of his feeble state he was kept in quiet, and fed with light food, as rice, coffee and tea, adapted to his palate, to his inactive state, and to the feebleness it was thought his digestive organs must share with his general system. He recovered, but he was under the effects of disease many weeks.

On the Reciprocal changes which take place in the blood and in the air, in Respiration; it being the Prize dissertation upon that subject, for the year 1813. BY E. HALE, JUN. M. D.

[Communicated for the New England Journal of Medicine, &c.]

THE composition of the atmosphere is so well known that at first view, it should seem an easy matter to discover by direct experiment what changes it undergoes in respiration. The air may easily be examined before, and after it enters the lungs. But, the inferences from such an examination are liable to much uncertainty. It is impossible for a person to breathe *naturally* while his mind is attending to the process. If he do no more even than to enumerate his inspirations, his respiration soon becomes laboured, and he feels the necessity of a deep inspiration to relieve a sense of oppression at his lungs. Hence it is an unfair presumption, to conclude that the results of such a respiration are precisely the same as those of a natural one. The uncertainty arising from this cause would be much less, were it true that the same portion of air which is inhaled at one inspiration, is expelled at the next succeeding expiration. But this is far from being the case. The quantity of air which remains after a complete expiration is much greater than that inhaled at an ordinary inspiration. The fresh air inspired is therefore mixed with what was before in the lungs; so that any variation in the fullness or quickness of any part of the respiration necessarily varies the qualities of the air expired. By continuing the experiment, so as to subject a large quantity of air to examination, as was done by Messrs. Allen and Pepys, the difficulty is in some measure obviated: yet even they were not able to remove it altogether.

It will be proper therefore to inquire into the chemical properties of the fluids brought together in respiration. By estimating the natural results of their affinities, we shall be enabled

to judge in some degree of the correctness of inferences derived from experiments.

These fluids, it is well known, are on the one part, *atmospheric air*, consisting of Oxygen, azotic, and a small portion of carbonic acid gasses; and on the other the *venous blood*, whose elementary principles are oxygen, azote, hydrogen and carbone, with a small portion of iron, phosphate and carbonate of lime, &c.

The azotic and carbonic acid gasses have neither of them, in their natural state, any affinity for either of the elementary ingredients of the blood. Their properties, so far as respiration is concerned, are purely of the negative kind. Oxygen gas has a relation to azote, hydrogen, carbone and iron.—The only productions therefore, which are purely chemical, that the nature of these elementary principles admits of, are nitrous gas, nitrous oxyd, nitrous and nitric acids, carbonic acid, water and oxyd of iron. The combinations of oxygen with azote, have never been suspected, and cannot be supposed, to exist among the results of respiration.

Thus it appears that water, carbonic acid, and oxyd of iron are the only supposable products of respiration; so far as it is a mere chemical operation.

I am sensible it has been common to consider the blood as in some respects a simple fluid, capable of possessing affinities of its own, independent of those of its ultimate principles. This may undoubtedly be correct in regard to the living actions of the body. But its chemical properties necessarily depend on those of the substances, of which it is composed. When submitted to examination, it may soon cease to be blood; but its nature, as a chemical fluid is not altered, unless by new combinations among its elements. Although its proximate principles are easily destroyed, its ultimate ones are constant, and its relations may as readily be inferred from the nature of the substances which enter into its composition, as those of any other fluid.

There does not appear to be any combination among the substances composing the blood that will materially alter its attractions for atmospheric air.

All the oxygen of the blood is probably intimately united with other substances; but none of its combinations possess any attraction for either azote or carbonic acid. The iron of the blood is already oxydised at least in some degree; consequently is capable of receiving only a smaller quantity of oxygen from the air. Hydrogen and carbone when united are too easi-

ly separated to prevent their combination with any other substance. None of these combinations therefore affect the inference derived from the nature of the elementary ingredients of the atmosphere and venous blood, that the union of oxygen with carbone, hydrogen and iron, are the only supposable chemical effects of respiration.

The French chemists, after discovering the phenomena of combustion, readily conceived respiration to be perfectly analogous. They had found that in the former process, oxygen combines with combustible matter, giving out its carbonic; and in the latter, they perceived oxygen gas to disappear in the lungs, at the same time that heat is produced. Hence they concluded, that respiration is only a slow combustion.

