John W. Francis, from the author

AN

INAUGURAL DISSERTATION

ON

ANIMAL HEAT.

SUBMITTED TO THE PUBLIC EXAMINATION

OF THE

FACULTY OF PHYSIC

UNDER THE AUTHORITY OF THE TRUSTEES OF COLUMBIA COLLEGE, IN THE STATE OF NEW-YORK,

The Right Rev. BENJAMIN MOORE, D.D. President;

FOR THE DEGREE OF

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TO

RICHARD S. KISSAM, M. D.

Surgeon of the New-York Hospital,

IN

TESTIMONY OF HIS

PARENTAL CARE AND ATTENTION

IN HIS

MEDICAL STUDIES,

THIS

DISSERTATION

IS

RESPECTFULLY ADDRESSED

BY

HIS GRATEFUL PUPIL,

THE AUTHOR.

TO

JAMES S. STRINGHAM, M. D.

Professor of Chemistry and Legal Medicine
in Columbia College, New-York,

THIS

DISSERTATION

IS

RESPECTFULLY DEDICATED

BY

HIS MUCH OBLIGED FRIEND AND
HUMBLE SERVANT,

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THE phenomena of animal heat have engaged the attention of many eminent physicians from the earliest ages to the present day. Theory after theory has arisen, flourished, been neglected, and forgotten. The subject, however, is of the highest importance, and, as such, demands our serious attention.

In treating on this subject, I shall first briefly state the phenomena which take place in every living animal with respect to heat, and then notice the various theories that have been from time to time advanced as to the cause of animal heat, together with their proofs and objections.

There are various degrees of animal heat. Some animals preserve a heat of 100 or more of Fahrenheit's thermometer in all the different temperatures of the atmosphere. Others keep only a few degrees warmer than the medium which surrounds them; and, in some of the imperfect animals, the heat is scarcely one degree above the air or water in which they live. Animal heat is equally diffused over the body in health. Sometimes, indeed, the heat is less in the extremities; but this difference is very small, and may be attributed to their exposure. One fact is ascertained by the experiments of Du Hamel, Tillet, Dobson, Fordyce, and Blagden, viz. that the greatest degree of external heat will not materially alter the animal heat of the system, and, consequently, that it cannot be affected by the alterations of temperature of our climates.

Various theories have been advanced as to the cause of animal heat. Hippocrates accounted animal heat a mystery, and bestowed on it many attributes of the deity. Galen tells us that the dispute between the philosophers and physicians of his time was, whether animal heat depended on the motion of the heart and arteries, or whether, as the motion of the heart and arteries was innate, the heat was not also innate. He rejects both, and endeavours to explain it on the peripatetic philosophy; but his leading principles being erroneous, his deductions are, of course, inadmissible. The moderns have advanced many theories, some differing only in minute particulars, others in general causes; most of them, however, may be referred to one or other of the general causes of heat, viz. mixture, fermentation, and mechanical means, each of which we shall consider.

When chemical philosophy first came into vogue almost every operation in the animal machine was said to be the effect of mixture. From observing that on the mixture of certain bodies far below the temperature of the human body, a degree of heat was produced, sometimes rising to actual inflammation, they immediately pronounced mixture to be the cause of animal heat. Various opinions were entertained respecting the place where the mixture happened, as well as concerning the nature of the fluids of which it consisted. Van Helmont, Sylvius, and several others, supposed the mixture to take place in the intestinal tube, and ascribed it to an effervescence between the pancreatic juice and the bile. Others discovered acids in one place and alkalies in another, but the general opinion, for near two centuries, was, that ascescent fluids taken in and meeting with others of an alkaline nature, already prepared in the body, gave rise to the degree of heat peculiar to animals. Some again supposed it was owing to a mixture of chyle with the blood. But those who are in the least acquainted with the laws of the animal economy need not be told that these opinions are mere conjectures, founded on principles gratuitously assumed. If we should admit this hypothesis it would not account for the stability of heat in different climates and seasons, its equability over the body, nor hardly, indeed, for any one phenomenon attending the production.

