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AN

ESSAY

ON

RESPIRATION.

PARTS I. AND II.

BY JOHN BOSTOCK, M. D.

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TO

THE REV. JOHN YATES,

AS A TESTIMONY

OF RESPECT, GRATITUDE, AND

AFFECTION,

THIS ESSAY IS INSCRIBED,

BY

THE AUTHOR

PREFACE.

DURING the courſe of my attendance upon the fever-wards of this town, I had been frequently led to notice the rapid changes of temperature which the body experiences in this diſeaſe, and I felt deſirous to aſcertain how far they could be reconciled to the modern doctrines of animal heat. For this purpoſe, I entered upon a courſe of experiments, reſpecting the chemical ſtate of the reſpiration in fever, and I afterwards determined to extend my inquiries to other ſtates of the body, either natural or morbid, in which it might be ſuppoſed that this function would be affected. As however I was aware that the ſubject was not altogether new, it was proper to begin by making myſelf acquainted with what had been previouſly done by others, not only on this particular topic, but reſpecting reſpiration in general. It is well known, that this part of phyſiology has derived the moſt important aſſiſtance

from

from the modern diſcoveries in chemiſtry, and that it has alſo immediately engaged the attention of ſome of the moſt diſtinguiſhed among the philoſophers of the preſent period. Their labours have, for the moſt part, been peculiarly ſucceſsful, and by their means we have obtained an inſight into ſome parts of the animal œconomy, which were before concealed in impenetrable obſcurity. But the information that has been acquired, ſtill remains diſperſed through a great variety of publications, ſome of which are voluminous, and others not eaſily to be procured; and in conſequence of the rapid advances of chemical knowledge, it happens in a few inſtances, that the concluſions even of the moſt judicious phyſiologiſts are contradicted by ſubſequent experiments. I conceived therefore, that my firſt object would be to collect the beſt authenticated facts, and the moſt valuable opinions that had been advanced, and to arrange them in ſuch a manner, as to preſent a correct idea of the preſent ſtate of our knowledge.

The only connected view of the modern doctrines of reſpiration, which we have in our language, is contained in an article of the Supplement to the Encyclopædia Britannica, written by Dr. Thomſon; and a ſection in the ſyſtem of chemiſtry publiſhed by the ſame gentleman. I have demonſtrated my idea of the ability of this author by the frequent reference which I have made to his writings; but the nature of the works reſtricted him within very confined limits. It was not without a degree of ſurpriſe and regret, that I peruſed the account of reſpiration, in the ſyſtem of phyſiology which is compiled for the uſe of the firſt medical ſchool now in exiſtence, a new edition of which was publiſhed in the year 1791, under the immediate direction of a gentleman of talents and information, and under the patronage of a Univerſity, which ranked among its profeſſors the names of Monro, Black, and Rutherford. The lectures on the animal œconomy, which were delivered at Edinburgh by Mr. Allen, were indeed of ſuch a nature as to ſupply every deficiency; it is much to be lamented that they are now diſcontinued, and ſtill more, that there is no proſpect of their publication; had that been the caſe, the following pages would have been entirely ſuperceded.

Every one who is acquainted with the nature of phyſiological experiments upon the living body, may eaſily imagine, that as I advanced in my propoſed plan, many unforeſeen difficulties occurred, and many new ſubjects of inquiry preſented

ſented themſelves, ſo that the probable period at which I may expect to have them in a ſtate fitted to meet the public eye, ſeems ſcarcely leſs remote than at their commencement. Under theſe circumſtances, I have been induced to preſent to the public this eſſay in its preſent ſtate; ſhould any unforeſeen accident prevent the completion of my plan, I have ſome expectation, that this portion of it will not be without its uſe; it may at leaſt ſerve to remove ſome obſtacles from the path of the future inquirer, and may have an effect in directing the attention to one of the moſt intereſting departments of natural knowledge.

Liverpool,
Feb. 24*th,* 1804.

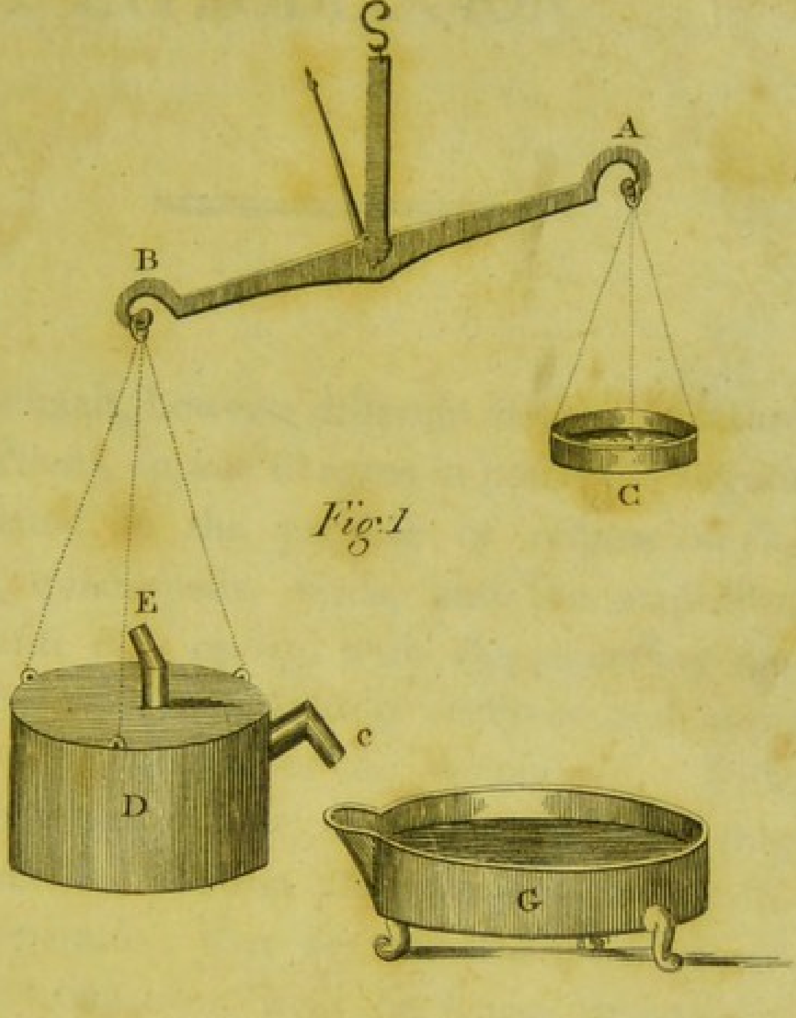

Fig: 1

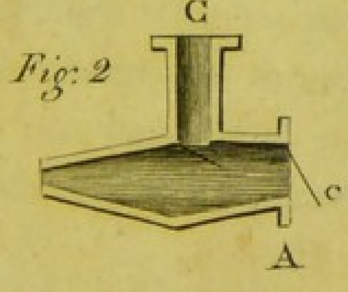

Fig: 2

INTRODUCTION.

ALL animals, however different in their ſtructure and functions, appear to agree in poſſeſſing organs appropriated to the purpoſe of reſpiration *(a)* In man, quadrupeds, birds, and the amphibia, they conſiſt of a cavity, with the neceſſary appendages, which alternately receives and emits a portion of the air of the atmoſphere. Fiſh, in conſequence of the medium in which they are immerſed, perform this function by a different apparatus; they are furniſhed with a paſſage communicating with the fauces or œſophagus, and terminating in the external ſurface of the body, through which a part of the water received into the mouth is forcibly propelled. In this paſſage, the branchiæ, or gills, are ſituated, and the blood which circulates in their fringed extremities,

(a) Note 1.

mities, is thus expofed to the action of a quantity of air, which the water always holds in folution. In many of the infects and lefs perfect animals, the refpiratory organs confift merely of a number of tubes or pores, provided with open mouths, and which fimply admit the external air to be received into them.*(a)* It appears, however, that notwithftanding the difference in the ftructure and mechanifm of thefe parts, they are all conducive to the fame operation, and anfwer the fame general purpofes in the animal œconomy.*(b)* In treating upon this fubject, I fhall begin by giving an account of the procefs of refpiration; I fhall in the fecond part point out its direct effects; in the third part, the different affections of refpiration will be noticed, whether occafioned by the various natural fituations in which the body is placed, or by the effects of morbid caufes operating upon the fyftem; I fhall conclude by inveftigating its ufes, and by an attempt to afcertain the connexion which fubfifts between refpiration and the other functions.

(a) Note 2. *(b)* Note 3.

PART

PART I.

AN ACCOUNT OF THE PROCESS OF RESPIRATION.

CHAP. I

A Description of the Human Organs of Respiration.

THE human organs of reſpiration are the lungs, the trachea, with its various ramifications, the pulmonary ſyſtem of ſanguiferous veſſels, and the diaphragm. The two firſt of theſe are the apparatus by which the external air is conveyed and received into the appropriate cells. The pulmonary arteries and veins are employed in carrying the blood through the lungs in ſuch a manner, that it may undergo the neceſſary change from the action of the air, while the laſt is the principal agent in the alternate enlargement and contraction of the cavity of the thorax.

The trachea, is a long tube, compoſed of cartilaginous rings, connected together by elaſtic ligaments; it commences in the fauces, and defcends into the thorax, where it divides into two branches, which communicate with the two divifions of the lungs. Each of thefe is fubdivided into fmaller branches, called the bronchia, which again ramify into others ftill more minute, until at length they terminate in what are called the veficles, or air cells of the lungs. In man, and the warm-blooded quadrupeds, the cartilaginous rings are incomplete at the back part, where they lie contiguous to the œfophagus; this deficiency is filled up by cellular fubftance, furnifhed with mufcular fibres. Thefe fibres are placed both longitudinally and tranfverfely, fo that by their contraction, the trachea is diminifhed, as well in its length as in its diameter. The action of the longitudinal fibres is alfo affifted by others, which pafs acrofs the intervals between each fucceffive ring. As the bronchia decreafe in fize, their cartilaginous rings become lefs and lefs apparent, and at length, when they terminate in the veficles, their ftructure is altogether membranous. A number of the air cells, with their accompanying veffels, are connected together by cellular fubftance, and form what are called lobules; thefe are again united into larger lobules, and thefe into divifions of more confi-

derable

derable ſize, which conſtitute the lobes of the lungs.(a)

The pulmonary ſyſtem of ſanguiferous veſſels, forms the moſt important part of the reſpiratory organs, and that to which all the reſt may be conſidered as ſubſervient. After the blood has completed its circulation through the body, it is returned by the vena cava to the right ſide of the heart. From this cavity, it is propelled through the lungs, along the branches of the pulmonary artery, which ramify with almoſt inconceivable minuteneſs over the bronchial air cells, forming what is called the rete mirabile of Malpighi; it is then collected by the pulmonary veins, and returned to the left auricle of the heart.

The pulmonary arteries and veins, with the air veſſels, and the accompanying lymphatics and nerves, are all connected together by cellular ſubſtance, and compoſe the lungs, two ſpongy maſſes which completely fill the thorax; the internal ſurface of this cavity is lined with a membrane, called the pleura, which is reflected over the ſurface of the lungs; it is aſcertained, that in all the different ſtates of reſpiration, the two parts of the pleura

(a) Note 4.

pleura are in perfect contact.(a) The diaphragm is a strong, expanded, and peculiarly irritable muscle, which separates the contents of the two great cavities of the body; in its natural state, it assumes an arched form, convex with respect to the thorax, but when in a state of contraction, its curvature is diminished, and consequently the cavity of the thorax becomes increased.

(a) Note 5.

CHAP.

CHAP. II.

A Description of the Mechanism of Respiration.

RESPIRATION confifts in the reception and emiffion of air into, and out of the bronchial veffels of the lungs, by means of the alternate enlargement and contraction of the cavity of the thorax. When the body is in its natural pofition, and the mufcles are relaxed, the lungs are in a ftate of expiration; but after they have continued in this condition for only a few feconds, we are under the neceffity of inhaling a quantity of frefh atmofpheric air. For this purpofe the diaphragm is contracted, and thus rendered lefs convex towards the thorax, by which means the capacity of the cheft is augmented. The intercoftal mufcles are, at the fame time, thrown into action; and by elevating the arch of the ribs, and thus increafing the diftance between the fternum and the fpine, ftill farther tend to enlarge the dimenfions of the thorax. The diaphragm is, however, the chief agent

agent in producing this effect;*(a)* and anatomists are generally agreed in supposing, that the use of the intercostals consists principally in fixing the lower ribs, so as to prevent their being drawn down by the action of the diaphragm, and thus obviating the effects of its contraction.*(b)* The contraction of this muscle in some degree displaces the viscera which lie contiguous to it, and pushing them against the parietes of the abdomen, causes a protrusion of the straight and oblique muscles which compose its anterior boundary. The capacity of the thorax being augmented, and a free passage being left by the trachea for the admission of the air, a portion of it will necessarily rush into the lungs, in order to maintain an equilibrium between the external and internal pressure.*(c)* The contraction of the diaphragm and intercostals soon ceases, the elasticity of the cartilages of the ribs re-acts, and the parts return to their natural state; at the same time a degree of contraction takes place in the muscles of the abdomen, and the viscera are restored to their former position; the thorax is thus reduced to its original capacity, and a portion of air is consequently expelled from the lungs.*(d)*

We

(a) Note 6. *(b)* Note 7. *(c)* Note 8. *(d)* Note 9.

We have now been defcribing what takes place in an act of ordinary infpiration; but this function, though fo effential to life that it cannot be interrupted for the fhorteft interval, without imminent danger, is, at the fame time, fo far under the control of the will, as that, according to inclination or neceffity, it may be exercifed in very different degrees. When we are defirous of making a full infpiration, befides the diaphragm and intercoftals, we call into action the external mufcles of the breaft, thofe of the fhoulder and other neighbouring parts, which, by elevating the ribs and fternum, ftill farther increafe the capacity of the thorax.(a) When, on the contrary, we wifh to produce a full expiration, the abdominal mufcles are more ftrongly contracted, the vifcera are thus pufhed more forcibly againft the diaphragm, and its convexity towards the thorax is increafed.

The grand object of refpiration is to effect a change in the blood, by the operation of the atmofpheric air; in order to accomplifh this end, it is neceffary, both that a frefh fupply of air be continually offered to the blood, and alfo that as large a furface of blood as poffible be prefented to the

air.

(a) Note 10.

air. The fabric and mechaniſm of the lungs are adapted to produce theſe effects in the moſt complete degree: the very act of reſpiration conſiſts in perpetually changing the air of the lungs, and the manner in which the blood of the pulmonary artery is diſtributed over the ſurface of the veſicles, in branches of almoſt inconceivable minuteneſs, affords the beſt opportunity to each particle of blood for undergoing the action of the air. Several phyſiologiſts, and among others Dr. Hales and Dr. Keill,*(a)* have attempted to eſtimate the number of the bronchial veſicles, and the extent of the membrane lining them. The reſults differ ſo much as to demonſtrate the fallacy of their calculations; but they agree in conceiving the membrane to be many times more extenſive than the ſurface of the whole body.

(a) Keill. Tent. Med. Phys. p. 80. *Hales's Stat. Essays. v. i. p.* 241.

CHAP.

CHAP. III.

Inquiry into the Bulk of a single Inspiration, and into the Capacity of the Thorax in its different States of Distension.

AFTER having obſerved the manner in which the act of reſpiration is effected, it naturally becomes an object of attention to determine the capacity of the lungs, and the quantity of air received and emitted by them. There are ſeveral circumſtances which render the latter part of this inquiry of peculiarly difficult reſearch, and notwithſtanding the labors of ſeveral ſkilful experimenters, the object of their inveſtigation has not been ſatisfactorily aſcertained. Owing to a difference in ſtature, and in the peculiar conformation of the thorax, probably alſo in the conſtitution and habits of individuals, great varieties prevail in the quantities of air reſpired by different perſons; and in the ſame perſon it is well known, that the reſpiration is materially influenced by the degree

of

of mufcular exertion, the ftate of the ftomach, the mental impreffions, and the powers of volition; all, therefore, that can be effected by experiment, is to afcertain the average quantity of air refpired in that ftate of the body, when it is leaft under the influence of external agents.

Many calculations were made by the older writers, to determine the capacity of the lungs, and the change which it experiences by refpiration, but as their eftimates were founded rather upon hypothetical reafoning than upon experiment, they were for the moft part remote from the truth. Borelli is the earlieft phyfiologift who eftablifhed an experimental inquiry into the quantity of air received by a fingle infpiration.*(a)* Dr. Jurin afterwards improved upon this method, and obtained refults which appear to have been tolerably accurate.*(b)* His experiments confifted fimply in receiving the air which was expelled from the lungs into a bladder, and by repeating this procefs for a fufficient number of times, he concluded, that the quantity of air employed in a fingle act of refpiration amounts to 40 cubic inches. No additional information feems to have been

(a) De motu Anim. p. 2. *prop.* 81.

(b) Halleri El. Phys. lib. viii. *sect.* 3. 8. *Hales's Essays v.* i. *p.* 243.

been acquired from the time of Jurin until the publication of Dr. Goodwyn's Eſſay on the Connexion of Life with Reſpiration.(a) This author enters fully into the diſcuſſion of the queſtions reſpecting the capacity of the thorax, and the quantity of air received in a ſingle inſpiration, and he inſtituted a number of ingenious and elaborate experiments for the purpoſe of aſcertaining theſe points.

His work commences with an attempt to meaſure the quantity of air left in the lungs after a complete expiration. Every animal, he remarks, immediately before death, produces a full expiration, and therefore, by aſcertaining the capacity of the thorax in the dead ſubject, we obtain a ſolution of the propoſed inquiry. The diaphragm being the only part of the cheſt which remains moveable, he endeavoured to fix it by applying a firm compreſs about the upper part of the abdomen. An opening was then made into the thorax, and the lungs, collapſing by their natural elaſticity, expelled the air which they previouſly contained, and thus left a cavity between the pleuræ, which he filled with water. This water he conceived would exactly meaſure the ſpace previouſly occupied

(a) Note 11.

occupied by the air contained in the lungs, and from the average of four different experiments, he concluded the quantity of air left in the lungs after a complete expiration, to be 109 cubic inches.*(a)*

Dr. Goodwyn next attempted to aſcertain the quantity of air received into the lungs by a ſingle inſpiration, and for this purpoſe he uſed the following apparatus.*(b)* D is a veſſel with two openings, through one of which E he inſpired, while the other c was immerſed in water contained in the diſh G; a quantity of water equal to the volume of air inſpired will neceſſarily paſs into the veſſel D, and from the weight of this water, it is eaſy to eſtimate the bulk of the air taken into the lungs. By taking the average of thirty inſpirations, and by uſing the neceſſary precautions, the quantity appeared to be twelve cubic inches, which when it entered the lungs, he ſuppoſed would, by their higher temperature, be expanded to fourteen cubic inches: fourteen cubic inches therefore he eſtimates to be the quantity of air employed in an ordinary act of reſpiration. Having previouſly found the quantity

(a) Note 12. *Connexion of Life with Respiration, p.* 26.

(b) Vide Fig. 1.

tity of air left in the lungs after expiration to be 109 inches, he concludes, that the difference of capacity which the lungs experience in the alternations of refpiration, is in the proportion of 109 to (109+14) 123.(a) Thefe numbers are much lefs than the quantities affigned by preceding writers, particularly by Dr. Jurin; it will therefore be proper to examine how far the experiments and obfervations of Dr. Goodwyn may be found objectionable.

The pofition, that every animal makes a complete expiration before death, which forms the bafis of his experiments, is in the main true; yet there is every reafon to fuppofe, that it will require many reftrictions before it can be admitted as the firft ftep in a phyfiological calculation. According to the nature of the difeafe or accident producing death, the ftate of the lungs muft vary confiderably. In many inftances, the refpiration is directly impeded by an obftacle preventing the ingrefs and egrefs of the atmofpherical air out of the bronchia; in fome cafes, the air when admitted, is prevented from undergoing its appropriate change from the action of the blood, while in a large clafs of difeafes, life is extinguifhed by the

(a) P. 28 & seq.

the gradual ceſſation of the powers, which enable the heart to propel the blood through the pulmonary veſſels. To determine preciſely, what is the different condition of the lungs in theſe different circumſtances, would require a long and difficult inveſtigation ; it is ſufficient for the preſent purpoſe to ſtate theſe differences, as it is evident that they muſt produce different effects upon the organs of reſpiration.

The method which Dr. Goodwyn employed to retain the form of the thorax, lies open to ſome objections. With whatever care the compreſs be applied about the region of the diaphragm, there is ſtill reaſon to believe, that a ſufficient power might puſh it down upon the viſcera contiguous to it, and ſo cauſe them to protrude into the loins, the groin, or ſome other part of the parietes of the abdomen, to which the bandages were not applied. Mr. Coleman farther urges againſt Dr. Goodwyn's experiments, that after a complete expiration, the diaphragm is raiſed as high as the 4th or 5th rib, but that a compreſs about the upper part of the abdomen, would be incapable of retaining it in this poſition, and that the ribs themſelves will alſo deſcend by the preſſure of the water admitted into the cavity of the

thorax,

thorax, and will consequently draw down the whole substance of the diaphragm. (a)

Dr. Goodwyn appears also to have been mistaken in conceiving, that a complete collapse of the lungs is produced by the admission of water into the cavity of the thorax. (b) A degree of pressure will in this case be exercised upon the surface of the lungs, which will produce a material contraction in their bulk, but it will not obliterate the cavities of the bronchial vesicles, an effect which must take place before all the contained air can be expelled. Both from the shape and texture of these organs, and from actual experiment, we know that scarcely any degree of external force can so far evacuate the contents of the lungs, as to render them specifically heavier than water. (c) Upon the whole we may conclude that Dr. Goodwyn's estimate of the capacity of the lungs after a complete expiration is not unobjectionable, yet the experiments must be considered as valuable, and as affording results probably not very remote from the truth.

The author will be found to be less fortunate

in

(a) *Coleman on Resp. p.* 89. (b) *Goodwyn, p.* 24.
(c) *Boer. prælect. t.* v. *p.* 2, *p.* 458. *Petit, Mem. Acad.* 1733.

in his ſecond ſubject of inquiry, viz. reſpecting the quantity of air received by a ſingle inſpiration. He remarks himſelf, that in breathing through the tube E, the water is raiſed out of the veſſel G, contrary to the power of gravity, and that a greater effort is therefore neceſſary to receive the due quantity of air into the lungs. *(a)* He conſequently attempted to obviate this ſource of inaccuracy, but the nature of the apparatus ſeems to render this impracticable. Dr. Menzies has alſo pointed out another cauſe of error in this experiment; when the mouth is removed from the tube, the external air will immediately ruſh in and repel part of the water into the veſſel G, and conſequently the meaſure of an inſpiration will be eſtimated below the truth; that this actually takes place Dr. Menzies found by adding valves to the apparatus. *(b)*

A more important error into which Dr. Goodwyn has fallen, conſiſts in the comparative eſtimate which he forms of the capacity of the lungs in the ordinary ſtates of inſpiration and expiration. He calculates the quantity of air left in them after a complete expiration to be 109 cubic inches, and he ſuppoſes that 14 inches are received

(a) *Goodwyn, p.* 33, 4. (b) *Menzies on Reſpiration, p.* 19.

received at each inſpiration ; hence he infers, that in the ordinary proceſs of reſpiration, the lungs experience a dilatation and contraction from 123 to 109 cubic inches. *(a)* But it is obvious that the author here confounds together an ordinary and a complete expiration.

The quantity of air emitted from the lungs is, within certain limits, under the control of the will, ſo that we are enabled by a voluntary effort to expel a conſiderable quantity of air after an ordinary expiration. The amount of this quantity has been variouſly eſtimated, from 220 to 70 cubic inches; we ſhall probably be not far from the truth in fixing it at 170 cubic inches. *(b)* Yet it is certain, that after the moſt powerful voluntary effort of expiration, the lungs will not be completely emptied, they will then be only reduced to the ſtate in which Dr. Goodwyn made his experiments, when he found them to contain 109 cubic inches.

Admitting therefore the juſtice of his calculations, the inference which he draws from them will be inadmiſſible; the difference of capacity in the ſtates of expiration and of inſpiration will

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(a) Goodwyn, p. 36, 7. *(b)* Note 13.

be, the quantity of air that can be expelled after an ordinary expiration, (which I have eſtimated at 170 cubic inches) added to 109, the quantity ſtill remaining in the lungs, in all 279 cubic inches; and 279 added to 14, the meaſure of a ſingle inſpiration, equal to 293 inches; but we ſhall find that this meaſure of a ſingle inſpiration is not correct. From this view of Dr. Goodwyn's work it appears, that though his experiments were ingeniouſly contrived, and appear to have been carefully executed, yet that ſome of his moſt important concluſions are erroneous.

A ſhort time after the appearance of Dr. Goodwyn's work, Dr. Menzies publiſhed his Eſſay on Reſpiration. He begins by inſtituting an inquiry into the quantity of air employed in a ſingle act of reſpiration, and alſo into the capacity of the thorax, as Dr. Goodwyn had previouſly done; but he arrives at concluſions which are materially different, and which there is every reaſon to conſider as much more nearly approaching the truth. He procured an allantoid capable of containing 2400 cubic inches, which was fixed to a tube furniſhed with two valves. *(a)* The allantoid was placed at the valve c, ſo as to receive

(a) Vide Fig. 2.

ceive only the air of expiration, and he continued breathing into it until it was completely filled. By comparing the number of expirations with the bulk of the allantoid, he calculated that each confifted of fomewhat more than 40 cubic inches. *(a)* This refult he confirmed by fixing another allantoid filled with air to the tube C, and from obferving the number of infpirations which this fecond allantoid afforded, he found the bulk of each infpiration to correfpond nearly with his previous eftimate of a fingle expiration. Not fatisfied with thefe calculations, Dr. Menzies had recourfe to experiments of a different kind. A man was placed in a large veffel of water, warmed nearly to the temperature of the blood, and the degree of afcent or defcent of the water in the upper part of the veffel being accurately noted, from thefe data it was eafy to form an eftimate of the alteration in fize which the thorax undergoes during refpiration. *(b)* The trials made with this apparatus correfponded remarkably with thofe performed by means of the allantoid, and thus afforded a ftriking proof of their correctnefs. In the experiments of Dr. Menzies there appears indeed no affignable caufe of error, and their coincidence with each other, and alfo with thofe of Jurin

(a) Menzies, p. 30. *(b) p.* 24—30.

Jurin mentioned above, muſt be conſidered as a powerful argument in favor of their accuracy.

In attempting to eſtimate the capacity of the lungs, Dr. Menzies adopts Dr. Goodwyn's eſtimate of 109 cubic inches, as the meaſure of the air left in them after a complete expiration, and adding to this quantity 70 cubic inches, which he found he could ſtill expel after an ordinary expiration, he ſuppoſes 179 inches to be the bulk of the thorax in the ſtate of ordinary expiration; to this he adds 40 cubic inches, which gives 219 as the capacity of the lungs after an ordinary inſpiration. *(a)* Theſe calculations of the bulk of the thorax are probably below the truth, as there is no doubt that conſiderably more than 70 inches can be expelled from the lungs by employing a powerful effort of expiration.

Mr. Davy in his reſearches concerning the nitrous oxide, performed ſome experiments for the purpoſe of aſcertaining the capacity of the lungs in their different ſtates of diſtenſion, and the quantity of air received into them by a ſingle inſpiration. In order to determine this latter point, he merely practiſed reſpiration in a peculiar pneumatic

(a) Menzies p. 32.

pneumatic apparatus, until he had acquired the power of breathing in it without any extraordinary labour or attention, in a perfectly natural manner. From a number of trials he conceived, that at each inſpiration he took into the lungs no more than 13 cubic inches. *(a)* He alſo attempted to diſcover the capacity of his thorax in a ſtate of complete expiration; he proceeded to the ſolution of this problem in the following manner. He endeavoured to prove by previous experiment, that when a quantity of hydrogene is reſpired it is neither abſorbed by the blood nor in any way altered, but is ſimply mixed with the air contained in the lungs. He accordingly inſpired a quantity of this gas, and then making a complete expiration, he aſcertained what quantity of the hydrogene he had diſcharged from his lungs, and what proportion it bore to the other gaſes expelled at the ſame time. From theſe data he determined what quantity of hydrogene was ſtill retained, and ſuppoſing it to have been uniformly mixed with the whole air in the lungs, he eaſily calculated the abſolute volume of air ſtill remaining in them. Making the due allowances for the expanſion which it would experience from the heat of the body, he ſuppoſes that the capacity of

(a) *Researches, p.* 433.

of his lungs in a ſtate of complete expiration amounts to no more than 41 cubic inches. *(a)*

It is difficult to reconcile theſe eſtimates with thoſe of Dr. Menzies, which were apparently deduced from accurate and well contrived experiments. With reſpect to Mr. Davy's method of determining the bulk of a ſingle inſpiration, we may remark in general, that the quantity of air taken into the lungs varies ſo much at different times, and depends in ſo great a degree upon the will, that other circumſtances being the ſame, thoſe experiments will ſtand the beſt chance of accuracy in which an average is taken from a conſiderable number of inſpirations; in this particular the experiments of Dr. Menzies poſſeſs a decided ſuperiority. There is alſo another circumſtance to be noticed with reſpect to the peculiar apparatus employed by Mr. Davy, which may be ſuggeſted as a probable cauſe of inaccuracy. He informs us that he attempted to breathe in the mercurial air-holder in ſuch a manner that the reſpiration might require no unuſual effort, and he concludes, that in this caſe the function would be exerciſed in the ſame manner as in its natural ſtate. But in breathing into this apparatus,

(a) *Researches, p.* 409.

ratus, notwithſtanding the nice adjuſtment of its parts, ſome degree of reſiſtance is to be overcome, which does not exiſt in ordinary reſpiration, and therefore if the ſame degree only of exertion be employed, a leſs quantity of air will certainly be emitted from the lungs. It would require a number of ſucceſſive reſpirations greater than appears to have been employed by Mr. Davy, before any painful ſenſation will ariſe from this impediment; uneaſineſs in the organs of reſpiration is produced more readily by an alteration in the quality than the quantity of air received into them.