It is not surprising that this opinion was so generally received, when we consider the time in which it prevailed. The discoveries in pneumatics which raised chemistry to so high a rank among the sciences, had just been made. Almost every man who made any pretensions to chemical knowledge, was enthusiastic in speaking of the wonderful properties of the gases, particularly of oxygen. The virtues attributed by the alchemists to the philosopher's stone, could hardly be more extravagant than those, which this gas was imagined to possess. The simplicity of Lavoisier's theory of respiration was also captivating, at a time when the minds of physiologists were worn out with the complex systems of former philosophers.

Had it not been for the belief of the importance of oxygen to the blood, the arguments in favour of the opinion, that it enters that fluid from the atmosphere, could hardly have gained so much confidence. Even the analogy, which has been supposed to exist between respiration and combustion, favors a different theory, at least, quite as much as it does this. In combustion, although oxygen combines with the combustible matter, yet only a small proportion of the products remains in a fixed state; much the greater part flying off in vapour and gases.

In like manner, if we were to attach any importance to this analogy, it might be supposed that the results of the combinations of oxygen in the lungs are such as to pass off with the expired air, rather than such as should remain in the blood.

We have seen that carbone, hydrogen, and iron are the only substances in the blood that can be suspected of exerting any action on atmospheric air. Of these only the last would yield a product by combination that should retain its oxygen

in the blood, Water, if formed, might indeed become fluid and not be expired in vapour. But as a large quantity of vapour is expelled, at every expiration and as the fluidity of the blood is sufficiently insured by other means, this would render the operations of nature unnecessarily complex, without producing any adequate advantage to the system.

The quantity of iron in the blood is exceedingly small; and this already oxydised in a greater or less degree, before it enters the lungs. It is well known that this metal imbibes oxygen from contact with watery fluids much faster than it does by mere exposure to the air, and that the blood contains a large proportion of water. Why then should we resort to the unnatural conclusion that iron, when intimately combined with this fluid, is in a low state of oxydation; but upon exposure to the air passes immediately to a highly oxydised state? Indeed were this actually the case, the quantity absorbed would be so small as hardly to be worthy of notice in estimating the effects of respiration. But from the view I have taken of the subject, it does not appear that even this small quantity of oxygen passes from the air to the iron of the blood in the lungs. The change of colour in the blood has been regarded as evidence of the addition of oxygen to the iron in respiration. But before this can be allowed, it ought to be proved that the blood really owes its colour to the oxyd of iron it contains.

I cannot avail myself of Mr. Ellis' arguments upon this subject; for he seems to forget that the question is not whether oxygen gas enters the blood, *in the state of gas*. Under this mistake he contends that as the absorbents will not take up air, or if they did, they would carry it to the right, and not to the left side of the heart, oxygen gas cannot be supposed to enter the blood.* There appears to be however, not only no evidence that oxygen does enter the blood, but evidence sufficient to show that it does not enter it. I have already detailed that which is deduced from the affinities of the substances composing that fluid, and the atmosphere.

The results of experiments on respiration equally confirm the same opinion. It was long ago observed that the only change, perceptible without chemical tests, which the air undergoes by a natural respiration, was a small diminution of its bulk. This diminution however was not found by Dr. Priest-

* Ellis on Air, Vol. I. page 116 and seq.

ley and Dr. Crawford when small animals breathed air, confined over mercury. Hence the inference is unavoidable that a portion of air was absorbed by the water when similar animals breathed over this fluid. When man is the subject of experiment, the additional force required to throw the air into a vessel, and the weakened state of the respiratory muscles, occasions some of the air to be retained in the lungs. There is always a sense of fatigue and debility attending such a respiration, which shows that it is not completely performed.

The extensive experiments of Messrs. Allen and Pepys decidedly prove that such are the causes of the seeming disappearance of a portion of the air respired. When large quantities of air in each experiment were breathed by themselves, the deficiency in the expired air was extremely small, amounting sometimes to but little more than $\frac{1}{1000}$ part; and in one instance there was an actual increase. But what particularly deserves consideration, the apparent loss of azote was always proportionate to that of oxygen.* They afterwards made a Guinea pig breathe a long time, in two experiments when there was no alteration in the volume of the air respired, and once one cubic inch was gained†. As an equal volume of oxygen gas is consumed in the formation of a given quantity of carbonic acid gas, no oxygen could have entered the blood in these respirations.

