrogen also existed in that fluid, we have no proof that their affinity for each other is sufficient to form water; for, out of the body, actual ignition, or a great degree of compression, is required to reduce them to a fluid form.

197. If we consider the vast extent of the cellular surface (114.) of the lungs, and the similarity which in its structure and functions (157.) it bears to that of the external surface of the body, we are naturally led to the belief, that the pulmonary excretion, like every other, and especially like that from the skin, is carried on by an appropriate structure, and according to the ordinary laws of the exhalent system. That the great bulk of fluid exhaled from the lungs is derived from this source, none will venture to deny. Why then should we imagine another mode of production for this fluid, which is so totally at variance with it? At least, the insufficiency of the ordinary function to

* On Respiration, p. 129.

furnish all the water exhaled, ought first to be clearly proved. The daily loss of weight which the whole body experiences by the escape of perspirable matter, is estimated by Haller at about 60 oz. in the warmer climates, and from 56 to 30 in more temperate climes *. Of this latter quantity, Dr Hales calculated the loss by the pulmonary exhalation to be about 6 oz. † ; but the more accurate trials of Lavoisier and Seguin make it to amount to about one-third of the whole. The mean loss sustained by perspiration was, according to them, 2 lb. 13 oz., of which the pulmonary discharge was 15 oz. and the cutaneous 1 lb. 14 oz. ‡. When, therefore, we bear in mind, that the surface of the bronchial cells is ten times greater (114.) than that of the external skin, and is, like it, duly furnished with exhalent vessels, we are so far from seeing the necessity of resorting to the supposition that water is formed chemically in the lungs, that we can more readily imagine the pulmonary exhalation to be rated in these estimates below what the extent of its exhalent surface may be considered able to supply.

198. Rejecting, therefore, the opinion, that any water is formed by the chemical union of oxygen and hydrogen in the lungs or blood-vessels, it is not within our plan (which professes to treat only of the phenomena which arise out of the changes the air suffers) to inquire farther into the laws of its pro-

---

* Elementa Physiol. tom. v. p. 58.

† Vegetable Statics, vol. i. p. 10.

‡ Mem. de l'Acad. 1790.

duction. One consequence, however, which seems to follow from considering the aqueous vapour to be wholly emitted by the exhalents of the lungs, we cannot omit to notice; for, on this supposition, there seems no reason to think that any part of the caloric liberated in the lungs by the decomposition of the air, is, as Dr Menzies imagined, employed to raise that fluid into vapour. The power of the skin, when its heat is many degrees below that of the lungs, is sufficient to emit its excretion in a vaporific form, nor is there any reason why the exhalents of the lungs should not, without the aid of a fresh portion of caloric, emit vapour also. In fact, both the external and internal surfaces of the body always do so; and it is only when the exhalation is excessive that the insensible perspiration is condensed into a fluid form.

HERE, for the present, the author closes his inquiry. After the review which has now been taken of the changes induced by living vegetables and animals on the air, it was originally his intention to have entered on an investigation of the effects which these changes produce upon them. Had he pursued the subject, it was his design, by a strict application of the preceding facts, to have attempted the illustration of certain phenomena which occur in germination and vegetation; and likewise in the evolution, continuance, and suspension of living action in the inferior classes of animals. The phenomena of incubation, and the evolution of viviparous animals, would next have engaged his attention; and these would have been succeeded by an endeavour to explain the phenomena of respiration, as connected with the appearances and properties of the blood, and the distribution and maintenance of animal heat. A short view of some pathological states of the system, arising immediately out of the preceding discussions, would then have concluded the whole.

This enumeration, it is obvious, comprehends some of the most curious and important problems in physiology; and many of them are of such a nature as not to admit, in the present state of science, of completely satisfactory proof. All, therefore, that could be hoped for, would be a probable explanation, essentially different, indeed, from all the theories which have been hitherto proposed, but perfectly consistent with the view already taken, and supported and illustrated by the aid of such additional facts and experiments as a farther prosecution of the subject would necessarily introduce.

As, however, the whole of the author's reasoning would be founded on the principles which, in this publication, he has attempted to establish, it is evident that the truth or falsity of the principles themselves, ought first to be ascertained. With this view, he submits them to the judgment of the public in their present form. If they shall be favourably received, and the author be in consequence encouraged to proceed, he will endeavour to execute the remaining part of his plan in the best manner that he is able. If, on the contrary, they shall be shewn to be fallacious, he will at once desist from the farther prosecution of the inquiry : and will thus be spared the mortification of adding to the perplexity of a subject which he was unable to explain.

FINIS.

Abernethy & Walker, Printers,
Old Bank Close.

FARTHER
INQUIRIES
INTO THE CHANGES INDUCED ON
ATMOSPHERIC AIR,
&c.

Abernethy & Walker, Printers,
Lawnmarket.

FARTHER

INQUIRIES

INTO

THE CHANGES INDUCED

ON

ATMOSPHERIC AIR,

BY

THE GERMINATION OF SEEDS,
THE VEGETATION OF PLANTS, AND
THE RESPIRATION OF ANIMALS.

BY

DANIEL ELLIS.

EDINBURGH:

PRINTED FOR W. BLACKWOOD, SOUTH BRIDGE STREET;
AND J. MURRAY, 32. FLEET STREET,
LONDON.

1811.

ADVERTISEMENT.

At the close of his former publication, the Author intimated his intention of prosecuting his inquiries, if the public approbation of his labours should be such as to encourage their continuance, and future experience should appear to confirm his views. An interval of four years has now elapsed since the date of his former Work, during which period many important additions have been made to the various branches of research connected with the subjects discussed in this volume; and the new views, which the late discoveries of Mr Davy have introduced into Chemical Science, may also be expected to extend their influence to all its Physiological applications. From these causes, joined to the Author's endeavours to correct some of his former opinions, and to illustrate and improve others, the additions which he now brings forward, have insensibly acquired a size greater than that of his original Work.

In these additional Inquiries, the Author has greatly extended his researches into the "Changes induced on the Air by the Vegetation of Plants;" and if his success bear any proportion to the pains which he has taken, he hopes that he may be found to have disentangled some of the perplexities, and reconciled some of the differences which have hitherto prevailed on this important subject. The views which he has suggested respecting the "Colours of Plants" arose naturally in the order in which they are presented; and the attempt to connect them with other curious operations in the vegetable economy, will, he trusts, add considerably to their interest and value. If an apology be deemed necessary for the length to which he has extended the section on the "Agency of Light," he would beg to state, that some parts of that section were composed with reference to ulterior views, which, for the present, he has been induced to postpone.

On the second branch of his Inquiries, namely, that which relates to "Animal Respiration," the Author has diligently sought information from all the sources which lay open to him; and the important additions, which the labours of his Contemporaries have enabled him to make to this part of his Work, will, he trusts, be considered not only as valuable in themselves, but as tending to augment the confidence of his readers in the conclusions to which he had before endeavoured to conduct them. To the chapter on the "Source of Carbon," he has also been enabled to add many new facts; and the illustrations which he has now subjoined will, he hopes, remove some objections to his doctrine, and more completely develope and recommend his views.

In contending with so many opposing facts, and such variety of conflicting opinions, the Author can scarcely presume to hope that he has always escaped error; but, he trusts, he may claim the praise of having diligently sought the truth. While too, on all occasions, he has exercised the most perfect freedom of discussion, he hopes that he has uniformly expressed himself in the language of one who would wish always to combine a just respect for the opinions of others, with the attachment he may feel to his own. Throughout his Work, it has been his anxious wish "to render unto Cæsar the things that are Cæsar's;" and if, through ignorance or inadvertence, any fact or opinion should not be assigned to its rightful owner, he entreats the reader to correct his mistake, but not hastily to pronounce it wilful; for he can truly declare, that, though not insensible to the desire of an honest reputation, he would not knowingly invade the mental rights and property of another for all the fame which all the discoveries in all the sciences could ever give.

EDINBURGH,
MARCH 20. 1811.

CONTENTS.

ERRATA.

Page 52 Note, *for* Physiologia *read* Phytologia
—— 72 line 26 *for* vinea *read* vinca
—— 155 Note, *for* tom. iii. *read* tom. iv.
—— 340 —— 25 *for* bottles *read* bottle
—— ibid —— 26 *for* they were *read* it was

Primo enim paranda est *Historia Naturalis*, et *Experimentalis*, sufficiens et bona; quod fundamentum rei est: Neque enim fingendum, aut excogitandum, sed inveniendum, quid Natura faciat, aut ferat.

BACON.

ADDITIONS TO CHAP. I.

OF THE CHANGES INDUCED ON THE AIR BY THE GERMINATION OF SEEDS.

* 200. In our former publication, we distinctly considered the operation of water, heat, and air, in promoting the germination of seeds; and the conclusions to which we then came are, for the most part, acquiesced in by chemists. The facts, indeed, on which they rest, are so numerous and precise, as scarcely to leave room for any difference of opinion respecting them. Such additional evidence, however, as we have since been able to collect in their support, we shall now briefly notice, preserving, as nearly as we are able, the arrangement observed in our former treatise.

201. The presence of water has been stated (1.) as one of the first agents required for the commencement of the germinating process; and the force which seeds, by imbibing it, are capable of exerting, is well illustrated in some experiments made by Mr

* This and the following numbers are continuations of those which mark the paragraphs in the former publication.

Boyle. He filled several strong phials with horse beans, and then pouring water upon them, he corked the phials, and tied them down. As the seeds imbibed the water and increased in bulk, many of the phials were burst asunder. To make a nearer estimate of the expansive force of the swelling beans, he procured hollow cylinders of brass, of different lengths and diameters, to which wooden plugs were fitted, in such a manner as to move freely up and down in the cylinders as the seeds should swell. A cylinder of this kind, six inches in length and two in diameter, was filled with beans, which were then covered over with water; and as the seeds imbibed the water, they raised the plug, although a common half-hundred weight was laid upon it. When the cylinder was less than six inches deep, and was four inches broad, the beans, in swelling, would raise the plug, although loaded with a weight of above one hundred pounds *.

202. We before remarked, that this swelling of seeds took place equally in water that contained air, and in that which had been previously deprived of its air by long boiling. Our observations on this point are confirmed by the experiments of M. T. de Saussure. He found that seeds of coffee, which had even lost the faculty of germinating, exhibited, nevertheless, a partial development of their radicle, not only in common, but in boiled water, and even in vinegar and saline solutions. This enlargement, however, is not to be considered as a true germina-

* Boyle's Works, by Shaw, vol. ii. p. 285. 2d edit.

tion; for it neither changes the taste, the odour, nor any other properties of the seed *.

203. To that swelling of the seed which constitutes germination, M. Scheele considered the presence of oxygen gas in the water to be necessary, and asserted that this gas was converted into carbonic acid†. According, also, to M. de Saussure, peas and other seeds germinate in water that contains oxygen, but do not properly unfold their radicles in boiled water, when the water does not exceed more than seven or eight times the weight of the seed; but when it is one or 200 times as great, the radicles have then been prolonged. This happens, says he, because ebullition does not entirely deprive water of its air, and when, therefore, the quantity of this fluid is sufficiently large, enough of air is present to excite a feeble development of the seed ‡.

204. Water enters into the seed, partly through the pores of its external coat, and partly through the small opening placed in the *hilum* or scar. M. Senebier asserts, that if the opening in this scar be closed, the swelling or germination of the seed is prevented §; but in some experiments made by Dr Jones, this was not found to happen. He placed some large beans in a saucer of mould with the opening in the scar uppermost, and watered them in such a manner that the fluid could not enter it, and yet they seemed to germinate in the ordinary time.

* Recherches Chim. sur la Vegetation, p. 2.

† On Air and Fire, p. 167. ‡ Recherches, p. 3.

§ Mem. Physico-Chimiques, tom. iii. p. 333.

205. The entrance of water, in the first instance, seems to be the effect of simple imbibition; but after penetrating the external coat, the fluid appears to be conveyed by a vascular structure. M. Senebier found, that if seeds, after being immersed in coloured liquors, were cut into thin slices, they exhibited coloured points through the substance of the cotyledons, which indicated a vascular structure. The colour became less and less as it approached the germ; and from the plume of a bean, when cut through, a transparent fluid escaped from five vessels, arranged in a circular order. The two vessels which united the cotyledons to the plantule were full of juice, which, on being held up to the light, exhibited a slight tinge of colour *.

206. The agency of heat in promoting the development of the seed is universally admitted; but the operation of light has been much disputed. From a review of the facts, we were before led to conclude (3.) that light, simply, possessed little power either in promoting or retarding germination. M. Michelotti, however, supports the opinion of M. Senebier, that light is injurious to this process †: but the experiments of De Saussure seem to prove that this arises only from the operation of the heat that exists in the solar beam, when seeds are confined in close vessels; for when the same temperature was preserved, they germinated equally well in transparent and opaque vessels ‡.

* Physiol. Veg. v. iii. p. 362. et seq.

† Philosophical Magazine, v. ix. p. 246.

‡ Recherches, p. 23.

207. The first correct experiments, made to ascertain the specific changes induced on atmospheric air by the growth of seeds, were published by the celebrated Scheele, in his treatise on Air and Fire, about the year 1777. He found that the air, in which moistened peas were made to grow, underwent no change of volume; but that its oxygen gas disappeared, and a quantity of carbonic acid was produced. As the quantity of acid formed equalled in bulk that of the oxygen gas which the air naturally contained, he proceeded to ascertain whether this gas had not been converted into the acid in question. With this view, he mixed together, in a bottle, one part of oxygen with three parts of nitrogen gas, and put some peas into it, with water sufficient to carry on their germination. The peas soon began to grow, and when they grew no more, he observed no variation in the bulk of air, but one-fourth of this air was attracted by milk of lime. It is therefore, says he, the oxygen gas of the air which, by germination, is changed into carbonic acid. This conclusion he farther established by confining peas in pure oxygen gas, where, though they did not grow so well, yet they changed it into carbonic acid; and had he continued the experiment a sufficient time, he had no doubt but that the whole of this gas would have experienced a similar change *.

208. Dr Ingenhousz confirmed and extended these facts, by proving that every kind of air, which was fatal to animal life, was incapable of supporting ger-

* On Air and Fire, p. 151. 156.

mination, either in the sunshine or in the shade; but that seeds grew both in common air and in oxygen gas, and formed carbonic acid in each *. The subsequent experiments of Gough, Cruickshank, and De Saussure; and those which have been already related (9. et seq.), confirm, in all respects, the conclusions of Scheele. In his late Researches, also, De Saussure extended his experiments to a great variety of seeds, and, in all that he tried, he found the bulk of carbonic acid produced to be exactly equal to that of the oxygen gas which had disappeared †. These various experiments seem to establish, beyond the reach of doubt, the facts, that oxygen gas is essential to germination; that it is converted into carbonic acid in that process; and that the bulk of acid gas produced is exactly equal to that of the oxygen gas which disappears.

209. In the former part of our inquiry (12.) we stated the bulk of carbonic acid, formed in germination, to be somewhat less than that of the oxygen consumed, which we ascribed to the *condensation*, supposed, at that time, to attend the conversion of oxygen gas into carbonic acid. We then, however, mentioned many difficulties which opposed the accurate determination of this point; and our opinion at the time, was at variance with the observations of Scheele, of Cruickshank, and of De Saussure. We have since found, in many trials, that no apparent diminution of the volume of air occurs in germina-

---

* Experiences sur les Vegetaux, tom. ii. p. 2. 10.

† Recherches Chim. p. 9. 10.

tion : but in other instances, this has, in a small degree, appeared to happen. Aware, however, of the various causes by which the volume of air may, to a small extent, be affected, and calling to mind the late accurate experiments of Messrs Allen and Pepys*, in which no diminution of volume was found to attend the conversion of oxygen gas into carbonic acid by combustion, as Crawford and others had previously supposed, we do not hesitate to express our conviction, that the bulk of carbonic acid produced in germination is, like that in combustion, exactly equal to that of the oxygen gas which has disappeared.

210. In thus stating the volume of carbonic acid, produced by seeds, to be equal to that of oxygen consumed, it is necessary to bear in mind that we speak of its formation in the process of germination alone. We have before shewn, that moistened seeds equally produce carbonic acid when confined in nitrogen (*5.*), or in tubes filled with mercury (19.); but no sign of development is then observed, and the volume of gas, instead of continuing the same, is greatly augmented. These experiments have been repeated by Mr Acton, who found the formation of carbonic acid by seeds, introduced into a tube of mercury, to commence in a few minutes; and in twenty-four hours the air, on analysis, afforded $\frac{90}{100}$ of carbonic acid, although no evident sign of putrefaction appeared. Hence he contends, that, since seeds thus form carbonic acid when no oxygen gas is present, it must be difficult, if not impossible, to fix

* Phil. Transact. 1807.

the precise period at which germination ceases in oxygen gas; or to determine what part of the acid is really formed by the process of germination, and what by the spontaneous decomposition of the seed. Consequently, the attempts to establish an identity of quantity, between the oxygen lost and the acid produced, must be fallacious, and the conclusion thence derived unworthy of credit *.

211. We readily admit this difficulty in estimating the quantity of carbonic acid; but a due attention to circumstances will, we think, enable us entirely to remove it. To this end, it is important to observe, that, where this acid is formed without the presence of oxygen gas, no apparent change is effected in the seed, but the bulk of air is increased; where, on the contrary, it is produced in that gas, the oxygen disappears, and the evolution of the seed goes on, but the volume of air suffers little or no variation. Whether, therefore, we attend to the sensible effects produced in the air, or to those which occur in the seed, the difference is most striking; and so likewise must be the causes from which such difference proceeds. These causes, we have argued (19.), arise from the seed, in one case, affording only carbon, which unites with the oxygen gas of the air, and converts it into carbonic acid; while, in the other case, this acid gas is at once furnished by the seed alone. The one result, therefore, accompanies the evolution of the seed, the other attends its spontaneous decomposition; but it is only with the former process that we are at present concerned.

* Nicholson's Journal, vol. xxiii. p. 217 et seq.

212. Now, in the experiments on germination already related (8. 9.), the gradual disappearance of oxygen and production of carbonic acid seem sufficiently to establish their conversion into each other; and the stationary bulk which the residual air maintained, after all the oxygen was consumed, together with the equality of volume between the above-named gases, appear, likewise, to prove that none of this acid gas was furnished by the seed alone. If the seed, in its germination, had furnished both the elements of carbonic acid, then, as in its decomposition, the whole bulk of air employed ought to have been increased, which was not the case; neither, on such a supposition, are we able to assign any probable reason why the production of acid should, for a time, have ceased at the precise period when all the oxygen was consumed. We are, therefore, inclined to suppose a period, more or less long, to intervene between the formation of carbonic acid, during the growth of the seed, when oxygen gas is present, and its production by the same body, when the whole of that gas is consumed; between that process which announces the evolution, and that which indicates the decomposition of the seed.

213. Should it, however, be objected to our experiments, that the process of decomposition may instantly follow the termination of living action, and thus confound the results, this objection will not apply to the experiments of M. de Saussure, who analyzed the residual air at intermediate periods of the process, and always found the quantity of carbonic acid formed, and of oxygen gas remaining, to make

together $\frac{21}{100}$ of the whole air employed. Thus, says he, if seeds be made to germinate in 100 cubic inches of air, which contain 21 parts of oxygen and 79 of nitrogen gas, it will be found, that, if the germination has produced 14 cubic inches of carbonic acid, there remain but seven cubic inches of oxygen in the residual air; or, if only seven cubic inches of acid are formed, there will then remain 14 of oxygen gas. The volume of oxygen consumed is, therefore, equal to that of acid gas produced; and this result as yet admits of no exception *. In such circumstances, the carbonic acid, which seeds are capable of affording under decomposition, cannot be supposed to mix with that formed in germination, unless we not only reject the evidence derived from identity of volume between the two gases, but admit the apparent absurdity of believing, that, in the same body, the act of decomposition can consist with the function of life. For these reasons we adhere to our former opinion, that, in germination, carbonic acid is formed only by the union of the carbon of the seed with the oxygen gas of the air, and that the whole of the oxygen that is lost is to be found in the carbonic acid produced.

214. But though, in germination, the conversion of oxygen gas into carbonic acid is by no one denied, yet by some it is maintained that a part only of this gas is so changed, and that another portion of it is *absorbed* by the seed. The vague and indefinite use of the word *absorption* has, perhaps, contributed not a little to obscure the reasonings on this subject,

---

* Recherches Chim. p. 10.

which renders it necessary for us to state the sense in which we employ that term. In every science, precision in the use of terms is essential to correct reasoning; but in physiology, which embraces the properties and habitudes both of living and inanimate bodies, this condition is more indispensably required; and the uniform observation of it demands scarcely less attention than the process of reasoning itself. Add to this, that physiology, besides possessing a language of its own, converses extensively with almost every branch of science, and is therefore perpetually exposed to the danger of ambiguity, from the transference and misapplication of terms. Hence it has happened that its doctrines and its language have inclined sometimes to mechanical, at other times to chemical, science, and, not unfrequently, they have assumed a metaphysical aspect.

215. But whatever meaning the mechanical or chemical philosopher may attach to the word *absorption*, the physiologist can, with consistency, employ it to express only that operation by which fluids are received into the body through a living organized structure, or system of vessels. Such an operation differs from the imbibition of fluids by inanimate bodies, which is sometimes named absorption: it differs also from the attraction of gaseous bodies by fluids or by solids, which often passes under the same name: and, finally, it differs from the ascent of fluids in capillary tubes, which, whatever be its cause, does not partake of the nature of a living action. In all our investigations, therefore, we have constantly used the word *absorption* in its proper physiologi-

cal sense, and have never employed it to express, what we conceived to be, either a mechanical or chemical action.

216. In this view of absorption, although water enters the seed at first, as we think, by simple imbibition, yet there is no evidence that gaseous fluids can be absorbed and decomposed, in the manner which many eminent chemists have supposed: neither, as we have argued (17.), can this oxygen be held to gain admission by the operation of chemical affinity. The only probable reason, in favour of the absorption of oxygen, was derived from the belief that more of that gas was lost than could be found in the carbonic acid produced: but as the facts adduced (207. 8. 9.) seem to demonstrate the fallacy of the experiments on which this opinion was founded, the inference deduced from them can no longer be maintained. Even those, if such there be, who may still be disposed to deny the identity of volume preserved in the conversion of these gases, must, we think, admit the approximation to it to be extremely near; and therefore, the portion of oxygen, supposed to enter the seed, must be so small as to be incompetent to produce the effects which have been assigned to it. And to contend that the oxygen of the air enters the seed merely from certain effects which it is supposed to produce, is to invert the ordinary rules of philosophizing, which require that this alleged agent be shewn to be present in that body, before we venture to describe its operation.

217. But Mr Acton endeavours to support an absorption of oxygen in germination, from the results

of experiments made on the growth of seeds in oxygen gas. For a few days, the volume of gas, in these experiments, diminished, and the oxygen was more or less completely changed into carbonic acid: hence it is inferred that oxygen gas is absorbed by seeds, and this to the amount, in some cases, of one-third of the whole gas employed *. To these experiments many objections occur; for, in the first place, pure oxygen is not the gas in which seeds naturally grow, and no such diminution of volume attends their ordinary germination in atmospheric air. Moreover, a mere diminution is no proof of the absorption of oxygen, in the physiological sense of that term; for, in Mr Acton's experiments, paste and other inanimate substances produced a similar diminution of oxygen, although no living power could then have been exerted. On the other hand, this diminution, when it does happen, appears to be accidental; for in the experiments of Scheele and Cruickshank (6.) no sensible change of volume seems to have attended germination, even in oxygen gas. These experiments, therefore, are not only exceptionable in themselves, but are contradicted by others, and by the results of germination in atmospheric air. They are farther objectionable from not distinguishing between the operations of living and inanimate matter; and, were it even allowed that oxygen was absorbed in these experiments, such a concession would not prove that it is equally absorbed in atmospheric air, where the process is in all respects natural, and the results are altogether different.

---

* Nicholson's Journal, vol. xxiii. p. 229.

218. The important experiments of Messrs Allen and Pepys, to which we have already referred, enable us to make another correction in our conclusions concerning the carbonic acid formed in germination. We before followed the opinion of M. Guyton, that this acid contains only one-fifth of its weight of carbon; but these gentlemen have found that the proportion of carbon comes much nearer to the determination of Priestley and Lavoisier, the former of whom made it one-fourth, and the latter $\frac{1}{3.57}$ of the compound. The average result of their experiments afforded $\frac{28.60}{100} = \frac{1}{3.49}$ of carbon. They ascertained also, with great precision, the specific gravities of oxygen and carbonic acid gases; finding a cubic inch of the former to weigh 0.3382, and a cubic inch of the latter 0.4726 of a grain, which weights, they observe, correspond almost exactly with those previously assigned by Mr Davy.

219. If, therefore, the quantity of carbon, given out by the seeds in our experiments (8. 9.), be recalculated on the suppositions that no change of bulk attends the conversion of oxygen gas into carbonic acid, and that the relative specific gravities of these two gases are such as is above stated, it will be found, that the quantity of carbon, existing in the acid formed by germination, is greater than we before assigned when proceeding on different data, being $\frac{1}{3.18}$ instead of $\frac{1}{4.84}$, as before stated (13.): and, consequently, its proportion approaches nearer to the determination of Allen and Pepys, who, as we have seen, make it to constitute $\frac{1}{3.49}$ of the compound.

220. But now that the specific gravities of oxygen

and carbonic acid, and the composition of this latter gas, seem to be determined with so much accuracy, we can have no difficulty in estimating the actual quantity of carbon which growing seeds afford in a given time, if we know the quantity of oxygen gas that is consumed: for as the bulk of acid produced is exactly equal to that of oxygen lost, the quantity of carbon given out by the seeds will be indicated by the excess of the weight of this acid gas over that of the oxygen. Thus in the experiment already related (8.), twelve peas were put into a jar with 16.5 cubic inches of air, of which 3.46 inches were oxygen gas. By the fourth day, the whole of this oxygen was consumed, and the radicles of the seeds were about an inch in length. Now, as 3.46 cubic inches of carbonic acid were likewise produced, which contain 0.465, or rather less than half a grain of carbon, such may be considered to be the quantity of that substance furnished by these twelve peas in four days; or one pea, in that time, yielded 0.039 of a grain of carbon. Hence the quantity of carbon yielded by seeds, before they become capable of replacing it by absorption from the soil, is extremely small. Dr Thomson observes, that, from a good many trials, made with as much care as possible, the quantity of carbon separated, during the whole process of malting, by the formation of carbonic acid gas, does not exceed two per cent *.

221. With respect to the mode in which the carbon of the seed unites with the oxygen gas of the

* Syst. Chem. vol. v. p. 446. 3d edit.

air, we have, at present, nothing to add to what has been already stated (16. et seq.). We have there argued, that no proof exists of any part of the air being absorbed by, or attracted into the seed; but have supposed that the combination of carbon and oxygen takes place exterior to its vascular structure.

ADDITIONS TO CHAP. II.

OF THE CHANGES INDUCED ON THE AIR BY THE VEGETATION OF PLANTS.

SECT. I.—*Of the Vegetation of Plants in the Shade.*

222. THE necessity of water, heat, and air, to carry forward the process of vegetation, is universally admitted, and the general operation of these agents is abundantly established by the facts (23. 4. 6.) already related. In our former discussion of the specific changes induced on the air by vegetation (29. et seq.), we endeavoured to establish, by experiment, the same general facts as have been described to take place in the germination of seeds; but the facts, which are supposed to lead to an opposite conclusion, are so numerous, and, apparently, so decisive, as still to persuade many eminent chemists, that oxygen gas, instead of being consumed, is really produced by the ordinary process of vegetation. In the present chapter we propose, therefore, to reconsider our own experiments and opinions, and to examine, with more attention, those which are opposed to them, in order to discover, as far as we are able, the causes of these discordant results, and thus to arrive at as much truth

and certainty as our present knowledge of the subject will enable us to attain.

223. It will be seen, as we proceed in this discussion, that our opinions on this subject are by no means so novel as they have been commonly supposed, nor as we ourselves, at one time, thought them to be; and we shall gladly introduce the opinions of preceding writers, not only because they must add weight to our own, but because we are anxious to do entire justice to those who have gone before us in investigating this much controverted subject.

224. The experiments which have been detailed in the former part of our Inquiry (31. et seq.), and which were designed to prove the complete conversion of oxygen gas into carbonic acid by vegetation, were repeated a great number of times, and with every attention to guard against fallacy, which our own experience, and that of many friends who witnessed their progress, could suggest. In a subsequent repetition of these experiments, we varied the mode of analysing the residual air, by first raising the jar, and then passing the alkaline solution (32.) which contained the carbonic acid, into a tube filled with mercury, and inverted in a trough of that fluid. To this solution, a portion of diluted acid was then added, which excited a brisk effervescence, and caused the disengagement of more than two cubic inches of carbonic acid gas. On comparing the volume of this gas with the diminution which the whole air had suffered, we found it to be nearly equal; so that we thus obtained, in a distinct and palpable form, a bulk

of carbonic acid, corresponding, nearly, with the quantity of oxygen gas, which the air, employed in the experiment, actually contained.

225. There is, however, one possible source of fallacy in these experiments, which our friend Mr Murray, who repeatedly witnessed their progress, and has expressed himself fully satisfied of their apparent accuracy, has since suggested, to which it becomes us to attend. "The plants submitted to experiment, both in the experiments of Scheele and of Mr Ellis, were," he observes, "those the white and succulent stems of which are large in proportion to their leaves, such, for example, as peas and mustard. Now, it appears from the experiments of Ingenhousz and Saussure, that it is only by the green parts of plants that carbonic acid is decomposed and oxygen evolved ; while from the white and succulent parts, it is established by Saussure, that carbonic acid is formed by the action of the oxygen of the surrounding air upon them *." A similar objection occurred to Dr Henry, who saw the experiments repeated in June 1807, and satisfied himself of their general accuracy in other respects, by obligingly taking the trouble to analyse the residual air.

226. In order to meet this objection, it is necessary to recur again to experiment, and to employ plants of a greater age, or such as have no white parts belonging to them. With this view, we procured several plants, which grew in small garden pots, and placed them to vegetate in given quantities

* Syst. Chem. vol. iv. p. 53. 1st edit.

of atmospheric air. To accomplish this purpose without injury to the plant, we caused several circular tin dishes to be made, some of which were about six inches in diameter, and one and a half in depth, and others of little more than half these dimensions. Each dish was divided through its middle into two parts, and exactly in the centre of each part a semicircular cut was made, so that, when the two parts were brought together, a circular hole was formed, just large enough to admit the stem of the plant. One of the parts was made a little smaller than the other, so as to pass about an inch within it; and to the bottom of this smaller part a flat piece of tin was partially fastened, in such a manner as to receive the bottom of the larger half in a sort of sheath, by which the two parts were rendered perfectly steady. As the plant grew in the pot, the two parts of the dish, previously separated, were brought together, one on each side the stem, and made to slide into each other without injury to the stem, which rose through the central opening,—while the circular dish itself now rested securely on the margin of the pot. By this arrangement, the plant was left growing in its natural situation, and all sources of fallacy from the mould were effectually removed. The junction of the two parts, and the aperture which received the stem of the plant, were now made water-tight by a proper luting, and the jar was then inverted over the plant into the tin dish, which was now filled with water.

227. One of these dishes was made to inclose a young bean as it grew in a pot, to the height of three

and a quarter inches, and, after the junctures were secured by luting, a small jar, of the capacity of 18.5 cubic inches, filled with atmospheric air, was inverted over it, and the communication with the external atmosphere was cut off by water poured into the dish. In this situation, the plant grew in a room, in full day light, from the 10th to the 14th of August, in a temperature varying from 60 to 65 or 68°. It increased about two inches in height during this period, and at length pressed against the upper part of the jar. The air of the jar was now examined. It did not contain any sensible portion of carbonic acid, which had, probably, been attracted by the water that had entered the jar; but 100 parts of the residue lost only 16 by slow combustion with phosphorus, so that the atmosphere had lost $\frac{5}{100}$ of its oxygen gas.

228. Another young and vigorous bean plant, growing in a pot to the height of seven inches, was confined, in a similar manner, in about 96 cubic inches of atmospheric air, a little more than half a cubic inch of the water of potassa being previously placed in an egg-cup underneath the jar. From the 11th to the 15th of August, the plant grew at the rate nearly of three quarters of an inch a-day, and the water, at the same time, rose gradually into the jar. On the 16th and 17th its growth was much less, and at last it was not perceptible. Some of the lower leaves of the plant now began to look dry and black. In the whole, it had grown in height more than two and a half inches, and the water, when the experiment was terminated, had risen one and a half inch into the jar.

229. By the former experiment (227.), it was proved that the oxygen gas of the air was in part consumed, and the same circumstance may be presumed to have happened in this case, from the gradual decline and final cessation of growth which took place. The formation of carbonic acid may also be presumed, from the continued rise of the water into the jar; but to prove this more clearly, we raised the jar, and passed the alkaline solution into a small jar filled with mercury, and inverted in a bason of that fluid. A small quantity of diluted sulphuric acid was then added to the solution, which caused an immediate effervescence, and the extrication of nearly two cubic inches of carbonic acid gas, which were afterwards entirely attracted, with the usual phenomena, by pure lime water. These experiments therefore prove, that by the growth of young beans, which contain no white parts, but, on the contrary, have both leaves and stems of a deep green colour, the oxygen gas of the air is changed into carbonic acid, in the same manner as by the mustard plants mentioned (32.) in the former part of our Inquiry. Similar results were obtained by placing radishes, a plant of hydrangia mutabilis, and some others, in like circumstances. These experiments were likewise seen by Mr Murray, who considers them as obviating entirely the objection which he had previously suggested *.

230. To remove any objection that may be made to the early age of these plants, or to their being en-

* Syst. Chem. v. iv. p. 47. 2d edit.

tirely of an herbaceous nature, we next procured a young willow plant, about 14 inches high, the stem and greater part of whose branches were of a firm and woody texture. It grew in a garden pot, and a tin dish, of a large size, was adjusted to it, in the manner already related. On the 28th of July, a bell shaped jar, of the capacity of 250 cubic inches, was inverted over it, the mouth of which was, as usual, surrounded with water. The apparatus was set aside in a room, in full day light, but not exposed to the sun, where the temperature, through the day, varied from 60 to nearly 70°. The plant looked quite fresh and in a growing state for a few days, but on the 31st, two of its leaves fell off, and, by the 3d of August, nearly half its leaves had fallen. Apprehensive that if the leaves were suffered to remain within the jar, decomposition might ensue, and disturb the natural result, we removed them by immersing the whole apparatus in a deep trough of water, and then cautiously raising the jar, we drew away all the fallen leaves under water, without permitting any fresh air to enter. At the same time, a small quantity of the air of the jar was passed into another jar, and was afterwards found, on analysis, to contain no carbonic acid, but it lost $\frac{15}{100}$ by the slow combustion of phosphorus. The apparatus was afterwards replaced in its former situation.

231. During the following days, the leaves continued to fall, and by the 6th of August, few remained on the branches; the plant, however, in other respects, looked healthy, and had grown about an inch. We now again removed the fallen leaves as before,

and examined a second portion of the air, which, after washing in lime water, now lost only $\frac{13}{100}$ by the test of phosphorus. The plant was then restored to its former situation, and suffered to remain till the 18th of August, at which time its stem had reached the top of the jar, having grown in all nearly three inches. The extremities of its branches still looked thriving and succulent, but they were almost entirely defoliated. The atmosphere being now a third time examined, lost, in repeated trials, only $\frac{11}{100}$ by the combustion of phosphorus, so that $\frac{10}{100}$ of its oxygen had, in the whole, disappeared. In this experiment, the gradual deterioration of the air in which the growing plant was confined, cannot otherwise be explained than by supposing its oxygen to be converted into carbonic acid, which acid was attracted by the water, in which the vessel was inverted.

232. In the same manner, we placed on the 10th of July, a young and vigorous myrtle tree, seven and a half inches high, and growing in a garden pot, in 130 cubic inches of air which was confined by water. On the 18th, the plant looked fresh, but half a dozen leaves had fallen, and many more fell the next and succeeding days. On the 22d, the pot and jar were immersed in water, and a portion of its air removed; 100 parts of this air being shaken with lime water lost three parts, and the remainder lost by combustion with phosphorus 16 parts more, making in the whole $\frac{19}{100}$; so that $\frac{5}{100}$ of the atmosphere had been consumed, of which three existed in the form of carbonic acid, and the two other parts may be reasonably supposed to have been also attracted, in

the form of acid gas, by the water over which it stood, and through which it was passed. These experiments, therefore, prove, that ligneous, as well as herbaceous, plants, during their growth, equally convert the oxygen gas of the air into carbonic acid.

233. It is worthy of remark, that, in both these experiments with the willow and myrtle, the plants were almost entirely deprived of their leaves, a circumstance for which we cannot satisfactorily account. There was no want of moisture in the earth in which they grew, nor was there any substance in the jars to abstract their moisture, and thus to accelerate the fall of the leaves. The leaves, also, did not appear diseased, nor in any respect changed from their natural state; neither have we observed any thing similar to happen with herbaceous plants. There can be no doubt but that the atmosphere, in which the plants were confined, was supersaturated with moisture, which would certainly check evaporation from the surfaces of the leaves; but whether the stagnation thus created was the immediate cause of the phenomenon, we do not undertake to decide. The qualities of the atmosphere had not undergone, in other respects, any very great change, not above one-fourth of its oxygen having been destroyed in the case of the myrtle; nor, if this had proceeded to a greater extent, would it explain the fall of the leaves, though it might have occasioned the death of the plants. But neither of the trees, in this instance, died: on the contrary, they were growing, in a small degree, all the time, and, after being released from their confinement, they again recovered their leaves.

234. In the foregoing experiments, no circumstance, as far as we can discover, was present, which could interfere with the natural progress and result of vegetation. The plants remained growing in the earth, and except that they were confined in a small volume of air, every other circumstance was natural to them. Nor was any substance admitted into the glass vessels, except the plants, which could, in any material degree, affect the air, unless it be conceived that the small quantity of luting, employed to close the junctures, operated in that way. We found indeed, that the lutes we employed did, in a small degree, act on the air; but we satisfied ourselves that the small quantity used, compared with the volume of air employed, and covered, as it often was, with a considerable depth of water, could not, in any material degree, interfere with the expected results. To do away, however, the possibility of error from this cause, we varied the method of experiment in the following manner, in which no luting was employed.

235. We procured some young succulent cuttings of willow, and placed several of them together in phials of pure water, which phials were then confined under separate jars of atmospheric air, inverted in water. The jars were then set aside in the shade on the 9th of July. By the 16th of that month, the cuttings had thrown out small rootlets, which, by the 22d, were, in some instances, nearly an inch in length. On the 23d, the air in one jar was examined. It lost $\frac{7}{100}$ by agitation with lime water, and afterwards, by the slow combustion of phosphorus, it lost eleven parts more, making $\frac{18}{100}$ in all; so that nearly half the oxygen

gas of the air employed was consumed by vegetation, of which seven parts existed in the state of carbonic acid at the time of examination, and the remaining three parts may reasonably be supposed to have been attracted, in the state of acid gas, partly by the water over which the vessel was inverted, and partly by that through which it was passed, when submitted to examination.

236. On the 26th of July we raised a second jar, under which cuttings of willow had been confined, and in which, also, a small quantity of solution of potassa had been placed. Into this jar the water had risen half an inch, and, on pouring diluted sulphuric acid into the alkaline solution, a very brisk effervescence was excited. In a third jar, where a similar quantity of solution had been inclosed, the same phenomena were exhibited. In a fourth jar, $\frac{11}{100}$ of the oxygen gas were consumed. In a fifth, where nine similar cuttings of willow were placed in a large mouthed phial under a jar containing 73 cubic inches of atmospheric air, the air, on being examined on the 5th day, lost $\frac{11}{100}$ by lime water, which it rendered milky, and the residue lost seven parts more by the slow combustion of phosphorus. Allowing also a little acid gas to have been attracted by the water during the analysis, we are enabled to account for the whole oxygen which the air originally contained, of which $\frac{14}{100}$ had been converted into carbonic acid, and $\frac{7}{100}$ remained at the time the analysis by phosphorus was made. In these experiments, therefore, no substance, except the vegetables, was present, which could, in the smallest degree, affect the air; and the results correspond entirely with those already given.

237. We still farther varied these experiments, by taking up a young bean out of the pot in which it was growing, and carefully washing all the mould from its roots. The plant was about seven inches high, and its roots were put into a cubic inch glass measure, nearly filled with water, on the 20th August, when the thermometer was at 66°. The glass measure was then put into an egg-cup containing one cubic inch of the water of potassa, and the egg-cup was placed in a deep saucer. A bell-shaped jar, of the capacity of 86 cubic inches, containing atmospheric air, was then inverted over the plant, and mercury poured round its mouth, to cut off the communication with the external air; a small stratum of water, about $\frac{1}{10}$ of an inch deep, being poured on the surface of the mercury. By the 21st, the plant had grown more than one-fourth of an inch above a thread which had been tied round the jar to mark its height, and the mercury had risen a little into the jar. By the 22d, the mercury was about $\frac{3}{15}$ of an inch high in the jar, and the plant was still growing. By the 23d, the mercury stood $\frac{5}{10}$ of an inch high in the jar, and notwithstanding the film of water that covered the mercury, the lower leaves were much discoloured. We therefore put an end to the experiment, by raising the jar out of the mercury; and then passing the alkaline solution into a small jar of mercury immersed in a bason of that fluid, we obtained, by adding to it a due quantity of diluted sulphuric acid, about one cubic inch of carbonic acid gas. Now, the diminution in the volume of air, in this experiment, must have

arisen from the attraction of carbonic acid by the alkaline fluid, and this carbonic acid must have been in part formed out of the oxygen gas of the atmosphere; for had it been formed by the plant, independently of the surrounding atmosphere, no diminution of volume would have taken place.

238. Several cuttings of willow were, likewise, placed in a large-mouthed bottle filled with water, and confined in about 85 cubic inches of atmospheric air, from the 20th to the 24th of August. The mouth of the inverted jar was surrounded by mercury, and it was set aside in day light, but not exposed to the direct rays of the sun. One hundred parts of this air lost five by agitation with lime water, and thirteen parts more, by the slow combustion of phosphorus; so that the three remaining parts, required to make up the $\frac{21}{100}$ of oxygen gas, which the air originally contained, must have been converted into carbonic acid, which was in part attracted by the film of water that covered the mercury, and partly by the water though which it was passed when submitted to examination.

239. The whole of the experiments which have been now detailed appear to us to establish, in a satisfactory manner, the conclusions which, in our former publication (32. 3. 4.), we ventured to draw; for they prove that the oxygen gas of the atmosphere is converted into carbonic acid gas by the process of vegetation, and that the bulk of the latter gas nearly or exactly corresponds with that of the former; and, consequently, they demonstrate that the air is deteriorated by the growth of plants, in the same manner

as by the germination of seeds, and that no part of the oxygenous portion of the atmosphere combines with the substance of the plant.

---

240. It may, perhaps, add weight to the foregoing conclusions, if we are able to shew, that, notwithstanding the apparent contrariety of fact and opinion which has prevailed on this subject, the writings of various authors furnish decisive evidence that growing vegetables produce in the air those specific changes, which we have held to be essential to vegetation. Even so far back as the time of Dr Hales, an experiment is related which entirely accords with these views. That eminent philosopher set a plant of peppermint, on the 29th of June, in a glass cistern of earth, and filled it with water. Over this cistern he inverted a glass vessel, and then drew out part of the air by a syphon, so as to leave 49 cubic inches of air in the vessel. By the side of this vessel he placed another of the same size, inverted in a similar manner, but without any plant beneath it. In a month, the mint in the first vessel had put forth several young shoots. The water, in both vessels, rose and fell with the variations in atmospheric pressure and temperature; but the water, in which the peppermint stood, rose so much above its first fixed station, and above that in the other vessel, that one-seventh part of the air must, says Dr Hales, have been reduced to a fixed state. This happened chiefly in the summer months. In the beginning of the following April, the old mint was taken out, and a

fresh plant put in its place, to try if it would imbibe any more of the air, but it faded in four or five days; and yet a fresh plant, put into the other vessel, whose air had been confined for the same time, lived nearly a month, or almost as long as another plant lived in newly confined air *.

241. This great diminution of the air of the vessel in which the mint had grown, Dr Hales supposed to afford a proof that the leaves and stems of plants imbibe elastic air; but we now know that carbonic acid must have been formed, and afterwards attracted by the water over which the vessel was inverted. That the pure part of the air had, in a great measure, disappeared, may be inferred, also, from its incapacity to support a fresh plant that was placed in it, which the air, confined the same time in a similar vessel, was found able to do. Dr Hales himself, indeed, proved this fact by an analysis of the residual air; for he found, that, after being infected by the mint, so that a fresh plant would not grow in it, it suffered a farther diminution of four cubic inches, when a mixture of sulphur and iron-filings with water was placed in it. If, therefore, we consider the first diminution of the air by the plant to have been occasioned by the formation and attraction of seven cubic inches of carbonic acid (which is one-seventh of the whole air employed), and add to it four cubic inches of oxygen gas, abstracted by the mixture of iron and sulphur, we shall have eleven cubic inches as the whole loss which the air suffered; and $\frac{11}{49} = \frac{1}{4.45}$, we now know

* Veg. Statics, vol. i. p. 329. 1st edit.

to be nearly the proportion of oxygen gas which the atmosphere contains.

242. We have already given (27.) the results of Dr Priestley's experiments on this subject, and have shewn them to be so much at variance with each other, as altogether to preclude the possibility of any just inference being drawn from them. It may, however, be useful to examine more minutely the circumstances in which these experiments were made, since we may thereby be led to a discovery of the causes of their discordance, and a detection of some sources of error, which the state of chemical science did not, at that early period, enable this candid philosopher fully to appreciate.

243. Dr Priestley had himself at first expected, that, " since common air is necessary to vegetable as well as to animal life, both plants and animals would have affected it in the same manner;" but he found, that, after a sprig of mint had grown for some months in a glass jar, standing inverted in a vessel of water, the residual air was still capable of supporting combustion, or respiration. He observes, however, that, in this experiment, the root of the mint decayed, and the stalk also, beginning from the root, and yet the plant continued to grow upwards through a black and rotten stem: and in this manner a sprig of mint lived, the old plant decaying, and new ones shooting up in its place, all the summer season *.

244. But we now know, that, under the decomposition of vegetable bodies, various gases are disen-

* Obs. on Air, abridged, vol. iii. p. 250.

gaged, and, among these, oxygen gas itself may be either directly or indirectly set free. Thus M. de Saussure confined two plants of *mentha aquatica* in two equal jars of common air, and placed them to vegetate in sunshine for three weeks. By the side of each plant he put a piece of withered dead mint; but in one case the mint was covered by water, while, in the other, it was suspended in the air of the vessel. The plant, which vegetated in the neighbourhood of the suspended mint, had not ameliorated its atmosphere, but that, which grew in the vessel with the moistened dead leaves, had added many times its volume of oxygen gas to the common air which surrounded it *. Dr Priestley himself, indeed, observes, that, " in repeating his experiment, care must be taken to remove all the dead leaves from about the plant, lest they should putrefy, and affect the air †; and yet he inadvertently draws his conclusion from an experiment where this process of putrefaction was constantly going on.

245. There are other experiments, however, of this philosopher, made in a less exceptionable manner, and to hich no apparent objection occurs. He had several instances, he says, of vitiated air being meliorated, and of common air being considerably improved, by the shoots of strawberries, and of some other plants, which he could, by bending, introduce into jars of air, while the roots continued in the earth. This he considered to be the fairest method of trial,

* Recherches Chim. sur la Vegetation, p. 229.

† Obs. abridged, vol. iii. p. 251.

as the plant grew, in every respect, in its natural way, except that part of the stem was obliged to lie in water, and the shoot was in air confined in a narrow jar *. All the cases in which common air was improved by vegetation, he elsewhere says, were those in which the roots of the plant were in the ground, and flexible sprigs from them were bent, and made to pass, in this manner, through water into the jars containing the air †. Most of these experiments, however, were made in June and July, and, as they were conducted in the open air, it is highly probable that they were carried on under a direct exposure to the rays of the sun, which we shall hereafter see to exert peculiar effects on the gases produced in this process.

246. Several sources of fallacy exist in the experiments which Dr Priestley made to restore the purity of air that had been vitiated by the combustion of wax, tallow, and alcohol. He placed sprigs of mint, and other plants, in portions of this air; and he found that five or six days were sufficient to restore the air, when the plant was in its vigour. He farther considered this restoration to depend on the *vegetating state* of the plant; for when the fresh leaves only of mint were kept in such air, for a long space of time, and were frequently changed, no melioration could be perceived in it ‡.

247. On these experiments we may remark, that combustion generally terminates in closed vessels

* Obs. on Air, vol. iv. p. 299. † Ibid. p. 307.

‡ Obs. on Air, vol. iii. p. 252. 253.

some time before the whole of the oxygen gas is consumed; and, consequently, enough of that gas might remain to support vegetation, especially that of the hardier plants. But a still greater source of fallacy is, probably, to be found in the carbonic acid formed in these processes, which, if retained in contact with the plant, might, as we shall hereafter see, be decomposed by Light, and thus furnish oxygen gas to carry on the growth of the plant. It is worthy of remark, that Mr Scheele never found foul air to be rendered salubrious by vegetables, even when placed in the light of the sun; but, in all his experiments, he carefully removed the carbonic acid by washing it in milk of lime before he placed the vegetables in it *. And, in accordance with this fact, Dr Priestley himself observes, that when he employed air that had been vitiated by iron-filings and brimstone, or by nitrous gas, it did not fail to kill the plant †.

248. Dr Priestley's next attempts to establish the purifying powers of plants were made on air that had been injured by animal respiration. Into a jar, nearly filled with air rendered noxious by mice dying in it, he put a sprig of mint, on the 20th June; and another portion of the same air he put into a phial, which was placed by the side of the former. In seven days, a mouse lived five minutes without uneasiness in two and a half ounces of the air in which the plant had grown; but died in two or three seconds, in the same quantity of the air of the other phial.

* On Fire and Air, p. 37. 163.

† Obs. on Air, vol. iv. p. 301.

In a similar quantity of common air, another animal lived seven minutes; and Dr Priestley concluded, "that the restored air wanted about one-fourth of being as wholesome as common air *." In another experiment, a fresh mouse lived 14 minutes in two and a half ounce measures of respired air, that had been restored by vegetation; and in air, rendered incapable of supporting the flame of a candle by Dr Priestley's own respiration, he also found, that a candle would again burn, after a sprig of mint had been made to grow in it. In this case too, the effect, he adds, was not owing to any virtue in the leaves, but to the vegetating powers of the plant †.

249. In these experiments, the same sources of fallacy may be pointed out as in those made on air injured by combustion. No precaution was taken to remove the carbonic acid formed by the respiratory process, nor is it stated whether the experiments were conducted in sunshine, or in the shade. The higher orders of animals, also, die in confined gas some time before the whole of the oxygen is consumed, so that vegetation might, to a certain extent, go on in air incapable of supporting animal life.

250. Dr Priestley, in the last place, endeavoured to restore the purity of air that had been injured by the putrefaction of vegetable and animal bodies. His general hypothesis required that vegetables should continue to live, and that animals should die, in such vitiated air; but he observes that insects, of

* Obs. on Air, abridged, vol. iii. p. 266. 7.

† Ibid. p. 268.

various kinds, live perfectly well in tainted air, which would have instantly killed other animals. He has even been obliged to take plants out of such putrid air, on purpose to brush away the swarms of insects that infested them, so fast did they multiply in these circumstances. Not only, however, did animals thus live in air, in which, on Dr Priestley's hypothesis, they might have been expected to die, but vegetables died in that in which they ought to have lived; for he found, that "when air had been freshly and strongly tainted with putrefaction, sprigs of mint have presently died on being put into it, their leaves turning black; but if they did not presently die, they throve in a most surprising manner *." From the facts already stated (244.), it appears that the air, vitiated by putrefaction, is restored, in certain circumstances, to a state capable of supporting vegetable and animal life, and inattention to these circumstances suggests at once a probable reason for that contrariety of result which these experiments afford.

251. Towards the end of the year 1771, continues Dr Priestley, these experiments did not answer so well, the restored air having relapsed to its former noxious state. In 1772, he had again the most indisputable proofs of the restoration of putrid air by vegetation; but, in 1778, the experiments were unfavourable, for whether they were made with air injured by respiration, by combustion, or by any other means, the air was not rendered better, but worse,

* Obs. on Air, abridged, vol. iii. p. 262.3.

by vegetation; and the longer the plants continued in it, the worse it became. As the general result of all his experiments, however, Dr Priestley thought it *probable*, that the vegetation of healthy plants, growing in situations natural to them, had a salutary influence on the air in which they grew, since one clear instance of the melioration of air in these circumstances, should, says he, weigh against a hundred, where the air is made worse by it *.

252. Such is the series of experiments from which Dr Priestley endeavoured to shew that plants, by their vegetation, purify corrupted air: and such are the terms in which his opinion is finally expressed. The *probability* only which he attached to this opinion, has, by others, been converted into *certainty*; and time hitherto has appeared rather to confirm, than to correct the error. The contrariety, however, that pervades all his experiments, and the "fearful odds" against which his general conclusion is drawn, are, of themselves, sufficient to beget doubt and distrust; and if, to these circumstances, we add the numerous sources of fallacy which have now been stated, and the want of precision in the methods of analysis employed †, we must be convinced that

---

* Obs. on Air, abridged, vol. iii. 265. 273. 275.

† Dr Priestley did not discover the use of nitrous gas, as an eudiometrical test, till the close of the year 1772, when his experiments on plants in that year were nearly brought to a conclusion. Previous to this period, he employed the burning of a candle, and the respiration of mice, as tests of the purity of air*; and it is worthy of remark, that his experiments in 1778, when his methods of analysis were improved, totally contradict those of an earlier period, when they were confessedly imperfect.

* Obs on Air, vol. iv. p. 303.

these experiments are altogether inadequate to decide the general issue of the present question.

253. Lastly, Dr Priestley relates that his method, in making these experiments, was to put the roots of the plants into phials filled with earth and water, and then to introduce them, through water, into the jar containing the air on which he was making the experiment,—a method which itself gives rise to new sources of error. It was observed long since by Dr Ingenhousz, that fresh vegetable mould, confined in common air, considerably depraved it, by forming, as he supposed, carbonic acid *. Dr Thomson confirmed this fact, by finding, as M. Humboldt had also done, that newly turned up soil causes a removal of the oxygen gas of the air, and a production of carbonic acid †. And M. de Saussure found that pure earth, confined either in atmospheric air, or in oxygen gas, formed carbonic acid in proportion to the oxygen lost, without at all diminishing the volume of the air, farther than by the subsequent attraction of a small portion of the acid. The oxygen consumed, he adds, is found in a quantity rigorously equal in the carbonic acid produced: whence it follows, that the earth does not assimilate the oxygen to itself, but only affords carbon unite with it ‡. From these facts, it is evident that the earth, in Dr Priestley's experiments, must have continually vitiated the air, and thus have introduced a constant source of fallacy into their results.

* Exper. sur les Veget. tom. ii. p. 189.

† Syst. Chem. vol. iv. p. 458. 1st edit.

‡ Recherches Chim. p. 179.

254. We have been drawn into these detailed remarks, not from any desire to depreciate Dr Priestley's labours, but from the circumstance of their having first given origin to the opinion, that plants, by their vegetation, at all times purify the air, and from a consideration of the importance which has ever since been attached to them. In the experimental sciences, it is chiefly by the successive detection of each other's errors that we gradually advance to truth; for rarely, indeed, does it happen, that human sagacity can at once foresee and appreciate all the possible circumstances in an experiment, which may influence and controul its result. There is, therefore, no cause to wonder that this illustrious philosopher did not discover those sources of fallacy, which the more advanced state of science has alone enabled his successors to point out: and the reflection, that our apparently more correct views may, at no distant day, undergo a similar revision, ought not only to teach us becoming diffidence in our own opinions, but may serve to check that rising triumph, which little minds are sometimes apt to feel, when they see thus exposed the mistakes of superior men.

255. The accurate and decisive experiments of M. Scheele, on the germination of seeds, have been already detailed (207.), and his experiments on the vegetation of plants seem to have afforded precisely the same results. Dr Priestley, he observes, found that vegetables make foul air salubrious; but, in his own experiments, they always injured the air. He kept vegetables in a closed matrass, filled with foul air, both in darkness, and in the light of the sun,

and examined some part of this air every two days, but he always found it foul *; so that the experience of Scheele is directly opposed to that of Priestley.