The eſtimate which Mr. Davy has formed of the capacity of the lungs, in the ſtate of complete expiration, differs ſo materially from other direct experiments, and is ſo incompatible with the ideas which are derived from a conſideration of the anatomical ſtructure of the thorax, that it is impoſſible to avoid ſuſpecting the exiſtence of ſome error or fallacy in the proceſs which he employed. The only conjecture which occurs to me, in any degree likely to remove the difficulty, is the ſuppoſition, that the hydrogene was not diffuſed uniformly through all the cavities of the lungs; a ſuppoſition which is in ſome degree countenanced by the conſideration of the different ſpecific gravities of the gaſes, and which derives confirmation

confirmation from the experiments of M. Jurine, of Geneva. He received the air of one expiration into four different veſſels, and he found the different portions of air to exhibit different chemical properties. According to the order in which they had been expelled from the lungs, they contained a greater proportion of oxygene, and a ſmaller of carbonic acid. Hence it may be conjectured, that in the experiment of Mr. Davy, the hydrogene which he had inſpired, had not mixed itſelf with the air contained in the veſicles, in the ſame proportion as in the larger branches of the trachea, conſequently the air upon which the experiment was made, would contain a greater proportion of hydrogene than the average of the whole air contained in the lungs, and the eſtimate of their capacity, taken from the quantity of hydrogene, would be neceſſarily below the truth. *(a)*

The experiments which I have related of Goodwyn, Menzies, and Davy, are the moſt accurate that have been performed upon the mechaniſm of reſpiration; it will however be proper to notice the eſtimates of ſome other authors, even though we may find them to be leſs ſatisfactory.

An

(a) Note 14.

An intereſting account is given in the Encyclopedie Methodique *(a)* of ſome experiments performed by M. Jurine, on the chemical change produced upon the air by reſpiration in different conditions of the body. In the courſe of the detail, he is led to mention the bulk of a ſingle inſpiration, which he eſtimates at 20 cubic inches; this the editor, M. Hallé, conſiders as too large a quantity; but there is no mention made of the reaſons upon which the opinions either of M. Jurine, or of M. Hallé are founded. Mr. Kite, in his eſſay on ſubmerſion, ſtates the quantity of air received by an ordinary inſpiration to be 17 cubic inches, *(b)* and M. Delametherie, the learned editor of the Journal de Phyſique, reduces it to a much leſs quantity, *(c)* in neither inſtance, however, are we informed of the experiments by which theſe reſults were obtained. Mr. Abernethy employed an inverted jar filled with mercury, and by reſpiring as much as poſſible in the ordinary manner, he expelled from the lungs 12 cubic inches. *(d)*.

Mr. Coleman, in the courſe of his work on ſuſpended reſpiration, is led to compare the capacity

(a) Article Medecine, t. i. *p.* 493, *& seq.* *(b) p.* 47.
(c) Journal de Physique, t. xlvi. *p.* 108.
(d) Essays, p. 142.

city of the lungs after drowning, with their capacity in a ſtate of health, and thus to diſcover the diminution which the cavity of the thorax experiences in a ſtate of complete expiration. He conducted his experiments by applying a ligature about the trachea of an animal which he had previouſly drowned, he then detached the lungs from the body, and preſſed out all the air into an inverted jar of water. He afterwards inflated the lungs, and noted the quantity of air they were capable of receiving when fully diſtended; theſe two quantities he compared together *(a)*. The reſults of his experiments were very different from what might previouſly have been expected; the lungs of a cat, which when inflated were capable of holding 16 drachms of air, after drowning, were found to contain only half a drachm; and in a dog, which he killed by hanging, the proportions were ſtill more extraordinary, nearly as forty-three to one. We have no reaſon to doubt either the faithfulneſs with which theſe experiments are related, or the accuracy with which they were performed, yet, as the lungs always exactly fill the cavity of the thorax, they cannot be reduced in capacity without a proportionable reduction in the ſize of the cavity containing

(a) p. 91. *& ſeq.*

containing them, and it ſeems almoſt inconceivable, that ſo enormous a difference could take place in the capacity of the thorax of the ſame animal. *(a)* We may therefore conjecture that there exiſts a ſource of fallacy ariſing from the nature of Mr. Coleman's experiments. I ſhall point out ſome circumſtances which may, in part at leaſt, remove the difficulty.

When an animal is killed by interrupting the paſſage of the air into the bronchial veſſels, whatever may be conſidered as the immediate cauſe of death, it is found, that a conſiderable portion of the air which is left in the lungs is converted into carbonic acid gas. According to the plan upon which Mr. Coleman conducted his experiments, a part, at leaſt, of this gas muſt neceſſarily have been abſorbed by the water through which it was received, and upon which it was lodged; conſequently his eſtimate of the quantity of air left in the lungs will be leſs than the truth. This abſorption of carbonic acid gas may tend in ſome degree to reconcile the apparent contradiction in Mr. Coleman's experiments, but the circumſtance which I ſhall next point out is more extenſive in its operation, and will have a much more important effect upon our reaſonings.

When

(a) Note 15.

When an animal is in the act of drowning, the ſenſe of ſuffocation which is experienced produces a violent effort to expire. All the muſcles which can conduce to this effect are brought into ſtrong action, and contracted to the utmoſt, in order to expel from the lungs as large a quantity as poſſible of the air which is contained in them. Theſe muſcles, however, becoming exhauſted by their efforts, are quickly relaxed, and the thorax returns nearly to its natural dimenſions. In conſequence of the ſituation in which the animal is placed, the external air has no acceſs to the lungs, and on this account the air which remains in them becomes conſiderably rarefied. Owing to this circumſtance, a portion of the mucous fluid which lines the membrane of the veſicles is converted into an aqueous gas, which is mixed with the rarefied air, and fills the cavities of the lungs. *(a)* The uneaſineſs of the animal continues to encreaſe, and after a ſhort time, a ſecond violent effort to expire enſues, and a part of this rarefied air, together with ſome of the aqueous vapour, is forced out. This proceſs is repeated until a large proportion of the air originally contained in the lungs is excluded from them, and the powers of the animal being completely exhauſted,

(a) Note 16.

haufted, the refpiratory organs ceafe to act. It is obvious on the firft view of the fubject, that in the method of experimenting employed by Mr. Coleman, all the aqueous vapour was entirely difregarded in his eftimate of the contents of the lungs. When they were removed from the thorax and expofed to the preffure of the atmofphere, the greateft part of this vapour would be immediately condenfed, and any portion of it which ftill retained its elaftic form, would be totally deftroyed in its paffage through the water contained in the inverted jar *(a)*. It may be farther remarked, that owing to the circular fhape of the bronchia and veficles, and more efpecially to the cartilaginous texture of the former, it is impoffible by mere preffure to expel all the air which they contain. When they have once been rendered pervious by infpiration, no degree of compreffion can render them fpecifically heavier than water; it is even doubtful whether this effect can be produced by placing them in the vacuum of the air pump: *(b)* but it does not appear that thefe precautions were employed in the experiments now under confideration.

From the circumftances which I have pointed out,

(a) Note 17.
(b) Petit Mem. Acad. 1733, *Boer Prælect. t.* v. *p.* 458.

out, it is evident that Mr. Coleman has eſtimated below the truth the capacity of the lungs in their ſtate of complete expiration, and there is reaſon to apprehend, that in determining their capacity in the ordinary condition of inſpiration, he has fallen into the oppoſite error. In order to aſcertain this point, he detached the lungs from the body, and after diſtending them with air, meaſured their contents by preſſing it, as in the former experiment, into a graduated veſſel filled with water. From the ſtructure of the lungs, and the elaſticity of their ſubſtance, it is not improbable that when they were in this manner fully diſtended, they would be ſtretched to a ſize beyond what they occupied while contained in the cavity of the thorax. *(a)*

Theſe remarks upon the method by which the proceſs of reſpiration is effected, will ſhew, that though phyſiologiſts are agreed as to the mechaniſm by which the air is received into and ſent out of the lungs, yet that with reſpect to their relative capacity in the different ſtates of reſpiration, their opinions and experiments are much at variance.

Before we can arrive at a perfect knowledge of

(a) Boer. Instit. § 603. *Blumenbach Instit. Phys. p.* 11.

of this part of the ſubject, it will be neceſſary to aſcertain preciſely the 5 following points. 1ſt. The quantity of air received into and ſent out of the lungs in an ordinary act of reſpiration. 2d. The proportion which this bears to the quantity of air contained in the thorax after an ordinary expiration, which may be conſidered as the natural condition of the thorax. 3d. The quantity of air which can be ſtill expelled after an ordinary expiration; and 4th. The proportion which this bears to the air ſtill left in the lungs, which will meaſure their ſtate of complete expiration. Laſtly. The quantity of air which can be taken into the lungs by the greateſt effort of inſpiration.

The firſt of theſe points is, I apprehend, aſcertained by the experiments of Dr. Menzies. Though the calculations of other phyſiologiſts are conſiderably different, yet, there ſeems no obvious ſource of inaccuracy in the proceſſes which he employed, and the coincidence of the different experiments with each other is certainly too remarkable to be merely the effect of accident. Reſpiration is an act ſo much influenced by external cauſes, that we can never expect to attain any certainty as to the quantity of air ordinarily received into the lungs, without taking the average of a confiderable number of infpirations. By means of the allantoid, Dr. Menzies was enabled to compare together 56; in this refpect alone his experiments poffefs a decided fuperiority over thofe of any other phyfiologift. The coincidence which took place between the refults of his experiments performed with the allantoid, and thofe in which a man was placed in a veffel of warm water, convey to the mind an almoft irrefiftible conviction of their truth. We may therefore conclude, with refpect to the firft of thefe points, that 40 cubic inches is the quantity of air employed in an ordinary act of refpiration. *(a)*

The proportion which an ordinary expiration bears to the quantity of air ftill left in the lungs has been varioufly eftimated. The calculations of the older writers upon this fubject feem to have been very incorrect. It appears that in performing their experiments they removed the lungs from the thorax, and then obferved what quantity of air they were able to force into them, in order to their complete diftention; but the uncertainty of this mode of proceeding is fufficiently obvious. Dr. Menzies eftimated the medium capacity

(a) Note 18.

capacity of the lungs in a ſtate of ordinary expiration at 179 cubic inches; I have already ſtated the reaſons which induce me to conſider this quantity as under-rated, and to conceive that the lungs in their natural condition contain about 280 cubic inches of air.

The experiments and obſervations which have been made to determine the quantity of air left in the lungs after a complete expiration, have been pretty fully detailed, and it appears, that notwithſtanding the ingenuity and labour which have been exerciſed upon this point, it muſt ſtill be conſidered as undetermined. Upon the whole, Dr. Goodwyn's eſtimate of 109 cubic inches is the neareſt approximation to the truth, though, probably, not altogether correct.

From the above data it may be eſtimated, that by each ordinary expiration $\frac{1}{7}$ part of the whole contents of the lungs is diſcharged, and that by the moſt violent expiration, ſomewhat more than $\frac{4}{7}$ of the air contained in them is evacuated. Suppoſing that each reſpiration occupies about 3 ſeconds, (a) a bulk of air nearly equal to three times the whole contents of the lungs will be expelled

(a) *Haller*, viii. 4, 29. *Thomson's Chemistry*, v. iv. p. 485.

expelled in a minute, or about 4114 times their bulk in 24 hours. The quantity of air refpired during the diurnal period, will be 1152000 cubic inches, or 666½ cubic feet.

CHAP.

CHAP. IV.

Inquiry into the cause of the first Inspiration, and of the Alternation of Inspiration and Expiration.

IN viviparous animals refpiration commences immediately after birth, when the circulation which had been previoufly carried on between the mother and the fœtus is interrupted. *(a)* The manner in which the commencement of this important action is effected, and the change which is then produced in the animal conftitution, fo as to render this function abfolutely neceffary for the continuance of exiftence, have been frequently made the fubject of inveftigation. The latter of thefe inquiries is the foundation of the celebrated problem which was propofed by Harvey for the confideration of his contemporaries. "Quomodo nempe embryo," he afks, "poft "feptimum menfem in utero matris perfeveret? "cum

(a) Note 19.

"cum tamen eo tempore exclusus statim respiret; "imo vero sine respiratione ne horulam quidem "superesse possit; in utero autem manens ultra "nonum mensem, absque respirationis admini- "culo, vivus at sanus degat." *(a)* This question may be stated still more generally; why is the animal which has once respired, under the necessity of continuing the respiration without intermission; when, if the air had never been received into the lungs, the same animal might have remained for some time without exercising this function?

Several of the older physiologists proposed answers to this question, but they were founded either upon theories which are now universally discarded, or upon mistaken notions respecting the use of respiration. Dr. Whytt's hypothesis is the first which I shall notice; it is not indeed free from objections, but it deserves attention on account of the reputation which it long maintained in some of the most distinguished schools of medicine.

This author commences his inquiry into the cause of the first inspiration by remarking, that before

(a) Harveii Exerc. de gen. anim. p. 361. *Haller,* viii. 5, 1.

before birth, the blood of the mother is properly prepared for the uſe of the fœtus, but that when the communication between them is interrupted, it becomes neceſſary for the young animal to perform the requiſite alteration in its fluids, by means of its own reſpiration. As the uneaſy ſenſations of hunger and thirſt excite in us a deſire of food, when it is wanted for the nouriſhment of the body, ſo immediately after birth, from the want of freſh air in the lungs, there ariſes an uneaſy ſenſation about the thorax, which may be conſidered as the appetite for breathing. This appetite he conſiders as natural to the newly born animal, and the expanſion of the cavity of the thorax is effected by the ſentient, active principle, in order to prevent the fatal conſequence which would enſue from the deficiency of air in the lungs. Dr. Whytt proceeds to obſerve, that this appetite for air commences only at birth, becauſe, in conſequence of the ſtruggles of the fœtus at this period, its circulation will be conſiderably quickened, and the lungs will receive a larger ſhare of blood than previouſly paſſed through them; this ſtimulates them into action, and ſeems to be the immediate cauſe of the appetite for breathing. He farther urges, that the circumſtance of the animal being now immerſed in a medium which is proper for reſpiration, and

having

having the air applied to the face, mouth, and noſtrils, may cauſe it to attempt this new action of the thorax. Upon the whole he conſiders breathing "as owing to a peculiar ſenſation of "the body, which determines the mind or ſenti-"ent principle to put certain muſcles or organs "in motion."

With reſpect to the problem propoſed by Harvey, which was mentioned above, he obſerves it "to be of ſo very eaſy ſolution, that it is not "a little ſurprizing, that many phyſiological "writers ſhould have attempted it in vain." When the fœtus has once breathed, he remarks, that the blood which formerly paſſed through the foramen ovale & ductus arterioſus, now paſſes through the veſſels of the inflated lungs, and will ever after continue to flow through them. If, therefore, the reſpiration be diſcontinued, the blood will ſtagnate in theſe veſſels, and the animal will be ſuffocated, becauſe, according to his hypotheſis, the alternate motion of the lungs in reſpiration, is abſolutely neceſſary to carry on the circulation of the blood through them. *(a)*

Haller conceives that the fœtus, having before birth been in the habit of ſwallowing a portion of

(a) On Vital Motions, Sect. 9.

of the fluid in which it is immerſed, after it leaves the uterus, continues to open its mouth for the reception of nutriment, when a portion of the air with which it is now ſurrounded, is neceſſarily taken into the lungs. The veſicles being thus expanded, permit the blood to paſs through the pulmonary veſſels, which was formerly tranſmitted by the foramen ovale and ductus arterioſus. The lungs then acquire the condition of thoſe of other breathing animals, and like them require a regular ſupply of freſh air to prevent the blood from ſtagnating in its paſſage from the right to the left ſide of the heart. *(a)*

The following ſolution of this difficulty is propoſed by the celebrated author of the Zoonomia. Dr. Darwin imagines that the power of deglutition is acquired by the fœtus before it leaves the uterus, in conſequence of its repeated attempts to ſwallow a part of the liquor amnii, in which it is immerſed ; but the act of deglutition is of ſo different a nature from that of inſpiration, that he thinks this latter cannot be acquired before birth. When the circulation is interrupted between the mother and the fœtus, that diſagreeable ſenſation is experienced by the infant, which ariſes

(a) Note 20.

arifes from the want of frefh air ; all the mufcles of the body are put in motion to relieve this anxiety ; and among others, thofe of the breaft, ribs, and diaphragm. The action of thefe being found to anfwer the defired end, " refpiration is " difcovered," and the fame action is ever after repeated when the difagreeable fenfation recurs. *(a)* It will be unneceffary to enter into a formal refutation of thefe theories ; though they may be entitled to the praife of ingenuity, they are evidently built upon the affumption of principles which are at leaft doubtful, if nor untenable. The queftion concerning the commencement of refpiration is fo intimately connected with the ideas which we entertain refpecting its ufes, that we can fcarcely expect a complete folution of the difficulty until we have previoufly afcertained the purpofes which this function ferves in the animal œconomy. There are, however, fome peculiarities in the ftructure and mechanifm of the refpiratory organs of the fœtus, which it will be proper to notice in this place.

The lungs of an animal which has never refpired, are of a firm and compact texture, and fo condenfed, as to have a fpecific gravity greater than that of water ; whereas, no mechanical preffure

(a) *Zoonomia, v.* i. *Sect.* 16, 4.

ſure is found ſufficient to evacuate them, ſo far as to cauſe them to ſink in water, after they have been once inflated. (a) In this ſtate the pulmonary arteries and veins receive only ⅓ part of the quantity of blood which circulates through them, after they have been diſtended by the act of inſpiration. (b) The cavities of the bronchia and air veſicles are nearly obliterated, and that part of them which remains pervious, is filled with a peculiar mucous fluid. (c) As the lungs are in all caſes contiguous to the parities of the thorax, it is obvious, that the ſize of the cavity containing them, while in this condenſed ſtate, muſt be proportionably contracted. Accordingly we find that the arch of the ribs is depreſſed, by which means the ſternum is made to approach nearer to the ſpine, the diaphragm is puſhed up into the higher part of the cheſt, and its concavity towards the abdomen is conſiderably greater than it is ever found to be in an animal which has once reſpired. (d)

The ſpace occupied by the lungs themſelves is ſtill farther diminiſhed by the great ſize of the heart in the fœtus, and by the interpoſition of the thymus

(a) *Boer. prælect. t.* v. *Pars 2a. p.* 457. *Petit, Mem. Acad.* 1733. (b) *Haller in Boer. Præl. t.* ii. *p.* 172. (c) *Petit, ubi supra.* (d) *Senac, Mem. Acad.* 1724.

thymus between the laminæ of the mediaſtinum. *(a)* This contracted ſtate of the thorax is in part alſo produced by the poſture of the fœtus before birth; the head is bent forwards, ſo that the chin reſts upon the breaſt, the ſpine is arched, and the knees are raiſed up to meet the face. *(b)* The abdominal viſcera are therefore puſhed ſtrongly againſt the diaphragm, and ſo far increaſe its curvature, that a conſiderable portion of them is forced into the region of the thorax: the liver, which in the fœtus is unuſually bulky, *(c)* is entirely concealed by the ribs. As ſoon however as the animal leaves the uterus, its poſition is changed, the limbs and the trunk are ſtraightened, and the preſſure conſequently removed from the thorax and abdomen; the elaſticity of the cartilages of the ribs being at liberty to act, raiſes the arch of the thorax, and thus encreaſes the diſtance between the ſternum and the ſpine; the liver and the reſt of the abdominal viſcera fall down into their proper ſituation, and permit the diaphragm to deſcend and aſſume the curvature which is natural to it after birth.

While

(a) Haller, xxix. 4. 33. *do. in Boer. Præl. t.* v. *Pars* 2*a, p.* 459.

(b) Harveii Exer. de gener. anim. p. 353. *Denman's Midwifery, v.* i. *p.* 293.

(c) Haller, ubi supra.

While theſe changes are going forward in the relative poſition of the contents of the thorax and abdomen, the increaſed ſize of the former cavity cauſes the air to ruſh through the open mouth of the animal into the bronchial veſicles; whence the blood, meeting with leſs reſiſtance to its paſſage through the veſſels of the lungs, than through the foramen ovale is propelled along the pulmonary artery. The reſpiratory organs are then in their natural condition, or in the ſtate of moderate expiration, and the neceſſity for an inſpiration depends upon the ſame cauſe, which renders the alternation of expiration and inſpiration eſſential to the future exiſtence of the animal.

It appears therefore that the firſt degree of expanſion which is produced in the lungs of the fœtus, is not effected by muſcular contraction, but depends merely upon the removal of external preſſure, which permits the different parts of the thorax and abdomen to aſſume their natural poſition; the conſequence of this new ſituation of the viſcera is an encreaſe of the capacity of the cheſt, and this ſpace is immediately filled with a portion of the ſurrounding air. Any farther encreaſe of the ſize of the thorax muſt be effected by the contraction of the diaphragm and intercoſtals; perhaps in ſtrict propriety this muſcular

cular contraction ought to be confidered as the firft beginning of the action of refpiration.

Nearly allied to this inveftigation concerning the commencement of refpiration, is a queftion which has alfo been the fubject of much difcuffion among phyfiologifts, viz. what is the immediate caufe of the fucceffive alternations of infpiration and expiration ?

The folutions which have been attempted of this queftion, have been no lefs numerous than thofe refpecting the caufe of the firft infpiration, but as the fubject is in itfelf perhaps more obfcure, fo we fhall find the explanations which have been advanced, ftill lefs fatisfactory. I fhall mention the opinions of Haller and Whytt, which are the leaft exceptionable, and are ftill received by many refpectable phyfiologifts.

Haller, proceeding upon the fuppofition that in expiration the paffage of the blood through the lungs is impeded, obferves, that a preffure upon the brain, and a reflux of the blood through the veins, will be produced from this circumftance. Hence arifes a neceffity for encreafing the capacity of the thorax, in order to remove the compreffion of the pulmonary veffels, and in

confequence

consequence of a painful sense of anxiety or suffocation, proceeding from a partial stagnation of the blood, the will calls into action the muscles of inspiration, which remove the impediment to the transmission of blood through the lungs, and consequently the painful feeling which was caused by the obstruction. The same uneasy sensations are however produced by inspiration, when protracted beyond a certain period; the muscles therefore, which were acting to enlarge the thorax, cease to contract, their relaxation is followed by a state of expiration, which continues until the resistance to the passage of the blood recurs, and brings back the consequences stated above. Haller considers the action of the muscles of respiration as altogether voluntary. *(a)*

Dr. Whytt offered an explanation in many respects similar to that proposed by Haller. He conceives, that the passage of the blood through the pulmonary vessels, is impeded by expiration, and that a sense of anxiety is produced from this cause; this unpleasant feeling acts as a stimulus upon the nerves of the lungs, and excites the energy of the sentient principle, which by causing a contraction of the diaphragm, enlarges the

(a) El. Phys. viii. 4. 18, 27, 28. *Not. ad Boer. Prælect. t.* v. *p.* 120.

the thorax, affords a free paſſage to the blood, and thus removes the uneaſy ſenſation ; when the ſtimulus no longer exiſting, the muſcles ceaſe to act. *(a)*

The queſtion under conſideration embraces two diſtinct ſubjects of inquiry ; we have in the firſt place to aſcertain the cauſe of the neceſſity for the admiſſion of freſh air into the lungs, and ſecondly, we have to inveſtigate the connexion which ſubſiſts between this cauſe and the contraction of the diaphragm. The firſt of theſe is ſo intimately connected with the opinions which we may adopt reſpecting the uſe of reſpiration, that the farther examination of it muſt be neceſſarily poſtponed to a ſubſequent part of the eſſay. I ſhall therefore conclude this chapter, with a few obſervations upon the latter part of the inquiry, viz. the nature of the connexion which ſubſiſts between the peculiar ſtate of the lungs produced by an interruption to the reſpiration, and the contraction of the diaphragm.

As the alternate motion of the thorax in reſpiration, proceeds without interruption in all the different conditions of the body, it is improper, in attempting to aſſign a cauſe for it, to have

(a) *Whytt on Vital Motions, sect.* 8.

have recourſe to any effects which are produced only in extraordinary circumſtances; we cannot therefore eſtabliſh our hypotheſis, upon a ſtate of the lungs which takes place only in long continued or laborious reſpiration. On this account the firſt poſition in the hypotheſes of Haller and Whytt is inadmiſſible, ſince it ſuppoſes the exiſtence of a peculiar ſtate of the pulmonary circulation, which does not appear to take place, unleſs where the muſcles of reſpiration have been acting in the moſt violent manner. It may be doubted, whether in ordinary caſes an uneaſy feeling of ſuffocation actually occurs, as we are only ſenſible of this effect, when the lungs are compreſſed or diſtended in an unuſual degree, or when the air has been too long retained in them without alteration. Admitting however the exiſtence of this uneaſy feeling, and aſſuming it as the efficient cauſe of the ſucceeding enlargement of the thorax, we ſhall hereafter endeavour to aſſign a more probable cauſe for it than the ſtagnation of the blood in the lungs.

One of the properties which, in an eſpecial manner, diſtinguiſhes the living body from every other kind of matter, is its irritability, or the power which certain parts of it poſſeſs of contracting themſelves under peculiar circumſtances.

This power refides in the mufcular fibres, and is never exerted unlefs when called into action by the application of a ftimulus. Stimuli are external or internal; under the firft denomination are comprehended a great variety of bodies which have fcarcely any property in common, except their effect upon the mufcular fibre. The internal ftimuli confift of affections originating in the nervous fyftem, of the paffions and emotions of the mind, and in a remarkable manner of the exertions of volition. The motions which refult from thefe contractions have been varioufly arranged, either according to the caufe producing them, or to the purpofes which they ferve in the animal œconomy. One of the moft common, and at the fame time, one of the moft important divifions, is into voluntary and involuntary motions, an arrangement obvioufly founded upon the fhare which the will has in their production. There are a very confiderable number of animal motions, which are intermediate between the two extremes; thefe are to a certain extent produced by the application of external ftimuli, and confequently independent of volition; yet within certain limits we have it in our power to encreafe or reftrain them. Animal motions which are produced by external ftimuli, are diftinguifhed from each other, according to the

manner

manner in which the ſtimulus is applied; in general the motion is excited in the part upon which the ſtimulus immediately acts, while in other inſtances, the ſtimulus is applied to a part which has little apparent connexion with that in which the motion is ultimately produced. Almoſt all the voluntary actions are of this complex deſcription, and ſome of thoſe alſo which are produced by the agency of external ſtimuli.

There is a claſs of motions of very peculiar importance in the animal œconomy, which have obtained the name of inſtinctive actions; they ſeem to originate primarily from the operation of external ſtimuli, but the effect is generally produced in a part different from that to which the ſtimulus was applied. In the firſt inſtance they appear to be totally independent of volition, though, after they have ſubſiſted for ſome time, the will acquires the power of modifying their extent and duration. The proceſs of deglutition, the motion of the eye lids, and the evacuation of the inteſtinal canal, may be adduced as inſtances of this ſpecies of animal action. Here, the animal immediately after birth, produces an effect depending upon the co-operation of a complicated ſyſtem of muſcular contractions; the efficient cauſe, appears to be a ſtimulating body applied to a part often remotely connected with the mufcles which are excited into action, where the animal, muft of neceffity, be entirely ignorant of the propofed effects, and perhaps fcarcely confcious of their exiftence. *(a)*

To apply thefe obfervations to the action of the thorax : it appears, that when the blood has remained for fome time in the pulmonary veffels, without the accefs of frefh air, it undergoes a change, either in its compofition or its diftribution, which caufes it to act as a ftimulus to the diaphragm, and excites this mufcle to contract. The connexion between this change in the ftate of the blood, and the contraction of the diaphragm, we are unable to explain, and we can only recognize it from its effects ; whether it be produced by an alteration in the chemical properties of the blood, or whether it depend upon an accumulation or deficiency of it in any part of the thorax, whether the action be directly upon the diaphragm or its nerves, or whether it be brought about by the intervention of any neighbouring organ, are queftions which the prefent ftate of our knowledge will not permit us to anfwer. But in whatever manner the contraction

of

(a) Note 21.

of the diaphragm be produced, the confequent enlargement of the thorax admits the accefs of air into the pulmonary veficles; the blood is thus enabled to undergo the neceffary change, and the ftate which caufed the contraction of the diaphragm no longer exifting, this mufcle becomes relaxed, and expiration enfues. If the juftice of thefe remarks be admitted, we may divide refpiration into three diftinct fpecies, each of which originates from a different principle of action. What has been juft defcribed, which may be confidered as the ordinary procefs of refpiration, is the firft fpecies: in this the effect feems to depend upon the action of a ftimulus applied to an irritable part. When, however, from any accidental or morbid caufe, the regular train of actions is interrupted or impeded, fo as to prevent the blood from undergoing the neceffary change in its compofition, an uneafy fenfation arifes, and we are urged by an inftinctive feeling to encreafe the contraction of the intercoftals and diaphragm, and to employ the external mufcles of the thorax. Thefe motions, which produce the fecond fpecies of refpiration, muft however, be ftill confidered as independent of volition, becaufe they take place to the fulleft extent in thofe cafes where we cannot fuppofe the powers of the will to be exercifed either directly, or by the intervention of aſſociation. As however we gradually advance in life, we acquire a degree of voluntary power over the muſcles which are ſubſervient to reſpiration, and can ſo far counteract the irritative, or inſtinctive motions, as very conſiderably to accelerate or retard their action. In this manner is produced the third ſpecies of reſpiration, or that which depends upon the exerciſe of volition.*(a)*

(a) Note 22.