Dr. Crawford, concludes that one sixth part of the products of the combinations of oxygen in the lungs is watery vapour. But since it is known that a large quantity of vapour is produced by the exhalent vessels of the lungs, it is more natural to suppose that all which is expired is thus secreted from the blood. The experiments of Messrs. Allen and Pepys seem decisive in proving that none is produced by the combinations of oxygen with hydrogen. Those of Dr. Priestley and Dr. Crawford, to which I have before referred, confirm this opinion. In these cases as there was as much carbonic acid gas produced, as there had oxygen gas disappeared, and as an equal volume of oxygen gas is required, to form carbonic acid, none of course could go to the formation of water.

The chemical affinities of the substances concerned, do not afford any thing decisive in determining whether or not, water is formed in respiration. We are not indeed acquainted with any process in nature, where hydrogen unites with oxygen, at so low a temperature as that of the lungs; although carbone combines with it at a lower temperature, particularly in the

* Philosophical Transactions for 1808, part II. † Ibid. for 1809, part II.

process of fermentation. But this fact only renders carbonic acid a *more probable* result of respiration than water; for we are ignorant of the influence which the living principle may have on chemical attraction. We need however hardly regret that this evidence is so incomplete, if we may rely upon the experiments of Messrs. Allen and Pepys, confirmed as they are by those of many other gentlemen.

Thus far, I have taken it for granted that the relative situation of the air and blood in the lungs is such as to admit of the action of their chemical affinities. I am aware that this opinion has been strongly controverted. Mr. Ellis contends that, "to the operation of chemical affinity, 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 of direct experiment."* It is very observable that Mr. Ellis here labours under the mistake I have before mentioned. Under this false impression he strives hard to prove that aeriform fluids cannot pass into the blood vessels. But it is not pretended, and I believe never has been, that either "the air, or its oxygen gas," passes through the membranes of the lungs, in the state of gas. Every writer,† upon this subject; whose works I have seen, makes the obvious and important distinction between the base of a gas, and the gas in its aeriform state. It is the former which has been supposed to pervade the thin membranes of the lungs.

In no book but that of Mr. Ellis himself, can I find the absurd proposition, that "by superior affinity the blood abstracts air through the coats of the cells and vessels of the lungs,—and again giving out nearly the whole of this air, through these same blood-vessels and cells."‡ All that is contended for is, that, notwithstanding the intervention of these thin coats, the air and blood are so circumstanced as to be capa-

* On Air Vol. I. page 125.

† I have not seen Sir Humphry Davy's "Researches into the nature and respiration of Nitrous Oxyd," to which Mr. Ellis so often refers; but it is manifest from his quotations, that Mr. Davy made the same distinction between the use of a gas, and the gas itself.

‡ On Air Vol. I. p. 126.

ble of exercising the same attractions in the lungs, as they would out of the body.

It is evident that the carbone of the carbonic acid expired, is derived from the venous blood. How is it separated from that fluid? Mr. Ellis says by the vital action of the exhalents of the lungs. His words are, "these exhalent vessels of the lungs,—appear to be endued with a power not only of exhaling water, but also of emitting carbone;"* and again "the carbone supplied in human respiration, is truly an animal excretion, carried on by the exhalent vessels of the lungs."† But if this be the case, why is the presence of oxygen in the lungs absolutely necessary to the continuance of life? It is well known that unless some gas containing oxygen is constantly respired, carbone is not separated, but the venous blood passes unchanged to the left side of the heart. Does the carbone after being in a measure thrown out of the living system into the air cells and bronchiæ, return back into the blood when there is no oxygen to carry it away? Why is it not expired with the watery vapour by which Mr. Ellis supposes it be held in solution?‡

These considerations, I trust, are sufficient to show that Mr. Ellis's opinion cannot be correct. We recur therefore again to the question, by what means is carbone separated from the venous blood?

The principal argument against the operation of chemical affinity is that the air and blood do not come into actual contact. Let us then examine the validity of this argument.