256. From what has been stated, however, we may collect, that both these eminent men were, to a certain extent, correct, and that both erred only by pushing their conclusions too far. Little doubt can remain but that, in many of Priestley's experiments, the air was much ameliorated; and it is equally credible that, in all those of Scheele, it continued unimproved. Both philosophers seem to have been aware how much the state of the plant, and the agency of light, influenced the result; but both failed in duly discriminating the composition of the air, which they were accustomed to denominate foul; for while the air used by Priestley (247.) seems always to have contained a portion of carbonic acid, that employed by Scheele (247.) consisted only of nitrogen gas. It was this difference in the composition of the air employed in the experiments, which gave rise to a difference in their results; and the mode in which it contributed to do so will hereafter be distinctly stated.

257. The experiments of Priestley and Scheele were repeated, and pursued to a great extent, by Dr Ingenhousz. Common air, says this author, is equally necessary to the life of animals and of plants. It is, however, only one-fourth part of this air that serves this purpose; and hence a plant, confined in

* On Air and Fire, p. 163.

a given quantity of air, perishes in the shade, after having changed about one-third of it into carbonic acid, even although this carbonic acid be daily removed. The cause of the death of the plant, therefore, is not, he adds, the presence of carbonic acid; but it dies from having completely deteriorated the air, just as an animal would have done *. Plants also, he continues, grow in oxygen gas, and, like animals, produce carbonic acid in it; and oxygen gas, instead of being destructive to plants, is the support both of animal and vegetable life, the true *pabulum vitæ* of both the living kingdoms of nature †.

258. The opinions of M. Senebier, concerning the use of air in vegetation, coincide nearly with those which have been just related. He maintains, that plants die in nitrogen gas, but live and grow in oxygen, or in an artificial atmosphere composed of these two gases; and, consequently, he infers, that the presence of oxygen gas is indispensably necessary to vegetation. He farther maintains, that this oxygen gas disappears, and is replaced by carbonic acid gas; and that an atmosphere, rendered noxious to plants confined in it, is again made capable of supporting vegetation, by withdrawing the carbonic acid, and adding to it oxygen gas, in place of that which has been consumed. Experience, he continues, teaches us, that vegetation never takes place without forming carbonic acid, as is proved by placing plants in pure oxygen gas ‡.

* Exper. Pref. vol. ii. p. 35. † Ibid. 37.

‡ Physiol. Veget. vol. iii. p. 113. 115. 116.

259. But M. Senebier goes still farther, and maintains, that the carbonic acid, which is thus formed by plants, when they are confined in closed vessels, does not proceed from the plant. It is, says he, more abundantly produced in the shade than in the light, and always, in both cases, when the heat is greatest. The atmosphere of such plants diminishes, and loses precisely the quantity of oxygen gas necessary to form the carbonic acid which we find; so that this acid is necessarily composed of the oxygen which is exterior to the plant, and of carbon that has escaped from the vegetable to unite with it. If, he adds, the carbonic acid had proceeded directly from the plant, the atmosphere would have been vitiated without being changed, and the proportion of oxygen gas would still have been found in it, after its acid gas had been removed by washing *.

260. In the year 1797, M. T. de Saussure published some experiments on the vegetation of plants in atmospheric air. He raised some garden peas (pisum sativum) in water, till they attained the height of between three and four inches. These plants were then put into a glass containing water, in which their roots were immersed. This glass was next placed on a saucer filled with water, and over it a recipient, containing 50 cubic inches of atmospheric air, was inverted. The apparatus was then set aside in a room, but did not receive the direct rays of the sun. At the end of ten days, the plants had considerably increased in weight; the volume of air, in which

* Physiol. Veg. vol. iii. p. 121.

they were confined, was diminished one cubic inch and a half: its purity, also, was greatly impaired; and it contained $\frac{6}{100}$ of carbonic acid. Experiments made with plants of mint, in the shade, afforded similar results; but when the apparatus was exposed, for the same time, to the direct agency of the solar rays, the air experienced no change, either in its purity or its volume. From these and other experiments, M. de Saussure concludes, that plants, like animals, continually form carbonic acid, when they vegetate in atmospheric air, either in the sunshine or in the shade; that the oxygen of the air contributes to the formation of this acid; and that the reason why carbonic acid is not detected in the apparatus exposed to the sun, is, because it is then decomposed as fast as it is formed *.

261. In subsequent experiments, relating to vegetation †, M. de Saussure directed his attention almost entirely to ascertain the influence of solar light in decomposing carbonic acid. With this view he confined plants in mixed atmospheres of common air and carbonic acid gas, and exposed the apparatus to the direct rays of the sun, whereby he obtained very different results. We shall, therefore, postpone our report of these experiments to a future occasion, observing, for the present, that the experiments related above, accord entirely, in their results, with our own, and with those of other writers, respecting the effects produced in atmospheric air, by plants which vegetate in the shade.

---

* Annales de Chimie, t. xxiv. p. 139. et seq.

† Recherches Chim. sur la Vegetat.

262. To the experiments of Dr Woodhouse on the vegetation of plants in atmospheric air, we have already referred (30.), and have stated their general agreement with those which have now been related. He found that plants, confined either in atmospheric air or in oxygen gas, previously washed in lime water, rendered it impure, by forming carbonic acid. He farther asserts, that growing vegetables do not purify the air, but that the same effects are produced in the air by the growth of plants, as by the germination of seeds, and from the same causes. He supposed, indeed, that the carbon, which united with the oxygen, was afforded, not by the living plant, but by a dead portion of the leaves *. The facts, however, which have been so fully stated, sufficiently evince that this acid may be formed by the living, as well as by the dead, vegetable substance.

263. From the series of experiments and opinions which has now been delivered, we see that all the more distinguished philosophers, who have applied themselves to the investigation of this subject, with the single exception of Dr Priestley, agree in the following general facts: That oxygen gas is, at all times, necessary to vegetation; that, in the shade, or in perfect darkness, this gas constantly disappears, and is replaced by carbonic acid; and that, when the atmosphere contains no oxygen gas, vegetation no longer goes on. Even the experiments of Dr Priestley cannot be considered as opposing any serious objection to these conclusions; for though it were grant-

* Nicholson's Journal, vol. ii. p. 152.

ed, that, in certain circumstances, growing vegetables purify the air, yet such concession would not destroy the fact, that, in other circumstances, they cause its deterioration; and the imperfections, and contrarieties, which have been demonstrated in the experiments themselves, altogether annihilate their authority in establishing the general conclusion, which he has sought to deduce from them.

264. But in the experiments, which we before published (32. 3. 4.), we endeavoured to shew, from a careful observation and analysis of the air before and after the experiment, not only that a substitution of carbonic acid for oxygen gas took place in vegetation, but that the bulk of acid produced corresponded nearly, if not exactly, to that of oxygen gas which disappeared. In the experiments, also, which have now been related (236.), where the air was examined before the oxygen gas was entirely consumed, the bulk of acid abstracted by lime water, and the bulk of oxygen gas removed by phosphorus, composed together almost exactly the absolute quantity of oxygen which the atmosphere originally contained; which coincidence appears to us decisive, not only of the conversion of the oxygen gas into carbonic acid, but proves, farther, that none of this gas is retained by the living plant. We may add, that the results obtained by Hales, by Scheele, by Ingenhousz, and by Senebier, concur, more or less completely, to establish the same position, which is still farther fortified by the analogy derived from the case of seeds (208.), and by the inconsistency of supposing, without any better evidence than conjecture, that, while

the greater part of the oxygen is converted into this acid, a very small portion should still be retained by the plant.

265. But, perhaps, some who admit the conversion of oxygen gas into carbonic acid by plants growing in the shade or in darkness, may still contend that this is not a perfect vegetation, because the direct influence of light is withdrawn. We acknowledge the great influence of light in this process, but we have seen (25.) that it is not essential to it; for plants live and grow in situations from which light is wholly excluded. All the experiments also, which we have heretofore detailed, both in this and in our former treatise, although they were not made under a direct exposure to the sun's rays, were conducted in open rooms where light had the freest access; and the plants assumed all the characteristic properties and appearances, which were peculiar to them. It is, likewise, sufficiently evident, that, even in our own climate, and especially in high northern latitudes, a vast number of plants live and flourish in natural situations, where the direct rays of the sun seldom or never penetrate; and yet, in such situations, they attain a state of perfect vegetation. How many hours, also, of our brightest days, and even how many entire days, are we deprived of the direct influence of the sun's rays, at the very season when vegetation is advancing with the greatest rapidity and vigour? If, indeed, this direct influence were essential to vegetation, many plants, which we now behold, would never be produced at all, and all the tribes of vegetables would experience such frequent and continued

checks to their growth, that, in our own climate at least, we could scarcely ever hope to see many of them attain to a state of maturity.

266. Should we even allow, to the fullest extent, that plants, vegetating under direct exposure to the sun, produce changes in the air different from those which they produce in the shade, yet this, surely, does not disprove the correctness of our former observation. It only proves, that, by a change of circumstances, a change in effect is induced; and as this effect is not essential to the end we desire to obtain, it must be considered as extrinsic and accidental; for that condition can, at no time, be held necessary to the production of a given effect, which may, at any time, with impunity be excluded.

267. After what has just been observed concerning the changes induced on the air by vegetables growing in the shade, and in perfect darkness, it is almost superfluous to add, that similar effects must be produced by plants which vegetate through the night. Dr Priestley very early observed, that a fresh cabbage leaf greatly deteriorated the air in which it was confined for a single night, although it had not acquired the least smell of putrefaction *; and Ingenhousz likewise remarked, that the more vigorous the plants were, the more was the air affected. He found that a withered plant, confined in air through the night, produced in it scarcely any change; while a similar plant, in a state of active vegetation,

* Obs. on air abridged, vol. iii. p. 251.

diminished the oxygen gas of the air, and formed carbonic acid *. Many facts of a similar kind are to be met with in different parts of his work †.

268. With the view of confirming these facts, we placed some young mustard plants in a jar containing 40 cubic inches of atmospheric air. They were supported on a small whalebone hoop, covered with netting, beneath which a small cup, containing water of potassa, was placed. The jar stood inverted in a saucer of mercury; and at eight o'clock in the evening, when the temperature was 60° Fahrenheit, it was set aside in a dark room. By seven o'clock the next morning, the mercury had risen three-tenths of an inch into the jar; and, on raising the jar and pouring diluted acid into the alkaline solution, a brisk effervescence was excited, proving, in the usual manner, the conversion of the oxygen gas of the air into carbonic acid. In many subsequent observations on the vegetation of young beans in jars of atmospheric air, we likewise remarked their growth in the night, when the temperature was sufficiently high, to be little inferior to that which they make in the day;—a fact which is exemplified equally in the warmer regions of the globe, where a rapid succession of crops is obtained, and in high northern latitudes, where the short season of summer permits the intervention only of a few weeks between the periods of seed-time and of harvest.

---

* Exper. &c. t. ii. p. 117. 118.

† Ibid. vol. ii. p. 45. 46.

269. All that has been hitherto said, with regard to the changes induced on the air by plants, bears an immediate reference to the agency of the leaves, and of other green parts, in effecting these changes. But other parts of living vegetables produce, likewise, changes in the air, and these, in the next place, we propose shortly to consider.

270. Dr Priestley confined a full-blown rose in about eight cubic inches of atmospheric air, contained in a small glass jar inverted in water. On examining the air the next day, by the test of nitrous gas, he found it to be much depraved; and, by the following day, it was still farther depraved, but the flower, when withdrawn on the third day, did not seem to have lost any of its fragrance *. M. Scheele found, that, by confining fresh roots, fruits, herbs, flowers, and leaves, in separate vessels of common air, one-fourth part of the air was changed, in a few days, into carbonic acid gas †.

271. Dr Ingenhousz placed some flowers of marigold (calendula) beneath an inverted vessel of atmospheric air, and left them all night in his chamber. The next morning, a portion of the air was examined, and found to be incapable of supporting flame; it suffered, also, but little diminution when mixed with nitrous gas. He then exposed the vessel, with the remain-

* Obs. on Air, vol. iv. p. 311.

† On Air and Fire, p. 150.

ing part of the air, to the sun, for several hours, and found it, on examination, to be still more deteriorated. The flowers of honeysuckle produced similar effects, as did many others which he submitted to experiment *; so that flowers equally deteriorate the air in sunshine and in the shade.

272. In his experiments on flowers, M. de Saussure found that none, not even those of aquatic plants, could be developed in nitrogen gas; that the buds just ready to expand were suddenly checked, and those already developed speedily died. He considers flowers to displace in part the air that surrounds them, and to replace, by carbonic acid, the portion of oxygen which they consume. Those which are recently gathered, and confined in atmospheric air, do not sensibly alter its volume. The flowers which he employed did not suffer during the experiments; they had not lost their form or freshness, and many, which were only in bud, became fully expanded. The petals consumed more oxygen in sunshine than in the shade; but he tried in vain to excite detonation in oxygen gas, in which he had caused bunches of fraxinella to vegetate for eight days; and he thinks, therefore, that the inflammation, which may be excited in the atmosphere of this, and of some other plants, is owing to the combustion of an essential oil, and not to that of hydrogen gas †.

273. The changes thus induced on the air by the

* Exper. sur les Veget. t. i. p. 270.

† Researches, &c. p. 125. 129.

flowers of vegetables were supposed by Dr Darwin to be necessary to the elaboration and perfection of their sexual organs, and to the production of fruit. Each petal of the corolla of a flower has, according to him, a vessel which conveys its juices to the extremity, where they undergo a change of colour, as is seen in some party-coloured poppies. From the extremities of the petal, the juices are again returned, as in the leaves, by other vessels, and employed for the sustenance of the stigma, anthers, &c. This office of the petals may be inferred, he adds, from their vascular structure being visible to the eye; from many plants putting forth their flowers in autumn, before any leaves appear; and from others completing the process of impregnation early in spring, before their green foliage, or even their floral leaves, come forward; lastly, from the white petals of the christmas rose (helleborus niger) changing to a deep green after the seeds have grown to a certain size, and the nectaries, stamens and stigma have dropped off,—facts which shew, that the first structure was necessary to the production of the honey, wax, and pollen, but was no longer required after these were formed *.

274. With respect to fruits, Dr Ingenhousz observes, that, whether they were ripe or unripe, whether exposed in sunshine or placed in the shade, they always vitiated the air in which they were confined. His experiments were made on peaches, lemons, pears and apples, and the results were invariably the same †. M. de Saussure, however, found, that

* Physiologia, p. 51.

† Exper. vol. i. p. 274.

green sour grapes, while growing on the vine, and exposed to the sun in closed vessels, ameliorated common air without producing carbonic acid, and ripened in fifteen days; but when the vessels contained lime, they vitiated their atmosphere, in the same circumstances, and did not ripen. Grapes, apples, and pears, when separated from their respective trees, and placed in vessels of common air exposed to the sun, have vitiated their atmosphere in twenty-four hours, and produced carbonic acid; and when the experiment was made in perfect darkness, the acid produced was equal in bulk to the oxygen that had disappeared *.

275. Many experiments on the fresh roots of several plants were made by Dr Ingenhousz, who found them, in like manner, to deteriorate the air †; but as the roots were all immersed in vessels filled with water, these experiments are not so unexceptionable as those which he made on flowers. He elsewhere observes, however, that if a carrot, or the root of any other plant, be drawn fresh from the earth, and exposed, with its leaves, in a vessel of common air to the sun, the air will soon be manifestly deteriorated; but if the root be placed under water, and the foliage only be left in the air, the deterioration produced by the root will soon be repaired by the leaves ‡.

276. But the most precise information on this

---

* Recherches, &c. p. 129.

† Exper. t. i. p. 274. ‡ Ibid. t. ii. p. 38.

branch of the subject is afforded by the experiments of M. de Saussure. He observes, that a small portion of oxygen gas exists in the earth, and he was desirous to know whether the contact of this gas with the roots of plants was useful to vegetation. He therefore took several tubulated recipients, and, filling them with distilled water, set them to stand in separate basons of mercury. Having then drawn some young chesnut trees out of the earth, he put the roots of one tree through the mouth of each vessel, so that they should be entirely in the vessel, while the leaves were in the open air; the mouth of each vessel was then carefully closed by a lute applied to the stem of the plant. Through the bottom of each recipient, a given quantity of gas was next introduced, which caused the water to fall, until the extremities only of the roots were immersed in it, while their greater part was exposed to the immediate contact of the gas employed.

277. In this manner he exposed the roots to nitrogen, hydrogen, and carbonic acid gases, and likewise to atmospheric air. The plants, whose roots were in contact with carbonic acid gas, died about the seventh or eighth day; those which had their roots exposed to nitrogen and hydrogen gases died at the same time, which was about the thirteenth or fourteenth day: while three separate plants, whose roots vegetated in contact with atmospheric air, were still vigorous at the expiration of three weeks, when an end was put to the experiment. He adds, that the nitrogen and hydrogen gases suffered no change, but the bulk of air was increased by

the addition of carbonic acid furnished by the roots; while the atmospheric air was diminished from the attraction of the carbonic acid, either by the roots themselves, or by the water. From these experiments, and from other considerations, M. de Saussure concludes, that the contact of oxygen gas with the roots of plants is useful to vegetation *.

278. Lastly, M. de Saussure confined the woody stems of plants, immediately before the appearance of their buds, in vessels of common air, with sufficient water to carry on their growth, and they came into leaf like those in the open air; but in nitrogen and hydrogen gases they were unable to make this development, and perished without exhibiting any sign of vegetation. When deprived of their leaves, the ligneous stems of plants vitiate common air, either in sunshine or in the shade, without changing its volume; and replace, with an equal bulk of carbonic acid, the oxygen gas which they make to disappear. The stems of the willow, the oak, the poplar, the apple and the pear, were employed in these experiments.

279. These effects which the flowers, the fruits, the roots, and the stems of plants produce in the air, at all times, and in all situations, accord completely with those which the leaves and other green parts of vegetables produce in the shade, since they all, more or less, convert the oxygenous portion of the air into an equal bulk of carbonic acid. These facts afford,

* Recherches, p. 104. 106.

therefore, additional evidence of the power of the vegetable to produce this specific change in the air, and shew that, with respect to these parts at least, it is the natural effect of vegetation. As, however, carbonic acid is thus, at all times, produced by these several parts of plants; and as all the green parts have likewise been shown to produce a similar effect in the shade; it is evident, that, if a contrary effect arise when the green parts are exposed to the rays of the sun, it must proceed from the peculiar agency of the sun upon those parts alone. We proceed therefore, in the next place, to investigate the changes induced on the air by the leaves and green parts of living vegetables, when exposed to the direct agency of the solar rays.

SECT. II. *Of the Vegetation of Plants in Sunshine.*

280. In our former publication we were led, by the results of our own experiments, to consider the consumption of oxygen gas, by living plants, as the natural and necessary effect of vegetation; and this conclusion has, we presume, been abundantly confirmed by the additional facts and circumstances which have just passed under our review. But the opinion, which maintained the production of this gas by growing vegetables exposed to the sun, appeared so directly opposed to these views, that we were induced to question the accuracy of the facts, and to combat the correctness of the reasoning on which it rested;

and while, in certain circumstances, we admitted (45.) this production of oxygen, we denied (46.) that it either was or could be considered a necessary function of vegetation. A more extended inquiry into the original facts, together with the additional light, which the late researches of M. de Saussure have afforded, have contributed to raise our view of the importance of this subject; and therefore, in the present section, we have submitted it to a fuller and more distinc discussion.

281. On several occasions, we have already alluded to the operation of light in modifying the changes which living vegetables produce in the air; and we have also noticed the remarkable influence which it exerts in the production of the colours of plants. As we now propose to enter more fully into the nature of its operation on he vegetable kingdom, we shall briefly recite such additional facts and observations, relating to its more general effects, as have since come to our knowledge.

282. We before ascribed (25.) to M. du Fay the first observation respecting the direct agency of light in changing the colours of plants; but a more extended inqui y has taught us, that we must go much farther back to collect the first notices which relate to this sub ect. According to M. Senebier, Aristotle, in his theory of colours, remarks, that when water and the solar rays act togeth r on plants, they produce a green colour; and, in conjunction with earth, the sa e water produces whiteness. Hence all those parts of plants which lie beneath the soil, as roots and bulbs, appear white, while the parts,

which are exposed to the sun, exhibit a green colour *.

283. The celebrated Ray possessed still clearer notions on this subject. He remarked that plants continued green while they vegetated under a glass bell exposed to the light; but when the light was excluded, by covering them with an opaque vessel, they lost their green hue, and acquired a whitish yellow colour. Their stalks, at the same time, became long, slender and feeble, and their leaves small. The cause of the green colour he, therefore, rightly ascribed to the direct agency of light, rather than to that of heat or air †.

284. These experiments were carried much farther by M. Bonnet, who caused peas and beans to grow in glass tubes, when their colour was not affected; but in boxes, when the light was completely excluded, the plants became white. Even in wooden cases, however, the plants exhibited a green colour in particular parts, placed opposite to small perforations which were made to admit the light. The exclusion of light, he adds, obstructs the development of the leaves, and promotes the elongation of the stem; but neither want of air, nor a greater or less degree of heat, seemed to affect the degree of etiolation ‡.

285. This philosopher farther discovered, that plants, which had become white by the exclusion of

* Physiol. Veg. vol. iv. p. 265.

† Hist. Plantar. vol. i. p. 15.

‡ Recherches sur les Feuilles, p. 210. 331.

light, recovered their green colour when the light was restored. Thus French beans, which were much etiolated, acquired, in 24 hours, a sensible green tint by exposure to day-light in summer; but, in the dark days of autumn, they still retained their whitish colour, even when thus exposed. He also noticed the great exertions which vegetables make to expose their leaves to the action of the sun; thus, a tuberous root, confined in a vessel where the light was partially excluded, made, in five weeks, eleven turnings or movements to follow it *.

286. The red and purple, as well as the green colours of vegetables, and all the varied hues of flowers, seem, likewise, to owe their perfection to the agency of light. Dr Grew remarked, that those parts of the roots of plants, which remain under the soil, are generally white, while the parts exposed above ground are frequently coloured; thus the tops of sorrel roots are red, and those of turnips and radishes are sometimes purple. These changes in the colours of the roots, as well as the green colour of the leaves, and the different colours of flowers, he attributed to the action of the air †; but the experiments of Mr Davy (25.) prove that the colours of flowers, as well as of leaves, depend immediately on the operation of light.

287. The changes produced in the other sensible properties of plants, by the agency of the solar rays, were first remarked by Scheele, who considered

* Recherches, &c. p. 340.

† Anat. of Plants, p. 270. 2d edit.

light to be decompounded in vegetables, and to assist in the formation of their resinous and inflammable matter *. In the year 1774, Dr Robison observed, that the leaves of tansey, which had grown in a coal mine, and were white, afforded no aromatic smell, when rubbed between the fingers; but after they were planted in a place exposed to light, their white leaves died down, and fresh green sprouts shot up, which possessed the smell and all the other properties of common tansey. He repeated the experiment with great care on lovage, on mint, and other plants. They all throve well in darkness, but with a blanched foliage, that had no resemblance to the ordinary foliage of the respective plants. When brought into day light, they all died down, and the stocks then produced the proper plants in their usual dress, and having all their distinguishing smells †.

288. After these general remarks on the agency of light in producing the colours, and otherwise changing the sensible properties of plants, we proceed to consider its power in modifying the qualities of the atmosphere, in which vegetables are made to grow. From the history of facts already given (243.), it appears that Dr Priestley first advanced the opinion, that plants, in certain circumstances, purified the air. This effect he considered to depend on the *vegetating state* of the plant; but as, on most occasions, he has omitted to state whether his experiments were conducted in sunshine, or in the shade, it cannot be said

* On Air and Fire, p. 158.

† Black's Chem. Lectures, vol. i. p. 532.

that he was, at all times, fully aware of the importance of the agency of light. In many instances, however, he has duly remarked it. Thus, for example, in his experiments on the green matter that forms on the sides of vessels filled with stagnant water, he asserts that the pure air is never produced in the shade, but only under a direct exposure to light; and no degree of warmth, he adds, will supply the place of the sun's rays. He farther observed, that the water, which contained most carbonic acid, afforded oxygen gas most abundantly; and that this gas was entirely dissipated by the action of the sun, leaving a residue of pure oxygen. When the glass vessels, containing this matter, were exposed to the sun, and were in the act of yielding pure air most abundantly, he found that the process ceased entirely, if he intercepted the solar rays by covering the vessels with black wax, or by removing them into a dark room *. These facts clearly demonstrate the power of light in the development of oxygen gas, and likewise furnish evidence of the decomposition of carbonic acid by the operation of the same agent.

289. The experiments of Priestley were prosecuted by his illustrious contemporary Scheele, who, as we have already seen, (255.) could never succeed in his attempts to purify noxious air by vegetation, whether his experiments were conducted under exposure to the sun, or in the shade. The cause of uncertainty in the experiments of Priestley, and of entire failure in those of Scheele, is ascribed, by Dr Ingenhousz, to

* Obs. on Air, vol. iv. p. 337. et seq.

the apparatus which they employed being placed indifferently in sunshine or in the shade; or to their having suffered the plants to remain in confinement until they had lost their vigour or their life *. It must, however, be observed, in justice to those philosophers, that they were both duly apprized of these circumstances, and conducted many of their experiments with an immediate regard to them. While therefore we admit, that the variation in the results of some of Priestley's experiments may have arisen from inattention to these particulars, yet the discordance between him and Scheele is, more probably, to be ascribed to the different nature of the gases which they employed, in consequence of the precaution which the latter always adopted, of first removing the carbonic acid from the foul air which he submitted to experiment.

290. But certainly the experiments of M. Ingenhousz, both by their number and variety, have contributed, more than those of any other philosopher, to elucidate the subject of the purification of our atmosphere by plants. In air that had been so far depraved by respiration as to extinguish a lighted candle, he placed a plant of peppermint, and then exposed the vessel, for three hours, to the sun, at the end of which time the air again supported flame. When, however, the common nettle was placed in a similar portion of respired air during the night, the air was not at all improved; but the apparatus being

* Exper. sur les Veget. tom. ii. pref. p. 31.

afterwards exposed to the sun for two hours, the air was brought back to its original purity. He also found, that mustard plants, when confined in a vessel of common air through the night, rendered it impure; but that this same air, together with the plants, being the next morning exposed to the sun, was restored, in an hour and a half, to its former purity *. When, likewise, similar portions of the same respired air were confined in vessels with similar plants, and respectively placed in sunshine and in the shade, the air, exposed to the sun, recovered its purity in a few hours, while that, placed in the shade, was rendered more impure than before. He repeated these comparative experiments a great many times, and varied them in different ways, until there remained not the least doubt, but that it was to the light of the sun that the purification of the air was owing, and to the absence of this light, that the opposite effect was produced †.

291. Dr Ingenhousz next made experiments on air that had been vitiated by the burning of a common candle, suffering the carbonic acid which it contained to remain in it. With this air he filled three bell glasses of the same size, and introduced into each glass some plants of young mustard. One of the glasses he exposed to the sun at mid-day; another he placed in the shade; and the third was removed to a place of perfect darkness. The next morning, before the sun had again shone on the

* Exper. tom. i. p. 263. 265.

† Exper. &c. tom. ii. pref. p. 31.

plants, he examined the air of the first glass, and found it to be considerably vitiated, and to contain a little carbonic acid, which he ascribed to the nocturnal effect of vegetation; while the air, in the two other glasses, contained one-fifth of acid, and was greatly more vitiated. Similar experiments were made on three different portions of common air, and with nearly the same results *. In the shade, he adds, plants deteriorate the air as much as in perfect darkness, although the colour, which they assume in darkness, differs much from that which they acquire in the shade; for, in the latter, they retain their verdure, while, in the former, they become white or yellow †.

292. Besides these experiments on the power possessed by plants of purifying the air vitiated by the several processes of respiration, vegetation, and combustion, Dr Ingenhousz made various others with the leaves of plants immersed in water, and exposed to the light of the sun. From these experiments, he was led to conclude, that plants not only correct impure air in the space of six days or more, as Priestley's experiments seemed to indicate, but that they perform this important office in a very few hours; that this operation is not owing to *vegetation*, but to the influence of the solar light on plants; that plants possess the power of giving out, in a pure state, the air which they contain and have derived from the atmosphere; that this operation commences some

* Exper. t. ii. p. 45. 46.

† Ibid. p. 47.

time only after the sun has gotten above the horizon, is more or less vigorous in proportion to the clearness of the day, and the situation of the plant to receive the direct rays of the sun, and is suspended entirely during the darkness of the night; that plants, in the shade, do not ameliorate the air, but render it more noxious; that the leaves, and green branches and stems of plants alone perform this office of purification; and that acrid, offensive, and poisonous plants act in the same manner as the most sweet and salutary; that the pure air proceeds chiefly from the inferior surface of the leaves, and that young leaves do not furnish so much of it as those which have acquired their full size and vigour; that some plants yield purer air than others, and that aquatic plants particularly excel in this respect; and, lastly, that the sun does not possess the power of improving the qualities of atmospheric air without the concurrence of plants. These conclusions were drawn from the results of more than 500 experiments, made between the months of June and September *.

293. Of this great number of experiments, however, almost all were made with the leaves after they were separated from the branches, and immersed in glass vessels of water. Dr Ingenhousz says, that this mutilation does not vary the result; for when he passed the pliant branches of vegetables, whose roots remained in the earth, into jars of water, the same phenomena were exhibited; and therefore, he afterwards used the leaves and cut branches as being most convenient †. And with respect to immersion in wa-

* Exper. t. i. pref. p. 64. et seq. † Ibid. p. 283.

ter, he does not consider it so foreign to the nature of vegetables as to derange their functions. Many plants, he observes, will vegetate a long time, although covered entirely with water, and aquatic plants live continually in it. In fact, the water, according to him, only intercepts the communication between the plant and the atmosphere; and though it prevents the plant from receiving any thing from the air, it does not put any obstacle to the discharge of what it contains *.

294. These ideas may, to a certain extent, be correct. M. Bonnet has shewn that the leaves of various plants, when separated from their branches, continue fresh several weeks, and, in some instances, even months, when their surfaces are laid on water. In some cases, the experiment succeeded best when the upper surface of the leaf, and in others, when the under surface, was applied to the water †. It may be doubted, however, whether leaves continue fresh so long when they are completely immersed, since the means of evaporation are then cut off, and consequently the water received must remain stagnant in the leaf. In the greater number of Dr Ingenhousz's experiments, however, the process continued only a few hours, and the leaves, therefore, though unable to vegetate, may be still considered as possessing life.

295. The employment of water is farther objectionable, because it not only removes the vegetable from

* Exper. t. i. sec. 8. p. 43.

† Recherches sur l'Usage des Feuilles, p. 8.

its natural situation, but places it in circumstances where the external air is entirely excluded. The existence of aquatic plants under water is no proof that terrestrial ones are able to support a condition so unnatural to them; for the structure in each may be very different which enables them to live and flourish in situations so very dissimilar. Thus we know that the leaves of the *ranunculus aquaticus*, which float on the surface of the water, have a flat and expanded form, while those, which are entirely immersed, exhibit a roundish and filamentous figure.

296. But it is another objection to these experiments, that they bring many new sources of fallacy into view, which greatly embarrass the original question; since it is not easy to determine how much of the air is furnished by the water itself, and how much by the vegetable immersed in it. These, accordingly, have been points much disputed among those who have made experiments on the subject. We prefer, therefore, those experiments of Dr Ingenhousz which we have before detailed (290. 1.), as affording the best evidence adduced by him of the purification of the atmosphere, by plants exposed to the sun.

297. In these experiments, the air, vitiated by respiration and combustion, appears always to have contained carbonic acid, at least no notice is given of this acid having been previously removed. Farther, Dr Ingenhousz supposed plants to possess the singular property of converting carbonic acid into respirable air, when exposed to the sun; and he asserts, that if they are confined in mixtures of common air

and carbonic acid, and exposed to the solar influence, this acid gas will be changed into an air insoluble in water *. Lastly, we may add, that the rapid manner in which the purification of the air is stated to have occurred in the experiments of Dr Ingenhousz, effectually excludes the idea of the oxygen gas being derived from decomposition of the vegetable substance.

298. M. Senebier instituted, likewise, a great number of experiments to prove the production of oxygen gas by plants, exposed to the direct rays of the sun. Almost all of them were, however, made by immersing leaves in water, previously impregnated with carbonic acid; and they do not apply, therefore, directly to the present question. In one instance, indeed, he placed a branch of raspberry tree, having its end dipped in water, in a recipient filled with common air, and inverted in water. The recipient was then exposed, through the day, to the direct influence of the sun. The air at first, when examined by nitrous gas, was reduced to 1.04, and at the close of the experiment, when again tried, it was reduced to 1.00; so that, if the method of analysis be deemed correct, it had experienced some improvement.

299. M. Senebier considers the agency of the sun to be indispensable to the production of the oxygen gas which leaves afford, when confined either in water or in air; and this operation of light, he adds, is continued through the winter, by those plants which retain their verdure. He has seen mosses, when pla-

* Exper. t. ii. p. 74. 77.

ced under water and exposed to the sun, afford pure air, when the temperature was many degrees below *zero*; and many evergreens, under similar circumstances, are always able to afford it. In every case, he believes the oxygen to be derived from the decomposition of carbonic acid, which he supposes to be effected in the leaves by the affinity of light for oxygen; and the oxygen produced is, he adds, always proportional to the quantity of carbonic acid that is present. He denies that oxygen is ever derived from the decomposition of water; for in boiled water, which contains no carbonic acid, plants do not afford oxygen, while in waters impregnated with this acid gas, they yield it most abundantly. Neither is the oxygen furnished solely by the leaves, independently of light, since it is not obtained from them when the apparatus is placed in the shade *. From these facts we collect, that the presence of carbonic acid is necessary to the production of oxygen, which can be furnished only by the decomposition of this acid gas through the joint agency of the plant and of solar light.

300. In 1779, Mr Davy made some experiments with the view of discovering the origin of the oxygen gas which vegetables, in certain circumstances, seem capable of affording. He introduced a small plant of *arenaria tenuifolia*, growing in a pot of moist earth, into a jar containing 14 cubic inches of carbonic acid. The jar was inverted in mercury, and exposed to the solar rays, for four successive

* Phys. Veg. t. iii, p. 195. et seq.

days of fine weather in the month of July. The plant, at this period, looked well and healthy; the mercury had risen considerably into the jar; and on examining the gas, there was a deficiency of 2.3 of a inch. After removing all the carbonic acid by an alkaline solution, the remaining gas measured two cubic inches, and proved to be oxygen. This experiment he repeated two or three times on different plants with similar results.

301. That the oxygen, obtained in these experiments, did not proceed directly from the vegetable, Mr Davy ascertained, by having, at the same time, kept another plant, in all circumstances exactly similar, in a jar of nitrogen gas. The diminution of bulk in this gas did not exceed $\frac{3}{10}$ of an inch; and $\frac{6}{10}$ more were abstracted by the hydro-sulphuret of potassa, which he considered to be oxygen formed from the decomposition of the water of the plant *.

302. Mr Davy having thus ascertained the fact of the decomposition of carbonic acid, next made experiments to prove that plants, by their own powers, independently of the agency of light, were unable to effect this decomposition. He placed a small plant of *chironœa centaurium* in a jar of pure carbonic acid inverted in mercury, and then set it aside in a dark closet. At the end of four days, the air had diminished a little, and the plant looked sickly. The air was now examined; and after the carbonic acid was removed, there remained only a small portion of an incombustible gas.

* Beddoes' Contributions to Science, p. 163.

303. As in the preceding experiment, the plant was unable to decompose carbonic acid without the aid of light, so likewise he found that light, by itself, was alike insufficient for this purpose; for no alteration was effected in this acid gas, though exposed, for any length of time, to the action of the solar rays *. It follows from the foregoing series of facts, that neither the power of the plant alone, nor the agency of light alone, is able to decompose carbonic acid, but that its decomposition is effected by the power of the plant, in concurrence with the direct agency of the solar rays.

304. Dr Woodhouse exposed the leaves of nine different plants in separate vessels of atmospheric air, mixed with about $\frac{1}{15}$ of carbonic acid, to the light of the sun for seven hours; at the end of which time, he found that the acid gas had disappeared, and the atmospheric air was greatly increased in purity. When the air was previously vitiated by carbonic acid, formed by the putrefaction of a fungus, he found it to be restored to a great degree of purity by fresh leaves exposed to the sun; but when similar leaves were kept in air through the night, carbonic acid was always produced, and the purity of the air thereby diminished. He maintains, as Senebier had before done, that oxygen gas is never produced by plants, unless carbonic acid previously exist in the air; and he ascribes the production of oxygen gas, in Priestley's experiments on vitiated air, to the pre-existence of carbonic acid in it †.

* Contributions, p. 161.

† Nicholson's Journal, 8vo. vol. ii. p. 153, & seq.

305. The last author who has directed his attention to the effects produced in the air by living plants, exposed to the direct influence of the solar rays, is M. T. de Saussure, in his elaborate treatise on vegetation, to which we have already so frequently referred. In many examples, however, atmospheric air does not seem to have experienced any amelioration; which accords with the observation of Senebier and Woodhouse, that plants do not, even in sunshine, sensibly improve the atmosphere, unless it previously contain carbonic acid. M. de Saussure found, that three young peas, about 4 inches high, placed in 50 cubic inches of pure atmospheric air, and exposed, from five to six hours a day, to the direct rays of the sun for ten days, in which time they increased in weight eight grains each, had not sensibly changed either the purity or volume of their atmosphere *. When the atmospheric air was previously washed in lime water, to free it from the carbonic acid naturally present in it, the same results were obtained. After five or six days direct exposure to the sun, the air remained unchanged, both in purity and in volume, though the plants continued sound and vigorous in all their parts †.

306. Similar results were afforded by plants of *vinea minor*, placed in 290 cubic inches of atmospheric air, and exposed, six hours a day, for six days, to the light of the sun; at the end of the experiment, the plants were found to have suffered no alteration, nor to have produced any change either in the purity

* Recherches, p. 29. 32. † Ibid. p. 34. 35.

or the volume of the air. In the same manner, the atmospheric air, in which two plants of *mentha aquatica* had grown for ten days, and been exposed six hours each day to the rays of the sun, experienced no change either in its purity or its volume. The *lythrum salicaria*, vegetating for seven days, under a similar exposure to the sun's rays, produced no change in the volume or composition of its atmosphere; and M. de Saussure adds, that he could not be mistaken in the measurement of the volume of air more than one-fourth of a cubic inch. The *pinus genevensis* and *cactus opuntia*, placed in the same circumstances, were likewise found to produce no sensible change in the purity or volume of their atmosphere *. Hence we learn, that, when plants are confined in given portions of air, and placed alternately, for several days, in sunshine and in the shade, they neither vitiate nor improve the atmosphere in which they are made to grow.

307. But we have before seen, that, in the shade, plants, by their growth, uniformly convert the oxygen gas of the air into carbonic acid, and such conversion, we have argued, is the natural and necessary effect of vegetation. If, therefore, no acid be found in the air, when the process takes place in sunshine, either that gas cannot, under such circumstances, have been formed in vegetation, or, if it has been formed, it must at once have been reconverted into oxygen gas by the agency of the solar rays. Now since, in these experiments, the plants actually grew; since oxygen gas is necessary (30.) to vegetation, and

* Recherches, p. 42. et seq.

is, by that process, converted into carbonic acid, such conversion must have occurred while these plants were kept in the shade; consequently, although no carbonic acid was found in the vessels after they had been exposed to the sun, its non-appearance is not to be received as proof against its formation in the shade, but only of its reconversion into oxygen gas under exposure to the sun.

308. This inference is completely borne out by the results of experiments related by M. de Saussure. He confined plants in vessels of atmospheric air, from the top of which he had previously suspended a portion of lime; and then placed them to vegetate in sunshine. On the second day, the atmosphere was diminished in volume; and on the fifth day, the air was examined and found to be much depraved: it contained only $\frac{16}{100}$ of oxygen gas, and no sensible quantity of carbonic acid. This experiment he repeated many times with different plants, and always with similar results. We see, by these experiments, says M. de Saussure, that there had been an attraction, and consequently a formation, of carbonic acid; and we observe also another remarkable fact, namely, that plants, even in sunshine, produce, with the oxygen of the atmosphere, pure carbonic acid. It is, therefore, only because they decompose it, after having formed it, that they do not, like animals, vitiate the atmospheric air in which they are made to grow *. Hence then we learn, that, in sunshine as well as in the shade, plants, by their growth, naturally convert the oxygen gas of the air into carbonic acid.

* Annales de Chimie, tom. xxiv. p. 145.

309. That vegetables, however, with the aid of the solar rays, also possess the power of decomposing this acid, and reconverting it into oxygen gas, seems sufficiently established by the numerous facts already stated: and various experiments, related by M. de Saussure, abundantly confirm the same position. He found, as Drs Priestley and Percival had long before done, that plants would not, even in sunshine, vegetate in pure carbonic acid. If their atmosphere contained two-thirds or three-fourths of this gas, it was alike fatal to them. They vegetated seven days in an atmosphere containing one half its volume of carbonic acid; ten days in one which contained one-fourth; and when it contained one-eighth, they have acquired nearly as great a weight as in atmospheric air. Lastly, when only one-twelfth of carbonic acid was present, the plants, he says, have increased in weight, and prospered better than in pure atmospheric air *. In the shade, the smallest quantity of carbonic acid, added to common air, is injurious to vegetation. The plants have died in six days, when it constituted one-fourth of their atmosphere; and even when it was only one-twelfth, their vegetation has been feeble, and their increase of weight small. In all cases, oxygen gas, he adds, must be present; for if a small portion of carbonic acid be added to pure nitrogen gas, plants die in it, even though exposed to the sun †.

310. Having thus ascertained the general effects of carbonic acid on vegetation, he proceeded to make

* Recherches, p. 31.

† Ibid. p. 33.

direct experiments on its operation, when introduced in small quantity, and exposed to the influence of the solar rays. He procured seven plants of *vinca minor*, each about eight inches high, and placed them in an artificial atmosphere of 290 cubic inches, composed of common air, mixed with $\frac{7\frac{1}{4}}{100}$ of carbonic acid. The roots of the plants were immersed in a little water, and the vessel that contained them was inverted in mercury, covered by a thin film of water, to prevent its noxious operation on the plants *. This apparatus was exposed six hours to the direct rays of the sun, for six days. On the seventh day, the plants were withdrawn, and had undergone no alteration: the bulk of air had not apparently increased nor diminished: neither did it exhibit any sign of containing carbonic acid, when washed with lime water. To make up, however, for the 21.75

* M. de Saussure observes, that this effect of mercury on vegetables was first noticed by the Dutch chemists, and he found it always to operate when the experiment was continued for any length of time. In one instance, where we had confined some willow slips standing in the shade, in a small bottle of water under a jar of atmospheric air, inverted over mercury, the leaves, in two days, were changed to a dark brown colour, and by the third day were quite black. This blackness extended through their substance, and they easily separated from the stem, though they had no putrid smell. The air did not contain any sensible quantity of carbonic acid, and afforded nearly its full proportion of oxygen. It was not in the least altered in bulk, for only a small globule of mercury, not bigger than a pin's head, had gotten within the jar, and the lowest part of the leaves was more than three inches above it, which was the height of the phial, in which the vegetables were placed.

cubic inches of carbonic acid originally present, but which had now entirely disappeared, the atmosphere was increased by 14.75 cubic inches of oxygen gas, and seven cubic inches of nitrogen, which, together, supply exactly the bulk of the carbonic acid lost *. Similar experiments were made with plants of *mentha aquatica*, *lythrum salicaria*, *pinus genevensis*, and *cactus opuntia*, and nearly with the same results ; the carbonic acid having, in all cases, more or less completely disappeared, and its place being supplied by nearly an equal quantity of oxygen, with a small portion of nitrogen gas †.

311. The foregoing experiments seem to afford undeniable evidence of the decomposition of carbonic acid by growing vegetables, when they are exposed to the direct rays of the sun, since not only did this acid uniformly disappear, in circumstances where no substance was present, which could attract or combine with any considerable portion of it, but a large volume of oxygen gas was produced, without the vegetables having undergone any change, by which they can have been rendered able to supply it. That this superabundant oxygen did not proceed from the decomposition of the plant, or of the water which it contained, is farther proved by the results of the experiments (305.), made in pure atmospheric air. These experiments were purposely made at the same time, with similar plants, and under the same circumstances ; and yet, as we have seen, no absolute

* Recherches, p. 41.

† Ibid, p. 44. & seq.

addition of oxygen gas was made to the atmosphere in which the vegetables grew, but both its volume and composition remained permanently the same. Even if decomposition of the vegetables had taken place, we do not know that they could have directly afforded pure oxygen gas; but we do know that this gas is furnished by them in union with carbon; so that the existence of an additional quantity of oxygen, in such circumstances, would have afforded equal evidence of the decomposition of carbonic acid, by the agency of solar light.

312. But not only by the direct rays of the sun, but without the intervention of a strong light, it is probable, says M. de Saussure, that plants, growing in atmospheric air, decompose a part of the carbonic acid, which they themselves form out of the surrounding oxygen gas, although the fact be not susceptible of direct proof. Marsh plants, as the *polygonum persicaria* and the *lythrum salicaria*, yielded oxygen gas by a weak and diffused light, when confined in an atmosphere of nitrogen*: and different species of *epilobium* vegetated a long time, and grew as well in pure nitrogen gas, as in common air, though exposed only to a weak light, or protected from the direct action of the sun. The nitrogenous atmosphere, at the end of two months, was increased in bulk, and contained $\frac{1}{100}$ of oxygen gas. When similar plants were confined in pure nitrogen gas, and kept in perfect darkness, though they were renewed

* Recherches, p. 54.

every twelve hours, lest their vegetation might languish, yet they produced no oxygen gas, but augmented their atmosphere, by a quantity of carbonic acid *.

313. Even in perfect darkness, continues M. de Saussure, some plants would seem to possess the power of decomposing carbonic acid. He confined plants of peas and of salicaria, in two equal recipients of atmospheric air, and set them aside in a place perfectly dark. One of the recipients contained a portion of lime or potassa, while the other held the plants only: in both vessels, the plants were daily renewed. At the end of four or five days, the air, in both recipients, was vitiated; and in several repetitions of the experiments, he constantly found that the air of the vessel, which held the lime or potassa, contained less oxygen gas, than that of the vessel which had no alkali. This difference, it is added, may be presumed to arise from less acid being present for decomposition in the vessels, which contained the alkali, than existed in the recipients which held no alkaline substance †.

314. We have already stated (25.) that artificial light changes the colour of plants in a manner similar to that of the sun. M. Decandolle found, that various plants, raised from seeds in a vault lighted by lamps, became green both in their stems and leaves, nearly like those which grew in the open air. He confined also the leaves of various plants in inverted glass vessels of water, and placed them in a

* Recherches, p. 199. 201.

† Ibid. p. 55.

vault illuminated by six lamps, which afforded light equal to fifty ordinary candles. For the first twenty-four hours, no air was afforded, but at the end of that time it collected in considerable quantity ; and, on being analysed by M. Vauquelin, was found to consist principally of nitrogen and carbonic acid, with only $\frac{1}{100}$ of oxygen gas *. These experiments shew, that though artificial light renders vegetables green, yet it is not sufficiently powerful to decompose carbonic acid in plants, at least in any considerable quantity; and in this respect, therefore, it resembles, in its operation, the effects of a weak natural light.

315. The probability of the decomposition of carbonic acid by plants, growing in a weak light, is supported by many facts, which seem otherwise incapable of explanation. The germination of seeds we have seen to produce a considerable consumption of oxygen ; and it might, therefore, be naturally expected that plants should continue to affect the air at least in an equal degree. The changes, however, effected by plants in a state of full growth and vigour, on the surrounding air, says Mr Murray, are much less than have been imagined, and, when supplied merely with water, they are so inconsiderable as not to be very perceptible. Hassenfratz, he continues, inclosed growing plants in atmospheric air for a month and a half or two months at a time ; and though the plants grew, the air remained unaltered in volume, and in the proportion of oxygen †. This slowness of change

* Journ. de Phys. tom. lii. p. 124.

† Syst. Chem. v. iv. p. 54.

in the air is remarked, likewise, by Dr Woodhouse, who says that a plant in perfect health, confined in atmospheric air, will live in it for a long time without producing any sensible change. In his experiments, many vegetables did not affect the air in five days; some diminished its purity in three hours; and others altered it in a most slow and gradual manner, causing little change in it in twenty days *.

316. If, therefore, we compare this slow consumption of oxygen gas by plants, with its more rapid conversion by seeds, we must either suppose that plants require the aid of those advantages, derived from the air, in a degree infinitely less than seeds; or, what is more probable, we must believe that the necessary changes, which their vegetation produces in the air, are, at all times, more or less counteracted by some other office which they perform; in other words, that the oxygen gas, which is converted into carbonic acid by the act of vegetation, is again restored, more or less completely, to the atmosphere, by some other process. This restoration we have seen to be actually made, and it now only remains that we endeavour to discover the manner in which it is carried on, and try to ascertain whether it be, or be not, executed by the proper vegetative powers of the plant.

* Nicholson's Journal, vol. ii. p. 152.

317. If we take a retrospective view of the circumstances in which oxygen gas was produced in all the experiments which have been now detailed, it will, we think, appear, that, conformably to the opinion of Senebier and Woodhouse, carbonic acid gas was always previously present. Thus, when Dr Priestley employed air that had been vitiated by respiration, combustion or putrefaction, which for the most part contains carbonic acid, the air was frequently much ameliorated and rendered capable of again supporting vegetable life; but when he withdrew the oxygen gas of the atmosphere, by means which imparted to it no carbonic acid (247.), the plants never failed to die. So, likewise, M. Scheele was never able to render vitiated air pure, even by exposure to the sun, because he previously abstracted (247.) from it the carbonic acid which it contained. In the air, depraved by respiration and combustion, which Ingenhousz employed (290. 1.), carbonic acid seems always to have previously existed. The experiments and observations of Senebier (298. 9.) and Woodhouse (304.) altogether tend to prove, that oxygen gas is never afforded by the leaves of plants, either in water or in air, unless carbonic acid be present; and that the quantity of oxygen supplied is in proportion to the carbonic acid that disappears. Lastly, in the experiments of De Saussure we have seen, that, when no carbonic acid previously existed in atmospheric air (305.), no additional quantity of

oxygen was afforded to it; but that, when this acid gas was first mixed in certain proportions with the air, it constantly disappeared (310.), and the atmosphere then contained a superabundant quantity of oxygen gas. The numerous and varied proofs of the decomposition of carbonic acid, afforded by the experiments of Priestley, Ingenhousz, Senebier, Davy, Woodhouse, and De Saussure, add great weight to the opinion for which we now contend, since they amount to demonstrative evidence, that carbonic acid is actually decomposed by the concurring operation of the green parts of vegetables and of solar light.

318. In what manner, then, does carbonic acid obtain admission into the vegetable, so as to be afterwards afforded by it in the form of oxygen gas? It is well known, that this acid is readily attracted by water, and is almost universally contained in it. Water, it is likewise well known, is absorbed by the roots and leaves of plants; and that it carries air with it into the vegetable body, is established by numerous observations. Dr Hales remarked air bubbles to rise, very abundantly, in hot weather, from vegetable sap collected in tubes, so as to make a froth an inch deep at the top; and when, also, the sap was placed in an air pump, it afforded, as the receiver was exhausted, great plenty of air bubbles *. Mr Knight found the sap, whether extracted from the tree near to the ground, or at a distance from it, al-

* Veg. Statics, vol. i. p. 125. 3d edit.

ways to contain a large portion of air *; and M. Coulomb, on piercing an Italian poplar, observed the sap to flow out abundantly, with a continued noise of air bubbles, which ascended and burst in the orifice †. These bubbles M. Senebier found, in some cases at least, to be carbonic acid; for if the clear sap of the vine was mixed with lime water, a white precipitate was formed, which could again be dissipated with effervescence by a few drops of nitric acid ‡. M. Vauquelin also, in his analysis of the sap of the elm, found it to contain lime, which was held in solution by carbonic acid, of which there existed a considerable excess in the sap ||. These facts, therefore, afford sufficient evidence of the existence of this acid gas in the sap of plants, and of its escape when this sap comes into contact with the air.

319. Besides the carbonic acid, which thus enters the substance of the plant by the medium of water, other gases, dissolved in water, in the same way gain admission into the vegetable body. Thus, the experiments of De Saussure seem clearly to prove, that oxygen gas may, in this manner, be carried into the vegetable ¶; and since nitrogen gas is likewise contained, in a smaller proportion, in water, it may be presumed, that, by a similar conveyance, it gains admission also, and is again given out unaltered in those

* Phil. Transact. 1805.

† Phil. Magaz. vol. ix. p. 310.

‡ Physiol. Veg. tom. ii. p. 343.

|| Thomson's Chem. vol. iv. p. 262. 1st edit.

¶ Recherches Chim. p. 111.

cases (310.) where we have seen it to be afforded by plants in confined air.

320. The gases, which thus enter by the roots and exist in the sap, will be conveyed with it to the leaves. M. Senebier plunged branches of the peach tree into recipients containing water charged with carbonic acid; so that, in some instances, the ligneous extremity of the branch was also immersed, while, in other instances, this extremity was confined in an empty bottle. The result was, that the leaves of the branches, whose ends were immersed, afforded in sunshine double the quantity of oxygen that was yielded by the others; whence he concludes, that the carbonic acid passes with the water into the leaves and is then decomposed *. In the experiments of M. de Saussure, the attraction of oxygen by water, the absorption of this water by the roots, and the subsequent expulsion of oxygen from the leaves, are clearly established †; so that the conveyance of gaseous fluids into the leaves, by this route, cannot be reasonably doubted.

321. But not only by the roots does carbonic acid thus obtain admission into vegetables, but likewise by the leaves. The power of the leaves to absorb water has been amply established by the experiments of Bonnet; and the gaseous fluids, which exist in this water, may readily be conceived to enter with it. The production of oxygen gas by leaves, immersed in water containing carbonic acid (288.), can-

* Physiol. Veg. tom. iii. p. 225.

† Recherches, p. 111.

not be explained without supposing this acid first to enter, in solution, into the substance of the leaf; and that it actually does enter, seems to follow from the experiments of M. Senebier. He immersed the leaves of *sedum* in boiled water, and placing them under the receiver of an air pump, exhausted the vessel till the leaves sank. They were then removed into water impregnated with carbonic acid, and, in seven minutes, they rose from the bottom of the vessel, and, when exposed to the sun, afforded oxygen gas in great abundance; which proves, he adds, that leaves imbibe this acid gas, and again expel it in another state, under exposure to the sun. When, on the contrary, the leaves were thoroughly exhausted of air, and exposed to the sun in boiled water, in which no air existed, they did not furnish an atom of air *.

322. Much attention has been employed to ascertain the part of the leaf in which the carbonic acid is contained and decomposed, and the surface from which it afterwards issues. As the gas itself is invisible, its escape can be detected only by placing the leaves in water; or, if they are retained in air, by marking the increase of volume which the air may receive. This latter method, however, enables us to judge only of the fact, but not of the mode in which it is accomplished. From experiments, made by immersing leaves in water, Dr Ingenhousz supposed the air to issue chiefly from the *inferior* surface of the leaves. From the leaves of some fleshy plants,

* Phys. Veget. tom. iii. p. 242.

which were cut in pieces and exposed in water to the sun, he observed very pure air to issue, in continual jets, from their parenchymatous structure; but in other plants, which afforded air very abundantly, its escape was not visible from either surface of the leaves *.

323. M. Senebier also found the *inferior* surface of the leaves to yield the most air; that the epidermis, when removed from the other parts, afforded no air, but that the air then issued abundantly from the denuded parenchyme; wherefore he concludes, that the parenchyme is the true seat of the air that is obtained. The air, which thus exists in the parenchyme, he supposed to escape through the epidermis; for when the surfaces of the leaves were covered with paste or varnish, no air then issued; but he was never able to discover the pores or orifices through which it obtained a passage †. The foregoing observations and experiments seem to prove that the gaseous fluids of plants exist principally in the parenchyme of the vegetable, and from thence issue into the surrounding atmosphere.

324. According to M. Jurine, the parenchyme of the leaf is composed of an aggregation of small cells or *utriculi*, in which a green juice is contained. The form of these cells is either spherical, cylindrical, elongated or irregular; and according to their form, the spaces between them, which he calls *utricular interstices*, will be more or less large. It is in these

* Exper. tom. i. p. 12. 24. 26.