Fermentation was observed to be generally accompanied with heat; and hence, when the former theory was given up this was adopted. Although there were various modifications of this opinion, yet, in general, it was confined to one species of fermentation, viz. the putrefactive. Although animal matter is very apt to putrefy, yet it is still very uncertain whether there is any heat evolved. M. Beaume and Dr. Pearson could not, even by the assistance of very sensible thermometers, discover any difference between the temperature of the surrounding medium and that of putrefying mixtures. If this theory were true we should expect to find the heat in proportion to the bulk of the putrefying mass; this, however, is not the case, for it has been found that the carcase of a whale in a state of putridity has not produced any sensible heat. But what at once overturns this hypothesis is, that heat is far more considerable in a living than in a dead body, and certainly putrefaction goes on more rapidly in the latter than in the former.

The opinion that animal heat was owing to friction took its rise from an observation, that it generally keeps pace with the state of the circulation. This was introduced soon after Harvey's discovery. Physiologists looked upon it as a matter almost capable of mathematical demonstration, yet they differed as to the manner in which the friction was produced. Dr. John Stevenson published, in 1771, the following, as some of the prevailing opinions: 1. That the heat of our bodies is owing to the mutual attrition of the arteries and blood. 2. That the lungs are the fountain of heat. 3. That the attrition of the parts of the solids on one another produces it. 4. That it is to be imputed to the mechanical attrition of the particles of our fluids. 5. That whole process by which our aliment and juices are constantly undergoing some alteration.

1. Those who hold that animal heat is owing to the mutual attrition of the arteries and blood, endeavour to prove it by the conical figure of the arteries, their flexures, and branchings into exquisitely small capillaries, the propelling power of the heart, and their strong resistance. From all these they infer, that the particles of the blood, perpetually getting new motions, directions, and rotations, are attenuated, condensed, have their angles grinded off, and are made homogenous: thence, they say, follow the

fluidity, red colour, and heat of the mass which is here perfected. They aver also that arterial blood is sensibly hotter than venous, which they consider as a further proof of the position. But objections to these may very easily be adduced—if the arteries were cones, the blood would move quicker the farther it was from the heart a *basi ad apicem*. The contrary is proved; the motion is slow in the lesser arteries: and it is altogether unphilosophical to say that heat should be produced by the attrition of a fluid on a solid, which is exemplified in falls of water, where there is great friction and no heat. The falsity of the notion that arterial blood is sensibly hotter than venous, is proven by the experiments of Dr. Crawford.

2. It was supposed by some that the lungs were the fountain head of heat in the human body; they adduce, in proof, that the numberless air vesicles in the lungs cause the blood to be pressed forwards and backwards until it becomes hot. In opposition to this opinion, the same arguments, with respect to the attrition of solids on fluids, mentioned in the preceding section, are applicable. Dr. Hook's experiment is much against the alternate motion of the vesicles being necessary to produce heat; he fixed the nose of a double pair of bellows in a wind-pipe of a dog, and cut away the ribs, diaphragm and pericardium, and made 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 in-

termitting the supply the dog would immediately fall into dying convulsive fits, and revive again as soon as the lungs were filled with fresh air. This serves to show that the animal can live and the circulation go on well when the vesicles are constantly and equally distended, and, consequently, that there is no necessity for that peculiar motion upon which the theorists of the day build their additional attrition.

3. That it is owing to the attrition of the parts of the solids on one another. The heart and arteries move most; and hence it seems probable that the heat should be owing to this cause. But this theory is liable to very important objections, as it is known that heat is never produced by the friction of solids, when a fluid is interposed: and this fact at once overthrows Dr. George Martin's theory, as it is founded on this erroneous principle.

4. That it arises from the mechanical attrition of the particles of fluids on one another. That this is not the case is universally allowed. Blood can never be heated, when out of the body, by the greatest motion or shaking possible. This, and many other experiments, prove that heat is never produced by the attrition of fluids.