It is a part of the definition of chemical attraction, that it takes place only at insensible distances. If this definition be rigidly adhered to, I grant, it cannot strictly be said, to be exerted in the lungs. But it is not the name for which I contend. Changes are effected in bodies, not in complete contact, without any discoverable agency but their affinity, which are perfectly similar, in every known respect, to those which are purely chemical. This may not be the exertion of chemical attraction. Yet chemists themselves allow an extension of their rule in regard to the operations of Galvanism, much greater than is necessary to apply to our present purpose.

There is a kind of analogy between some of the galvanic operations and the actions I speak of, which may serve to illustrate the possibility of the latter. By the galvanic action, oxygen and hydrogen may be made to pass from the opposite poles of a battery; and the more fixed alkalies

* On Air, p. 199.

† Ibid. p. 200.

‡ Ibid. p. 173. and seq.

and acids, may be transmitted in an invisible state from one vessel to another at a considerable distance, with no other communication than a small wire. How much more readily may carbon be supposed to pass through the thin membranes of the lungs.

But we are not left to mere probability to establish this point. Dr. Priestley found by experiment that venous blood became florid and emitted carbonic acid by the action of oxygen gas, though separated from it by the coats of a bladder scraped thin. Dr. Goodwin in some degree changed the blood in the veins of living animals to a florid colour, by directing a stream of oxygen gas against their coats.*

Mr. Ellis insists, that as the air cells and blood vessels are parts of distinct systems, there must necessarily be a greater distance between their contents, than there was in these experiments of Dr. Priestley and Dr. Goodwin. "If, indeed," he says, "air did permeate the bronchial cells, it would more readily pass into the cellular substance which connects them together, than into the pulmonary vessels."† I answer, in his own words, that this "supposition is wholly gratuitous, and in opposition to the results of direct experiment."‡ M. Bichat, speaking of forcing air into the trachea of dogs, says, "whenever air is forced into the lungs with a too great impetuosity, it occasions besides the passage of the fluid into the blood, its insertion into the cellular texture, where it spreads and produces emphysema of the breast, neck, &c. But if the impulsion is moderate, there is only the passage of the air in its natural state into the blood, and never a penetration of it into the cellular substance."§ The truth is, there is no cellular substance intervening between the coats of the cells and the blood vessels. Although they are physiologically considered as distinct from each other, yet they are too closely connected, and too delicate to be separated by the nicest dissection.

There is one other fact which in my view clearly proves the action of the chemical affinities of the blood and air through membranes, at least, as thick, as those of the air cells and capillary vessels. When the thorax of a dead body is opened, the lungs are found more or less filled with black

* Goodwin on the connexion of Life with respiration, p. 32, Philadelphia Edition.

† On Air Vol. I. p. 125.

‡ Ibidem.

§ Researches, translated by Watkins, p. 248.

blood. Let these lungs be freely exposed to the air, and in a short time the surface becomes florid.

These several cases prove pretty decisively, that the blood may act on the air by the force of its chemical attraction; and since the effects are chemical, and no other power has been discovered in the lungs, capable of producing such effects, I think it a fair presumption, that this is the power by which the changes in respiration are effected.

It will be objected that the time, in which a respiration is performed, is too short for so much to be effected by such an agency. But it ought to be remembered that the air inspired is mixed with what was before in the lungs. All experimenters agree that these organs contain more than a hundred cubic inches of air, when they are in the state of expiration; while, according to Messrs. Allen and Pepys, Mr. Davy and others, not more than 16 or 17 cubic inches are inhaled at an ordinary inspiration. The air is retained in the lungs, therefore, during a length of time equal to the performance of at least six respirations.

Besides, it is no unnatural conclusion to suppose, that the powers of life may aid in rendering the chemical affinities more efficient. Although we have perhaps no instance in the human body, where these powers originate an action, without the assistance of either chemical or mechanical means: there are many, where they do much to modify, and give effect to these means. Of this, the process of digestion is a signal example. It is probable that the various secretions are also effected in the same way.

Thus it appears that the only change which the air undergoes, in respiration, is to receive a portion of carbone, which combines with its oxygen. That this carbone is derived from the venous blood, is manifest from there being no other substance in the lungs capable of affording it, and from the certainty that the same effect is produced on the air by that fluid out of the body. The change therefore, which the blood suffers, is giving off a portion of its carbone, at the same time that its colour passes from a dark to a florid red.