† Mem. Physico-Chimiques, tom i. p. 128. et seq.

interstices that the air, which leaves contain, seems to be lodged. In a leaf of fritillary (*fritillaria*) and of purslane (*portulaca*), small luminous points may be seen by the microscope, or even by the naked eye, which points are said to be produced by the light reflected from the air contained in the utricular interstices. To prove this, he removed the pellicle of a leaf of fritillary, and cut the parenchyme, so that he could observe several of these luminous points; and then, by gently compressing the parenchyme under water to force out the air, he saw it issue in the form of bubbles, and the water, having taken its place, rendered the interstices transparent, which were before opaque. In the petal of the rose, these inter-cellular spaces are very large; and if the petal be slightly compressed under water, the air it contains will be seen to move with rapidity, following the different inflections of the utricular interstices *.

325. But for the escape of the air thus lodged in the interstices of the parenchyme, some passages must exist, and these are to be found in the external pores. It has been much disputed among late writers, whether the leaves of plants are furnished with a proper and distinct *epidermis*. The late M. de Saussure considered what is called the epidermis to be composed of three different textures. M. Mirbel, however, denies its existence, and considers, what is so called, to be the union of the contiguous surfaces of the external cells; and M. Jurine, also, describes this covering as formed by the exterior stra-

* Philos. Mag. vol. xvi. p. 109.

tum or face of the *utriculi*, for he could never find the distinct epidermis of Saussure. But whether this covering be, or be not, a distinct membrane, it is admitted by all to be furnished with innumerable pores. In a leaf of the bulbous lily, Hedwig counted 577 pores in the small space of a square line; and M. Jurine reckoned 140 in the same space of a leaf of fritillary. These pores are in form either oblong, oval, or irregular; and they extend, in different directions, over the surface of the leaf. Some leaves have them on both sides; some, on one side only; and other leaves are wholly destitute of pores. In the leaves of trees, they are chiefly distributed on the under surface, in the juniper on the upper, and in the fir and larch they are found on both surfaces. All the parts of the flower are likewise furnished with them. When observed by the microscope, after moistening the pellicles, the pores appeared to M. Jurine to be filled with a black matter, which was a small bubble of air contained in them; for, by gentle pressure with a fine needle, the bubble was detached, and the part then became transparent. And when the pores of different pellicles, previously removed from the leaves, were compressed under water, the air that issued from them gradually disappeared, which led him to conclude that it was carbonic acid.

326. That these pores communicate with the utricular interstices before described, he supposed from the following experiment. He placed the leaves of the *geranium peltatum* and *rumex sanguineus* in water under the receiver of an air pump, and observed air

to appear on both their surfaces when the receiver was exhausted, and again to disappear, when the equilibrium was restored; from which he concluded that the air had been first drawn out of the leaf, and had afterwards re-entered it. In the *olea fragrans*, the upper surface of whose leaves is destitute of pores, the inferior surface alone was covered with bubbles, which disappeared in the same manner when the atmospheric equilibrium was restored *. It must however be observed, that the air, in these experiments, might be derived from the water, and its disappearance be caused by its re-attraction by that fluid, when the pressure of the atmosphere was restored. That air, however, does escape from leaves *in vacuo*, is rendered certain by the experiments of M. Senebier, who found a leaf of *sedum* to sink in boiled water placed under an exhausted receiver, and again to swim after being placed for a few minutes in water charged with carbonic acid †.

327. Besides entering vegetables through the medium of solution in water, air seems to gain admission into them by other means. According to Dr Bell, Dr Hill first shewed the cuticle of vegetables to be an organised vascular substance, which, in trees and shrubs, has external openings, but not in herbaceous plants. When a portion of a tree is placed under an exhausted receiver, air enters through the cuticle and issues from the wood, and we may therefore conclude that the proper entrance

* Philosophical Magazine, v. xvi. p. 4. 11. 110.

† Physiol. Veget. v. iii. p. 242.

of air into plants is through these cuticular vessels *. Many experiments of a similar nature were made by Dr Hales, who concluded from them that air entered plants not only with the nutrient matter by the roots, but also through the bark and the leaves †. These experiments, however, as Dr Smith observes, prove only that the vegetable body is pervious to air when emptied of its sap, or when pressed by the weight of the atmosphere ‡; but they afford no proof that the transmission of air through the vegetable structure is a natural and necessary function of vegetation.

328. Some experiments of Dr Priestley furnish evidence of a different nature respecting the entrance of air into plants. He found that the willow plant *(epilobium hirsutum)*, while growing in water and confined in jars of hydrogen gas, greatly diminished the bulk of gas, and rendered it more pure; so that it was affected by nitrous gas, and exploded, when fired, like a mixture of common and inflammable air. The leaves continued green, and appeared to be always loaded with air bubbles, which were continually detached, and their place supplied by others. These bubbles, he supposed to be oxygen gas, and to have been the cause of the purification of the hydrogenous atmosphere; and he believed them to proceed from the plant itself, and not to be separated from the water in which it grew §.

---

* Manch. Mem. v. ii. p. 403.

† Veg. Statics, vol. ii.

‡ Introduct. to Bot. p. 200.

§ Obs. on Air, vol. v. p. 9. 10.

329. According to Dr Ingenhousz, plants, inclosed *in vacuo*, continually give out air, which in sunshine is oxygen gas, and in the shade is carbonic acid and nitrogen. The air contained in onions and in many aquatic plants he represents to be nearly the same as that which surrounds them. In tubes filled with nitrogen, the gas afforded by rushes was of the same quality; and when the tubes were filled with oxygen, the plants afforded gas of the same degree of purity. Similar results were obtained when the tubes were filled with hydrogen or carbonic acid gases, the air afforded being in every case similar to that which previously surrounded the plants. The experiments succeeded in sunshine and in the shade, and the change in the quality of the air was effected in half an hour; so that it is concluded, that the air existing in plants is never stagnant, but is constantly and speedily renewed *.

330. M. Senebier ascertained, by experiment, the truth of the foregoing observations, as far as they relate to plants which have a reservoir for air, such as the stems of onions and rushes; but that they do not hold with regard to plants of a different structure. The air which generally issued first from plants *in vacuo* he found to be nearly similar to that of the atmosphere, but it diminished in purity as the experiment was prolonged. It varied, however, in the same plants, at different times; and contained different proportions of oxygen, carbonic acid, and nitrogen gases †.

* Exper. t. ii. p. 96. 99.

† Physiol. Veg. t. iii. p. 120. et seq.

331. Many experiments of M. de Saussure seem, likewise, to prove that air exists naturally in plants. He found that green-leaved plants, especially such as had fleshy leaves, lived a long time in pure nitrogen, and afforded to it a considerable portion of oxygen gas in the sun, but yielded only carbonic acid in the shade. Similar results were obtained when the plants were confined in an atmosphere of the gaseous oxide of carbon. Many fleshy plants continued, also, to vegetate *in vacuo*, which M. de Saussure supposed them capable of doing only in consequence of the air they contained in their parenchymatous structure, which air was decomposed and eliminated by the action of the sun *. If, farther, we believe that the decomposition of carbonic acid by leaves is effected only in their parenchymatous structure, we must necessarily suppose, that, in the experiments of Davy and De Saussure, as well as in all others in which this gas was decomposed, it must first have entered into the substance of the leaf. From the facts, therefore, which have now been stated, it seems to follow that gaseous fluids obtain admission into plants, both through the medium of solution in water, and under an elastic form.

332. But the carbonic acid, which may thus enter vegetables, we have seen to undergo decomposition (317.), and to be partly expelled in the state of oxygen gas. How far, then, are we entitled to consider these operations as the necessary result of a living vegetable function? Preparatory to the decision of this question,

* Recherches, p. 200. 9. 14.

it may be useful to bring together and contrast the peculiarities which distinguish, respectively, the consumption and the production of oxygen gas by living plants; for whether we regard the agents employed, the mode and circumstances in which they act, or the results which are afforded, there are no two processes, which, in many respects, present phenomena more dissimilar. Both in the shade and in sunshine, however, oxygen gas is essential to vegetation; but in one situation, it is converted into carbonic acid; in the other, this acid is re-converted into oxygen gas. Without the presence of oxygen, the living plant is unable to survive; but an atmosphere of carbonic acid speedily destroys its life. When oxygen is consumed, the union of that gas with carbon appears to take place exterior to the vascular system of the plant; when the same gas is produced, the decomposition of carbonic acid is effected in the cellular structure of the leaf. To the continuance of living action the formation of carbonic acid seems to be essential; but its decomposition is, in no respect, necessary to the life of the plant. The one operation proceeds continually, by day and by night, in sunshine and in the shade; the other takes place only at intervals, and while the plant is exposed to the sun. Where, without light, oxygen is consumed, the plant lives and grows, but its colour and properties are impaired; where light is present, and oxygen is produced, the colour and other properties of the vegetable attain the greatest perfection. Lastly, for the conversion of oxygen gas into carbonic acid by plants, a certain temperature is required; but the production of this gas, by the decomposition of carbonic acid, occurs in temperatures unequal to the support of vegetation. These numerous and striking dissimilarities must be allowed to create an essential difference between that operation of plants, by which oxygen gas is converted into carbonic acid, and that by which this acid is re-converted into oxygen gas.

333. Now the former process, by which oxygen is consumed, must be regarded as necessary to vegetation, because without air plants do not grow; and no other known change is produced in this air than the conversion of its oxygenous portion into carbonic acid gas. This conversion, also, is connected with the living powers of the plant; for when these powers are suspended by cold, the plant no longer continues to vegetate, and the oxygen of the air is then no longer changed. It proceeds, likewise, at all times, and in all situations; so that if a circumstance which universally accompanies vegetation, and without which it cannot continue, be entitled to be considered a necessary condition of that function, then the conversion of oxygen gas into carbonic acid by living vegetables must be so considered.

334. But, on the other hand, the production of oxygen gas, by plants exposed to the sun, is not, necessarily, accompanied by any signs of vegetation; neither does it require the conditions, which are essential to the exercise of that function. Thus, although air is indispensable to vegetation, yet various experiments prove that oxygen gas is afforded by the leaves of plants, when they are immersed in water. It may be said, that the air, existing in the water, is,

in this case, supplied to the plant; but Ingenhousz asserts, that oxygengas is likewise produced by plants immersed in water, from which all the air has been previously expelled by boiling *. Nay, the experiments of Priestley, Senebier and Woodhouse demonstrate, that the production of oxygen by vegetables takes place not in proportion to the quantity of that gas which the water may contain, but to that of the carbonic acid which exists in it. It is a fact, however, that boiled and distilled waters, deprived of air, do not support the vegetation, even of aquatic plants, and water fully saturated with carbonic acid is still more destructive; for the *conferva rivularis* and *potagmogeton crispum*, says Dr Ingenhousz, were soon destroyed in it †; and yet from such water, so impregnated in a slighter degree, he often obtained, by means of plants, a great quantity of air of exquisite purity ‡.

335. Nor is it only when they are thus secluded from air, or immersed in water deprived of air, or in that which is impregnated with carbonic acid, that this operation of yielding oxygen gas is performed by the leaves of vegetables; for the experiments of Davy (300.) shew, that these leaves, when confined in pure carbonic acid, equally afford oxygen gas, and those of De Saussure prove the same thing to happen in vessels of hydrogen or nitrogen gas ||, although it is universally admitted that these gases are incapable

---

* Exper. tom. ii. p. 321.

† Exper. tom. ii. p. 270. ‡ Ibid. p. 283.

|| Recherches Chim. p. 84. et seq.

of supporting vegetation. If, therefore, we pronounce this operation of the leaves to be a vegetative function, we must maintain that vegetation takes place not merely without the presence of oxygen gas, which is necessary to vegetation, but that it actually goes on most rapidly when a gas is present, which is absolutely destructive to that process.

336. In like manner it has been shewn (24.), that heat is necessary to vegetation, and that this process does not go on if the heat be, to a certain extent, withdrawn; yet M. Senebier found, that all vegetables, which continued green, furnished oxygen gas in the sun, when the temperature of the air was many degrees below zero *. Thus, we see that the presence of light enables the plant to furnish oxygen gas, when the heat, necessary to vegetation, is withdrawn; and, on the contrary, it has before been shewn (25.), that all plants will vegetate in circumstances where heat is present, but where light is either partially, or totally excluded.

337. But not only without pure air and heat do plants, when exposed to the sun, seem capable of yielding oxygen; but they afford it also in such a state of mutilation, as must be completely destructive to their vegetative power. There is a great difference between the mere possession of life, and the performance of living action. Neither the stem alone, nor the leaf alone, can execute the proper vegetative function of the plant; for the stem must possess an embryo plant or bud to enable it to grow, and, though

* Physiologie Veg. tom. iii. p. 278.

the leaf may, for a long time, be kept in life, yet it neither grows, nor produces any new vegetable matter, when it is separated from the stem. In many of the experiments, however, of Ingenhousz * and Senebier †, oxygen gas was not only produced by the leaf after it was detached from the plant, but even after it had been cut into small pieces, or was so much withered as to be unable to support an erect posture. When, indeed, it was beaten to a pulp, Dr Ingenhousz did not find it capable of affording pure air; but this only shews that a certain state of vegetable organization is necessary to the discharge of this office, but does, by no means, prove, that it is performed by the vegetative function of the plant.

338. Dr Ingenhousz himself, indeed, attributed the salutary influence, which plants exert on the air, to the direct light of the sun, conjoined with a certain state or condition of the vegetable structure, but not, in any degree, to vegetation, as such. At first, indeed, he believed it to depend on vegetation; but if vegetation, he adds, were the cause, plants should produce the same effects at all times, and in all situations, where they are able to vegetate; but they live and grow, to a certain size, in darkness, where they neither afford good air, nor possess the power of improving that which is bad ‡. Since, then, it appears, that the production of oxygen gas by plants takes place independently of vegetation, and without the

---

* Exper. tom. ii. p. 166.

† Mem. Phys. Chim. tom. i. p. 118.

‡ Exper. tom. i. p. 58. tom. ii. p. 35.

conditions essential to the exercise of that function, it cannot be regarded as an operation necessary to the existence of the plant, or dependent on its living powers, but must be considered as a secondary and subordinate office, the performance of which is entirely governed by the accidental circumstances in which the vegetable may happen to be placed.

339. But if it be thus granted, that the operation, by which plants afford oxygen, may go on in circumstances where the powers of vegetation are not only suspended, but where they are unable to exist, then it also follows, that the entrance of gaseous fluids into plants, by which that operation is supported, is not necessarily to be considered as the result of a vital function. These gases have been shewn (318.), in certain cases, to enter plants with the fluids which they absorb from the soil; and if this, as is probable, be the ordinary way in which they gain admission, then we may say generally, that gases are *absorbed* by plants. But as these fluids may likewise be received into plants, although they hold no gases in solution, and vegetation may then also go on, the absorption of gases cannot, in this sense, be deemed a proper and necessary function of the plant.

340. Farther, gaseous bodies enter into plants, in an elastic form, and, as we have seen (300.), under circumstances fatal to the exercise of their living powers; in which case, their admission must depend on the operation of a chemical, or mechanical cause. Do we then possess evidence that such an affinity subsists between vegetables and certain gases, as is sufficient to explain the phenomena which have been

now described? That an affinity subsists between the carbon of vegetables, and the oxygen of the air, the phenomena of vegetation abundantly testify; and some experiments of M. de Saussure seem, also, to shew, that, beside converting oxygen gas into carbonic acid, certain vegetables possess an *attractive* power for that gas, under which it enters, unchanged, into the vegetable body. Thus, he remarks, that if the leaves of the *cactus opuntia* be confined, through the night, in atmospheric air inverted over mercury, they will remove a portion of its oxygen without producing an atom of carbonic acid, or, in the smallest degree, affecting its nitrogenous portion*. When the leaves have thus acquired a full dose of oxygen, they begin, says M. de Saussure, to produce carbonic acid, by yielding their carbon to unite with the surrounding oxygen,—a combination which does not, in the least, affect the volume of air. The greater number of leaves, and, in particular, those which are not fleshy, thus form carbonic acid, at the same time that they receive, or, as M. de Saussure says, inspire oxygen gas †.

341. But though the entrance of oxygen gas, to the exclusion of nitrogen, seems, in the foregoing examples, to favour the notion of the operation of an *elective* affinity; yet M. de Saussure observes, that when the *cactus* has been confined through the night in pure hydrogen gas, it has afterwards afforded, by the action of the air-pump, small quantities both of hydrogen and nitrogen gases. He also found, that

---

* Recherches Chim. p. 64. † Ibid. p. 67.

where a small portion of carbonic acid was added to atmospheric air, it entered the vegetable in a proportion equal to that of oxygen; but none entered, if the plant was confined in pure carbonic gas *. In the experiments also, which have been related (328. et seq.), not only oxygen, but hydrogen, nitrogen and carbonic gases obtained free admission into the vegetable body. The experiments likewise of Davy (300.), and of De Saussure (310.), prove the entrance of carbonic gas into the leaves of vegetables; so that if the entrance of these gases be ascribed to chemical affinity, this affinity is not confined to one gas, but extends to many. It farther appears, that, though oxygen is generally converted into carbonic acid after its entrance into the leaves, yet it is again completely expelled in the state of oxygen gas, under exposure to the sun, and the other gases are often expelled without any sensible change in their properties, or any diminution in their quantity; wherefore no part of them can have been appropriated by the plant †.

342. Another curious circumstance, in the history of these phenomena, is, that various gases not only enter vegetables, without regard to their nature or composition, but they do so in a quantity, which, in many cases, considerably exceeds the bulk of the containing body. The leaves of the *cactus*, when confined through the night in atmospheric air, remove a portion of its oxygen equal to three-fourths of their own volume; and if the experiment be pro-

* Recherches, p. 70. 72. † Ibid. p. 82. et seq.

longed to thirty-six or forty hours, these leaves will have taken up a volume of this gas greater by one-fourth than their own bulk *. It farther appears, that the organic structure of the leaf is necessary to the exercise of this office; for when the leaf is previously reduced to a pulp, it does not diminish the volume of atmospheric air, but only changes its oxygen into carbonic acid †;—a fact which accords with an observation of Ingenhousz, who remarked, that leaves, when beaten to a pulp, did not, like those which were entire, afford oxygen gas in the sun. The leaves of various other plants, of different species, were found, by M. de Saussure, to take up a quantity of oxygen, equal or superior in bulk to themselves, when they were confined in atmospheric air and kept in darkness ‡; and this gas was again expelled from them when they were exposed to the sun ||. Thin-leaved plants, however, which possess a small extent of parenchymatous structure, do not at all vary the bulk of their atmosphere, when they are placed alternately in sunshine and in the shade; and, if kept entirely in the shade, they diminish both the purity and the volume of their atmosphere §.

343. Granting, therefore, that, in the foregoing examples, chemical affinity is, to a certain extent, exerted between the vegetable and the elastic fluid that surrounds it, yet the almost indiscriminate reception of such fluids would seem to indicate that mechanical causes likewise participate in the pro-

* Saussure Rech. p. 66. † Ibid. p. 74.
‡ Recherches, p. 81. || Ib. p. 82. § Ib. p. 91.

duction of this effect. This is rendered more probable, from the circumstance, that the organic orm of the leaf is an essential condition in the operation; for, if chemical action alone were concerned, there does not appear any good reason why affinity should not in part produce a diminution in the air, eve although the texture of the leaf were broken down;—a circumstance which cannot be supposed entirely to suspend or change, though it may be conceived to modify, the action of this power. It is farther remarkable, that thin leaves, although, in chemical composition, they resemble thick ones, exert little or no effect of this kind, which shews that it is not so much the quality, as the form, of the substance, that influences the operation.

344. Since, then, it appears, that, though chemical affinity be present, and allowed to operate, it is yet unequal to the explanation of the phenomena in question; let us, in the next place, direct our view to the consideration of mechanical causes. Now, mechanically considered, gaseous bodies can only be conceived to enter plants to the exclusion of some other substance. When, therefore, the vessels or cells of plants are already filled, no air can be supposed to gain admission, but by the displacement of a corresponding bulk of the contained fluids. Dr Hales, accordingly, found, that although, by means of the air-pump, he could produce a free current of air through sticks of considerable size, yet, from young and succulent shoots, placed in similar circumstances, and which yielded their fluids very

slowly, little or no air issued *. Unless, therefore, we suppose some cause capable of emptying the vessels of plants, it is difficult to conceive how the air can, simply by the force of mechanical pressure, enter and be transmitted through them.

345. All the circumstances, which have been already stated, seem to shew that the air, which enters plants in an elastic form, is chiefly, if not entirely, diffused through their cellular structure, and does not gain admission into their vascular system. Hence the hollow stems of herbaceous plants, and the cellular structure of thick leaves, chiefly furnish air; and when, therefore, the texture of these parts is broken down, the vegetable substance no longer exhibits the property of diminishing or attracting air. The universal distribution of this cellular tissue through the vegetable body, its loose and porous structure, its comparative emptiness, and the ready communication that probably obtains through its whole extent, fit it, in an especial manner, for receiving and containing elastic fluids; while the closer texture of the vascular system, its less general communication, and, above all, its occupancy by the vegetable fluids, present, on every side, obstacles to the transmission of air.

346. But granting that the air, which enters in an elastic form, resides chiefly in the cellular or parenchymatous structure of plants, we have yet to investigate the cause of its entrance, and the still more unaccountable fact of its existing in the vegetable, in

* Veg. Statics, vol. i. 155. 160.

a bulk that exceeds its own absolute volume. Speaking then mechanically, no other cause of the first entrance of air into vegetables occurs, but that of pressure: and when we consider the vast force which the atmosphere exerts, in order to preserve an equilibrium, we seem to approach a cause adequate, in a certain degree, to the purpose assigned it. If, therefore, from variations in temperature, from changes in the state of the vegetable, or from chemical agency, a partial *vacuum* should, at any time, be created in the plant, air would quickly rush in to restore the equilibrium; and thus, on mechanical principles alone, the entrance of air may, to a certain extent, be explained. This explanation, however, applies only to elastic fluids generally; but does not explain why, when atmospheric air is employed, its oxygenous portion only should enter the leaf, indicating an *election*, which can be accounted for only on the principles of chemical affinity. Hence, then, we should say, that the air was brought within the sphere of attraction, by the operation of mechanical pressure, but that the actual attraction of oxygen was determined by the superior chemical affinity which the vegetable substance possessed for that gas. When, however, oxygen is not present, then nitrogen or hydrogen will enter according to their respective affinities, or, if they be considered to exert none, they will, in a certain degree, be forced in by mechanical pressure alone.

347. After the admission of such fluids, different chemical affinities may be exerted towards different gases. Thus, oxygen may be converted into car-

bonic acid, and the subsequent attraction of this acid by the vegetable substance may favour and promote its farther entrance: and such, probably, is the cause why oxygen is received in a quantity so much exceeding that of every other gas, except carbonic acid. But some other cause, beside chemical change, seems, in many cases, to operate in the production of these phenomena, else there appears no reason why the organic form of the plant should be so essential to their completion. Now, we know, that, besides being chemically attracted, air adheres to the surfaces of all bodies; and, therefore, the more extensive the surface may be, the more extensive will be the adhesion between it and the air. To us it appears probable, that some such operation is exerted by the extended cellular structure of plants; and therefore it is, that this structure aids the operation in question. It may, indeed, be said, that the same circumstance would enlarge the sphere of chemical action, and, by thus more extensively causing the production of carbonic acid, and its subsequent attraction by the vegetable substance, give rise to the phenomenon in question. But whether the elasticity of the air be, in these examples, overcome by this mechanical operation of adhesion, or by the effect of chemical change, or by both conjointly, we are compelled to conclude that cellular or fleshy leaves possess the property of receiving and retaining a quantity of elastic fluid, which, at the ordinary pressure and temperature of the atmosphere, considerably exceeds their own bulk.

348. This conjoined operation of mechanical and chemical causes, which we have supposed to pro-

duce the entrance of air into the vegetable body, is exhibited in the production of many other effects. Thus water is capable of receiving and containing within its pores different gaseous fluids, in very different proportions. Nitrogen gas enters in small quantity, oxygen in a larger proportion, and carbonic gas in a much larger still. Now the mechanical operation of pressure acts alike on all these gases, and, to a certain extent, causes their entrance into the water; and hence, if the pressure be artificially increased, the quantity of gas that enters is increased, or, if it be diminished, or entirely removed, the gases in part escape, or are no longer retained. But the reason why oxygen is received in larger quantity than nitrogen, and carbonic gas than oxygen, can only be explained on the supposition of a difference in the affinity subsisting between water and those gases respectively. In the same manner, the entrance of all elastic fluids into vegetables, may, to a certain extent, be deemed mechanical; but, after their admission, a chemical cause may be considered to operate. Hence it is, that, in the foregoing experiments of M. de Saussure, the oxygen gas which so largely enters the leaves of fleshy plants, cannot be abstracted by the air pump, nor, while they are kept in darkness, by placing them in temperatures equal to 40° Reaumur *; but it is entirely expelled (342.) in its primitive form and quantity, by the chemical agency of the solar rays.

349. From the facts which have now been stated

---

* Recherches Chim. p. 67. 69.

we collect, that plants, which vegetate in sunshine, require always the presence of oxygen gas (309.); and that, by the act of vegetation, they constantly change this oxygen (308.) into carbonic acid. We farther learn, that carbonic acid enters plants, both with the fluids which they absorb (318.), and also, under certain circumstances, in an elastic form (300.); that this acid gas is conveyed to the leaves, and is there decomposed (290. et seq.), by the joint operation of the plant and of solar light; and that it is from this source alone (317.), that the oxygen gas afforded by plants is derived. It likewise appears, that this operation of affording oxygen is not properly a vegetative function (338.), but only a subordinate office, accomplished by the direct agency of the sun; that it is carried on in the cellular or parenchymatous structure (323.), and not in the vascular system of the leaf; and that it may, and does exist (308.) with that function by which oxygen is consumed, and which is essential to the vegetation of the plant. Hence it is, that, when plants are made to grow in closed vessels exposed to the sun, the oxygen gas which is consumed by the function of vegetation, is again restored (308.) by the decomposition of the acid that is formed, and no change (306.), therefore, appears to be effected in the composition of the air. But in situations, where the direct agency of light is excluded (263.), no decomposition of carbonic acid is perceptible, and the air, therefore, soon becomes unfit to sustain vegetation. In its general nature and effects, therefore, the function of vegetation is precisely the same in sunshine and in the shade; for oxygen gas is alike necessary in both situations, and is in a similar manner converted into carbonic acid. Under direct exposure to the solar rays, however, this acid gas is again decomposed, and its oxygen is restored to the atmosphere; while, in the shade, no such operation takes place, and the air, therefore, remains permanently depraved.

350. But farther, it also appears, that the production of oxygen is entirely confined to the leaves (292.) and other green parts of plants; and that the flowers, the fruits, the stems and roots of vegetables (279.) both in sunshine and in the shade, convert always the oxygen gas of the air into carbonic acid. Since, likewise, the leaves acquire their green colour by the direct influence of the same agent as occasions the development of oxygen, may we not reasonably presume, that some necessary connection obtains between the production of that gas and the formation of this colour? Let us then pursue these inquiries a little farther, and try to discover the nature of that relation which appears to subsist between the production of oxygen gas, and the formation of the green colour in plants.

Sect. III. *Of the Relation subsisting between the production of Oxygen Gas, and the formation of the Colours of Plants.*

351. Since, from the conclusions deduced in the foregoing section, it appears, that the pure air fur-

nished by plants proceeds from the decomposition of carbonic acid, and since this decomposition is effected by the joint action of the green parts of vegetables and of solar light; why, it may be asked, are these parts so exclusively concerned in this operation, and what are those peculiarities of structure or of composition, which thus enable them to produce changes in the air, so different from those which all the other parts of the vegetable perform, even in sunshine, and so contrary to their own proper functions in the shade? This operation has been shewn to be in no respect necessary to the life of plants; for they live and grow in situations where they afford no oxygen gas. Neither can it be considered as a necessary result of vegetable organisation; for the structure of white leaves, which afford no oxygen, is as perfectly developed as that of green leaves, which yield it in abundance; and when plants are successively rendered green, or white, by the alternate admission or exclusion of the solar rays, in which states they respectively furnish a pure or impure air, it cannot be supposed that the vegetable organisation has undergone such material changes as should qualify it thus rapidly to present such contrasted results.

352. This decomposition of carbonic acid, which thus gives birth to oxygen, we have likewise seen to be effected in the parenchymatous structure of the leaf, and the agency of the solar rays appears to be essential to its completion. But it is in the same part of the plant that its colourable juices reside, and these juices, also, acquire their green colour from the di-

rect influence of the sun; so that the decomposition of carbonic acid, and the formation of the green colour, not only occur at the same time, and in the same place, but they are accomplished by the immediate operation of the same powerful agent. There exists, says M. Senebier, a very singular relation between the parts of leaves which furnish most air, and those of etiolated leaves which first become green. It is in the angles formed by the nerves of the leaves that we observe these two phenomena. It is in these that the excretory vessels of the parenchyme terminate, and it is there that light announces its operation, as we see by the air which this part affords, and by the green colour which at the same time it assumes *.

353. But not only do these operations appear thus to go on at the same time, and in the same part of the plant, but they seem to have a near connection with each other; for it is only by the green parts of vegetables that oxygen gas is afforded; while all their other parts yield only an impure air. M. Senebier made many experiments on the seminal leaves of French beans, and on the young leaves of other plants, but never obtained from them any oxygen gas while they retained their white colour. Neither did the etiolated leaves of lettuce or cabbage afford air when exposed to the sun, or the little which they sometimes yielded, was always very impure †. Why, then, should the decomposition of carbonic acid always attend the production of the green colour in plants, and why should their white colour appear

* Mem. Phys. Chim. t. ii. p. 98. † Ibid. t. i. p. 110. 115.

always to be accompanied by the retention of that gas? Could we discover the connection between these facts, it might, perhaps, lead us to an explanation of the cause of the green colour of plants.

354. As we have before endeavoured to shew (338.), that the decomposition of carbonic acid by plants cannot properly be deemed a vegetative function, so likewise the changes which plants exhibit, in passing from an etiolated to a coloured state, and the circumstances under which these changes take place, equally prove their independence of a living action. When an etiolated plant is gradually exposed to light, we first observe, says M. Senebier, the most tender parts pass from a white to a yellow colour; the yellow then becomes deeper; next, some green spots are seen at the extremity, and on the borders of the leaf, and in the angles of its nerves. These spots multiply, extend, and meet; the stalk of the leaf afterwards becomes green, and, lastly, the stem. The new leaves are green from the first, and thus upon the same stem we may see some leaves very green, others much less so, and a stem that is still white. In young leaves, the extremity is often greener than the other parts; and in the *narcissus* he often saw the point of the leaf more green than the rest, and the plant, which had just issued from the bulb, less green than that which had been some days exposed; whence it seems to result, says M. Senebier, that the green colour depends absolutely on a combination effected exteriorly, and which, to a certain extent, is independent of the internal vegetation of the bulb *.

* Mem. Phys. Chim. t. ii. p. 88. 90.

355. In prosecution of this idea, he procured a piece of tinfoil, an inch square, and with it covered a portion of the green leaf of a narcissus. In a few days, the leaf still appeared green over its whole extent, except in the part covered by the tinfoil, which part was very yellow. So, likewise, if an etiolated leaf of the same plant was covered with a piece of tinfoil, and then exposed to the sun, the whole leaf gradually became green, except in that part covered by the metal * ;—facts which not only demonstrate the direct influence of the solar rays, but likewise the locality of their action.

356. In like manner too, as we have seen green leaves to produce oxygen gas in sunshine, after their separation from the stem, so do etiolated leaves become green, when placed in the same circumstances. M. Senebier exposed, in his window, the etiolated leaves of the hyacinth, to the direct rays of the sun, and in a few hours they became green ; but the thin leaves of other plants did not exhibit this change of colour, evidently because they became dry before it could take place. When, also, etiolated plants are exposed in water to the sun, they become green, in the same manner as leaves afford oxygen gas, when placed in similar circumstances. He has seen the leaves of French beans, which sprang white out of the earth, become sensibly green in an hour, under exposure to an ardent sun †. Not only, therefore, do the mode and circumstances in which the green

---

* Mem. Phys. Chim. vol. ii. p. 90.

† Ibid. p. 78. 91. 93.

colour forms in leaves, but the rapidity with which it is effected by the direct action of the sun, shew it to be accomplished by a power which acts locally and exteriorly to the plant; and whose operations are carried on in a manner distinct from those which properly constitute vegetation. These facts, therefore, supply another coincidence between the decomposition of carbonic acid, and the formation of the green colour in plants, and still farther confirm the immediate relation that appears to subsist betwixt them.

357. To account for the green colours of vegetables, various hypotheses have been proposed. M. Humboldt, having observed some growing plants to retain their green colour in mines, from which light was excluded, and in which the atmosphere was impregnated with hydrogen gas, was led to ascribe the production of the green colour to the operation of hydrogen *. It is evident, however, that plants become green in the common atmosphere, where little or no hydrogen gas is present; and M. de Saussure, in various trials, could never observe that plants became more green from the languid vegetation which they experienced in vessels of hydrogen gas †.

358. M. Humboldt farther attributed the white condition of the leaf to the operation of oxygen; and we have seen, that the leaf becomes green only when oxygen gas is expelled from it. But the non-expulsion of oxygen is no proof that the white colour is caused by the operation of that substance;

* Thomson's Chemistry, vol. v. p. 362. 4th edit.

† Recherches, p. 210.

for, previous to its expulsion, this oxygen exists in union with carbon, and, therefore, as oxygen, it cannot be considered as producing the white colour in question.

359. From the circumstance of carbonic acid being decomposed, and its oxygen only expelled, when plants become green, M. Senebier, in conformity with the opinions of the day, was, at first, led to ascribe this colour to the operation of phlogiston *, and subsequently, to the retention of carbon, which he supposes to be largely deposited in the parenchyme of the leaf †. But, granting that carbon is thus retained in the leaf, we possess no evidence that it is able to produce the green colour. The juice which communicates this colour is contained in the cells of the parenchyme. It is of a resinous nature, and soluble in alcohol, to which it imparts its green hue: and this green solution possesses the same properties, and exhibits the same changes, from whatever leaves it is obtained. If a phial, about one-third filled with this solution, be exposed to the direct light of the sun, its green colour is discharged in about twenty minutes, and the liquor resumes nearly the transparency of alcohol, except that it is a little tinged by the vegetable matter, while a yellowish precipitate is at the same time thrown down ‡. In what manner, then, is this discharge of the green colour produced?

* Mem. Phys. Chim. passim.
† Phys. Veg. t. iii. p. 158.
‡ Mem. Phys. Chim. t. iii. p. 6.

360. The heat that exists in the solar beam, is not, says M. Senebier, the cause of this change of colour; for, when light is excluded, the green tincture preserves its colour for many months, although it be exposed to a considerable degree of heat. Even at the temperature of 60° Reaum. the colour suffers no change, if the solution be kept in darkness; so that light, not heat, is the agent concerned in producing this change. But though light thus accelerates this change, yet its direct influence is not essential to its production; for the green colour will be gradually, but much more slowly, discharged in the shade *.

361. Besides light, however, air also is necessary to the success of the experiment; for vessels, which are quite filled with green solution, and closely stopped, suffer no change of colour for many months, even in sunshine: and those portions of solution, which present the greatest surface to the air, or which are contained in phials with the largest quantity of air, experience the greatest change; so that the discolouration is always proportional to the quantity of air that is present †. It is, however, only the oxygenous part of the air that contributes to these changes; for when confined in phials which contain only nitrogen gas, the solution experiences no change, although it be exposed to the sun; but when oxygen gas is present, the discolouration is then more rapidly effected, than by common air alone ‡. It follows from these facts, that the presence of oxygen gas, and the agen-

* Mem. Phys. Chim. t. iii. p. 8. 9. 13.
† Ibid. p. 15. 16. ‡ Ibid. p. 18.

cy of light, are conjointly required to produce these changes of colour in the green solution: and, in conformity with his hypothesis, concerning the cause of the green colour of plants, M. Senebier supposes the oxygen to be necessary to carry off the carbon, on the presence of which substance, he believes this colour to depend *.

362. The observations of Senebier were repeated by M. Berthollet, who remarks that the green colour of the solution disappears rapidly in sunshine, more slowly in the shade, and not at all, or very slowly indeed, in perfect darkness. To discover the effects produced in the air, he inverted a vessel, half filled with green solution, over mercury, and exposed it to the light of the sun. When the colour was discharged, the mercury was found to have risen into the vessel: and, consequently, says M. Berthollet, the oxygen had combined with the colourable parts of the solution †. He did not observe the precipitation which M. Senebier mentions, but the solution continued transparent, and of a clear yellow colour. When

* Physiol. Veg. tom. iii. p. 175.

† Authors, in general, have denominated the matter which, in these experiments, affords colour, the colouring parts of the vegetable. This matter, however, is not itself coloured, but is only capable of exhibiting colours, by the addition of other matters: and hence we have ventured to call it the *colourable*, rather than the *colouring* parts of the plant, by which we merely indicate its property of becoming coloured, but not its actual possession of colour. When, by the addition of other matter, it is made to assume colour, and is, in that state, employed as a dye or a pigment, it may then be called colouring matter.

evaporated, however, the colour deepened, and passed to brown, affording at length a black carbonaceous residuum. If the vessel contained no oxygen, light did not act on the colour of the solution, and nitrogen gas experienced no diminution. Light, therefore, says M. Berthollet, favours the attraction of oxygen, and the *combustion* of the colourable matter, which at first is imperceptible, and gives to the solution only a yellow colour; but soon afterwards it is perfected by the action of heat, and the liquor becomes brown, and leaves a black residuum *.

363. With all due deference, however, to the opinions of this eminent and philosophical chemist, we must venture to protest against the employment of the term *combustion*, to express that combination which oxygen, in these instances, is supposed to form with the colourable matters of the solution; for neither in the circumstances of their union, in the phenomena which attend it, nor in the results which are afforded, does this combination at all resemble the ordinary process of combustion. If that process be rightly defined, the combination of oxygen with certain bodies, accompanied by the phenomena of light and heat, and terminating in the destruction of the properties of the combustible matter, then no process in which such phenomena are not observed, and such results afforded, can, with propriety, be named combustion. If the mere union of oxygen with combustible matter were sufficient to constitute combustion, then the living functions of vegetation

* Elemens de l'art de la Teinture, vol. i. p. 54. 55.

and respiration, and the several processes of fermentation and putrefaction, would all fall under the same denomination; but who, that duly considers the nature, the purpose, and end of these several operations, would desire thus to confound them, under the general title of combustion? It is evident that the black residuum which M. Berthollet seems to consider as evidence of actual combustion, was not the result of the natural combination of these bodies, but of the artificial heat to which he submitted them in the process of evaporation.

364. Dr Bancroft, in his valuable treatise on the "Philosophy of Permanent Colours," remarks, that it cannot be M. Berthollet's intention to apply the term *combustion*, to alterations which result from a simple addition of oxygen to colourable matters, without a destruction or separation of any of their component parts. Various acids, says he, which contain oxygen, weaken or extinguish colours, not, however, by any effect which can properly be termed combustion; for none of the colourable parts are destroyed or carried away, and the addition of an alkali restores the original colour. Many dyed substances, he adds, have their colours dissipated by exposure to the sun and air, without any other change of tint than the simple diminution of their original body, or quantity of colouring matter; so that the cloth is left as white as before it was dyed, without any thing like combustion having ever taken place in it, or in the matter with which it was dyed *. Instead,

* Philos. of Perm. Colours, vol. i. p. 49, 50.

therefore, of considering the green colour, in Senebier's experiments, to have been destroyed by combustion, he supposes it to have been effected by the operation of oxygen, and conceives the precipitate that was formed to have been the colourable matter saturated with oxygen *.

365. This opinion, that the discharge of colour depends on the simple addition or combination of oxygen, appears to us not less gratuitous than that of M. Senebier, which considers it to arise from the escape of carbon; for, though it be granted that oxygen disappears, yet we are not told into what combination it enters, nor in what way it acts on this colourable matter. Dr Bancroft himself, in his observations on Berthollet's doctrine, remarks, that the muriatic acid discharges colours in the same manner as other acids, although it has never been proved to contain oxygen; or, if it do, this oxygen is united by an affinity too powerful to be overcome by any known substance or means. When, therefore, says he, this acid changes or destroys colours, it must be by producing effects different from those of combustion; and as these changes are in most cases similar to those produced by the other acids, which contain oxygen, it seems reasonable to conclude, that these also act upon colours by producing other effects than those of combustion †. If, however, this reasoning be valid against the hypothesis of combustion, because the muriatic acid cannot afford oxygen to carry on that process, it must, for similar reasons, apply equally against Dr Bancroft's hypothesis, which ascribes the

---

* Philos. of Perm. Colours, vol. i. p. 43. † Ibid. p. 51.

discharge of colour to the simple combination of oxygen.

366. But, if it thus appear, that the muriatic acid discharges vegetable colours, like other acids, although its oxygen cannot be separated, may we not presume that these other acids act not in virtue of the oxygen which they contain, but by reason of their acid properties alone? And since we also know that oxygen gas readily combines with the carbon of vegetables, and forms an acid substance, may we not farther presume, that the oxygen, employed in these experiments, really entered into such a combination, and that the acid, thereby formed, contributed to the discharge of the vegetable colour?

367. That carbonic acid is really formed, when oxygen gas is placed in contact with this green solution, seems almost certain from the following fact. If, says M. Senebier, sulphuric acid be poured into a green solution, which has been previously discoloured by light, it occasions a strong effervescence, the mixture then reddens, and a white precipitate is thrown down, similar to that which the green tincture affords, when, in its coloured state, it is made to combine with the same acid*. There can, we think, remain little doubt, but that the elastic matter, which is thus driven off by sulphuric acid, is carbonic acid gas; and hence, when oxygen gas disappears in these experiments, and combines with the colourable matter, the discharge of colour that ensues is not properly to be ascribed, as M. Senebier supposes,

* Mem. Phys. Chim. vol. iii. p. 44.

to the simple removal of carbon, nor, as Dr Bancroft believes, to the simple addition of oxygen, but rather to the action of the acid compound, which these elementary bodies compose.

368. M. Bergman is said to have proved that alcohol attracts double its bulk of carbonic acid; and, therefore, this acid will necessarily be attracted by these solutions, as fast as it may be formed. To prove that, like other acids, it is able to discharge their colour, we inverted a bottle, filled with green solution, in the pneumatic trough, and passed up into it a stream of carbonic acid, as it issued from the mouth of a retort, that contained carbonate of lime and diluted sulphuric acid. A considerable portion of the solution was allowed to escape, and the colour of the remainder was rapidly discharged, so that it was reduced nearly to a colourless state: and when suffered to remain at rest, a flocculent precipitate subsided, and left the liquor perfectly transparent. A similar effect was produced, but much more slowly, by breathing through a tube into a bottle which contained a quantity of green solution; and as nitrogen gas has been shewn (361.) to effect no change in the solution, the discharge of its colour must also, in this instance, have been caused by the action of carbonic acid gas. Thus, then, we see, not only that carbonic acid is formed by the solution, when it is exposed to the light and air, but that it possesses, also, the power of discharging its colour, in circumstances where it must be considered to act by the exertion of its acid properties alone.

369. This conclusion is farther supported by the

fact, that the colour which is discharged, when the green solution is exposed to light and air, is again restored, according to M. Senebier, by the addition of an alkali *; and we found, by experiment, the same restoration of colour to be made when an alkali was added to the solution which had been deprived of colour by the direct application of carbonic acid. The green colour, however, did not resume its former intensity; and M. Berthollet remarks, that when the green solution has undergone all the discolouration from exposure which it is able to exhibit, alkali then produces in it no change †.

370. The phenomena afforded by these solutions of the colourable matter of vegetables in alcohol, agree with those which are presented by infusions of the same substances in water. The flowers of most plants, and also the leaves of many, yield to water materials which become red or green, according as acid or alkaline matter is made to predominate in them; but if the acid and alkali be furnished in certain proportions, the colour disappears altogether.

371. Not only, however, is the green colour of vegetable infusions destroyed by the direct application of acids, but, like that of the green solutions, it is gradually discharged by exposure to the air. M. Becker remarked, that if a vegetable infusion, that has been made green by alkali, be exposed to the air, it gradually passes to a yellow colour ‡. This,

* Mem. Phys. Chim. tom. ii. p. 152. and tom. iii. p. 47.

† Elem. de la Teinture, vol. i. p. 54.

‡ Berthollet's Elem. de la Teinture, vol. i. p. 64.

however, is not generally true; for we found the blue infusion of red cabbage, which had been first rendered green by an alkali, to acquire a red colour by exposure to the air. Indeed, it is reasonable to expect, that different plants should furnish colourable juices which are variously affected by chemical agents, since not only do vegetables vary in their properties from each other, but their colourable matter is often extracted from different parts of the plant. These colourable matters not only possess peculiar properties, but differ essentially from each other, says Dr Bancroft, and must therefore be applied in different ways, and with very different means, to produce permanent colours in other matters. Many species, however, of these colourable matters suffer, he adds, nearly similar changes from the action of acids, alkalies, and other chemical agents; from which it may be presumed, that there is something of a common or similar nature in many of them *.

372. That the discharge of colour in the foregoing examples, proceeded from the formation of an acid, seems to follow from the experiments of M. Berthollet. He placed portions of tincture of turnsole in contact with oxygen gas, over mercury, both in darkness and in the light of the sun; the first portion continued a long time without alteration, and without producing any diminution in the air, but the second lost its colour and became red; the oxygen gas had in great part disappeared and a little carbonic acid was formed, which, doubtless, says M. Ber-

* On Perm. Colours, p. 72. 3.

thollet produced the change of colour from blue to red *. Hence, therefore, we learn, that vegetable infusions, like the solutions before mentioned (367.), form carbonic acid, when they are placed in contact with oxygen gas; and that this acid, when thus formed, is able to discharge their colour.

373. Thus, then, it appears, that the phenomena exhibited by solutions of the colourable parts of vegetables in alcohol and in water very nearly agree; that both are rendered respectively red or green by the predominance of acid or alkaline matter; and that, according to the proportions in which these ingredients exist, various intermediate tints are produced. Even the pale liquor, obtained by digesting etiolated leaves in alcohol, was found by M. Senebier to become green by the addition of alkali †; so that these leaves contain resinous or colourable matter similar to that which green leaves afford, and the chief difference between the two solutions is in their proportion of alkaline matter.

374. But acids and alkalis not only change the colourable matter of vegetables, when it is extracted by alcohol or water, but they act also on the entire leaf. Sulphurous acid quickly discharges the colour of green leaves, and when these leaves are plunged in the vapour of nitric, muriatic, or sulphuric acids, they pass rapidly from a green to a yellow colour. Etiolated leaves suffer at first no change, but at length become whiter. Similar effects are produced in green

* Berthollet's Elem. de la Teinture, vol. i. p. 56.

† Mem. Phys. tom. ii. p. 152.

leaves, when immersed in water charged with these several acids; for they exhibit, at first, a fawn colour, and then become white; but etiolated leaves are not sensibly affected *. We observed, in some instances, that the green leaf not only became yellow or brown in acid liquors, but the liquor itself acquired a reddish hue. These effects of acids on the colours of leaves are also visible in common culinary processes; for waters, denominated hard, from containing an excess of acid, greatly discolour the vegetables which are boiled in them; and the green colours of pickled vegetables are very much degraded by the action of the acid liquor in which they are preserved.

375. As acids are thus found to discharge the green colour of plants, so alkalis may be expected to improve it; and experience confirms the expectation. If green leaves, says M. Senebier, be plunged in alkaline liquors, they preserve their colour, and the liquor acquires a greenish tint. Leaves rendered yellow by decomposition become brown in alkaline fluids; but etiolated leaves, and those which are reddened by age, pass to a green, though this did not happen to the etiolated leaves of the French bean †. We observed, that if water of potassa, or muriate of soda, was added to the water in which green leaves were infused, it improved their colour; but if the green colour was first degraded by the action of the water, these substances did not restore it.

376. If, then, it be established, by the foregoing

* Mem. Chim. tom. ii. p. 147. † Ibid. p. 149.

facts, that the matter which gives rise to the green colour of plants (359.) resides principally in the juices of their leaves; if these juices, when extracted by water or alcohol, exhibit, in various instances, yellow, brown, red and green colours (368. et seq.), according to the proportion of acid or alkaline matter which they may contain; and if, farther, the entire leaves themselves (374. 5.) exhibit similar changes of colour when exposed to the operation of the same agents; may we not reasonably presume, that these same agents, if present, will exert a similar action on the leaves during their growth? Let us then inquire whether the actual condition of green and of white leaves authorises us to conclude that they possess corresponding proportions of alkaline and acid matter.

377. With respect to green leaves, it is known, that these, and other green parts, afford alkali in much larger quantity than any other parts of the plant. From a report made to their government by the French chemists, it appears, that herbaceous plants afford five times more ashes than large trees, and that the leaves of trees yield more than their trunks *. Alkaline salts, says M. de Saussure, form, beyond comparison, the most abundant ingredient in the ashes of herbaceous plants. In many instances, the ashes of young leaves, growing on a bad soil, contained at least three-fourths of their weight of alkaline salts; the young leaves of trees also contained half, or sometimes three-fourths, of their weight of these salts, but

* Murray's System of Chemistry, vol. ii. p. 80. 1st edit.

the proportion diminished as the plant advanced in age*. These facts prove that the green parts of vegetables contain saline compounds in great abundance: and since it has been shewn, that the colourable juices of leaves (369.), and the leaves themselves (375.), have their green colour improved, or are even changed from white to green, by the addition of alkaline matter, the same chemical changes must occur in the living leaf, if means can be found to decompose its saline compounds, and thus, by releasing their acid part, to occasion an excess or predominance of alkaline matter.

378. Now the decomposition of carbonic acid in plants, by the agency of solar light, seems to be the mean employed by nature to accomplish this purpose; for, by this mean, the acid is not only withdrawn from its combination and expelled, but the alkali is, at the same instant, rendered predominant, and exists in a state fitted to exert its specific action on the colourable juices of the leaf; and this action, as we believe, it does exert, and the leaf, in consequence, exhibits a green colour. The colouration of the leaf, therefore, is not immediately owing to the expulsion of oxygen, nor even to the subtraction of carbonic acid, but to the predominance of alkaline matter which this subtraction of acid occasions; consequently, the verdure succeeds to the decomposition of carbonic acid, and the evidence of that decomposition is the expulsion of oxygen gas. Hence, therefore, to speak correctly, we cannot so properly

* Recherches, p. 285.

say, that the green leaf affords oxygen, as that it becomes green when that gas is expelled; and thus it is that the decomposition of carbonic acid by solar light gives rise at once to the production of oxygen gas, and to the formation of the green colour in plants.

379. The relation which we have thus traced between these operations, enables us to explain why the expulsion of oxygen appears to take place only from the green parts of plants, since it is only when oxygen is expelled that those parts acquire a green colour; why light is equally necessary to the expulsion of oxygen and to the production of the green colour, because alkaline matter cannot be rendered predominant, and produce this colour, unless carbonic acid, which affords the oxygen, be first decomposed; and, lastly, why no oxygen is produced, and no green colour is formed in darkness, because no carbonic acid is then decomposed, and its presence suspends the action of the alkaline matter. The mode, however, in which these operations are carried on in the leaves, the circumstances in which they take place, and the agents by which they are effected, all conspire to prove that the processes are purely chemical, and proceed in a manner entirely independent of those functions, which contribute to the evolution, the growth, and nutrition of the plant.

380. Such is the view, which, in the progress of our researches, we have been led to form of the causes which influence and produce the green colour of plants. To us they appear sufficient to account for the phenomena; and they are, indeed, so familiarly and precisely illustrated by the well-known

changes of colour which vegetable infusions exhibit from the action of alkaline matter, that we much wonder the opinion has so long slumbered in obscurity, although, from the erroneous views which have prevailed concerning the changes which vegetables produce in the air, its full development could hardly be expected. An opinion, indeed, approaching to that which has been now delivered, seems to have occurred, more than a century ago, to M. Geoffroy, who attributed the green colour of vegetables to the combination of a highly rarefied oil with the fixed and volatile salts of the sap; and this opinion he was led to form, from finding that a solution of the essential oil of thyme in alcohol became green by the addition of oil of tartar *.

381. But M. Geoffroy, says M. Senebier, in his remarks on these experiments, did not see that the alkali alone always produces these effects upon the etiolated parts of plants, upon their green solutions which have been discoloured by light, upon their essential oils, and even upon the pale and yellow tinctures of etiolated leaves †. It is true, that, by considering this rarefied oil as necessary to the production of colour, M. Geoffroy departed from, or rather fell short of the truth, which is the more to be wondered at, as he was well aware of the effects which the same alkalis produce in ordinary vegetable infusions. If, however, it be allowable to substitute the term colourable matter for essential oil, or to consi-

* Mem. de l'Acad. Roy. an. 1707.

† Mem. Phys. Chim. t. ii. p. 176.

der the elementary nature of these substances as nearly the same, then the opinion of M. Geoffroy will come near to a true expression of the fact. It is, we think, much more remarkable, that M. Senebier, who saw all that he has mentioned, and much more that we shall have to relate, was yet turned aside from the conclusion to which his experiments naturally led, by the influence of an hypothesis which had nothing better than a name for its support.

---

382. But if the colour of green leaves depend, as we have supposed, on the predominance of alkaline matter (378.), that of white leaves may reasonably be presumed to arise from the deficiency of it; and the experiments of M. Senebier prove that such is the fact. Not only did he find alkaline matter to be less abundant in etiolated than in green eaves, but to exist in a more neutralized state; and may not, says he, this neutralization be produced by the union of the carbonic acid, in such leaves, with the alkali which they contain *? This acid he actually found, by other experiments, to abound most in etiolated leaves †; and his results are confirmed by those obtained by Davy (25.) and others. Since, therefore, etiolated leaves not only contain less alkali than those which are green, but this alkali is neutralized, or even supersaturated, by a predominant acid, it is not to be expected that it should produce its usual

* Mem. Phys. Chim. t. ii. p. 166. † Ibid. 169.

effects ; and such leaves, therefore, like green infusions which have been exposed to the air or mixed with acids, will, from a similar cause, experience a similar degradation of colour, and exhibit only a white or yellow hue.

383. But, farther, if carbonic acid thus superabound in etiolated leaves, and destroy their colour, by modifying or overpowering the action of alkaline matter, this etiolation must continue as long as the acid is thus retained ; and this must happen as long as the plant is kept in darkness, since little air is afforded by etiolated leaves, and that little is always impure *. Hence, therefore, the etiolated state of plants depends immediately on a deficiency of alkaline, or on a superabundance of acid matter, by which the usual operation of the alkali is reduced or counteracted : but the sun's rays, by withdrawing and decomposing this excess of acid, enables the alkali to resume its former action, and thereby to restore the green colour of the leaf. This view, therefore, corresponds with the local operation of light (355.), in the restoration of the green colour ; with the fact of the formation of this colour in leaves (356.), in which vegetation is necessarily suspended ; with the results of those experiments (375.), in which white leaves were rendered green, by immersion in alkaline fluids ; and also with those, in which the white juices of such leaves (373.) were made green, by the addition of alkaline matter.

384. These views, respecting the colouration of

* Mem. Phys. Chim. tom. ii. p. 155.

leaves, seem also to explain, not only why they lose their verdure in darkness, but the cause of that degradation and change of colour which they experience in autumn, at the period, and during the time of their fall. There are many reasons which make it probable, that alkaline matter is constantly conveyed into the plant, during every period of its growth, and is thus constantly supplied to act on the colourable matter of the leaves. When, therefore, vegetation fails, this alkali will be less abundantly supplied, and the acid will proportionally prevail. But not only will the alkaline matter, at this period, be thus diminished, but the quantity of acid will be increased; for, as vegetation declines or ceases, spontaneous decomposition will begin, and the acid matter thereby developed will counteract or overpower the alkali; so that, according to the different proportions in which these ingredients meet, the colourable juices of the plant will be differently affected, and will thus exhibit those various brown and yellow tints, which compose an autumnal scene.

385. But the green colours of vegetables, not only pass to a yellow or brownish hue, but sometimes change to red. The leaves of the oak, of the pear, the vine, and barberry, often exhibit this colour: and, among herbaceous plants, the leaves of buck-wheat, strawberry, and amaranth do the same. It is only when they have attained to maturity, or are about to fall, or when they have been injured by insects, says M. Senebier, that they present a red colour. In those leaves which turn red before they fall, the redness first appears in the stalk; in others, the redness

begins at the summit, and, spreading over the surface, gradually reaches the stalk. The leaves of some plants pass from green to yellow, and afterwards become red; those of other plants become red only when exposed to the sun, and assume a yellow colour in the shade; while others pass from red to yellow, after they have fallen. It often happens, that many leaves which usually redden, become dry without exhibiting this colour; and apparently without regard to the immediate influence either of light or of heat. In all cases, red leaves lose their redness after a certain period, passing to yellow and white, and the direct action of the sun seems to accelerate these changes *.