PART

PART II.

THE DIRECT EFFECTS OF RESPIRATION.

THE direct effects of respiration may be conveniently arranged under three divisions; the mechanical effects produced by the dilatation and contraction of the thorax; the change produced in the inspired air; and the alteration which the blood experiences during its passage through the capillaries of the lungs.

CHAP. I.

The Mechanical Effects produced by the Dilatation and Contraction of the Thorax.

THE procefs by which the cavity of the thorax is alternately dilated and contracted, and a portion of the air of the atmofphere received into and emitted from the bronchial veffels of the lungs, has been already defcribed. I propofe in the prefent chapter to examine what effects will be produced upon the contiguous parts, by the perpetual change of bulk which the thorax experiences. The older phyfiologifts, whofe knowledge refpecting the function of refpiration, was for

for the moſt part, very limited, and who were entirely ignorant of the nature of the chemical changes produced upon the air and the blood, were led to direct their attention more particularly to its mechanical effects. The queſtion, whether the blood was tranſmitted through the lungs with more eaſe during the ſtate of inſpiration or of expiration, was minutely diſcuſſed, and a number of experiments were performed to decide upon this point; but they will be found to apply rather to what takes place in extreme caſes, than to the ordinary proceſs of reſpiration. When attempts have been made to reſolve this queſtion by a direct experiment upon a living animal, the loſs of blood, the pain which was neceſſarily inflicted, and more particularly the agitation into which the organs of reſpiration were thrown, render the reſults of comparatively little value, ſo far as they relate to the ordinary action of the thorax; though we certainly learn from them, that the blood is tranſmitted through the lungs with more facility in the extreme ſtates of inſpiration than of expiration. In an experiment performed by Dr. Hales, where a tube was inſerted into the crural artery of a horſe, the blood was evidently propelled to a greater height when the animal ſighed deeply, which muſt be attributed to the encreaſed capacity of the lungs, permitting the blood

blood to paſs more freely through the pulmonary veſſels. *(a)* But though from this experiment, and others of a ſimilar nature related by Haller, it ſeems proved, that in theſe extraordinary caſes, the paſſage of the blood through the lungs is affected by their ſtate of diſtention, we muſt not conclude that this effect is produced in the uſual alternations of reſpiration. There are indeed ſtrong reaſons in ſupport of the contrary opinion. In the healthy ſtate of the body we reſpire about twenty times in a minute, while the average velocity of the pulſe is about eighty, ſo that the heart contracts four times during one act of reſpiration, *(b)* and conſequently it muſt receive its ſupply of blood from the lungs in all the different conditions of diſtention to which they are ſubject, yet we do not find that the pulſe is affected, either with reſpect to its ſtrength or its velocity. *(c)* It is difficult to produce any effect upon the pulſe by the moſt powerful voluntary efforts of inſpiration or of expiration, yet in theſe caſes, the cavity of the thorax certainly undergoes a much greater change in its dimenſions, than that which it experiences in its ordinary action.

The contrary doctrine was however, maintained

(a) Statical Essays, v. ii. *p.* 6. *(b) Haller,* viii. 4. 29. *Boer. Præl. t.* v. *p.* 128. *(c)* Note 23.

tained by Haller; he ſuppoſes, that during expiration, the bronchia are, to a certain degree, plaited or folded up, but that when the lungs are inflated, theſe folds are opened, and the blood veſſels rendered more eaſily permeable. *(a)* This opinion, which was generally adopted by Haller's contemporaries, *(b)* and by many of them exaggerated in a moſt extravagant degree, *(c)* ſeems to have ariſen from the following circumſtances. 1. They greatly over-rated the change of bulk which the thorax experiences in the different ſtages of reſpiration. If we average the contents of the thorax at 280 cubic inches, and the bulk of a ſingle inſpiration at 40, we shall have its cavity encreaſed or diminiſhed by $\frac{1}{7}$ part only; whereas the older phyſiologiſts conceived, that by every act of inſpiration, the cavity of the cheſt was doubled, quadrupled, or increaſed in even a ſtill greater proportion. *(d)* 2. In the experiments and obſervations which were made upon the dead body, they conſidered only the effects which are produced by a complete evacuation of the lungs, and then applied theſe to explain the effects of ordinary reſpiration. *(e)* 3. Haller and his friends appear to have been miſled by

(a) El. Phys. vi. 4. 10; viii. 4. 11; viii. 5. 21. *&* *alibi*; *notæ ad Boer. Prælect. t.* ii. *p.* 176. *(b)* Note 24. *(c) Bell's Anatomy, v.* ii. *p.* 188. *& seq.* *(d) Haller,* viii. 4. 11. *(e)* Note 25.

by the experiments which they performed upon living animals; they perceived that the tranſmiſſion of blood through the lungs was influenced by their different ſtates of diſtention, but they neglected to make the due allowance for the unnatural ſituations in which the ſubjects of theſe experiments muſt neceſſarily have been placed. Dr. Goodwyn, in order to controvert this opinion reſpecting the ſtate of the lungs after expiration, produced an artificial hydrothorax in dogs, by introducing a quantity of water between the pleuræ. He found in ſeveral inſtances, when he had filled nearly $\frac{1}{3}$ of the cavity of the thorax, that the paſſage of the blood was not apparently retarded, though the reſpiration was rendered laborious. *(a)* Upon the whole I think there is reaſon to conclude, that in the uſual act of reſpiration, the blood is tranſmitted through the lungs at all times with nearly equal facility, and that it is only in extreme caſes that the retardation deſcribed by Haller can be ſuppoſed to take place. *(b)* If any conſiderable impediment had been oppoſed to the entrance of the blood into the pulmonary veſſels during the ſtate of expiration, it muſt neceſſarily have produced upon the pulſe a correſponding effect, which would have been perceptible to the touch. *(c)*

This

(a) Goodwyn, p. 45. Note 26. *(b)* Note 27. *(c)* Note 28.

This doctrine of the older physiologists respecting the transmission of the blood through the lungs, gave rise to an opinion, which was almost universally adopted by them, that the motion of the thorax in respiration was an important agent in promoting the circulation of the blood. This hypothesis will be considered more fully, when we come to treat of the uses of respiration; I shall in this place only remark, that the same considerations which lead us to conclude that the lungs are nearly alike permeable to the passage of the blood in all the stages of ordinary respiration, must prove that their motion can have little or no effect in accelerating its progress through their vessels.

The œsophagus passes through the muscular portion of the diaphragm, and is so interlaced by the crossing of its fibres, that it seems as if the tube must necessarily be compressed by their contraction. (a) Hence it has been supposed, that during inspiration the passage to the stomach will be contracted or entirely obstructed; but here, as in the former case, it is doubtful whether this effect be produced, except in the more violent efforts of inspiration.

Some

(a) *Haller*, viii. 1. 36. xviii. 4. 9. *Senac, Mem. Acad.* 1729. *Winslow's Anat. Sect.* 3. *Art.* 13. *Bell's Anat. v.* i. *p.* 326.

Some of the older phyfiologifts imagined that important effects would refult from the action of the diaphragm, upon the aorta and vena cava. The aorta, however, defcends into the abdomen, between what are called the crura of the diaphragm, and feems to be fo well protected by the arching of thefe mufcular appendages, as to be altogether defended from the effects of compreffion. *(a)* There feems ftill lefs probability of ftrangulation being produced in the vena cava, as it not only pierces the diaphragm in its tendinous part, but paffes through a large opening, the edges of which are furnifhed with a peculiar ftructure, which would feem to prevent any alteration in its fhape or dimenfions. *(b)* Yet notwithftanding this ftructure, Haller obferved in the courfe of his experiments, that certain pofitions of the diaphragm fo far compreffed the vena cava, as to impede the paffage of the blood. *(c)* But we may conjecture that this effect was produced by the unnatural circumftances in which the animal was placed during the experiment ; we may alfo attribute fomething to the difficulty of making accurate obfervations upon animals fuffer-

ing

(a) Senac, & Bell, ubi supra.

(b) Winslow, Senac, & Bell, ubi supra.

(c) Haller, vi. 4. 10; viii. 1. 36; viii. 5. 23.

ing extreme torture, and convulſed by the near approach of death.

The older anatomiſts aſcribed very important effects to the ſucceſſive compreſſion and relaxation of the nerves which paſs through the diaphragm, or along its ſurface. The alternate motions of the thorax, and even of the heart itſelf, were aſcribed to this circumſtance, but later obſervations have proved that it was inadequate to the propoſed effect, and have alſo pointed out cauſes, which are more agreeable to the analogy of the animal œconomy. The par vagum, and great ſympathetic nerves, both paſs through the ſmaller muſcle of the diaphragm, *(a)* and it has been conjectured, that by the alternate contractions and relaxations of this organ, they are compreſſed at regular intervals, and thus the tranſmiſſion of the nervous energy is interrupted, ſo as to produce the vermicular motions of the ſtomach and inteſtines. I have already noticed the hypotheſis of Dr. Martine, who conceived, that the compreſſion of the phrenic nerve by the lungs, when in a ſtate of inſpiration, was ſufficient to account for the ſucceeding relaxation of the diaphragm,

(a) Haller, viii. 1. 35, & *seq. Winslow's Anat. Sect.* 6. 137, 400.

phragm, and confequent emiffion of air from the thorax.

The alternate enlargement and contraction of the bulk of the thorax, in confequence of the change in the diaphragm, muft neceffarily produce a correfponding change in the pofition of all the abdominal vifcera. The agitation and preffure which they will from this caufe experience, has been generally fuppofed to be inftrumental in propelling the blood along their veins; *(a)* but this effect, if it exift at all, has been much over-rated. From the experiments of Dr. Menzies, in which the body was immerfed in water for the purpofe of afcertaining the quantity of air employed in a fingle infpiration, it was found that the increafe of bulk which the body experiences, is exactly equal to the volume of the air received by the lungs. *(b)* Hence we muft conclude, that the cavity of the abdomen is at all periods of the act of refpiration of an equal capacity, the encreafe of bulk which the abdomen would acquire, in confequence of the relaxation of its anterior mufcles, being exactly balanced by the contraction of the diaphragm, and confequently the preffure exercifed upon the vifcera, muft

(a) Haller, viii. 5. 23. *(b) Menzies, p.* 24, *& seq.*

muſt be altogether, or nearly ſimilar in all the different conditions of the thorax. But a uniform preſſure upon the veſſels can have no effect in accelerating the flow of the blood; the veins are in this caſe reduced to the ſtate of rigid tubes, and their contents can only be propelled by the momentum which they poſſeſſed previous to their arrival at this part of the circulation. An encreaſe of preſſure under theſe circumſtances, muſt rather retard than promote the flow of the blood.

This view of the ſubject is countenanced by the deficiency of valves in theſe veſſels, a circumſtance in which the veins of the abdominal viſcera differ from thoſe of moſt other parts of the body. *(a)* The veſſels which lie near the ſurface, or contiguous to the great muſcles, and which conſequently are at all times peculiarly expoſed to the effects of irregular preſſure, are plentifully furniſhed with ſemilunar valves. When, therefore, by an extraneous cauſe, the capacity of any part of a vein is diminiſhed, the blood which it contained muſt neceſſarily be driven forward towards the heart, and upon the removal of the compreſſion, the veſſel regaining its natural

(a) Haller, ii. 2. 18, 21.

natural capacity will afford a more eafy admiffion to the fucceeding column of blood. But the acceleration which is produced in this cafe, depends entirely upon the prefence of the valves; when, therefore, we find a fyftem of venous veffels in which they are wanting, we may infer that the veins in queftion are not much expofed to the effects of preffure, and that when preffure is applied, it will not accelerate the motion of the fluids which they contain. *(a)*

Haller infifts ftrongly upon the effects which are produced in the liver by the contraction of the diaphragm. He conceives that it is a principal agent in propelling the blood along the hepatic veins, and more particularly in preffing the bile out of the gall bladder: he fuppofes that this organ is not furnifhed with any mufcular fibres, and that confequently its contents can only be evacuated by external preffure. There feems every reafon to conclude that the effects which he defcribes will be produced in the more violent and fudden motions of the diaphragm, particularly thofe which occur in the act of vomiting. *(b)*

 The

(a) Haller, iii. 2, 3; & viii. 5. 23. *Bell's Dissections, v.* i. *p.* 48. *(b) Haller,* viii. 5. 23. & xxiii. 3. 29.

The alternate action of the thorax is alſo ſuppoſed to exerciſe a conſiderable degree of compreſſion upon the lacteals, and conſequently to promote the flow of the chyle. *(a)* The remarks made above with regard to the effect of the diaphragm upon the veins of the abdominal viſcera, apply to the preſent caſe, ſo far as reſpects the uniformity of the preſſure in the ordinary act of reſpiration. But the lacteals differ from the veins of the viſcera in being furniſhed with numerous valves, *(b)* it is therefore probable that the chyle will be propelled towards the thoracic duct, in the more violent action either of the diaphragm, or the muſcles of the abdomen, and ſtill more in thoſe voluntary exertions where both theſe parts are ſtrongly contracted at the ſame time. But it is only in the more unuſual ſtate of the reſpiratory organs, that this effect is produced; in ordinary caſes it is probable, that the progreſſive motion of the chyle is effected ſolely by the muſcular action of the veſſels themſelves. The thoracic duct ſoon after its commencement, lies under what are called the crura of the diaphragm, and it is ſuppoſed by Haller, that the paſſage of the chyle along the duct is materially promoted by the contraction of this part. *(c)* But it

(a) Senac, Mem. Acad. 1724. *(b)* Note 29.
(c) Haller, xxv. 2. 6,

it is probable that in this, as in the former inſtance, it is only in the more violent exerciſe of the reſpiratory organs that any effect can be produced upon the motion of the chyle, and that in ordinary circumſtances, the muſcularity of the veſſel is adequate to the propulſion of its contents. As a deciſive argument in favour of this opinion, it has been obſerved, that the chyle has ſtill continued its progreſſive motion along the thoracic duct after the thorax has been laid open, and conſequently the effect of external preſſure entirely removed. (a)

From the above remarks, we are led to conclude, that the effects produced by the dilatation and contraction of the thorax, which takes place in ordinary reſpiration, are much leſs conſiderable than was conceived by the older phyſiologiſts. In the experiments which they performed upon living animals, they neglected to make a due allowance for the unnatural ſituation in which the ſubjects were neceſſarily placed, and in thoſe which they made upon the body after death, they proceeded upon the ſuppoſition, that the moſt complete change in the capacity of the thorax, of which it is capable, was in all in-

 ſtances

(a) *Senac, Mem. Acad.* 1729. *Cruickshanks on the Absorb. p.* 169.

ſtances produced. The experiments, therefore, exhibit the effects ariſing from a dilatation and contraction of the thorax, which can take place in the moſt extreme caſes only, and the concluſions are conſequently inapplicable to the phenomena of ordinary reſpiration.

CHAP.

CHAP. II.

The change produced by Respiration, in the Inspired Air.

THAT a change is produced by reſpiration upon the air received into the lungs, could ſcarcely eſcape the notice of the moſt curſory obſervers of the operations of the animal œconomy. The ancients were not unacquainted with this circumſtance, but the firſt accurate notions which we obtained reſpecting it, were deduced from the experiments of Boyle. This philoſopher, who ſo ſucceſsfully advanced the theory and practice of the various branches of experimental ſcience, was peculiarly fortunate in his reſearches into the nature and properties of the atmoſphere. He not only proved by means of the newly invented machine, the air-pump, the abſolute neceſſity of air to the ſupport of animal life, but he farther diſcovered, that the action of the

the lungs is quickly ſuſpended, unleſs they are furniſhed with a regular ſupply of freſh air. From this fact it was naturally concluded, that the air had undergone ſome important change during its continuance in the pulmonary veſicles, and a variety of hypotheſes and conjectures were formed to account for this alteration. The knowledge which was then obtained reſpecting the air was, however, almoſt entirely confined to its mechanical properties, ſo that the theories of reſpiration formed during this period, were neceſſarily crude and imperfect. Boyle perceived that the air in paſſing through the lungs became loaded with a quantity of aqueous vapour, and he farther ſuppoſed, that it acquired what he calls recrementitious ſteams; *(a)* but reſpecting the nature of theſe ſteams he forms no conjecture. He obſerved alſo, that the air in which an animal had reſpired for ſome time, was conſiderably diminiſhed in volume, an effect which he attributed to the loſs of part of its elaſticity or ſpring. The contemporaries of Boyle, for the moſt part, coincided with him, in his ideas reſpecting reſpiration; there were, however, ſome philoſophers who ſuppoſed, that beſides the addition of theſe vapours, the air, during its continuance in the lungs,

(a) Boyle's Works, vol. i. *p.* 99, *&c seq.*; *vol.* iii, *p.* 371, *&c seq.*

lungs, imparted ſomething to the blood. Among theſe, the firſt in point of genius and originality was Mayow of Oxford. He inveſtigated the properties of the air, and the effects produced upon it by reſpiration, with great acuteneſs, and concluded, that a peculiar volatile ſpirit, which was one of the conſtituents of the atmoſphere, was abſorbed by the blood during its paſſage through the lungs. Borelli, *(a)* Lower, *(b)* Willis, *(c)* and others, adopted opinions in many reſpects ſimilar to that of Mayow; they imagined, that either a portion of the whole maſs of air, or ſome particular conſtituent of it, was abſorbed by the blood, and by this means converted this fluid from the venous to the arterial ſtate. But ſo little real knowledge was at this time poſſeſſed reſpecting the compoſition of the atmoſphere, that they entirely failed in their attempts to aſcertain the nature of the matter abſorbed, and their hypotheſes appeared ſo extravagant, and ſo little founded upon truth, that their doctrines fell into diſcredit, became neglected, and at length were totally forgotten. *(d)*

Dr. Hales devoted much of his attention to this ſubject, and performed many experiments with

(a) *De Motu Anim. p. 2a. prop.* 113. *(b)* *De Corde, p.* 159—165. *(c)* *Willis, pharm. Rat. p.* ii. *p.* 34. *(d)* Note 30.

with a view to illuftrate the manner in which the air is affected by the lungs; he concludes nearly as Boyle had done, that it acquires a noxious vapour, and that its elafticity is diminifhed.(a) The learned Boerhaave confeffes his inability to explain the change which the air experiences by refpiration. (b) The opinion of Haller was not materially different from that of Boyle and Hales; he has collected all the different theories which have been advanced upon this fubject, and after reviewing them with his accuftomed candour and perfpicuity, he concludes that the air, when it is emitted from the pulmonary vesicles, is combined with a quantity of water, and a peculiar noxious vapour, and has its elafticity diminifhed. (c) Such was the imperfect ftate of our knowledge, when Haller wrote his Elements of Phyfiology! This noble monument of induftry and genius was fcarcely publifhed, when Dr. Black commenced his experiments upon fixed air, and among other interefting difcoveries, fatisfactorily proved, that this peculiar gafeous fubftance is generated in the lungs during refpiration. (d) Shortly after this period, the brilliant difcoveries of Scheele and Prieftley made us

(a) *Statical Essays, passim.* (b) *Prælect, t.* v. *p.* 169. *& seq.* (c) *Notæ ad Boer. Prælect. t.* v. *p.* 170. *El. Phys.* viii. 3. 11; viii. 5. 19, 20. (d) Note 31.

us acquainted with the nature and composition of the atmosphere, and by this means effected a revolution in chemistry, and in all the branches of natural philosophy connected with this science. The atmosphere, which until this period had been regarded as an homogeneous, elementary body, was discovered by these celebrated experimenters, to be compounded of two aeriform fluids, possessing distinct properties, and serving totally different purposes in the œconomy of nature. These substances, which have since obtained the names of the oxygenous and the azotic gasses, were found to exist in the atmosphere, in the constant proportion of about 22 to 78, *(a)* but after an animal had breathed for any length of time in the same quantity of air, the oxygenous part appeared to be considerably diminished. Dr. Priestley repeated these experiments under a variety of circumstances, and imagined, that in these cases the air had undergone the same change in its properties, which it experiences from combustion, fermentation, the calcination of metals, or other operations by which the air is deprived of part of its oxygene, which, in consequence of his peculiar theory, he stiles phlogistic processes. The change

(a) Note 32.

change produced in the air by paffing through the lungs, according to this philofopher, confifted in the removal of part of the oxygene, and in the addition of phlogifton, and a quantity of aqueous vapour. *(a)*

About a year after the publication of Dr. Prieftley's experiments, a memoir on refpiration appeared from the pen of the celebrated M. Lavoifier. *(b)* After paying a tribute of refpect to the genius of Dr. Prieftley, he proceeds to an accurate examination of his experiments, and the conclufions which were deduced from them. He agrees with the Doctor in fuppofing, that the proportion of oxygene is diminifhed in air which has been refpired, but upon a careful analyfis of the refidue, he finds it to differ from the air left after the calcination of metals, which is merely azotic gas, in containing a quantity of carbonic acid. He alfo obferved, that the bulk of the air was fomewhat diminifhed, and we learn in general from thefe experiments, that the changes produced in air by refpiration, confift in the removal of part of the oxygene, in the addition of a quantity of carbonic acid gas, and in the diminution of its volume. He fuppofes that the

(a) Phil. Trans. 1776; *Obferv. on Air, v.* iii. *b.* 9.
(b) Mem. Acad. 1777.

the azote is not affected by the procefs, and that it ferves merely to dilute the oxygenous part of the atmofphere. *(a)*

In this paper M. Lavoifier does not mention the aqueous vapour which is fo evidently difcharged from the lungs by refpiration; it is impoffible that it could have been overlooked by fo accurate an obferver; we may therefore conjecture, that he omitted to mention it, becaufe at this period he confidered it as only diffufed through the air expired from the lungs, by the procefs of evaporation, and not formed in confequence of the operation of any chemical affinities.

The conclufions of this philofopher refpecting the changes produced by refpiration upon the air taken into the lungs, are for the moft part acquiefced in by the modern phyfiologifts, and the refearches which have been fince made upon this fubject are principally directed, either to afcertain with more precifion the proportion of the refpective ingredients in the air of expiration, or to frame hypothefes to account for the operation of the lungs in effecting thefe changes.

I fhall

(a) Note 33.

I ſhall defer my remarks upon the theory of reſpiration, until I have conſidered the alteration which is effected in the blood, by its paſſage through the lungs; in the remainder of this chapter I ſhall examine the experiments that have been made with a view to aſcertain with more preciſion the amount of the changes produced upon the air.

When an animal has expired in a quantity of atmoſpheric air, in conſequence of its being no longer fit for reſpiration, it is found that the whole of the oxygene is not removed from it,(a) and it appears, that independently of their bulk, different animals poſſeſs this power of abſtracting oxygene in a greater or leſs degree. It is not eaſy to deduce any general concluſions from experiments of this kind, as there are ſeveral incidental circumſtances, by which the death of the animal appears to be accelerated or retarded, but upon the whole, we are warranted to conclude from them, that the animals whoſe temperature is the higheſt, are the leaſt able to exiſt in air wanting its due proportion of oxygene. Accordingly birds, who poſſeſs a temperature of about 104°, have been found to expire when two-thirds of the oxygene

(a) Note 34.

oxygene is removed ; a guinea-pig and a moufe, whofe temperature is about 98°, can confume about three-fourths, while frogs are capable of living until nearly the whole of the oxygene is confumed *(a)* It muft however be obferved, that when animals die in confequence of the want of frefh air, their death is not fo much to be attributed to the deficiency of the oxygene, as to the prefence of the carbonic acid, which when it exifts in any confiderable quantity, exercifes upon the body a highly noxious influence. It is therefore neceffary to guard againft its effects by prefenting to the air a fubftance which has the property of abforbing it as rapidly as it is produced. When thefe precautions were employed, Lavoifier found, that a guinea-pig could live without inconvenience in air which contained only one-fifteenth part of its bulk of oxygene, and when the proportion was ftill farther diminifhed, the only apparent effect produced, was a degree of drowfinefs in the animal. *(b)* As the temperature of a guinea-pig is nearly that of a man, it is probable that human life might be fupported with air of the fame compofition.

Another

(a) Higgins's Minutes of a Society, &c. p. 158. *Chaptal's Chemistry, v.* i. *p,* 127.

(b) Mem. Acad. 1789. *p.* 574.

Another queſtion reſpecting the conſumption of oxygene, of a more intereſting nature, but of more difficult inveſtigation, is the abſolute quantity of this gas conſumed by reſpiration in a given time. The firſt calculations which were made upon this ſubject, in conſequence of the imperfect nature of the apparatus employed, and of the want of a ſufficient dexterity in the management of pneumatic experiments, were unavoidably vague and inaccurate. The difficulty was much encreaſed by a circumſtance firſt noticed by Dr. Crawford, and afterwards more fully inveſtigated by M. Jurine of Geneva, and M. Lavoiſier, that the reſpiration of the ſame animal in different ſtates of the ſyſtem, and under the operation of different external circumſtances, affects the air in very different degrees. This curious fact, which affords an inſight into ſome of the moſt important operations of the animal œconomy, muſt unavoidably produce great differences in the reſults of the beſt conducted experiments, and will render it impoſſible for us to arrive at more than an approximation to the truth. The circumſtances which have been diſcovered to influence the chemical effects of the reſpiration are, the temperature of the air reſpired, the degree of muſcular exertion, the ſtate of the digeſtive organs, and the condition of the ſyſtem as affected by fever; it is highly probable

probable that other circumſtances will be diſcovered, by multiplying and varying our experiments upon the living body. Theſe different affections of reſpiration will undergo a farther examination in the third part of this eſſay; they are noticed in this place in order to ſhew, that the greateſt degree of certainty which we can attain upon this ſubject, is no more than an average deduced from a number of experiments, performed upon the body under the various circumſtances in which it is placed, and the conſequent changes to which it is liable.

An experiment performed by Lavoiſier, upon a guinea-pig, ſeems to have been the firſt in which a perfect apparatus, and the neceſſary degree of accuracy were employed.(a) The animal was confined over mercury, in a jar containing 248 cubic inches of gas, conſiſting principally of oxygene. In an hour and a quarter, the animal breathed with much difficulty, and being removed from the apparatus, the ſtate of the air was examined. Its bulk was found to be diminiſhed by eight cubic inches, and of the remaining 240 inches, 40 were abſorbed by cauſtic potaſh, and conſequently conſiſted of carbonic acid gas. Taking 100 parts of this air, theſe numbers will

(a) *Mem. Acad.* 1780. *p.* 401—8.

will be as follows; the air was diminiſhed to 96·5, or by 3·5 cubic inches, and of the remainder 16·5 were converted into carbonic acid gas, and abſorbed by pot-aſh, which reduces the quantity of air to 80 cubic inches. Towards the concluſion of the experiment, the air would be neceſſarily much leſs fit for performing the functions of the lungs than the air of the atmoſphere, in conſequence of the carbonic acid gas which it contained; but as the air employed was originally much purer than the atmoſphere, the author ſuppoſes, that the quantity of oxygene deſtroyed, was probably about the ſame which would have been conſumed under the ordinary circumſtances of reſpiration,

The ſame philoſopher performed a ſecond experiment upon the ſame ſpecies of animal, with ſtill more accuracy, in which pure oxygene was employed.(a) This experiment continued during an hour and a half, and the animal being then removed from the jar, the air was analyzed as in the former caſe. 1728 cubic inches of air were found to be reduced to 1673, i. e. had ſuffered a diminution of 55 inches, cauſtic pot-aſh abſorbed about 229·5 inches, leaving a reſidue of pure oxygene. Theſe

(a) *Ann. de Chim. t. v. p.* 261 *&* *seq.*

Thefe numbers, eftimated as in the former cafe, will be nearly as follows; 100 inches were reduced to 96·82. or by 3·18 inches, the pot-afh abforbed about 19 inches, reducing the whole quantity of air to 77·82 parts. The quantity of carbonic acid was here fomewhat greater than in the former experiment, which may be attributed to the air employed being pure oxygene, and to the procefs having been continued for a fomewhat longer fpace of time than in the former inftance. Upon the whole, the refults correfpond as nearly as can be expected, from the very delicate nature of the experiments.