It is not to be forgotten that it is only in regard to its ultimate principles, that this change in the blood is known to take place. As to the proximate principles of that fluid we know not in what the change, so necessary to the continuance of life consists. Indeed we are unable to perceive any difference except in colour, in the physical properties of the venous and the arterial blood.

How the loss of a portion of carbone should so change the nature of a fluid, in which that substance is still a large ingredient, as that from being absolutely destructive to life, it becomes every way adapted to the various purposes of nutrition and secretion, is altogether inconceivable. Nor can we hope to be better acquainted with this subject till we know more of the state of combinations in the animal and vegetable fluids. We find a few simple elements compose an almost infinite variety of fluids, yet the difference of composition which constitutes this variety, seems beyond the limits of chemical research.

When we add to this, our ignorance of the influence, which the impressions of vitality may have upon the solids and fluids of the animal body, we perceive a wide chasm between what we do know, and a complete understanding of the effects produced in the living system, by the changes, which the blood receives in respiration. We know indeed that the venous blood is an improper stimulus to the left side of the heart, and to the whole arterial system. But how a proportion of carbone in addition to what exists in arterial blood, too small to be discovered by chemical examination, should give such deleterious properties to this fluid, is, in the present state of human knowledge, altogether inexplicable.

The extrication of carbone from the blood is the only excretion we are acquainted with, where a single elementary ingredient is separated by itself; and the only one whose retention is attended with consequences so immediately fatal. A retention of the others eventually produces disease; but in this, the evil is suddenly destructive to life. The cause is hidden with the other secrets of living actions.

By ascertaining the changes in the elementary principles of the blood, one step is gained in physiological inquiry; but it is a step far distant from a perfect knowledge of the phenomena of respiration, as one of the operations of a living body. Yet this is the highest point, to which investigations of the subject have been, or perhaps can be, carried. All beyond is conjecture; and conjecture which only serves to shew the vanity of the mind that indulges it.

Hitherto I have said nothing upon the question of the production of animal heat by respiration. This subject involves so many considerations that it requires a separate discussion. Indeed a full examination of it would render a distinct treatise necessary.

If the view I have taken of respiration be correct; the only mode in which heat can be communicated by that process to the

blood, is by a change of capacity for caloric, in conformity to the theory of Dr. Crawford. The experiments of that gentleman are extensive and elaborate, and performed seemingly with every possible attention to accuracy. But in a series of complex processes, where the conclusions depend on a perfect observation of temperature to the tenth part of a degree, it ought not to be considered as disrespectful to him if we do not feel the fullest confidence in his results.

The quantity of matter in the air respired, is so small in comparison to that of the whole body, that it is difficult to conceive it sufficient to preserve the temperature of the body and supply its wastes. Then the air inspired is to be raised from the temperature of the atmosphere to that of the body, and the fluid exhaled from the lungs is to be converted into vapour by portions of the heat thus produced. Were we to suppose the change of capacity of the blood, to be equal to what Dr. Crawford makes it, the demand would be much greater than any supposable change in the air could possibly supply.*

Mr. Brodie's experiments appear to show, that respiration has no effect in producing animal heat. But similar ones performed in this country do not confirm his results. Yet these by no means prove that heat is the direct consequence of the performance of that function.

It is evident that it is only the reception of heat into the system in a latent state, which can be supposed to be a part of the offices of the lungs. The phenomena of its extrication to subserve the purposes of life do not belong to our present subject. Neither does it belong to us to inquire, whether the heat of the body may be supposed to be derived from the ingesta, or to be produced by the vital powers without regard to chemical means.

Uncommon case of Aneurism, by R. D. MUSSEY, M.D. Professor of the Theory and Practice of Physic at Dartmouth College.

[Communicated for the New England Journal of Medicine, &c.]

IN the month of June 1814, Jethro Freeman, a black man, called at my study in Salem, Massachusetts, and requested advice for a swelling in his neck, and an almost incessant dry cough. My friend and partner in business, Dr. Daniel Oli-

* See the Author's Inaugural Dissertation p. 26.