386. In this transition from a green to a red colour, the nerves and stalks of the leaves never redden. As these parts were not previously green, it results, says M. Senebier, that it is the green matter alone which suffers this change of colour; and hence the stalks of the *melampyrum*, which were previously green, become red like the other parts of the leaf. In general, the superior surface of the leaf first changes colour, and often passes through various tints into yellow, before the inferior surface ceases to be green †.

387. The leaves, which have thus become red by age, yield to water a yellowish red or brick colour, and a red one to alcohol. The green leaves of the Canadian vine impart their green colour to alcohol, but the red leaves of the same plant tinge it red,

* Mem. Phys. Chim. t. iii. p. 69. 72. † Ibid. p. 73.

which passes afterwards to orange. If sulphuric acid be poured into the green solution, it reddens it; and the red tincture is rendered green by alkalis. The red leaves of this vine lose their colour in water charged with sulphuric acid, but the water acquires a lively red tinge; and the same leaves, immersed in an alkaline solution, have resumed almost their primitive green colour *. As thus the leaves of the same plant, in their green and red states, yield their respective colours to alcohol; as these differently coloured solutions pass to red or green, according as acid or alkaline matter is made to predominate in them; and as the entire leaves themselves exhibit similar variations in colour, when exposed to the operation of the same agents, we may fairly conclude, that the several tints which these leaves exhibit, in the successive periods of their growth, maturity, and decay, depend essentially on analogous changes, which take place in the condition of their colourable juices, according as these juices are modified by the operation of those causes, which vary the state and proportions of their acid and alkaline matter.

388. This redness of autumnal leaves, M. Geoffroy, indeed, ascribed to an acid developed at that period, which, by overpowering the alkali, gave rise to the red colour, in the same manner as distilled vinegar changes a green solution to a red colour †. And M. Senebier himself, who, as we have seen, attributes the

---

* Mem. Phys. Chim. t. iii. p. 77. et seq.

† Mem. de l'Acad. an. 1707.

green colour to a cause different from that of an alkali, nevertheless ascribes the red colour, in these instances, to the predominance of acid matter, which is developed under that incipient decomposition which the leaves, at the decline of vegetation, experience *. The production of acid matter by vegetable infusions, in their transition from a green to a red colour (372.), leads M. Berthollet also to believe, that an acid may be developed in those leaves which, in autumn, redden before they pass to yellow †.

389. But besides this red colour, which the leaves of different plants exhibit, in the more advanced periods of their vegetation, there are other leaves, says M. Senebier, which proceed red from their buds, and become green only in the latter periods of their growth; such are those of the apricot, the walnut, the maple, and the pear, the leaves of which last plant are of the same colour in their earliest and most advanced age ‡. In these instances, in which the young leaves are at first red, it is easy to suppose, that the colourable juices contain an excess of acid, which enables them to exhibit this colour; but by the action of the solar rays, this excess, at a later period, is reduced, and the alkali becoming finally predominant, gives rise to its accustomed hue. In the young leaves of most buds, the colour is only white, indicating a less proportion of acid; and these leaves,

* Mem. Phys. Chim. vol. iii. p. 83. 85.

† Elemens de la Teinture, vol. i. p. 57.

‡ Mem. Phys. Chim. t. iii. p. 75.

too, become green under a similar exposure to the solar rays.

390. But although the light of the sun thus contributes to produce the green colour of plants, yet, as it operates chiefly by decomposing acid matter, and thereby reducing its predominance in the colourable juices of the leaf, it follows, that its agency is not essential to this production of colour, if the alkali, from any other cause, can be rendered predominant in those juices. Thus, when the colourless solutions of etiolated leaves, or the colourless infusions of flowers, are rendered green by alkalis, the agency of light is no way concerned in producing the effect; and so, likewise, if the alkali, in living plants, can, in any way, be rendered predominant, under circumstances where light is excluded, still the colourable juices of the leaf may, nevertheless, be expected to exhibit more or less of a green colour. M. Senebier remarks, that the leaves of etiolated French beans often exhibit a green appearance at the part where they are connected with the stalk; and this appearance is seen at the commencement of vegetation, while the seed is yet buried in the earth, and before light, therefore, can have come into contact with it *. So, likewise, the buds of the chesnut, while enveloped in their thick and gummy cases, possess a green hue before their development; whence, says he, it is not to be doubted, that vegetables, and parts of vegetables, may be coloured green, although light does not seem to act immediately upon them †.

* Mem. Phys. tom. ii. p. 86. † Ibid. p. 97.

391. We have already seen, that, in a very weak and diffused light, some plants appear to decompose carbonic acid, and to retain their verdure; and many leaves also, which grow in the shade, are not less green than those which are fully exposed to the sun. All such facts may be easily explained, on the supposition that alkaline matter naturally superabounds in the plant, and produces its specific effects on their colourable juices; while, in other cases, the direct agency of light is more or less necessary to withdraw and decompose the acid, whose presence prevents the appearance of the green colour. It is, however, highly probable, as M. de Saussure observes, that carbonic acid is decomposed in plants, even in a very weak light (312.), although, from the slowness of the operation, and the immediate consumption of the oxygen by vegetation, the fact cannot be easily detected. Such an opinion permits us to suppose a redundance of alkaline matter, without resorting to the less obvious supposition of an unusual supply of it, to counteract the effects of the retained acid.

392. Besides those transitions from red to green, which the young leaves of certain plants have been shewn to exhibit, there are other leaves which present contrary characters, being at first nearly green, and becoming at length perfectly and permanently red. The red cabbage affords an example of this kind; and a comparison of its peculiarities with those of the green variety may assist us in explaining its cause. In the common cabbage, all the leaves which are exposed to light, acquire a green colour, while those in the centre or heart of the plant have but lit-

tle of a greenish hue, or are entirely white. These white leaves, however, have been shewn (373.) to contain the same colourable juices as those which are green, and, when extracted by alkohol, they pass to a green colour by the addition of alkaline matter. Hence the colourable matter is more or less distributed through the whole plant; and, in the manner already stated, it is rendered white or green according as the plant, or any part of it, is exposed to, or secluded from light. This colourable matter, as we before remarked, is chiefly contained in the parenchyme of the leaf, and the colour is transmitted through the epidermis which invests it.

393. In the red variety, however, the colour is not communicated by the juices of the parenchyme, but by the outer skin or epidermis itself; while the parenchymatous juice beneath is yellow or white. The redness, too, of this plant is seen both in those leaves which are secluded from light, and in those which are exposed to its action; consequently this agent exerts no immediate effect in their colouration. The leaves of this cabbage yield a bluish tint to water, which, like that of turnsole, is rendered red by exposure to the air, or by impregnating it with carbonic acid. In the same manner, it is rendered green by alkalis, so that the red cabbage must contain a colourable matter, similar to that which is diffused through the green and white leaves of the common variety. This, indeed, is easily seen by inspecting the plant itself; for though the skin of its leaves is red, yet the parenchyme of the outer leaves is, in many parts, sensibly green, and transmits even a

dusky green hue through the red covering which invests it. So, likewise, those portions of leaf, which have been infused in water or alkohol, and thereby nearly deprived of colour, become red or green, according as they are immersed in acid or alkaline liquors; and the same may be said of the substance of the parenchyme, from which the epidermis has been previously removed. Hence the same colourable matter resides in the leaves of the red cabbage as in those of the green variety, as is thus manifested by its undergoing the same effects, when exposed to the operation of the same agents. Consequently, the difference of colour in the two varieties is to be sought in that of the epidermis alone.

394. Now, the cause of the red colour in this epidermis appears to be that same predominance of acid, which occasions it in other instances; for the red tint, which these leaves afford to alkohol, is not affected by exposure to the air, but is exalted by acids, and rendered green by alkalis; and these transmutations of colour may be indefinitely repeated. If, farther, the epidermis be raised, and carefully removed by dissection, it will be changed instantly to a rich green colour by immersion in an alkaline liquor, and may be again restored to a red hue by plunging it in an acid; but if the immersion in alkali be prolonged, the colourable matter dissolves, and the green tint passes into yellow. If, also, pieces of the entire red leaf be immersed in acid liquors, the edges of the parenchyme soon change from green to red, and the colour of the epidermis is at the same time considerably heightened; while, in alkaline fluids, simi-

lar portions of leaf have the green hue of their parenchyme improved, and even the red epidermis finally assumes a green colour. This alternate change and exaltation of colour, according as acid or alkaline matter is made to predominate, authorise us to ascribe the production of colour, in the epidermis of these leaves, to the same excess of acid which imparts the red tinge to autumnal leaves (387.), and to the young leaves of certain other plants (389.), although the seat of colour be placed in a different part of the vegetable structure.

395. In the same manner, the colour of certain roots, as of the radish, of some varieties of potatoe, and of the turnip, seems to reside in the outer coat, while, in the carrot and beet, it is diffused through the substance of the root. In the radish, the colour extends over almost the whole root; in the turnip, it is confined to that part which is exposed to light; and even the radish, where it comes into contact with light (286.), assumes a purple hue. Now, if light operate in the way that has been stated, it will variously affect the colours of these parts, according to the degree of its action. It is, as we have seen, in the green parts alone that oxygen is largely expelled; in other words, that carbonic acid is largely decomposed; and, therefore, it is in those parts only that the alkali is enabled to exert its full effect. But if the light act in a less degree, and decompose less acid, the alkali will be less predominant, and impart not a full green, but some modification of purple.

396. In illustration of these views, we may re-

mark, that the skin of the turnip, beneath the soil, where light cannot act, is entirely white, because the alkaline matter is fully saturated with acid; in the part above ground, the colour is purple from a partial decomposition of acid; and in some parts, it becomes nearly green as more acid is decomposed, and the alkali exerts a fuller degree of action. We have, also, often observed different parts of the same leaf of this plant to exhibit green, yellow, red, and purple colours, from accidental circumstances, which determined, as we suppose, in different degrees, the predominance of acid or alkaline matter. Thus, too, in the root of the radish, where it is entirely secluded from light, the acid may be conceived to prevail; but exposure to light effects a partial decomposition, which reduces the excess of acid matter, and permits the purple tint to appear. This series of changes is well shewn by an infusion of red cabbage, which has, at first, a bluish tint, but passes through various grades of purple to a confirmed green, by successive additions of alkaline matter: or, if an acid be employed, the same infusion is gradually heightened to redness, and may be again brought back to purple by adding to it alkaline matter. And since the action of light, in the colouration of plants, has been shewn to be entirely local (355.), it follows, not only that a succession of colours may thus be produced in the roots or leaves, but that various colours may, at the same time, be present in different parts of the same leaf or root, according as it is more or less subjected to the influence of light, and, probably, according as the structure of the plant, and the qualities of its colourable matter, more or less favour or modify the operation of this agent.

397. The foregoing observations are farther applicable to those chequered or party-coloured leaves, many of which draw so much regard from their elegance and beauty. In many instances, these appearances proceed from age, from injury, or disease: but the regularity and uniformity of the colours, in other cases, indicate a natural state of the plant. In the *perfoliata pictis foliis*, says M. Senebier, the green leaves are spotted with yellow. The green leaves of the pimpernel of the mountain have yellow stripes. In a species of aloë, the middle part of the leaf is green, and the borders are yellow, while others are green and yellow, or yellow and green *. Now, in green plants, the yellow colour is formed by reducing the green, as in the leaves which fall in autumn, and this is effected by the development of acid matter. If, therefore, acid naturally prevail in one part of a leaf more than in another, a proportional degradation of colour will be produced, and this, too, in particular parts or places, according, probably, to variations in the structure of the leaf, or some other accidental circumstance; so as to give rise to that mixture of green and yellow which has been above stated. Should the acid abound still more, it may, as in certain red leaves of autumn, give rise to a red colour. Thus in the tri-coloured amaranth, the leaves are, at the same time, green, yellow, and red; but this is most frequent in autumn, as the power of vegetation declines †.

---

* Mem. Phys, tom. iii. p. 89. † Ibid. p. 93.

398. The red matter of these tri-coloured leaves is soluble in water, and is dissipated by light, leaving a yellow resinous matter, in which acid abounds *. It is also rendered green by alkalis †,—an observation which suggests the probability that its redness depends, as in other cases, on an excess of acid. Hence, then, the green colour of certain parts of these leaves would seem to depend on an excess of alkali, the yellow on a smaller portion of that substance, and the red on the predominance of acid matter:—all which colours are, in succession, exhibited by autumnal leaves, and by certain vegetable infusions, when submitted to the action of acid and alkaline matter. Of the truth of these opinions, an observation of M. Senebier affords additional evidence; for he remarked, that the tri-coloured leaves of the amaranth, exposed under water to the sun, afford air from their green parts alone, while the red and yellow portions yield no air ‡; so that, in the same leaf, we see the green colour accompany the decomposition of carbonic acid, and the yellow and red colours continue, when this acid is retained.

399. The general causes which thus give rise to the colour of leaves, seem, likewise, to act in the colouration of flowers. We have already noticed (25.) the agency of light in the production of these colours, by observing that roses, if kept in perfect darkness, are altogether deprived of colour. Blue hyacinths, says M. Senebier, become grey in dark-

* Mem. t. iii. p. 94. 95. † Ibid. p. 94. ‡ Ibid. p. 99.

ness; poppies are white before their development; and carnations and roses do not acquire their rich hues but a short time before they are disclosed. Light, therefore, seems to act through the calyx of the flower in these instances; but the greater number of petals are green in the bud, before they are exposed to light. Others are white in the bud, and redden when they are disclosed *.

400. M. Becker made a great number of experiments on infusions of the flowers of different plants, by adding to them different acids, and various earthy, metallic, and alkaline salts. In general, the acids produced in these infusions a red colour, and the alkalis rendered them green, but sometimes yellow. In several instances, also, the nitric acid produced a yellow colour. All the acids reddened infusion of roses, but the alkalis and lime water rendered it yellow, without passing through green. The expressed juices of various flowers were likewise changed, from blue or violet to red, by acids; and brought back to their original colours by alkalis, which, if added in larger quantity, rendered them green or yellow. It results from these facts, that acids and alkalis rapidly change the colours of flowers; that acids, except the nitric, render them red, (and it also reddens the infusion of roses), while a green colour is constantly produced by the alkalis. In the change from blue to red by acids, the colour passes through different grades of purple; and the green colour, produced by the alkalis, passes ultimately to yellow,

* Mem. Phys. Chim. tom. iii.

which neither acids nor alkalis, nor even light itself, can change *.

401. Many experiments of a similar kind were made by Dr Lewis. The colour of many blue flowers, he observes, is extracted by water, but not by alcohol; and the expressed juice, also, of such flowers, is for the most part blue. Both the blue juice and infusion are reddened by acids, and rendered green by alkalis and lime water. The red colour, produced by acids, at length fades by exposure; and the green, produced by alkalis, changes to yellow. Red flowers impart their colour both to water and alcohol. The rose-coloured infusions were acted upon by acids nearly in the same manner as the blues; but the full deep reds were not. The deep infusion of red poppies was changed not to green, but to purple, by alkalis. Yellow flowers give out their colour to water and alcohol; but neither acids nor alkalis alter the species of the colour, though both of them vary its shade; acids rendering it paler and alkalis deeper. The fine yellow dust of the anthers of flowers gave a fine bright yellow to alcohol, and a duller yellow to water, which colours were heightened by alkalis, and turned red by acids. Even white flowers contain colourable matter, which is rendered green by alkalis, but not reddened by acids. The sulphurous acid vapour, which destroys the colour of other flowers, does not affect that of white ones †.

* Mem. Phys. tom. iii. p. 107.

† Neumann's Chem. by Lewis, p. 430. et seq.

402. Mr Delaval, also, digested red, purple, and blue flowers in acidulated water, and thus obtained red liquors, which, by very small portions of alkali, were changed to purple, blue and green. The addition of the alkali must, he says, be very gradual; for if too much be added at once, the intermediate colours between the red and the green will not be produced. He likewise digested red, purple, and blue flowers in alcohol, to which they yielded their colourable matter, and became white. From most of them, however, the alcohol acquired either no colour, or only a faint tinge; but when it was acidulated, it became red, and, by the addition of an alkali, assumed purple, blue and green colours *.

403. The foregoing facts clearly shew, that all flowers contain a colourable matter, which, whether it be extracted by water or by alcohol, experiences changes from acid and alkaline matter similar to those which the juices of the leaves exhibit, but modified, in some instances, by the peculiar qualities of the flowers employed. Hence we may venture to ascribe their colours to the diversified operation of the same general agents, which produce the colouration of leaves; and the infinite variety of tints which they afford, must, therefore, be referred to the various combinations of acid and alkali with their colourable matter, modified by the peculiar qualities of that matter, and probably by the structure of the

* Manch. Mem. vol. ii. p. 165.

part itself, as it may affect the reflexion or transmission of light.

404. That the red colours, at least of the damask rose, arise from the action of an acid substance, may be collected from some experiments of M. Senebier. He remarks, that the petals of this flower are rendered white by digestion in alcohol; but their former colour is restored if they be exposed to light and air. If, however, they be confined only in nitrogen gas, they do not recover their fine colour, but have only a yellow hue. The sun's light gradually accelerates the restoration of their colour, when they are confined in common air, but it is not indispensably necessary; for, where the air has free access, these petals resume their colour in obscurity, but more slowly than when exposed to light. When the colour is thus recovered, it is again given out to alcohol, and these operations may be repeated several times, until the colourable matter is exhausted. Various other flowers afford the same phenomena, but not in so marked a degree *. From these facts, we collect, that, though the sun accelerates the restoration of the colour, yet that air also is necessary; and since it is the oxygenous part of the air which alone seems to produce the effect, we may, from the analogy of similar changes on the colourable matter (372.) of plants, conclude, that carbonic acid is produced, and by its action restores the colour. This conclusion is supported by an observation of M. de la Folie, who remarks, that roses, which have been whitened by the vapour of burning

* Mem. Phys. Chim. tom. iii. p. 120. et seq.

sulphur, are reddened by acids, and rendered green by alkalis *.

405. M. Geoffroy, in his experiments on the oil of thyme, mixed with it different portions of acid and alkali, and obtained various tints of yellow, red, purple, violet, blue, green, and black. When the oil was dissolved in a considerable quantity of alcohol, it afforded a greyish colour, which became blue, by the addition of oil of tartar, and was rendered red by distilled vinegar, but recovered its blue tint, by a farther addition of alkali. In other instances, by the use of alkali, the colour passed to green; so that the oil of tartar, says M. Geoffroy, acts differently on the essential oil of thyme; for according as it is more or less concentrated, it renders it blue or green. As different colours are thus formed by the simple mixture of oils and salts, M. Geoffroy was led to suppose, that similar colours might be formed in plants by the same combinations. The principal colours, says he, which we observe in plants and in flowers, are green, lemon-yellow, orange-yellow, red, purple, violet, blue, black, and white; and of these colours, differently combined, all the others are composed. The blue, purple, violet, and green, he attributed to the operation of alkali; and the red and yellow, to the predominance of acid; the black he considered to be formed by the action of acid on a purple; and the white to be produced by a very copious reflection of light, from transparent and colourless particles †.

* Mem. Phys. Chim. tom. iii. p. 140.

† Mem. de l'Acad. Roy. an. 1707.

406. This enumeration of the principal, or fundamental colours of vegetables, will recall to the mind of many readers, the system of colours, which the celebrated Werner has contrived, and applied to the description of the bodies which compose the mineral kingdom. The colours of minerals yield, perhaps, neither in number, in diversity, nor in splendour, to those which vegetables present; yet they are all reduced to eight, which are regarded as standard or simple colours. These colours are white, grey, black, blue, green, yellow, red, and brown. Although several of these are physically compound, yet, for the purposes of description, it is convenient to regard each as simple, and as constituting, in its pure and unmixed state, what may be denominated the *characteristic colour*. To these eight fundamental colours, Werner refers all the variety of compound colours which minerals present, employing the predominating colour to express the chief character, and qualifying it by the others, according to the quantity in which they appear to enter into the compound *. If, from the enumeration of Geoffroy, we reject one of the yellows as superfluous, his number of fundamental colours will agree with that of Werner; and even their characters will differ only in two instances, M. Geoffroy considering violet as a characteristic, or simple colour, which Werner describes as a compound of blue, red, and brown; and the latter

* Jameson's Treatise on the Ext. Characters of Minerals, p. 2.

has no colour answering exactly to the purple of Geoffroy.

407. It is, however, our object rather to investigate the general causes which give rise to colour in vegetables, than to detail and describe their particular varieties and hues. But if the colourable matter of vegetables were extracted in various ways, and submitted to the action of acid and alkaline agents, of known strength, and in various proportions, not only might a vast variety of tints be produced, but it would not be difficult to determine, by calculation, the relative proportions in which those agents, in different instances, contributed to their formation. And were the fundamental colours properly defined, and their grades and varieties arranged, and classified according to the method of Werner, we might perhaps obtain not only a regular series or suite of colours, answering, in some degree, to the diversity found in nature, but might, also, arrive at comparative estimates of the proportions of acid and alkaline matter, by which they were respectively produced.

408. Lastly, the colours of fruit experience changes similar, in many respects, to those of leaves and flowers; and, apparently, from the varied operation of the same general causes. In their earliest state, many fruits are green; but in the more advanced periods of growth, they assume different tints of colour. To the production of these colours, however, the light of the sun seems to be necessary; for, according to M. Senebier, neither peaches, pears, nor cherries, acquire their proper lively colours, if, at the period of ripening, they are secluded from the action

of light. Those leaves also, which intercept the sun's light, delineate on the fruit beneath the bounds which they prescribe to its action; and if a portion of fruit be covered with a piece of tinfoil, the uncovered portion will become perfectly red, while the covered part will exhibit only a pale or straw colour. So, likewise, if grapes, which would have become violet by exposure, be inclosed in black paper or glass, which excludes the light, they assume only a grey colour. Those green fruits which do not redden, lose also their green colour in ripening, and become yellow. This change is quickened by the action of light, but it also takes place without it; for many fruits pass from green to yellow, although they are secluded from light *.

409. The red juices of many fruits are extracted by water and alcohol; and both the solutions, and the juices themselves, says Dr Lewis, are sometimes made more florid by acids, and generally turned purple by alkalis †. The skins of fruits, likewise, yield their colourable matter to water and alcohol. The red skin of the peach imparts a red tinge to water and alcohol, but the white skin of the same fruit renders alcohol green. When the red skin has been thus deprived of its colour, it again assumes it, like the petals of roses, by exposure to light and air. The skins of different green, red, and yellow fruits imparted their respective tints to alcohol, and some, in a small degree, to water ‡; and the red tinctures of these

* Mem. Phys. Chim. t. iii. p. 146. 7.

† Newmann's Chemistry, p. 432.

‡ Mem. Phys. Chim. tom. iii. p. 148. 9.

fruits were exalted by acids, and changed to purple or green by the addition of alkalis *. The foregoing facts sufficiently prove, that the colourable matter of fruits, like that of flowers, and of leaves, is extracted by the same means, and acted upon, in a similar manner, by the same chemical agents; wherefore we are entitled to conclude, that its nature is essentially the same, and that it exhibits different appearances and colours, according to the modified action of those causes which produce the colouration of all the other parts of the plant.

410. The process of maturation, during which the changes of colour in fruits principally take place, is known to be accompanied by spontaneous changes, under which acid matter is developed, and produces effects on the colour of fruits not unlike those which decomposition occasions in the leaves, at the period of their fall. Whatever be the nature of the acid developed, its power in producing changes in the colourable matter will, probably, be nearly the same; and the existence of such acid is often sensible to the taste, though disguised frequently by the presence of saccharine matter. It is a farther confirmation of these views, that green fruits, like other green parts of plants, were found by M. de Saussure (274.) to afford oxygen gas in sunshine; which fact evinces, that they not only previously contained carbonic acid, but that, as in other cases, they became green, when this acid was decomposed and expelled.

* Mem. Phys. Chim. tom. iii. p. 153.

Sect. IV.—*Of the Physical and Chemical Agency of Light, in promoting the Colouration of Plants.*

411. So far we have spoken of the colours of plants, as arising from variations in their chemical constitution alone ; and these variations we have attributed to the decomposition of their saline compounds, by the agency of solar light. We have next to inquire into the manner in which light exerts this action, a subject of very nice and difficult investigation, but the importance of which will, we hope, furnish an apology for the apparent temerity of our attempt, even although we should fail in its execution.

412. Besides this chemical action, which light has been shewn to exert, we have also to consider its property of imparting colour ; for when it has been said that the predominance of acid or alkaline matter renders the vegetable juices red or green, it must be understood to mean only, that such a state or constitution of those juices is thereby induced, as, in the language of Sir Isaac Newton, enables them, more or less, to reflect or transmit the red, or green-making rays of light. We do not, however, propose to go far into the consideration of these subjects, but to select and exhibit such facts only, as force themselves on our notice, by the near connection which they seem to have with the more immediate objects of our research.

413. Since the great æra in science, created by the genius of Newton, who first decomposed the solar beam, and thus

"Untwisted all the shining robe of day,"

it has been considered, that "colours are not qualifications of light, derived from refractions or reflexions of natural bodies (as was generally believed), but original and connate properties, which, in divers rays, exhibit divers colours*." By means of the prism, Sir Isaac Newton was enabled to separate the solar beam into seven differently coloured rays, which run gradually into each other according to their particular degrees of refrangibility. Of these rays, the violet is the most refrangible; next the indigo; then follow the blue, the green, the yellow, the orange, and the red, which is the least refrangible of all. In the property of reflexibility, these rays, also, follow the order of their refrangibility.

414. These primary rays of light are simple and homogeneal, and cannot be changed in colour either by reflexion or refraction. Different mixtures of them may, however, be made to compound colours, like to those of homogeneal light; and various other colours, unlike to any of those of the primary rays, may be also formed by composition. But the red, yellow and blue rays are incapable of being produced, like all the rest, by the combination of other colours; they are therefore always simple and uncompounded. When all the primary rays are mixed together in due proportion, they then constitute white light.

* Opera Om. tom, iii. p. 301.

415. Having thus examined the several properties of the primary rays, this great philosopher proceeded to apply his discoveries to the explanation of the permanent colours of natural bodies. These colours, says he, arise from hence; that some bodies reflect some sorts of rays; others, other sorts more copiously than the rest. Every body reflects the rays of its own colour most copiously, and from the excess or predominance of these rays in the reflected light derives its colour. And while bodies become coloured by thus reflecting this or that sort of rays, it is to be considered, he adds, that they stop and stifle in themselves those rays which they do not reflect *. Hence it appears that Newton considered all coloured matter to reflect light; and this reflexion he supposed to be made by some power of the body which is evenly diffused all over its surface, and by which it acts on the body without immediate contact †.

416. According to Dr Wells, however, both Kepler and Zucchius had previously shewn, by experiment, that light is reflected without colour from the surfaces of bodies; and that the colours of bodies depend, therefore, not on the light reflected by their anterior surfaces, but upon that portion which has entered their internal parts, and is from thence sent back through those surfaces ‡.

417. Beside the colours which are thus exhibited by reflexion, bodies appear coloured by the light that is transmitted through them; and the bodies which thus appear of any colour by transmitted light, may,

* Optics B. i. prop. 10. prob. 5. † Ibid. B. ii. prop. 8.

‡ Phil. Transact, an. 1797, p. 418.

also, says Newton, look of the same colour by reflected light *. But the numerous experiments of Mr Delaval prove, that, in transparently coloured liquors, the colouring matter does not reflect any light; and that, if the light, which such liquors transmitted, be stopped, they do not vary from their former colour to any other colour, but become entirely black †. This conclusion he extends to transparently coloured solids as well as fluids ‡. The light, also, which opaquely coloured bodies, as plants, return to the eye, continues Mr Delaval, is reflected by their white opaque substance; and the colours of vegetables, therefore, are produced by the light reflected from this white matter, and transmitted from thence through the coloured coat, or covering, which is formed on its surface by the colouring particles §.

418. Admitting, then, that the permanent colours of natural bodies depend on the varied reflexion or transmission of the differently coloured rays, we have yet to learn why these bodies reflect or transmit some sorts of rays in preference to others, so as to present to our view that great diversity of colours which they exhibit. Sir Isaac Newton resolves this property of bodies into the varying thickness or density of their particles, and lays it down as a general law, that the forces of bodies to refract and reflect light are very nearly proportional to their densities; so that nothing more, says he, is requisite for producing all the co-

---

* Optics B. i. part 2. prop. 10.

† Manch. Mem. vol. ii. p. 146.

‡ Ibid. p. 159.   § Ibid. p. 192.

lours of natural bodies than the several sizes or densities of their transparent particles *.

419. These conclusions he drew from observing the colours afforded by plates of air and water, submitted to compression between two object glasses, whereby various colours were made to emerge, in successive orders, according to the degree of pressure employed. Extending the same principles to explain the permanent colours of natural bodies, he ascribed the yellow, orange, and red colours which certain liquors exhibit at different depths, when viewed in a conical glass by transmitted light, to the varying thickness or density of such fluids, which successively intercept and extinguish the more refrangible rays, until, near the top, they transmit the least refrangible, or red rays alone †. In like manner, he supposed vegetable infusions to be turned red by acids, because it is the nature of acids to dissolve or attenuate; and that the same infusions were rendered green by alkalis, because these substances precipitate or incrassate ‡. Mr Delaval follows Newton in attributing these phenomena to the same causes; and more lately, M. Haüy observes, that, in all these changes of colour the union of the moleculæ of the two liquids forms mixed moleculæ, the thickness of which is different from that of the component moleculæ, and determines the reflexion of colour answering to that thickness §.

---

* Optics, B. ii. part 3. prop. 10.

† Ibid. B. i. part 2. prop. 10.

‡ Ibid. B. ii. part 3. prop. 7.

§ Elem. Nat. Phil. vol. ii. p. 243.

420. This hypothesis, founded solely on the mechanical constitution of bodies, has, however, been warmly impugned by Dr Bancroft, who contends that the phenomena, exhibited by the coloured rings in Newton's experiments, do not warrant the conclusions drawn from them; for that the same colours occurred, and were repeated over and over again, at very great diversities of thickness; so that thickness could not be the only cause of these repeated variations of colour. And, indeed, they are to be explained, he adds, in the same manner as the colours of the prism and the rainbow, and ought not to have been employed to explain the permanent colours in different substances *.

421. On Mr Delaval's attempts, to shew that the different colours of animal and vegetable substances depend on an increase or diminution in the size of their particles, Dr Bancroft remarks, that, instead of employing mechanical means, which alone ought to have been used in his experiments, he has recourse to chemical agents, which change the composition of bodies, and produce effects different from those which arise from mere variation of density †. And in some examples given by Dr Bancroft, so far are the refractive and reflective powers of bodies from being in proportion to their density, that they are observed to be rather in opposition to it ‡.

* On Permanent Colours, p. 7.

† Ibid. p. 19. ‡ Ibid. p. 23.

422. But though the Newtonian hypothesis appears thus to fail in its general application to the permanent colours of natural bodies, yet, in the examples of liquors which possess an uniform composition, and which, at different thicknesses, exhibit different colours, when viewed by transmitted light, it may, perhaps, to a certain extent, be deemed satisfactory; for as all bodies intercept a portion of the light that falls on them, it is reasonable to suppose, that the increasing mass or density of the fluid should furnish an increasing obstacle to the passage of light, until, at the thickest part of the liquor, the least refrangible, or red-making rays alone are transmitted; and such seems to have been truly the cause of the redness which the upper portion of the liquor presented in Newton's experiment (419.), and which Dr Halley's hand exhibited at great depths in the sea *. The red colour of the sun too, in certain states of the atmosphere, seems to arise from a similar extinction of the more refrangible rays; and so, likewise, the light from our lamps, in very hazy weather, is not only diminished in intensity, but varied in species, so as to approach nearly to perfect redness.

423. But when vegetable infusions are changed in colour by acids and alkalis, a great discordance seems to obtain between the imputed properties of those substances, and the established qualities of the several rays of light. For if acids do dissolve and attenuate (419.), they must, on the mechanical hypothesis, be considered to facilitate the passage of light; and yet, after the addition of an acid, the more re-

* Optics B. i. part 2. prop. 10.

frangible rays are at once stopped, and the red-making rays, which are the least refrangible, are those only which pass; while, on the contrary, alkalis, which are supposed to precipitate and incrassate, and consequently to increase the resistance to the transmission of light, stop suddenly the red rays, and permit the green, and even the violet, the most refrangible, and therefore the most easily intercepted of all the rays, to pass;—effects, in each instance, the reverse of those which these agents ought to produce. The small portion, also, of acid or alkaline matter, that is required to change the colour of an infusion, can hardly be supposed to vary the density of the mixture so much as, by that mean alone, to produce such great alterations in its appearance: and this is still more difficult to conceive, when the addition is made, as it often is, by a body in a gaseous form. Lastly, the constitution of the fluid has itself undergone a change, not merely from a change of density, but of composition also, which obliges us to take into account the chemical, as well as the mechanical effect.

424. An observation, also, of Mr Delaval, points out a great dissimilarity in the action of different liquors upon the rays of light. In Newton's experiment (419.), says he, it is probable that the liquor employed was an aqueous or spirituous infusion of the woods used in dyeing red, which transmits yellow, orange, or red colours, according to its thickness; but the red solutions of flowers, and many others, do not transmit yellow or orange colours, even when spread thin; for when thus disposed in thicker or

thinner masses, they do not vary the *species* of their colour, but only transmit a diluter, or more intense red *. We may therefore venture to conclude, that the opinion of Newton, respecting the colours produced in vegetable infusions by acids and alkalis, was derived, merely by analogy, from what he had observed to happen in liquors apparently of the same nature, but which act in a very different manner on light; and this difference, we may add, furnishes a new proof that chemical composition, not less than mechanical constitution, influences the permanent colours of natural bodies.

425. Sir Isaac Newton himself, indeed, may be considered as admitting the influence of chemical composition in varying the action of bodies upon light; for, in stating his general law, he makes a well-known exception with respect to inflammable bodies, which, according to him, refract light more powerfully than other bodies of the same density. So much stress, indeed, did he lay on the great refractive power of these bodies, that he even thought it "rational to attribute the refractive power of all bodies, chiefly, if not wholly, to the sulphurous (*i. e.* the inflammable) parts with which they abound; for, adds he, it is probable that all bodies abound more or less with sulphurs †." "And this great man," says Dr Bancroft, "having also concluded that the permanent colours of natural bodies were analogous to those produced by the refractions of thin, colourless,

* Manch. Mem. vol. ii. p. 235.

† Optics, B. ii. part 3. prop. 10.

transparent plates, the chemists were generally induced to make all colour depend on the *principle* of *inflammability* or *phlogiston*, which, until very lately, was supposed to exist in metals and other substances, where there certainly is no evidence or appearance of it."

426. "But since the existence of phlogiston in bodies has been denied by the pneumatic chemists," continues Dr Bancroft, "they have, in most cases, attributed the origin and changes of colours to the application or combination of different gases, and particularly of *oxygen*, in different proportions; and it has been supposed that these gases possessed considerable refractive powers, and were thereby enabled to produce effects on colours, like those which the followers of Stahl had imputed to phlogiston *." "Hence," he adds, "M. Berthollet, in his work on dyeing †, intimates that many important observations remain for those who would follow the steps of the great Newton, and compare the *refractive* powers of the different gases, and of other substances, the constituent principles of which are now known."

427. This opinion concerning the influence of oxygen, in producing the colouration of bodies, seems to have arisen from the well-known changes of colour which certain metals exhibit during their oxidation. If, however, oxygen were the cause of these changes of colour, some uniformity of effect might be expected to attend its different degrees of combina-

* Philos. of Perm. Colours, p. 27.

† Elem. de la Teinture, tom. i. p. 5.

tion; but it is well known that metallic oxides exhibit every variety of colour, with little or no regard to the quantity of oxygen they contain. Thus, many oxides, which contain but little oxygen, are white; others, which contain a great deal, are black. Mercury, with different portions of oxygen, forms a black and red oxide, but these colours are not peculiar to such combinations; for sulphur, in union with mercury, produces compounds, which exhibit similar colours. Iron, when combined with 27 *per cent.* of oxygen, is black; with 48 *per cent.* it is red; and these same combinations exhibit sometimes green and brown colours. So many, indeed, and so diversified are the colours which the oxides of iron are capable of assuming, that all the fine varieties of colours, employed in Wedgwood's pottery, are said to be produced by the oxides of this single metal *. Hence, then, it appears, that, although oxygen combines with metals in various proportions, and the compounds exhibit various colours, yet no uniform colour, even in the same metal, accompanies the varying proportion of oxygen. On the contrary, similar colours are produced by very different combinations of this element; and different substances, by uniting with metals, exhibit similar colours where no oxygen is concerned. Consequently, no particular colouring property belongs exclusively to oxygen, and, therefore, the colours of metallic oxides must be attributed to the properties of the compound, and not exclusively to those of the oxygen they may contain.

* Bancroft on Perm. Colours, p. 17.

428. Neither does the supposed refractive power of oxygen give any support to this doctrine; for, from the experiments of M. M. Biot and Arrago, it appears, that hydrogen, in this respect, exerts the greatest force; and oxygen, on the contrary, is one of the bodies that has the least refractive power. Hence it is from the hydrogen they contain, that water, oil, and other substances, refract light in a ratio so much surpassing their density *. These results, therefore, accord perfectly with the facts observed, and the conjectures submitted by Newton, respecting the great refractive power of inflammable bodies; but they yield no support to the opinion entertained of the predominance of this property in oxygen.

429. With respect, also, to vegetable bodies, it is not easy to conceive how the presence of oxygen should produce their various colours. Except the pure bases of the alkalis, the earths, and the metals, almost every substance in nature appears, from Mr Davy's researches, to contain a small portion of oxygen; so that, in every change of composition which bodies undergo, oxygen must be more or less engaged. The universality of its presence, however, taken in connection with the ever-varying proportions in which it exists in bodies, and the infinite variety of shades and colours which these bodies present, furnishes arguments against the idea of its acting as the colouring principle of matter: nor does it readily appear why the oxygen, more than any other ingredient of the body, should give rise to the formation

* Haüy's Traité de Phys. vol. ii. p. 185.

of its colour. Because air is necessary to the vegetation of plants, M. Fourcroy, indeed, ascribed the different shades of vegetable colours to the different degrees in which he supposed oxygen to combine with them *; but it has been sufficiently shewn, that, during vegetation, the oxygenous portion of the air never combines with plants; and that, as far as external agents are concerned, light, and not air, is the ostensible cause of their colouration. The ordinary effects of acids in changing colours, must not be attributed to the oxygen they contain, but to their action as acids; for muriatic acid, which does not yield its oxygen to any known substance, acts, like other acids, upon vegetable infusions; and oxy-muriatic acid, which, of all substances, most readily parts with its oxygen, entirely destroys all vegetable colours †. In whatever light, therefore, this subject is viewed, no good reason appears for considering the element of oxygen as essentially concerned in producing the colouration of natural bodies.

430. Since, then, neither the mechanical doctrine of density, nor the supposition of phlogiston, nor the actual combination of oxygen with bodies, seems sufficient to explain those affections of light, from which the diversity of their permanent colours proceeds, we must seek out some other mode of ac-

* Mem. de l'Acad. an. 1789, p. 335.

† In our opinion, the late experiments of Mr Murray seem to establish the compound nature of oxy-muriatic gas, in opposition to the view of Mr Davy, who regards it as a simple substance *.

* Nicholson's Journal, Feb. 1811.

counting for these phenomena, less liable to objection than any that has been hitherto proposed. It is remarked by Dr Bancroft, that, though the prism and other transparent colourless substances shew us the different colours of the several rays of light, by separating them from each other, in consequence of their greater or less refrangibility; yet he is persuaded that the *permanent colours* of different bodies, or substances, *are not produced by mere refraction;* but depend on other properties, which determine or occasion the reflection or transmission of some particular sort or sorts of rays, and an absorption or disappearance of the rest: and these properties he conceives to be certain *affinities* or *elective attractions,* existing in, or between the differently coloured matters, and the particular sorts or rays of light so absorbed or made latent *.

431. That an affinity, or attraction, is exerted between light and the particles of bodies, may be justly inferred from the great refractive power of inflammable bodies, which, all other things being equal, must be supposed to attract light more powerfully than other substances; and it is *variation* in point of strength, says Dr Thomson, which constitutes the characteristic mark of chemical affinity †. The phenomena of phosphorescence, and many other chemical facts, afford evidence of the same nature. Thus, many metallic oxides, as Scheele first observed, are soon restored to their metallic form, by the action of

* On Perm. Colours, p. 29.

† Syst. Chem. vol. i. p. 246. 1st edit.

light *. The muriate of silver, when exposed to the solar rays, begins to be discoloured at the end of a few seconds; after a minute, says M. Senebier, its surface is sensibly violet; and in half an hour, the violet is changed to the shade of umber, and then suffers no farther change. This change is effected entirely by light alone; for when this muriate is exposed to heat, or cold, or moisture, in a dry air, or *in vacuo*, it suffers no change, if the light be carefully excluded. If, however, the light be thrown on it by a lens, it is then coloured in an instant. If it be covered by one leaf of paper, the discolouration does not begin till the expiration of a minute; if with two leaves, three minutes are required; if with three leaves, ten minutes; and four leaves entirely prevent the action of light †.

432. In effecting this discolouration, it was remarked by Scheele, that the violet ray acted sooner than any other ‡; and Senebier having thrown the prismatic rays, in succession, on portions of this muriate, observed that the violet ray acted in 15 seconds; the indigo in 23 seconds; the blue in 29; the green in 37; the yellow in five minutes and a half; the orange in twelve minutes; and the red in twenty minutes; but the three last species never produced the effect so strongly as the others §.

---

* On Air and Fire, p. 78. et seq.

† Mem. Phys. Chim. t. iii. p. 199.

‡ On Air and Fire, p. 91.

§ Mem. Phys. Chim. tom. iii. p. 199.

433. The muriate of silver, which is thus acted on by light, is composed of 75 parts oxide of silver, 18 muriatic acid, and seven of water. Its discolouration has been ascribed to a partial reduction of the oxide; but, by operating upon the salt under water, Scheele and Berthollet found, that muriatic acid was liberated; and to the disengagement of this acid alone, the discolouration seems to be owing *. In conformity with this explanation, Dr Bancroft observes, that, if this salt be covered with muriatic acid, instead of water, it experiences no change, though exposed, for many days, to the direct rays of the sun †.

434. The action of light on vegetables, seems to resemble, in many respects, its operation on inanimate bodies. We have already seen, that light, in its undecomposed state, causes the expulsion of oxygen gas, and gives rise, at the same time, to the green colour of plants. To discover the manner in which it was more immediately concerned in these operations, M. Senebier sowed different quantities of lettuce seeds in several small cups. One of these cups he left exposed to the light and air; another he placed in darkness; a third he confined under a large glass vessel, whose bottom was thrust so far up into its body, as to leave a hollow space, nine or ten inches in height, and four or five in width; this vessel was then filled with water, through which the light, that fell on the seeds beneath, necessarily pass-

* Murray's System of Chemistry, vol. iii. p. 123.

† On Permanent Colours, p. 35.

ed; a fourth cup was placed under a similar vessel that contained a yellow fluid; a fifth, beneath a similar vessel filled with a red fluid; and a sixth, under one that contained a fluid of a violet hue; so that through these fluids, the yellow, red, and violet rays were respectively transmitted, while the others were, for the most part, intercepted.

435. Observing, then, the effects produced by the different portions of light, which were thus permitted to act, he found that the plants, illuminated by the yellow rays, grew most rapidly in height; next, those in the violet rays; afterwards, those in the red rays. The plants which grew in light, transmitted through water, were still smaller, and approached in size to those which flourished in the open air; while those in perfect darkness attained the greatest height of all. These last plants perished on the eighth day, and those in the yellow light on the ninth day; while all the others continued to vegetate. At the end of about five weeks, the plants, growing under the red vessel, were four inches and nine lines in height; under the violet vessel, three inches and three lines; under the water vessel, two inches and ten lines; and one inch and three lines in the open air.

436. With respect to the general appearance of the plants, the leaves of those which grew in red light were smaller and less smooth, than those of the plants in violet light; or than the leaves of the plants confined under water, or than the leaves of those which grew in the open air. As to colour, the leaves exposed to yellow light were at first green,

but afterwards became yellow; those in red light appeared green, and preserved a tinge of that colour; those in violet light were quite green, and their colour augmented with their age; while those raised in obscurity possessed no verdure at all *.

437. These experiments were repeated on French beans, and with results nearly similar. In proportion as the plants grew in height, in different kinds of light, the number and size of their leaves diminished. In the free air and light, the leaves of beans, shooting out of the earth, became green in a day or two, according to the intensity of the light; those leaves which received light through water, had a deeper green colour; those in violet light acquired a deep green, approaching to blackness; those growing in red light were also green, but less so than natural leaves. In all these experiments, more or less light was reflected by the fluids employed, or intercepted in its transmission through them. If, therefore, the effects produced come near to those which attend the action of entire light, it must, says M. Senebier, be owing less to the intensity of illumination, than to the quality of the illuminating ray. Hence, when the violet ray renders plants as green at least as entire light, this cannot arise from its power of illumination; for this ray is only a part of entire light, and is, besides, transmitted through a glass vessel, and through the fluid which that vessel contains. But the violet light, which thus acts on the colour, does not equally act on the growth and development of the plant; whence it is concluded, by M. Senebier,

* Mem. Phys. Chim. t. ii. p. 55. et seq.

that the height and size of the plant are proportional to the intensity of illumination, while its verdure depends more on the quality of the ray *.

438. The above experiments, not only prove the action of light in the colouration of plants, but demonstrate, likewise, that this action is exerted most powerfully by the violet ray. They shew, also, that this property of the violet ray is independent of its *illuminating* power; but they do not authorise the conclusion, that the height and size of the plant depend on the intensity of illumination, for we know that other agents contribute to these conditions. By other experiments, M. Senebier also ascertained, that the *heating* power of the violet ray had no particular influence in effecting these changes of colour; for, by placing thermometers in the differently coloured rays of the prismatic spectrum, he found that the violet ray possessed less heat than the others, and less, also, than the entire beam of light; whence, says he, it results, that these properties of the violet ray are independent, in a certain degree, both of its heating and illuminating power, and appear to depend on some particular quality of the matter of which it is composed †.

439. M. Berthollet, likewise, who, as we have seen (362.), considers the action of light, in discharging colours, to resemble combustion, and thus to cause the combination of oxygen with bodies, admits, nevertheless, many facts which present an apparent contradiction to it. It is to the action of the

* Mem. Phys. tom. ii. p. 62.

† Ibid. p. 72.

sun's rays, says he, that the production of vegetable colours is owing. Light, also, disengages oxygenfrom nitric and oxy-muriatic acids, from some metallic oxides, and from plants in a state of vegetation. In these examples, he continues, effects opposite to those of combustion are produced; but when it contributes to the destruction of colours, it combines oxygen, and produces a kind of combustion. What, however, are the circumstances, and what the affinities, which determine sometimes one effect, and sometimes the other, he does not know; but both, he adds, are equally proven *. Dr Bancroft, also, refers to different instances, in which light acts sometimes by separating, and sometimes by combining, oxygen with bodies; which he ascribes to the varied operation of an affinity exerted between them †. These various, and apparently contradictory effects of light, may, perhaps, receive illustration from a more minute inquiry into the nature and constitution of that subtile matter. The researches of modern philosophers furnish many new and important facts to aid our investigation; and it is by collecting and comparing these that we are led to indulge the hope of being able to penetrate somewhat farther into the secrets of its chemical action.

---

440. The colours of the primary rays of light, and their different degrees of refrangibility and re-

---

* Elem. de la Teinture, tom. i. p. 58.

† On Permanent Colours, p. 46.

flexibility, were, as we have seen, discovered by Newton; and the same incomparable philosopher pointed out also the difference in their *illuminating* power. The most luminous of the prismatic rays, says he, are the yellow and orange; next to these, the red and green; the blue is fainter; and the indigo and violet are still more dark and faint. The most luminous and fulgent part is in the brightest yellow, where it inclines more to the orange than to the green *. Dr Herschell, by causing the prismatic rays to fall successively on an object placed beneath a lens, found the illuminating power to be small in the red ray, greater in the orange, and greatest in the centre of the spectrum, between the yellow and green. From the full deep green, the illuminating power decreased very sensibly, being in the blue nearly on a par with that of the red, and in the violet it was least of all. By other experiments, he ascertained that this property resides in peculiar rays, which are distinct from those that impart heat; for the illuminating rays are capable of being transmitted through substances by which the heating rays are stopped, and *vice versa*;—facts which the experiments of Scheele had before shewn to be the case with the heat and light which radiate from a common fire †. To these facts we may add, that the light of the moon, though composed of all the prismatic rays, does not, even when concentrated by a lens, impart the least sensible heat. Hence then the rays, which render objects

* Optics, B. i. prop. 7.

† On Air and Fire, p. 70.

visible, and enable them to exhibit colour, possess peculiar properties, and are entirely distinct from those which excite or produce heat.

441. The heating or *calorific* rays, which enter into the composition of solar light, appear to have been first distinguished as a distinct species of matter, by M. Rochon, who, in the year 1775, discovered that the rays, which differ in refrangibility, differ also in their power of heating bodies. He observed that an air thermometer, moved through the prismatic spectrum, rose in proportion as the rays followed one another from the violet to the red extremity; so that the heat between clear red and the most intense violet appeared nearly as eight to one *. M. Senebier also, (438.), by a similar mode of experiment, ascertained the same fact with less precision; and Dr Herschell, in a similar manner, calculated the difference nearly as seven to two. Pursuing the subject still farther, Dr Herschell ascertained that this calorific effect was produced by rays, different from those which impart light and colour; that it was, in fact, produced by *invisible* rays, which increase progressively in power from the violet to the red extremity of the spectrum, and exert the greatest effect about half an inch beyond the boundary of the latter †. These facts are farther confirmed by Sir H. Englefield, who having successively collected the prismatic rays into the focus of a lens, found the violet, in three minutes, to raise the thermome-

* Haüy's Nat. Phil. vol. ii. p. 256.

† Phil. Trans. 1800.

ter, placed in its focus, only one degree; while the red rays, in two minutes and a half, raised it sixteen degrees; and when the thermometer was carried quite out of visible light, it rose, in two minutes and a half, eighteen degrees *. From these experiments we learn, that the *heating* power of solar light consists in *invisible* rays, which are entirely distinct from those which produce illumination and colour. It may be added, that Scheele had previously ascertained, that the caloric, which radiates from a common fire, and causes ignition, consists, likewise, in *invisible* rays †.

442. But we have also seen (432.), that the *chemical* action of light is most powerfully exerted by the violet rays, which are far removed from the centre of the spectrum, where the illuminating power is greatest, and still farther from its red extremity, where the calorific effect is most intense. As, therefore, in their refrangibility, they seem to follow laws so distinct from the two other species, it was natural to expect that they should, also, consist of a distinct kind, or third species of matter. This, M. Ritter has discovered to be actually the case, and that, like the calorific rays, these chemical rays are *invisible*, and possess the greatest power beyond the violet boundary of the spectrum. He found that muriate of silver, which became black when placed beyond the confines of the violet ray, gradually lost its dark tint, as it was moved through the other rays towards the red extremity of the spectrum; and when the

* Murray's Syst. Chem. vol. i. p. 519. † On Air and Fire, p. 76.

same substance, a little blackened, was exposed to red light, it recovered in part its whiteness, especially when presented to the invisible calorific rays, which lie beyond the limit of the red extremity. He observed, also, that phosphorus, when placed near the red rays, instantly exhibited white vapours; but, when moved into the violet rays, these vapours no longer appeared, and its combustion was extinguished. Hence he was led to conclude, that the solar spectrum was comprised between two sets of *invisible* rays, which produce the opposite effects of combining and separating oxygen from bodies, while the intermediate parts of the spectrum partake more or less of the action that is exerted by the invisible rays on either side *. These chemical rays were, likewise, discovered by Dr Wollaston, who found that muriate of silver was blackened, not only in the space occupied by the violet ray, but in an equal degree, and to about an equal distance, beyond the visible spectrum; and, by narrowing the pencil of light received on the prism, the discolouring rays were made to fall almost entirely beyond the violet: whence he inferred, that visible light does not possess this discolouring property, but owes its influence, in this respect, to the admixture of invisible light †.

443. From the foregoing series of facts, it would appear, that solar light is made up of three distinct species of rays; one species of which produces heat, and promotes the combination of oxygen with

---

* Haüy, vol. ii. p. 259. † Phil. Trans. 1802, p. 379.

bodies: a second species is luminous, and imparts colour to objects; and the third species exerts a chemical action on bodies, and causes the separation of oxygen from them. It likewise appears, that, while the luminous rays differ gradually from each other in their degrees of refrangibility, the calorific and chemical rays are, in this respect, entirely opposed, and are therefore found, in greatest intensity, near to and beyond the opposite boundaries of the prismatic spectrum.

444. The different chemical effects, produced in bodies by the agency of light, which M. Berthollet so strongly remarked (439.), but was unable to explain, receive, we think, an easy solution from these views of the different nature and operation of its calorific and chemical rays; for these two portions of light seem respectively fitted to produce the opposite effects of combination and decomposition, which he has noticed. These effects, M. Ritter has farther remarked, to resemble those which are produced by the opposite electricities of the Voltaic pile; for, while positive electricity, in the decomposition of water, occasions, like the calorific rays, the combination of oxygen with the metallic wire, no such effect takes place at the opposite, or negatively electrified wire *. To follow this analogy with greater precision and effect, let us consider, somewhat more particularly, the chemical operation of Galvanic electricity, as it has been developed in the late important experiments of Mr Davy.

* Haüy, vol. ii.

445. It appears, from the investigations of this distinguished chemist, that the elements of bodies are not only separated from each other by the operation of Galvanic action, but are actually transferred to distant places, in a state or condition which, for a time, entirely suspends the exertion of their chemical powers. Thus, if two portions of a neutral salt, as muriate of soda, be exposed, in separate glasses, to the respective poles of the Voltaic battery, and the circuit be completed by a moistened substance, plunged on each side into an intermediate vessel that contains pure distilled water, the salt in each glass undergoes decomposition. At the positive pole, the acid is attracted, and the alkali is repelled: at the negative pole, the reverse operations take place; while the repelled alkali from the one side, and the repelled acid from the other, meet in the middle vessel, and recompose muriate of soda. "So from the general phenomena of decomposition, and transfer, it is easy," says Mr Davy, "to explain the mode in which oxygen and hydrogen are separately evolved from water. The oxygen of a portion of water is attracted by the positive surface, at the same time that the hydrogen is repelled by it; and the opposite process takes places at the negative surface; and in the middle, or neutral point of the circuit, whether there be a series of decompositions and recompositions, or whether the particles from the extreme points only be active, there must be a new combination of the repelled matter *."

* Phil. Trans. 1807.

446. "These facts," continues Mr Davy, "seem fully to invalidate the conjectures of M. Ritter, with regard to the elementary nature of water. He conceived that he had procured oxygen from water without hydrogen, by making sulphuric acid the medium of communication at the negative surface; but, in this case, sulphur is deposited, and the oxygen from the acid, and the hydrogen from the water, are respectively repelled, and a new combination produced *." In support, however, of his opinion, M. Ritter has since urged the result of another experiment, to which many eminent chemists have yielded their assent. He decomposed water in two separate glasses, connected with each other by a metallic arc, and obtained oxygen at the positive, and hydrogen at the negative pole, in the usual manner. Deeming it impossible, however, that the two corresponding elements, which were extricated, could be repelled through the substance of the metallic arc, he was induced to deny altogether the decomposition of the water, and to maintain its elementary nature, conceiving that, by its combination with positive electricity, it formed oxygen gas, and, by its union with negative electricity, it constituted hydrogen gas.

447. The fallacy of the experiment, which led to this conclusion, has, however, been clearly detected by Mr Murray. He formed the Galvanic arrangement employed by Ritter, and observed oxygen and hydrogen to be liberated in the respective glasses; but, on a closer examination, he discovered that the

* Phil. Trans. 1807.

two extremities of the metallic arc soon acquired, by the law of *induction*, electricities the opposite to those possessed by the Galvanic wires. Consequently, the effects of a double battery we e obtained, and, in each glass, two particles of water were decomposed, just as happens in one glass, when the two wires of the battery approach each other.