Dr. Menzies firft attempted to afcertain the quantity of oxygene confumed by a man in the courfe of a day. He found by experiment, that one-twentieth part of air, which had been once refpired, is converted into carbonic acid gas; this he concludes muft have been oxygene, as that part of the air alone is affected by refpiration. He conceives that 720 cubic inches of air are refpired in a minute, of which confequently 36 will be confumed. From thefe data he eftimates, that in the fpace of 24 hours 51840 cubic inches, or 17625·6 grains of oxygene, are confumed and converted into carbonic acid gas. In this calculation feveral important particulars appear to have been overlooked, and accordingly it will be found to differ from the refults of the more accurate experiments, which have been fince performed by M. Lavoifier, and Mr. Davy.

The experiments which were made by M. Lavoifier, in conjunction with his friend M. Seguin, were conducted with every poffible attention to accuracy, and with an apparatus more complete, than any which has ever been employed in phyfiological refearches. An account of them is detailed in two papers in the memoirs of the Academy of Sciences for the years 1789 and 1790; but notwithftanding the peculiar advantages under which they were performed, their refults will not be found in all inftances to coincide. M. Seguin was himfelf the fubject of the experiments; the authors begin by pointing out the different effects which are produced by the refpiration under the different circumftances in which the body is placed, and they farther remark, that individuals may probably differ in the abfolute quantity of oxygene which they confume in the fame circumftances. Making a due allowance for thefe variations, they conclude, that the mean confumption of oxygene by a man during 24 hours, is fomewhat more than 22 French cubic feet, or 46037·38 Englifh cubic inches, a quantity a quantity of gas which will weigh 15661·66 grains troy. *(a)*

Lavoifier was ftill continuing to purfue his experiments on this fubject, and had conftructed a very expenfive apparatus, for afcertaining with ftill more precifion, the amount of the feveral changes produced by refpiration, when this great philofopher fell a facrifice to the fury of Robefpierre, and received fentence of death. He had already performed a number of experiments with his new apparatus, and earneftly requefted a refpite of a few days, in order to prepare them for publication ; but his requeft was not granted. M. De la Place, who pronounced his eulogy, has fortunately given us the moft important refults ; they will be found to differ in fome particulars from the former experiments, though with refpect to the quantity of oxygene confumed, they nearly coincide ; it is ftated that a man in 24 hours confumes 15592·5 grains. *(b)*

The only experiments on this fubject which have been performed fince the death of Lavoifier, are thofe of Mr. Davy, which appear to have been executed with great accuracy. From a

(a) Note 35. (b) *Suppl. to Enc. Brit. v.* ii, *p.* 594.

number of trials made upon his own refpiration, he found, that 100 cubic inches of atmofpheric air, after having once paffed through the lungs, had loft between 4 and 5 parts of oxygene, hence he calculates, that 31·6 cubic inches of oxygene are confumed in a minute; *(a)* this will give 45504 inches in 24 hours, a quantity which will weigh 15471·36 grains. *(b)* This efti-mate coincides nearly with that of M. Lavoifier, though it was obtained by a different procefs and by the ufe of a different apparatus; we may therefore conclude, that between 45, and 46,000 cubic inches, or about 15500 grains, 2lbs. 8oz. troy, is the average quantity of oxygene con-fumed by a man in 24 hours.

Having afcertained the proportion of oxygene which is confumed in refpiration, it next remains to determine the quantity of carbonic acid gas, which is produced. It appears that Dr. Black firft demonftrated its exiftence in air which was emitted from the lungs, and that M. Lavoifier, afterwards examined it with more accuracy, and found that the air in which an animal had ex-pired, contained about $\frac{1}{6}$ of its bulk of carbonic acid gas. *(c)* In the experiment which this phi-lofopher

(a) *Davy's Researches, p.* 431—4. *(b)* Note 36.
(c) *Mem. Acad.* 1777.

losopher performed with a more perfect apparatus, upon a guinea-pig confined in oxygene, the carbonic acid amounted to nearly $\frac{1}{5}$ of the bulk of the whole air employed, when the animal had been detained in the apparatus until the air was reduced into a ſtate, no longer fit for reſpiration. *(a)* Theſe experiments, however, only prove what proportion of carbonic acid gas, will render air incapable of ſupporting life, without acquainting us with the quantity of this gas, produced under the ordinary circumſtances of reſpiration.

M. Jurine of Geneva, appears to have been the firſt who attempted to calculate the abſolute quantity of carbonic acid formed by the reſpiration of man; he imagined that it conſtituted about $\frac{1}{10}$ part of the air emitted from the lungs. *(b)* Dr. Menzies inſtituted a ſet of experiments to diſcover the abſolute quantity generated in a given time; he infers from them, that $\frac{1}{20}$ part of air which has been once reſpired, is carbonic acid, and eſtimates, that a man in 24 hours, ſends out from the lungs 51840 cubic inches, or nearly 4lbs. troy; *(c)* but this eſtimate is probably over-rated.

The

(a) Ann. de Chim. t. v. p. 261, *& seq.* *(b) Enc. Meth. Medicine, t.* i. *p.* 494. *(c) Menzies, p.* 50.

The circumſtances which have been already pointed out, as influencing the conſumption of oxygene, have at leaſt as powerful an effect upon the production of carbonic acid gas. Accordingly we ſhall find the calculations of the moſt accurate experimenters upon this ſubject ſo widely different from each other, that it ſeems ſcarcely poſſible to arrive at any tolerable degree of certainty.

M. M. Lavoiſier and Seguin, in their firſt memoir of 1789, eſtimate the average quantity of carbonic acid gas, formed by a man in 24 hours, at 17720 · 89 grains troy ; in their ſubſequent memoir, publiſhed in the following year, this quantity is diminiſhed to 8450 · 24 grains ; and in the eulogy of La Place it is ſtated, that Lavoiſier in his laſt experiments, reduced it ſtill lower to 7550 · 40 grains. Mr. Davy on the contrary, whoſe experiments ſeem to have been performed with great exactneſs, though with a leſs complicated apparatus than that employed by the French chemiſts, ſuppoſes the carbonic acid formed in 24 hours to amount to 17811 · 38 grains, *(a)* a quantity which is not very different from that firſt announced by Lavoiſier. Between ſuch oppoſing authorities, it is not eaſy to decide ; upon the whole I feel inclined

(a) Researches, p. 434.

clined to acquiefce in the experiments of Mr. Davy, and to conclude, that the quantity of carbonic acid gas, generated by a man in 24 hours, amounts to about 17800 grains or fomewhat above 3lbs. troy.

Another of the changes produced by refpiration upon the air, is a diminution of its volume. This circumftance was noticed by the older phyfiologifts, but in confequence of the manner in which their experiments were performed, it was very confiderably over-rated; they attributed it to a diminution of the elafticity or fpring of the air. *(a)* Lavoifier, in the firft memoir which he publifhed upon refpiration, afcertained the degree of diminution with more accuracy, and ftated, that air when rendered unfit to fupport life, was reduced $\frac{1}{60}$ in bulk. *(b)* The experiments of Dr. Goodwyn, afforded the fame refult; *(c)* but Dr. Crawford, by fome miftake, was led to conclude that, when the procefs was conducted with accuracy, there was no perceptible diminution. *(d)* In the account which Lavoifier gives of the firft experiment upon the guinea-pig, he found the diminution to amount to $\frac{1}{51}$ of the bulk of the air employed, *(e)* and in

(a) Note 37. (b) *Mem. Acad.* 1777. (c) *Goodwyn, p.* 51. (d) *On Animal Heat. p.* 146. (e) *Mem. Acad.* 1780, *p.* 401.

in the fecond fet of experiments, the diminution was found to be $\frac{1}{32}$ part; *(a)* the greater abforption in thefe cafes probably depending upon the greater purity of the air employed. It is fomewhat remarkable, that in the experiments performed by Lavoifier, in conjunction with Seguin, upon the refpiration of man, though in other refpects fo remarkable for their accuracy, there is no mention made of this circumftance, nor is it noticed by La Place, in his account of the experiments in which Lavoifier was engaged, immediately previous to his execution. In thefe inftances we cannot determine, whether Lavoifier conceived that no diminution actually took place, or whether he only neglected to notice it; upon the whole, the latter appears the more probable fuppofition.

The general fact of the diminution of bulk in refpired air, has been fince confirmed by Mr. Davy, though the exact degree of abforption varied fo much in his different experiments, that it is difficult from them to fix upon a quantity which may indicate the ordinary amount of this diminution. In the confideration of this queftion, as in the preceding one refpecting the confumption of oxygene, and the production of carbonic

(a) *Ann. de Chimie, t.* V. *p.* 261.

carbonic acid, there are two diſtinct objects of inquiry. We may examine the degree of diminution produced in a given quantity of air, in which an animal has been confined, until it is no longer fit for ſupporting the reſpiration ; and in the ſecond place, we have to aſcertain the amount of the diminution which takes place in air that has only once paſſed through the lungs, as is the caſe in the proceſs of ordinary reſpiration. The firſt of theſe points only was examined by Lavoiſier. Mr. Davy has made experiments upon both. In air which had only once paſſed through the lungs, he found the diminution in different trials to vary from $\frac{1}{70}$ to $\frac{1}{100}$; *(a)* when he received the ſame air repeatedly into the lungs, it was found to be diminiſhed as much as $\frac{1}{18}$ of its original bulk. *(b)* The former experiments however alone indicate the effects of natural reſpiration, and taking an average of their reſults, the amount of the diminution will be about $\frac{1}{80}$ part of the whole air received into the lungs. I am not acquainted with any experiments to aſcertain the diminution of air that has been once reſpired, upon the accuracy of which we can depend, excepting thoſe of Mr. Davy.

Mr. Abernethy, in oppoſition to the generally received opinion, conceives that the quantity of air

(a) Researches, p. 431—3. *(b) Do. p.* 435.

air emitted from the lungs is greater than that received by them. This ſingular hypotheſis is principally founded upon the ſuppoſition, that the carbonic acid diſcharged by reſpiration, is not generated by the union of the oxygene received by the blood as it paſſes through the lungs, with the carbone contained in this fluid, but that it is merely an exhalation from the ſurface of the veſicles. He eſtimates that 100 cubic inches received by the lungs, when diſcharged from them are augmented to 107½ inches. The diminution in the bulk of a quantity of air in which an animal has been confined, he attributes to its abſorption by veſſels with which he thinks it probable, the ſurface of the lungs is furniſhed, ſimilar to thoſe of the ſkin. (a)

If we have found it difficult to aſcertain with accuracy the amount of thoſe changes produced by reſpiration which we have hitherto examined, we ſhall probably experience ſtill more uncertainty in determining the quantity of aqueous vapour that is emitted from the lungs. This ſubſtance is from its nature leſs eaſy to collect and meaſure than a permanently elaſtic fluid, and accordingly the phyſiologiſts who have attempted to diſcover the

(a) *Essays, p.* 146, 8.

the quantity of it mixed with the air of expiration, have for the moſt part proceeded rather upon calculations derived from collateral circumſtances, than from the direct reſult of experiment.

The exhalation of water from the lungs in reſpiration, was a circumſtance which could not eſcape the moſt curſory obſerver; accordingly we ſhall find that it was noticed by the older phyſiologiſts, and was indeed regarded by them as one of the principal purpoſes which is ſerved by this function. *(a)* Dr. Hales performed many experiments for the purpoſe of aſcertaining its quantity; he contrived to paſs the air which he expired through a flaſk filled with wood-aſhes, which in conſequence of the pot-aſh contained in them, have the property of ſtrongly attracting moiſture. By obſerving the increaſe of weight which the aſhes had acquired in a given time, he eſtimates that the water emitted from the lungs in 24 hours, will amount to 9792 grs., above 20 oz. *(b)* The nature of his proceſs, however, did not admit of much accuracy. Dr. Menzies attempted to ſolve this problem by actually collecting in an allantoid fitted to the mouth, the water emitted from the lungs in a given time; his eſtimate is much leſs than that of Hales; he ſuppoſed that the quantity of water

(a) Note 38. *(b)* *Statical Essays*, *v.* ii. *p.* 322—4.

water exhaled in 24 hours would amount to no more than 6 oz. or 2880 grains. *(a)* Mr. Abernethy, by breathing into a glaſs veſſel of a peculiar conſtruction, collected in an hour 180 grs. of water, containing, as he ſuppoſed, a quantity of mucous matter. According to his eſtimate, the quantity emitted in 24 hours, would amount to exactly 9 oz. or 4320 grains, but as the ſubſtance which he obtained was not pure water, there muſt be ſome deduction made from it on this account. We are not informed what proportion the water bore to the mucus diſſolved in it. *(b)*

The difficulty of actually collecting and weighing the pulmonary exhalation, is probably the cauſe which induced Lavoiſier in his experiments upon reſpiration, to aſcertain its quantity by a calculation, founded upon the proportion between its conſtituent parts, compared with the compoſition of the other ſubſtances which are received into and diſcharged from the lungs. He firſt determined by direct experiment the quantity of oxygene conſumed, and of carbonic acid produced; the compoſition of carbonic acid is known, and by comparing the oxygene which had diſappeared with the quantity which would have been neceſſary to form the acid, he found that the oxygene conſumed was more than ſufficient

(a) Menzies, p. 54. • *(b) Essays, p.* 141.

ficient to compofe the carbonic acid which was actually produced. *(a)* He fuppofed that this fuperabundant quantity of oxygene was employed in the formation of water, by uniting in the lungs with a portion of hydrogene; he eftimates the amount of the water by knowing what quantity of it a given weight of oxygene can produce. This method employed by Lavoifier muft be confeffed to be extremely ingenious, but at the fame time, before we can depend upon its truth, we muft be well affured that the feveral propofitions on which it refts, are themfelves well founded. There are, I confefs, feveral reafons which ftrongly incline me to fufpect their validity, they will be confidered in the following chapter; at prefent I fhall only ftate the conclufions which this diftinguifhed philofopher has deduced from his calculations. *(b)*

Lavoifier firft formed an eftimate of the water fuppofed to be generated in the lungs, from the experiments which he made upon the refpiration of a guinea-pig confined in pure oxygene. He afcertained, that the carbonic acid produced would have acquired for its formation, a quantity of oxygene less by 55 cubic inches, than had in fact difappeared; this oxygene, the author adds, muft either

(a) Note 39. *(b)* Note 40.

either have been abſorbed, and have entered into ſome combination with the maſs of blood, or have united with a quantity of hydrogene diſcharged from the lungs, together with the carbone, and thus have produced water; he conceives that the latter ſuppoſition is the moſt probable. (a) The reaſons for this opinion are not detailed as fully as might be wiſhed, but he appears to have been influenced by the experiments, in which he conceived, that more caloric is diſengaged by the formation of a given quantity of carbonic acid in the lungs, than from the formation of the ſame quantity of carbonic acid by the combuſtion of charcoal in the calorimeter. This exceſs of caloric, he imagined was derived from the union of the hydrogenous and the oxygenous gaſes. (b) In the experiment with the guinea-pig, the carbonic acid amounted to 107 grs., the water was eſtimated at about 25 grs. Nearly the ſame train of reaſoning was purſued in the elaborate experiments upon the reſpiration of man, to which I have already referred, performed by the united efforts of Lavoiſier and Seguin. In the firſt of theſe memoirs, the quantity of water emitted from the lungs of a man in 24 hours is ſtated to be no more than 337·18 grs.; but in the 2d., where a more perfect apparatus was employed, the weight of water

(a) *Ann. de Chimie, t. v.* (b) *Mem. Acad.* 1789.

ter expired in 24 hours, is eſtimated at 11180·57 grs., nearly 2lbs. troy, a quantity very much larger than the eſtimate derived from the former experiments. In the experiments performed by Lavoiſier, of which an account is given by M. La Place, the quantity of water was ſuppoſed to be ſtill more conſiderable, even as much as 13704 grs. in the courſe of 24 hours. The proportion between the carbonic acid, and the water diſcharged by reſpiration, as deduced from theſe laſt experiments of Lavoiſier's, differs very conſiderably from that in the experiment performed upon the guinea-pig, and no leſs remarkably from each other. In the memoir of 1789, the carbonic acid was 17720·89 grs. the water only 337·18 grs. or as 1000 to 19 nearly; in the memoir of 1790, the carbonic acid was 8450·24 grs. and the water 11188·57 grs. or as 1000 to 1323; in the eulogy of La Place, the carbonic acid is 7550·40 grs. and the water 13704, or in the proportion of 1000 to 1815 nearly; from ſuch very diſcordant calculations it is impoſſible to draw any concluſions. Mr. Davy has not formed any eſtimate of the quantity of water exhaled in reſpiration.

M. Lavoiſier in his earlieſt experiments upon reſpiration, concluded that the azote remained entirely paſſive; aud was received into and emitted from

from the lungs without undergoing any change; *(a)* to this opinion he always adhered, and it has been adopted by almoſt all the modern phyſiologiſts. Dr. Prieſtley, indeed, in his experiments, thought that there was an abſorption of azote as well as oxygene, *(b)* but as the apparatus which he employed was not ſuſceptible of perfect accuracy, and as his experiments were performed in the infancy of pneumatic chemiſtry, this circumſtance was generally attributed to ſome accidental cauſe. Dr. Prieſtley's ſuppoſition has, however, lately received a ſtrong confirmation from Mr. Davy, who uniformly diſcovered a deficiency of a ſmall quantity of azote in his experiments upon reſpiration; he eſtimates the volume of azotic gas abſorbed to be 5·2 cubic inches in a minute, *(c)* a quantity which, in 24 hours, will amount to about 2246·4 grs. or 4·68 oz. From the peculiar accuracy with which Mr. Davy's experiments appear to have been performed, I feel ſtrongly induced to acquieſce in his opinion, notwithſtanding the great authorities in favour of the oppoſite doctrine. The expreſſions which Lavoiſier employs, render it probable that he derived his notion reſpecting the inactivity of the azote rather from theory than from actual experiment, and the poſitive

(a) Mem. Acad. 1777. *(b) Priestley's Exp. on Air*, *v.* iii. *p.* 380. *(c) Davy's Researches, p.* 434.

ſitive aſſertion of ſo diſtinguiſhed a philoſopher ſeems to have deterred ſucceeding experimenters from any farther inveſtigation of the ſubject. (a)

An opinion, directly the reverſe of Mr. Davy's, was advanced by M. Jurine; he imagines that azote is actually generated by reſpiration. (b) But an attentive review of his experiments will eaſily explain the cauſe by which he was led to form ſo ſingular a ſuppoſition. After taking a full inſpiration, he received the air from his lungs into four different veſſels, and he found, that the portion which was laſt expired, contained a greater quantity of azote than the others. He alſo found, that when the air had been retained for ſome time in the lungs, or when the ſame quantity of air had been ſeveral times reſpired, that it contained more azote, than when only taken once into the lungs and immediately diſcharged from them, as in ordinary reſpiration. But it is obvious, that in all theſe caſes, the encreaſed quantity of azote referred only to the greater proportion which it bore to the reſt of the air, and not in any degree to its abſolute volume. M. Jurine has, however, performed another experiment, which appears more direct, and had there been no ſource of inaccuracy, would

H certainly

(a) Note 41. (b) *Enc. Meth. Medecine*, *t.* i. *p.* 493—7.

certainly have been decisive. He respired a quantity of pure oxygene, and upon examining it after it had been emitted from the lungs, he found it to be mixed both with carbonic acid and azote. But in this case there is every reason to believe, that this azote, which M. Jurine detected in the air of expiration, was derived from the contents of his lungs before the experiment. As it does not appear that he used any precautions to empty them before he breathed the oxygene, we may conclude that they would contain somewhat more than 200 cubic inches of azote, *(a)* which would immediately mix with the air inspired, and consequently be expelled along with it. In Lavoisier's very accurate experiment upon the guinea-pig, where the quantity of air previously contained in the lungs of the animal bore only a small proportion to the volume of gas in which it was immersed, after the animal was removed from the apparatus, and the carbonic acid absorbed by caustic pot-ash, the remaining oxygene was nearly as pure, as at the commencement of the experiment. *(b)*

After having examined in succession the different changes which are produced upon the air by

(a) Note 42. *(b) Ann. de Chim. t.* v. *p.* 262.

by refpiration, I fhall conclude this chapter by a fummary view of thefe effects, so far as the prefent ftate of our knowledge will enable us to form any precife ideas refpecting them.

1. A quantity of oxygene is confumed in refpiration; in ordinary circumftances atmofpheric air, which has been once refpired, lofes nearly ·04 of its bulk of oxygene; in 24 hours a man confumes a quantity which will weigh about 2lbs. 8 oz.; fomewhat more than 26 cubic feet.

2. A quantity of carbonic acid is generated by refpiration; its volume is lefs than that of the oxygene abforbed, nearly in the proportion of 37 to 45; the weight of carbonic acid formed in 24 hours is about 3lb.; a volume of about 22 cubic feet.

3. The whole volume of the air is diminifhed by refpiration; the degree of diminution is not very accurately afcertained, but it may be eftimated at about $\frac{1}{80}$ of its bulk.

4. A quantity of aqueous vapour, the amount of which is ftill undetermined, is emitted from the lungs.

5. It

5. It is probable that a ſmall portion of azote is abſorbed, upon an average about $\frac{1}{100}$ part of the air reſpired, making in 24 hours about 4½oz. or 4 cubic feet.*(a)*

6. From the aſcertained proportion in which the oxygene and pure charcoal exiſt in carbonic acid, it appears that a greater quantity of oxygene is conſumed, than is neceſſary for the formation of the carbonic acid which is produced.*(b)*

(a) Note 43. *(b)* Note 44.

CHAP.

CHAP. III.

The change produced upon the blood by Respiration.

THE extreme vaſcularity of the lungs, and the great proportion of blood which is always preſent in them, induced even the oldeſt anatomiſts to ſuppoſe, that ſome important change was effected in this fluid by reſpiration; and the diſcovery of Hervey, which ſhowed that all the blood in the body paſſes through the lungs during the courſe of each complete circuit, ſtrongly tended to confirm this opinion. Soon after the doctrine of the circulation became generally received, the diſtinction between the arterial and the venous blood, was clearly pointed out, and it was perceived that this change is produced in the capillaries of the lungs. A number of conjectures, as may be imagined, were formed both to explain

plain the nature of the change which takes place, and the manner in which it is effected.

A numerous and learned class of physiologists supposed that the alteration which the blood experiences in its passage through the lungs, is in a great measure mechanical. They conceived that the blood, while in the pulmonary vessels, experiences a continual and violent agitation, by means of which, its particles, before loosely mixed, and consisting of several heterogeneous substances, are comminuted and perfectly united together, so that the whole mass acquires a uniform and homogeneous consistence. *(a)* Baglivi supposed that the blood was rarefied, *(b)* and Helvetius, that it was condensed in the lungs; *(c)* Boerhaave imagined that the particles acquired that peculiar organization, which he thought essential to the existence of perfect blood. *(d)*

Another set of philosophers, in which we meet with the names of Hervey, *(e)* Boyle, *(f)* Hales and Haller, imagined that the blood, in its passage through the lungs, parted with some noxious

(a) Note 45. *(b) Opera, p* 457. *(c) Mem. Acad.* 1718. *(d) Prælect. t.* ii. *p.* 184; 232 *&* *seq.* Note 46. *(e) Exerc. de Motu Cordis, p.* 232. *(f) Works, v.* i. *p.* 99. *& seq.*; *v.* iii. *p.* 371. *& seq.*

noxious or ſuperfluous matter, and with a quantity of aqueous vapour, which being mixed with the air, was removed by expiration.

A 3d claſs of phyſiologiſts were of opinion, that the change from the venous to the arterial ſtate, depended upon ſomething which the air imparted to the blood, whence it acquired its vital properties, and became adapted to the performance of its appropriate functions. None of theſe opinions is altogether correct even in the outline, and when their reſpective advocates proceeded to detail them more at length, they degenerated into mere fanciful hypotheſes. Lower had endeavoured to prove, independently of any theory, that the bright ſcarlet colour which the blood acquires in the lungs, was owing to the action of the air, and we are now ſo well aſſured of the fact, that it ſeems to us ſomewhat ſurpriſing, that his experiments ſhould have produced ſo little conviction. In order to diſprove a notion which prevailed in his time, that the red colour acquired by the blood in the lungs, depended upon an innate fire kept up in the heart, he inflated the lungs of a dog recently killed, and he found that the blood was reddened exactly in the ſame manner as during the life of the animal. Another favourite hypotheſis prevalent

lent in his time to account for the change of colour which the blood experiences in the lungs, viz. that it depends upon the comminution of its particles, owing to the agitation and preſſure which it undergoes in this part of the circulation, he endeavoured to overthrow, by ſhewing, that the blood in the lungs of a newly killed animal would be reddened in the uſual manner, though they were kept continually dilated, and conſequently almoſt in a ſtate of reſt. As a further proof that the change of colour depends upon the action of the air, he obſerves, that when the blood is received into a baſin, the ſurface only of the craſſamentum, which is expoſed to the air, acquires the ſcarlet colour, while the other parts remain of the dark purple hue. *(a)* The experiments and reaſonings of Lower produced little conviction; the mathematicians *(b)* ſtill thought it more probable that the red colour which the blood acquires in the lungs, depends upon the compreſſion and agitation which it experiences in this organ. Haller ſtrongly oppoſed the doctrine of Lower, and conceived that the different ſhades of colour depend upon the greater or leſs proportion of red globules in the blood, or the degree of compreſſion which they ſuffer,

(a) De Corde, p. 159—165. *(b) Pitcairne, Diſſertationes, p.* 62—70.

ſuffer, and explains the bright ſcarlet of the top of the clot, upon the ſuppoſition, that the red particles ſubſide to the lower part, in conſequence of their gravity, or are more compreſſed than thoſe at the upper ſurface. *(a)* After a long interval, the opinion of Lower was revived by Sig. Cigna of Turin ; he endeavoured to prove, that the change of colour in the blood, from a purple to a ſcarlet, in all caſes depends upon the action of the air, and his experiments are ſuch as fairly to eſtabliſh the fact. They ſeem, however, to have excited little attention, and Cigna himſelf, in a ſubſequent memoir, appears almoſt inclined to deſert his former opinions. *(b)* It was at this period that Dr. Prieſtley commenced his inquiry into reſpiration ; he repeated and varied the experiments of Cigna, placed his concluſions beyond the reach of doubt, and diſcloſed a new and important ſeries of facts, which ſerved as the ground-work for all the information that has been ſince obtained. The mutual action of the blood and air, which had been hitherto admitted as a plauſible ſuppoſition, was now decidedly proved ; Dr. Prieſtley introduced a piece of dark-coloured craſſamentum into an inverted

(a) Prælect. Boer. Notæ ad, t. ii. *p.* 214. *El. Phys.* vi. 3. 17. Note 47. *(b) Priestley's Exper. on Air, v.* iii. *p.* 358—360.

inverted jar of atmofpherical air, the blood, after fome time, affumed the arterial color, and the air was found to have experienced the fame change in its chemical properties as by refpiration. He afterwards proceeded to examine the effects produced upon the craffamentum by the component parts of the atmofphere applied feparately, and by the other aeriform fluids which had been recently difcovered. The purple coagulum was found to be reddened by oxygenous gas, ftill more rapidly than by the air of the atmofphere: while in azote, in hydrogene, and in carbonic acid, the contrary effects were produced, fo that pieces of bright fcarlet craffamentum were reduced to the dull purple hue of venous blood. The conclufions from thefe experiments were highly interefting and important: they fhewed that the alteration of color which the blood experiences in the lungs, depends upon the oxygenous part of the atmofphere; and reciprocally, that the change which the air undergoes when received into the lungs, depends upon the action of the blood in the pulmonary veffels. The change which is in thefe cafes effected upon the air, Dr. Prieftley fuppofed to be fimilar to that produced by combuftion, which, in conformity with his peculiar theory, he imagined to confift in the addition of phlogifton. He therefore

fore concluded, that the air in the lungs acquired phlogifton from the venous blood, and that this abftraction of phlogifton conftituted the principal difference between venous and arterial blood. Dr. Prieftley carried the refemblance between his experiments and the actual ftate of the lungs ftill farther, by introducing a moiftened bladder between the craffamentum and the air; he found that in this cafe, the fame changes were produced as in the former inftance, and thus obviated an objection which might have been made againft his experiments, that the blood and the air are not actually in contact in the lungs, but are feparated by the membrane of the veficles and the coats of the arteries. He likewife found, that the action between the air and the blood, was not interrupted by the intervention of a ftratum of ferum, or milk, but that when water, and fome other fluids which he tried, were interpofed, the change could be no longer produced.(a)

The train of experiment which had been opened fo fuccefsfully by Dr. Prieftley, was purfued with no lefs affiduity by M. Lavoifier. This philofopher, by examining with more accuracy the ftate of the expired air, found that it had acquired

(a) *Priestley's Exp. on Air. v.* iii. *p.* 362—374. *Phil. Trans.* 1776.

acquired a great addition of carbonic acid; he had already demonſtrated, that this ſubſtance is compoſed of oxygene and carbone, and he accordingly concluded, that the air had acquired its carbone from the venous blood. In his ſubſequent experiments he ſo far modified his original opinion, as to conjecture that, in addition to the carbone, a quantity of hydrogene was likewiſe diſengaged from the venous blood, which uniting with another portion of oxygene, produced the water of expiration: according to Lavoiſier, therefore, the chemical compoſition of venous and arterial blood differed in the former poſſeſſing a greater proportion of hydrogene and carbone.