5. That animal heat is caused by that process by which aliment and fluids are perpetually undergoing some alterations, or the animal process. In support of this opinion, Dr. Stevenson states that putrefaction is the most subtile of all dissolvents, effectually disjoining and separating all the component parts of putrefying bodies; and he imagines, in this powerful solution of bodies, that the intestine action of their minute particles creates, collects, or, in some

way or other, is the cause of heat. That this is not the case has already been shown.

Such were the theories in 1771. The anatomists and physiologists of that day all embraced one or the other, with various modifications.

Dr. Cullen, in his Institutes, advanced a new conjecture. May it not (says he) be supposed that there is some circumstance in the vital principle of animals which is common to those of the same class, and of like economy, and which determines the effect of motion upon the vital principle to be the same, though the motion acting upon it may be in different circumstances? The Doctor was driven to this supposition, from the difficulty of explaining how so many animals of different ages, sizes, and temperaments, should possess very nearly the same degree of heat. But we have no reason to suppose that the principle of life is different in different animals; and how are we to conceive that the same degree of motion should in one class of animals always produce a certain degree of heat, and in another class as regularly a different one? But to say that the vital principle can generate heat or cold, independent of mechanical or chemical means, is contrary to experience, and seems of itself absurd.

In the Philosophical Transactions, Dr. Hunter asserts, that certain animals entirely destitute of nerves are endowed with a power of generating their heat. This he brings as an argument against those who account the nervous system the seat of animal heat. But this remark is very doubtful, since we allow nerves to be in the most perfect animals where they cannot be seen; but we allow from their effects. Why should we then deny them to those in which we have the very same evidence, viz. sense and motion?

We come next to examine the theory of a great chemist, and one well acquainted with the animal economy. Dr. Black, having observed a close and striking connection between the state of respiration and the degree of heat in animals, so that they appeared to be in an exact proportion to one another, he was led to believe that animal heat depends on respiration, that it is all generated in the lungs by the action of the air upon the principle of inflammability, and from thence diffuses, by means of the circulation, over the rest of the vital system. This supposition is supported by many forcible arguments; it is well known that mephitic phlogisticated air, or carbonic acid gas, is constantly exhaling from the lungs of living animals. Since, therefore, atmospheric air, by passing through the lungs, acquires the very same properties as by passing through burning fuel, it is obvious that the change in both cases must be attributed to one and the same cause, viz. its combination with phlogiston. It has likewise been urged that the celerity with which the principle of inflammability is separated in respiration is very closely connected with the degree of heat peculiar to each animal; thus man, birds, &c. vitiate air very fast; serpents, and all amphibious animals, very slowly: the latter are of an inferior temperature to the former, and breathe less frequently. There are many proofs which evince the improbability of the lungs being the source of animal heat; for though it be granted, that there subsists a very striking connection between the state of respiration and the degree of heat in animals, and that they are even in proportion to one another, yet it by no means ensues that the former is positively the cause of the latter; for were that really the case, it is obvious, that those animals which are destitute of the organs of respiration would generate no heat: this, however, is not the case, for those fishes which are even destitute of gills appear, from various experiments, to be warmer than the ordinary temperature of the mediums in which they live. If the heat of animals be generated solely in the lungs, it follows, that the heat must decrease as it recedes from the supposed centre; a satisfactory evidence of which will doubtless be considered requisite to render Dr. Black's opinion probable. So far, however, are we from meeting with those positive proofs which we had reason to expect, that we are not presented with a single plausible argument in favour of it.