448. This fact was rendered very obvious by an ingenious and simple variation in the mode of making the experiment. The wires of the battery were made to pass through glass tubes, and the tubes were then placed in the two glasses, which, as before, were connected by the metallic arc. Instead of water, however, both the tubes and glasses were filled with an infusion of red cabbage, which held a neutral salt in solution. As soon as the electricity was put in motion, the neutral salt, in each tube and glass, was decomposed; and the effects were at once conspicuous on the vegetable infusion. For, on the side connected with the positive end of the battery, the fluid in the tube was reddened; while, in the glass of the same side, it was rendered green. On the contrary, the fluid in the tube connected with the negative side was green, and in the glass of the same side it was red. Hence, decomposition had taken place on each side; and while the positive pole of the battery attracted, as usual, the acid, which reddened the infusion in the tube of that side, the negative extremity of the arc attracted the alkali in the glass below, and changed its fluid to a green; and, by the opposite electricities of the respective wires, reverse effects were produced in the fluids of the tube and

glass connected with the negative side of the battery. " These facts, therefore," to repeat the language of Mr Davy, " seem fully to invalidate the conjectures of M. Ritter and some other philosophers, with regard to the elementary nature of water, and perfectly to confirm the great discovery of Mr Cavendish."

449. Such, then, is the general mode in which Galvanic electricity exerts its chemical action; but let us farther observe, separately and more minutely, what happens at each pole of the battery. First, then, at the positive pole, a particle of water, for example, is decomposed; its oxygen is attracted, and its hydrogen is repelled. But this is not all that happens; for the attracted oxygen combines, in most instances, with the metallic wire, and thus we observe not only the act of decomposition, but that of combination also.

450. At the negative pole, the operations are somewhat different. A particle of water is here, also, decomposed; but its hydrogen is attracted, and its oxygen is repelled. The attracted hydrogen, however, does not combine, like the oxygen at the opposite pole, but passes off in a gaseous form. Hence the electricity, accumulated at the negative pole, is sufficient only to separate the elements of the compound, but not to combine them anew; and it may, therefore, be said to accomplish decomposition only, while the other pole effects combination also. So truly is this the case, that silver, says Mr Davy, though one of the least oxidable of the metals, easily unites with oxygen when it is positively electrified, while zinc, one of the most oxidable, is incapable of

combining with it, when it is negatively electrified *. In gold and platina, which have a very weak affinity for oxygen, even the accumulation of positive electricity is insufficient to effect oxidation; and hence, when these metals are employed, the oxygen passes off in a gaseous form, like the hydrogen at the opposite pole.

451. The chemical actions, which the Galvanic fluid thus exerts, are equally accomplished by the operation of common electricity. By the concentrated discharge of this fluid, iron-wire, and other metals are speedily reduced to the state of oxides †. The formation of water, by the combustion of hydrogen, and the production of nitric acid by the electrization of atmospheric air, in the great experiments of Mr Cavendish, afford, also, pure examples of combination. On the other hand, by the varied operation of the same agent, the products, thus formed, may again be decomposed. Thus, M. Beccaria revivified many metallic oxides by the agency of electricity, restoring the oxide of zinc to its metallic form, and reconverting cinnabar into real quicksilver ‡. So, in the experiments of the Dutch chemists, and of Dr Pearson, water was resolved into its constituent elements, by the agency of the same power ¶; and nitric acid might, doubtless, be made to undergo a similar change, since Dr Wollaston readily decomposed it by Galvanic electricity §. Hence, then, the

* Phil. Trans. 1807.

† Priestley's Hist. Elec. p. 276. ‡ Ibid. p. 277.

¶ Murray's Chem. vol. ii. p. 172.

§ Phil. Trans. an. 1801, p. 428.

positive electricity of the common machine is able, under different circumstances, to effect both decomposition and combination.

452. But the ingenious experiments of Dr Wollaston farther shew, that the negative electricity of the same machine, like that of the Voltaic battery, effects only decomposition. He coated a portion of silver-wire, $\frac{1}{110}$ of an inch in diameter, with sealing wax, and, by cutting through the middle of the waxed portion, exposed a section of the wire. The two coated extremities of the divided wire were then immersed in a solution of sulphate of copper, placed in an electric circuit between the two conductors of a common machine; and sparks, taken at one-tenth of an inch distance, were passed, by means of the wires, through the solution. After one hundred turns of the machine, the wire which communicated with the negative conductor had a precipitate formed on its surface, which was evidently copper; but the opposite wire had no such coating. Upon reversing the direction of the current of electricity, the order of the phenomena was of course reversed; the copper being shortly redissolved by assistance of the oxidating power of positive electricity, and a similar precipitate formed on the opposite wire. A similar experiment, made with gold wire, in a solution of corrosive sublimate, had the same effect *. These facts, therefore, seem clearly to prove, that, by the common machine, as well as by the Voltaic battery, decomposition only is effected at the negative pole;

---

* Phil. Trans. an. 1801, p. 429.

while, at the positive pole, decomposition and combination successively take place, according to the nature of the bodies employed, and the intensity with which the electric matter is made to act.

453. After thus tracing the operation of the different electricities, in promoting the chemical actions of decomposition and combination, let us pass to the consideration of those, which the calorific and chemical rays of the solar beam are respectively found to produce. It is known, that, by concentrating the solar rays upon any body, a most intense heat is produced; that metals are converted into oxides; that the diamond or charcoal is made to combine with oxygen, and form carbonic acid; and, in fact, that almost all the combinations, which can be effected by combustion, may be, in this manner, accomplished. Now the portion of solar light which contributes to these effects, must be the calorific rays; for neither the illuminating, nor the chemical rays produce heat, and they are, consequently, incapable of exciting combustion.

454. But these same calorific rays, not only thus cause the combination of oxygen with bodies, but, under different circumstances, they occasion the separation of this element from them. Thus, if a compound combustible be exposed to the concentrated action of the solar rays, its several elements are first separated, and almost, at the same instant, recombine into a new form, according to the nature of the body employed, and the greater or less intensity with which the caloric is made to act; and hence the various gaseous, fluid, and solid products, which the

combustion of the same body, or of different bodies, affords. The calorific rays of light, therefore, like positive electricity, are able to effect in bodies the chemical actions of decomposition and combination, and these in an order depending on the state in which the bodies are presented to their action.

455. But besides these operations, which the calorific rays effect, we have seen, that the decomposition of metallic oxides (431.), of different acids (439.), and especially of carbonic acid, is produced in that part of the prismatic spectrum which is farthest removed from the heating power of light, and, consequently, by the chemical rays, which possess no heating power. In all these cases, however, we observe decomposition only to take place, and no subsequent combination to follow, in the manner in which that operation is accomplished by the calorific rays; for the oxygen is only released from its combination with the metal, with carbon, or with any other substance with which it was previously combined, but no new product is, at the same time, formed, neither are the usual phenomena of combustion exhibited. Hence, therefore, we may remark a striking difference between the action of the chemical and calorific rays of light; for though both species agree in the property of effecting decomposition, the calorific rays alone give rise to new combinations. Here, then, we trace a close analogy between the operations of solar light and of electricity, not only in the general similarity of effect which they produce in bodies, but also in the particular laws of their action; for negative electricity, like the chemical rays, produces only

decomposition in bodies, while positive electricity, like the calorific rays, occasions both decomposition and combination.

456. Farther, the phenomena, which accompany these chemical changes, attest the great similarity in the operations of these subtile agents; for light and heat, the characteristics of combustion, excited by the calorific rays, are the well-known attendants of ordinary electricity. In the low state, indeed, in which Galvanic electricity sometimes effects decomposition, no heat is perceptible; but when it is employed in a state of greater intensity, Mr Davy found it rapidly to evaporate water, and to inflame and volatilize nitrate of ammonia *. Whenever bodies, brought by artificial means into a high state of opposite electricities, are made to restore the equilibrium, heat and light, says he, are the common consequences; and they are, also, the constant result of intense chemical action. But where large quantities of electricity, of low intensity, act, or the combinations are slowly effected, there is an increase of temperature without luminous appearance †.

457. To the exhibition, however, of these phenomena in combustion, the presence of oxygen is necessary; for the calorific rays do not excite combustion *in vacuo*, nor in any gas deprived of oxygen, even when the most inflammable substance is employed. The very same condition is required for the excitation of electricity. Colonel Haldane observed, that, when the Voltaic pile was placed *in vacuo*, its action immediately ceased; that in nitro-

* Phil. Trans. 1807. † Ibid.

gen gas it did not even commence; while, in oxygen gas, or in atmospheric air, it acted with energy, and the oxygen disappeared. These facts were confirmed by Mr Davy, who found, that, in gases devoid of oxygen, no Galvanic electricity could be excited; but it was more or less abundantly developed, when oxygen gas was present *.

458. But oxygen gas is necessary to combustion, in consequence of the great affinity, which, at a high temperature, it exerts towards combustible bases; and, during the process, it enters into combination with these bases, and its latent caloric is disengaged. In like manner, Dr Wollaston remarks, that the excitation of Galvanic electricity depends on oxidation; and that the oxidation of the metal is the primary cause of the electric phenomena, and is not occasioned by the electricity itself. In several experiments, in which silver and zinc were plunged in water, holding mineral acids in solution, the zinc was dissolved, and hydrogen gas liberated by decomposition of the water. It would appear, therefore, says he, that, in the solution of a metal, electricity is evolved during the action of the acid upon it †.

459. This explanation, continues Dr Wollaston, receives additional confirmation from comparative experiments made with common electricity; for, in the experiment already related (452.), the copper, after being precipitated, "was shortly redissolved by assistance of the oxidating power of common electricity ‡."

---

* Murray's Chem. vol. i. p. 590. 1st edit.

† Phil. Trans. 1801, p. 427. ‡ Ibid. p. 430.

So, likewise, by using an amalgam of silver or of platina, which are not liable to be oxidated, he could obtain no electricity. An amalgam of tin, on the contrary, afforded a good degree of excitation. Zinc acted still better; but the best amalgam was made with both tin and zinc, a mixture which is more easily oxidated than either metal separately. In farther proof of this position, he found, that, when he confined a small machine, with its cushion and conductor, in a vessel of common air, electricity was excited; but when he substituted carbonic gas, the excitation was immediately destroyed; and again returned upon re-admission of atmospheric air. The oxidated metal of the rubber, adds Dr Wollaston, is always negative, and so, likewise, in the Voltaic pile, the oxidated zinc is in the same state. From these facts, he concludes, that electricity, in the common machine, and in the Voltaic pile, originates from the same source; and the power of the latter, he adds, is now known to depend on oxidation *.

460. But if, from the foregoing facts, it appear, that, in ordinary cases, oxygen gas is alike necessary to the development of caloric in combustion and to the excitation of electricity; if, in each instance, this gas disappear, and its ponderable matter enter into a similar combination; and if no caloric or electricity be developed unless these chemical changes take place, are we not constrained to believe, that the same subtile matter, which, during combustion, is exhibited in the form of caloric, appears, during electrization, in the guise

* Philos. Trans. 1810, p. 433.

of the electric fluid? For why should oxygen gas be thus essential to the development of electricity, if it be not *chemically* instrumental in affording electric matter? and in what other way can it, in these experiments, be conceived to afford such matter, except by suffering that reduction of its elasticity, which it has been shewn to undergo? If the air were only mechanically concerned, and acted simply as a *conducting* body, or, if the electricity were excited only by friction, then no reason appears why oxygen gas should be thus essential to the operation, and much less why it should undergo such *chemical* changes; for, that these changes are necessary, is proved by the fact, that even oxygen gas itself is unequal to the production of electricity, if a metallic substance, incapable of oxidation, be employed as an amalgam. Surely the conversion of zinc into an oxide by combustion in oxygen, does not furnish any other or better evidence of the extrication of caloric from that gas, than the formation of a similar oxide in electrization affords of the development of electric matter from the same gas, in the experiment of Dr Wollaston. In both cases, the subtile matter is not simply *conducted* by the air, but is *generated* out of it; and, in both cases, therefore, its production must be the *consequence*, and not the *cause*, of oxidation.

461. But while, in these examples, we attribute the production of electric matter to the chemical action of oxidation, we do not mean to say that this is the only, or even the usual way, in which electricity is developed. The experiments of various philoso-

phers sufficiently shew, that electricity is afforded by the simple contact of different bodies, by friction, and by other means, in which no chemical change takes place, and where, therefore, we have no evidence that any chemical action is exerted. But the excitation of electricity, by these mechanical means, does not disprove its development by chemical agency, any more than the extrication of caloric by friction or percussion disproves its liberation in the ordinary process of combustion. We therefore conceive, that the electric and calorific matter may be developed in bodies, both by chemical and by mechanical means; and we consider this circumstance to yield no little support to that opinion of their similarity, which we have now been endeavouring to maintain *.

462. Thus, then, we see, from the foregoing statement and comparison of facts, that the calorific and chemical rays of solar light severally decompose and combine various bodies, precisely in the same manner, and with the same phenomena, as the different states or kinds of the electric fluid have been shewn to do;

---

* In the excitation of common electricity, indeed, both methods are usually called into action; for the simple friction of the cylinder and rubber yields, in a mechanical manner, the electricity that is supplied by the communication maintained with the earth; while the amalgam furnishes, at the same time, a chemical source of electric matter, in consequence of the oxidation which it is made to undergo. And as, in combustion, the chemical combination of oxygen is promoted by the communication of heat, so, in electrization, the oxidation of the amalgam must be facilitated by the electricity that is mechanically excited.

and that, in all cases, the same conditions are required for their operation, and the same laws of action are respectively observed. We do not deem it necessary, for our purpose, to institute a comparison between the *physical* properties of these subtile fluids; for it is only with their *chemical* agencies that we are at present concerned. Neither do we venture, from the examples which have now been selected, to decide on the question of *identity* between the two species of *invisible* light, and the two states or kinds of electric matter. Our present object will have been gained, if we have succeeded in shewing such a similarity of *chemical action* between these subtile agents, as will entitle us to infer, that, where similar effects are, in other instances, produced by them, we may reasonably impute to them a corresponding similarity of action.

---

463. Having thus endeavoured to establish the similarity of chemical operation between light and electricity, let us next proceed to an explanation of those decompositions which are effected by solar light. We have seen, that, by the direct agency of the calorific rays (453.), various bodies are decomposed, and their elements recombine into new forms, under which changes the phenomena of light and heat are exhibited. But the decomposition of many bodies is, likewise, accomplished by the chemical rays of light (454.), without the attendant phenomena of heat or luminous appearance; neither do the

separated elements enter into any new combination. In conformity, therefore, with the analogical inference already deduced (462.), we must suppose these chemical rays, like negative electricity, to exert an attractive force towards the inflammable element of the compound, and a repulsive force towards its oxygenous ingredient, whereby, as in electro-chemical operations, these elements are separated, and the decomposition of the body is effected. Whether, in ordinary cases, the calorific rays assist in effecting these decompositions, and to what extent they act, we have not the means of deciding; but as the chemical rays, after their separation from the calorific, appear to be equally capable (442.) of exerting this decomposing power, we must conclude that it is chiefly accomplished by those rays alone.

464. Applying, then, these views to the subject of carbonic acid, we have seen that this gas is formed, in combustion, by the direct agency (453.) of the calorific rays; and so powerful is the union between its elements, that, until the late discovery of potassium, no single substance, at the ordinary temperature of the atmosphere, was capable of effecting their separation. At a high temperature, however, Mr Cruickshank found that a partial decomposition of this acid was accomplished by heating together carbonate of lime and iron-filings, whereby the acid was reduced to the state of an oxide *. In the experiments of Mr Tennant, in which carbonate of lime and phosphorus were submitted to a high degree of heat,

* Nicholson's Journal, 4to, vol. v, p. 4.

the complete decomposition of carbonic acid was effected, and the charcoal appeared in the form of a black powder *. Mr Davy, also, remarked, that potassium readily inflamed and oxidated in carbonic gas †; and Dr Henry and Mr Dalton have likewise ascertained, that, by a continued repetition of electric shocks, this gas may be decomposed, even while it retains its elastic form ‡. In all all these examples, however, the decomposition is effected under circumstances absolutely destructive to the vegetable body; and, except when potassium is employed, caloric, or the intense agency of positive electricity, is called into action.

465. But at the ordinary atmospheric temperature, and even in a degree below *zero* (299.), carbonic acid is decomposed in plants by the power of solar light; and since this decomposition always precedes (378.) the formation of the green colour, and this green colour is effected (437.) by the sole agency of the chemical rays, it follows, that these rays alone are essentially concerned in the operation. If, therefore, these rays, in their chemical action, be considered to resemble negative electricity, they must be held to exert an attraction towards the carbon or inflammable base, and a repulsion towards its oxygen, in the same manner as in the decomposition of water by the negative pole of the Voltaic battery, its hydrogen is attracted and its oxygen is repelled. Consequently, the oxygen of the acid will pass off in

* Phil. Trans. 1791, p. 183. † Ibid. 1809, p. 73.
‡ Dalton's Chem. Phil. part 2. p. 232.

an elastic form, and the carbon will remain behind in union with the colourable matter of the plant.

466. But supposing solar light thus to act in decomposing carbonic acid in plants, some other agent must be called in to aid its operation; for the experiments of Mr Davy (303.) prove, that light alone is unable to decompose this acid gas; and it is equally certain that the plant itself is unequal to this effect without the agency of light. In the experiments of Mr Tennant and Dr Pearson, in which carbonic acid was decomposed at a high temperature by phosphorus, the acid was employed in a fixed state, while in union with lime or alkali; and it was only under such circumstances that its decomposition could be effected. The elasticity of the acid, in its uncombined state, may be considered as counteracting the affinity of the phosphorus to its oxygen; but in its concentrated state, says Mr Murray, it is more liable to be acted upon, and its decomposition may be farther promoted, by the affinity which the lime or alkali exerts to the phosphorus and oxygen, and in consequence of which they enter into combination *.

467. Now the same bodies which, in these experiments, were employed to reduce the elasticity of carbonic gas, exist abundantly in the juices of plants; and with these alkaline substances, this acid gas will naturally combine. That it does enter into such combinations, may be inferred from the fact, that, though it exist in large quantities in many leaves, it is not abstracted by the air pump, nor expelled by

* Syst. Chem. vol. ii. p. 355.

heat (348.), but is readily given out under the chemical action of solar light; and, indeed, M. Vauquelin and others have detected it in combination with alkaline matter. In this state, therefore, of combination and concentration, it may yield to the decomposing power of the solar rays, although it resisted their intensest action, while retaining its elastic form. And thus we learn, in what manner the chemical condition of the leaf may conspire with the agency of light in promoting the decomposition in question, and why neither the leaf alone, nor the power of light alone, is of itself able to effect it.

468. But after the oxygen of the acid is expelled, the carbon is necessarily retained in the leaves, and serves, probably, some useful purpose in those organs. It has been already remarked (25.), that plants not only owe their green colour, but their odour and combustibility, to the immediate agency of light; and as the chemical rays alone are necessarily concerned in the formation of this green colour, we must, also, suppose that they essentially contribute to the production of those other properties, which are so immediately associated with it. The resinous matter, which imparts this colour, is also the most inflammable part of plants, and that, too, in which, probably, the odour and combustibility principally reside; but in what manner the chemical portion of light immediately acts in the production of this resin, no facts enable us, at present, to state. The celebrated Scheele supposed light to be decomposed in vegetables, and to contribute to the formation of their combustible matter; for the generation of a green

resin in plants, after having been brought out of a cellar, where they are almost white, and standing only a couple of days exposed to the sun, makes this, says he, probable to me *; and even Newton conjectured, that gross bodies might receive much of their activity from the particles of light that entered into their composition †. Lastly, M. M. Chaptal and Hassenfratz found, that plants, which vegetated in darkness, contained much less carbon than those which had grown in the light; and M. Senebier also ascertained, that such plants yielded less oil and resinous matter ‡;—facts which seem directly to prove the influence of light in increasing the proportion of combustible matter.

469. But whence, it may be farther asked, is the alkali, which so much abounds in the ashes of the green parts of vegetables (377.), and so directly contributes to their colour, derived? It doubtless pre-exists in the vegetable body, and is not a product of combustion; for it is found in combination with different acids, and even in an uncombined state, before the process of combustion is instituted; and that process, as M. Vauquelin has remarked, serves only to develope the saline matter, as the other constituents are consumed. How, then, does this matter gain admission into the living plant? It is probably held in solution in the fluids, which plants absorb from the

* On Air and Fire, p. 158.

† Optics, Query 30.

‡ Thomson's Chemistry, vol. v. p. 360. 4th edit.

situations in which they grow. Thus, it is known, that terrestrial plants afford potassa, while those which grow in, and near the sea, chiefly furnish soda; but M. M. du Hamel and Cadet found, that, if marine plants be removed to in-land situations, they gradually cease to yield soda, and at length potassa only is obtained *;—facts which prove the influence of situation, in determining the kind and quantity of alkaline matter.

470. From the recent investigations of Mr Davy, we learn, also, that almost all solids and fluids, even the purest distilled water, contain saline matter; so that nearly every substance which enters vegetables must convey to them a portion of it. It cannot, however, be supposed, that the alkali is formed in the vegetable directly from its proper elements; for the bases of the alkalis so rapidly abstract oxygen from almost every other substance, that they cannot, as Mr Davy remarks, exist near the surface of the earth in a pure form. And farther, as the alkalis themselves attract acids, with scarcely less avidity than their bases attract oxygen, the same reasoning obliges us to suppose, that, as pure alkalis, these substances do not enter plants; wherefore we must conclude, that they chiefly gain admission, in combination with different acids, that is, in a neutro-saline state †.

471. After their entrance into vegetables, these saline bodies may be presumed to undergo various

* Murray's Chemistry, vol. ii. p. 94, 95.

† Phil, Trans. 1807.

changes; for, both in terrestrial and marine plants, alkaline matter is found in combination with different vegetable and mineral acids. Hence, therefore, we may reasonably suppose, that these saline compounds are decomposed, and formed anew, at different periods; and the great quantity of alkali that exists in the leaves, its presence in an uncombined state, and the chemical agency of light to which it is there exposed, all concur to shew, that these changes are principally effected in those organs. By such decompositions, alkaline matter may be developed, even although no oxygen gas be afforded, since the acid that is separated may not be decomposed; and thus light, by its chemical agency, may contribute to produce the green colour in plants, not merely by decomposing carbonic acid, in the manner already described, but by acting on such other saline compounds as may be present in the vegetable fluids.

472. These views, respecting the entrance of saline matter into plants, and its subsequent decomposition, derive support from some well-established facts, relating to the use of lime in the practice of agriculture. The good effects of this substance, when laid upon soils, endure only for a limited period; and hence its application is usually repeated every ten or twelve years. In some soils, the lime may, perhaps, act mechanically in improving the texture; in others, it may act act chemically, both in improving the texture, and in facilitating the decomposition of organic matters; but it may also, we believe, act *physiologically* on the plant itself, by mixing with its fluids, and serving those useful purposes in the vegetable econo-

my which alkaline matter seems destined to perform. The experiments of Mr Davy prove that lime, as well as alkali, is present in the vegetable fluids * ; and it has been shewn to exert similar effects on the colourable juices (401.) of the leaves; so that we possess evidence, not only of its existence in the plant, but of its capacity to effect a necessary change in the vegetable juices. Such views explain why lime is useful in every kind of soil that is deficient in calcareous matter; and account, also, for its gradual disappearance or exhaustion. Is the large quantity of alkali, which green vegetables afford, obtained only in some compound form, from potassa existing in the earth? or, in the process of vegetation, is lime, which resembles it in so many of its properties, actually *transmuted* into it? The more hidden secrets of nature are but just beginning to be disclosed; and the dreams of alchemy are, perhaps, destined to take form and substance, from the researches of modern chemistry.

473. The analogy between the chemical operation of the different electricities of the Voltaic pile, and the calorific and chemical rays of light, which we have thus endeavoured to support, will, we conceive, derive confirmation from the results of certain electrical experiments, which have been made on vegetable matters. For the different electricities rapidly change vegetable colours, by developing, in all cases, as we believe, acid or alkaline matter, precise-

* Phil. Trans. 1807.

ly in the same manner as these changes appear to be more slowly effected by the chemical agency of light.

474. When the electric spark, says M. Cavallo, is taken in the tincture of certain flowers, it produces effects like an acid, which led some of the earlier electricians to consider the electric fluid as an acid *;—an opinion which has been more lately maintained by M. Brugnatelli, who goes so far as to assert that it actually forms saline compounds by combining with metals †. But this change of colour does not seem to occur when the air is excluded; for Dr Priestley did not observe it to take place in syrup of violets, when he electrified it in closed glass tubes ‡. Mr Nicholson and Mr Carlisle found an infusion of litmus to be reddened by the positive wire of the Voltaic pile, while it remained blue at the negative one; and Mr Cruickshank, who observed the same phenomena, supposed the acid, which caused the redness, to be the nitric, and the alkali, which gave a deeper tint of blue at the negative wire, to be ammonia §. Dr Wollaston impregnated a card with a strong infusion of litmus, and then passed along it, by means of two fine gold points, touching the card at the distance of an inch from each other, a current of electrical sparks. In this state, a few turns of the machine were sufficient to produce on the card a redness at the positive wire, very manifest to the

* Ess. on Med. Elect. p. 19.

† Wilkinson on Galvanism, vol. ii. p. 137.

‡ Hist. of Elect. p. 703.

§ Wilkinson on Galvanism, vol. ii. p. 49. 54.

naked eye; and the negative wire, being afterwards placed on the same spot, soon restored to the card its original blue colour. Similar results were obtained, in less time, by the different electricities of the Voltaic battery *.

475. Now, in all the foregoing examples, there is just reason to conclude, that the changes of colour were, in every instance, produced by the formation, or development of acid or alkaline matter: and the sources from which both might be derived are clearly pointed out in the late refined analyses of Mr Davy. He found that water, after repeated distillation, still contained saline matter, which was decomposable by electrization; that this matter was likewise obtained from the substance of glass, and from every earthy body; and, lastly, that the nitrogen, naturally existing in water, by combining with nascent oxygen or hydrogen derived from its decomposition, formed, respectively, nitric acid or ammonia, which were easily discoverable by the proper tests. But when the water which he used was rendered perfectly pure; when no saline matter existed in the substance of the vessels employed; and when nitrogen was wholly excluded, by conducting the operation in vessels of pure hydrogen gas, then neither acid nor alkaline matter was either generated or developed, but the water was resolved into its constituent gases alone †.

466. From these facts, we learn why, in Dr Priestley's experiment (474.), air seemed necessary

* Phil. Trans. 1801, p. 432. † Ibid. 1807.

to the change of colour in the vegetable infusion, since its nitrogen contributed to the formation of nitric acid. In the experiments of Brugnatelli, this acid, also, was probably produced; and either the formation of acid and alkali, or their development by the decomposition of saline matter, is clearly indicated in the experiments of Nicholson and of Cruickshank. In Dr Wollaston's experiment, the acid and alkali were probably derived from saline matter, existing either in the infusion with which the card was impregnated, or in the card itself; for Mr Davy remarks, that the minute quantity of saline compound, contained in paper tinged with turmeric, and which has been plunged in pure water, affords alkaline matter at the negative point of the circuit; and acid, in the same manner, is developed from litmus paper, at the positive point *. All these facts, therefore, afford striking illustrations of the great facility with which acid and alkaline matter may be generated, or developed, by the agency of electricity; and of the capacity of these substances, when thus produced, to act on the colourable juices of vegetables.

477. Nor is it only on these juices, after their extraction from the plant, that electricity is found to act; for results, in some degree similar, follow the operation of the same agent, when applied to the living leaf. Dr Priestley found, that electric explosions, passed over a green leaf, injured its texture, and left a yellow track behind †;—which effect was

* Phil. Trans. 1807.

† Hist. Elect. p. 684.

probably occasioned by the development of acid matter from the saline compounds of the leaf. In an experiment of Mr Davy, where a plant of mint was submitted to electrization in the Voltaic circuit, its saline compounds were actually decomposed; for green colouring matter, with resin, alkali and lime appeared in the negative vessel, and vegetable prussic acid in the positive vessel. In another experiment, a healthy plant of mint, which survived electrization, afforded, in ten minutes, alkali and lime to the negative, and acid matter to the positive side;--- facts, says Mr Davy, which shew that the electrical powers of decomposition act even on living vegetable matter *.

478. Since, then, from these facts, it appears, that electricity so readily decomposes the saline compounds in plants; since the separated portions of these compounds so easily act on the colourable juices of vegetables; and since, when light promotes the colouration of plants, it seems to act only through the medium of similar decompositions,---may we not reasonably conclude, that, as these subtile agents have, on other occasions, been shewn to produce so many effects in common, they act also, on these occasions, in a similar manner, and by the exertion of similar powers? The argument from analogy, which we thus desire to urge, does not hinge on one or two points of coincidence, but is borne out by the whole series of successive operations that has been now disclosed; and, if solar light and electricity be allowed

* Phil. Trans. 1807.

to have any one property in common, or to produce any one effect in common, we must think ourselves entitled to conclude, that they exert but one common action in their chemical operation on the saline compounds of plants.

---

479. But granting that Light acts *chemically* on the juices of plants, in the manner that has now been stated, and changes their composition, so as variously to affect their colour, yet, as this action is confined to the *invisible* rays, the actual production of colour cannot be ascribed wholly to chemical agency; and the question, therefore, still returns, How do plants, or the juices of plants, act on the *visible* or *colorific* rays of light, so as to present that rich variety of colours, which they actually exhibit to our view? To give some insight into this matter, it is necessary, as Sir Isaac Newton observes, to understand not only the nature of bodies, but the nature of light; for both, he adds, must be understood, before the reasons of their actions upon one another can be known *.

480. Having, therefore, as he conceived, established the fact, that "bodies reflect and refract light nearly in proportion to their densities," he proceeded to consider those properties of light which occasion it to be thus affected. These properties he supposed to consist in certain dispositions of the

* Optics, B. ii. part 3. prop. 10.

rays, which caused them, in their passage through any refracting substance, to be alternately reflected or transmitted, at equal intervals, for many vicissitudes; and the returns of these dispositions in the rays, he named fits of easy reflection or transmission. Of what kind, however, this action or disposition in the rays may be, he does not inquire; but suggests the possibility of its depending on a vibratory motion, excited in bodies by the rays which impinge on them. But whether this hypothesis be true or false he does not consider, contenting himself with the bare discovery, that the rays of light are, by some cause or other, disposed to be reflected or refracted for many vicissitudes *.

481. As thus, the mechanical hypothesis, even in the hands of Newton, seems to fail in assigning the cause, why light is variously refracted and reflected by bodies, we may, perhaps, be permitted to speculate on the facts and analogies, which the late discoveries in the elementary constitution of bodies, and in the nature of light, suggest to us; and try whether they will conduct us a step farther in the investigation of this intricate subject. "Prudens interrogatio," says Lord Bacon, "est dimidium scientiæ. Vaga enim experientia, et se tantum sequens, mera palpatio est, et homines potius stupefacit quam informat."

482. From the view which has been given (440. et seq.) of the nature and composition of solar light, it appears, that although, in many of their physical

* Optics, B. i. part. 3. prop. 12.

properties, the three species or portions of light agree, yet, in other properties, they very sensibly differ. We have already considered the nature and operation of the *invisible* portions of the solar beam, which effect chemical changes in bodies; and we have now to direct our attention to that portion which is *visible*, and imparts the sensations of light and colour. In the investigation of complicated actions, it is only by thus analysing and breaking them down into separate and distinct parts, that we can hope to discover the full reason and effect of their combined operations.

483. Many examples have been already given of the reciprocal action which bodies and the invisible portions of light exert on each other; and the phenomena of phosphorescence indicate the exertion of a similar action between bodies and the luminous or visible rays. So, likewise, whether the light afforded in combustion come from the air, or from the combustible body, it equally manifests in bodies the existence of a portion of the luminous rays. These rays, however, although thus retained in bodies, appear to exert on them no chemical action; for the body exhibits no apparent change of properties, whatsoever be the colour of the light which it retains, or emits. The emission of light, also, by vegetable and animal substances under putrefaction, is not the cause, but the consequence, of that process; neither does it appear, that the light afforded by certain living animals is necessarily productive of any chemical change in the animal, or in the air that surrounds it; but merely accompanies those changes, which are necessary to the exercise of the vital func-

tions. Farther, we have seen, that the chemical changes of decomposition and combination are effected by invisible light, in which changes the luminous or visible portion has no necessary share.

484. But in the operations of invisible light on bodies, we have traced a close analogy to the actions of the electric fluid; and may we not extend this analogy to those affections of the luminous or colorific rays, which enable bodies to present all the endless modifications of colour? Such an idea, however, at once presupposes the exertion of an attractive force between bodies and this portion of light; and should the circumstances already stated be deemed insufficient to establish this fact, the phenomena afforded by inflection seem decisive of the question. It is well known, that if different bodies be made to approach a beam of light, as it enters a darkened room through a small hole in the window, the light will be drawn out of its rectilinear course. Sir Isaac Newton found that this effect was produced not only on the entire beam, but, in different degrees, on the different colorific rays; for the fringed shadows of bodies held in red light were larger than those of bodies held in green light; and these last were still larger than those of bodies held in the violet rays: so that the same body, says Newton, acted upon the red, or least refrangible ray, at a greater distance than on the violet, and by those actions disposed the red light into larger fringes, and the violet into smaller, and the lights of intermediate colours into fringes of intermediate sizes *. Surely

* Optics, B. iii.

no experiments can more clearly shew, not only the general fact of an attraction exerted by bodies upon the entire beam of light, but also upon the individual colorific rays, of which that beam is composed; for when one species of matter is thus drawn out of its course by another, in what language can we express the fact, if we do not call it attraction? How, then, must we suppose this attraction to be exerted?

485. "Have not the small particles of bodies certain powers, virtues or forces," says Newton, "by which they act at a distance, not only upon the rays of light for reflecting, refracting, and inflecting them, but also upon one another, for producing a great part of the phenomena of nature? For it is well known that bodies act one upon another by the attractions of gravity, magnetism and electricity." "How these attractions may be performed," he continues, "I do not here consider. What I call attraction, may be performed by impulse, or by some other means unknown to me. I use that word here to signify only, in general, any force by which bodies tend towards one another, whatsoever be the cause. For we must learn, from the phenomena of nature, what bodies attract one another, and what are the laws and properties of the attraction, before we inquire the cause by which the attraction is performed. The attractions of gravity, magnetism, and electricity, reach to very sensible distances, and so have been observed by vulgar eyes; and there may be others which reach to so small distances, as hitherto escape observation; and perhaps," he adds, "*electrical attraction* may reach to such small distances, even without

being excited by friction *." Thus, then, we see, that, although in his inquiries concerning the motions of light, Newton makes no mention of attraction, yet, in his profound meditations on the general phenomena of nature, he not only employs that term, but points to the very species of attractive force, which the foregoing investigations have suggested; and, with almost prophetic sagacity, has foretold the existence of those "virtues or forces" in the elementary particles of bodies, on which the researches of Mr Davy have rendered it probable that their attractive energies depend.

486. From these researches, it would appear, that the elements of matter naturally possess different electrical states, which respectively determine their attraction or repulsion by the opposite poles of the Voltaic battery; and these natural electricities Mr Davy supposes them to retain in all the combinations into which they enter †. If this doctrine be just, and if the rays of light do obey laws similar to those of the electric fluid, there is no difficulty in conceiving that the particles of matter may exert different forces upon the luminous rays; and hence their refraction would vary both according to the natural electricities of the elements of the body, and to the properties of the individual rays; and the modified operation of such forces might be conceived sufficient to account for the varied refraction and extinction of the different rays of light.

---

* Optics, Query 31.

† Phil. Trans. 1807.

487. But if it were granted that the refraction and extinction of certain rays depended on some power which resembled electro-chemical attraction, yet the admission of such a power would not explain the phenomena of reflexion. Hence, should we admit, says M. Haüy, the existence of an affinity between light and the particles of matter, we should still be embarrassed in reconciling the repulsive force, which reflexion indicates, with affinity, which is an attractive force *.

488. If, however, we call to mind the motions that arise in bodies submitted to electrization, in which we observe attractions and repulsions not only to succeed each other with great rapidity, but frequently to exist together, we can have no difficulty in conceiving that certain rays may be refracted and extinguished in bodies, at the same time that others are reflected. Sir Isaac Newton himself, indeed, considered reflexion to be caused, not by the solid parts of bodies, but by some subtile matter which intercedes their pores †; and surely all the phenomena of electricity and of magnetism lead us to believe, that, where repulsion is ascribed to such a power, the corresponding attraction must be attributed to the exertion of a similar force. "Do not bodies and light," says Newton, "act mutually upon one another, that is to say, bodies upon light, in emitting, reflecting, refracting, and inflecting it, and light upon bodies in heating them?" &c. "And do not the rays

---

* Traité de Phys. tom. ii. p. 264. 2d edit.

† Optics, B. ii. part 3. prop. 8.

of light, which fall upon bodies, and are reflected or refracted, begin to bend before they arrive at the bodies, and are they not reflected, refracted and inflected by one and the same principle, acting variously in various circumstances * ?" If the view which has now been suggested be admitted, this question must be answered in the affirmative; for the various phenomena of refraction, reflexion, and inflexion, will fall to be regarded as modifications of electrical action. And thus have we endeavoured to connect the truths, which the intellectual eye of Newton perceived at so vast a distance, with the facts which the discoveries of Davy seem to have brought so immediately within our view.

489. But were the physical cause of refraction and reflexion thus attributed to electro-chemical agency, still the laws which these motions of light observe, become the subjects of mathematical consideration. Granting, also, that these motions arise from the varied exertion of attractive and repulsive forces, yet still the greater mass of matter, or its greater density, would necessarily augment the effect, even where the composition of the substance continued uniform; and a change in composition would act still more powerfully, and independently, in some degree, of the quantity of matter. In consistency, therefore, with these views, the Newtonian doctrine of density, in varying refraction and reflexion, may still, to a certain extent, be considered just; but the mass of matter, or its density, would, in such an hypothesis,

* Optics, Queries 4 and 5.

be regarded rather as conditions which modify, than as causes which produce the phenomena: and hence the mechanical properties of bodies would, in such actions, be considered subordinate to those which arise from their chemical constitution.

490. But whatever hypothesis may be adopted concerning the cause of refraction and reflexion, the certainty that these effects are produced, and that to them the colours of natural bodies are attributable, will not be disputed. It only remains, therefore, that we apply our knowledge of these subjects to the explanation of those colours with which nature has adorned and embellished the vegetable world.

491. The green colour which plants exhibit may be either simple or compound. Newton remarks, that a leek appears most resplendent when viewed by green light, and next when seen by the blue and yellow rays which compound a green *. In the opinion of M. Berthollet, the green colour of plants is produced by simple and homogeneal light, and not by a compound of the yellow and blue rays †; but M. Haüy asserts, that, if a slip of a green plant be laid on yellow paper, and held between the light and the eye, and the paper be then agitated so as to aid the sensation, the green slip will appear more or less blue, which proves, says he, that the green colour is a compound of yellow and blue, and not a simple colour ‡.

---

* Optics, B. i. part 2. prop. 10.

† Elem. de la Teinture, tom. i. p. 12.

‡ Traité de Phys. t. ii. p. 64. 2d edit.

492. This experiment resembles those made by M. Buffon, who found, that, by looking for some time on two objects of opposite colours, fixed upon one another, the sensation of a third colour, different from either of the former, was produced, to which he gave the name of *accidental colour*. The production of this colour has, by some philosophers, been attributed to a mixture of the differently coloured rays reflected by the two objects, combined with a certain disposition in the eye, to be acted on by the colour which conveys the stronger impression. But the experiments of M. M. Æpinus and De la Hire, seem to shew, that sensations of different colours may successively arise in the eye, where no mixture of colours from the object can be supposed to take place: thus, if the eye be directed to the sun, and then closed, the image upon the retina will be first red, then yellow, then green, and last of all blue * ; —facts which shew that these accidental colours depend more on the condition of the organ of vision, than on the colours of the rays which the luminous body emits. The same explanation may, perhaps, be extended to the *accidental* colours produced by reflected light; for by looking long and steadily at two differently coloured objects, kept in perpetual motion, the eye becomes dazzled and weakened, and is unequal, therefore, to that distinctness of vision, which at first represented them in their proper colours. According to the natural strength, also, of the visual organ, the same objects may, to different

---

* Priestley's Hist. Vision, vol. ii. p. 631.

persons, present different colours; so that no proof of the compound nature of the green colour of plants can be drawn from this experiment, relating to *accidental* colours, as M. Haüy seemed to suppose.

493. The operation of electricity on the organ of vision, supports the view above stated, respecting accidental colours; for when the eye is positively electrified, objects are said to appear red, large and distinct; and when it is in a negative state, they look blue, small and obscure *. M. Ritter placed his eye in contact with the negative pole of the Voltaic pile, when it acquired the opposite state of electricity, and the objects all appeared of a red tint; and afterwards, on applying his eye to the positive pole, which caused the retina to pass to a negative state, the objects all assumed a purple hue. Thus, the organ of sight, says M. Haüy, subjected to the combined action of light and electricity, becomes, as it were, a common link joining together phenomena observed till then in isolated experiments, and with instruments which appeared to have no affinity with each other †. And do not these similar sensations of colour, produced by the sun's light and by electricity, support the analogies traced, in so many other instances, between the operations of these subtile agents?

494. The mode, also, in which the green colour is formed in vegetable infusions, leads to the belief that it is truly a simple colour. When an alkali is added gradually to a colourless infusion, the liquor,

---

* Haüy's Elem. Phil. vol. ii. p. 55.

† Elem. Phil. vol. ii. p. 260.

at first, is violet, then blue, and then green, the colours emerging in the order of the refrangibility of the rays; and as, in this order, the green stands before the yellow, it is reasonable to suppose it to be, in this case, a simple colour, caused by the transmission of the green rays, rather than a compound of the less refrangible yellow with the blue. Add to this, that many of the vegetable colours, as the blue, the red and the yellow, must necessarily be simple, because they cannot be formed out of any other colours. But though the green be thus, for the most part, considered simple, yet it may, in some instances, be compounded, as must be the case with the colours of a vast variety of flowers, which present such an endless variety of hues.

495. As a general summary of the conclusions, which, in the foregoing discussions, we have endeavoured to establish, respecting the agency of light in promoting the colouration of plants, we may observe, in the first place, that, by the chemical agency (465.) of this subtile matter, the saline compounds of plants are decomposed, and the acid and alkaline matter, thus developed, combine with the colourable juices of the vegetable. In consequence of this combination, these juices are enabled to act variously on the luminous rays. When the alkali predominates, the more refrangible rays, as the violet, blue, and green, are reflected, and the other rays are extinguished; when the acid prevails, the least refrangible, or red rays, are reflected, and the others disappear; and from intermediate admixtures of these ingredients, intermediate colours, both simple and compounded, will a-

rise. The colours, however, which these juices present to our sight, are not reflected by the coloured particles (417.), but by the opaque matter on which they are imposed, so that the coloured matter transmits only, but does not reflect light; and this light arriving at the eye, produces an impression, which conveys the sensation of the individual colour.

496. Hence, too, it follows, that, when light is wholly excluded, the chemical changes in the vegetable juices, which enable them to exert these actions on the colorific rays, do not take place, and, consequently, the green colour of the leaves, which depends on the predominance of alkaline matter, and the red colours of leaves and of flowers, which arise from an excess of acid, are equally prevented from appearing; for the juices being unable, in this state, to decompose the solar beam, return it almost or entirely unchanged to the eye, whence the objects are destitute of colour, or have the appearance of whiteness. The colours of plants, therefore, depend primarily on the chemical action of light, in changing the constitution of their juices; and these juices, by their physical operation on the colorific rays, are then enabled to exhibit all their infinite variety of hues.

497. We are fearful that some of our readers may think we have extended these remarks on the agency of light too far, and that many of the observations, which have now been made, are but remotely connected with the professed objects of our inquiries. We entreat them, however, to suspend their judgment,

till they shall have accompanied us farther in our progress, and are thereby better enabled to appreciate the importance of the discussions in which we have been engaged. If Physiology, and the arts of Agriculture and of Medicine, which so much depend upon it, be ever destined to pass the narrow bounds which are at present drawn around them, to rise from facts to principles, and from effects to their causes, it can only be by the large and liberal cultivation of almost every branch of natural knowledge. "Interim nemo expectet magnum progressum in scientiis, (præsertim in parte earum operativa), nisi philosophia naturalis ad scientias particulares producta fuerit, et scientiæ particulares rursus ad naturalem philosophiam reductæ. Hinc enim fit, ut plurimæ artes mechanicæ, atque ipsa medicina, atque (quod quis magis miretur), philosophia moralis et civilis, et scientiæ logicæ, nil fere habeant altitudinis in profundo; sed per superficiem et varietatem rerum tantum labantur."—"Itaque minime mirum est, si scientiæ non crescant, cum a radicibus suis sint separatæ *." It is under the impression of these truths, that, through the whole of our inquiries, we have freely sought assistance from every department of science, to which we were able to apply,—convinced, that "that philosophy beareth best its own name, which doth not strain all to two or three principles, like two or three bells in a steeple, making a pitiful chime, but trieth to rise up to Nature's

---

* Nov. Organ. lib. i.

own number, and so to ring all the changes in the world *."

Sect. V.—*Of the supposed Utility of Carbonic Acid, and of the Decomposition of Water in Vegetation.*

498. From the circumstances of carbonic acid being decomposed in plants, by the agency of the solar rays, many writers have been led to consider that gas as necessary to vegetation in sunshine; and others have extended this opinion so far, as to suppose it essential to the growth of all plants through the day. In our former work, we combated the evidence (43.) on which this opinion was founded; but as new experiments have been brought in its support by M. de Saussure, and as, from the view which we have now taken of the phenomena of vegetation in sunshine, our former notions may be expected to have undergone some modification, we shall re-examine, as concisely as possible, the more material evidence relating to this question, with the hope of removing some of the difficulties, and apparent contradictions, which, at present, seem to attend it.

499. Is then carbonic acid essential to the vegetation of plants? We have no direct experiment, says M. de Saussure, by which this is demonstrated, and many facts seem even to establish the contrary.

* Grew's Anatomy of Plants, p. 223. an. 1682.

It is known, for example, that if plants be put to vegetate in a vessel filled with atmospheric air, deprived of its carbonic acid, and the vessel be then exposed to the sun, the plants continue to flourish, and the air experiences no change, either in purity or in volume *. These experiments he repeated many times, and always found the plants to grow as well in air, from which the carbonic acid had been previously removed by lime-water, as in those cases where this operation had not been performed †; from whence it necessarily follows, that the presence of carbonic acid in atmospheric air is not essential to the vegetation of plants.

500. But, from experiments of a different nature, M. de Saussure was led to draw an opposite conclusion. In vessels in which plants were placed to vegetate, he suspended a quantity of lime, which had been slaked by water, and afterwards dried briskly at a boiling heat; the vessels were, also, inverted into saucers of lime-water. After the second day, the atmosphere of the plants, exposed to the sun in this apparatus, diminished in volume. On the third day, the inferior leaves began to turn yellow: by the fifth day, they had all fallen, and the plants no longer exhibited signs of vegetation. Their atmosphere, at this period, had lost in bulk three cubic inches of the fifty originally employed; it was less pure than at first, but contained no carbonic acid. Sometimes the plants died in two days, when thus exposed to the sun; while others, confined in similar ves-

* Annales de Chimie, t. xxiv. p. 136. † Ibid. p. 144.

sels without lime, continued fresh and vigorous. These experiments with lime prove, says M. de Saussure, an attraction, and, consequently, a formation of carbonic acid by plants which vegetate in sunshine; from whence we collect, that plants growing in the light, form carbonic acid with the oxygen of the atmosphere *.

501. The foregoing conclusion seems to be fairly deduced from the results of the experiments; but M. de Saussure farther considers them to prove, that the presence of carbonic acid is necessary to vegetation, because the plants ceased to vegetate, apparently from this acid being withdrawn †. This inference, however, is completely at variance with the fact, that vegetables grow in air that has been entirely deprived of its carbonic acid (499.); and it is also contradicted by other experiments of a similar nature, conducted in the shade. For in the shade, says M. de Saussure, not only have the plants not died in recipients which contained lime, and were inverted in lime-water, but they have flourished better than in similar recipients which did not contain that substance. In ten days, a plant, confined with lime in the shade, acquired seven grains in weight; the volume of air lost four and a half cubic inches: it was become very impure, and contained $\frac{3}{100}$ of carbonic acid ‡. But in the recipient without lime, the plant acquired only five grains in weight, and the atmosphere contained $\frac{11}{100}$

* Annales de Chimie, tom. xxiv. p. 144. 145.

† Ibid. tom. xxiv. p. 145. ‡ Ib. t. xxiv. p. 146.

of acid gas *. Thus, then, we see, that lime, which is considered to destroy plants by abstracting the carbonic acid from their atmosphere in sunshine, is actually favourable to vegetation when that process is conducted in the shade. In what way, then, does lime, in these different experiments, affect the vegetation of plants, and from what cause does this remarkable difference in its action proceed?

502. It appears, that, in the experiments made in sunshine (500.), the carbonic acid was entirely attracted by the lime; but in the shade (501.), the atmosphere still contained $\frac{1}{100}$ of that gas. Did then the plants die in sunshine, because their atmosphere contained no carbonic acid? and did they live in the shade, because a small portion of carbonic acid was present in it? If the first of these positions were true, then vegetation should cease in air deprived of carbonic acid (499.), which is not the fact; and, contrarily, if the second were true, then plants should not grow in air deprived of this acid gas, which nevertheless they are found to do. Consequently, the presence of carbonic acid is not essential to the vegetation of plants, even in sunshine, neither is it injurious to those which grow in the shade; and since, therefore, neither the presence nor the absence of this gas materially affected the vegetative process in these experiments, we must seek out for some other cause of the contrariety in result which they afford.

503. Now this cause must be connected with some circumstances arising out of accidental situation; for

* Recherches Chim. p. 36.

no other difference, in the conduct of the experiments, is stated, than that the plants were respectively placed in sunshine and in the shade. The atmosphere of the plants in sunshine, at the time of their death, between the 5th and 6th day, contained $\frac{16}{100}$ of oxygen gas *, which is probably much more than existed in the vessel placed in the shade at the end of the 10th day; so that the cause of death, in the former case, could not have proceeded from the absolute deterioration of the air. Neither can we ascribe the death of the plants to the abstraction of moisture by the lime, as we had formerly supposed (43.); for it is not stated that the leaves had become dry, and their fall might take place from causes entirely independent of their desiccation (233.). Moreover, such a cause would have equally affected the plants which grew in the shade.

504. As, then, neither the state of the atmosphere, nor the condition of the plants as to moisture, seems sufficient to account for their death, may we not suppose that the lime itself exerted some deleterious action in sunshine, which it did not produce in the shade? This is rendered highly probable by the observations which M. Braconnot has made on these very experiments. He contends, that the death of the plants was not owing to the privation of carbonic acid alone, but to the action of the lime itself in a state of vapour; for he found, that if litmus-paper was suspended in a phial that contained moistened lime, it, in a short time, was changed to a blue colour,

* Recherches Chim. p. 35.

proving the volatilization of the lime *. We have seen, too, the deleterious effect which mercury, in a similar manner, produces (310.); and this mode of explaining the fact furnishes a reason why the death of the plants happened only in sunshine, since the lime, in that situation, was much more readily volatilized.

505. In the foregoing experiments, the roots of the plants were immersed in pure water, but, in subsequent trials, M. de Saussure operated on the branches of a tree, whose roots were growing in the earth. Into a glass balloon, of the capacity of 200 cubic inches, he introduced an ounce of lime, and, to remove all objection respecting its property of attracting moisture, he lightly moistened it with water. A branch of honeysuckle, or of the peach, furnished with leaves, was then passed into the balloon, and so disposed that its leaves should neither touch the lime, nor the sides of the vessel. The neck of the balloon was then carefully luted to the branch, and the apparatus was exposed to the sun. The leaves of the branch continued green twelve days, when they began to dry, and at the end of three weeks they had all fallen. The branch, however, did not die; but in a month after, while retained in the balloon, which still remained closed, it put forth new leaves. A similar branch, introduced into another balloon, and placed in the same circumstances, but without lime in the vessel, preserved its freshness for more than two months. The

* Nicholson's Journal, vol. xviii. p. 22.

lime, in the former experiment, was found to be saturated with carbonic acid; and since, in these experiments, the atmospheres still contained a considerable portion of oxygen, it is concluded, that the fall of the leaves was not owing to the want of that gas, but to the absence of carbonic acid alone *.

506. It may be remarked, that, in these last experiments, the effects produced on the plants are very different from those before related, as is also the time required for their completion; for, in the first experiments (500.), the leaves soon became yellow, and in a few days fell off; while in the second, they continued green, became dry, and did not fall entirely till the end of the third week. Now, it is very doubtful whether the mere abstraction of carbonic acid would occasion either the one or the other of these states of the leaves; and it is in no respect probable that it should produce them both. In fact, the results of these two series of experiments correspond with those which we have related (43.) on a former occasion; and although M. de Saussure states, that, in the latter series, he previously moistened the lime, yet he did it only slightly; and as evaporation from the leaves was soon checked by their confined situation, they could not have become dry, but from the attraction of their moisture by the lime. While, therefore, the effects on the plants were probably produced, in the first experiments, by the vaporization of the lime, it appears to us almost certain, that, in the last examples, they arose from the abstraction of

* Recherches, p. 38. 9.

moisture, both from the plant itself, and probably from the atmosphere in which it was confined.

507. But farther, according to M. de Saussure, plants in the shade grow better in vessels which contain lime, than in hose in which this substance is not present. The carbonic acid, which they then form, is, in great part, attracted; but we are not able, says he, to judge of the effect of a total privation of this acid, because it is formed in too great quantity to be entirely removed; the effect, however, of a partial privation, he adds, is to favour vegetation *. We have, however, given examples (44.) of the total privation of this acid gas from plants vegetating in the shade, without its producing any apparent change in their vegetative powers; and, on the contrary, we have related other experiments, in which the carbonic acid, formed in vegetation, was allowed to remain, without apparently producing any suspension of that process, except in so far as the oxygen gas of the air suffered diminution; so that, in the ordinary vegetation of plants in a given portion of atmospheric air, neither the presence nor the absence of the carbonic acid which they form seems materially to influence the process. This inference is still more certain with regard to plants which grow naturally in the open atmosphere, where the proportion of carbonic acid is always nearly uniform, and seldom or never exceeds 1000 *per cent*.

508. In certain circumstances, however, lime is so far from promoting vegetation, even in the shade, that it altogether arrests that process, as appears from the

* Recherches Chim. p. 37.

following experiments. A young bean plant, growing in a garden pot to the height of six inches, was inclosed by the tin apparatus, in the manner already described (226.); and by the side of the plant was placed an egg cup, which contained two drachms of freshly pounded lime. Over the plant and cup, a glass jar, containing about 38 cubic inches of atmospheric air, was then inverted, and the mouth of the jar was surrounded by lime-water; the temperature of the room was 61°, and the apparatus was set aside in a part of the room that received the full light of day, but was not exposed to the direct rays of the sun. During the first day, the jar continued perfectly dry, the plant was rather diminished in height, and the lime had considerably increased in bulk. By the end of the second day, the appearances were nearly the same, but the lime was now much more bulky; and the water had risen into the jar to the height nearly of half an inch. Through the two succeeding days, the lime continued to swell, the water to rise, and the jar remained dry; but the plant, though looking fresh and green, exhibited no sign of growth. At the end of the fifth day, the leaves became pale, and had a withered appearance, and the stem began to turn black. By the close of the following day, the lime was so much swollen as nearly to fill the egg cup; the stem of the plant was more black; the leaves were much paler and more withered, and had fallen two inches below the thread, which was at first tied round the jar to mark their greatest height; and, notwithstanding the water had, by this time, risen nearly an inch, the jar still continued quite dry.

509. In this experiment, therefore, we observe a gradual abstraction of moisture, both from the plant, and from the atmosphere in which it was confined, so as entirely to arrest the growth of the plant, and in a few days to destroy its life. During the whole experiment, the roots were abundantly supplied with water from the dish in which the pot of earth stood; but this was insufficient to counterbalance its attraction by the lime. The gradual rise of the lime-water into the jar indicated the formation and subsequent attraction of carbonic acid; but, nevertheless, the atmosphere had not undergone any change, except from the abstraction of moisture, which could occasion the death of the plant; for it was found, on analysis, to contain more than half its original quantity of oxygen. Hence the death of the plant must be referred to the abstraction of moisture partly from the plant itself, and partly, as we believe, from the atmosphere which surrounded it, by which the air was rendered incapable of supporting vegetation.