That the air in the lungs had acquired from the venous blood a quantity of carbone, was a fact no longer to be doubted, but it was ſtill uncertain in what manner this change of compoſition was effected. Either the oxygene of the air is combined directly with carbone abſtracted from the blood, and the carbonic acid is generated by their union, or a quantity of oxygene is abſorbed by the blood, and an equal quantity of ready formed carbonic acid diſcharged from it. Lavoiſier in his firſt memoir propoſes theſe two hypotheſes without deciding in favour of

either

either of them; but in his later papers he adopts the former, and confequently imagines, that all the changes produced upon the blood by refpiration, are brought about in the lungs. The hypothefis of Lavoifier was adopted with fome flight variations by moft of his contemporaries; for the opinion of thofe chemifts who ftill adhered to the doctrine of phlogifton, as Prieftley and Crawford, muft be regarded as fcarcely differing from Lavoifier's, except in the name of the fubftance emitted from the venous blood, which was by one party called inflammable matter, or phlogifton, and was by the other, with more precifion, ftated to be carbone and hydrogene.

In order to complete this theory, it was neceffary to point out an adequate fource, whence the blood may be fupplied with this carbonaceous or inflammable matter, which is perpetually difcharged from the lungs. Dr. Crawford was the firft who paid much attention to this circumftance; he propofed the following explanation. The folids of the living body have a conftant tendency to decay; their particles are continually changing, thofe which are no longer fit for performing their functions are removed, and difcharged from the body, while new ones are depofited in their room. The arterial blood which is diſtributed to all parts of the body in the minute capillary veſſels, is the vehicle by means of which this operation is performed; it conveys the nutritious matter to the different parts of the body, and depoſits it in ſuch a manner as to repair the neceſſary waſte, while at the ſame time it receives the putreſcent particles, which are become uſeleſs or noxious to the ſyſtem, and carries them to the lungs, where they are united to oxygene, and diſcharged, together with the air of expiration. It is to the addition of this extraneous matter that Dr. Crawford attributed the change from arterial to venous blood, and by the removal of it in the lungs, the blood, he imagined, is brought back to the arterial ſtate. *(a)* The hypotheſis of Dr. Crawford, reſpecting the origin of the combuſtible matter, was generally received by the phyſiologiſts who adopted Lavoiſier's method of explaining the changes produced by reſpiration. This philoſopher himſelf does not indeed appear to have paid any particular attention to the ſubject; it may be inferred from ſome of his expreſſions, that at one period he inclined to the opinion of Dr. Crawford, but in his later memoirs he ſeems to conſider the products of digeſtion as the immediate ſource of the

(a) Obſerv. on Animal heat, p. 150. *& ſeq.* Note 48.

the matter consumed in respiration. He however still supposes, that the changes are effected in the lungs, and it is therefore obvious that his theory must be imperfect, as the blood becomes completely venalized before it receives the contents of the thoracic duct.

This theory of respiration, though certainly simple and ingenious, was, however, found to be encumbered with many difficulties, for, independent of other objections, Dr. Crawford's explanation of the manner in which the blood acquires the supply of inflammable matter, is by no means satisfactory. It is indeed admitted, that the particles of which the solids are composed are perpetually changing, but it is contrary to all analogy to suppose, that the arteries are the instruments by which this change is effected. The body is provided with a distinct set of vessels, which from their office are called the absorbents, whose appropriate function it is to remove all superfluous matter. There can scarcely be a doubt, but that it is by the action of the absorbents, that the useless particles are removed, and these vessels have no communication with the sanguiferous system, except by the intervention of the thoracic duct, which receives all the substances absorbed, and pours them into

into the left fubclavian vein. There is no paffage hitherto difcovered, by which the blood can receive any thing *ab extra*, from the time it leaves the heart, until it arrives at the mouth of the thoracic duct, when the circuit is nearly concluded, and the blood has already become completely venalized.

A fecond objection againft this hypothefis, perhaps no lefs decifive, was derived from a circumftance which had been not unfrequently obferved, that on fome occafions the blood is converted to the venous ftate, while it ftill continues in the great trunks of the arteries. This change has been known to take place during furgical operations, in which the courfe of the blood along the arteries had been intentionally ftopped, and alfo in cafes where an extraneous, morbid body had preffed upon the veffels, and had prevented them from tranfmitting their contents in the ufual manner. Where the tourniquet has been applied for any length of time to the trunk of a great artery, the blood which firft flows through the veffel when the inftrument is removed, is fometimes obferved to be of the venous colour. Mr. Hunter remarks, that extravafated blood is in all cafes of a dark purple colour, though there is every reafon to fuppofe,

that

that it often proceeds from the rupture of an artery. He punctured the femoral artery of a dog, and afterwards carefully excluded the air from the orifice, a tumor was formed in the adjoining cellular membrane, this he opened after ſome time, and the blood which it contained was found to exhibit all the venous characters. He then proceeded to perform the following ſtill more direct and deciſive experiment; he laid bare the carotid artery of a dog and paſsed round it two ligatures, leaving between them an interval of two inches. After ſome hours he pierced the part of the veſſel between the ligatures, and he found it to contain dark purple blood. *(a)* In theſe inſtances it may be inferred, that the change which the blood experienced could not depend upon any ſubſtances either received or diſcharged, but upon an action which took place among the bodies already contained in it. It is indeed admitted, that the capillary arteries of the lungs poſſeſs the power of tranſmitting through their coats, various ſubſtances, which cauſe the mutual changes produced in the air and the blood; but the difference in the thickneſs and texture of the branches of the aortic ſyſtem, and the minute capillaries in the lungs, is a ſufficient reaſon for ſuppoſing that their

(a) Observ. on the blood, p. 65--7.

their contents cannot be affected in the ſame manner by external agents. The capillaries of the lungs, are moreover expoſed to the action of the oxygene of the inſpired air, whereas the arteries in the other parts of the body, independent of the thickneſs of their coats, are not expoſed to any external agent, which can be ſuppoſed to have the power of affecting the compoſition of the blood which they contain. It appears indeed, that the abſence of oxygene is a more important circumſtance than the mere thickneſs of the coats; as we learn from the experiments of Dr. Goodwyn, that the colour of the blood in the ſmall veins of the neck of a rabbit, was in ſome meaſure brightened, by directing a ſtream of oxygene againſt the outer ſurface of the veſſels, when the cellular ſubſtance was carefully removed. *(a)*

As it appeared therefore from theſe circumſtances, that blood may be converted from the arterial into the venous ſtate without the addition of any extraneous ſubſtance, it became neceſſary to abandon the theory of reſpiration which attributed this change to the abſorption of inflammable matter, during the aortic circulation, and to form

(a) Goodwyn, p. 63.

form one in which the alteration might be produced by the mutual action of the constituent parts of the blood upon each other. *(a)* This was accordingly done by M. La Grange, who proposed a new hypothesis, which M. Hassenfratz adopted, and illustrated by several important observations and ingenious experiments. *(b)* The theory of M. La Grange, has been in some respects modified by Mr. Allen, *(c)* and with his improvements, appears to afford an easy explanation of every phenomenon.

According to this hypothesis, the blood in its passage through the capillaries of the lungs, absorbs oxygene, which is loosely united to the whole mass of fluid; by this union its colour is changed from a deep purple to a bright scarlet. The oxidated blood is then carried along the arteries; in the course of the circulation, the oxygene leaves the whole mass of blood, and forms an intimate union with a part of the hydrogene and carbone contained in it, by this operation it loses its bright colour and assumes the venous appearance. This portion of hydrogene and carbone, reduced to the state of an oxide, is then carried along the veins, until it arrives at

(a) Note 49. *(b) Ann. de Chim. t.* ix.
(c) De la Rive de Cal. Anim. p. 35.

the lungs, where, after being united with an additional quantity of oxygene, it is difcharged from the blood, and forms the carbonic acid, and the aqueous vapour which are found to exift in the air of expiration. According to this hypothefis, venous and arterial blood differ only in the arrangement of their component parts, in the latter cafe the oxygene is loofely combined with the whole mafs, whereas in the former, it is in clofe union with a portion of hydrogene and carbone only. As a confirmation of the affumed principle upon which this hypothefis is founded, M. Haffenfratz adduces an experiment performed by M. Fourcroy, in which venous blood mixed with oxygene immediately acquired the fcarlet colour, but after fome time, though it was ftill expofed to its action, it again became purple, *(a)* owing, as he conjectured, to a more intimate union, which gradually took place between the oxygene and fome part of the blood. Dr. Prieftley had feveral years before obferved, that arterial blood was rendered purple by expofure to azote, or to any of the gafes which did not contain oxygene; in this cafe it may be inferred, that the gas attracted the oxygene from the mafs of blood with which it was loofely combined, for if the oxygene

(a) Ann. de Chim. t. vii. *p.* 148.

oxygene had already been united to the carbone and hydrogene, ſo as to form carbonic acid and water, it is not probable that the mere expoſure to azote, could have decompoſed theſe ſubſtances.

The difference between the arterial and the venous blood, according to this hypotheſis, being ſuppoſed to conſiſt principally in the more or leſs intimate combination of the oxygene, M. Haſſenfratz propoſed to obſerve the effects which would be produced, by adding to the blood oxygene in a condenſed ſtate; he employed for this purpoſe the oxygenated muriatic acid. A quantity of it was accordingly poured into ſome blood, when the fluid acquired a deep and almoſt black colour. That this colour depended upon the oxygene, and not upon the muriatic acid, he proved by performing a ſecond experiment, in which he uſed the common muriatic acid; the blood in this caſe was merely rendered brown. From the effects of the oxygenated acid, he inferred, that the oxygene, being preſented to the blood in a condenſed ſtate, had immediately formed a union with the hydrogene and carbone, and had conſequently produced the ſame change inſtantaneouſly, which in ordinary circumſtances, is only brought about gradually during the courſe of the

circulation.

circulation. M. Haſſenfratz afterwards filled a number of tubes with arterial blood, and ſealed them hermetically; after ſome time the blood became purple, and acquired the venous appearance. The ſame action appears to have taken place in this inſtance, between the conſtituent parts of the blood, which is produced in the capillaries of the aortic ſyſtem, and it was effected in a ſituation where nothing could be abſorbed by the blood or diſcharged from it. The oxygene which had been previouſly diſſolved in the whole maſs, and had imparted to it the bright ſcarlet colour, afterwards entered into a union with part of its hydrogene and carbone; the colour was changed to a deep purple, and from the ſituation in which the blood was placed, it had not the power of diſcharging the oxidated ſubſtances which it contained, or of attracting a freſh ſupply of oxygene.

Theſe experiments certainly afford a ſtrong confirmation of the truth of M. La Grange's hypotheſis, and indeed they may be conſidered as demonſtrating that a change in the blood, ſimilar to that which it undergoes in the courſe of the aortic circulation, may be effected merely by a different arrangement of its conſtituent parts. Another circumſtance in which this hypotheſis

poſſeſſes

poſſeſſes a moſt decided ſuperiority over the former, though not noticed by La Grange and Haſſenfratz, is the facility with which it accounts for the introduction into the blood of a regular ſupply of inflammable matter. According to the former theory, the change from the venous to the arterial ſtate of the blood, depended upon the diſcharge of the ſuperfluous inflammable matter, a proceſs effected in the capillaries of the lungs; the blood was therefore ſuppoſed to return to the right ſide of the heart deprived of this ſubſtance, but in paſſing along the minute arterial branches, it acquired ab extra the hydro-carbonous matter, and was thus brought back to the venous ſtate: I have already alledged the objections againſt this ſuppoſition. In the hypotheſis propoſed by M. La Grange, we may conceive the inflammable matter to enter the blood during any part of the circulation, as the different ſtates which this fluid aſſumes, depend not upon the abſolute quantity of the ſubſtances which it contains, but upon the ſtate of their combination. The nature of the inflammable matter of the blood will be conſidered more fully hereafter, at preſent I ſhall merely ſtate, that the obvious ſource of ſupply conſiſts in the contents of the thoracic duct, which are poured into the left ſubclavian vein.*(a)*

M. M.

(a) Skey de mat. combust. sang.

M. M. La Grange and Haſſenfratz, in conformity with the opinion of M. Lavoiſier, tha the oxygenous part of the atmoſphere only was employed in reſpiration, ſuppoſed that this gas alone is abſorbed in the lungs. The contrary opinion is, however, embraced by Mr. Davy. He obſerves, that from the experiments of Prieſtley and Cigna it appears, that venous blood can become florid through a ſtratum of ſerum, and that conſequently either the whole of the air of the atmoſphere, or the oxygenous part of it, muſt be diſſolved by the ſerum before it can arrive at the red particles, ſo as to change their colour. He thinks it probable that the whole air is abſorbed by the ſerum, and that in this condenſed ſtate, it is decompoſed by the affinity which ſubſiſts between oxygene and the red globules. A small portion of the azote is alſo retained by the blood, but the greateſt part of it is liberated without undergoing any change. *(a)*

Dr. Thomſon alſo ſuppoſes, that the atmoſpheric air in its whole ſubſtance is abſorbed, but that the greateſt part of the azote is again diſcharged. In favour of this ſuppoſition, he urges the experiments of Dr. Prieſtley, and Mr. Davy,

(a) Researches, p. 447.

Davy, in which a ſmall quantity of azote is conſumed in reſpiration, and alſo the experiments of the latter upon the reſpiration of the nitrous oxide. As in this caſe the air of expiration conſiſts of a part only of the gas inſpired, mixed with a quantity of azote, it may be inferred, that the nitrous oxide is decompoſed by the blood, a proceſs which both Mr. Davy, and Dr. Thomſon conceive, can only be brought about by its having been previouſly diſſolved in this fluid. *(a)* But the ſtate of combination in which the oxygene and azote exiſt in the air of the atmoſphere, and in the nitrous oxide, differs ſo conſiderably, that even admitting the abſorption of this latter compound by the blood, we ſhall not be authorized to infer, that the ſame action will take place with reſpect to the former. Upon the whole I think it probable, that the blood exerciſes its attraction upon the oxygene alone, but that in conſequence of the powerful affinity which a large maſs of one of theſe gaſes poſſeſſes for a ſmall quantity of the other, the oxygene which is abſorbed by the blood, ſtill retains a minute portion of azote in combination. *(b)*

It appears then that the change produced in the

(a) Davy's Researches, loc. cit. Thomson's Chemistry, v. iv. p. 492. Note 50. *(b)* Note 51.

the compofition of the blood, during its paffage through the lungs is two-fold; a portion of inflammable matter is difcharged from it, and a quantity of oxygene is abforbed. It feems upon the whole probable, that thefe changes are contemporary, and that the abforption of oxygene, and the difcharge of inflammable matter, at leaft in ordinary cafes, proceed in exact proportion to each other. The immediate effect upon the blood is to alter its colour from a deep purple to a bright fcarlet. M. Haffenfratz's experiment, in which arterial blood affumed the purple hue, when enclofed in tubes hermetically fealed, (a) favours the fuppofition, that this change of colour is more influenced by the prefence of the oxygene than of the inflammable matter.

As the blood is a heterogeneous fluid, compofed of feveral fubftances, which are loofely connected together, and poffefs different chemical properties, it has been made a fubject of inquiry, upon which of its conftituents is the action of the air more immediately exercifed. Of the two parts into which the blood feparates by its fpontaneous coagulation, the craffamentum and the ferum, the former only has been made the fubject of experiment,

(a) *Ann. de Chim. t.* ix. *p.* 269.

periment, and has been found to effect a diminution of oxygene, and a production of carbonic acid gas in the same manner, as when the entire mass of blood is exposed to the action of the air. From this circumstance, and also from the apparent similarity of the serum to other animal substances which are not known to have any specific action upon the air, it is inferred, that the crassamentum is the immediate agent in producing the change which is brought about in the process of respiration. The crassamentum itself is a compound body, consisting of fibrine and the red particles, the former of these substances appears to possess the same chemical properties with the muscular fibre, whereas the red particles are bodies of a peculiar organization, they give the blood its colour, and by the different shades which they assume, afford the principal indication of the approaches which it makes to the venous and arterial states. On this account it has been presumed, that the power of attracting the oxygene from the atmosphere, resides in the red particles. The nature, and even the form, of these particles is still involved in much obscurity; there is perhaps no subject in the whole range of physiology, respecting which more contradictory opinions have been advanced. *(a)* It has,

(a) Note 52.

has, however, been long known that they contain a quantity of iron, and from ſome late experiments performed by M. M. Fourcroy and Vaquelin,(*a*) the iron appears to be united to the phoſphoric acid, the compound ſtill retaining an exceſs of the metal in an oxidated ſtate. The other component parts of the red globules have been examined merely by the vague method of deſtructive diſtillation, from which it can only be aſcertained, that they contain the ſame chemical elements with animal ſubſtances in general. The experiments of M. M. Fourcroy and Vaquelin render it probable, that the colour of the red particles depends upon the phoſphate of iron, but it is ſtill uncertain, whether this metallic ſalt poſſeſſes the property of attracting the oxygene from the atmoſphere, or whether this operation be not rather effected by the red particles in their whole ſubſtance. (*b*)

Lavoiſier was the firſt philoſopher, who formed any accurate idea reſpecting the nature of the inflammable matter diſcharged from the blood. In his firſt memoir in 1777, he conſiders it as compoſed entirely of carbone, but he afterwards conceived that a quantity of hydrogene was alſo diſengaged from the blood, and this opinion has been

(*a*) *Systeme des con. chim. t.* ix. *p.* 152. (*b*) Note 53.

been adopted both by the French and Engliſh phyſiologiſts. The firſt of theſe effects, the diſcharge of carbone from the blood, appears to be ſufficiently eſtabliſhed ; a quantity of carbonic acid is found in the air after expiration, which did not previouſly exiſt in it, while the air is found to be deprived of part of its oxygene. Carbonic acid is aſcertained to be a compound of oxygene and carbone, and conſequently the oxygene which has diſappeared, is ſuppoſed to be converted into carbonic acid, by the addition of a quantity of carbone which has been abſtracted from the blood. *(a)*.

The proof of the diſcharge of hydrogene from the blood is leſs direct. It has been ſuppoſed, in the ſame manner with the carbone, to be emitted in combination with oxygene, and it is indeed true, that a quantity of water, the ſubſtance produced by the union of hydrogene and oxygene, is expelled along with the air of expiration. But we may point out another origin for the aqueous vapour without having recourſe to the hypotheſis of its generation, by the direct union of hydrogene and oxygene, either in the lungs, or in the courſe of the circulation. The inner ſurface of the bronchia and air veſicles, is lined with a copious

(a) Note 54.

pious ſecretion of a mucous matter, and as the lungs are continually kept at a temperature of 98°, it may be ſuſpected that the water exhaled from them proceeds merely from the evaporation of part of this fluid. As, however, the contrary opinion is generally received, and is countenanced by the higheſt authorities, it will be proper to examine upon what foundation it is ſupported.

It appears that the diſcharge of hydrogene from the lungs was firſt ſuſpected by Lavoiſier, in conſequence of the experiment which he made upon the reſpiration of the guinea-pig in oxygene. *(a)* He was induced to form this conjecture by obſerving, that a greater quantity of oxygene had diſappeared than what was ſufficient to compoſe the carbonic acid which was produced, and he conſidered its union with hydrogene, and the conſequent production of water, as the moſt probable method in which the ſuperabundant oxygene could be employed. M. Lavoiſier had already, in a former ſet of experiments, noticed, that ſomewhat more caloric was diſengaged by the proceſs of reſpiration than could be aſcribed to the formation of the carbonic acid emitted from the lungs,

(a) Ann. de Chim. t. v. *p.* 265; *and t.* xxi. *p.* 227. *Mem. Soc. Roy. Med.* 1782, 1783, *p.* 574.

lungs,*(a)* and this difficulty he ſuppoſed might alſo be removed, by admitting the formation of a quantity of water, a proceſs in which, as well as in the generation of carbonic acid, caloric is evolved. It is evident, that the firſt of theſe arguments for the formation of water in the proceſs of reſpiration, depends, in a great meaſure, upon the truth of the peculiar theory advanced by M. Lavoiſier, which ſuppoſes that the oxygene attracts the inflammable matter of the blood during its paſſage through the lungs, without entering into that fluid. But according to the hypotheſis advanced by M M. La Grange and Haſſenfratz, the whole of the oxygene employed in reſpiration is conceived to be in the firſt inſtance abſorbed by the blood, and there is no reaſon to conclude, that the exact quantity of this gas, which is received into the ſyſtem by the lungs, is always diſcharged by the ſame channel. Even if we were to admit this to be the caſe, it would ſtill remain to be proved, that the carbone which enters into the compoſition of the carbonic acid, had previouſly exiſted in that ſtate of combination which conſtitutes pure charcoal.*(b)* This ſubſtance was conceived by Lavoiſier to be an elementary body, and his calculations were formed upon

(a) Mem. Acad. 1780, *p.* 405. 1789. *p.* 569. *(b)* Note 55.

upon this ſuppoſition; but the experiments of Guyton and Tennant, have demonſtrated, that what was formerly conſidered as pure carbone, already contains ·36 of oxygene, and that conſequently, carbonic acid conſiſts of a greater proportion of oxygene than Lavoiſier ſuppoſed. It is however unneceſſary to purſue this calculation, as we are not aſſured of the truth of the previous ſteps upon which the hypotheſis reſts.

With reſpect to the want of correſpondence between the quantity of caloric evolved in the lungs, and the carbonic acid generated, although we might endeavour to remove this difficulty, by taking into account the caloric diſengaged during the formation of water, provided this circumſtance were deciſively proved by other evidence, yet it muſt be confeſſed, that the experiments reſpecting animal heat, and the apparatus employed for meaſuring its exact quantity, are of too delicate a nature to ſerve as a proof of the fact.

In addition to theſe arguments, which were urged by M. Lavoiſier, to prove the generation of water in reſpiration, M. Seguin adduces the experiments of Cigna, Prieſtley, and Hamilton, where the ſcarlet colour of the arterial blood was rendered purple by expoſure to hydrogenous

genous gas. The hydrogene, he ſuppoſes, is in this caſe abſorbed by the blood, and he concludes, that if the abſorption of hydrogene can convert arterial into venous blood, the diſcharge of this body muſt bring it back to the arterial ſtate. *(a)* I think it a ſufficient reply to this argument to remark, that the ſame change in the colour of arterial blood is brought about by the application of the azotic and carbonic acid gaſes, and even by placing it in vacuo. *(b)*

From this view of the arguments which have been adduced to prove the generation of water in the lungs, and at the ſame time from a conſideration of the more obvious ſource of the aqueous exhalation, which is furniſhed by the moiſture always exiſting on their ſurface, I do not heſitate to conclude, againſt the authority of Lavoiſier, *(c)* that the diſcharge of hydrogene from the blood has been admitted without ſufficient evidence, and that at preſent we have no proof of the emiſſion of any ſubſtance from this fluid, depending upon the effects of reſpiration, except carbone. *(d)* I regard the water of the pulmonary exhalation as produced by evaporation

K from

(a) Ann. de Chim. t. xxi. *p* 225. *&* *seq.* *(b) Priestley's exper. on Air, v.* iii. *p* 364. *(c)* Note 56. *(d)* Note 57.

from the mucous fluid which lines the inner ſurface of the bronchia and veſicles, and which is ſeparated from the blood by ſecretion; the water that is contained in the blood mixed with the ſerum, is poured into it along with the contents of the thoracic duct, which are at all times largely diluted with this fluid. *(a)*

The arterial has been obſerved to differ from the venous blood, in containing a leſs proportion of ſerum and water, and this difference has been attributed to the operation of the lungs; *(b)* but it may, with more propriety, be aſcribed to the effects of ſecretion and tranſpiration. It is probable that the blood in its paſſage along the capillary arteries, is continually parting with its fibrine, in order to repair the waſte which the muſcular fibres experience, and the proportion of its ſerum will be conſequently increaſed when it arrives at the veins. On the other hand, during its paſſage through the minute arteries of the lungs, a quantity of the water which it contained is employed in forming the mucus which lines the bronchia and veſicles, and in ſupporting the pulmonary tranſpiration. In the mean time the blood has received a freſh ſupply of chyle, which is

(a) Note 58. *(b) Richerand, p.* 133.

is the immediate ſource whence the fibrine is produced.

The hypotheſis advanced by M. Cuvier, *(a)* which ſuppoſes that reſpiration aſſiſts in the converſion of chyle into fibrine, appears to be better founded. We learn from accurate analyſis, that there is a greater proportion of azote, and conſequently a ſmaller proportion of carbone, in fibrine than in the chyle, the ſubſtance from which the fibrine is formed, ſo that in order to produce this change, it is neceſſary to remove from the chyle a part of its carbone; but this we have already found to be one of the principal effects produced by the proceſs of reſpiration. *(b)*

The thoracic duct pours its contents into the left ſubclavian vein, at a little diſtance from the termination of the venous ſyſtem in the right ſide of the heart. It has been obſerved, that when the blood arrives at this part of the circulation, the chyle ſtill exiſts in a ſtate of imperfect mixture with this fluid, but that after it has completed its paſſage through the lungs, the globules of chyle are no longer viſible, and the whole forms one uniform maſs. This intimate mixture

 of

(a) *Leçons d'anat. comp. t.* i. *p.* 92. *(b)* Note 59.

of the chyle with the other parts of the blood, has been confidered as an effect of the refpiration, but I think it more probable, that it is merely a mechanical operation, produced by the tranfmiffion of the blood through the minute network of veffels which compofes the termination of the pulmonary veins, and the commencement of the correfponding arteries.

The difcovery of Dr. Black, that bodies which indicate the fame temperature, are frequently combined with different quantities of caloric, perhaps the moft valuable of all the modern difcoveries in chemiftry, opened a path to the explanation of the fingular phenomena of animal heat. This philofopher had previoufly examined the change effected in the air by refpiration, and had found it to be in many refpects fimilar to that produced by combuftion; he accordingly inferred, that in the fame manner as in the burning of an inflammable body, an extrication of caloric muft take place in the lungs. This hypothefis refpecting the origin of animal heat was, however, advanced by Dr. Black, only as a plaufible fpeculation, and remained open to many objections, when Dr. Crawford turned his attention to the fubject. He performed many elaborate experiments to determine the fpecific heat of

a variety

a variety of different ſubſtances, and purſuing the analogy which had been pointed out by Dr. Black, between the effects of combuſtion and reſpiration, he particularly examined the ſtate of the blood before and after it had paſſed through the lungs. The reſult of his labours was the diſcovery of the important fact, that the blood has its capacity for heat increaſed by its converſion from the venous to the arterial ſtate, in conſequence of the change produced in it by the action of the air. He determined the capacity of venous and arterial blood to be in the proportion of 10 to 11½.(a)

As the production of animal heat appears to be one of the moſt important uſes of reſpiration, I ſhall, in adherence to my propoſed plan, defer to the 4th diviſion of this eſſay, the farther conſideration of this curious part of the animal conſtitution. I ſhall then examine at ſome length the theory of animal heat as originally propoſed by Dr. Crawford, together with the various modifications which it has received from the ingenuity of thoſe phyſiologiſts who have ſtill more recently inveſtigated the ſubject.

We have now reviewed in ſucceſſion the changes

(a) *Observations on Animal heat, p.* 277.

changes which are ſuppoſed to be effected in the blood by reſpiration. It will be perceived, that notwithſtanding the diſcovery of many important facts, the ſubject ſtill remains involved in much obſcurity. This muſt be, in a great meaſure, attributed to the difficulty of performing experiments upon a ſubſtance like the blood, compoſed of a number of different ingredients, connected together by a complicated ſyſtem of affinities, which is liable to be diſturbed by the operation of almoſt every external body. The difference between the arterial and the venous blood is rather inferred from the compariſon of a number of obſervations, that have been made upon the body in its different ſtates, than aſcertained by the direct reſult of experiment, and conſequently the information which we have acquired upon this ſubject, is ſtill vague and imperfect. There are, however, a few points, which the labours of the modern phyſiologiſts ſeem to have clearly eſtabliſhed, and from the progreſſive ſtate of chemical ſcience, we have reaſon to expect that their number will gradually increaſe.

The preſent ſtate of our knowledge, reſpecting the change produced on the blood by reſpiration, may, I think, be comprized in the following propoſitions.

1. The

1. The blood which is expelled from the right ventricle of the heart, into the pulmonary artery, is of a dull purple colour; during its paſſage through the capillaries of the lungs, its colour is converted into a bright ſcarlet, in which ſtate it is returned to the left auricle of the heart.

2. The change of colour is effected by the oxygenous part of the atmoſpheric air, which is received into the veſicles of the lungs.

3. The blood in paſſing through the lungs, emits a quantity of carbone, which is expired in combination with oxygene, under the form of carbonic acid gas.

4. The blood poſſeſſes an attraction for oxygene, and during its paſſage along the capillaries of the lungs, it abſorbs a quantity of this ſubſtance.

5. To one of theſe circumſtances, viz. the emiſſion of carbone, and the abſorption of oxygene, or to the combination of the two, is to be aſcribed the change from the purple to the ſcarlet colour, which the blood undergoes during its paſſage through the lungs.