At the same time that Drs. Cullen and Black taught their respective theories, Dr. Duncan advanced another. This was soon after published by Dr. Dugud Leslie, formerly student of medicine at Edinburgh. This theory is the following. 1. That the blood contains phlogiston. 2. That this phlogiston is evolved, extricated, and brought in a state of activity and motion by the action of the blood vessels to which it is subjected in the course of the circulation. 3. That the evolution of phlogiston is a process which throughout nature produces heat, whether that heat be apparently excited by mixture, fermentation, percussion, friction, inflammation, agitation, or any similar cause. 4. That this heat, which must be produced in consequence of the evolution of the phlogiston from the blood of different animals, is, in all probability, equal to the highest degree of heat which these animals in any case possess. Dr. Duncan endeavours to prove the second proposition by the reddened colour of the blood. This, he says, is an evidence of the evolution of phlogiston, and that if heat arise from this, it must be greater when the blood is darker. Even if the first and second propositions were admitted, the third is liable to a very great objection, viz. that from putrefying bodies phlogiston is evolved in great quantity, as Dr. Dugud Leslie has acknowledged; yet he himself affirms, that no sensible heat is produced by putrefying animal substances. It is needless, however, to say more on this theory, since his fundamental principle, namely, that the vneous blood is warmer than the arterial, has been shown to be false by Dr. Crawford.

We shall now proceed to the consideration of Dr. Crawford's theory. He begins with saying, that heat is either a sensation of the mind, or the principle exciting the sensation; that this principle, or element of fire, is called, in philosophic language, *absolute* heat, and that sensible heat expresses the same, as *relative* to the effects it produces.

Heat is contained in great quantities in all bodies, when at the common temperature of the atmosphere.

Heat has the property of a constant tendency to diffuse itself over all bodies, till they are brought to the same degree of sensible heat. This, it is evident, can only be meant to apply to inanimate matter. As the animal body is constantly communicating heat to the surrounding medium, it must, of course, have the power of generating heat.

It has been observed, that the respiratory animals preserve a temperature higher than the surrounding atmosphere; but animals without respiratory organs are very nearly of the same temperature with the medium in which they live.

Among the hot animals, those are the warmest which have the largest respiratory organs, and which consequently breathe the greatest quantity of air in proportion to their bulk. The degree of heat is, in some measure, proportionable to the quantity of air inspired in a given time. Animal heat is, therefore, increased by exercise, and whatever accelerates respiration. On these grounds Dr. Crawford proceeds to defend the following propositions. 1. That atmospheric air contains a greater quantity of absolute heat than that which is expired from the lungs. In defending this axiom, he has asserted, that air is altered in its properties by respiration and combustion; it is diminished in its bulk; the air expired from the lungs occasions a precipitation in lime water; a part of it, therefore, consists of fixed air; and, by experiments, he determines that the last named air contains less absolute heat than atmospheric; he also ascertains by experiment, that oxygen contains more absolute heat than any other gas or combination of gases.

Dr. Crawford's second proposition is, that the blood which passes from the surface of the lungs to the heart, by the pulmonary veins, contains more absolute heat than that which passes from the heart to the lungs, by the pulmonary artery, which he proves by experiment.

Upon these facts, established by his numerous experiments, Dr. Crawford proceeds to an explanation of animal heat. In the process of respiration a quantity of heat is separated from the air, and absorbed by the blood; this disengagement of heat is owing to the union of the oxygen of the inspired air with the carbon of the blood. The emission of carbon occasions in the blood an increase of capacity; it absorbs the heat set at liberty by the decomposition of the air, and contains it in a latent state. To the extrication of carbon is owing also the florid colour of arterial blood. The blood thus charged with heat, acquires carbon in its circulation, and, in proportion to its acquisition, it gives out heat, and on a re-exposure of the blood to the action of the air in the lungs, the process is repeated.

It has been stated, in objection to this theory, and in support of the notion, that animal heat is owing to the absorption of oxygen, that the phosphoric, lithic, muriatic, and sebaceous acids are found in the body, and that the volume of air is diminished by respiration.

Mr. John Bell, who considers the existence of acids in the body a sufficient proof that oxygen is taken into the blood, has invented the following theory. If oxygen does communicate heat, it does so, not in the lungs, not to the blood, but to the whole body, through the medium of the blood; he mentions, that the oxydation of the blood is a very imperfect process, the blood bears no marks of an acid, the oxygen is but slightly attached to the blood, which admits of its very ready assimilation; and, according to the well known law of nature, that bodies, in passing from a rare to a dense state, give out heat, he thinks the generation of animal heat well explained. If oxygen enters into the blood, and is so slightly attached to it as Mr. Bell believes, we may, 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 enclosed 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 sunk 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.