510. This conclusion will appear more evident from the result of another experiment made at the same time with a similar plant, and precisely under the same circumstances, except that the lime, instead of being used in the form of dry powder, was slaked, and reduced to the consistence of a thick fluid by the addition of water. In this case, the jar was rendered dim by moisture, and continued in that state through the whole experiment; the plant had grown half an inch in height by the end of the first day; and the water had risen into the jar about three-tenths of an inch. During the two following days, the plant continued

to grow, and the water to rise; but on the fourth day, two black spots appeared on one of the leaves, which gradually spread, and a fine mould began to form. At the end of the sixth day, two of the leaves had become black, and the rest, though green, looked sickly; the slaked lime had now become nearly dry, but the jar was still dimmed by moisture; the plant had grown in all about an inch in height. Hence, by the gradual rise of the water, it appears, that the lime, as in the former case, attracted the carbonic acid which the plant formed; but the dimness of the sides of the jar shewed the continued presence of moisture in the air. The chemical changes which the air had undergone were the same nearly in both cases; and, therefore, the difference in appearance which the plants exhibited can be ascribed only to a different state with regard to the moisture, both of the atmosphere and of the plant. Whether the sickly state of the vegetable, towards the close of the experiment, is to be ascribed to the deleterious action of the lime, or to the stagnation of its fluids, in consequence of its confined situation, we do not undertake to say; but there is no reason to think that the air had experienced any chemical change, sufficient to account for the decay and decomposition which had evidently begun to take place.

511. In order to prove, that, in his experiments, the lime did not affect the plants from any operation peculiar to it, but that of abstracting carbonic acid, M. de Saussure states, that he sometimes employed potassa; but as he has not given the details of any experiment made with this substance, we are unable

to judge of the conclusiveness of the methods which he employed. He only says, that lime or potassa, which destroys the thin leaves of plants which vegetate in sunshine, does not exert this action upon the vegetation of the *cactus opuntia*, and other plants which have thick leaves; because, says he, their thick parenchyme, and the little porosity of their epidermis, enable them to retain obstinately the carbonic acid which they have formed *. We must not, however, conclude, that the life of the cactus is preserved, in consequence of its thus retaining carbonic acid, but in consequence of its hardy constitution, which enables it to resist the deleterious operation of the substances to which it is exposed †.

512. Lastly, we may observe, that, even in sunshine, it is to the presence of oxygen gas (309.), and not to that of carbonic acid, that the vegetation of the plant is immediately to be ascribed; and the presence of this acid, in any considerable quantity, is actually injurious to vegetation (309.), unless it be decomposed by the agency of solar light. But by its decomposition, this acid does no more than afford oxygen; consequently, it is useful in vegetation, not as carbonic acid, but only as it affords oxygen gas. If, therefore, this gas be supplied from any other source, carbonic acid is not required to afford it; and hence vegetation goes on in atmospheric air, though it contain not more than $\frac{1}{1000}$ part of carbonic acid, and even, as M. de Saussure has shewn (499.), when that small portion is removed.

---

* Recherches Chim. p. 89. † Ibid. p. 64. Note.

In as far, however, as carbonic acid is decomposed by solar light, and thereby yields oxygen gas, in so far may it be considered useful to vegetation; but it is not, under any circumstances, necessary to that process, if oxygen gas be duly supplied from any other source.

---

513. Besides furnishing oxygen gas, by the decomposition of carbonic acid, many writers have supposed that the leaves of plants afforded it, also, by the decomposition of water. This opinion, however, seems rather to have been inferred from reasoning, than proved by experiment. Vegetables, it is said, augment their bulk, when furnished only with water and air; and since oxygen and hydrogen (the constituents of water) are likewise two of the most abundant ingredients in plants, it is inferred that water suffers decomposition, and is thereby enabled to contribute to the nutrition of the plant. Because, also, the leaves of plants contain inflammable matter, and, in certain circumstances, emit oxygen gas, M. Berthollet was led to suppose, that, when exposed to the sun, they possessed the power of decomposing water, as well as carbonic acid. In this manner, he believed that the oxygen was derived from two sources; while the hydrogen of the water and the carbon of the acid contributed to form the resinous or inflammable matter *. It is the evidence adduced

* Thomson's Syst. Chem. vol. v. p. 362. 4th edit.

in support of this supposed decomposition of water, and consequent production of oxygen, that we propose now to examine.

514. It has been said, that, when the leaves of plants are exposed to light, in contact with pure water only, they afford oxygen gas,—a proof that their vessels are able, when assisted by the action of light, to decompose a portion of the water which they absorb *. This oxygen, however, may proceed from so many other sources, that the quantity afforded cannot be received as proof that it is derived from the decomposition of water; and the small portion that is yielded by leaves, when thus placed in water, may, with greater probability, be ascribed to the air actually existing in the substance of the leaf before its immersion.

515. But an experiment of Mr Davy seems to afford the best evidence that has been adduced on this subject. He filled a glass cylinder, of the capacity of ten cubic inches, with mercury; and then conveyed into it two small vine leaves, which were passed through the mercury, so as to detach all atmospheric air from them. The mercurial apparatus was now inserted in a vessel of cold water; and aqueous gas, furnished from another vessel containing water, which had been long in ebullition, was passed through a long tube into the cylinder, where it was condensed by the cold mercury. In this manner, the cylinder was filled with water that held no air in solution, and, in this state, it was exposed to light. In a very

* Murray's Syst. Chem. vol. iv. p. 76.

short time, air globules began to form on the leaves, and in about six hours sufficient air was collected to be examined; it measured two cubic inches and a half, and was nearly pure oxygen gas. Since, in this experiment, no gas of any kind was held in solution by the water, and pure oxygen gas was produced, it must have arisen, says Mr Davy, from the decomposition of the water, by the combination of its oxygen with light, and of its hydrogen with the vegetable *.

516. But although it be granted that the water employed, and the surfaces of the leaves, were, in this experiment, freed from air, yet the aëriform fluid in their substance was not expelled. That such a fluid exists in leaves, has been clearly established; and, indeed, Mr Davy's own experiments shew, that, when leaves are confined either in nitrogen or in hydrogen gas, they afford oxygen gas in sunshine. Thus, a small plant of minianet, exposed to the sun in twenty-eight cubic inches of hydrogen gas, yielded two and a half cubic inches of oxygen; and in experiments made with leaves in nitrogen gas, six-tenths of a cubic inch of oxygen were obtained †. Mr Davy, indeed, supposed this oxygen to proceed from the decomposition of the water of the plant; but this is to take for granted the thing that requires to be proved; and the supposition cannot be received, until the source of fallacy just stated has been first disposed of.

517. If water were decomposed by plants, no good

* Beddoes's Contributions, p. 159. † Ibid. p. 154. 163.

reason occurs why its hydrogen should not be liberated, as well as its oxygen is supposed to be; for oxygen enters more abundantly into the composition of the vegetable than hydrogen does, and may, therefore, be supposed to possess an affinity equally strong for the other elements of the vegetable substance. Neither, if we admit the liberation of oxygen by these means, does it follow, that the mere absence of hydrogen is sufficient to authorise the conclusion that it has actually combined with the plant, and been converted into vegetable matter; for, as far as observation yet goes, organised bodies do not appear to be increased in bulk, by matter supplied in an elementary form, but in a state of combination; and the augmentation, which vegetables receive by the function of assimilation, seems to be very different from the mere aggregation of particles, which form an inanimate substance by the operation of chemical affinity, or by the laws of crystallization.

518. The subject of the decomposition of water in vegetation has particularly engaged the attention of M. de Saussure. He observes that plants, vegetating, by the aid of distilled water, in oxygen gas, or in common air deprived of its carbonic acid, augment their weight. This increase of weight, however, affords, says he, no proof of the decomposition of the water, nor of the fixation of its elements, since it may be owing to the mere introduction of that fluid into the vegetable; and, from many experiments, he satisfied himself, that, when plants are reduced to the same state of dryness before and after the experiment, this apparent augmentation of their weight is

little, if at all perceptible. He placed three plants of *lysimachia vulgaris*, weighing 129¼ grains, in a recipient containing 250 cubic inches of common air deprived of its carbonic acid, the roots of the plants being plunged in a little distilled water. The plants were then kept, alternately in sunshine and in the shade, for eight days, at the end of which time they were perfectly sound, and had grown considerably, but had not at all affected the purity or the volume of their atmosphere. In their green state, they now weighed 141 grains, and, after being dried in the common temperature of the atmosphere, their weight was only 40½ grains. Similar plants, precisely of the same weight, were, at the same time, gathered and dried in the same manner, and were found to weigh 38½ grains; so that those plants, which had grown eight days in confined air, exceeded them in weight, when both were reduced to the same state of dryness, only by two grains. These comparative experiments he repeated many times on different plants, and prolonged them to a fortnight or even a month; but he never found the weight of the confined plants to exceed that of others, which had not been confined, by more than two grains, and sometimes not at all, although they had, in some cases, grown several inches in length *.

519. To these experiments it may, perhaps, be objected, that the difficulty of reducing plants to a similar degree of dryness prevents us from arriving at any very accurate knowledge of their solid substance,

* Recherches, p. 221.

Though this, however, should be, in some degree, allowed, yet it must be granted, that the similarity of result, obtained in a great many trials, and the fact that no greater additional weight was acquired by the plants in a month than in eight days, although their bulk had then greatly increased, give great weight to the inference deduced from these experiments, since they shew that water, even if it be said to be decomposed, adds little or nothing to the solid vegetable substance.

520. But the great argument for the decomposition of water is drawn from the production of oxygen gas; and were it, therefore, to be said, that, in these plants, the two grains in weight were acquired by the addition of the hydrogen of the water, the plants, adds M. de Saussure, should have eliminated all the oxygen with which this quantity of hydrogen was combined, that is to say, at least 22 cubic inches, which should have been found in the recipient; whereas, it has been stated, that the atmosphere experienced no alteration whatever *. In the same manner, 19 cubic inches of oxygen should have been eliminated in an experiment made on some plants of *vinca minor*; but neither had their atmosphere undergone any sensible change †. If it be said, that the oxygen was converted into vegetable matter, as well as the hydrogen, then the argument for the decomposition of water, derived from the production of oxygen, necessarily falls to the ground; and as no evidence of this decomposition afterwards remains,

* Recherches Chim. p. 222. † Ibid. p. 224.

there is no just reason for supposing that it ever takes place.

521. As farther arguments against the decomposition of water, M. de Saussure observes, that he could never find in the atmosphere of confined plants, which grew many months in sunshine, any additional quantity of oxygen, which, however, might have been expected if they had directly decomposed water; but the air, in general, suffered no sensible change *. Whenever plants afforded oxygen gas in sunshine, it proceeded not from the decomposition of water, but of the carbonic acid contained in the substance of the leaf †; for when solution of potassa was placed in the vessel to attract the carbonic acid, the leaves then afforded no oxygen gas ‡. From these and other facts, M. de Saussure concludes, that living plants do not, in any case, directly decompose water, by assimilating its hydrogen, and expelling its oxygen in a gaseous form; nor do they ever afford oxygen gas, but by the immediate decomposition of carbonic acid §. This conclusion seems to be fully warranted by the facts above stated, and perfectly accords with the opinion delivered (317.) in a former section.

522. But though the preceding experiments (518.) afford no support to the opinion that water is decomposed by growing vegetables, or even combined and fixed in them during vegetation, yet M. de Saussure believes this fixation to be prevented by the deficien-

* Recherches Chim. p. 230. † Ibid. p. 233.
‡ Ibid. p. 235. § Ibid. p. 237.

cy of carbonic matter, deeming it probable that the quantity of oxygen and hydrogen in plants cannot be increased beyond certain limits, unless the carbon be supplied in proportion. To verify this idea, he refers to the experiments made on plants which grew in atmospheres of common air containing carbonic acid (310.), and in which that gas was decomposed, and oxygen gas expelled. He considers the carbon, which was previously united with this oxygen, to have been *assimilated* by the plant, and thereby to have augmented its vegetable substance. Thus the 21.75 cubic inches of carbonic acid, decomposed by the seven plants of *vinca minor*, afforded to the plants 4.2 grains of carbon. These plants weighed, in their green state, before the experiment, 168¾ grains, and contained 51 grains of dry vegetable matter; but, when reduced to dryness after the experiment, their weight amounted to 61 grains; so that, after thus decomposing the carbonic acid, their vegetable substance, it is said, was increased by ten grains. But of these ten grains, 4.2 only can be attributed to the carbon of the acid, and therefore the remaining weight, equal to 5.8 grains, must, says M. de Saussure, be ascribed to the fixation or solidification of water. Plants of *mentha aquatica*, in the same manner, augmented their dry vegetable substance by six grains, of which one half is ascribed to the assimilation of carbon, and the other half to the fixation of water *.

523. In opposition to this view of vegetable nutrition, we may observe, that etiolated plants, which

---

* Recherches Chim. p. 225.

decompose no carbonic acid, and therefore may be presumed to assimilate no carbon, contain, nevertheless, hydrogen, oxygen and carbon, as well as green plants in which this assimilated carbon is supposed to perform so important an office; so that, in such plants, at least, this supposed use of carbon, in promoting the fixation of water, is not necessary to vegetable nutrition and growth. Carbonic acid, also, seems to be decomposed in the cellular structure or parenchyme of the leaf, and not in its vascular system; and, for reasons already stated (468.), it appears to contribute rather to the augmentation of its resinous or colourable matter, than to its nutrition and growth. Admitting, therefore, that carbon is retained in the plant, there is no evidence of its *assimilation*, in the proper physiological sense of that term; for the resinous matter, which it contributes to form, is not a part of the vegetable structure, but a chemical product, which, according as light is either admitted or excluded, may, or may not, be formed or withdrawn, without injury to the organic structure of the plant.

524. In our apprehension of the term *assimilation*, it is that function or power, which living bodies possess, of converting and applying inorganic matter to their own support and increase, in a manner which has not yet been imitated by any mechanical or chemical means; and this function we believe to be performed, in the vascular system of plants, by a slow and successive series of changes, which the vegetable is able to carry on and accomplish, only while it possesses life. These changes, as far as regards the substances concerned, may be considered chemical; but,

in relation to the instruments by which they are performed, and the living power necessary to their execution, they do not fall to be considered as objects of that science. To conclude, that, because carbon is retained in the plant, it is at once assimilated and applied as food for its support, is to reduce the series of important changes, which terminates in vegetable nutrition, to one solitary chemical action, which we have seen to go on in circumstances and situations totally independent of the living powers and properties of the plant.

525. Neither because vegetables are chiefly composed of oxygen, hydrogen and carbon, and because water and carbonic acid are composed of the same elements, are we therefore entitled to suppose that these substances, by a chemical action upon each other, are, or can be, converted into vegetable matter, and disposed into that fine and varied structure which we call a plant. It is this structure itself which alone is capable of executing these changes; and the chemist can do no more than furnish the materials to carry them on, or, by varying the circumstances of the action, in some degree modify its result. The mere exertion of the affinities of bodies, be they what they may, can never compound an organic structure, whose distinctive character or essence depends, not on the nature of its substance or elements, but on the mode of their composition and arrangement, and on the properties which, as living matter, they acquire and possess. In physiology, therefore, it is necessary to consider, not merely the chemical nature of the bodies concerned, and of the combinations into which

they enter, but, likewise, the structure and properties of the instruments, by which these combinations are effected. It is, we believe, chiefly from inattention to these circumstances, that chemical hypotheses in physiology, though apparently consistent with chemical principles, have seldom received the approbation of those, who, from their knowledge of the structure and properties of living bodies, are, perhaps, best entitled to decide upon them; and, notwithstanding the confidence with which such hypotheses have been sometimes proposed, they have rarely extended beyond the confines of the laboratory in which they were formed.

526. Now that we have distinctly considered the effects which growing vegetables produce in the air, both when they are exposed to the sun, and when they are placed in the shade, it may, perhaps, be expected that we should add a few observations on the general question of the *purification* of the atmosphere by vegetation, to which so much importance has been attached. It appears, then, that oxygen gas is essential to vegetation; and that plants, at all times, both in sunshine and in the shade, convert it into carbonic acid. In sunshine, however, and probably, also, in some degree, in the shade, they possess the power of reconverting carbonic acid into oxygen gas; hence, therefore, the question, as to the absolute depravation or purification of the atmosphere by vegetation, must be decided by the difference in de-

gree in which one or other of these operations is found to prevail.

527. If, then, from what has now been stated, it appear, that living plants, at all times, and in all situations, require the presence of oxygen gas, and, during their growth, uniformly convert it into carbonic acid; and if it also appear, that the same plants, only at some times, and in some situations, decompose carbonic acid, and thereby afford oxygen gas to the atmosphere, it evidently follows, that, as the consumption of oxygen is uniform and general, it must exceed, in extent, its production, which is only occasional and partial. Add to this, that the plants which, during sunshine, are employed in producing oxygen, are, at the same time, engaged in consuming it; so that, even during that period, they may probably make no absolute addition of oxygen to the air; and, at other periods, they must, in common with other plants, directly deprave it. When, however, these two operations go on at the same time in sunshine, it is difficult to estimate the rate at which they respectively proceed, and, consequently, to determine the ratio which they bear to each other.

528. To the consumption of oxygen gas by plants which grow in the open atmosphere, no other limits appear to be placed than those which are afforded by the size of the plant, and the extent and vigour of its vegetative power; but the production of this gas by plants must depend on the supply of carbonic acid, and on the presence and efficiency of solar light. In consequence of the power which plants possess of simultaneously forming and decomposing (308.) car-

bonic acid in sunshine, we have seen, that, when confined for long periods in a given quantity of air, they produce in it little or no permanent change; but how far these two operations thus compensate each other in the open air, we possess not the means of deciding. To us, however, it appears, that, in the open atmosphere, vegetables can receive carbonic acid only, or chiefly, by means of their absorbed fluids; for the air that surrounds them, unlike that in closed vessels, scarcely contains any sensible portion of this acid, and, did it contain more, this gas would probably diffuse itself more readily through the atmosphere, than enter the substance of plants. In this view, therefore, although plants, in closed vessels, may maintain the atmosphere in a permanent state of purity, they may not be able to do so in the open air, from not receiving so abundant a supply of acid gas.

529. On the other hand, it appears, that carbonic acid is largely conveyed into plants, both in a state of simple solution, and of combination with alkaline matter; that these saline compounds are decomposed by light; and that the carbonic acid, at the same instant, is resolved into its constituent elements, so as to yield its oxygen to the air. These decompositions we have seen to be purely chemical, and to depend immediately on the presence and operation of light; hence their extent and rapidity will be bounded only by the supply of acid, and by the decomposing power which light is able to exert. Thus, in young and succulent plants, which possess abundance of saline matter, the green colour

rapidly forms, and carbonic acid is decomposed, so as to furnish oxygen to the atmosphere in a quantity that appears to exceed considerably that which the plant, by its vegetation, is, in the same time, able to consume. If, indeed, in certain circumstances, the decomposition of carbonic acid by plants did not exceed its formation, it is difficult to conceive how the composition of the air could be maintained unchanged in the experiments of M. de Saussure (305.); since the portion of time, in which the plants were exposed to the sun, was much less than that in which they were kept in the shade; and, even in sunshine, they were equally employed in forming carbonic acid, although, on many occasions, no trace of it could be discovered. They must, therefore, in the shorter period in which they were exposed to the sun, have decomposed not only all the acid which they had previously formed in the shade, but all that, also, which their vegetation produced while this decomposition was going on. These considerations, therefore, raise our view of the extent and importance of this decomposing power in plants; but, giving to them all the weight they deserve, we think, from the various facts and circumstances already stated, that growing vegetables deprave the atmosphere in a degree that greatly surpasses their power to ameliorate and improve it.

530. But if the operation of vegetables in purifying our atmosphere be, even under the most favourable circumstances, merely negative, and if, upon the whole, they must, like animals, be considered greatly to deprave it, where, it may be asked, are we to

look for those causes of purification, by the operation of which, the uniformity of composition in the atmosphere is, at all times, and in all situations, maintained? To this most interesting, but difficult question, no satisfactory answer can, we think, be returned, in the present state of chemical knowledge. The modes in which the atmosphere is depraved by the living functions of animals and vegetables, by combustion, and by various other processes, in which its oxygen is withdrawn and made to enter into new combinations, are pretty well known, and, to a certain extent, may be appreciated with tolerable accuracy; but the various means by which this oxygen is released from its combinations, in the diversified modes of decomposition which are perpetually taking place, have been much less regarded, and cannot, therefore, with equal accuracy, be traced. Until this department of chemistry attain to greater perfection, it is, therefore, impossible to present a tolerably accurate view of this subject. We may, however, be certain of the general fact, that, as oxygen is withdrawn from our atmosphere, in order to enter into new combinations, so it can again be restored to it only by decompositions which shall set it free; and these decompositions must be as numerous, and to an extent as great, as the combinations to which they succeed. To follow, however, this circle of actions through all its round, may demand the persevering industry of ages; and it is only when this shall be accomplished, that chemistry will have advanced our knowledge of the individual relations of our globe, in a degree corresponding with that to which physical astronomy has carried its general connections with the universe.

531. But there have been writers, who rested their views of the purification of the atmosphere by vegetation, not so much on observation and experiment, as on what they conceived to be its necessity in the general economy of nature; and, with more perhaps of piety than of prudence, and certainly with a "zeal not according to knowledge," they have represented the contrary doctrine as derogatory to the wisdom of Providence, and a calumny against Nature herself. It is indeed true, and it is among the most gratifying truths in the pursuit of science, that every real step which we make in the knowledge of nature, serves to illustrate the skill and wisdom with which all its parts are contrived to advance the general purposes of the whole; but of this whole, it should also be recollected, that we, "as yet, see but in part, and, as through a glass, darkly." Hence imperfect and erroneous views of the order of nature may be often taken, and false conclusions may be grounded on them; and if these conclusions be afterwards announced as examples of divine wisdom, and be allowed to borrow the authority of *final causes* for their support, the history of science abundantly testifies that the vainest conceits of fallible man may, in time, come to be worshipped as the wisest institutions of unerring nature. It behoves us, therefore, to employ no ordinary portion of delicacy and caution in pronouncing on the general plans and purposes of Providence, from the little and partial views of nature, which, at present, we are permitted to take, lest, in the effer-

vescence of our zeal, we degrade the wisdom we pretend to exalt, and pervert the designs of the goodness we profess to revere.

532. With respect, also, to the charge of calumniating nature, he, surely, who, by assiduous observation of the facts which she offers to his contemplation, seeks to discover the laws of their connection, and proposes his opinion of those laws as the simple result of his inquiries, may be regarded less as a calumniator, than he who supplies the imperfection and deficiency in his facts, by the suggestions of imagination, and confidently imposes upon nature laws and conditions, which she utterly disowns and disclaims. For ourselves, indeed, accustomed always to regard facts more than opinions, and to yield less deference to names than to things, we are little influenced by those speculations, which, in the language of Bacon, may be pronounced *anticipations*, rather than *interpretations*, of nature. Still less are we moved by considerations of the supposed consequences which others may attach to our opinions, but embrace the sublime admonition of an eminent and philosophical Professor,—" To follow TRUTH wheresoever she may lead us, and, in all our researches, to be afraid of nothing but ERROR."

ADDITIONS TO CHAP. III.

OF THE CHANGES INDUCED ON THE AIR BY THE RESPIRATION OF ZOOPHYTES, WORMS, MOLLUSCA, INSECTS, FISHES, AND REPTILES.

533. In our former chapter on the respiration of the lower animals, we followed nearly the classification of the Linnæan school; but the farther we have advanced in our inquiries, the more have we perceived its imperfections. When treating simply of the changes induced on the air by the living functions of animals, we did not deem it necessary to enter minutely into the peculiarities of the animal structure, since it was our object, at that time, rather to ascertain the general facts with respect to the air, than to describe the organs by which respiration is performed, or the effects which it produces in the system. In the prosecution of this last branch of our subject, we have been obliged to enter on a more minute survey of the animal system; and it became, therefore, an object of importance to adopt an arrangement that should lead us through the chief va-

tions; but has replaced the two genera, Cancer and Monoculus, among the apterous insects. It is the arrangement of this author which we propose to follow in the remaining part of this work, because it appears to us to be the most natural, precise, and comprehensive; and, being founded entirely on anatomical distinctions, it is best suited to display that progressive variation in the structure and functions of animals, which it will hereafter be our business to describe and explain.

535. Conformably to these views, we shall consider the lowest and most simple animals as forming a distinct class, under the appellation of Zoophytes. In many individuals of this class, the necessary concurrence of water and heat to the commencement of living action has been already exemplified (48. et seq.); and the same agents are essentially necessary to the development of the ova, from which other animals of this class proceed. Spallanzani removed the liquid from a number of the eggs of animalcules, so as to leave them perfectly dry, and they remained unchanged for ten days; but on putting them into their native liquid, they were soon hatched *. So, likewise, he found, that, although great cold did not destroy the living faculty, in many animalcular ova, yet it suspended their evolution, which was always promoted by a due degree of heat.

536. With respect to the operation of air, the same author has remarked, that he never found any animalcule to be produced in vegetable infusions

* Tracts on Anim. and Veg. p. 66.

which were placed *in vacuo*, though the reverse uniformly happened, when the receiver contained a certain quantity of air. If, however, infusions, containing animalcules, were placed under the exhausted receiver, the animalcules lived many days, but perished long before others of the same kind, which were kept in the open air *. It may be doubted, however, whether the vacuum was in these experiments rendered perfect; for Dr Hooke found, that, when a quantity of vinegar, replete with eels, was included in a small phial, and stopped very close, all the included animals in a very short time died, as if they had been stifled †. The specific change effected in the air, by the respiration of this class of animals, may be inferred from the experiments of Mr Davy, who found (52.) that zoophytes not only require the presence of air in the water in which they live, but act upon it like fishes, that is, convert its oxygen gas into carbonic acid.

537. In the class Vermes, which falls next to be noticed in the order of our present arrangement, the first experiments, which we have met with, were made by the celebrated Scheele. He kept some leeches in a closed phial, which was half filled with water, and half with air, till they died. He then examined the air. It had no peculiar smell, but it extinguished the flame of a candle. These animals he found would live two years in the same water, when it had a free communication with the air; but

* Tracts, &c. p. 42.

† Micrographia, p. 217.

they died in two days, in a bottle quite filled with water, and closely stopped; and the water, when examined, contained no oxygen gas *.

538. We have before detailed (55.) the accurate experiments of M. Vauquelin, on slugs and snails, which belong to the third or Mollusca class. He found that the oxygen gas of atmospheric air was completely changed into carbonic acid, without any alteration taking place in the whole bulk of air employed, or in the quality of its nitrogenous portion. Consequently, the bulk of acid gas produced must have exactly equalled that of oxygen gas lost, and no portion of oxygen could, therefore, have been retained in the animal system.

539. We have already endeavoured (56. et seq.) to point out the fallacies in Spallanzani's experiments, which seemed to militate against these conclusions. Of this writer's labours, three additional volumes, containing reports of several thousand experiments, have been since published by M. Senebier †. They do great credit to his industry, but, for accuracy and precision, they will not bear a comparison with those of the justly celebrated chemist, whom we before mentioned. In these experiments, the changes produced in the air by living animals, by animals recently dead, and by others under a state of putrefaction, form the chief subjects of investigation. In all cases, the oxygen gas of the surrounding air

* Treat. on Air and Fire, p. 167.

† Rapports de l'air avec les êtres organisés, tomes iii. à Geneve, 1807.

more or less disappeared, and carbonic acid was, in a greater or less degree, produced. Sometimes the consumption of oxygen was complete, and an equal bulk of carbonic acid was produced; at other times, the oxygen gas only partially disappeared, and the carbonic acid formed was in a much smaller proportion. Sometimes the nitrogen was in part consumed, at other times it remained unaltered, and in some instances its quantity was actually increased; yet no attempt is made to investigate the causes of this discordance, or to discover the sources whence such great variations could proceed. Equal credit seems to have been attached to the most imperfect and the most perfect trials; and the author appears, by the repetition of his experiments, to have simply added to their number without proportionally increasing their value. Of the labours of his predecessors and contemporaries he seems either to have been entirely ignorant, or to have maintained an injurious silence; and hence he is sometimes found to announce well-known facts, with all the surprise and importance of new discoveries. Lastly, the effects produced in the air by living, by dead, and even by putrefying animals, are considered by him to proceed from the same causes, and to be accomplished in a similar manner; and thus the results of decomposition are uniformly confounded with those which succeed to the exercise of living action.

540. From these various sources of inaccuracy, it is scarcely possible to deduce, from this author's experiments, any thing farther, with regard to respiration, than the general facts, that, in all animals, oxy-

gen gas is necessary to the continuance of living action; that this gas disappears during the exercise of the respiratory function; and that a portion of carbonic acid is, at the same time, produced: but the real nature of these changes in the air, the extent to which they proceed, and the mode in which they are accomplished, cannot be learned from the labours of Spallanzani. With all his zeal for experiment, and all his ingenuity, this philosopher, indeed, seems not clearly to have apprehended the true nature and objects of experimental science. The mere multiplication of experiments serves only to multiply error, unless the mind exercise a severe judgment over the observations of sense, and, by separating what is necessary from what is accidental, endeavour to trace the order and succession of events, and to ascertain the mode and degree in which they are finally accomplished. "Inductio, enim, quæ procedit per enumerationem simplicem," says the great Father of experimental science, "res puerilis est, et precario concludit; et periculo exponitur ab instantia contradictoria, et plerumque secundum pauciora quam par est, et ex his tantummodo quæ præsto sunt, pronunciat. At *inductio*, quæ ad inventionem et demonstrationem scientiarum et artium erit utilis, naturam separare debet, per rejectiones et exclusiones debitas; ac deinde post negativas tot quot sufficiunt, super affirmativas concludere *." Even when this separation and rejection shall be made, the facts thus acquired

* Nov. Organ. lib. i.

will still remain barren and unproductive, if not quickened into life by the pervading energy of the mind. This "commercium mentis et rerum," it was, indeed, the great aim of Lord Bacon's philosophy to promote. "For thoughts," says one of his earliest disciples, "cannot work upon nothing, no more than hands. He that would build a house, must provide materials; and, on the contrary, the materials will never become a house, unless, by certain rules, we join them all together. So, it is not simply the knowledge of many things, but a multifarious copulation of them in the mind, that becomes prolific of farther knowledge *."

541. Concerning the respiration of insects, we before remarked (51.), that the ancients observed these animals to die when their bodies were smeared over with oil, which effect Mr Ray rightly attributed to the "intercluding of the air." Mr Boyle, among the almost innumerable experiments which he made with the air-pump, found, that flies, bees, and butterflies died in a few minutes when the receiver was exhausted of its air. It was notable also, he adds, that though bees and flies will walk and fly a great while after their heads are cut off, or even one half of the body move several hours after being severed from the other, yet, upon the exsuction of the air, not only the motion of the body, but of the limbs, ceases, as if the presence of air were more necessary than that of their own heads †.

* Grew's Anat. of Plants, p. 8.

† Boyle's Works, 4to, vol. i. p. 79. 112.

542. In our former work, we ascribed the first discovery of the specific changes which insects induce on the air to M. Vauquelin; but we have since found, that it ought rather to have been given to Scheele. He shut up several flies in a phial, and in a few days they all died, but had not diminished the volume of air. Milk of lime, however, reduced the bulk of residual air one-fourth part, and the remaining air extinguished a taper. He likewise inclosed a bee in a bottle, containing 20 ounce measures of air. A small piece of lime was, at the same time, introduced into the bottle, and in its side, near the bottom, a small hole was made. The bottle was then immersed in water, and as the water passed in, a quantity of lime-water was formed, which attracted the carbonic acid produced by the insects. As by this attraction of acid the volume of air continued to diminish, the water also continued to enter through the hole, until, by the seventh day, it had risen to about one-fourth of the height of the bottle. If two bees were put into the bottle, the same quantity of air was changed into carbonic acid in half the time; and he found also, that caterpillars and butterflies produced similar effects in the air.

543. To prove that it was the oxygenous portion of the air that was thus changed into carbonic acid, he confined two large bees in the same bottle, when it was filled with oxygen gas; and then set it to stand in milk of lime. He observed every day the milk of lime to rise through the hole into the bottle; and by the eighth day, the vessel was nearly filled

with it, and the bees were dead *. No experiments can more clearly shew the necessity of oxygen gas to the life of insects; nor more completely demonstrate its almost entire conversion into carbonic acid.

544. The ova of insects, not less than the perfect animals, require the combined operation of water, heat, and air, to carry forward their evolution. Many of these ova are deposited in water, and are evolved only during their immersion in that fluid; and the influence of heat is strikingly exemplified by the fact, that it is only during the warmer part of the year that the young broods of insects appear. That air, also, is necessary, may be concluded from the experiments of Spallanzani, who placed the eggs both of terrestrial and aquatic insects under the exhausted receiver of an air-pump, but none of them were hatched, although every other condition, necessary to their development, was present †. He found, also, that the ova of various insects were evolved in large vessels, even when the vessels were hermetically sealed; but, in proportion as the size of the vessels was diminished, the progress of evolution was retarded, and when the volume of air was reduced to a few inches, no signs of evolution then took place ‡.

545. The same observations may be extended to the intermediate, or *larva* state of insects. Spallanzani ascertained that these larvæ soon died when confined *in vacuo*, or in close vessels of air, though

* On Air and Fire, p. 148. 155.

† Opuscules de Phys. t. i. p. 141. ‡ Ibid. t. ii. p. 251.

abundantly supplied with food *. He discovered also, that they soon perished in nitrogen and hydrogen gases; that oxygen gas was necessary to support their life, and was consumed by their respiration; and that a quantity of carbonic acid was produced. The cabbage and silk-worm caterpillars consumed, more or less completely, the oxygen gas of the air, and produced carbonic acid. The more vigorous and active they were, the more oxygen they consumed. In a high temperature, also, they consumed more than in a low one; and when the temperature fell to *zero*, they then produced no change whatever in the air employed †. He found, likewise, the larvæ of the dragon-fly, and of other aquatic insects, to require the presence of oxygen, in the water in which they lived, to consume it by the exercise of their living functions, and to form carbonic acid ‡.

546. If, farther, we follow the *larva* to its *chrysalis* or *pupa* state, it will be found still to require the presence of the same agents, to enable it to go through its series of successive changes. The influence of heat, in carrying forward these changes, is well illustrated by some experiments of M. de Reaumur. That celebrated naturalist placed the pupæ of many species of insects in a green house, and the butterflies appeared in the middle of winter, some in ten or twelve days, others in three weeks, and others at a later period, corresponding to the earlier or later periods at which they naturally appear in the sum-

* Tracts, &c. p. 227.

† Rapports de l'air, &c. t. i. p. 17. et seq.

‡ Ibid. p. 72.

mer months. All of them, however, changed much more rapidly, from the permanent continuance of the heat, than if they had been exposed, even in summer, to the variable temperature of the open air. The insects, thus prematurely produced, were perfect in all their parts, and the females laid their eggs as usual; so that, by this method, butterflies may be made capable of giving birth to two generations in the year, though in ordinary circumstances they afford but one. The same results were obtained by experiments made on the pupæ of various other insects.

547. M. de Reaumur, likewise, confined several pupæ in a hollow sphere of glass, and placed them, with the eggs, under a brood-hen, leaving an aperture in the glass, by which fresh air could enter. The next day, the interior sides of the glass were covered with vapour that issued from the pupæ, and had collected into small drops of water. These were removed, and did not appear afterwards, the greatest transpiration having occurred during the first day. At the end of four days, he saw, as he observes, the first butterfly that had, perhaps, ever been born under a hen, and the first of this species of pupa that had ever remained so short a time in that state; for others of the same species, in the open air, require from twelve to sixteen days to complete the same series of changes *.

548. On the other hand, by keeping the pupæ of insects in a vault, where the temperature was only 8 or 10° Reaum. above the freezing point, their e-

* Mem. pour l'hist. des Insectes, tom. ii. p. 8. et seq.

volution was retarded in the summer season, and, instead of being changed into butterflies in July, they still retained their pupa state in the month of November. In this manner, they were kept more than a year in their pupa state, and this mode of existence might probably be prolonged for many years, without affecting their power of reviviscence, especially if they were kept in an ice-house, where the temperature did not rise above *zero* *.

549. That air is necessary to the development of these animals, may be inferred from the experiments of the same author. In July, Reaumur confined the pupæ of butterflies and moths in glass tubes four or five inches long, which were hermetically sealed; and, in these circumstances, they remained in their original state for more than five months, without indicating any appearance of development †. But when Spallanzani kept the pupæ of silk-worms in larger tubes, which were likewise sealed, their evolution proceeded, and they attained their perfect insect state. Similar observations were made on the pupæ of flies and butterflies ‡; and we may therefore conclude, that fresh air is necessary to carry forward the change of the insect from its pupa to its fly state. By subsequent experiments, Spallanzani also found, that the oxygen gas of the air was consumed in the transformation of the pupa to the insect; that carbonic acid was produced, but that

* Mem. &c. tom. ii. p. 18.

† Mem. tom. ii. p. 6.

‡ Tracts, &c. p. 200. 248.

the nitrogenous portion of the air remained unaltered. In temperatures at or below zero, no change however was produced in the quality of the air *. We repeated these experiments on the pupæ of the common fly, and found, that, during their transformation, they consumed all the oxygen gas of the air in which they were confined, and produced an equal volume of carbonic acid.

---

550. It has been already shewn (63.), that Fishes require the presence of oxygen gas in the water in which they live, and convert it into carbonic acid. These facts have been confirmed by the experiments of M. Sylvestre †, and by those of Spallanzani. The latter philosopher ascertained, by experiment, that fishes consume oxygen gas and produce carbonic acid, when confined either in water or in air; that they lose their vivacity, but do not become lethargic under cold, and then consume much less oxygen; and that oxygen gas is changed into carbonic acid by their skins, as well as by their gills. Their bodies, also, act upon the air after death, and, like other animal substances, consume its oxygen under decomposition ‡.

551. We are indebted to M. M. Humboldt and Provençal, for experiments on the respiration of fishes, which possess still greater accuracy. Their

* Rapports, &c. tom. i. p. 44. 50.

† Cuvier's Leçons d'Anat. comp. tom. iv. p. 305.

‡ Rapports, &c, t. i. p. 186.

attention was first directed to ascertain the quantity and composition of the air that exists naturally in river water. For this purpose, they filled glass-balloons with given quantities of water, taken from the river Seine, and expelled the air from it by submitting it to ebullition. The air that came over was received in vessels filled with mercury, or with distilled water, recently boiled, that no foreign air might mix with that obtained from the water in the balloon. From the results of ten experiments, conducted in this manner, they found, that the water of the Seine contained rather less than $\frac{1}{30}$ of its volume of air. This air they farther found to be composed of about $\frac{31}{100}$ oxygen, with from six to eleven *per cent.* carbonic acid, and the remainder was nitrogen gas. The composition of the air, contained in river-water, they state to be as constant in its proportions as that of the atmosphere; for in experiments continued many months, in times of drought and during the melting of snow, the proportion of oxygen in the water of the Seine never varied more than from 0.309 to 0.314 *. This estimation of the quantity and composition of the air, contained in river-water, does not differ so much as might have been expected from that afforded by the water of springs; for, according to Mr Dalton, air expelled from common spring water, after losing from five to ten *per cent.* carbonic acid, consists of 38 *per cent.* of oxygen and 62 of nitrogen gas †.

552. Having thus determined the quantity and kind of air contained in a given volume of river-wa-

---

* Mem. de d'Arcueil, tom. ii. p. 367. et seq.

† Chem. Phil. part ii. p. 272.

ter, these chemists proceeded to ascertain the changes which it experienced by the respiration of fishes. With this view, they confined young fishes in bell-glasses of river-water, inverted over mercury; and suffered them to remain till their respiration became laborious. The animals were then withdrawn, and the water, in which they had respired, was transferred into the balloon, and its air expelled, by submitting it to ebullition, in the manner before stated. Seven tenches were, in this manner, confined in 4000 cubic centimeters, equal to 250.5 cubic inches of river-water, where they remained eight hours and a half. A portion of this water, equal to 2582 cubic centimeters, or 161.5 cubic inches, was then transferred from the glass-bell into the balloon, and its air expelled by heat. The air, thus obtained, measured 453 parts, at temperature 50° Fahrenheit. These 453 parts were then washed in lime water, by which they were reduced to 300, so that 153 parts of carbonic acid were thus removed. The residue was afterwards analysed by combustion with hydrogen, and by mixture with nitrous gas; and the mean of three analyses afforded 0.035 of oxygen; wherefore it is concluded, that the 453 parts of air, obtained from water which had been in contact with the respiratory organs of fishes, consisted of 10.5 oxygen, 289.5 nitrogen, and 153.0 carbonic acid gas. But by former experiments, it was found, that an equal volume of pure river-water afforded 524 parts of air, consisting of 155.9 oxygen, 347.1 nitrogen, and 21.0 carbonic acid; consequently, say these chemists, these seven tenches have *absorbed*, in eight hours, 145.4 of oxygen, and 57.6 of nitrogen gas; and they have produced, in the same time, 132 parts of carbonic acid *.

553. The fact of the disappearance of a portion of the air in this experiment we do not dispute; neither do we question the accuracy of the analysis of the air which was actually obtained; but to the notion that the air, which disappeared, was *absorbed* by the animal system, we cannot so readily accede. No reasons, either anatomical or chemical, are assigned in support of such an opinion; neither is any evidence offered of the existence of these gases in the animal system, nor of their subsequent expulsion from it. The mere loss of a part of the oxygen seems to have afforded sufficient proof that the whole of it had first been absorbed; and that the portion of it, not returned in the form of carbonic acid, was permanently retained in the system. We forbear to repeat the general arguments against an absorption, or attraction of air by the gills of fishes, which have been already stated (74.5), but shall notice a few sources of fallacy in these experiments, which may, perhaps, explain the manner in which a part of the air might disappear, without resorting to the improbable supposition of its absorption.

554. The means of ascertaining with absolute precision, the volumes of air before and after respiration, have always constituted the great difficulty in experiments on this function, even in terrestrial animals; but as the sources of fallacy have been successively detected and removed, the correspondence between the

---

* Mem. d'Arcueil, tom. ii. p. 376.

inspired and expired volumes of air has increased, so as, in most cases, to have reached, at length, an almost perfect equality. If, however, these difficulties exist, when we operate with air alone, how much more may they be expected to attend experiments on air, dissolved in water? In the experiments which have now been detailed, it may be said, that, if the same degree of ebullition were kept up, the same quantity of air might be expected to be obtained from the water, after the fishes had breathed in it, as before, provided this quantity really existed in it; but as, by the act of respiration, the composition of the air is changed, this circumstance may, perhaps, vary the quantity retained by the distilled water through which it is subsequently passed, and thus occasion a variation in the result. Farther, in the act of transferring the water from the glass-bell into the balloon, a considerable portion of the acid gas might escape; for, according to Scheele, the carbonic acid, formed by aquatic animals, must always separate from the water, since, as carbonic acid, it does not remain with water in the open air *;—a fact which is evinced by the small portion of carbonic acid contained in river-water, notwithstanding the great quantities that must be constantly forming in it both by the living functions of animals, and by the decomposition of dead organised matter.

555. That these, or other sources of fallacy, exist in these experiments, may be farther assumed from

---

* On Air and Fire, p. 165.

the great variations in the results which they afford. In a table, which presents the results of seven different experiments, conducted in a similar manner, the consumption of oxygen to the carbonic acid produced, is represented, in one instance, as bearing the proportion of 100 to 91; in another, the proportion is as 100 to 50; and in a third experiment, as 100 to 20 *; so that the same animals appear, at one time, to have consumed only one-tenth; at another time, one-half; and, at a third time, four-fifths of the oxygen employed;—contrarieties which sufficiently demonstrate the existence of some undiscovered sources of error. These experiments, therefore, can be considered as proving only the general fact of the conversion of oxygen gas into carbonic acid by the respiration of fishes; but they do not enable us to determine the actual extent to which this conversion proceeds. As, however, in the experiment that has been detailed (552.), nine-tenths of the oxygen, which disappeared, were obtained in the form of carbonic gas, it is, surely, more reasonable to suppose that the remaining one-tenth was converted into the same gas, than that it entered into, and was retained in the animal system. On this point we may be permitted to call in the aid of analogy; for as, in the respiration of every other class of animals, the bulk of acid produced is proved to be nearly or exactly equal to that of oxygen consumed, it may be fairly inferred that a similar equality obtains in the respiration of fishes, to which, indeed, the volumes of air in

* Mem. d'Arcueil, tom. ii. p. 378.

the foregoing experiment make a near approximation.

556. In like manner, from the same table, we learn, that, in some experiments, the loss of nitrogen amounted to about $\frac{1}{11}$; in other instances, to one-sixth; and in one case, it extended to about $\frac{1}{17}$ of that gas. This disappearance of nitrogen is, likewise, in every instance, ascribed to *absorption*; and the circumstance is pointed out as constituting an important difference between the respiration of fishes, and of the mammalia, in whom no absorption of nitrogen is conceived to take place. No other evidence, however, of this absorption of nitrogen is afforded, than the mere disappearance of a portion of that gas; and all the differences in the results are at once ascribed to variation in the exercise of this supposed absorbing power. In any investigation purely chemical, we are persuaded that similar discrepancies would have led to some attempts to discover their cause; but, in the application of chemistry to physiology, the most contradictory results obtain equal credit, and all the anomalies which may arise are at once charged, not to errors in experiment, but to some unknown operation of the animal system, which is called into existence for the mere purpose of carrying on some other operation, that is equally improbable and unknown.

557. As the air dissolved in water does not exceed $\frac{1}{30}$ of the volume of that liquid, and only $\frac{31}{100}$ of this air are pure oxygen, the relation of fishes to the oxygen contained in water corresponds with that of an animal breathing in a gaseous mixture, which

contains less than $\frac{1}{100}$ of that gas. This small quantity of oxygen gas in water might lead to the belief that it is not so necessary to the life of fishes as of other animals; but these animals soon suffer from the smallest suspension of their respiration; and this distress seems to arise more from the deficiency of oxygen, than from the presence of carbonic acid *. For when water is completely deprived of its oxygen, fishes die in it in a few minutes; but if oxygen be present, they live very well in water that contains more than one-eighth of its volume of carbonic acid †.

558. To maintain the respirability of the air dissolved in water, ample means are provided. Dr Priestley long since remarked, that boiled water decomposed air by attracting its oxygen ‡. And Scheele, also, observed, that water attracted oxygen, but not nitrogen ||. M. de Marty, however, observes, that nitrogen is attracted by water in limited quantity, and that it afterwards attracts oxygen, so as accurately to analyse atmospheric air. To saturate water completely with air, exposure to the atmosphere seems to be necessary; for M. Humboldt remarks, that water, which has been deprived of air, does not recover either the same volume of air, or the same proportions of oxygen and nitrogen under close vessels, as when it is exposed to the free atmosphere §. From

* Mem. d'Arcueil, t. ii. p. 379. † Ibid. p. 380.

‡ Phil. Trans. vol. lxii. p. 247.

|| On Air and Fire, p. 163.

§ Mem. d'Arcueil, tom. ii. p. 370.

this superior attractive power which water possesses for oxygen, the air dissolved in it is found to contain always a due proportion of that gas. But the experiments of Scheele (554.) also shew, that, as the oxygen is thus attracted, the carbonic acid is expelled, so that that gas never exceeds a certain quantity in the water, either of springs or of rivers. In like manner, Dr Priestley observes, that hydrogen, during its solution in water, seems to expel nitrogen, while nitrogen and oxygen are capable of existing together in that fluid *. By this difference in attractive power which water possesses for the different gases, connected with the expulsive force which they seem to exert towards each other, the noxious gases, formed in water by the exercise of the animal functions, and by the decomposition of organic bodies, are regularly expelled ; and thus the air, destined to support the living functions of aquatic animals, like that of the atmosphere which we breathe, is maintained nearly in an uniform state of composition and purity.

559. The last class of inferior animals, in the arrangement which we have now followed, is denominated Reptiles, in which are included all the animals that formed the Linnæan class of Amphibia. The experiments already given (60 et seq.), clearly prove, that frogs and toads, which belong to this class, entirely convert, by respiration, the oxygen gas of the air into nearly an equal bulk of carbonic acid, without producing any change in its nitrogenous portion.

* Obs. on Air, vol. i. p. 59.

Dr Carradori, also, discovered, that these animals lived much longer when they were immersed in water that had a free communication with the atmosphere, than when the air was excluded *. According to Spallanzani, frogs die sooner in boiled, than in common water. In their respiration, they consume oxygen, and form carbonic acid. Those which have been recently fed, consume more of this gas than those which have suffered a long abstinence. Under great cold, they become lethargic, but their heart still continues to beat, and they still, in a smaller degree, continue to change the air; but the consumption of oxygen increases with an increase of temperature. These animals also change the air by their skin, as well as by their lungs; and act upon it after death, and under putrefaction †. The ova of frogs were likewise found to require air to carry on their evolution. Small tadpoles, while yet attached to the egg, were confined in vessels half filled with water, while the other half contained common air, or oxygen, or nitrogen gas. Those in the two former vessels were perfectly developed, and became large enough to swim about; but those confined with nitrogen perished ‡.

560. Spallanzani extended his experiments to many other animals of this class, and obtained similar results. Different species of serpents he found to die in hydrogen gas, or when confined under water, but

* Phil. Magaz. vol. xvi. p. 245.

† Rapports, &c. tom. i. p. 468.

‡ Ibid. p. 466.

to live in common air, and convert its oxygenous portion into carbonic acid. They became lethargic from cold, and the heart then beat very slowly or not at all; the respiration was then also suspended, and little or no effect was produced in the air. The skin of these animals acted upon the air as well as the lungs *; and when their blood was reddened by exposure to the air, its oxygen also disappeared, and carbonic acid was produced †. Similar results were obtained in experiments on the respiration of vipers, tortoises, lizards, and salamanders ‡.

561. The preceding facts sufficiently shew, that various animals, in all the foregoing classes, and in every stage and form of their existence, require the presence of oxygen gas to maintain the functions of life; that this gas, by the exercise of these functions, is converted into carbonic acid; and that the degree in which this conversion proceeds, depends much on the healthy condition of the animal, and the vigour of its circulating system. Since, also, in every instance where the experiments have been made with the requisite accuracy, the bulk of carbonic acid produced, nearly or exactly equalled that of the oxygen which disappeared, we may conclude, from analogy, that such is universally the extent to which this change in the air takes place in animal respiration; and since, farther, the nitrogen gas of the air appears to suffer no necessary change (73.) in the exercise of this function, we may also conclude, that, as far as

* Rapports, &c. tom. i. p. 249. † Ibid. p. 239. 263.

‡ Ibid. p. 275. 287. 295. 353.

regards the air, the substitution of an equal bulk of carbonic acid for the oxygen gas that is lost, comprises the only essential change which the atmosphere experiences during the performance of this animal process. We have before maintained (74 et seq.), that the oxygen of the air does not enter the animal system, either by the living function of absorption, or by the operation of chemical affinity; and have consequently concluded, that the union of this substance with the animal carbon takes place exterior to the vessels of the living animal.

ADDITIONS TO CHAP. IV.

OF THE CHANGES INDUCED ON THE AIR BY THE RESPIRATION OF BIRDS, OF QUADRUPEDS, AND OF MAN.

562. WE before pointed out (79. et seq.) the general agency of water and heat in contributing to produce the phenomena of living action in the superior animals; and from the writings of Boyle and others, we adduced facts to shew the constant necessity of fresh air to maintain the functions of life. This necessity Mayhow first proved to arise from the consumption of what he called the nitro-aërial or pure part of the air in respiration, which wholesome part was shewn by Dr Black to be converted into carbonic acid. By his discovery of oxygen gas, Dr Priestley was enabled to demonstrate the specific portion of the air which was thus changed *; and M.

* It should be mentioned, in justice to the celebrated Scheele, that he, likewise, discovered oxygen gas independently of Dr Priestley, as, in the following paragraph, the Doctor himself, with

Lavoisier, by ascertaining the true composition of carbonic acid, enabled us to determine in what way the acid gas, obtained in respiration, might be formed. Such is the order in which the facts, relating to our knowledge of the properties and uses of the air in respiration, have been successively disclosed.

563. But we farther endeavoured to shew, that the actual quantity of carbonic acid, formed in respiration, corresponded very nearly (122.) with that of oxygen gas which had disappeared; and that, allowing for a small degree of *condensation*, which we then supposed to attend the conversion of these gases into each other, "the whole of the oxygen gas lost (123.), was employed to form the carbonic acid in question." This conclusion, we maintained, rested not only on

---

his usual candour, admits. "Having made this discovery", says he, "some time before I was in Paris in 1774, I mentioned it at the table of M. Lavoisier, when most of the philosophical people in the city were present, saying that it was a kind of air in which a candle burned much better than in common air; but I had not then given it any name. At this, all the company, and Mr and Madame Lavoisier as much as any, expressed great surprise. I told them that I had gotten it from *precipitate per se*, and also from red lead. Speaking French very imperfectly, and being little acquainted with the terms of chemistry, I said *plomb rouge*, which was not understood till M. Macquer said I must mean *minium*. Mr Scheele's discovery was certainly independent of mine, though, I believe, not made quite so early *." Yet, when speaking of oxygen gas, M. Lavoisier observes, "This species of air was discovered, almost at the same time, by Dr Priestley, Mr Scheele and myself †."

* Treat. on Phlogiston, p. 56. an. 1800.

† Elem. Chem. p. 84. 4th edit.

the most correct experiments which had then been made, but was supported by the whole range of analogical facts, presented in the vegetation of plants, and in the respiration of the inferior animals; and it is a conclusion of such great importance, in conducting us towards a true explanation of the respiratory function, that we shall not hesitate again to recite, very briefly, the evidence adduced in its support, together with such additional facts and arguments, as subsequent researches have enabled us to collect and bring forward.

564. Among his earliest experiments relating to respiration, Dr Priestley found, that if a mouse was confined in a jar of air, inverted in mercury, until he died, no diminution in the bulk of air took place, but the residual air lost nearly one-fifth of its bulk, when shaken with water *;—facts which entitle us to infer, that no portion of the air was absolutely lost, but that the whole of its oxygen was changed into an equal bulk of carbonic acid. Similar results, as we before remarked (83.), were obtained by Dr Crawford, who also observed no diminution to attend the respiration of air inverted over mercury, but the residual air afterwards lost one-fifth of its bulk by agitation with solution of potassa. Dr Menzies, in his experiments on respiration, found no diminution to occur in the volume of air respired; and therefore necessarily inferred that the bulk of acid formed was equal to that of oxygen gas which disappeared †.

* Phil. Trans. an. 1772.

† Dissert. on Respirat. p. 59.

565. In the year 1806 Mr Dalton's attention was directed to this subject, and he satisfied himself, by numerous experiments, that the bulk of carbonic acid, formed in respiration, was exactly equal to that of the oxygen gas consumed. On repeating these experiments, Dr Thomson obtained, in some cases, nearly the same results; but, upon the whole, the bulk of oxygen that disappeared was somewhat greater than that of the carbonic acid formed. The difference, however, varied considerably, and kept pace with the diminution in the whole bulk of air; whence he considers it to arise from the abstraction of a part of the air by some other way than by respiration; and if this be allowed for, he believes "the bulk of acid produced to be precisely equal to that of oxygen gas lost *. Hence, says he, this oxygen must be changed into carbonic acid in the lungs; for oxygen gas, when changed into carbonic acid, does not sensibly alter its bulk †.

566. These conclusions have been since confirmed by some very accurate experiments of Messrs Allen and Pepys. They caused a person to inspire, from a gasometer, 3460 cubic inches of atmospheric air, which were afterwards expired into another gasometer; and to both gasometers graduated scales were affixed, by which the quantities of air received and expelled could be accurately measured. The time occupied in the experiment was eleven minutes: about 58 respirations were made; and the deficiency in the whole volume of air, at the close of the expe-

* Syst. Chem., vol. v. p. 736. 3d edit. † Ibid. p. 774.

riment, amounted only to 23 cubic inches. One hundred parts of the respired air afforded, on analysis, 8.5 carbonic acid, 12.5 oxygen, and 79 nitrogen gas*. The experiment was repeated several times; and in one instance, 9890 cubic inches of air were breathed for 24½ minutes, with the loss of only 18 cubic inches, and 100 parts of the expired air then afforded, on analysis, 8 carbonic acid, 13 oxygen, and 79 nitrogen †.

567. Now the air employed in these experiments contained, in 100 parts, 21 oxygen and 79 nitrogen; and, in the numerous analyses which were made of this air after its respiration, the portion of oxygen that disappeared was exactly replaced by that of carbonic acid produced; so that, in every instance, these two gases formed together $\frac{21}{100}$ of the respired air, the remaining 79 parts being pure nitrogen gas. It is, therefore, concluded, " that the quantity of carbonic acid gas emitted is exactly equal, bulk for bulk, to the oxygen consumed ‡."