6. The

6. The ſame change of colour is produced, when a portion of the clot of venous blood, is expoſed to the action of oxygenous gas, and the proceſs is not interrupted by the interpoſition of a moiſtened membrane, or a ſtratum of ſerum, between the craſſamentum and the air.

7. When arterial blood is expoſed to the azotic, hydrogenous, or carbonic acid gaſes, or placed in the vacuum of an air pump, it aſſumes the purple hue of venous blood.

8. The oxygene, which is, in the firſt inſtance, looſely combined with the whole maſs of blood, during the courſe of the circulation, gradually enters into a more intimate union with a part of its carbone, and forms with it an oxide. When this oxide is brought back to the lungs, it unites with an additional quantity of oxygene, ſo as to form carbonic acid, and is then removed with the air of expiration.

9. The blood receives its ſupply of carbonaceous matter from the contents of the thoracic duct, which are poured into the left ſubclavian vein.

10. It

10. It has been ſuppoſed that, beſides the carbone, a quantity of hydrogene is diſcharged from the blood by the lungs, but, this opinion appears to have been adopted, without ſufficient foundation.

11. In conſequence of the abſtraction of a quantity of carbone from the blood, the proportion of azote in the reſiduum is augmented; it is alſo probable, that a ſmall quantity of azote is abſorbed by the blood, immediately from the atmoſphere.

12. When blood is converted from the venous to the arterial ſtate, its capacity for heat becomes increaſed; the ſpecific heats of venous and arterial blood are in the proportions of 10 to 11½.

13. The blood, in conſequence of its paſſage through the lungs, appears to have its conſtituent parts more uniformly mixed together, but this effect is to be attributed merely to the agitation and preſſure which it experiences during its courſe through the minute and convoluted net-work of veſſels which compoſe the capillaries of the lungs.

14. Beſides

14. Beſides the circumſtances which have been enumerated, the arterial differs from the venous blood, in conſequence of the ſubſtances ſeparated from it to form the different ſecretions; but theſe changes are not to be attributed to the effects of reſpiration, and it is abſolutely impoſſible to appreciate their amount.

15. A conſiderable quantity of aqueous vapour is emitted from the blood, but this is the effect of tranſpiration, and conſiſts of water which had been poured into the blood ready formed by the thoracic duct, along with the other products of digeſtion and abſorption.

CHAP.

CHAP. IV.

On the respiration of the different Gases.

IT is probable that no aeriform fluid, except the compound of oxygene and azote which exiſts in the atmoſphere, is adapted to the permanent ſupport of life. Of the other gaſes, there are ſome, which on account of their irritating nature, it is abſolutely impoſſible to receive into the trachea; theſe properly conſtitute the non-reſpirable gaſes. There are, however, others which it is poſſible to inſpire, though their employment is followed ſooner or later by the extinction of life. I ſhall detail ſome of the principal experiments that have been performed on this ſubject, as the nature of the change produced upon the blood by common reſpiration may be in ſome meaſure illuſtrated, by obſerving the effects

fects which follow the ufe of the other gafeous bodies.

Soon after the difcovery of the appropriate power which the oxygenous part of the atmofphere poffeffes, of fupporting animal life, feveral trials were made of the effects which would refult from breathing it in an unmixed ftate. The accounts given by thofe who were the fubjects of thefe trials were various; Dr. Prieftley, who firft made the experiment, conceived that he felt an agreeable lightnefs in the cheft,*(a)* fome perfons fuppofed that it produced exhilarating effects upon the fyftem, while others imagined that the employment of this gas was followed by uneafinefs and pain about the region of the thorax. Thefe different fenfations muft be attributed in a great degree to the effects of imagination; in part, however, they may be afcribed to the gas which was employed in the earlier period of the pneumatic experiments having been often in an impure ftate, mixed with acid, acrid, or metallic particles.*(b)* A difference in the effects produced by the refpiration of the gas, ought perhaps in fome degree alfo, to be imputed to the manner in which it was received into the lungs, whether only

(a) Observ. on Air, v. ii. *p.* 162. *(b)* Note 60.

only in ſmall quantities, or by deep and laborious inſpirations, and whether it was employed in a condenſed or a rarified ſtate.

As Dr. Prieſtley was the firſt perſon who himſelf reſpired oxygenous gas, ſo he was likewiſe the firſt who obſerved the effects which it produced upon animals altogether immerſed in it. His experiments were performed upon mice; they decidedly proved the ſuperior power which this gas poſſeſſes of ſupporting animal life, but no other certain concluſions can be deduced from them.*(a)* M. Lavoiſier afterwards turned his attention to this ſubject, and in the experiments upon guinea-pigs, to which I have already referred, he noticed with more accuracy the effects reſulting from the reſpiration of oxygene. He examined the ſtate of the internal organs of animals which had been for ſome time confined in this gas, and he conceived that a degree of redneſs and turgeſcence of the veſſels was produced, and other effects which indicated that the ſanguiferous ſyſtem had been in a ſtate of encreaſed action.*(b)* There is, however, reaſon to infer, that in this caſe, either the gas employed was in an impure ſtate, or that there were ſome circumſtances attending the ſitua-

tion

(a) Note 61. *(b) Mem. Soc. Roy. Med.* 1782, 1783. *p.* 576.

tion of the animals, or the manner in which the experiment was conducted, which affected the results; for the same philosopher, in the subsequent memoir of 1789, where there appears to have been the greatest attention to accuracy, and where the most perfect apparatus was employed, forms entirely opposite conclusions. In this paper, we are informed, that he confined guinea-pigs in pure oxygene, and in mixtures of oxygene and azote, in different proportions, until the former constituted only one-fifteenth part of the compound. In all these cases, he found that the same quantity of oxygene was consumed, a circumstance, he observes, in which respiration differs remarkably from combustion, though in many respects, these operations are similar to each other. The effects produced upon the animals were precisely the same, whether they were confined in pure oxygene, or in any of the mixtures of it, except that when the proportion of azote was very large, they exhibited marks of drowsiness. The author expressly informs us, that neither the temperature nor the circulation were in any respects affected by the inspiration of pure oxygene for the space of several days.(a) These experiments must be considered as very valuable; there is no reason to doubt

(a) Note 62.

doubt their accuracy, and they may be relied on with more confidence, as the author ſeems to have had no peculiar theory in view when he performed them; indeed the reſults are different from what we might previouſly have expected, and are unfavourable to the analogy which Lavoiſier had always endeavoured to eſtabliſh between reſpiration and combuſtion.

We have an account given by Dr. Higgins of the reſpiration of pure oxygene by the human ſubject; in one experiment, thirty-eight pints of this gas were reſpired without interruption. No inconvenience was experienced, a ſenſe of warmth was, however, produced in the cheſt, and the pulſe was conſiderably quickened.(a) From the experiments of M. Lavoiſier, I am induced to conjecture, that this encreaſed rapidity of the circulation muſt be attributed rather to the greater efforts which almoſt neceſſarily attend reſpiration when continued for any length of time in the way in which it was performed in this proceſs, than to the effects of the application of the increaſed quantity of oxygene to the lungs. Dr. Higgins, in a 2d experiment, breathed a quantity of oxygene under an additional preſſure, and by

(a) *Minutes of a Society, &c. p.* 144—6.

by very full inſpirations. He conceived that by theſe means its conſumption was much promoted,(a) but more numerous and accurate expeririments will be required before this inference can be fairly eſtabliſhed.

In the year 1794, Dr. Beddoes publiſhed his experiments upon this ſubject. They were performed upon rabbits, and the attention was principally directed to an examination of the ſtate of the internal organs of the animal, after it had been ſubjected for ſome time to the influence of pure oxygene. The author commences his inquiry, by ſtating, that "Dr. Prieſtley and "M. Lavoiſier, found animals either to die or to "become exceedingly ill in ſuch air," (oxygene little diluted) "while it continues more oxidated "than the atmoſphere." (b) There is no reference made to any particular paſſage in the writings of theſe philoſophers, but from the extracts which I have given, it would appear that their ſentiments are altogether of an oppoſite nature. (c) Dr. Beddoes quotes the earlier experiments of M. Lavoiſier, but the later and more deciſive ones in the memoir of 1789 are not noticed. It is on the contrary taken for granted, that when an

(a) *Minutes of a Society, &c. p.* 152. (b) *Beddoes on fact. Airs, pt.* i. *p.* 13. (c) See Notes 61 and 62.

an animal has been for ſome time confined in air, which contains a greater proportion of oxygene than the atmoſphere, the body becomes, as he expreſſes it, oxygenated He had previouſly formed an hypotheſis reſpecting the operation of oxygene upon the ſyſtem, according to which the long continued inſpiration of it was ſuppoſed to induce an inflammatory ſtate, and he accordingly found indications of this peculiar condition in every part which he examined. The lungs were florid, and in ſome places they were marked with livid ſpots, the pleura was inflamed, the heart retained its irritability for a longer ſpace of time than uſual, the blood coagulated more rapidly, and the liver was of a lighter colour than ordinary. The whole ſyſtem was, as it were, ſo completely ſaturated with oxygene, that the immerſion in hydrogenous gas, and in water, did not deſtroy the animals, until after a longer interval than uſual; a fact which remarkably coincided with the theory reſpecting the cauſe of death in ſubmerſion, which the author had previouſly formed. The great difference between the reſults of theſe experiments, and thoſe of M Lavoiſier, will appear the more remarkable when we conſider, that in the experiments of the latter, the animals were expoſed to the influence of the oxygene for ſeveral days, whereas thoſe of Dr. Beddoes

 occupied

occupied a much ſhorter ſpace of time. Indeed we are expreſſly informed, that the air in which the animal had been confined, "ſeemed to "have ſuffered little diminution either in quan-"tity or quality,"(a) notwithſtanding which, theſe ſingular changes were induced upon the body. It is impoſſible to reconcile opinions and experiments ſo contradictory as thoſe which I have related; our judgment muſt in ſuch inſtances be guided partly by the general character for correctneſs and impartiality which we attach to the works of the reſpective authors, and partly by comparing them with the reſults of experiments which in any degree reſemble them.

We are indebted to Mr. Davy for ſome intereſting experiments upon the reſpiration of oxygene, which appear to have been performed with his accuſtomed correctneſs. He ſuppoſes that life is finally deſtroyed by the long-continued employment of this gas, and refers for the proof of his opinion, not only to the reſults of his own experiments, but to thoſe of Dr. Beddoes and M. Lavoiſier. He does not, however, notice any particular paſſage in the works of this latter author, and conſidering the very direct nature, both of his

(a) *Beddoes on Airs, pt.* i. *p.* 16.

his experiments and opinions upon this point, it may be conjectured, that Mr. Davy has been inadvertently misled by the assertions of Dr. Beddoes. Indeed Mr. Davy's own experiments appear to lead to conclusions diametrically opposite to those of Dr. Beddoes. He respired the same portion of oxygene for several successive times, and then accurately ascertained the exact quantity of it which had been consumed, and of carbonic acid which had been generated, when he found, that both these quantities were less than in the ordinary respiration of atmospheric air, he obtained the same results when mice were made the subjects of his experiments. *(a)* It appears therefore, that in this instance, if any alteration be produced in the system, it must arise rather from a deficiency than from an excess of oxygene. But notwithstanding the attention with which these experiments appear to have been conducted, an objection may, I think, be urged against them from the erroneous estimate which Mr. Davy formed of the capacity of the thorax in a state of complete expiration, a datum which is employed in the calculation of the quantity of oxygene left after the process. *(b)*

From the review of the experiments which have

(a) Researches, p. 444.

(b) Note 68.

have been made upon the ſubject, I think myſelf juſtified in concluding, that we have not yet any direct proof that the reſpiration of oxygene is injurious to the animal œconomy. It is, however, not impoſſible that it may be the caſe, were the gas applied to the lungs for a ſufficient length of time, but we have no ground either from experiment or obſervation, to enable us to determine what peculiar morbid action would be induced.

Among the remaining reſpirable gaſes, that which appears to be the leaſt injurious to the living body is the oxide of azote. This aeriform fluid was firſt diſcovered by Dr. Prieſtley, and was by him ſuppoſed to be "in the higheſt de- "gree noxious to animals." *(a)* The ſociety of Dutch chemiſts, who afterwards examined its properties with more accuracy, coincided with Dr. Prieſtley, as to its effects upon animal life. *(b)* The experiment was, however, repeated by Mr. Davy, and he diſcovered not only that this gas may be reſpired for a ſhort time without inconvenience, but that the employment of it is ſucceeded by a ſingular excitement of the nervous ſyſtem, which differs from that produced by alco-

hol

(a) Observ. on Air, v. ii. *p.* 55. *(b) Journ. de Phys t.* xliii. *p.* 329

hol and opium, in not inducing a ſubſequent ſtate of exhauſtion. Mr. Davy has alſo proved, that it is abſorbed by the blood in conſiderable quantity, and he imagines that it is decompoſed by this fluid. (a) I have already ventured to expreſs my doubts as to the correctneſs of this latter concluſion. (b)

Hydrogenous gas has been frequently reſpired, (c) and it is now pretty generally agreed, that it is altogether paſſive when received into the lungs, and that death ſucceeds the employment of it in conſequence of the excluſion of oxygene, in the ſame manner as by ſuffocation, or drowning. M. Lavoiſier diſtinctly aſcertained this fact, in his experiments related in the memoir of 1789, and it has been ſince confirmed by Mr. Davy. (d) It muſt be remarked, however, that a contrary opinion reſpecting the effects produced by the reſpiration of hydrogene has been maintained by ſome eminent chemiſts, even by Dr. Prieſtley himſelf; (e) but his experiments were made in the earlier period of the pneumatic chemiſtry,

(a) *Researches, p.* 409, *& seq.* —— (b) See Note 50. (c) *Scheele on Air and Fire, p.* 160..*Fontana, Phil. Trans.* 1779, *& Journal de Phys. t.* xv..*Pilatre de Rozier, Journ. de Phys. t.* xxviii. *p.* 425. —— (d) *Researches, p.* 466. (e) *Observ. on Air, v.* i. *p.* 229.

chemiftry, when the gafes were frequently employed in an impure ftate, and the experiments of Mr. Davy, clearly demonftrate that hydrogenous gas produces different effects upon the fyftem, according to the fubftances from which it is procured.(a)

The experiments that have been made upon the fubject of azotic gas are few and imperfect; it has been generally fuppofed, that it exercifes no noxious effects upon the blood, but like hydrogene, deftroys life fimply by preventing the accefs of oxygene. Dr. Higgins indeed remarks, that an animal dies fooner when immerfed in this gas, than from the fimple interruption to refpiration,(b) but we are not informed upon what data this opinion is founded. Mr. Davy likewife experienced the fenfe of fuffocation more fpeedily from the ufe of azote, than from that of hydrogene, but it appears that the gas employed in the experiment contained a quantity of carbonic acid,(c) to which we may, with great probability, afcribe its noxious effects; and the fame philofopher, when fpeaking in general terms of the action of azote in refpiration, feems to confider it as merely excluding oxygene.(d) This opinion

(a) Researches, loc. cit. *(b) Minutes of a Society, &c. p.* 133. *(c) Researches, p.* 466. *(d) Do. p.* 334.

opinion is adopted by Dr. Thomſon,*(a)* and was uniformly maintained by M. Lavoiſier.*(b)* It would certainly appear reaſonable to conclude a priori, that a ſubſtance which enters ſo largely into the compoſition of the atmoſphere, and which conſequently compoſes ſo great a proportion of the contents of the lungs, could not exerciſe any noxious effects upon the animal ſyſtem.

The only remaining gas which is capable of being received into the lungs, is the carbonated hydrogene or hydrocarbone. If it be inſpired in an undiluted ſtate, it is followed by inſtant death, and when employed in ſmall quantity only, mixed with atmoſpheric air or with oxygene, if it be uſed for any length of time, it induces vertigo, dimneſs of ſight, convulſions, loſs of ſenſation, and in ſhort, every ſymptom of approaching diſſolution. It evidently acts more rapidly and powerfully than thoſe gaſes which merely exclude oxygene from the blood, and muſt conſequently be conſidered as exerciſing a poſitively noxious influence upon the animal œconomy. Theſe fatal effects have been attributed by ſome authors to its ſuddenly abſtracting all the looſe oxygene from the

(a) *Chemistry, v.* iv. *p.* 492. (b) *Mem. Acad.* 1789. *p.* 574. *&* *alibi.*

the blood, and thus completely depriving that fluid of the property by which it ſupports the irritability of the muſcular fibres of the heart and arteries. To this hypotheſis it may be objected, that the hydro carbonous gas does not in other caſes appear to poſſeſs that powerful attraction for oxygene, which could induce ſo rapid a change in the compoſition of the blood; and it may be farther urged, that if it be taken into the lungs, mixed with a quantity of oxygenous gas, provided it be not too much diluted, it will ſtill be found to produce its accuſtomed fatal effects. The noxious influence of this gas is, I conceive, with more propriety referable to its action upon the nervous ſyſtem, upon which it produces *directly sedative* effects. (a)

All the remaining gaſes are found to be ſtrictly non-reſpirable, i. e. incapable of being admitted into the trachea. It is obvious that this muſt be the caſe with the irritating acid or alcaline gases, and with the nitrous gas, which during its paſſage into the lungs, muſt unavoidably be brought into contact with oxygene, and thus produce nitrous acid vapour The only ſubſtance reſpecting the reſpirability of which there could be any doubt, is the

(a) Note 64.

the carbonic acid gas, which, though poſſeſſed of the decided characteriſtics of an acid, exhibits them in a much ſlighter degree than that in which they generally exiſt. The impoſſibility of taking it into the lungs, even by the moſt powerful voluntary efforts, when it compoſes a large proportion of the air, was however proved by the experiments of M. Pilatre de Rozier,*(a)* executed with that intrepidity which formed ſo remarkable a trait in the character of this philoſopher.

With theſe remarks I ſhall conclude the ſecond diviſion of my ſubject, and at the ſame time, the preſent volume. The third part of the eſſay, in which I propoſe to treat of the different affections of reſpiration, muſt principally conſiſt of original obſervations and experiments, and though I have made ſome progreſs in them, they are ſtill in too imperfect a ſtate to meet the public eye. Phyſiologiſts have in general been more inclined to form hypotheſes than to execute experiments, and it has neceſſarily enſued from this unfortunate propenſity, that their ſcience has advanced more ſlowly than almoſt any other department of natural philoſophy. The ſubject of reſpiration has indeed of late attracted the attention of the chemiſts,

(a) Journ. de Phys. t. xxviii. *p.* 422. *& seq.*

chemifts, and its phenomena have been inveftigated with confiderable affiduity. A proportional advancement in our knowledge has been confequently produced, but there ftill remain many queftions to be folved, and many difficulties to be removed. Some of thefe difficulties have appeared in thofe parts of the fubject which have paffed under our review, but they will be found to exift in much greater number in the remaining divifions. How far it will be in my power to remove any of them, it would at prefent be prefumptuous in me to determine, though I think myfelf juftified in auguring favourably of the events of my experiments, from the interefting refults of thofe which I have already performed.

NOTES.

NOTES.

Note I.

M. CUVIER ſtates, that there are ſome very imperfect animals, in which no appearance of reſpiratory organs has yet been diſcovered; *(a)* but we are induced by analogy to ſuſpect, that in theſe caſes, future and more accurate obſervations will enable us to detect the apparatus by which a function is performed, which ſeems abſolutely eſſential to the exiſtence of animal life.

In conformity with the cuſtom of the modern phyſiologiſts, the word reſpiration is here employed to ſignify that operation by which the blood and the air are enabled to act upon each other,

(a) *Leçons d'Anatomie Comparée, t.* i. *p.* 42.

other, though, according to the etymology of the term, it ought only to be applied to thoſe animals that are furniſhed with lungs. The neceſſity of oxygene for the ſupport of the life of fiſh, was firſt diſcovered by Dr. Prieſtley *(a)*; it has been ſince confirmed by Sig. Carradori *(b)* and others.

Note II.

A good deſcription of the comparative anatomy of the organs of reſpiration may be found in Mr. John Bell's Anatomy, *vol.* ii. *c.* 3.

Note III.

It has been imagined by ſome phyſiologiſts, that inſects differed from other animals in the chemical change which their reſpiration produced in the air, but this opinion is proved to be erroneous by the experiments of Vauquelin. *(c)*

Note IV.

This idea reſpecting the termination of the bronchia,

(a) Experiments on Air, v. iii *p* 382—7. *(b) Ann. de Chimie, t.* xxix. *p.* 171, 2. *(c) Ann. de Chim. t.* xii. *p.* 273.

bronchia, which is generally adopted by the modern anatomiſts, reſts altogether, or in a great meaſure, upon the microſcopical obſervations of Malpighi. Succeeding phyſiologiſts have not been able to trace this minute ſtructure, and ſome authors of reſpectability have not ſcrupled to deny its exiſtence. Among others, Helvetius endeavours to prove that the bronchia terminate in a cellular or ſpongy tiſſue, compoſed of a delicate or membranous ſubſtance, the cells of which have no determinate figure, or regular connexion with each other. *(a)* There is indeed every reaſon to ſuppoſe with Cheſelden, *(b)* that Willis and other ſubſequent anatomiſts, who have deſcribed and delineated diſtinct, rounded veſicles, have exceeded the limits of accurate obſervation. Malpighi, upon whoſe authority the belief of the exiſtence of this peculiar ſtructure is founded, expreſſes himſelf in the following manner, "Dili-
" genti indagine inveni totam pulmonum molem,
" quæ vaſis excurrentibus appenditur, eſſe aggre-
" gatum quid ex leviſſimis & tenuiſſimis mem-
" branis, quæ extenſæ & ſinuatæ pene infinitas
" vesiculas orbiculares, & ſinuoſas efformant, ve-
" luti in apum favis alveolis ab extenſa cera in
" parietes conſpicimus." And again, " mem-
" branæ iſtæ veſiculæ videntur efformari ex de-
" ſinentia

(a) Mem. Acad. 1718. *(b) Cheselden's Anatomy, p.* 173.

" finentia tracheæ, quæ extremitate, et lateribus " in ampullofos finus faceffens, ab his in fpatia, " & veficulas inæquales terminatur."*(a)* This defcription by no means conveys the idea of the regularly rounded veficle, which has been adopted by fucceeding anatomifts, and is delineated by Willis.*(b)* The analogy of the frog and tortoife is certainly in favour of the opinion of Helvetius; the ftructure of their lungs, which is lefs minute, evidently difcovers the termination of the bronchia to be a congeries of irregular, membranous cavities. Haller, with his accuftomed candour, after ftating the oppofite authorities, confeffes that he was at one time inclined to doubt the generally received opinion, although he afterwards faw reafon to acquiefce in the doctrine of Malpighi.*(c)* M. Dumas, who gives a full account of the different opinions that have been entertained upon this fubject, feems inclined to adopt an intermediate one between that of Helvetius and Haller.*(d)*

Note V.

Many phyfiologifts, fanctioned by the autho- rity

(a) Epist. de pulm. i. *(b) Pharm. Rat. sect.* i. *cap.* i.; *Tab.* 3. *(c) El. Phys.* viii. 2. 29, 30. *& Notæ ad Boer. Prælect. t.* ii. *p.* 154, 5. *(d) El. Phys. t.* iii. *p.* 435—9.

rity of Galen and Harvey, have ſuppoſed, that there was a quantity of air contained between the lungs and the thorax; Hoadley *(a)* and Hales *(b)* endeavoured to prove this point by experiment. Indeed ſo prevalent was this opinion, even half a century ago, that Haller thought it neceſſary to apologize for embracing the oppoſite ſide of the queſtion; his philoſophic language on the occaſion deſerves to be quoted. "Candide, "nulla parte ſui roboris diſſimulata, adverſariorum "argumenta propoſui; ſuperat, ut oſtendam, "cur ab iis aut auctoritatibus, aut experimentis "non paſſus ſim me perſuaderi. Neque enim "hic quid-quam valuit aut præceptoris mei gra-"tia, aut adfinis mei, adque eam uſque primam "illam Boerhaavianæ ſententiæ propugnationem "ſatis amici, Hamburgeri odium, neque concer-"tationis inde natæ tædium; neque ulla certe "cauſa mei diſſentiendi eſt, præter ea, quæ "nunc produco, rationum momenta. Neque me "malus pudor retinuerit. Facile enim video ſi-"quidem contrariam meæ opinionem expenſam "præponderare ſentirem, veram me gratiam, "ſinceramque laudem, apud candidos virtutis "æſtimatores meriturum, ſi ex longo opere, "quod in ætatem durare velim, errorem etiam "ſero

(a) *Lectures on Respiration, chap.* i. *(b) Statical Essays, v.* ii. *p.* 81.

" fero adgnitum profcriberem."*(a)* Many anatomifts and phyfiologifts of the firft refpectability conceived that they had detected fmall pores in the veficles of the lungs, which permitted a part of the air in them to tranfude into the cavity of the thorax, between the pleuræ; *(b)* but later obfervations have decided againft the exiftence of thefe paffages.

Note VI.

The opinions of the moft learned amongft the ancients refpecting the nature and ufe of the diaphragm, are very remote from the ideas which we now entertain upon this fubject. Fabricius feems to have been the firft who clearly pointed out its mufcularity, and its great importance in the act of refpiration. *(c)* Some anatomifts, among others Senac*(d)* and Winflow,*(e)* from the connexion of the centre of the diaphragm with the parietes of the thorax and the mediaftinum, were led to fuppofe, that this part remained immovable, and that it was the fides of it only which had their pofition changed by the contraction

(a) Haller, viii. 2. 5. *Dumas, t.* iii. *p.* 423. *&c seq.* *(b) Boerhaave Prælect. t.* v. *pars.* i. *p.* 22. *(c) Fabricii Op. De Resp. c.* 8. *&c.* *(d) Mem. Acad.* 1724, & 1729. *(e) Mem. Acad.* 1738.

tion of its mufcular fibres; but Haller, and moft of the modern anatomifts conceive that the whole of the diaphragm is lowered during infpiration,*(a)* although it is generally admitted, that the degree of motion in the centre is lefs confiderable.

Note VII.

The contefts of anatomifts on this fubject have been at leaft as violent, as refpecting the exiftence of air in the cavity of the thorax;*(b)* Haller prefaces his account of the controverfy with the following remark: "Multo plus de "iftorum mufculorum officio dubitationis ab "omni tempore fuit, cumque acres inimicitias "harum carnium actio mihi conciverit, accura-"tius erit in hac quæftione verfandum: at in-"terim liceat, inter miferias vitæ humanæ etiam "iftam deploraffe, potuiffe ob res adeo ab omni "utilitate propria, atque confervatione noftri "remotas, tantas iras, tantafque & tam acer-"bas nobis intentare lites."*(c)* The older anatomifts generally fuppofed from the fituation and ftructure of the intercoftals, that the externals by their contraction increafe the capacity of the cheft, and confequently ferve for infpiration, but that the internals on the contrary diminifh the

 cavity

(a) Haller, viii. 1. 36. *(b) Dumas, t.* iii. *p.* 492—6. *(c) Haller,* viii. 1. 13.

cavity of the thorax. This opinion has been adopted by ſome of the more modern phyſiologiſts; *(a)* ſo late as the year 1778, Sabatier, both from obſervations made upon human ſubjects who had received wounds in the thorax, and from experiments performed for the expreſs purpoſe upon dogs, concludes that the action of the intercoſtals, both external and internal, tends to contract the cheſt, and that they therefore belong to the muſcles of expiration. *(b)* The contrary opinion, that both ſets of intercoſtals ſerve for inſpiration, was firſt ſtarted by Mayow; *(c)* Haller ſtrenuouſly defended it, and at preſent this opinion is generally received. He forms a comparative eſtimate of the increaſe of capacity which the thorax receives from the action of the diaphragm and of the intercoſtals. *(d)* The ſame author remarks, after Winſlow and Senac, that the action of the intercoſtals is principally uſeful in thoſe caſes where, from any accidental cauſe, the contraction of the diaphragm is impeded.

From an accurate inſpection of the ſkeleton, and from obſervations made upon the living body,

(a) Hoadley's Lectures, p. 8. *(b) Mem. Acad.* 1778. *Richerand, El. phys. p.* 138. *(c) Tract. quinque, p.* 278, *& seq.* *(d) Boer. Prælect. t.* v. *pars* i. *p.* 84—9 *El. phys.* viii. 4. 6.

body, anatomifts are, for the moft part, agreed in fuppofing, that there is fome difference in the manner in which the function of refpiration is carried on in the male and female fex; the former appearing to make more ufe of the diaphragm, and the latter of the mufcles about the thorax. *(a)* It is fcarcely neceffary to remark, that by this variation of ftructure, the impediments to refpiration, which would otherwife have been produced by a ftate of pregnancy, are in a confiderable degree obviated.