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." This assertion is confirmed likewise by direct experiment. When Dr. Girtanner inserted oxygen gas into the jugular vein of a dog, he howled dreadfully, breathed quick, and died in three minutes. The existence of acids in the body can be explained upon Dr. Crawford's principles, and is not, consequently, an objection.

I shall now attempt an explanation of the aforesaid acids; and, first, the phosphoric, which we find united with lime. Phosphorus, blended with food, is taken into the system in large quantity; lime also. These articles, having an affinity to each other, combine, and constitute the phosphuret of lime.* This com-

* The phosphuret of lime has so strong an affinity for oxygen, that I have seen it decompose water at the temperature of 60°.

pound, having a very powerful attraction for oxygen, decomposes the watery part of our fluids; the oxygen of the water combines with the phosphorus, and produces phosphoric acid, and the acid unites with the lime to form phosphate of lime.

The two next mentioned acids, lithic and muriatic, have never yet been decomposed; they are, of course, at present, to be considered as elements, and as such must have been taken into the body. With respect to the sebacic acid, it is never found until after death; when putrefaction commences, a portion of the oxygen of the atmosphere comes in contact with the fat, and, by combining with it, constitutes sebacic acid.

I shall now proceed to the consideration of the diminution of bulk which atmospheric air undergoes in respiration.

This diminution of bulk was early noticed by Boyle, who estimated it at about one thirtieth of the air employed. Mayow, whose genius enabled him to anticipate so many discoveries of modern chemistry, confined an animal in a glass vessel inverted over water; 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 it was reduced about one fourteenth part of its bulk. In the experiments of Hales the degree of diminution varied from one thirteenth to one thirtieth of the whole air employed; and in those of M. Lavoisier from one thirty-first to one sixtieth 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 on passing lime water into the jar, the air was diminished one twenty-eighth 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 a solution of caustic potash was added to the residual air it became mild, and the volume of 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 when this acid is completely attracted by an alkaline fluid, the loss of bulk amounts nearly to one fifth of the whole air employed. 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 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.

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 Des Jurin, Hales, Haller, and Sauvages at 40 cubic inches. With the conclusion of these latter authors the experiments of Dr. Menzies nearly coincide. He procured two vessels of equal capacity; the one he exhausted by means of the air pump, and the other was filled with atmospheric air; they were so arranged that he inspired the air from one vessel, and expired into the other, and the quantities were found to be nearly the same. Dr. Menzies expired as naturally as possible, and, by computation, ascertained that the average bulk of air thrown out of the lungs by each expiration was 42.8 cubic inches. Several persons of the middle size repeated this experiment, with nearly the same result, the difference being scarcely 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 result.

Dr. Bostick says that 40 cubic inches is the greatest quantity of air employed in an ordinary act of 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 weakened, and unable, in consequence, to expel so much air as when in a state of health and vigour; 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 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, contained 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 agrees very well with that of Dr. Goodwyn, 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 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. 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 281. But the 41 cubic inches of air, when inspired at the temperature of 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 understood as indicating proportions only, the absolute quantities being different in different persons. These facts decidedly show how much the volume of air in the lungs will at all times depend on the relative capacity of these 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. It has been shown, that as carbonic acid is formed in respiration, the oxygenous portion of the air disappears: a number of experiments have been made to ascertain the proportion that the carbonic acid produced bears to the oxygen lost. This being an important point, I shall take the liberty of detailing some of them. Mr. Davy confined a small mouse in a jar, inverted over mercury, containing 15 cubic inches of atmospheric air, previously deprived of its carbonic acid by long exposure to caustic alkali. In fifty-five minutes the animal was taken out, apparently dying. Of the quantity of oxygen gas originally present in the jar, 2.6 cubic inches had disappeared, and 2 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 in one fourth of a minute, making one inspiration and one expiration seven or eight different times; and he found, by analyzing the respired air, that the quantity of oxygen gas lost in each respiration was from 5 to 6 cubic inches, and that the carbonic acid produced was from 5 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 $\frac{22}{100}$ oxygen gas originally present, $\frac{8}{100}$ had disappeared, and $\frac{7}{100}$ of the 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 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 arise only from some necessary connection between 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 was any such source, why should not this acid be expired in a quantity beyond what the residual air of the lungs will supply, 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 show, that the oxygen gas, which composes, in part, the carbonic acid formed in respiration, is derived from the respired air *alone:* and that the production of this acid bears always a *constant proportion* to the loss of *oxygen gas.*