568. In subsequent experiments on the respiration of a Guinea pig, these chemists found, that when 310 cubic inches of atmospheric air were breathed for 25 minutes by this animal, its volume experienced no variation whatever, and the portion of its oxygen, which disappeared, was replaced by an equal bulk of carbonic acid §; wherefore, they justly conclude, " that when atmospheric air alone is respired, even

* Phil. Trans. 1808, p. 254.

† Ibid. p. 257. ‡ Ib. p. 279.

§ Ibid. 1809, p. 414.

by an animal subsisting wholly upon vegetables, no other change takes place in it than the substitution of a certain portion of carbonic acid gas for an equal volume of oxygen *."

569. It is, we conceive, impossible to refuse entire credit to the accuracy of these experiments, or to the legitimacy of the conclusions drawn from them, so far as relates to the chemical changes actually induced on the air; and these conclusions we have seen, also, to be supported by the experiments of many other able chemists, and by the results afforded in the exercise of the respiratory function in almost every other class of animals. We grant, however, that other chemists, equally able, whose experiments we have already detailed (122.) have concluded, that, because the carbonic acid obtained did not quite equal in bulk the oxygen lost, a portion of this latter gas was *absorbed* in the lungs. In some late experiments on the respiration of rabbits, and of Guinea pigs, M. Berthollet adopts this opinion. In these experiments, the bulk of acid gas produced did not quite equal that of oxygen which disappeared, so that the loss of oxygen appeared to vary from 1.07 to 4.09 *per cent* †. These two results, therefore, differ more from each other, than the former does from that of Allen and Pepys; and as this difference may be conceived to indicate error in that experiment which afforded the least acid, it may reasonably be inferred, that, had greater correctness been attained, no

* Phil. Trans. 1809, p. 427.

† Mem. d'Arcueil, t. ii. p. 461.

loss whatever would have appeared in that which yielded the most. In experiments, where so many causes concur to render the apparent bulk of acid less than it ought to be, and less than that of the oxygen lost, it is surely more reasonable to give greater credit to those results which indicate an equality of volume between these gases, than to those which declare a difference; since the former not only go with the latter to the fullest extent, but, pursuing the same track, have actually gone beyond them, and thereby reached a point, which the others have been unable to gain. In fact, to prefer those experiments, which indicate a difference, to those which prove an equality of volume, would be not only to halt in our progress, but to make a retrograde movement, and thus to suffer a negative inference to outweigh a positive proof.

570. Even Messrs Allen and Pepys, however, whose experiments so clearly demonstrate the equality of bulk in these two gases, in every case of natural respiration, have been led, by the results of other experiments, to suppose, that, " when respiration is attended with distressing circumstances, there is reason to conclude that a portion of oxygen is absorbed *. In the deduction of such an inference, regard seems to have been paid to mechanical and chemical considerations alone, and little or no attention given to the structure and properties of the living instruments by which respiration is performed. We before endeavoured to impress the necessity of at-

* Phil. Trans. 1808, p. 280.

tending to the natural actions of the respiratory organs (92. et seq.), in the conduct of all experiments made on the exercise of this function, by shewing, that, in all examples of natural respiration, little or no variation occurred in the volume of air employed, but that, in proportion as these organs suffered distress and oppression, the greatest irregularities prevailed.

571. These facts are supported, in all their circumstances, by the results obtained by Messrs Allen and Pepys. We have seen (568.), that, when a Guinea pig was made to breathe a given quantity of air in a natural manner, no variation whatever was observed in its bulk; and even in man, in whom many causes, which do not affect the lower animals, contribute to produce error, it appears, that, when the respiration was nearly natural, the general average of the deficiency, in the total amount of common air inspired, was only about six parts in 1000 (566.); and in one instance, it was considerably less than two. "The smallness of this deficiency," say these chemists, "surprised us very much; and, in our opinion, it principally arises from the difficulty, or," as they elsewhere say, "the imposibility of always bringing the lungs to the same state after forcible expiration *."

572. Notwithstanding the justice of this remark, they seem, however, entirely to have neglected its import in the inference which they have drawn from their fourteenth and fifteenth experiments. In the first of these experiments, 300 cubic inches of atmos-

* Phil. Trans. 1808, p. 253 254 258.

pheric air were, in the space of three minutes, passed eight or ten times through the lungs, until respiration became extremely laborious, and the operator was compelled to desist. On analysing the respired air, it was found to contain, in 100 parts, only 5.5 oxygen, 9.5 carbonic acid, and 85 parts of nitrogen gas. In the fifteenth experiment, which occupied, also, about three minutes, until the operator became quite insensible, the same quantity of air was employed, and afforded, by analysis, nearly the same results; for it contained, in 100 parts, four of oxygen, ten of carbonic acid, and 86 of nitrogen. In the former experiment, we observe, therefore, an increase of six parts of nitrogen, and a loss of six parts of oxygen; and in the latter, the oxygen had lost 7 from 21, and the nitrogen had gained 7 upon 29 *; and hence it is inferred, that "when," as in these experiments, "respiration is attended with distressing circumstances, there is reason to conclude, that a portion of oxygen is absorbed †."

573. To this inference, as far as it regards what is here called an *absorption* of oxygen, we must beg leave to object. That the united volumes of oxygen and carbonic acid expired, were less than the total volume of oxygen inspired, we readily grant; but we deny that this fact affords any adequate proof of an absorption of this latter gas. To the chemist, indeed, the mere fact of the disappearance of a portion of oxygen may supply sufficient evidence of its absorption, in the sense in which he may choose to em-

* Phil. Trans. 1808, p. 260. † Ibid. p. 280.

ploy that term ; but the physiologist farther requires to know, by what organs or vessels it is removed, in what course it is conveyed, and what uses it is destined to serve. On none of these points, however, does he gain any information ; and all the anatomical knowledge which he possesses of the structure of the lungs, and of the properties of the living absorbent system, is adverse to such a doctrine. Should he apply to the chemist for a solution of his difficulties, he is told that oxygen does not chemically combine with other bodies, unless it be brought into actual contact with them ; and he knows, that, in the present case, this contact is impossible, because the membranes, both of the air-cells and blood-vessels, are interposed between the air and the blood in the lungs. Even if, contrary to all experience and analogy, he were to concede to the chemist the existence of pores or other passages in the cells and blood-vessels, through which this oxygen might be attracted and combine with the blood, he is equally embarrassed to discover the reason or mode in which it is again so speedily expelled, or what useful purpose it can serve, since no portion of it is permanently retained. The science of chemistry furnishes no example of similar operations,—of fluids which attract gases and combine them, so as to reduce their elasticity, and then, without any apparent change of condition or circumstances, almost instantly discharge them in a new and elastic form.

574. If, farther, we compare the results of the

two series of experiments made by Messrs Allen and Pepys, the difficulties, in a physiological point of view, greatly accumulate upon us. For, if an *absorption* of oxygen really take place in the lungs, how does it happen, that, in the first thirteen experiments, made with several 1000 cubic inches of air, and which occupied from ten to twenty-four minutes of time, a very small loss in the whole bulk of air, and not the smallest in its proportion of oxygen, occurred; while, in two other experiments, made with only 300 inches of air, and continued only for three minutes of time, a great deficiency in the whole bulk of air, and a loss of one-third of its oxygen, took place. In all these experiments, except the twelfth, in which, instead of loss, there was actually an increase of eleven cubic inches upon the bulk of air respired *, the same person appears to have breathed, and the air was of similar composition. Consequently, the cause of variation in the result is to be sought, not in any difference in the animal organs, or in the original composition of the air, but, probably, in some circumstances of dissimilarity, which accompanied the progress of the experiment.

575. Now the bare statement of facts points out a great dissimilarity, not only in the chemical results, but in the circumstances accompanying the experiments, and in the effects which they produced in the system. For in the first thirteen experiments, which occupied from ten to twenty-four minutes, and in which no loss

---

* Phil. Trans. 1808, p. 256.

of oxygen occurred, the air was only *once* passed through the lungs, the breathing was nearly natural, the operator scarcely fatigued, and his pulse not raised more than about one beat in a minute *. But in the two experiments, in which oxygen is said to be absorbed, the same air was passed *eight or ten times* through the lungs; and, in less than a minute, the operator found himself obliged to take deeper and deeper inspirations. At last, the efforts to take in air became very strong and sudden, with a great sense of oppression and suffocation in the chest, indistinct vision, buz in the ears, loss of recollection, and, at the end of three minutes, perfect insensibility †. This difference in the effects produced in the system, we do not hesitate to ascribe to a difference in the *composition* of the air (91. 92.), which, in the first experiments, was respired in a natural state, but, in the two last, by repeated breathing, was rendered more and more unfit to carry on respiration, until, at length, its power of supporting that function altogether ceased.

576. But because under circumstances, in which the mental and animal powers were in complete abeyance, the respiratory organs were not able to make so complete an expulsion of the inspired air as they effect in their natural state of health and vigour, are we, therefore, entitled at once to conclude, that all the air which was not expelled was really *absorbed?* Setting aside the anatomical difficulties in the case, let us, for a moment, look only to the chemical conse-

* Phil. Trans. 1808, p. 253. † Ibid. p. 260. 262.

quences, to which such a conclusion would conduct us. If the mere disappearance of any gas, received into the lungs, be sufficient evidence of its absorption, then every gas, which is not returned, must be held to be absorbed. Are we then prepared to admit that hydrogen and nitrogen gases are absorbed by the blood? for, when their respiration is carried to its full extent, they too equally disappear. This supposed absorption, however, cannot proceed from the operation of chemical attraction, for little or no affinity (98. 104.) subsists between these gases and the blood. Neither can it arise from the operation of the living system; for it occurs only when the living powers are about to cease. To us there appears but one way of escaping from these manifold difficulties, which is simply to conclude, that the inspired air, which is not returned, is retained in the cells of the lungs. Such a supposition dissipates at once all anatomical and chemical difficulties, and explains why no air disappears in natural respiration, when the expiratory powers are in full vigour and able to expel it, and why its disappearance increases in proportion as the actions of these powers decline and cease.

577. It is, however, worthy of remark, that, in these last experiments, not only was there a diminution in the whole bulk of air, but its relative proportions likewise varied; for, in 100 parts, the oxygen and carbonic acid amounted together only to about two-thirds of the usual quantity of oxygen, and the deficiency was supplied by a superabundance of nitrogen gas. We are not prepared to say why, in this very embarrassed state of the respiratory function,

the relative proportions of the expired air should thus vary; but the fact proves only the retention of oxygen in the lungs, but not its absorption by the blood. Should it even be maintained that oxygen was absorbed, because, in these two experiments, a portion of it disappeared, then, by the same mode of reasoning, we must also contend, that, in the thirteen preceding experiments, no absorption of oxygen took place, because no part of it was retained; and as these last experiments alone come near to the natural exercise of this function, they authorise us to conclude, that such supposed absorption of oxygen constitutes no necessary part of healthy respiration. In truth, in some instances where a mixture of oxygen and hydrogen gases was respired, the oxygen and carbonic acid in the expired air uniformly exceeded, by one *per cent.* the total oxygen inspired *; from which it may be inferred, that these variations in the proportions of the expired air proceed entirely from accidental causes, and are totally independent of any absorbent function in the lungs.

---

578. But, beside the argument derived from the supposed loss of oxygen in respiration, analogies and experiments of a different nature have been brought forward in support of the doctrine that oxygen combines with the blood. It is known that the blood becomes red, when it is placed in contact with oxy-

---

* Phil. Trans. 1809, p. 425.

gen gas; and may not the base of this gas produce this effect by combining with the blood? Mercury, lead, and iron, says M. Lavoisier, become red when they combine with oxygen, and since the blood is rendered red by pure air, may not its redness proceed from a similar combination *? By very decisive experiments, M. Lavoisier shewed, that the base of oxygen gas combined with metals, during their conversion to the state of oxides; but we have suggested doubts (427.), whether the red colour, even of metallic oxides, depends on any colouring property in oxygen; and were this, for the present, conceded, it would not serve the case before us, since we possess no direct evidence that oxygen combines with the blood. It is, therefore, incumbent upon those who believe the blood to be rendered red in consequence of its oxygenation, to afford some palpable evidence that the base of oxygen gas really combines with that fluid, in a manner similar to that in which it unites with metals.

579. It does, indeed, appear to us somewhat remarkable, that M. Lavoisier, whose accurate and comprehensive views respecting the combinations of oxygen, enabled him to introduce such great improvements into the general doctrines of chemistry, and who first clearly remarked (118.) the difference between the effects produced in the air by combustion and by respiration, should still, apparently from the mere circumstance of similarity of colour, have concluded that respiration was nothing but a simple

* Mem. de l'Acad. des Sciences, an. 1777, p. 192.

combustion, and its result an oxidation of the blood. To establish his theory of oxidation, M. Lavoisier trusted not to colour alone, but actually proved the combination and disengagement of oxygen, as the metal was alternately reduced or revived. In the process of oxidation, he remarked that the air lost one-sixth of its bulk, and contained no carbonic acid; in that of respiration, the loss of bulk was only $\frac{1}{80}$, and a quantity of carbonic acid was produced. He even found that caustic alkali abstracted one-sixth of its bulk from air that had been respired, and therefore concluded that such air contained one-sixth of carbonic acid; and after this acid gas was abstracted, the residue exactly resembled that left by the calcination of metals *.

580. Thus, then, it appears, that although, in these experiments, M. Lavoisier distinctly ascertained, that, in oxidation, all the oxygen combined with the metal, and no carbonic acid was formed; and, on the contrary, that, in respiration, all the oxygen was converted into carbonic acid, and none therefore could combine with the blood; yet he, nevertheless, supposed this oxygen to unite with the blood, and to produce effects in it similar to those which followed the actual combination of this element with metallic matter. M. Lavoisier and his associates exclaimed loudly against their predecessors for employing phlogiston as a principle, which, like another Proteus, was able to exhibit every colour, and to assume every shape †; but we may safely assert, that there is scarcely any

---

* Mem. de l'Acad, 1777, p. 185. † Ib. an. 1789, p. 568.

colour or shape in nature, which, in their turn, these philosophers have not made oxygen to exhibit and assume. And if it was absurd in the Phlogistians to ascribe effects to an agent which they could not prove any where to exist, it is, surely, not less absurd in their successors to attribute similar effects to oxygen, even where it exists not.

581. It is, farther, an objection to this supposed operation of oxygen, that, in the lungs, the blood and air do not come into contact, and, therefore, although the combination of oxygen with that fluid might be conceived to happen when they are placed together out of the body, yet the intervention of organised membranes may be supposed to prevent such an union in the living system. In the ordinary operations of chemistry, such an interposition of animal substance would be considered sufficient to vitiate the result of any similar experiment in which it was employed; but in the application of this science to the living body, neither membranes nor blood-vessels are conceived to oppose any obstacle to the exertion of chemical action, or, in the smallest degree, to affect its result. In support of this supposed operation of oxygen on the blood, some experiments of Dr Priestley have been appealed to, as affording decisive evidence that this substance has the power of penetrating a compact membranous body, and may, consequently, penetrate the cells and blood-vessels of the lungs. The importance which has been attached to these experiments, in all the late hypotheses which have been proposed to explain the function of respiration, renders it necessary for us to examine them

with some minuteness, in order to discover the true relation which they bear to the present question.

582. Dr Priestley, who, as we shall hereafter see, supposed that venal blood became red by imparting its phlogiston to the air, knew well that the blood in the lungs was separated from the air by a membranous substance, which, however, according to Dr Hales, does not, in thickness, exceed the $\frac{1}{1000}$ part of an inch. To ascertain the effect of this circumstance, he put some black blood into a bladder moistened with a little serum, and then tying the bladder very close, he hung it in a free exposure to the air. The next day, all the lower surface of the blood, which had been separated from the air by the intervention of the bladder, had acquired a coating of a florid red colour, as thick, it appeared, as it would have acquired, if it had been immediately exposed to the open air; so that this membrane had been no impediment to the action of the air on the blood. This experiment was repeated, without previously moistening the bladder, and with the very same result *.

583. But although, in these experiments, the blood was rendered red by the agency of the air, yet we are not entitled to conclude, that this redness was produced by the combination of its oxygen, unless we can shew not only that this substance comes into contact with the blood, but is likewise capable of changing it to a red colour. Dr Priestley himself, who believed the blood to become red by the loss of phlogiston, could draw

* Obs. on Air, abridged, vol. iii. p. 369.

no such conclusion; and it is not a little remarkable that this philosopher, who had before so well observed the reciprocal effects produced in the air, when it thus changed the colour of the blood *, should, in these experiments, have entirely overlooked them. It is still more remarkable, since these experiments have drawn so much attention, and seem now to be the chief or only remaining evidence urged in support of the hypothesis of oxygenation, that some attempt has not been made to inquire farther into the actual circumstances which attend them. It is this examination which we now propose to make, in the hope, that, if it do not lead us to a knowledge of the true cause of this phenomenon, it may at least serve to shew to what it is not to be ascribed.

584. With this view, we procured a quantity of black blood, and putting it into a sheep's bladder, suspended it from the top of a jar containing about 100 cubic inches of atmospheric air. The jar was inverted in a saucer containing mercury, and within it a small cup of solution of potassa was likewise placed. The blood, in a short time, assumed a florid hue, and a dimness extended over the inside of the jar. By the next day, the mercury in the saucer had risen $\frac{2}{10}$ of an inch into the jar, and it continued to rise several days; so that, by the fifth day, it had reached nearly to an inch in height. The jar was then raised, and diluted acid being poured upon the alkaline solution, disengaged from it a large quantity of carbonic acid gas. By this experiment, there-

* Obs. on Air, vol. iii, p. 336.

fore, we are taught, that, when black blood assumes a red colour by being thus placed in a moistened bladder, and exposed to atmospheric air, the air itself, at the same time, undergoes a change; for its volume is diminished, and carbonic acid is produced.

585. To ascertain these facts with greater precision, we put another quantity of black blood into a small bladder, and suspended it, as before, from the top of a small jar inverted in mercury, and which contained 18.3 cubic inches of atmospheric air. Under this jar, also, a small cup of solution of potassa was placed. The blood, as before, was soon reddened, and the jar became dim. In two days, the mercury had risen nearly half an inch into the jar, and, by the close of the fourth day, it stood seven-eighths of an inch high, where it remained for some time quite stationary. On analysing the residual air, it was found to suffer no change, either from agitation with lime water, or by being exposed to the contact of phosphorus; so that, though all the oxygen had disappeared, no carbonic acid was present, but that gas was entirely attracted by the water of potassa employed.

586. The capacity of the jar, in the above experiment, has been stated to be equal to 18.3 cubic inches; and the bladder, with its contents, together with the cup and solution, we found to occupy a space equal to 5.2, which reduces the actual bulk of air, employed in the experiment, to 13.1 cubic inches. The mercury which, in consequence of the attraction of the carbonic acid, had risen seven-eighths of an inch into the jar, occupied a space equal to

three cubic inches; so that, of the 13.1 inches of air originally employed, three had disappeared, and $\frac{3}{13.1} = \frac{1}{4.37}$ or a portion of the air was thus converted into carbonic acid, which comes very near to the proportion of oxygen gas which the atmosphere is known to contain. Hence we infer, that, in this experiment, all the oxygen gas that disappeared was converted into carbonic acid; and, consequently, we deny that any oxygen penetrated the bladder, in order to combine with the blood.

587. As thus it is denied that the blood, in these experiments, received any ponderable matter from the air, so likewise it will appear, from the facts which follow, that the air receives no such matter from the blood. We filled bladders with water, and suspended them in jars of atmospheric air, in the manner described above; and found that the oxygen gas of this air was converted into carbonic acid, in the same manner as when the bladders were filled with blood; and if the experiment was continued a sufficient length of time, the whole of the oxygen gas was, in like manner, made to disappear. The same effects followed from the introduction of moistened empty bladders; and, indeed, it is the usual effect produced in the air by every moistened animal substance. If, therefore, the moistened bladder be thus capable, by itself, of acting on the air, we are entitled to conclude that it exerts the same action when it is filled with blood; and as, on this supposition, the oxygen gas will unite with the carbon, furnished directly by the bladder, we have no ground whatever to suppose this carbon to come from

the blood. Hence, therefore, when black blood is reddened by the air, through the coats of a moistened bladder, the air yields no oxygen to the blood, nor acquires from it any carbon; but the carbon of the bladder, by its combination with the oxygen of the air, passes into the state of carbonic acid gas.

588. Even Dr Priestley, who neglected to observe the effects produced in atmospheric air, by the contact of bladders, was well aware of the action they exerted on nitrous gas, which we now know to be composed of the same elements as common air. He observed an incrustation of lime to appear, when nitrous gas, which had been previously kept in a bladder, was mixed with lime water; or, when this gas was transferred from one vessel to another through a bladder, it produced a similar effect. To prove that this effect arose from the bladder, he put a bladder into a jar of nitrous gas, and, after it had continued there twenty-four hours, he transferred the gas into a glass-vessel of lime-water, which it rendered turbid *. In other experiments, when nitrous gas was left in moistened bladders, it completely lost its property of diminishing common air; but if the bladder was kept quite dry, the gas then underwent little or no change †. He even found that nitrous gas was, in like manner, deprived of its oxygen, by being placed in contact with blood, by which it was rendered unfit for eudiometrical purposes ‡.

589. The remarks, which have now been made,

* Obs. on Air, vol. i. p. 213.

† Obs. on Air abridged, vol. iii. p. 389. ‡ Ibid. p. 367.

on the mode in which moistened bladders act upon the air, may be extended to the action of all other animal membranes. In an experiment made by Mr Hunter, black blood was reddened by the air, when covered by goldbeater's leaf, which touched its surface *. But this leaf is an animal substance, and when, therefore, it was moistened by coming into contact with the blood, it acted on the air, like the bladder mentioned above, and to this action the red colour of that fluid succeeded. Dr Goodwyn, also, laid bare the jugular veins of rabbits, and, having intercepted the blood by ligatures, directed upon the coats of the veins a stream of oxygen gas. The blood, in some instances, became a little florid ; but, in other cases, no change of colour ensued, although the stream of oxygen was kept up for two minutes. As, however, the change occurred in some instances, *something*, says Dr Goodwyn, must have pervaded the vessels, and this, probably, by the force of chemical attraction ; but what that is, he adds, is not yet known, whether it be some principle escaping from the blood to form fixed air, or whether it be a part of the oxygen itself that enters into the blood. It is certain, however, that the change of colour which the blood undergoes, is occasioned by the chemical action of the air †.

590. In these experiments of Dr Goodwyn, there can be little doubt but that the oxygen gas of the air was more or less changed into carbonic acid;

* Treatise on the Blood, p. 62.

† Connex. of Life with Respiration, p. 63.

and the variation in appearance, which the blood exhibited, would correspond with the degree in which that change took place. An exhalation of fluid is constantly going on from all the moistened surfaces of the body, and may easily be conceived to have issued in a greater or less degree, from the recently exposed veins of the rabbits. According, therefore, to the activity with which this process proceeded, the colour of the blood would be more or less rapidly changed. In the same manner, Dr Barclay observes, that the blood in the lungs, when exposed to the air through the medium of its vessels, is always observed to change its colour a great deal faster, when the exhalents continue to act with a vital energy, than when they act slowly and feebly, as inanimate organs *; and we have elsewhere remarked (143.), that, when the exhalent function has entirely ceased, no change is induced on the air, and no apparent change seems then to be effected in the blood.

591. It is not, however, necessary that the carbonic matter, which combines with oxygen, should be afforded by a living action, else carbonic acid could not be formed by dead moistened bladders, nor by the blood after its removal from the body. In substances deprived of life, therefore, the carbon may rather be said to escape by evaporation, than by exhalation, which, physiologically speaking, is a living action; but in whichever way this matter come into contact with oxygen gas, carbonic acid is form-

* On Muscular Motion, p. 524.

ed, and the consequent effects on the blood, whether it be present in the body, or be withdrawn from it, will take place. In every case, however, a due degree of moisture and heat must be present to favour this chemical union; hence dry bladders produce no change in the air, and it remains equally unchanged by moistened bladders, if the necessary degree of heat be withdrawn.

592. That this change in the colour of blood is always accompanied by a corresponding change in the air, may be farther inferred from other experiments of Dr Priestley. He found, that, when the black crassamentum of blood was covered by serum or milk, it nevertheless acquired a florid hue, on being exposed to the air *; and Dr Wells observed, that a covering of albumen, also, did not prevent the action of the air on the blood †. Now we have already seen, that serum (97.) and albumen (153.) convert the oxygen gas of the air into carbonic acid; and we found by experiment, that the same effect was produced by milk, as probably would be the case with most of the animal fluids. Hence, it is evident, that, when the blood, in the experiments of Dr Priestley, became florid, through several inches of serum, the oxygen gas must have been at once changed by it into carbonic acid, and could never therefore, in the form of oxygen, be conveyed through this fluid to act on the blood.

593. On the other hand, Dr Priestley found a thin

* Obs. on Air abridged, vol. iii. p. 370.

† Phil. Trans. 1797.

stratum of water to prevent entirely this action of air on the blood *. M. Cigna found the same thing to take place, when a pellicle of oil was interposed †; and Dr Wells ascribes a similar effect to a solution of gum arabic. These substances, however, act little, if at all, in changing the air; and no change of colour, therefore, takes place in the blood. That black blood should have the power of attracting the oxygen of the air, through several inches of serum, and yet lose this power when a thin stratum of water is interposed, seems somewhat surprising, if the intervening fluid be, in each case, considered to be equally passive; but proceeding on the fact, that the serum exerts an action on the air, which the water is incapable of effecting, a new circumstance comes into view, and upon it the colouration of the blood may probably depend.

594. If, then, it appear, that the interposition of substances between the blood and the air necessarily prevents that contact which is essential to the chemical union of oxygen with that fluid; if it also appear, that the colour of the blood is never, in such cases, changed, unless such substances be interposed, as are themselves capable of acting on the air; and if, lastly, it be proved, that when the blood exhibits this change of colour, the air also suffers change, and that its oxygen, instead of combining with the blood, is really contained in the carbonic acid that is formed, we must conclude, that, whatever be the

* Obs. on Air, abridged, vol. iii. p. 370.

† Ibid.

mode in which the air contributes to change the colour of the blood, it cannot be by imparting to it any portion of its ponderable matter. Consequently, although these facts prove that oxygen gas possesses the power of changing the colour of the blood, as well through dead, as through living, animal membranes, yet they afford no evidence of the combination of oxygen with that fluid, but shew only the conversion of that gas into carbonic acid, precisely in the same manner as this acid gas is formed when the blood is reddened in the ordinary process of respiration.

595. Even when the air and blood are brought into contact, they only exert a reciprocal action on each other, by which carbonic acid is formed, but no oxygen appears to combine with the blood. We have already given various proofs (97.), that, when the blood is changed in colour by the agency of the air, the oxygen gas of the air disappears, and carbonic acid is produced. These facts are confirmed by the experiments of M. Berthollet, who confined recent blood in a vessel of common air, and, at the end of twenty-four hours, the air, on analysis, afforded nearly $\frac{4}{100}$ of carbonic acid. In two other experiments, similar results were afforded; and in all these experiments, the acid gas produced was exactly equal to the volume of oxygen that disappeared *. Unless, therefore, it be maintained, that the same oxygen can, at the same time, exist in two combinations, we must suppose, that, in these experiments, no oxygen com-

* Mem. d'Arcueil, t. ii, p. 462.

bines with the blood; and from whatever cause, therefore, the red colour of the blood may proceed, we may safely conclude, that it cannot arise from the combination of oxygen. It forms no part of our present intention to inquire farther into the cause of this redness of the blood; it is sufficient, for our present purpose, to have shewn that it cannot proceed from its oxygenation.

---

596. In our former work, we maintained (129.), not only that no oxygen entered the blood, but that the same circumstance might be affirmed of the nitrogenous portion of the air. This we stated to have been uniformly the opinion of M. Lavoisier, who, we are told, ascertained, by rigorous experiments, that neither an increase nor diminution of nitrogen occurred in respiration *. Such too was the conclusion of Goodwyn and Menzies; and it is borne out by the results afforded in the respiration of all the lower animals. In his late experiments, Dr Thomson observed a deficiency in the quantity of nitrogen; but it was very inconstant, and sometimes scarcely perceptible; and he therefore deems it rather an accidental, than necessary, condition of respiration †. In the experiments of Messrs Allen and Pepys on the respiration of a Guinea pig (568.), no change whatever occurred, either in the quantity, or the quality of

* Mem de l'Acad. an. 1789, p. 574.

† Syst. Chem, vol. v. p. 737, 3d edit.

the nitrogen gas; and in their experiments on human respiration (571.), the deficiency in bulk was so extremely small as to preclude all idea that nitrogen is necessarily retained in the system. Notwithstanding, also, this small deficiency in the whole bulk of air, yet its constituent parts, (estimating the carbonic acid as oxygen) maintained their usual proportions, the nitrogen gas amounting always to $\frac{79}{100}$, which could hardly have happened, if any portion of that gas had been abstracted from the general mass, and retained permanently in the system. In the late experiments, also, of M. Berthollet, no deficiency, but rather an excess of nitrogen appeared in the air expelled from the lungs *. Lastly, nitrogen gas is not absolutely necessary to the exercise of this function; for we have seen (127.) that animals live very well in pure oxygen; and both M. Lavoisier, and Messrs Allen and Pepys, found, that hydrogen gas might, also, be made to supply its place. These facts, therefore, entitle us to repeat, that nitrogen gas exerts no direct agency, nor suffers any apparent change, during the exercise of the respiratory function.

597. We, however, before remarked (107.), that Drs Priestley and Henderson, Mr Davy and others, from finding a smaller volume of nitrogen to be, in some cases, expelled, than had been previously taken into the lungs, concluded that a portion of this gas was *absorbed* in respiration. This opinion we contested, both on anatomical and chemical grounds, and endeavoured to shew (92.), that the error arose

* Mem. d'Arcueil, tom. ii. p. 459.

from the embarrassment into which the respiratory organs were brought by the respiration of an impure air, which rendered them incapable of effecting so completely, as usual, the expulsion of the air they received, and consequently led to its more abundant retention in the lungs.

598. As it was then our fortune to contend against what was called an *absorption* of nitrogen by the blood, so now we have to combat what has been named an *evolution* of this gas from that fluid. In most of the experiments related by Messrs Allen and Pepys, when the same person was employed to breathe, only a small deficiency occurred in the whole bulk of air; but, in one experiment, in which a different person operated, there was an increase of eleven cubic inches upon the whole bulk of air employed. As, however, this air contained the usual proportions of oxygen and nitrogen, no particular effect could have been exerted upon either of its constituent parts; and the "excess, therefore, of 11 cubic inches," to use the expression of these able chemists, "no doubt arose from the person not having been in the habit of exhausting his lungs, so that they contained more when he began, than when he left off *."

599. But in some experiments on the respiration of oxygen gas, which contained only 2.5 *per cent* nitrogen, Messrs Allen and Pepys were enabled to obtain nearly all the residual gas from the lungs, by

* Phil. Trans. 1808, p. 256.

furnishing oxygen gas to supply its place. After having made a forced expiration, the operator began to respire oxygen, until nearly all the air, previously existing in the lungs, was expelled. This air, partly by experiment, and partly by calculation, was found, at temperature 53°, to amount to 141 cubic inches, which consisted of 22.56 oxygen and carbonic acid, and 118.54 nitrogen. Hence, it is inferred, that, after a forced expiration, the lungs of this person must have contained 141 cubic inches of air, which, at temperature 97°, would be increased in bulk to 154 cubic inches *. In another experiment of the same kind, the volume of air in the lungs is estimated at 226 cubic inches, of which the oxygen and carbonic acid formed together about one-fifth †.

600. The large portion of nitrogen gas, which thus appeared to exist in the lungs after the most forcible attempt at expiration, induced these chemists to repeat their experiments on the respiration of oxygen gas. The experiment was made with 2668 cubic inches of oxygen, which contained four *per cent.* nitrogen. The gas was breathed for 13 minutes, and was afterwards analysed. The total nitrogen in the gas inspired was 106.72; in that expired, 211.80; so that 105.08 cubic inches of nitrogen had been obtained from the residual air of the lungs. " The question, therefore, is, whether this increase of nitrogen can be owing to the residual gas contained in the

* Phil. Trans. 1808, p. 266.

† Ibid. p. 276.

nitrogen; and, after the experiment, it contained 147.52; so that 50.12 cubic inches of nitrogen had been obtained from the animal. This increase of nitrogen, it is added, was much more than equal to the cubic contents of the animal's body *. In another experiment, the excess of nitrogen was 34.20; and in a third, made with a mixture of oxygen and hydrogen gases, the nitrogen obtained from the lungs of a Guinea pig amounted to 57.40 cubic inches †. These experiments seem clearly to shew, that, under certain circumstances, a volume of nitrogen gas is obtained from the lungs, greater than the actual capacity of those organs, and greater, consequently, than, in conformity with the mechanical properties of elastic fluids, they can be considered able to contain.

603. Is then this excess of nitrogen, as these chemists suppose, really given off from the blood? This question necessarily involves the previous considerations,—Does this gas, at any time, exist in that fluid, and from what sources can it be derived? Now we have no evidence that nitrogen gas is contained in the blood, but the base of that gas is known to exist in it; and, by chemical agents, which effect the entire decomposition of the blood, it may be obtained from it in a gaseous form. It can hardly, however, be contended, that, because the blood affords nitrogen gas under decomposition, it yields it spontaneously in the healthy living system, where no such decomposition takes place; though this, indeed, is the

* Phil. Trans. 1809, p. 417. † Ibid.

sort of analogical evidence, on which the greater part of our chemical physiology has been made to rest. Until, however, some direct evidence be adduced, that healthy blood undergoes such changes as will enable it to yield its nitrogen in a gaseous form, we may venture to deny, that the nitrogen gas, afforded in these experiments, was derived immediately from any natural changes occurring in that fluid.

604. Rejecting, therefore, the supposition, that the large quantity of nitrogen gas, obtained in these experiments, is derived from any spontaneous changes or decompositions going on in the blood itself, we are necessarily compelled to consider it as afforded by the residual air contained in the lungs. In support of this opinion, it may be stated, that the production of nitrogen is always greatest in the first expiration, and that its quantity progressively diminishes, until, towards the close of the experiment, it is reduced almost to nothing * ;—circumstances which seem plainly to shew that nitrogen is no longer obtained, when all the residual air, previously existing in the lungs, is removed. If this nitrogen were furnished by the blood, independently of the residual air, no reason occurs why it should thus diminish and cease to appear, as this air is abstracted ; for the function of respiration goes on, and the blood, as far as depends on itself, cannot be considered less fit to supply nitrogen. The fact, also, that no such excess of nitrogen is furnished in natural respiration,

* Phil. Trans. 1809, p. 409.

militates against the notion of its proceeding directly from the blood.

605. Assuming, then, that the nitrogen in question is primarily derived from the air contained in the lungs, can we suppose this gas first to enter the blood, and to be subsequently expelled from it? for such operations must take place if nitrogen be really given off from the blood. Now air can be conceived to enter the blood only by the force of mechanical pressure, by absorption, or by the exertion of chemical affinity. If it entered by the first mode, the structure of the cells and vessels would, probably, be irrecoverably injured, and the presence of the air in the vessels would speedily destroy (103.) life; if by the second mode, the air gained admission, it would be carried, by the absorbents, into the venal circulation (95.), which few will suppose to happen; and, lastly, it cannot be considered to enter by the exertion of chemical attraction, not only because the cells and vessels are interposed between the blood and air, but because no affinity subsists (110.) between nitrogen gas and blood. Denying, therefore, the possibility of nitrogen gaining admission into the blood vessels by any mode that has been hitherto suggested, and knowing no way in which this gas can be supplied by that fluid, without supposing it previously to enter into it, we are necessarily obliged to deny also the *evolution* of nitrogen from the blood.

606. We think, too, that the circumstances required to accomplish this supposed *evolution* of nitrogen, oppose the belief that it ever takes place; for it not only does not occur in the ordinary exercise of

this function, but no excess of nitrogen is ever afforded in other cases, unless its place be supplied by an equal or superior bulk of some other gas. Thus, when nearly pure oxygen was breathed, a bulk of that gas disappeared, equal to that of the nitrogen obtained; and when a mixture of oxygen and hydrogen gases, in the proportion of $\frac{22}{100}$ of the former to $\frac{78}{100}$ of the latter, was respired, then, also, a bulk of hydrogen disappeared, equal to that of the nitrogen obtained; so that when nearly pure oxygen gas is respired, say Messrs Allen and Pepys, a portion of it is missing at the end of the experiment, and its place is supplied by a corresponding quantity of nitrogen; and the same thing, they add, takes place when an animal is made to breathe a mixture of hydrogen and oxygen gases *.

607. These facts appear to afford evidence, that, in this supposed evolution of nitrogen from the blood, nothing more than a mechanical substitution of one gas for another takes place; and as in these experiments oxygen and hydrogen caused the expulsion of nitrogen by occupying its place, so we have no doubt but that, when atmospheric air was again breathed, these gases were in turn expelled, and the lungs recovered their former proportion of nitrogen gas. This must, we think, of necessity happen; for we have seen that oxygen suffers no loss of bulk in the lungs (567.), but is only changed into carbonic acid; and Mr Davy long since proved (104.), that hydrogen gas, in its respiration, undergoes no apparent altera-

* Phil. Trans. 1809, p. 427.

tion. Hence, then, we conclude, that the nitrogen gas, obtained in these experiments, is derived from the residual air existing in the cells of the lungs, and that its displacement is occasioned by the mechanical substitution of another gas.

608. Abiding, however, by this conclusion, and yielding, as we are disposed to do, assent to the accuracy of the results which these experiments indicate, we lay ourselves open to difficulties of another kind; for on comparing the state and capacity of the lungs, both in man and in the Guinea pig, with the actual volume of air which they respectively afford, how, it may be asked, are we able to account for the retention of a quantity of air in the lungs, apparently superior in volume to the capacity of the containing organs? Even at the ordinary state of temperature and density in which the air enters the lungs, this difficulty presents itself; and exposed, as this air afterwards is, to so great an increase of temperature, its volume, in conformity with the mechanical constitution of gaseous bodies, will be proportionally augmented. To us there occurs no other mode of relieving ourselves from this difficulty than by supposing that the air, instead of expansion, really suffers a reduction of its elasticity, in the lungs.

609. We have seen (114.), that the air, on entering the lungs, is diffused through an infinite number of small cells, whose diameters do not exceed the $\frac{1}{75}$ part of an inch; and if these cells, as some have supposed, possess a contractile power, either derived from their own structure, or from the action of the thick vascular plexus that is distributed upon them,

they may, although individually their action be small, produce nevertheless, by their combined operation, an effect of considerable magnitude and power. We confess, however, that another circumstance, arising out of this peculiar structure, offers, in our opinion, a better explanation of this supposed fact. It is well known, that a strong attraction or *adhesion* is exerted between air and the surfaces of all bodies, so that from many bodies it is detached with considerable difficulty. The more, therefore, that we increase the extent of surface between the air and the bodies with which it is brought into contact, the more do we augment the effect of this attractive force. Now the surface of the cells of the human lungs, independently of that of the bronchi, is estimated by Dr Keill at 21907 square inches (114.), which is ten times greater than that of the whole body. If, therefore, we call to mind, that no part of the air in these cells can be at a greater distance from their surface than half their diameter, or the $\frac{1}{100}$ part of an inch, and consider, also, the great extent over which this attraction operates, we offer no apparent violence to probability in believing that such a force, acting to such an extent, may constrain the elasticity of the air, and considerably reduce its volume.

610. That moistened animal substances exert a strong attraction towards gaseous fluids, may be inferred from certain facts which occur in the respiration of fishes. There can be little doubt, but that the oxygen gas, which is converted into carbonic acid by the gills and skins of these animals (550.), is attracted by the animal substance, and

brought into such state of contact, as to favour its union with the animal carbon. When, also, water is exposed to heat, its elastic fluids are in great part expelled, but the last portions of oxygen adhere with so much force, that they cannot be separated even by long continued ebullition; yet M. M. Humboldt and Provençal found, that, in such water, fishes were able to respire, and to convert its remaining oxygen into carbonic gas, although the oxygen did not exceed $\frac{1}{1000}$ of the volume of water employed *. While this fact, therefore, points out the admirable provision that is made, to secure to fishes, even under the most unfavourable circumstances, the due exercise of this necessary function, it indicates, at the same time, the existence of a strong attractive force between the animal substance and oxygen gas. So, likewise, in many experiments made by Spallanzani, the volumes both of oxygen and nitrogen gases, placed in contact with animal substances, were, for a time, considerably diminished; which must have been accomplished by some property in the animal substance, independent of its living power. In the same manner, we have seen, that the cellular and fleshy leaves of certain plants are able to receive and contain a volume of air superior to their own bulk (342.); but, perhaps, the chemical affinity, exerted between this air, after its conversion into carbonic acid, and the saline ingredients of the plant, may be deemed sufficient to account for the phenomenon. In the experiments, however, of Mr Acton (217.), dead vegetable matter

* Mem. d'Arcueil, tom. ii. p. 381-2.

was found to reduce the volume of oxygen gas, as well as living matter, in which case no saline substances could have been materially concerned.

611. In the example of charcoal, also, we possess facts which bear a near analogy to the circumstances under consideration. Scheele, Priestley, and Fontana remarked, that charcoal, after having been heated, possessed the singular property of attracting and condensing air; and that neither the air nor the charcoal suffered, in consequence, any change in their chemical properties. These experiments were repeated by Morozzo, who heated pieces of charcoal of a determinate size, and then passed them, in an ignited state, into tubes of mercury, where they remained till they became cold. Different gases were then passed up into the tubes, and were attracted and condensed in very different proportions. Atmospheric air was condensed in a proportion three times greater than the bulk of charcoal employed; hydrogen and oxygen in a bulk nearly double that of the charcoal; and carbonic and muriatic acid gases, ammonia, and sulphuretted hydrogen, in a bulk about eleven times as great. These experiments were repeated by Rouppe and Van Noorden, who employed an apparatus, in which the ignited charcoal was allowed to cool, without being plunged in mercury, or exposed to the contact of air. Atmospheric air and oxygen were then condensed by it in a volume about three times greater than the piece of charcoal employed; nitrogen and hydrogen gases in a somewhat smaller proportion; nitric oxide gas in a bulk more than eight times as great; and carbonic acid

gas in a volume nearly fourteen times greater than that of the charcoal. Morozzo again resumed his experiments, with the precautions employed by Rouppe, and, upon the whole, obtained results nearly similar to those of his former trials. During this remarkable attraction and condensation, the elastic fluids appear to suffer no change in their properties, but are again expelled unaltered by low degrees of heat; neither does the charcoal itself suffer any change, except from a small increase of weight. This attraction of gases by charcoal, says Mr Murray, can scarcely be denominated chemical affinity, but the union between the gas and charcoal must be regarded as rather of a mechanical nature. Gases, indeed, are thus condensed, to which, under any known circumstances, charcoal does not exert any chemical attraction; and they are all reproduced pure by a slight elevation of temperature *.

612. The foregoing experiments, thus repeated and varied by so many different philosophers, and which, as to their general results, are not easily exposed to deception, are nevertheless pronounced "vague and contradictory" by Mr Dalton, who seems inclined entirely to discredit them, and that too upon the faith of the following experiment. He made 1500 grains of charcoal red hot, then pulverized it, and put it into a florence flask, with a stop-cock; to this flask, a bladder, filled with carbonic acid, was connected, and the gas was then admitted into the flask, which became a little heavier. During the ex-

* Syst. Chem. vol. ii. p. 317. et seq.

periment, the carbonic gas entirely disappeared, but what became of it Mr Dalton does not state; he denies, however, its attraction by the charcoal, and asserts that the increase of weight, which that substance received, proceeded entirely from the moisture of the bladder. He considers the attraction of moisture to be the only cause of the increase of weight, which newly-made charcoal receives; and to the decomposition of this moisture, and the union of its elements with charcoal, he ascribes the abundance of gases obtained from charcoal at a red heat. Mr Dalton adds, that the expulsion of gases from charcoal by the heat of boiling water, as alleged in these experiments, is certainly not true, as Allen and Pepys have shewn; and most practical chemists know, says he, that no air is to be obtained from moist charcoal below a red heat *.

613. Notwithstanding the confidence with which Mr Dalton delivers himself on this subject, we shall proceed to point out a few circumstances in his experiment, which prevent our acquiescence in his conclusions. As he seems to have relied chiefly on the circumstance of *weight*, for determining the alleged attraction of these gases, it might have been expected, that, like his predecessors, he would have employed an apparatus which could not have communicated moisture to the charcoal, instead of the bladder which he actually used, and which, he says, did communicate moisture. But the circumstance of increased weight does not afford such satisfactory

* Chem. Phil. part ii. p. 235.

evidence of this alleged fact, as an observation of the *volume* of gas, to which the former chemists entirely trusted, but which Mr Dalton seems to have wholly neglected. A still greater objection to Mr Dalton's experiment arises from the *pulverized* state in which the charcoal was employed, by which its porosity, the very circumstance essential to the success of the experiment, was destroyed;—a circumstance, however, which he has entirely overlooked. Neither will his method of accounting for the gases afforded by charcoal, suffice for the present occasion; since water and charcoal could only furnish gases composed of oxygen, hydrogen, and carbon, but could never explain the production of muriatic and many other gases, which his predecessors obtained. Lastly, in asserting that, in these experiments, gases cannot be expelled at a low temperature, because in the experiments of Allen and Pepys they were afforded only at a red heat, Mr Dalton, as we suppose, has confounded together things which, in themselves, are perfectly distinct; for we know no instance in which those chemists have attempted to expel from charcoal the various gases with which it had been artificially impregnated; and yielding, therefore, entire credit to their results obtained from charcoal under ordinary circumstances, no inference can thence be drawn against the results of other experiments, in which the circumstances are totally dissimilar. For these reasons, we do not consider the experiment of Mr Dalton as, in the smallest degree, invalidating those before related; and if it be considered to prove any thing, it can only prove that this attraction and condensation of gases is not effected by *pulverized* charcoal, just as M. de Saussure found the attractive power of leaves to be destroyed (342.) when they were beaten to a pulp.

614. If, then, it appear, from the foregoing facts, that air adheres to the surfaces of all bodies; that animal substances possess a strong attraction for gaseous fluids; and that the air, received into the lungs, is spread over a surface ten times greater than that of the whole body; may we not reasonably presume, that such an attractive force, operating to so great an extent, may constrain the elasticity of the inspired air, and, like the porous structure of charcoal, considerably reduce its volume? If such or any similar cause be allowed to act, it would follow, that, although the lungs do not contain a greater bulk of air than has been commonly supposed, yet that, in fact, they contain a greater weight of that elastic fluid; and hence, when any portion of this air is expelled, it will occupy a greater space than before, by escaping from the constraint to which it was previously subjected. This attractive force between the air and the animal substance, may, also, be exerted by different gases; and hence, as in the foregoing experiments of Allen and Pepys, oxygen, nitrogen, and hydrogen gases may be successively attracted and retained in nearly equal proportions, and thereby, in turn, produce each other's expulsion.

615. This supposed reduction of the elasticity of the air in the lungs does not stand in opposition either to anatomical or chemical principles; for it is considered to take place only at the surface of the

bronchial cells, and no air is supposed either to enter their absorbent vessels, or to penetrate their substance. It may not be easy to assign the immediate cause of such a phenomenon, but some facts render it probable that it may be connected with the electrical states of the atmosphere, and of the animal substance. The air of the atmosphere is known to be always more or less charged with electric matter; and in the experiments of Mr Wilson, a few blasts of air from a common bellows, especially when the pipe of the bellows was previously heated, were found sufficient to render the tourmalin, glass, and amber positively electrified *. But during respiration, the air undergoes a change in its electrical state; for Mr Read found, that the air, depraved by respiration, always indicated a negative state of electricity, while the surrounding atmosphere was in a positive state †. It is, therefore, conceivable, that, in the act of drawing off its electric matter, a reciprocal attraction may be exerted between the animal substance and the air, by which the elasticity of the latter may, for a time, be diminished; and this cause, operating over so great an extent of surface in the lungs, may possibly contribute to reduce the whole volume of air.

616. Farther, the opinion that has now been stated, presents nothing inconsistent with, or adverse to the chemical changes which the air experiences in the lungs; but, on the contrary, the union of the

* Priestley's Hist. Elec. p. 212.

† Phil. Trans. 1794, p. 267.

oxygen and carbon in those organs must be much facilitated by the more condensed state of that gas, instead of being counteracted, as it would necessarily be, by the expansion which is generally conceived to happen. Without increasing the actual dimensions, we may also, by such an hypothesis, be said really to augment the capacity of the lungs; since we compress into the same space a much larger quantity of that aëriform fluid which they are destined to receive; and, perhaps, the very different estimates which have been given of the capacity of these organs, and of the ordinary bulk of air taken in and expelled in one respiration, may receive illustration from the operation of such a cause. In the ordinary exercise of this function, however, this circumstance is not discovered, because all the causes operating on the air, either mechanically or chemically, preserve, as it were, an equilibrium of action, and, therefore, neither excess nor deficiency any where prevails; but when this equilibrium is disturbed by causes which induce a preternatural state of those organs, and change the usual relations which they maintain with the atmosphere, then the circumstance of this compressed state of the air becomes apparent.

617. Messrs Allen and Pepys farther endeavoured to determine, by their experiments, the quantity of air received into the lungs in an ordinary inspiration. In these experiments, a person in eleven minutes passed 3460 cubic inches of atmospheric air through his lungs, in which time he made about 58 respirations. And as, on ordinary occasions, this same person breathed 19 times in a minute, it is inferred,

that, by multiplying the time consumed in the experiment by the number of natural respirations in a minute, and dividing the whole bulk of air by the product, we obtain the true bulk of air received into the lungs in each natural inspiration; thus, $11 \times 19 = 209$ and $\frac{3460}{209} = 16.5$ cubic inches, which is the quantity of air this person naturally inspires *.

618. It is evident, however, that the respiration of air, under the circumstances of this experiment, can with no kind of propriety be considered as a just measure of the quantity naturally inspired; for not only do the efforts of the mind, and the operations of the apparatus, interfere greatly with the natural actions of the respiratory organs, but the gross quantity of air received in 58 preternatural inspirations, can never with justice be assumed as a true measure of the quantity breathed in 209 natural respirations. On this part of the subject, we continue to think the experiments of Dr Menzies (85.) the most unexceptionable of any which have yet been made, although the quantity of 40 cubic inches, which he assigns, does appear somewhat large.

619. In their experiments on oxygen, Messrs Allen and Pepys found, that a larger quantity of carbonic acid was formed when that gas was respired than when common air was breathed, being, in the former case, upwards of 37 cubic inches *per* minute †, and in the latter only 27. This result is directly opposed to those obtained by Mr Davy (128.), and is not, in

* Phil. Trans. an. 1808, p. 256.

† Phil. Trans. 1808, p. 267.

all cases, in accordance with their own experience; for, in one instance, where atmospheric air was respired for five minutes and a half, the quantity of carbonic acid is stated to have amounted to 51 cubic inches *per* minute; and the faster respiration was performed, the more carbonic acid, it is said, was given off, and consequently the more oxygen was consumed*. Indeed, it is reasonable to expect, that, within certain limits, a variation in the condition of the respiratory organs, or in the exercise of their functions, should occasion a corresponding variation in the products obtained; but the facts just stated shew, that, if the respiration be sufficiently rapid, as much, or more, carbonic acid can be formed from common air than from pure oxygen gas; and therefore, the production of that acid is not so much regulated by the abundance of oxygen in the air inspired, as by some other essential condition of the animal organs.

620. With respect to the actual consumption of oxygen, these chemists conclude, that the atmospheric air, expelled from the lungs, usually contains from 8 to 8.5 *per cent.* of carbonic acid, and that the proportion of acid in no case exceeds ten *per cent.* They estimate the quantity of acid thrown off in eleven minutes at 302 cubic inches, which is about 27.45 *per* minute; and supposing the production uniform for twenty-four hours, the total quantity in that period would be 39534 cubic inches, weighing 18683 grains; the carbon in which is 5363 grains, or rather more than eleven ounces troy. The oxy-

* Phil. Trans. 1808, p. 257.

gen consumed in the same time, will be equal in volume to the carbonic acid gas; but it is evident that the quantity of acid expelled will depend much upon the circumstances under which respiration is performed *. This estimate falls considerably below that given by Lavoisier, Menzies, and Davy (126.); and we must be allowed to observe, that, although we give entire credit to the accuracy and correctness of these experiments, in determining the actual chemical changes induced by respiration on the air, yet, from many circumstances which have been already stated, we do not think them equally calculated to ascertain the quantities of air consumed in the natural exercise of this function.

621. From the foregoing series of facts, concerning the respiration of the higher classes of animals, we feel ourselves entitled to repeat with increased confidence, that "the whole of the oxygen gas which disappears in respiration is employed (123.) to form the carbonic acid produced in that process:" And that "the nitrogen gas of the air neither suffers any change itself (129.), nor produces any direct operation on the animal system." Or, in the words of Messrs Allen and Pepys, "When atmospheric air alone is respired, no other change takes place in it, than the substitution of a certain portion of carbonic acid gas for an equal volume of oxygen †." Consequently in man, as well as in the lower animals,

* Phil. Trans. 1807, p. 279, 280.

† Phil. Trans. 1809, p. 427.

the conversion of oxygen gas into carbonic acid constitutes the only essential change, which the air of our atmosphere experiences in the lungs during its respiration.

ADDITIONS TO CHAP. V.

OF THE SOURCE OF THE CARBON IN VEGETABLES AND ANIMALS BY WHICH THE CHANGES IN THE AIR ARE EFFECTED.

622. IN treating of the immediate source of the carbon in living vegetables and animals, and of the mode in which it was yielded by them to combine with the oxygen gas of the air, during the exercise of the respiratory function, we endeavoured to shew that its emission was connected with the exertion of a living action; and that it was not simply abstracted from the plant and animal by the operation of any affinity subsisting between it and the oxygen with which it combined. We therefore necessarily concluded, that the carbon was an excretion from the living body, depending, like other excretions, on the motion of the circulating fluids; and ceasing to be afforded, when those fluids ceased to move. Although this part of our Inquiry has appeared to attract the least attention, yet, to ourselves, it has always seemed

among the most important: and its immediate connection with our subsequent views of the theory of this function will, we hope, justify our attempt to give it all the additional evidence and illustration in our power.

623. Commencing with the seed, we endeavoured to shew (137.) that the carbon which it afforded to form carbonic acid, escaped from it in combination with the exhaled moisture, and therefore that the union of this carbon with the surrounding oxygen did not take place while it properly formed a part of the vegetable substance. In support of this opinion, we alleged that the presence of oxygen gas was not necessary to abstract the carbon of seeds (133.); and we related some experiments of M. Huber, in which carbon was given out by seeds, and combined with the nitrogen of the atmosphere, after all its oxygen was consumed.

624. Since that period, we have, in part, repeated these experiments, by placing successive quantities of steeped peas to germinate in atmospheric air, where they remained some time after all its oxygen was consumed. The residual air was then washed in lime-water to remove its carbonic acid, and it was afterwards mixed with an equal bulk of atmospheric air, previously deprived, also, of its small portion of carbonic acid. With this aëriform mixture a large phial was filled, in the usual manner, under water, and closely stopped. It was then kept inverted in water for several days, and a portion of it being afterwards passed into pure lime-water occasioned in it a slight, but very sensible discolouration. This

experiment was repeated, and afforded the same results.

625. We next varied the experiment, by mixing together equal quantities of the residual air in which beans had grown, and of oxygen gas obtained from oxide of manganese. Both the gases were first washed separately in lime-water, and were contained in a jar of the capacity of about 37 cubic inches. A small egg-cup, holding a cubic inch of the water of potassa, and covered with a piece of oiled cloth to prevent the entrance of water, was next passed under water into the jar; and the oiled cloth was immediately withdrawn. The jar with the alkaline solution was next removed from the trough on a saucer conveyed beneath it. By the tenth day, the volume of air had somewhat diminished, and the jar being now raised, the alkaline solution was passed into a tube filled with mercury, and inverted in a bason of that fluid. Diluted acid was next passed up into the tube, which excited effervescence in the solution, and produced the disengagement of about $\frac{1}{16}$th of a cubic inch of gas, which was afterwards attracted, with the usual phenomena, by lime-water. In these experiments, the carbonic acid could have been formed only by the union of the oxygen gas with the carbon which had previously combined with the nitrogen of the atmosphere during the process of germination; and the results, therefore, accord completely with those obtained by M. Huber.