Winflow feems to have committed a miftake in fpeaking of the opinion of Fabricius, refpecting the intercoftals; he fays, "Ab aquapendente "(Fabricius) avoit dés l'an 1599, trés bien de- "montré, que les mufcles internes & externes "concourent enfemble à la même action, c'eft-a- "dire, a lever les cotes." *(b)* Fabricius himfelf employs the following words: "Quapropter dica- "mus nos mufculos externos intercoftales cofta- "rum dilatare, internos vero contrahere, pluri- "bus adducti tum auctoritatibus, tum rationi- "bus." *(c)* And again, "Concludendum igitur "eft externos intercoftales thoracem dilatare, in- "ternos vero contrahere." *(d)* Haller has alfo

 fallen

(a) Boer. Prælect. t. v. p. 145. *(b) Mem. Acad. 1738.* *(c) Fabricii, Op. p. 176.* *(d) Ditto, p. 177.*

fallen into the fame inaccuracy with refpect to this author. *(a)*

Note VIII.

The celebrated Boyle appears to have been the firft, who pointed out the real caufe of the influx of the air into the lungs, when the capacity of the thorax is increafed. *(b)* Some good obfervations were alfo made upon this fubject by his contemporaries, Sylvius, *(c)* Borelli, *(d)* and Mayow. *(e)* Succeeding phyfiologifts for the moft part acquiefced in the opinion of Boyle, though fome of them, not contented with his fimple explanation, have perplexed by their fubtle hypothefes, a fubject in itfelf fufficiently intelligible. *(f)* The procefs is briefly, yet clearly, detailed by Haller in the following fentence. " Si " pulmonem, & una pectus, dilataveris, ut aer " pulmonalium veficularum in amplius fpatium " diffufus rarefcat, pro portione, qua is aer at- " mofphæra incumbente rarior fit, pro ea co- " lumna aerea in eum pulmonem defcendet, eo " celerius,

(a) El. phys. viii. 1. 14. *(b) Boyle's Works, v.* i. *p.* 99. *&c seq.* *(c) Franc. de le Boe Sylvii Opera, p.* 16. *(d) De Motu Anim. p.* 2. *prop.* 82, 83. *(e) Tract: quinque, p.* 271. *&c seq.* *(f) Baglivi Op. p.* 454. *Hoadley's Lectures, p* 12. *Bremond, Mem. Acad.* 1739.

" celerius, quo differentia denfitatum major " erit."

The firft operation therefore is the expanfion of the thorax, which is effected by mufcular contraction; as there is no air between the pleuræ, the lungs act as if fufpended in a vacuum; they confequently follow the expanfion of the thorax, and the external air rufhes into the veficles, as into the partially exhaufted receiver of an air pump, when a communication is formed with the atmofphere. The air enters the lungs becaufe they are expanded; the lungs are not, as was formerly imagined, pufhed open by the force of the air rufhing into them. (a)

Note IX.

Many of the older anatomifts fuppofed, that the lungs were endowed with a certain innate power of motion, by which the alternations of expanfion and contraction might be carried on, independently of the change in the capacity of the cavity containing them. They even attempted to eftablifh this pofition by experiment; but in thefe inftances there muft have been fome fallacy.

(a) *Boer. præl. t.* v. *pars* i. *p.* 15. 49, 50.

fallacy. We know of no method in which animal motion can be produced, except by the contraction of the mufcular fibre, and though a mufcular ftructure in the vefícles has been confidently afferted by fome writers of eminence,*(a)* later and more accurate obfervations have decided againft its exiftence. Helvetius has even endeavoured to prove, that the fibres which are obferved between the rings of the trachea, and the larger branches of the bronchia, are not mufcular,*(b)* but their mufcularity is admitted by Winflow, Haller, and moft other eminent anatomifts. *(c)* The lungs, in common with other bodies compofed of cellular texture, poffefs a degree of elafticity, by means of which, when ftretched beyond a certain limit, they will re-act and expel part of their contents. In the procefs of refpiration, however, they are entirely paffive, as they always remain in complete contact with the parietes of the thorax.

Notwithftanding thefe confiderations, which have very generally induced the modern phyfiologifts to difcard the idea of motion, originating in

(a) Willis, pharm. Rat. p. 9. *Malpighi, Phil. Trans.* 1671. *Bremond, Mem. Acad.* 1739. *(b) Mem. Acad.* 1718. *(c) Winslow's Anat. Sect.* 9. §. 133. *Haller, Phys.* viii. 2. 12.

in the lungs themfelves, it appears, from the following paffages in the Zoonomia, that Dr. Darwin ftill adheres to the old opinion, as he directly attributes their action to a caufe independent of the contraction of the diaphragm or mufcles of the thorax. " By the ftimulus of the blood in " the right chamber of the heart, the lungs are " induced to expand themfelves, and the pecto- " ral and intercoftal mufcles, and the diaphragm, " act at the fame time by their affociations with " them." *(a)* And again, " to thefe increafed " actions of the air cells, are fuperadded thofe of " the intercoftal mufcles and diaphragm, by irri- " tative affociation." *(b)*

M. Dumas alfo adopts the fame opinion, and conjectures, that the lungs are poffeffed of an innate power of motion; he even expreffes his concurrence in the hypothefis of M. Bremond, who inferred from one of his experiments, that the contraction and dilatation of the lungs alternated with that of the thorax, and confequently muft depend upon fome other caufe. *(c)*

When the lungs are removed from the body, and

(a) Zoonomia, v. i. *p.* 40. *(b) Ditto, v.* ii. *p.* 50.
(c) El. phys. t. iii. 440, 1.

and the paſſage into the trachea remains open, they gradually become collapſed, and are contracted into a ſmaller ſpace than they occupied whilſt in the cavity of the thorax. This has been generally aſcribed to the re-action of their elaſtic cartilages and membranes, which, in conſequence of the preſſure of the internal air, not being balanced by any air between the pleuræ, had been, while in the body, retained in a ſtate of over-diſtention. Boerhaave and his commentator Haller, attribute part at leaſt of this effect to the contraction of the muſcular fibres of the trachea and bronchia; *(a)* but it may be obſerved in oppoſition to ſuch great authorities, that the contractile power of the lungs, remains for a long time after their removal from the body, and cannot therefore with propriety, be aſcribed to a cauſe which muſt ceaſe with the vitality of the part.

Note X.

An elaborate account of theſe muſcles, and of the effects produced upon the thorax by their contraction, will be found in *Boer. prælect. t.* v. *pars* i. *p.* 126—144, *and Haller* viii. 1. 17—31..

The

(a) Boer. prælect. t. v. *pars* i. *p.* 6.

The moſt eminent anatomiſts have differed in opinion, reſpecting the degree of motion which the ribs exerciſe in reſpiration; while ſome authors, with Borelli *(a)* have conceived the action of the intercoſtals to be eſſential, others have adduced inſtances in which the cartilages of the ribs were completely oſſified, without any conſequent impediment to this function. *(b)* It is alſo acknowledged, that in the moſt accurate obſervations and experiments upon the living body, the motion of the ribs was ſcarcely viſible to the eye. Upon the whole, it ſeems the moſt probable, that they are nearly at reſt in ordinary reſpiration, but that in the moſt violent actions of the thorax, or in caſes where the contraction of the diaphragm is impeded by accident or diſeaſe, they are employed to compenſate for the deficiencies of this organ. But though there has been ſome difference of opinion reſpecting the degree of the motion of the ribs, and the occaſions on which

(a) *Borelli, p.* ii. *prop.* 84. *(b)* *Winslow Mem. Acad.* 1738; *Fabricius de Resp.* c. x. *sub finem*; Haller quotes *Cheselden, osteog. c.* iii. as recording ſome caſes where no injury was produced to the reſpiration by anchyloſes of the ribs; but this appears to be an overſight; Cheſelden ſays, "I have twice found them" (the cartilages which join the ribs and ſternum) "totally oſſified in men between 40 and "50 years of age, both of which died with a great diffi"culty of breathing."

which it is exerted, it is unanimoufly acknowledged, that the mechanifm of their articulations is peculiarly admirable. According to the obfervation of Bellini, the thorax is increafed in all its dimenfions, while its figure remains unchanged. (a)

The authors who have treated this fubject with the moft accuracy are, *Borelli, p.* ii. *prop.* 81—95. *Senac, Mem. Acad.* 1724. *Winslow, ibid.* 1720 & 1738. *Boer. Instit.* §. 615 ; *prælect. passim. Haller, phys. lib.* viii. *passim. Dumas, El. phys. t.* iii. *p.* 429—434.

Note XI.

An account of the various eftimates which were formed by the older phyfiologifts may be found in *Haller, El. phys.* viii. 4. 6.

Note XII.

Thefe eftimates were formed from perfons who had died of difeafe, the capacity of the lungs after hanging he found to be confiderably greater; the average taken from experiments made

(a) *Lemm.* 11.

made upon three bodies, was 260 cubic inches. The author, however, obferves, that fear always produces a deep infpiration, and that on this account, in confequence of the peculiar circumftances of their death, their lungs would be in a ftate of extraordinary diftention.(a) Mr. Coleman, on the contrary, from an experiment made for the exprefs purpofe upon a dog, concludes, that after hanging, the lungs contain only a very fmall quantity of air.(b) During the violent ftruggles which precede death, he fuppofes that the animal retains the power of expelling air from the trachea, though the preffure of the cord prevents any from being received. My friend Dr. Skey, in order to obviate the apparent contradiction between Dr. Goodwyn's and Mr. Coleman's experiments, fuggefted that the lungs of a man after hanging muft contain more than their ufual quantity of air, becaufe from the effects of terror, a full infpiration would be made immediately before death, and the air thus received muft be, for the moft part, retained by the preffure of the cord, while in an animal unconfcious of its approaching fate, it will be a matter of accident whether the lungs were in a ftate of infpiration or expiration at the moment of death. But I think that

(a) *Goodwyn, p.* 25, 26. (b) *p.* 97, 8.

that this idea, though ingenious, is ſcarcely ſufficient to remove the difficulty. The capacity of the lungs in the three criminals examined by Dr. Goodwyn, coincides very nearly with the eſtimate of the ordinary capacity of the thorax, deduced from another method of calculation. We muſt therefore either ſuppoſe, that in hanging, the compreſſion of the trachea is produced ſo ſuddenly as to retain the thorax in its ordinary ſtate of diſtention, or if we conceive that an additional quantity of air is received from the effects of fear, we muſt conclude that it is expelled at the moment when the ſenſe of ſuffocation firſt begins to be experienced, but that the paſſage being almoſt inſtantly cloſed, prevents the farther evacuation of the lungs. Upon the whole, I am inclined to the latter ſuppoſition.

With reſpect to Mr. Coleman's opinion, he informs us that "this experiment was often re-"peated, and ſometimes ſcarcely any air could be "expreſſed from the lungs." "In all inſtances, "the quantity of air that remained was very in-"conſiderable."X The uniformity of the reſults ſeems ſcarcely compatible with Dr. Skey's ſuppoſition. Mr. Coleman ſtates the following propoſitions among the general concluſions which he deduces from his experiments.

Coleman is correct, for the last act of life is a violent expiration =

" 26. That where refpiration is fufpended,
" from ordinary hanging, the animal has the
" power of expelling air from the lungs.

" 27. That although the mufcles of expira-
" tion perform their office, no power can be ap-
" plied to open the trachea to admit air.

" 28. That as no air can be received, the
" animal dies with the fame collapfe of the lungs
" from hanging as from drowning."

In fpeaking of the capacity of the thorax in its different ftates of diftention, Mr Davy remarks that, " Dr. Goodwyn, in his excellent work on
" the connexion of life with refpiration, has de-
" tailed fome experiments on the capacity of the
" lungs after *natural expiration*. He makes the
" medium capacity of the lungs about 109 cubic
" inches, which agrees very well with my eftima-
" tion."*(a)* But this ftatement of Dr. Goodwyn's opinion is not correct; in the part to which Mr. Davy refers, Dr. Goodwyn fays, " we fhall
" for the prefent adopt the medium quantity of
" thefe latter experiments, and fay that the lungs
" of the human fubject contain 109 cubic inches
" of air after *complete expiration*."*(b)*

Note

(a) Researches, p. 411. *(b) Goodwyn, p.* 27.

Note XIII.

I have fixed the quantity of air which can be expelled from the lungs, after an ordinary expiration, at 170 cubic inches, becaufe this is nearly the average of the eftimates which have been given by different authors, and alfo becaufe I find, that it coincides with fome trials which I have myfelf made upon the lungs of different individuals. The opinion of Mr. Kite on this fubject, feem to be evidently incorrect. "I "find," he fays, "by repeated experiments, "that a perfon in health, and in a ftate of per-"fect reft, ufually refpires about 17 cubic inches "of air; but, at the end of the expiration, there "ftill continues in the lungs 87 cubic inches," &c. *(a)* From the above quotation it appears, that he either entirely overlooked the quantity which we are now inveftigating, or confounded it with the air remaining after a complete expiration.

Note XIV.

Mr. Davy indeed remarks, that "as moft "gafes, though of different fpecific gravities "when brought into contact with each other, affume

(a) Essays, p. 47.

" aſſume ſome ſort of union, it is more than " probable, that gas inſpired into the lungs, from " being placed in contact with the reſidual gas " on ſo extenſive a ſurface, muſt inſtantly min- " gle with it." *(a)* But in oppoſition to this argument, we have the direct experiments of M. Jurine, who found the carbonic acid to exiſt in very different proportions in the different parts of the bronchial veſſels. *(b)* Yet the gaſes naturally contained in the lungs, poſſeſs many circum- ſtances favourable to their equable diffuſion, which would not operate in the caſe of the hydrogene reſpired by Mr. Davy. The perpetual motion which the lungs experience, the gradual pro- duction of the carbonic acid, the length of time during which it remains in contact with the other gaſes, would all tend to promote its uni- form mixture with them. It may likewiſe be remarked, that carbonic acid, though conſidera- bly heavier than the conſtituents of the atmo- ſphere, differs leſs from them in ſpecific gravity than the hydrogenous gas. From the experi- ments of Fontana, we learn alſo, that hydroge- nous gas, even when mixed in a glaſs jar with oxygene or common air, does not diffuſe itſelf equally through the veſſel, but remains in a greater

(a) Researches, p. 506—7. *(b) Enc. Meth. Medecine t.* i. *p.* 494.

greater proportion in the upper part of it. *(a)* Dr. Higgins informs us, that when an animal is confined for some time in a jar of air, the carbonic acid is found to exist in greater quantity in the lower than in the upper part of the vessel. *(b)* In the two experiments on the respiration of hydrogene, related by Mr. Davy, there is a difference in the results of about $\frac{1}{16}$ part; *(c)* when the larger quantity of hydrogene was employed, the capacity of the lungs appeared greater, because a larger quantity of the gas being forced into the vesicles, a greater proportion of the residual air was necessarily expelled.

With respect to Mr. Davy's estimate of a single inspiration, although in the experiments made with the mercurial apparatus, he fixes it at 13 inches, in another part of the essay, he informs us, that his lungs in a state of natural inspiration, contain 135 inches, and of natural expiration 118 inches; but the difference between these quantities, which will measure the bulk of a single inspiration, is 17 cubic inches. *(d)*

Note XV.

Borelli remarks, that both from the shape and texture

(a) Phil. Trans. 1779. *p.* 358. *(b) Minutes of a Society, &c. p.* 136. *(c) Researches, p.* 402--6. *(d) Researches, p.* 410.

texture of the bronchia, and the manner in which the diaphragm acts, it is evident that the lungs cannot expel nearly all the air which they contained.*(a)* Haller conceives it impoffible that the lungs can be contracted even to one half of their natural dimenfions, unlefs when removed from the thorax, and deprived of air by the operation of boiling.*(b)*

Note XVI.

The tendency which water has to affume the aeriform ftate, when it is freed from external preffure, is well illuftrated by M. Gay-Luffac *(c)* and by Mr. Dalton,*(d)* in their refpective effays upon the dilatation of the gaffes. From the experiments which were performed by many of the older phyfiologifts upon different animal fubftances placed in the vacuum of the air-pump, we learn, that the blood and the mucous fecretions poffefs the power of extricating gas in a much greater degree than water.

Note XVII.

This method of explaining the refults of Mr.

 Coleman's

(a) De Motu Anim. p. 2. *prop.* 94. *(b) Phys.* viii. 4. 11.
(c) Ann. de Chim t. xliii. *(d) Manchester Mem. v.* v.

Coleman's experiments was, I believe, firft pointed out by Mr. Allen in his lectures.

Note XVIII.

In eftimating the bulk of a fingle infpiration at 40 cubic inches, I have been principally influenced by the experiments of Dr. Menzies, as they appear to be the moft accurate in their execution, the leaft exceptionable with refpect to the nature of the proceffes employed, and the moft numerous and varied of any that have been performed upon this fubject. In favor of this opinion we have alfo the authority of Jurine, Sauvages, *(a)* Hales, *(b)* Haller, *(c)* Blumenbach, *(d)* Chaptal, *(e)* and Bell; *(f)* Richerand, *(g)* alfo eftimates the bulk of a fingle infpiration at between 30 and 40 cubic inches, and Fontana *(h)* at 35.

Note XIX.

There is fcarcely any opinion in phyfiology, however abfurd it may at firft view appear, which has not found fome fupporters; and accordingly it

(a) Haller, viii. 4. 6. *(b) Statical Essays, v.* i. *p.* 238. *(c) El. Phys.* viii. 4. 6. *(d) Instit. Phys. p.* 113. *(e) Chemistry, v.* i. *p.* 133. *(f) Anatomy. v.* ii. *p.* 193. *(g) Phys. p.* 147. *(h) Phil. Trans.* 1779. *p.* 349.

it appears that arguments have been urged to prove that the fœtus breathes whilſt in the uterus. *(a)*

Note XX.

This is the hypotheſis which is advanced by Haller in the *El. Phys.* viii. 5, 2; in his notes upon Boerhaave, which were publiſhed at an earlier period, he ſuppoſes that the firſt reſpiration is produced by the cry of the animal, in conſequence of the unpleaſant feelings which it experiences immediately after birth, from cold, preſſure, &c. *(b)* This, however, cannot be conſidered as any explanation of the phenomenon, as it is evident, that there muſt have been a previous inſpiration before the cry could have been produced.

Note XXI.

I conſider an inſtinctive action to be one which is attended with ſenſation, and directed to ſome uſeful purpoſe, but which has been derived neither from experience, obſervation, nor tradition.

Note XXII.

Though it is undeſirable to occupy the attention

(a) Haller, xxix. 4. 54; *Whytt on Vital Motions, sect.* 9. *Boyle's Works, v.* i. *p.* 110. *(b) Prælect. t.* v. *p.* ii. *p.* 456.

with exploded theories, yet as a part of the hiſtory of ſcience, it is in some degree neceſſary, and in many caſes highly intereſting, to take a curſory view of the doctrines that have been ſupported by men of celebrity, and which for a time have enjoyed a ſhare of popularity. For this reaſon I ſhall ſubjoin a ſhort ſketch of ſeveral different hypotheſes, that have been formed to account for the firſt inſpiration, and for the ſubſequent alternation of inſpiration and expiration, beſides thoſe, which, in conſideration of their own ſuperior merit, or the more diſtinguiſhed celebrity of their reſpective authors, I have thought proper to admit into the body of the work.

1. With reſpect to the cauſe of the commencement of reſpiration;

Borelli imagined that the blood in its paſſage through the lungs, received from the air certain particles, which by communicating to it a vibratory or oſcillatory motion, adapted it for the purpoſes of life, and rendered it proper for ſupporting the circulation. The blood of the fœtus, he ſuppoſed, acquires theſe particles from the blood of the mother, but that as ſoon as the communication is deſtroyed between them, it becomes neceſſary

ceſſary for the ſame action to be performed by the lungs of the young animal itſelf. *(a)*

Pitcairne conceived it to be a general law of the animal œconomy, that a muſcle which is without an antagoniſt, muſt perpetually undergo an alternation of relaxation and contraction, and as the force of the muſcles of inſpiration ſo far exceeds that of the muſcles of expiration, he conſiders the diaphragm and intercoſtals as without antagoniſts, and applying his general hypotheſis to the peculiar caſe of the action of the thorax, he ſuppoſes that the alternate action of the diaphragm muſt commence immediately after birth. *(b)*

Petit conjectures, that the animal ſpirits are, from the firſt exiſtence of the fœtus, flowing downwards into the muſcles of reſpiration, and tending to produce their contraction; but that in conſequence of the preſſure which the animal experiences while in the uterus, theſe muſcles are not at liberty to act. As ſoon however, as the poſition of the new-born animal permits this effect to be produced, the thorax becomes enlarged from the contraction of the diaphragm and inter-

coſtals,

(a) *De Motu Anim. pars* ii. *prop.* 118. *(b)* *Dissert. p.* 62.

coſtals, and the air conſequently ruſhes into the veſicles.*(a)*

Boerhaave imagines it a ſufficient ſolution of this difficulty to ſtate, that the fœtus in the time of birth, in conſequence of its ſtruggles, puts all its muſcles into action, and among others the diaphragm. From this cauſe the thorax becomes expanded, and the air ruſhes into it; the blood then flows through the veſſels of the lungs, and reſpiration ever after proceeds as in the adult animal.*(b)*

The method employed by Dr. Hartley to account for the commencement of reſpiration, bears a conſiderable reſemblance to the hypotheſis of Boerhaave. He ſuppoſes that reſpiration and crying are excited in the new-born child from the cold, handling of the midwife, and other vivid ſenſations, impreſſed immediately upon its coming into the world. Theſe ſenſations tend to throw the whole ſyſtem of muſcles into action, and in thoſe caſes, where there are antagoniſts, the ſtronger will overcome the weaker, and contraction will be conſequently produced in them.*(c)*

According

(a) Mem. Acad. 1733. *(b) Instit.* §. 691. *(c) Obſervations on Man, v.* i. *p.* 95.

According to M. Buffon, when the animal leaves the uterus, the air acts upon all the organs of ſenſation, but particularly upon the olfactory nerves. They are by this means ſtimulated, and the animal makes an attempt to ſneeze; the cavity of the cheſt is thus enlarged, and the air has liberty to flow into the lungs, and the veſicles become dilated. But the air which is received into them has its temperature increaſed, and being expanded, cauſes the re-action of the fibres of the veſicles, which forces it again out of the cheſt. *(a)*

Prof. Blumenbach ſuppoſes, that upon the ſtoppage of the circulation between the mother and the fœtus, a ſenſe of ſuffocation is experienced by the latter. This circumſtance, together with the external cold, and the other new ſtimuli to which the body is expoſed, cauſes a variety of motions to take place, and among others the dilatation of the cheſt, and thus produces the firſt inſpiration. *(b)*

2. With reſpect to the immediate cauſe of the ſucceſſive alternations of inſpiration and expiration;

Some

(a) Nat. Hist. t. ii. *p.* 446. *(b) Instit. Phys. p.* 117

Some of the attempts that have been made to refolve this problem, as thofe of Willis and Pitcairne, have proceeded upon the affumption of principles, altogether inconfiftent with the actual laws of the animal œconomy. The former of thefe authors conceived, that during mufcular contraction, the animal fpirits are conveyed from the tendon to the fibres of the mufcle itfelf, and in the fubfequent relaxation return into the tendon. In the animal functions this operation is fuppofed to be under the power of the will, but in the mufcles fubfervient to the natural functions, the changes are effected independently of the will, in regular alternation. He attributes the difference in the operation of thefe parts, to the origin whence they derive their nerves, which in the former cafe proceed from the cerebrum, and in the latter from the cerebellum. *(a)*

Pitcairne imagined that all mufcles which are without antagonifts, are neceffarily in a perpetual ftate of alternate contraction and relaxation, an effect which he afcribes to the interrupted influx of the animal fpirits into thefe mufcles, in confequence of the alternate compreffion which the brain experiences from the dilatation of the arteries. *(a)*

The

(a) Pharm. Rat. p. 18. *(b) Dissert. p.* 62.

The learned mathematicians Borelli and Bellini, ſimply ſtate the neceſſity of this regular action to the exiſtence of the animal, and appear to reſt ſatisfied with pointing out its final cauſe. *(a)*

Other phyſiologiſts have endeavoured to account for the phenomenon, by ſome circumſtance in the ſtructure, or mechanical action of the thorax, or of the parts contained in it. Dr. Martine explains the action of the organs of reſpiration, by ſuppoſing, that the lungs, when in a ſtate of diſtention, compreſs the phrenic nerve, and thus produce a relaxation of the diaphragm and intercoſtals, to which theſe nerves communicate ſenſibility. *(b)*

A peculiar theory was adopted by Boerhaave to explain this phenomenon ; he imagined that in the ſtate of full inſpiration, the lungs were ſo much diſtended as not to permit the blood to be tranſmitted through them with its accuſtomed facility. Leſs blood will therefore be received by the left ventricle, and conſequently leſs ſent to the cerebellum, and to the nerves which are diſtributed to the organs of reſpiration ; hence ariſes a deficiency

(a) Borelli, p. ii *prop.* 117. *Bellini, Lem.* 18. *(b) Edin. Med. Essays, v.* i.

a deficiency of the action of thofe parts, and a confequent relaxation of the mufcles which ferve to increafe the fize of the thorax. The diftended ftate of the thorax being removed, the blood has liberty to flow freely into the left ventricle, and is confequently fent in its proper proportion to the cerebellum, it therefore excites the mufcles of infpiration to contract, and brings back the lungs to their former condition. *(a)*

Dr. Hartley applied his doctrine of vibrations to the folution of this problem. He conceives that his peculiar theory explains in an eafy manner, the propenfity to alternate contraction and relaxation, which is obferved in many of the mufcles, and upon this general principle, he endeavours to explain the action of the diaphragm; he however attributes the effect in part to affociation or habit, and in part alfo to impreffions made upon the pleura and peritonæum, exciting vibrations in them which are communicated to the diaphragm and mufcles of the abdomen. *(b)*

I have ftated the opinion of Profeffor Blumenbach, refpecting the caufe of the firft infpiration;

(a) Instit. §. 619, 620. *(b) Observ. on Man, v.* i. *p.* 96.

ration; in conſequence of this action, the lungs become dilated, and the blood flows through them. The air and the blood then act upon each other, and the former of theſe ſubſtances undergoes that peculiar change, which renders it unfit for the farther ſupport of life. It is then diſcharged from the lungs, in conſequence of the efforts of the vis medicatrix naturæ, "naturæ "medicatricis conaminibus," and makes room for the influx of a new portion of air. *(a)*

Dr. Darwin has not formally propoſed any hypotheſis, to account for the alternations of reſpiration, but it may be inferred from ſome expreſſions in the Zoonomia, that he conſidered the actions of inſpiration and expiration to ſucceed each other in conſequence of aſſociation. To this explanation of the phenomenon, it may be objected, that according to the definition of the author himſelf, a complicated ſeries of actions can only become aſſociated together "*by habit*; "*i. e. by frequent repetition.*" *(a)* But the young animal, from the firſt inſtant of its birth, poſſeſſes as complete a power over the muſcles of reſpiration, as it ever acquires during the courſe of its exiſtence. Thoſe caſes, in which for a few

(a) Instit. phys. p. 117. *(b) Zoonomia, v.* i. *p.* 13.

few moments, a difficulty has occurred in the commencement of refpiration, are to be referred to a mechanical obftruction in the trachea, preventing the accefs of the air, or to fome other accidental circumftance, for they are by no means of frequent occurrence, and they are in general immediately relieved by a change of pofture, or by the removal of mucus from the fauces. The actions which are acknowledged to be compofed of a number of affociated motions, fuch as dancing, or playing upon a mufical inftrument, though many of them lefs complicated than the act of refpiration, can only be acquired by long practice, and by repeated efforts of attention.

Note XXIII.

The celebrated Harvey, feems to have been the firft who ftated, that the action of the heart and lungs are totally independent of each other. He however imagines, that the paffage of the blood along the pulmonary veffels, is affifted by the motion of the thorax. *(a)*

The connexion which was fuppofed by fome of the older writers, to exift between the motions of

(a) De Motu Cord. p. 11. 71. 77.

of the heart and the diaphragm, depended upon the ſtructure or mechanical action of theſe organs. The learned author of the Zoonomia, who has adopted the idea of this connexion, has deduced it from that general law of the animal œconomy, by which motions that are frequently repeated in ſucceſſion, acquire the power of recurring in the ſame order, independently of the original exciting cauſe. Dr. Darwin obſerves, that "innu-"merable trains or tribes of other motions are "aſſociated with theſe muſcular motions which "are excited by irritation; as by the ſtimulus of "the blood in the right chamber of the heart, "the lungs are induced to expand themſelves."*(a)* But it may be remarked, in oppoſition to this ſuppoſed aſſociation, between the motions of the heart and the lungs, that in the fœtus the heart commences its contractions immediately upon its formation, while the lungs remain perfectly at reſt. When the animal leaves the uterus, the motion of the lungs commences, but the periods of the contraction of the diaphragm bear no determinate ratio to thoſe of the ſyſtole of the heart. The frequency of the pulſe is often increaſed or diminiſhed far beyond its accuſtomed ſtandard, while little alteration is produced in the motion of

(a) Zoonomia, vol. i. *p.* 40.

of the diaphragm. The action of both these organs, however, depends upon the quantity and the quality of the blood transmitted to them, and any affection of this fluid, will of course influence both the circulation and the respiration. These functions also both depend ultimately upon the contraction of the muscular fibre, and are therefore equally liable to experience an alteration, from any circumstance which affects the state of the irritability or the sensibility of the system. But it is obvious that these changes, though contemporary and proportional, do not depend upon association; they are produced by an assignable cause, operating directly upon each separate organ, distinct from the effects of repetition or habit. Were it not foreign to my present subject, it would not, I think, be difficult to shew, that a similar objection may be urged against many parts of the physiology and pathology of the Zoonomia, and particularly against the sympathetic theory of fever.