Taking for granted, then, that the carbonic acid

* Mr. Davy 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.

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 acid produced, we proceed next to investigate the amount of this difference, and its cause. From the experiments of Drs. Priestley and Crawford, 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 principally 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 by 8 cubic inches. In this experiment, however, the air was not natural to the lungs, and the confinement of the animal was prolonged till he breathed with difficulty. We know that respiration is a function carried on by the exertion of muscular power, in a great degree obedient to the will. 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 those organs; and hence, we see, in all the foregoing examples, that the diminution in the volume of the 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.

In the experiments made by Mr. Davy on his own respiration, the oxygen that disappears every minute is estimated at 31.6 cubic inches, and the carbonic acid produced at 26.6, a difference in volume equal $\frac{1}{6}$.32; and Dr. Bostock concludes, that in twenty-four 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 may be asked, from what cause does this loss of bulk proceed? It is known, that by the conversion of oxygen gas into carbonic acid, in the process of combustion, a diminution of its bulk, amounting to about 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 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 employed. But this variation does not destroy our belief in the general fact; and if we consider that the correct observation of the fact itself requires an attention to many circumstances of much delicacy and difficulty, 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, a diminution of about one seventh of its bulk. And since this necessary diminution accounts nearly for the whole difference in volume between the oxygen lost and the carbonic acid produced in respiration, we may further conclude, that the whole of this oxygen gas has been employed to form the carbonic acid in question.

As the conversion of oxygen gas into carbonic acid is, at all times, going on in the lungs during the continuance of the respiratory process, it is next in order to examine how far the necessary diminution of bulk 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 variously estimated; and the cause of this variation in experiments made on animals confined in jars of air has been explained. It is our present object to inquire into that degree of diminution which takes place in the 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 one fiftieth, or sometimes only one sixtieth of its bulk, when expelled from these organs by the succeeding expiration. Dr. Menzies, taking the average amount of the loss which the air suffered by fifty-six successive respirations, observes, that the volumes of air received into and expelled from the lungs were nearly the same.

Several attempts were made by Dr. Davy to estimate the degree of diminution experienced by the air in a single inspiration, and it has been shown that his determinations greatly vary, 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 cubic inch, or one forty-third part their bulk. Dr. Bostock, on the other hand, concludes, that the air, by a single respiration, loses only one eightieth 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 be determined. 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 considered as sufficient to account for it.

Several attempts were made by Dr. Davy to estimate the degree of diminution experienced by the air in a single respiration, and it will be seen that his determinations greatly vary, according to the manner in which the experiment was made; in the only instance where all the circumstances were pretty natural, the 17 cubic inches of air which were inspired lost 0.5 of a cubic inch, or one thirty-third part their bulk. Dr. Bostock, on the other hand, concludes, that the air, by a sufficient [illegible], loses only one eightieth 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, [illegible] the process which we are [illegible] respiration, [illegible] which sensibly affect that process, we cannot but consider the actual loss of bulk which the air suffers by a single respiration as [illegible] extremely difficult, if not impossible, to be determined. Still, however, this process of general [illegible] is, that the difference between the volumes of air inspired and expired, in natural respiration, is exceedingly 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 considered as sufficient to account for it.