626. This expulsion or emission of carbon by seeds is farther confirmed by the experience of M. Senebier, who observed similar phenomena to occur in the

vegetation of plants. If, says he, plants be placed to grow in a vessel of common air, the whole of its oxygenous portion will be gradually changed into carbonic acid; and if the residual air be afterwards washed in lime-water, nothing remains but carburetted nitrogen gas; at least, he adds, if oxygen gas be added to this residual air, we obtain a fresh portion of carbonic acid gas. If, at the commencement of the experiment, pure hydrogen gas has been employed instead of nitrogen, and the residual air, after being washed, be then mixed with oxygen, the formation of carbonic acid is rendered still more sensible, by electrizing the aëriform mixture over lime-water, as the acid gas is then at once produced, and attracted by the lime-water *.

627. M. de Saussure, also, relates an experiment from which a similar inference may be drawn. He placed a plant of *lythrum salicaria* in 60 cubic inches of hydrogen gas, and exposed it to the sun, where, like some other plants of the same genus, it vegetated for five weeks. On examining the air at this period, it was found not to suffer any diminution by the addition of nitrous gas, neither did it contain any carbonic acid; but when a due proportion of oxygen was added to it, and it was then fired by the electric spark, it yielded water, together with portions of carbonic acid and nitrogen gases. Another quantity of hydrogen gas, which stood by the side of the former, but in which no plants had been placed, did

* Physiologie Veg. tom. iii. p. 116.

not afford any sensible quantity of carbonic acid by its combustion with oxygen gas *. The hydrogen gas, in the former case, must have derived its carbon from the plant, and this carbon, during the combustion, must have united with the oxygen to form carbonic acid. The production of water arose, no doubt, from the direct union of oxygen and hydrogen, while the nitrogen proceeded from the air previously existing in the plant.

628. Still farther to corroborate these results, we added to about 22 cubic inches of the residual air of vegetation 14 inches of oxygen, after both gases had been first washed in lime water. Water of potassa was then passed into the jar, in the manner before related (625.), and the jar was thus completely filled by the aëriform mixture and solution. A saucer, filled with mercury, was then passed under the jar, which was thus removed from the trough, and the water on the outside of the jar was removed by a sponge, and only a thin film of that fluid covered the mercury within. By the 16th day, the mercury had risen nearly four-tenths of an inch into the jar; and when, at this period, the jar was raised, and a diluted acid poured on the alkaline solution, it excited in it a very brisk effevescence. These facts sufficiently testify that carbon is given out both by seeds and plants in a form capable of uniting either with nitrogen or hydrogen gas; and they consequently prove, that the presence of oxygen gas is not necessary to abstract this substance.

---

* Recherches Chim. sur la Vegetation, p. 211.

629. But if hydrogen and nitrogen thus enter into union with the carbon of vegetables, in what place and mode are we to consider this union to be accomplished? Are we entitled to suppose that these gases penetrate the organized structure of the seed and plant, and are afterwards expelled from them, charged with carbon? If such a supposition be received, the operation must be effected by the agency of a living power, or by the exertion of chemical affinity. Against the former, it may, however, be urged, that vegetables, when confined in these gases, do not exhibit the phenomena of living action; and consequently we possess no evidence that such action is then exerted.

630. Shall we then ascribe this supposed entrance of these gases into vegetables to the operation of chemical affinity? That the actual union of these bodies is brought about by the exertion of such a force, we readily grant; but our present object is to discover the manner in which these elements are prepared for such an union, and the place where this union is accomplished. Now, before carburetted nitrogen or hydrogen was formed in these experiments, the carbon existed in combination with the other elements of the seed; and from those elements it must have separated, before it could combine anew. Was then this separation effected by a superior attractive force exerted by these gases, or did it proceed, in the first instance, from a diminished attraction, or incipient repulsion among the constituent elements of the vegetable? We adopt the latter view, and, consequently, suppose the carbon to be given off by the seed or

plant, rather than believe it to be drawn away by the attractive force of the gases to which it was exposed.

631. If we may be allowed to venture an opinion upon a difficult question in chemical doctrine, we would beg to remark, that chemists appear to have ascribed too much to the attractive, and too little to the repulsive forces, exerted between the particles of bodies. In artificial experiments, they generally effect decomposition by the employment of agents which exert a superior attractive force towards one or other of the elements of the compound; and the decomposition may then be truly said to be accomplished by the energy of attraction. But decomposition occurs, likewise, spontaneously in bodies, where no particular attraction is exerted by the surrounding bodies, and where, therefore, the first movements towards change must arise in the bodies themselves. By these spontaneous movements, the composition of the existing compound is subverted, and its separated elements are then at liberty to enter into new combinations with each other, or with the bodies which surround them. Hence the decomposition of a body may be effected either by the superior attraction exerted towards one or more of its elements by some other body; or it may proceed from a spontaneous change in the natural affinities of the elements of the body itself, in which case the separation of its parts may more properly be said to arise from repulsion than from attraction.

632. All the phenomena of spontaneous decomposition appear to us, indeed, to suggest the belief, that

the elements of the compound gradually pass from an attractive to a repulsive state, by which its original constitution is subverted. Thus, when carburetted hydrogen or nitrogen, or even carbonic acid, is formed by seeds placed either in hydrogen, nitrogen, or oxygen gas, we see no reason for supposing these gases to penetrate the substance of the seed, and, by a superior affinity, to draw off its carbon; but we rather suppose this carbon, by a previous change in the seed, to be released from its former state of combination, and brought into a condition more or less fitted to combine with the gaseous bodies that surround it, according to the order of their affinities for that substance.

633. Perhaps the expulsion or emission of carbon, under such circumstances, may receive illustration from the emission of odorous particles from bodies. It will not, we conceive, be said that the emission of these particles is caused by the attractive force exerted by the elements of the atmosphere, but that it proceeds from some spontaneous changes going on in the body itself, the atmosphere serving only as a vehicle for the conveyance of these particles, and as a medium which readily permits their escape and diffusion. If, however, these bodies be placed in certain circumstances, the emission of their odorous particles is more or less checked and prevented. This may be accomplished either by withdrawing heat or moisture, both of which were necessary to excite and facilitate those spontaneous changes on which the emission of the odour depended; or the odour may be arrested in its escape by mechanically stopping up the pores

of the body with varnish or oil. Should this, however, be done, the emission of the odorous particles cannot be said to be prevented by the absence of any cause required to attract them, but by the removal of those circumstances which were necessary to favour their separation, or by the interposition of other bodies which prevent their escape, even although their separation be made.

634. In like manner, we consider the separation of carbon from the other elements of seeds, so far to resemble these operations, that, at the commencement of germination, it is effected by the spontaneous changes which arise in the seed, and not by the attractive force of the oxygen of the air. When, however, oxygen gas is supplied, its superior affinity for the carbon of the seed leads to its combination with it, to the exclusion of nitrogen gas; and, by a subsequent agency, arising out of that combination, the development of the seed is accomplished. Although, however, oxygen gas be necessary to this particular change in the seed, we are not from thence to conclude that it is necessary to every change; for we have seen that the seed experiences other changes when no oxygen gas is present. Hence, therefore, it appears to us that the carbon yielded by seeds, both under decomposition and in germination, proceeds, primarily, from spontaneous changes excited in the seed itself, and that it is not removed by the operation of an attractive force, exerted by the surrounding oxygen.

635. What, then, are the circumstances or agents which favour or promote that spontaneous change in

the seed which leads to the separation of one or more of its constituent parts? They are essentially heat and moisture; for it is only when the seed is duly moistened, and placed in a proper temperature, that, either by decomposition or by germination, it is able to emit carbon. By a high degree of heat alone, indeed, the carbon of the seed may be separated and expelled; but the results are then altogether different from those which follow the spontaneous changes to which we have alluded, and to which the concurrence of moisture is necessary. Since, therefore, heat cannot be the sole agent by which the carbon of the seed is brought into a state capable of combining with the oxygen gas of the air, no other mode occurs to us in which this can be accomplished, but that of solution in water; and, therefore, we conclude that this carbon passes off with the moisture of the seed, or is, at least, brought into a state capable of doing so by its solution in that fluid.

636. To this conclusion it has been objected, that carbon is not soluble in water, because pure charcoal appears to resist the solvent power of that fluid. However much this objection may weigh against the solubility of pure carbon, it does not apply to carbon as it exists in vegetables, where the complex state of its combination may greatly facilitate its solution. For "in chemistry," says Dr Thomson, "it may be considered as a truth, to which at present few or no exceptions are known, that bodies are decomposed with a facility inversely as the simplicity of their composition; those which consist of the fewest ingredients being decomposed with the greatest diffi-

culty, and those which are formed of many ingredients, with the greatest facility *." In proof of the solubility of carbon, it may be added, that there is scarcely any vegetable fluid, either natural or artificial, which does not contain carbon in a state capable of forming carbonic acid by combining with the oxygen of the air. Thus it is known that the infusion of malt, in the process of brewing, produces an abundance of carbonic acid; and M. de Saussure observed, that vegetable juices and extracts dissolved in water, and vegetable infusions in passing to the state of vinegar, converted the oxygen gas of the air into an equal bulk of carbonic acid †.

637. It has been farther objected, that the union of carbon and oxygen cannot be effected at so low a temperature as is here supposed; but, unless we deny altogether the formation of carbonic acid in decomposition, as well as in germination, we must admit such combination to take place at the ordinary atmospheric temperature. It is well observed by Mr Murray, that, "although carbon, in its solid and insulated form, requires to be raised to a high temperature to cause its combination with oxygen, yet, when it makes part of a ternary or quaternary combination, in which state its cohesion no longer opposes the combination, it then combines with oxygen at any natural temperature ‡."

---

* Thomson's Syst. Chem. vol. v. p. 417. 3d edit.

† Recherches Chim. p. 140, 144.

‡ Syst. Chem. vol. iv. p. 565.

638. We have no grounds to believe that the carbon of vegetables, in combining with oxygen, passes off in a gaseous form; for although, in the compounds of carbonic acid, and of carburetted hydrogen and nitrogen, it exists in an elastic state, yet there is no evidence of its being able to maintain that state, unless it be combined with some permanently elastic body; and when, by decomposition, it is again separated from such combinations, it resumes a solid form. Thus Mr Tennant found charcoal to be precipitated in the decomposition of carbonic acid by phosphorus (464.); and Mr Cruickshank and Dr Henry, on submitting pure carburetted hydrogen and olefiant gases to electrization, observed the charcoal to be separated and deposited on the inner surface of the glass-tube, and the hydrogen then assumed a state of greater expansion *. As, therefore, this substance does not seem capable of existing by itself in an elastic form, like some other inflammable bases, we have additional grounds for concluding that it really passes off from the seed in solution, or in combination with water.

639. But while we thus suppose that carbon is rather given off by the seed or plant to unite with the surrounding gases, than that these gases are first attracted into the seed to combine with and carry off its carbon, we do not mean to deny that oxygen, and even other gases may not exert attractions towards the vegetable body, and, by their mechanical or

* Phil. Trans. 1809, p. 448.

chemical properties, penetrate, in a certain degree, the pores of the seed, and there unite with its carbon, after it has been brought into a state fitted for such combination by the spontaneous changes described above. All that we contend for, is, that the separation of the carbonic matter from the other elements of the seed is not effected by the attractive force of the oxygen, but that the seed itself must have previously undergone changes, by which its carbon is reduced to a state fitted to combine with the surrounding air; and so far is oxygen from being necessary to abstract this carbon, that this substance is often given off by the seed where no oxygen gas is present. In ordinary cases, therefore, we believe the union of carbon and oxygen to take place at the surface, or, it may be, within the pores of the seed or plant; but if oxygen be not supplied in due quantity, to unite with and carry off the carbon, this substance will escape, in combination with nitrogen or any gas that surrounds it, according to the laws of its affinity for it; or, if no gas be present, it then passes off in an elastic form in union with oxygen (19.), derived either from the decomposition of the seed, or of the water which it has previously imbibed. Even in hydrogen and nitrogen gases, carbon not only passes off in a simple form, but also in combination with oxygen (5.), derived from one or other of these sources.

640. Without attending to the state in which we have supposed the carbon to pass off from living bodies, some writers have imagined that we considered it to be afforded in a solid form. It has, however, been our aim throughout to establish the existence

of this substance in a fluid state; and we have no objection even to admit of its gaseous escape from bodies, if any evidence can be brought of its capacity to maintain that state, or if it can be shewn, by experiment, to escape in combination with any other permanently elastic matter; but, possessing no evidence that it really does so, and feeling assured that it actually is held in solution in water, we conclude in favour of the latter opinion. Hence we would not be understood to attach any new or peculiar notion to the mode or state in which carbon exists; but to speak of it as a solid, fluid, or gaseous substance, according as it may chance to exist in one or other of those states.

641. As, in the preceding discussion, we have spoken indiscriminately of the emission and combination of carbon with oxygen gas, both under the decomposition of organised bodies and during the existence of living action, it may be proper that we should state our opinion of the difference, although we are not able to define the exact limits of those states or terms. Thus we have seen, that carbon is afforded, in union with oxygen, by seeds confined in hydrogen or nitrogen gases (5.), or even in pure mercury (19.), where nothing resembling a living action can be supposed to exist. The same conditions of heat and moisture which, in these circumstances, enable it to yield carbon, are required, also, for the emission of this substance during germination; but no development takes place, unless oxygen gas be likewise present. Now the carbon, at this early period of the process, we suppose to be gi-

ven off by the operation of the same causes, and nearly in the same manner as it is afforded by inanimate bodies. By this carbon the surrounding oxygen is changed, and the development of the seed succeeds to this chemical action. In the progress of this development, the organization of the seed is unfolded; and when this is sufficiently complete, the emission of carbon, like the other functions of the seed, is then executed by an organised structure, and becomes obedient to those laws which govern and regulate the actions of living beings. It is the same with regard to moisture. The dried seed imbibes moisture, at first, like any other inanimate body. By this moisture, in conjunction with other agents, its organic structure is gradually developed, whereby a vascular system is evolved, and then properly commences the function of absorption. Hence, therefore, as it may be said that seeds, at first, simply imbibe and afterwards absorb moisture, so do we say that the carbon at first is afforded by them as it is by inanimate matter; but subsequently it passes off through an organic structure, when it may properly be said to be excreted or exhaled.

642. In support of the doctrine, that the carbon of vegetables passes off in combination with their exhaled fluids, we farther maintained (139.), that the changes in the air were most extensively effected, when plants possessed the greatest vigour, and the motion of their fluids was most actively carried on. On the contrary, whatever arrested the motion of these fluids, diminished the extent of the changes in the air, and consequently bore witness to a diminish-

ed exhalation of carbon. It is in conformity with this view that evergreens, which perspire less (23.) than other plants, were found by M. de Saussure to form less carbonic acid *; and when the perspiratory function wholly ceases, there is reason to conclude that no farther change is then induced on the air.

643. But the leaves of plants, like other organic substances, are enabled to afford carbon after all living action has ceased, provided they be placed in those circumstances of moisture and heat which give rise to the spontaneous changes necessary to the performance of this chemical action. Hence plants, like seeds, form carburetted hydrogen when confined in vessels of that gas; and their leaves, after separation from the stem, deteriorate the air if they are kept in the shade †. In the progress of their decomposition, also, either in water or in air, their carbon escapes in combination with different elastic fluids. From these facts it appears, that, in due degrees of moisture and of heat, a constant action is maintained between vegetables and the air, under which carbonic acid is produced. In the dead plant, however, this carbon is separated in the progress of those spontaneous changes which terminate in the destruction of the vegetable structure: in the living one, it is given off as an excretion, and therefore depends primarily on those laws and conditions which regulate the motions and conditions of the vegetable fluids. In many instances, however, it may be diffi-

* Recherches Chim. p. 96. † Ibid. p. 60.

cult, or even impossible, to fix the precise limits where the one operation terminates, and the other begins.

644. With respect to the *ulterior* source of the carbon in living vegetables, we have supposed (159.) that its supply, in the later periods of vegetation, is derived, like the other ingredients which go to the composition of the plant, from the soil or situation in which it may be placed to grow. From the fact, however, of carbonic acid being decomposed by plants which vegetate in sunshine, it has been inferred by M. M. Senebier and De Saussure, that the carbon of this acid was retained and assimilated so as to afford a constant supply of nutriment to the plant. But plants have been shewn to live and grow in the shade, where they decompose little carbonic acid, and can, therefore, in this manner, derive but little carbon; and, even in sunshine, it has been also shewn (305.) that they grow for long periods in atmospheric air without affecting permanently either its purity or its volume. Consequently, if it were granted, that, by the decomposition of carbonic acid, they obtained carbon in sunshine, they must again give out an equal quantity of it, when they form carbonic acid in the shade; so that no excess of carbon remains to be applied to the growth and augmentation of the plant. M. de Saussure, indeed, admits that plants grow in atmospheres perfectly freed from carbonic acid; but he then supposes them to obtain carbon by decomposing the acid which they had previously formed. It is, however, obvious, that they cannot, in this way, absolutely acquire carbon;

since, as has been well observed, this acid could not furnish to plants more of its base than it had previously taken from them *. When, too, we consider the almost inappreciable quantity of carbonic acid that exists in the atmosphere, and call to mind that its proportion remains uniformly the same, whether vegetation continue or be suspended, we cannot bring ourselves to believe, setting aside all physiological objections to this mode of nutrition, that plants obtain from the atmosphere any portion whatever of that carbon which administers to their nutrition and growth.

645. From the following experiment of M. Braconnot we learn, likewise, that plants, during their vegetation, may considerably augment their proportion of carbonic matter in situations in which no carbon can be supposed to be derived from the atmosphere that surrounds them. He confined 460 seeds of white mustard in a large glass bottle, filled in part with very fine white sand, which was previously deprived of all calcareous earth by washing it in muriatic acid. This sand he then moistened with distilled water, and filled the remainder of the bottle with atmospheric air, freed from carbonic acid. After closing the bottles very accurately, they were placed a few inches deep in a moist soil. Vegetation soon commenced, and considerable verdure was produced. At the end of six weeks, the plants were taken out of the bottle, and washed with great care, and dried. In this state,

* Nicholson's Journal, vol. xviii. p. 22.

they weighed 140 grains, and after the saline and other matters were driven off by a strong heat, there remained 23 grains of pure carbon; while 460 seeds of the same kind, which were submitted to a similar process of incineration, afforded much less carbon; so that the seeds in close vessels acquired, says M. Braconnot, 15½ grains of pure carbon, which appeared evidently to have been formed at the expence of water, and probably of light*. The air, he adds, had undergone little change; which, doubtless, arose from its being so fully exposed to the agency of light.

646. The foregoing result clearly shews that plants acquire carbon in air which contains no carbonic acid; but on its absolute correctness we are disposed to place but little reliance. The attempts to ascertain the quantity of carbon in vegetables, by the processes of distillation and combustion, in the manner they have been hitherto conducted, appear to us very unsatisfactory; for no account seems to have been taken of the quantity of that substance which, during the operation, passes off in a gaseous form. "When, however, a vegetable substance, composed of oxygen, hydrogen and carbon, united in the form of a ternary compound, is submitted to distillation, at a temperature not below that of ignition, the equilibrium of affinities, which constituted the triple combination, says Dr Henry, is destroyed; and the elements composing it are united in a new manner. Those which are disposed to enter into permanently

* Nicholson's Journal, vol. xviii. p. 25.

elastic combinations escape in the state of gas; and thus the aëriform products of the distillation of vegetable substances are mixtures of carbonic acid, carbonic oxide, olefiant, carburetted hydrogen, and simple hydrogen gases; or of two or more of these in various proportions *." Until these sources of fallacy be duly attended to, and fairly estimated, it is in vain to place any great confidence in the attempts to ascertain the quantity of carbon in plants by the destructive mode of analysis that has now been stated.

647. But M. de Saussure has attempted to shew, by arguments of a somewhat different kind, that growing vegetables derive carbon from the atmosphere. M. Hassenfratz had before endeavoured to prove, that plants which grew in pure water, and were exposed to the atmosphere, contained less carbonic matter than the seeds from which they sprang. M. de Saussure obtained a similar result when he caused plants to grow in places weakly illuminated; and the absence of light, therefore, may, he supposes, account for the results which M. Hassenfratz obtained †. In other experiments, however, conducted under exposure to the sun, different results were afforded. The roots of several plants of peppermint were plunged in phials of distilled water, and left to vegetate, in a free exposition to the sun and air, but protected from rain. After growing ten weeks, 100 parts by weight were increased to 216 parts.

---

* Phil. Trans. 1808, p. 283.

† Recherches Chim. p. 52.

These 216 parts were then reduced, by drying them at the temperature of the atmosphere, to 62 parts, which afforded, by incineration, 15.78 parts of carbon. At the same time that the plants above mentioned were put to grow in distilled water, an equal weight of similar plants was dried and incinerated in the same manner, but they yielded only 40.20 parts of dry vegetable matter, which contained only 10.96 parts of carbon; whence it is concluded, that the plants which had grown ten weeks in the open air, and were supplied only with distilled water, had acquired 4.82 parts more carbon than they possessed before the experiment.

648. In another instance, M. de Saussure placed four beans, weighing 120 grains, to vegetate in pure silicious earth contained in a glass capsule. They were watered with distilled water, and kept, for three months, in a free exposition to the sun and air. When taken up green, immediately after flowering, they weighed 1642 grains, but were reduced, by desiccation, to 202 grains, which afforded 51 grains of carbon; while four similar seeds, of the same weight, and dried and carbonized in the same manner, yielded only twenty-two and a half grains of carbon. Hence, says M. de Saussure, the first four beans had more than doubled the quantity of carbonaceous matter by vegetating in distilled water in the open air; and it cannot be doubted but that this matter was derived from the decomposition of carbonic acid found in the atmosphere *.

* Recherches Chim, p. 50. et seq.

649. Even although we pass over the sources of fallacy in estimating the quantity of carbon already stated, and yield entire credit to the accuracy of the results in these comparative experiments, yet many circumstances concur to prevent our acquiescence in the conclusion drawn from them. For, first, we have no evidence that the small portion of carbonic acid, contained in the common atmosphere, can enter plants in order to be thus decomposed and yield its carbon, since the results afforded by plants growing in closed vessels cannot, in this instance, be justly applied to those obtained under a free exposure to the air, where the proportion of carbonic acid is so much smaller, and its ready diffusion through the atmosphere must so rapidly take place. By similar experiments, indeed, M. de Crell was led to an opposite conclusion; for the quantity of carbon, contained in the carbonic acid of the atmosphere, could not, in his opinion, account for the addition of that substance which the plants, in his experiments, received; and he, therefore, supposed plants to possess the power of composing carbon, employing for this purpose only water, atmospheric air, and light *. M. Braconnot, also, by experiments of the same kind, has been led to conclude that vegetables find in pure water every thing necessary for them to assimilate; that vegetable mould and manures yield no nutriment, but are useful only as they improve the texture of soils, and retain and supply moisture; and that earths, alkalis, metals, sulphur, phosphorus,

* Phil. Mag. vol. xxiv. p. 150.

and charcoal, are developed from water by the organic powers of plants, assisted by solar light *.

650. The discoveries of Mr Davy, however, which are scarcely more to be valued for the actual additions they have made to the sum of our knowledge, than for the corrections they have introduced into what we were before supposed to know, have disclosed new sources of fallacy in these experiments, which the state of science, at the period they were made, could not have enabled their authors to foresee. " The experiments," says he, " in which it is said that alkalies, metallic oxides, and earths may be formed from air and water alone, in processes of vegetation, have been always made in an inconclusive manner; for distilled water may contain both saline and metallic impregnations; and the free atmosphere almost constantly holds in mechanical suspension solid substances of various kinds."—" The conclusions of M. Braconnot," he adds, " are rendered of little avail in consequence of these circumstances. In the only case of vegetation in which the free atmosphere, in his experiments, was excluded, the seeds grew in white sand, which is stated to have been purified by washing in muriatic acid; but such a process was insufficient to deprive it of substances which might afford carbon or various inflammable matters."

651. " In the common processes of nature," continues Mr Davy, " all the products of living beings may be easily conceived to be elicited from known combinations of matter. The compounds of iron, of

* Nicholson's Journal, vol. xviii. p. 27.

the alkalies, and earths, with mineral acids, generally abound in soils. From the decomposition of basaltic, porphyritic, and granitic rocks, there is a constant supply of earthy, alkaline, and ferruginous materials to the surface of the earth. In the sap of all plants that have been examined, certain neutro-saline compounds, containing potash, or soda, or iron, have been found. From plants they may be supplied to animals. And the chemical tendency of organization seems to be rather to combine substances into more complicated and diversified arrangements, than to reduce them into simple elements *." To these views of the economy of living beings we yield our cordial assent, and hold them to be not less consistent with the most advanced state of chemical science, than with the justest conceptions we can form of the varying structure and properties of organic beings. They lead us directly back to the opinion, that vegetables derive the carbonaceous matter that contributes to their growth through the fluids which they absorb from the situations in which they grow.

---

652. In the same manner as the carbonic matter is exhaled by plants, so likewise have we maintained that, during the continuauce of living action, it is given off by the exhalent function of animals. The facts which we adduced in support of this opinion

* Phil. Trans. 1808, p. 33, 34.

were chiefly of a physiological nature; and although they do not seem to have drawn much attention from others, yet to ourselves they have always appeared to approach nearly to perfect demonstration. They were derived chiefly from the "Memoirs on Respiration" by the late Abbé Spallanzani, and the continuation of this writer's labours enables us to add some facts in addition to those already stated (142.), in farther confirmation of this doctrine. Whatever difference of opinion may prevail as to the state in which the carbon exists, and the mode in which it is expelled from the body, we must think the following facts afford decisive evidence of the immediate dependence of this action upon the *living powers* of the animal system.

653. It has been already remarked (1.) that seeds, in a perfectly dry state, do not, in the smallest degree, affect the quality of the air in which they are confined; and such, too, we may conclude, must be the case with the rotifer (48.) and various other zoophytes, which, when rendered perfectly dry, remain unchanged for an indefinite period of time.

654. The experiments already detailed (142.) sufficiently prove, that, during the suspension of living action in the vermes and mollusca classes, no change whatever is induced on the air that surrounds them. The correctness of these facts we have since verified by experiment; for we found that snails, while confined in glass vessels over mercury, and kept in temperatures at or below the freezing point, remained quite torpid, and did not emit any sensible portion of fluid from their bodies, nor, in the smallest de-

gree, affect the quality of the surrounding air; but if the temperature was raised a few degrees, the vessel soon became dim from the exhalation of fluid, the animals revived, and the oxygenous portion of the air was then, as usual, converted into carbonic acid. We have before related examples of snails (48.) which, like the rotifer, were rendered torpid by the abstraction of water; and, since they remained in that condition for years without exhibiting any material change, it may be safely inferred, that, during the same period, they effected no change in the air that surrounded them.

655. In the insect class, Spallanzani found that caterpillars, which were in full vigour, consumed more oxygen and produced more carbonic acid than others; and when about to change into chrysalides, they consumed less than in their caterpillar state *. A caterpillar, confined in air at temperature 2° Reaumur, consumed, in five hours, only 0.02 of its oxygen gas, while a similar caterpillar, kept for the same time in a temperature varying from 16 to 17°, consumed 0.08 of oxygen gas. When several caterpillars were kept for a whole night in a vessel of air, preserved in a temperature at and below zero, the air, on examination, contained its usual quantity of oxygen, so that no portion of that gas had disappeared. When, however, the temperature was raised to $1\frac{1}{2}$° above zero, the animals then, in a small degree, consumed the oxygen gas of the air †.

---

* Rapports de l'air avec les ètres organisés, tom. i. p. 25.

† Ibid. p. 30, 31.

656. With respect to fishes, Spallanzani remarks, that, in great colds, they bury themselves in holes, and in the slimy beds of streams; but never become absolutely lethargic *. Some tenches, which were placed in water five degrees below zero, continued to move although the water froze around them. In cold air they soon became immoveable, but instantly recovered action when replaced in water †. In these low temperatures, they breathed only seven times in a minute; but as the temperature was increased, the respiration became accelerated. After a certain time, however, they died in vessels of frozen water from the want of fresh air ‡. A tench, kept during a whole night in air that was only half a degree above zero, consumed a small portion of its oxygen and formed carbonic acid; which is a farther proof that they do not become entirely lethargic from cold §. From these facts we learn, that fishes do not naturally become lethargic from cold, and therefore continue always, in a small degree, to act upon the air. They lose, however, their former vivacity, and, in proportion as their respiration is suspended, they consume less oxygen gas ¶.

657. In the reptile class, the same author observed, that, in a temperature one degree and a half below zero, the heart in serpents beat only twice in a minute, and respiration was suspended. When removed to a temperature of 7°, the heart soon recovered its action, and beat 10 or 12 times in a minute; and

* Rapports, &c. tom. i. p. 157. † Ibid. p. 158, 161.
‡ Ibid. p. 163. § Ibid. p. 165, 168. ¶ Ibid. p. 186.

in still higher temperatures, the pulsations amounted to 28 or 30 *per* minute *. From these and similar experiments, Spallanzani concluded that cold diminished the action of the heart and lungs, and, when the lethargy was complete, the motion of the heart almost entirely ceased, and very little effect was then induced on the air †. In like manner, frogs, and other animals of this class, became more or less lethargic under great reductions of temperature; but the heart continued to beat slowly, and the air, in a small degree, was changed; the consumption of oxygen, however, was always proportional to the elevation of temperature ‡. These facts prove, that, in all the lower animals, the consumption of oxygen gas by respiration is preceded by the motion of the animal fluids, which again immediately depends on the presence and operation of heat.

658. No experiments, we believe, have yet been made on the respiratory process of birds during their torpid state; but if the assertion, that these animals often pass the period of hybernation under water, be true, they must retain the faculty of reviviscence in situations from which air is excluded. As, therefore, in such situations, air is not necessary to their existence, it may be reasonably inferred, that when, in other situations, the actions of life are suspended, the air does not experience any chemical change.

659. In the mammalia class, we have already adduced (143.) the example of the marmot as com-

* Rapports, &c. tom. i. p. 230. † Ibid. p. 250.
‡ Ibid. p. 470.

pletely proving the fact, that no chemical change is effected in the air by the respiratory organs, during the continuance of complete torpor; and Spallanzani found, also, that neither bats, dormice, nor hedgehogs, produced any change in atmospheric air during the suspension of the circulating and respiratory functions. The results thus obtained, by experiments on the lower animals, may be extended, in all their circumstances, to man; for although we possess no direct proof of the air remaining unchanged during the suspension of respiration, yet the actual cessation of that function may be regarded as evidence of it; and we have also seen that man, like other animals, consumes more oxygen while he is vigorous, and during a state of exertion, than under the opposite circumstances of debility and inaction.

660. But if it thus appear, that, so long as the blood, in all animals, continues to move, the oxygen gas of the air continues to be changed; and conversely, if no change be effected in the air when this motion in the blood has ceased; we must suppose some necessary connection to subsist between these events: and farther, as the change in the air follows always the motion of the blood, and gradually ceases to be produced as that motion declines and ceases, we must also suppose the effect in the air to depend on this antecedent motion of the blood, which, therefore, in common philosophical language, we are entitled to consider as its cause. In the order of events, however, this motion of the blood is not the immediate cause of the change in the air, but only enables

the animal to furnish carbon in that mode and state by which this chemical action on the air is accomplished. Consequently we conclude, that the carbon supplied by animals, during the exercise of the respiratory function, is an animal excretion, dependent, like other excretions, on the motion and distribution of the blood.

661. We before maintained that this carbon, in living animals, was brought into actual contact with the air through the medium of the exhalent vessels in the lungs, and we remarked (156.) the peculiar circumstance of this exhalation being furnished as well from the venal, as from the arterial, blood in those organs. The following observations of Dr Barclay entirely accord with these views. " The change produced in the blood by respiration," says he, " is probably owing more to exhalation than absorption, for no artery exhales so freely as the pulmonic. A watery injection thrown into this artery with a small force, will flow copiously into the bronchi, and without occasioning any thing like rupture in its smaller vessels. But whether the exhalation be, or be not, the principal cause of change in the blood, pulmonic blood, when exposed to the air through the medium of its vessels, is always observed to change its colour a great deal faster, while the exhalents continue to act with a vital energy, than when they act slowly and feebly as inanimate organs *."

* On Muscular Motion, p. 524.

662. Besides this exhalation of carbonic matter by the respiratory organs of animals, many facts tend to prove, that, during the continuance of living action, a similar change is effected in the air by the skin. Spallanzani found that some insects, which possess no stigmata, consumed the oxygen gas of the air by the medium of their skin; and the same office must be performed by the skin in many worms and zoophytes, which possess no other distinct respiratory organ. Spallanzani farther observed, that the skins of fishes effected changes in the air similar to those produced by the gills *; and M. M. Provençal and Humboldt found, likewise, that the water in which the bodies of these animals had been confined, afforded the same aëriform products as when they were permitted to breathe by their gills †. In the experiments of Spallanzani, the skins of serpents were found also to consume the oxygen of the air, and to produce carbonic acid like the lungs ‡; and frogs, after being deprived of their lungs, still lived and consumed oxygen gas, which they appeared to effect by the action of their cutaneous organ §. Hence, then, the power of the skin to convert the oxygen gas of the air into carbonic acid seems to be possessed by all the lower animals.

663. From the results of various experiments by De Milly, Cruickshank, Abernethy and Jurine, we were before led to conclude (147.), that carbonic acid

* Rapports, &c. tom. i. p. 187.

† Mem. d'Arcueil, tom. ii.

‡ Rapports, &c, t. i. p. 250.

§ Ibid. p. 469.

was formed by the human skin, wheresoever the external surface of the living body came into contact with atmospheric air; but, in all the experiments made to establish this point, sources of fallacy, which render the opinion somewhat doubtful, may be pointed out. In De Milly's experiments, the water in which the body was bathed might afford carbonic acid, as Priestley (148.) and others remarked. In Mr Cruickshank's experiments, the hand and foot were confined in a vessel covered over with a moistened bladder; but this bladder, as Dr Klapp observed, might furnish carbon to unite with the oxygen gas of the air, and thus give rise to the production of carbonic acid *. The vitiation of the air in bottles fastened to different parts of the body, as reported by M. Jurine (147.), did not take place in similar experiments made by Dr Priestley †; and the carbonic acid, found by the same author, in the air confined under the bed-clothes, where different persons had slept, might, as M. Seguin remarked, proceed directly from the lungs. Lastly, the experiments of Mr Abernethy, who supposed both an absorption and transpiration of aëriform fluids to be carried on by the skin, are contradicted, in all their results, by the later and more accurate trials of Dr Klapp (150.), who found that no emanation of gas took place from the skin, when the hand was confined for several hours in hydrogen gas, in mercury, or in lime-water.

* On the Functions of the Skin, p. 14.

† Obs. on Air, vol. iv. p. 275.

664. But Dr Klapp farther contends, not only that no aëriform fluid is perspired by the skin, but that this organ does not change the quality of the air that surrounds it. He held his hand and wrist for three hours in a vessel of atmospheric air confined over mercury, and kept at the temperature of 60°; but on analysing the air, no change was found to have taken place either in its composition or its volume. A second experiment was made with oxygen gas nearly pure, but no carbonic acid was produced; neither did any change take place in the volume of the gas *. A similar result had been previously obtained by Dr Priestley, who kept his arm for an hour in warm water, while his hand was passed up into a jar of air inverted in water; but the contained air did not appear to have suffered any alteration †.

665. These experiments, to ascertain the action of the human skin on the atmosphere, have been lately repeated, with great care, by our friend Dr Gordon, and with results similar to those which have just been stated. He kept his hand and fore-arm, for an hour, in a vessel of atmospheric air, inverted over water and heated to the temperature of 88° Fahrenheit. The arm was then withdrawn, and the air allowed to cool down to the temperature of the surrounding atmosphere. Its bulk was now found to be exactly the same as before the experiment, and a portion of it being analysed by lime-water, and by phosphorus, appeared to possess no additional quan-

* On the Functions of the Skin, p. 25.

† Obs. on Air, abridged, vol. ii. p. 195.

tity of carbonic acid, nor to have lost any part of its oxygen gas. Precisely the same results were obtained in two other trials.

666. To obviate any objection arising from the circumstance of the hand having been passed into the vessel through water, Dr Gordon next immersed his elbow to the depth of about four inches in a trough of water at temperature 65°; so that the hand, and almost the whole of the fore-arm, remained quite dry above the surface. Over the hand and arm, a jar, filled with atmospheric air, was then cautiously inverted. When the mouth of the jar was brought into contact with the water, a portion of the air was removed by an exhausting syringe, and its place supplied by the water in the trough, so as to remove all danger of the escape of any part of the air actually employed in the experiment. During the experiment, warm cloths were kept constantly applied to the outside of the jar. The pulse beat 64 times in a minute, and the temperature under the tongue was 90°. A fine dew soon began to form on the sides of the vessel, and to trickle down in small streams. At the end of an hour, the hand was withdrawn; its surface felt warm and moist. When the included air had returned to the temperature of 60°, it was found to occupy the same bulk as at first; and, on being submitted to analysis, it yielded no carbonic acid, but afforded the same proportions of oxygen and nitrogen gases, as it possessed before the experiment. These results, therefore, accord with those obtained by Priestley and Klapp; and seem to prove not only

that no aëriform fluid, but also that no carbonic matter is exhaled by the human skin.

667. Adverse, however, as these experiments seem to the opinion that the oxygen gas of the air is affected by the human skin, yet, in others lately made by Dr Charles Mackenzie, and which we had an opportunity of witnessing, carbonic acid was clearly detected in air that had been kept for two hours in contact with the skin. Dr Mackenzie confined his hand and wrist in a glass vessel which contained about 50 cubic inches of atmospheric air. Around the mouth of the vessel a piece of oiled silk was fastened, through which the hand was introduced, and the silk cloth was then closely secured round the arm above, so as to cut off the communication with the external air. In a few minutes the inside of the vessel was bedewed with moisture, which, during the experiment, accumulated to the quantity of nearly half an ounce. The experiment was conducted in a warm room, and was continued for rather more than two hours. In order to examine the air, the hand, with the glass vessel still attached to it, was plunged under water in a pneumatic trough, and the oiled silk being then removed from the arm, the hand was withdrawn. A portion of the air was now passed from the glass vessel into a small tube filled with pure lime-water. It did not affect the lime-water in its transmission through it, but a white filmy crust formed on those parts of the sides of the tube from which the lime-water had been expelled, and in a minute or two, white threads of carbonate of lime likewise fell down through the mass of fluid. The

experiment was repeated, and with nearly the same results; but as the temperature of the room and of the body were lower than in the former experiment, the effects were not so distinct.

668. The foregoing experiments seem clearly to establish the existence of carbonic acid in air that has been kept in contact with the skin; and this acid gas must have been either formed by the action of the skin upon the surrounding air, or emitted ready formed by that organ; for no trace of carbonic acid could be discovered in the air before it was submitted to experiment. Now the experiments of Dr Klapp (663.) seem decisive against the supposition that gaseous fluids emanate from the skin; so that we are compelled to consider this acid gas as formed by the union of the oxygen of the air with the animal carbon, precisely in the same manner as it is formed by the skins of the lower animals (662.), and also by the respiratory organs of man himself. Dr Mackenzie ascertained, that, in these experiments, the carbon did not proceed from the oiled silk; for when the same silk was kept for 48 hours in contact with air, it produced in it no trace of carbonic acid.

669. The effects produced in the air by animal solids and fluids, after their removal from the living body, confirm, in all respects, the belief that they contain carbon in a state fitted to combine with oxygen gas. We have seen that both the serum of the blood (97.) and the entire mass of that fluid, convert the oxygen gas of the air into carbonic acid; and similar effects are produced by the other animal fluids. So, likewise, moistened bladders (587.) act equally

upon the air; and, in their spontaneous decomposition, all animal substances were found by Priestley (145.) and Spallanzani * to exert a similar operation. Now, by all these animal compounds carbon must have been furnished, and the mode of its separation from the other ingredients, and of its combination with oxygen, we conceive to be similar to what has already been stated (632.) to happen in the decomposition of vegetable bodies. Certain degrees of moisture and of heat are necessary to the exertion of this reciprocal action between animal substances and the air; and it is, we conceive, by the gradual operation of these agents that the animal carbon is released from its existing combination, and brought into a condition capable of uniting with the oxygen gas of the air. We therefore suppose a spontaneous change in the animal substance to precede the chemical union of its carbon with the surrounding oxygen; and, consequently, this carbon may be considered rather to separate from the other ingredients, than to be removed by the attractive force of oxygen.

670. In support of this opinion, it may be observed, that, provided due degrees of moisture and heat be supplied, animal substances are prone to change, whether oxygen gas be, or be not, present; and consequently, that gas is not essential to the separation of the elements of the compound. Undoubtedly, the presence of oxygen will greatly modify both the nature and extent of the changes which may take

---

* Rapports, &c. passim.

place; but such modification does not prove that it is essential to every change. The present state of chemical science does not enable us to trace the modifications of change which the varied application of the agents, concerned in the decomposition of animal substances, would produce; but many examples might be stated wherein such substances have undergone remarkable changes, in situations from which the atmosphere was entirely excluded.

671. So, likewise, animal substances undergo spontaneous changes which enable them to afford carbon, when they are confined in elastic fluids deprived of oxygen gas. Thus, M. Huber found, that bees, when confined in air, afforded carbon after all the oxygen was consumed (133.), and this substance then combined with the residual nitrogen, in the same manner as when it was afforded by vegetables;—facts which afford direct proof that carbon escapes from animal substances, in consequence of the spontaneous changes which they undergo, and that the attractive force of oxygen is not necessary to effect its separation.

672. In support of the foregoing facts, the following experiments may, likewise, be stated. A piece of fresh mutton was placed in a jar containing about fifteen cubic inches of atmospheric air, inverted over mercury. The meat was supported on a small hoop that was fixed about half way up the jar, and, beneath the hoop, a glass cup, containing water of potassa, was placed. The air gradually diminished in volume, and, by the third day, the mercury had risen to about one-sixth of the height of the jar, at

which point it remained stationary for many days. The apparatus was then immersed in a trough of water, and the alkaline solution was withdrawn under water. A quantity of the residual air was then repeatedly washed in lime water, to remove all suspicion of the existence of carbonic acid in it. Five different portions of this air were next passed into separate glass tubes, so as to occupy about half their volume, and each tube was then filled with oxymuriatic acid gas, obtained from oxymuriate of potassa and muriatic acid. The tubes were then closely corked under water, and kept inverted in that fluid, and exposed to the light of day. In 24 hours, one of the tubes was uncorked under water, and the water immediately rose into it to a considerable height. A portion of the remaining gas was then passed into another tube, filled with pure lime water, but it produced in it no discolouration. After two or three transmissions, however, carbonic acid was rendered manifest by the whitish film which formed on the sides of the tube, and by the white threads of carbonate which, after a few minutes, fell down through the body of the liquid. The gases in the other tubes were examined in succession, and all afforded the same phenomena, when transmitted through lime water.

673. In farther confirmation of these facts we placed portions of meat in a jar containing equal parts of atmospheric air and hydrogen, and also in another jar of pure hydrogen gas. Both jars were inverted over water, and remained in their respective positions for ten days. The residual gases were then washed in lime water, till they ceased to produce

any effect in it. Portions of them were then mixed, with equal bulks of oxymuriatic gas, in glass tubes which were closely stopped. At the end of 24 hours, the mixed gases were opened under water, when a great dimunition of volume took place; and the residual gas, after two or three transmissions through lime water, gave abundant evidence of the presence of carbonic acid gas. When the gaseous mixture, consisting of carburetted hydrogen and oxymuriatic acid gas, was exposed to the direct agency of the solar rays, a dense white cloud instantly appeared, which soon subsided. A diminution of the volume of gas was then immediately produced, and by the method already described, the presence of carbonic acid in the residual air was at once detected. This effect of the sun's rays in quickening the action of hydrogen and oxymuriatic gases was observed by M. M. Gay Lussac and Thenard *; and also by Mr Dalton †. These results, therefore, sufficiently prove, that animal as well as vegetable substances undergo such spontaneous changes as enable them to yield their carbon to the gases which may surround them; and they shew likewise that the carbon, which thus unites with nitrogen or hydrogen gas, may be again separated by the superior affinity of oxygen ‡.

---

* Mem. d'Arcueil, tom. ii. p. 349.

† Chem. Phil. p. 300.

‡ We were led to this method of experiment by witnessing, in the late experiments of Mr Murray on the nature of oxymuriatic acid gas, the facility with which the carburetted gases and oxymuriatic gas act on each other at low temperatures in day light: and, by exposing the gaseous mixture to the direct rays of the

674. But whether the carbon, furnished by animal bodies, escape by virtue of a living action, or whether it be afforded under those spontaneous changes which all organised bodies experience, the actual combination of this substance with oxygen gas is purely chemical, and to this combination, effects, apparently similar, seem to succeed. Thus, by exposure to oxygen gas, black blood is rendered red, as well after its removal from the body, as during its transmission through the lungs; and so, likewise, recently cut flesh often appears nearly black, but, by exposure to the air, it assumes a florid hue. Whatever, therefore, be the mode in which the carbon is supplied to act on the air, the sensible effect produced in the animal fluids, both during life and after death, is precisely the same, and must, therefore, be equally referred to the operation of a chemical action.

675. But although the union between oxygen gas and carbon be, in all cases, purely chemical, and the immediate effects to which it gives rise, both during the continuance of living action and under spontaneous decomposition, be precisely the same; yet the facts, which have now been detailed, sufficiently establish a difference in the mode in which this carbon is supplied. For in the living body, the emission of

---

sun, the action was immediately accomplished. It may be proper to add, that when, in these experiments, the tubes have been closely stopped, the carbonic acid is not detected, till after the gas has been repeatedly transmitted through lime water, its appearance at first being prevented by the presence of muriatic acid gas.

carbon is always preceded by the motion of the blood, and must, therefore, be considered, like every other separation of matter from that fluid, as an animal function, carried on and maintained by an appropriate organic structure, and according to the laws which regulate the exercise of living action; while, in inanimate bodies, this carbon is yielded in the progress of those spontaneous changes which ultimately terminate in the dissolution of the animal compound. Between the termination of the living and the commencement of the dead process, a period, more or less long, appears to intervene; and this period will vary, in different cases, according to the natural constitution of the body, and the mode and degree in which the external agents are employed. If the body retain its susceptibility of action, these agents will restore its living functions; if this susceptibility be lost, they only serve to hasten its decomposition and decay. Thus nearly do the powers required to exhibit the phenomena of life and of dissolution approximate each other; and thus do two series of actions, in their effects and consequences so entirely distinct, proceed under the operation of the same external agents.

676. With respect to the actual place of union between the carbon and oxygen in living animals, we suppose it to happen at the surface of the respiratory organ, whatsoever be the form of the organ, or in whatever part of the body it be placed. By the motion of the animal fluids, which universally precedes this combination, the carbon is brought to this surface in a state fitted to combine, and the air,

at the same time, by the action of the respiratory organ, is presented to the same surface, where its oxygen combines with the exhaled carbon, and both pass off together in the form of carbonic acid gas. This chemical union, especially in the higher animals, is increased and promoted by the great extent of surface of the respiratory organ; by the apparent attraction or adhesion subsisting between this surface and the air; by whatever accelerates the motion of the blood, and increases the exhalation of carbonic matter; and, lastly, by the constant supplies of fresh air furnished by the action of the respiratory organs.

677. In many animals, however, which belong to the inferior classes, and in the whole class of fishes, as well as in aquatic plants, the air is not presented to the respiratory organ in an elastic state, but through the medium of solution in water. We have seen that vegetable and animal substances possess the power of separating air from water; and a similar separation we must suppose to be made by the bodies, and especially by the respiratory organs, of aquatic animals. By the exertion of this power, the oxygen gas, contained in water, is brought into contact with the carbon as it exhales from the animal system, and a chemical combination, with the usual phenomena, takes place. In aquatic animals, no cells or receptacles for containing air are provided, so that, as in some animals of the vermes class, the union takes place at the surface of the blood-vessels; but in terrestrial animals, a cellular structure for receiving the air is interposed, at the surface of which the combi-

nation of the oxygen with the carbon is effected. Lastly, we may observe, that, in animals which live in water, the respiratory organ may be considered as external, and as being constantly moistened by the mass of surrounding fluid, while in those which breathe in air, this organ may be regarded as internal, and the portion of air that comes into contact with it as separated from the general mass, by which means the organ is protected from the effects of too rapid evaporation, and is always preserved in a moist and secreting state, a provision which the condition of aquatic animals did not render necessary.

678. To the mode in which we have supposed the carbon to be afforded by the lungs in respiration, it has been objected by Dr Bostock, that "it does not explain how the regular supply of this substance is, at each successive respiration, brought to the lungs in a state proper to be discharged *." We certainly admit, as this respectable writer states, that the carbon, in common with every other ingredient of the body, is derived primarily through the organs of digestion; but we do not suppose it to be excreted in its first transmission through the lungs. We regard carbon as a constituent part of the animal fluids, and consider it to be excreted in the lungs as long as the blood continues to move. Hence it follows that the excretory function in the lungs is only the *immediate* source of the carbon that is supplied to act upon the air; and that its *remoter* source must be sought in that function which, by supplying materials to the

* Edinburgh Medical Journal, NO. xiv. p. 165.

blood itself, enables it to support all the secretions, and to recruit and maintain the health and stability of the system. So long, therefore, as the blood continues to move, and the secretory functions continue to be performed, so long will carbon be supplied to act upon the air; but when the motion of the blood is suspended, or has finally ceased, then carbon is no longer furnished, nor is the air any longer changed.

ADDITIONS TO CHAP. VI.

OF THE PHENOMENA WHICH ARISE FROM THE CHANGES INDUCED ON THE AIR BY THE LIVING FUNCTIONS OF VEGETABLES AND ANIMALS.

679. In this last chapter of our work, we endeavoured to prove that the extrication of the subtile matter of caloric constitutes the only observed phenomenon that attends the conversion of oxygen gas into carbonic acid in the processes of vegetation and respiration. In proof of this extrication of caloric, it was shewn (165.), that oxygen gas possesses a large portion of specific heat; that, by the conversion of this gas into carbonic acid, its heat (166.) is necessarily set free; and that vegetables and animals, which produce this particular change in the air, exhibit, in consequence, a higher degree of temperature. To the evidence adduced in support of these positions we have now but little to add, which we the less regret, as the facts already stated appear sufficient to establish the general inferences deduced from them.

680. In the vegetable kingdom we have remarked (167.), that germinating seeds, when accumulated together, exhibit a sensible rise of temperature; and in the process of malting, Dr Thomson has seen the radicles of barley, when kept without turning on the malt floor, shoot out half an inch in a single night, and the heat rise as high as 100° *.

681. Many facts have been related (169.) in support of the opinion that plants possess a power of producing heat. The experiments, however, of Mr Hunter, to shew that trees possess a temperature higher than that of the surrounding air, are by no means satisfactory. He bored holes in trees to the depth of eleven inches, and found, that, in the month of March, when the atmosphere was 57.5°, the thermometer in the tree stood at 55° only. In April, when the temperature of the air was 62°, that of the tree was only 56°; but when, on succeeding days, the air fell to 47°, the tree then exceeded it in temperature by eight degrees; the day following, however, both the tree and the atmosphere were at 42°. In October, when vegetation began to decline, and the temperature of the air was at 51.5, that of the tree was 55.5; and a few days afterwards, when the atmosphere was at 47°, the tree was from 5 to 6° warmer. In November, a similar difference in temperature was observed; but in December, both the tree and air were found, in one experiment, to be exactly 29°. In other instances, the temperature of the tree was sometimes higher and sometimes lower than

* Thomson's Syst. Chem. vol. v. p. 304. 4th edit.

that of the atmosphere, by a difference varying from one to four degrees *.

682. These varying and contradictory results oppose the belief that the tree possesses any natural power of steadily maintaining a temperature higher than that of the surrounding air, and lead to the supposition that its fluctuating condition, in this respect, proceeds entirely from accidental causes. Variations in atmospheric temperature must be supposed to influence that of the tree; but the rapidity with which changes occur in the former would not, in an equal degree, affect the latter. Thus, in summer, the temperature of the tree rose slowly, when that of the atmosphere was rapidly changing; and when the latter was as high as 57°, the heat of the tree was generally less. On the contrary, when the atmosphere fell below 57°, the tree was a few degrees warmer; but if the cold continued a few days, both the tree and the atmosphere came down to the same temperature, even in April when vegetation was actively going on. These facts seem to prove, that the tree possesses no internal power of producing heat; but that its temperature follows that of the surrounding air, subject to such variation as arises from a difference in its conducting power, and other accidental circumstances.

683. But whatever doubts may exist as to the natural temperature of trees, the facts already adduced (170.), concerning the heat exhibited by certain plants, during the process of fecundation, are too

* Phil. Trans. 1778, p. 46.

striking to be liable to any exception. In addition to the observations of M. Hubert on the heat of the *arum cordifolium*, we may now state others, made on another species of the same genus, by M. M. Lamarck and Senebier. M. Lamarck observed the flower of this plant, at the period of fecundation, to communicate the sensation of heat, and a similar observation was made by M. Senebier. The *maximum* of heat occurred about six hours after mid-day, when it exceeded that of the atmosphere by nearly 12° Fahren. During the development of this heat, the flower in part became black; and the rapid combination of oxygen with the carbon of the flower may, says M. Senebier, be suspected as the cause of this appearance: but he did not, by experiment, ascertain the fact *. The more accurate observations of M. Hubert (171.), have shewn that this great increase of heat is accomplished chiefly by the external surface of the spadix of the flower; that the presence of air is necessary to its production; and that, during its development, the pure part of the air is consumed; whence it may be inferred that this heat is immediately derived from the extrication of caloric by the consumption of oxygen gas.

684. We have already given examples (173. et seq.), which prove that worms, insects, fishes and reptiles, possess, in many instances, a temperature higher than that of the medium in which they live; and we have also maintained, that, as this small excess of heat is constantly passing off to the surrounding

* Physiol. Veg. tom. iii. p. 314. 317.

medium (178.), its permanence can be secured only by the constant exercise of some necessary animal function. But no function which the animal exerts, except that of respiration, can be conceived thus to afford a constant supply of heat; and since, in the exercise of this function, the oxygen gas of the air is uniformly converted into carbonic acid, under which its latent caloric is necessarily set free, the liberation of this caloric by the perpetual decomposition of the air (180.) must be regarded as the natural and necessary means by which the animal temperature is sustained.

685. In the higher classes of animals, which possess a temperature greatly exceeding that of the surrounding medium, and which, under every vicissitude of heat and cold, preserve nearly an uniform degree of heat, we have referred the primary source of animal temperature to a similar extrication of caloric, arising from the decomposition of the air (189.), in the exercise of the respiratory function, as originally suggested by the illustrious Dr Black, and subsequently developed and confirmed by the elaborate researches of Dr Crawford. In support of this doctrine, it has been maintained, that the latent heat of the air is necessarily set free in the lungs (190.) during the exercise of the respiratory function; that the blood, after this extrication of caloric, possesses an increased portion (191.) of specific heat; and that the quantity of caloric thus actually afforded to that fluid may be considered sufficient (192. 3.) to account for the height and continuance of animal temperature.

Such is the result of his Farther Inquiries into the respiratory function of Plants and Animals which the Author submits to the Public. In all that he has yet written, he has chiefly confined himself to a consideration of the nature and extent of the changes which living animals and vegetables induce on the air; and the amount of his researches may be comprised nearly in the simple statement, *That oxygen gas is uniformly converted into carbonic acid during the exercise of the respiratory function, and that, by this chemical change in the air, its latent or specific caloric is set free, and enters into the vegetable and animal systems.*

The facts which establish this particular change in the air, and the consequent entrance of its caloric into the system, may now, he conceives, be considered as fully and universally ascertained; but the effects which this subtile matter afterwards produces, and the laws by which it is developed, have been less attentively regarded, and are, therefore, less perfectly understood. It was the Author's intention to have entered at once into a detailed investigation of these subjects; but the unexpected length to which his present inquiries have extended, and a wish to settle definitively the preliminary questions now discussed, have again arrested his progress, and brought him to a temporary pause.

Should it, however, be thought, that, in the present work, he has succeeded in establishing the general facts which relate to the *changes induced on* *the air*, and a desire be expressed that he proceed in his inquiries, he will yield a willing obedience to the command; and, in a subsequent volume, will endeavour to illustrate and explain the *reciprocal effects* which are produced in the vegetable and animal systems. Though duly sensible of the difficulties which he has to encounter, he yet hopes to be able to present a view of these subjects, adequate, as he thinks, to explain the phenomena, and free from the objections which lie against every explanation that has been hitherto proposed. In furtherance of this design, he now, therefore, ventures to announce his intention of attempting to trace *all the observed effects, which succeed to the exercise of the respiratory function in plants and animals, to the varied agency of that subtile or calorific matter, which is universally liberated, by the changes induced on the air, during the continuance of this living process.*