Mr. Hunter, supposes the existence of this sympathy, or association, between the motions of the heart and the lungs,*(a)* and the same opinion appears to be adopted by Dr. Currie. *(b)*

Note

(a) Hunter on the Blood, p. 54. *(b) Med. Reports p.* 75.

Note XXIV.

Helvetius, Senac, Winſlow, &c. Boerhaave, however, conceived that the lungs were rendered leſs capable of tranſmitting the blood during the ſtate of full inſpiration, and from the ſuppoſed effects of this retardation, he deduced his hypotheſis to account for the alternations of reſpiration.

Note XXV.

The celebrated experiment exhibited by Hooke, before the Royal Society, in which, after the motion of the heart had ceaſed, it was reproduced by inflating the lungs, has been adduced to prove, that by inſpiration the lungs are rendered pervious to the blood, *(a)* but it muſt be conſidered that in this caſe, the thorax was laid entirely open, and conſequently, when the lungs were not forcibly diſtended, they would collapſe ſo much as almoſt entirely to obliterate the cavities of the bronchia and veſicles.

Note XXVI.

Morgagni, relates a caſe, in which, upon examining

(a) Haller, viii. 4. 12.

amining after death, a conſiderable quantity of ſerous fluid was found in the cavity of the thorax, yet the pulſe had not been previouſly affected, nor had any impediment to the circulation been obſerved during the life of the patient.(a)

Note XXVII.

The experiments and obſervations of Mr. Coleman, upon the impermeable ſtate of the lungs in drowned animals, will more properly fall under our conſideration in a ſubſequent part of the eſſay.

Note XXVIII.

It has been obſerved, that when from any accidental cauſe, a portion of the cranium is removed, an alternate elevation and depreſſion of the brain is viſible to the eye, correſponding with expiration and inſpiration. This effect has been attributed to the reſiſtance which the blood experiences to its paſſage through the lungs in a ſtate of expiration ; a ſtagnation is thus brought about in the right ſide of the heart, and conſequently a leſs free diſcharge from the veins, on which

(a) *Seats and causes of Diseases*, v. i. *p.* 408, 9.

which account, in a highly vaſcular part, an increaſe of bulk will be produced; this is removed by the depletion of the veins, when the lungs from the enlargement of the thorax become again more completely pervious to the tranſmiſſion of the blood. There ſeems no reaſon to doubt that this phenomenon has been occaſionally obſerved, and it is probable that it depends upon the cauſe aſſigned above. But it muſt be remarked, that in thoſe inſtances, in which ſo great an injury is received by the cranium, the reſpiration is neceſſarily rendered laborious, the air is received into the lungs after long intervals, and conſequently in an increaſed quantity. (a) +

Note XXIX.

It has been aſked, why are the lacteals furniſhed with valves, while the veins which paſs along the ſame viſcera, and are conveying their contents in the ſame direction, are not provided with them. One cauſe of this difference in ſtructure is obvious; the chyle, not being poſſeſſed of any vis a tergo when it enters the lacteals, is propelled along them ſolely by the contraction of their muſcular fibres, and the effect of this

 muſcular

(a) *Haller*, vi. 4. 9; viii. 4. 11. 27.

+. This is a hypercritical remark, as it supposes that the appearance in question only occurs in cases of recent injury; whereas it is observable ever after, if the loss of bone be considerable, & not repaired. do you know that it is synchronous with the [illegible] mufcular contraction is evidently increafed by the prefence of the valves. When, on the contrary, the blood enters the abdominal veins, it retains a degree of velocity which is fufficient to carry it along them, and though its progrefs might have been accelerated by the addition of valves, we muft conclude that its prefent velocity is the beft adapted to the wants of the fyftem.

Note XXX.

I confider the abfolute neglect into which the works of Mayow had fallen in this country, until they were again brought to light about 20 years ago, as one of the moft curious circumftances in the hiftory of modern fcience. He lived at a period when the cultivation of natural philofophy was beginning to be popular, and after it had been fanctioned by feveral illuftrious examples. The atmofphere had alfo been made a particular object of attention, and the difcovery of the air-pump had enabled Boyle and others to make very important advances in the knowledge of its properties. Mayow's ftile is fufficiently clear and perfpicuous; although there are many hypothefes advanced in his effays, fome of which are founded upon the falfe philofophy then prevalent, yet they contain lefs theory and more experiments, than the writings of almoſt any of his contemporaries. The neglect of Mayow's works will appear more ſingular, when we conſider, that they iſſued from the preſs of Oxford, of which Univerſity he was himſelf a member, and that at the time of their publication, they appear to have been pretty generally read, and to have made as much impreſſion as could, at that period, be reaſonably expected. They paſſed through three or more editions in the ſpace of a few years, the whole or a part was republiſhed on the continent, and they are frequently quoted by the phyſiologiſts and anatomiſts, who lived about the concluſion of the 17th century. By ſome ſtrange and unaccountable fatality, they afterwards ſunk into almoſt complete oblivion, and more eſpecially in his native country, they came at length to be abſolutely forgotten. He is only twice referred to by Dr. Hales,*(a)* and then in ſuch a manner, as to render it doubtful whether he had ſeen his works, and in the diſcourſe delivered by Sir John Pringle before the Royal Society, upon the aſſignment of Sir Godfrey Copley's medal to Dr. Prieſtley, which commences with a ſketch of the diſcoveries that had been made in the ſcience of aerology, previous

 to

(a) Statical Essays, v. i. *p.* 234 & 236.

to the period when this philosopher entered upon his experiments, the name of Mayow is not mentioned.*(a)*

On the continent, although little estimated, and seldom referred to, his works were occasionally quoted, and Haller in particular, certainly appears to have been acquainted with them. They were first again brought into notice in this country by Dr. Reinhold Forster, who prefixed a short account of Mayow's doctrines; to his translation of Scheele's essay on air and fire.

The vicissitudes of Mayow's reputation, afford a striking instance of the tendency which exists in the human mind, to fly from one extreme to the opposite excess. When his works were thus brought before the public, his cause was eagerly embraced by different writers, his merits were exalted to the highest rank, and it was asserted, that he had anticipated the most valuable discoveries of Scheele, Priestley and Lavoisier. *(b)* But a calm investigation of the subject, will, I conceive, lead us to a very different conclusion. We shall,

(a) Discourses, p. 1—20. *(b)* See particularly Dr. Beddoes's pamphlet, entitled, "Chemical experiments and "opinions, extracted from a work published in the last "century."

ſhall, indeed, find in this author many marks of real and original genius, and it is evident, that on ſome points, he extended his views beyond thoſe of any of his contemporaries. It muſt, however, be confeſſed, that the real addition which he made to the general ſtock of knowledge was not very great. He endeavoured to prove, that there was ſomething in the atmoſphere, which was an immediate agent in the proceſſes of combuſtion and reſpiration; but this had been long ago aſſerted by others, and when he comes afterwards to explain more in detail, his ideas reſpecting the intimate nature of this peculiar matter, he evidently ſhews that he had not any conception of the ſubſtance to which we now aſcribe theſe phenomena. He is even doubtful, whether it forms an eſſential part of the air, and upon the whole inclines to the negative ſide of the queſtion. In order to account for the alteration produced in the air by its removal, he adopts an hypotheſis to the laſt degree fanciful, according to which it conſiſts in ſome mechanical change in the configuration of its particles, which principally operates in diminiſhing its elaſticity. He imagines that this matter enters into the compoſition of the nitrous, and other acids, and that it gives them the peculiar properties which belong to this claſs of bodies, but he al-

alſo ſuppoſes, that the ſame ſubſtance is a component part of the alcalis, and is the immediate cauſe of their cauſticity, and thus deciſively proves that he had no accurate conception of the modern doctrines reſpecting the conſtitution of acids.

As to the ſubject of reſpiration in particular, when ſpeaking of the action which the air exerciſes upon the blood, he has recourſe to the exploded doctrine of fermentation ; he ſuppoſes that the particles of the air and of the blood, when brought into contact, are rubbed againſt each other, in conſequence of which, the former are deprived of that ſpirit to which they owe their elaſticity. I think that an attentive peruſal of Mayow's works will convince the diſpaſſionate reader, that there is no ſubſtance which has been diſcovered by the modern chemiſts, that can perform the various effects which are attributed by this author to his nitro-aereal ſpirit.

Perhaps the moſt important of Mayow's obſervations, are thoſe reſpecting the mechaniſm of reſpiration, and particularly the very decided manner in which he ſtates that the external and internal intercoſtals are equally ſubſervient to the enlargement of the thorax. The ingenious experiment, however, where the action of the lungs

is

is illuſtrated by comparing them to a bladder incloſed in a pair of bellows, though brought forwards by Mayow as original, *(a)* is mentioned by Boyle in the moſt explicit manner, as what was already familiar to every one, and the concluſions which he deduces from it, are preciſely ſimilar to thoſe formed by Mayow. *(b)* There is one department, where the genius of Mayow was peculiarly ſucceſsful, by which, it is probable, he might have materially promoted the advancement of ſcience, had a greater degree of attention been beſtowed upon his writings; I refer to the contrivance of his apparatus and the execution of his experiments. In theſe reſpects he may be conſidered as far excelling any of his contemporaries, and as at leaſt equal to any chemiſt before the time of Scheele and Prieſtley.

In forming an eſtimate of the merits of Mayow, it is eſſential to obſerve, that a theory of combuſtion, almoſt preciſely ſimilar to that adopted by this philoſopher, had been a few years before publiſhed by Dr. Hooke, of which it is ſcarcely poſſible that Mayow could have been ignorant; it is however, not noticed by him. It appears, indeed, that Hooke has the advantage,

(a) *Tractatus quinque, p.* 274, 5. (b) *Works, vol.* i. *p.* 101.

tage, not only with refpect to originality, but likewife in the accuracy and fimplicity of his hypothefis. According to Profeffor Robifon, Mayow's exhibition of Hooke's doctrine is, "obfcure, complicated, and wavering, mixed "with much mechanical nonfenfe, of wedges, "and darts, and motions, according to the "fafhion of the times. Hooke's conception of "the fubject, is clear, fimple and fteady."*(a)* Dr. Thomfon paffes a fimilar judgment upon Mayow; he bears ample teftimony to the originality and accuracy of his experiments, but he adds, that "his reafoning is for the moft part "abfurd, and the additions which he made to "the theory of Hooke are exceedingly extrava-"gant."*(b)* Dr. Yeats has treated largely upon the writings of Mayow, but has, in my opinion, very far over-rated their merit. He exprefsly attributes to him the difcovery of oxygene, and fuppofes, that he was accurately acquainted with all its properties.*(c)* M. Fourcroy has publifhed a candid and judicious account of Mayow in the *Enc. Method.* *(d)* which was afterwards inferted in the *Annales de Chimie.* *(e)* In examining the writings

(a) Black's Lectures by Robison, v. i. *p.* 535, 6. *(b) Thomson's Chemistry, v.* i. *p.* 347. *(c) Observations on the claims of the Moderns, &c. p.* 14, *& alibi.* *(d) Chimie, t.* iii. *p.* 390. *(e) t.* xxix.

ings of the older chemifts and phyfiologifts, it is neceffary to remember, that expreffions which appear to coincide remarkably with the modern difcoveries, would not fuggeft the idea of thefe difcoveries to one who was previoufly unacquainted with them. It has accordingly happened in feveral inftances, that the modern doctrines have been fuppofed to be detected in books, where their exiftence was not fufpected, until thefe doctrines had been deduced from the experiments and reafonings of the chemifts of the prefent period.

Note XXXI.

Dr. Black made this difcovery by breathing through lime water, or the folution of a cauftic alkali, when he found that the lime was precipitated, and the alkali rendered mild; effects which he attributed to fixed air emitted from the lungs; he confequently conceived, that the change produced by refpiration upon wholefome air, confifts principally in the converfion of part of it into fixed air. It appears that he made thefe difcoveries in the year 1757, and it may be inferred, that they were about the fame time publicly announced in his lectures. *(a)*

(a) Black's Lectures, by Robison, v. ii. *p.* 87.

Note XXXII.

Beſides the oxygenous and azotic gaſſes, a ſmall quantity of carbonic acid gas, ſeldom exceeding ·01, has been found to exiſt in the atmoſphere, and is ſo conſtantly preſent in all the different ſituations in which the experiment has been tried, that it probably ought to be conſidered as one of its conſtituent parts. *(a)* The aqueous vapour, on the other hand, is perpetually varying in its proportion, and ſeems evidently to be an extraneous body, depending for its formation upon accidental cauſes. The proportion of oxygene which was indicated in the experiments of Scheele and Prieſtley, and in the more accurate examination of the atmoſphere, which was afterwards made by Lavoiſier, *(b)* is greater than that ſtated in the text. But the proceſs of Eudiometry has undergone of late conſiderable improvements, and the very accurate experiments of Berthollet, *(c)* De Marti, *(d)* and Davy, *(e)* agree in indicating the proportion of oxygene

(a) Sauſſure, voyage dans les Alps, t. vii. *c.* 6. §. 2010. *Humbolt, Journal de Phys. t.* xxxxvii. *p.* 202. *Encyc. Meth. Chimie, Air, t.* i. *p.* 745. *(b) Elem. of Chem. chap.* 3. *p.* 86. *(c) Ann. de Chim. t.* xxxiv. *p.* 73 *& seq.* *(d) Journal de Phys. t.* lii. *p.* 173. *& seq.* *(e) Nicholſon's Journ. v.* v. *p.* 175.

gene to azote to be nearly as 22 to 78. Thefe quantities muft be confidered as referring to the volume of the two gaffes; their proportional weights are fomewhat different, and will be as 26 to 74 nearly. *(a)* M. Fourcroy, however, in his late elaborate work, ftill fuppofes, that the atmofphere contains ·27 of oxygene. *(b)*

Note XXXIII.

I cannot avoid remarking, that in this memoir M. Lavoifier fpeaks of his obfervation refpecting the carbonic acid gas, as of a fact difcovered by himfelf, nor does he mention the name of Dr. Black, who it appears had publicly taught for twenty years that this aeriform fluid is difengaged by refpiration. It is much to be lamented that a man, who had fo many real claims to the admiration of pofterity, fhould in any inftance have attempted to encreafe his reputation at the expenfe of his contemporaries.

XXXIV.

This propofition is fubject to exceptions; M. Vauquelin found that fome of the fpecies of worms poffeffed

(a) Thomson's Chem. v. iii. *p.* 269. *(b) Systeme des conn. chim. t.* i. *p.* 156.

poſſeſſed the power of ſeparating the oxygene from the azote in the moſt perfect manner. He performed his experiments upon the limax flavus and the helix pomatia.(a)

XXXV.

The two papers agree very nearly in the weight of oxygene ſuppoſed to be conſumed in twenty-four hours; the firſt makes it 19080 French grs. the latter 19090. But though the weights ſo nearly correſpond, the aſſigned volumes are conſiderably different; the firſt memoir ſtates it to be 24, and the latter ſomething more than 22 Paris cubic feet. If we ſuppoſe 100 cubic inches of oxygene to weigh 50·5 Paris grs.(b) 24 cubic feet, or 41472 cubic inches will weigh 20943·36 grs.; 22 cubic feet, or 38016 inches will weigh 19198·08 Paris grs. or 15750·33 Engliſh Troy grs.

The Paris weights and meaſures of the original are reduced to the Engliſh ſtandard by means of the formulæ given by Profeſſor Robiſon in Kerr's tranſlation of Lavoiſier's Elements, *App. No.* 5.

XXXVI.

It muſt be remarked, that at the time when Mr.

(a) *Ann. de Chimie, t.* xii. *p.* 278. *& seq.* (b) *Kirwan on Phlogiston, p.* 36.

Mr. Davy performed theſe experiments, he conceived the atmoſphere to contain ·27 parts of oxygene;*(a)* in the more accurate experiments which he afterwards performed, he found the proportion of oxygene to be little more than ·21.*(b)* This circumſtance will render it neceſſary to make ſome alterations in the eſtimate of the effects of reſpiration, as we may conclude that the ſame Eudiometrical proceſs was employed in the analyſis of air which had been reſpired, as of the air of the atmoſphere.

Mr. Davy's eſtimate.

100 cubic inches of atmoſpheric air before the experiment,

	Oxygene.	*Azote.*	*Carbonic Acid.*
Conſiſted of	27	73	
After the experiment of	22·5	71·7	4·5
Conſequently there was an abſorptfon of	4·5	1·3	
And a production of			4·5

Corrected eſtimate.

Before the experiment	21	78	1
After the experiment	17·5	76·6	4·5
Conſequently there was an abſorption of	3·5	1·4	
And a production of			3·5

Note

(a) Researches, p. 327. *(b) Nicholson's Journal, v.* V. *p.* 175.

Note XXXVII.

Boyle firſt attempted to aſcertain the amount of the diminution, which the air experiences in reſpiration; he eſtimated it at $\frac{1}{30}$ part of its bulk. Mayow concluded from his experiments, that the air was diminiſhed $\frac{1}{14}$ part. *(a)* Hales made many experiments to aſcertain this point, but their reſults differ ſo conſiderably, as to render it impoſſible to fix any general ſtandard; it varied from $\frac{1}{30}$ to $\frac{1}{13}$ part of the whole air. *(b)* All theſe quantities, however different from each other, agree in being greater than the eſtimates of the modern chemiſts. This circumſtance is to be attributed to the abſorption of part of the carbonic acid formed in the lungs, by the water with which, in all the different experiments, the air of expiration was placed in contact.

Note XXXVIII.

The celebrated Sanctorius, who devoted ſo large a portion of his life to the inveſtigation of the quantity of matter perſpired from the body, under different circumſtances, eſtimated the pulmonary exhalation, at about half a pound in 24 hours.

(a) Tract. quinque, p. 405. *(b) Stat. Essays, v.* i. *p.* 238; *v.* ii. *p.* 320.

hours. *(a)* Dr. Hales, by purſuing the plan which is ſtated in the text, ſuppoſes the quantity to be above 20 oz. Dr. Home remarks, that the perſpiration from the lungs, is "computed "by Sanctorius to amount to 6 oz. in 24 hours, "by Hales to 23 oz.; the latter computation "ſeems too high, part of that moiſture being "probably owing to what is contained in a dry "air." *(b)* It appears, that in this inſtance, Dr. Home has been inaccurate in ſtating the calculation of Hales, and what is more remarkable, Haller, in giving the opinion of Home, ſays, "ad uncias 23 æſtimat Cl. Home," *(c)* and refers to the paſſage quoted above.

Note XXIX.

Dr. Prieſtley had previouſly obſerved this circumſtance, and had eſtimated that $\frac{1}{3}$ part only of the oxygene conſumed in reſpiration was employed in the formation of the carbonic acid emitted from the lungs; the reſt of the oxygene he conceived was abſorbed by the blood. *(d)*

Note XL.

It muſt be remembered, that a method of eſtimating

(a) Med. Statica, p. 36. *(b) Medical facts, p.* 238. *(c) El. Phys.* viii. 5. 40. *(d) Experim. on Air, v.* iii. *p.* 378, 9.

eſtimating the quantity of water produced in the lungs, very ſimilar to that made uſe of by Lavoiſier, and founded nearly on the ſame train of reaſoning, had been previouſly employed by Dr. Crawford; he ſuppoſes that about $\frac{1}{6}$ part of the oxygene conſumed, enters into the formation of aqueous vapour. *(a)*

Note XLI.

This is not the only inſtance in which, after the concluſions of Dr. Prieſtley have been controverted by his contemporaries, a more accurate inveſtigation of the queſtion has ultimately decided in his favour. The complicated apparatus, and the impoſing air of minuteneſs, which characterize the operations of the French chemiſts, irreſiſtibly engage the aſſent of the reader, and ſcarcely permit him to examine the ſtability of the foundation upon which the ſtructure is erected. The ſimplicity of the proceſſes employed by Dr. Prieſtley, the apparent eaſe with which his experiments were performed, and the unaffected, converſational ſtile in which they are related, have, on the contrary, been miſtaken for the effects of haſte and inaccuracy. Something muſt alſo be aſcribed to the theoretical language which pervades

(a) Obſerv. on Animal heat, p. 154, 347, 8.

vades and obſcures the chemical writings of this philoſopher, in conſequence of his unfortunate attachment to the doctrine of phlogiſton.

Note XLII.

Mr. Davy found that 71 · 7 cubic inches out of 99 which he expired were azote; *(a)* if therefore we eſtimate the average contents of the lungs at 280 cubic inches, 213 of theſe will conſiſt of azote.

Note XLIII.

In the firſt of the general concluſions, I have ſtated, that the atmoſpheric air received into the lungs, loſes about · 04 of the oxygene which was contained in it. Suppoſing the whole quantity of air reſpired in 24 hours to be 666 · 5 cubic feet, the quantity of oxygene which is removed will amount to 26 · 66 cubic feet. This eſtimate agrees exactly with the quantity of oxygene which I have ſuppoſed to be conſumed in 24 hours, deduced from the experiments of M. Lavoiſier and Mr. Davy. The quantity · 04 is however calculated from a different ſet of expe-

 riments,

(a) Researches, p. 432.

riments, and the number 666 · 5 involves the eſti-mate of a ſingle inſpiration, and of the quantity of air reſpired in 24 hours, both which were ob-tained from different ſources.

In the 2d general concluſion it is ſtated, that 22 cubic feet of carbonic acid gas are gene-rated in 24 hours, ſo that there is, upon the whole, a loſs of 4 cubic feet in 24 hours, ſo far as reſpects the conſumption of oxygene, and the production of carbonic acid gas. In the 5th general concluſion it is ſtated, that a quantity of azote is conſumed in reſpiration, amounting to about $\frac{1}{160}$ part of the whole air employed. Sup-poſing this, as in the former caſe, to be 666 · 5 cubic feet, the azote abſorbed, will be about 4 cubic feet. Theſe, added to the 4 cubic feet mentioned in the laſt ſentence, will make the total loſs by reſpiration in 24 hours 8 cubic feet. In the 3d general concluſion, it is ſaid, that the volume of the air reſpired is diminiſhed by $\frac{1}{80}$ part; now 666 · 5 divided by 80 exactly produces the number 8 · 33. The coincidence in this caſe is more remarkable than in the for-mer, as it involves a greater number of quanti-ties, all obtained by ſeparate calculations, and deduced from independent ſources. It muſt, I conceive, be not only regarded as a ſtrong argu-ment

ment in favour of their accuracy, but, in some measure, at least, of the hypotheses from which they were deduced.

Note XLIV.

M. Lavoisier states, that 100 parts of carbonic acid consist of 72 parts oxygene, and 28 parts carbone,*(a)* but it has been discovered since the period of his experiments, that the charcoal which entered into the composition of the acid, is itself previously in the state of an oxide, containing ·36 of oxygene; the composition of carbonic acid will therefore be pure carbone 18, and oxygene 82 parts; or the oxide of carbone which composes charcoal 28, and oxygene 72 parts.*(b)* Upon the supposition that the carbone in the blood is in the state of the black oxide, there will in the space of 24 hours, be a a surplus of oxygene of 5·59 oz.; but if the carbone be in a state of purity, the superabundant oxygene will be only 1·88 oz.

Note XLV.

This idea of the nature of the change produced

(a) Mem. Acad. 1789. *p.* 567. *(b) Thomson's Chemistry, v.* i. *p.* 50.

duced upon the blood by refpiration, was founded upon a fuppofition, that its velocity was greater during its paffage through the capillaries of the lungs, than in any other part of the circulation; an opinion which was at one time adopted by feveral phyfiologifts of eminence. Dr. Hales, who performed experiments exprefsly upon this fubject, informs us that he obferved the blood to move five times as quickly through the lungs as through the mufcular capillaries.(a) Senac, on the other hand, thinks that its motion through the lungs cannot be quicker, becaufe each of the auricles muft receive the fame quantity of blood in the fame time; (b) and others have fuppofed that the velocity muft be lefs in the lungs, the pulmonary circulation being altogether fhorter than that of the aortic fyftem, the velocity muft be as the fpaces gone through in the fame time. Haller does not imagine that the average velocity in the lungs differs from that in the reft of the body; he however remarks that it is fent through them with lefs force; becaufe there are fewer obftacles to its progrefs, and the right ventricle is confequently lefs ftrong than the left.(c)

Note

(a) *Statical Essays*, v. ii. p. 66. (b) *Sur le Coeur*, t. ii. p. 161. (c) *Haller*, viii. 5. 21.

Note XLVI.

The following remarks of Boerhaave, who was diſtinguiſhed for his extenſive acquaintance with all the branches of ſcience, may ſerve as a proof of the entire ignorance reſpecting the change effected in the blood by reſpiration which prevailed in his time. He imagines that as long as the foramen ovale and ductus arterioſus are kept open, the animal can continue to live without air, becauſe the blood will then be able to paſs from the right to the left ſide of the heart. He even ſuppoſes, that by frequently plunging the young animal in water, the cloſing of theſe holes might be prevented, and the creature be thus rendered amphibious. Why we cannot dilate the cheſt under water, and continue to live in this ſituation, he ſays, "hoc nobis Κρυπτου eſt," but he adds with his accuſtomed candour, "quod forſan vobis, veſ-"troque ævo, aliquid pateſcet."*(a)* M. Buffon actually performed ſome experiments upon this ſubject.*(b)* Theſe opinions, which at preſent aſſume almoſt a ludicrous appearance, were pardonable at the period when they were advanced. But we can ſcarcely extend our apology to the phyſiologiſt, who after all the pneumatic diſcoveries which

(a) Prælect. t. v. pars 2. p. 467—474. *(b) Nat. Hist v. ii p. 447.*

which have immortalized the names of Black, Prieftley, Lavoifier, and Crawford, can ftill fancy that, "by frequent immerfion in water, the af-"fociation between the movements of the heart "and lungs might perhaps be diffolved; and an "animal be inured to live commodioufly, for any "time, under water." *(a)*

Note XLVII.

It is curious to obferve the manner in which Haller fpeaks of this experiment. "Hoc vulgare "experimentum non a Lowero folum, verum "etiam ab Helvetio ferio propofitum eft."

Note XLVIII.

Dr. Crawford, in conformity with the theory at that time prevalent, imagined that carbonic acid was produced by the union of oxygene and hydrogene. He was confirmed in this error by the effects which hydrogene had been found to produce upon the craffamentum in the experiments of Dr. Prieftley, where bright fcarlet blood, by expofure to this gas, was rendered dark coloured,

(a) Beddoes on fact. airs, part i. *p.* 41.

coloured, and acquired the venous character. Dr. Hamilton alſo found that the ſame change was produced in the colour of arterial blood by introducing hydrogenous gas into the carotid of a living animal. *(a)*

Note XLIX.

M. Haſſenfratz notices the objections which are mentioned in the text, yet he lays the moſt ſtreſs upon the manner in which the old theory accounts for the production of animal heat. He aſſerts, that according to it, the caloric is altogether evolved in the lungs, and that conſequently their temperature ought to be much ſuperior to that of the other parts of the ſyſtem. He ſays, that according to Dr. Crawford's hypotheſis, "les poumons ſont le foyer où ſe degage toute la "chaleur que le ſang abandonne dans l'economie "animale." And again, "M. de la Grange "reflechiſſant que si toute la chaleur qui ſe diſ- "tribue dans l'economie animale ſe degageoit "dans les poumons &c." *(b)* When we conſider that this memoir was written many years after the publication of Dr. Crawford's treatiſe on

(a) Crawford, p. 147, *&c seq.* *(b) Ann. de Chim. t.* ix. *p.* 266.

on animal heat, it is ſomewhat ſurprizing, that ſuch expreſſions ſhould be employed by ſo experienced a philoſopher as M. Haſſenfratz. The friends of ſcience, muſt lament that the progreſs of knowledge is too frequently interrupted by a ſpirit of national jealouſy.

Note L.

Mr. Davy conceives, that after a complete reſpiration of atmoſpheric air, the gas contained in his bronchial veſſels, when reduced to the temperature of 55°, would amount only to 32 cubic inches, conſiſting of azote 23, oxygene 4·9, and carbonic acid 4·1 cubic inches.*(a)* He reſpired 100 cubic inches of the gaſeous oxide of azote for about half a minute, by this proceſs he found, that the gas was diminiſhed to 62 cubic inches, which upon examination, he found were compoſed of the nitrous oxide 29, azote 25·7, oxygene 4·1, and carbonic acid 3·2 cubic inches. He concludes that 32 cubic inches of gas, of the ſame compoſition, will be ſtill left in the lungs; theſe will conſiſt of nitrous oxide 14·7, azote 13·3, oxygene 2·1, and carbonic acid 1·9 inches. Before the experiment, the gas in the lungs, and in the apparatus, added together,

(a) Researches, p. 409.

together, were 132 cubic inches, compoſed of the oxide 100, azote 23, oxygene 4 . 9, and carbonic acid 4 · 1 inches; after the experiment, the gaſes in the lungs, and the apparatus taken together, amounted only to 94 inches, compoſed of the nitrous oxide 43 · 7, azote 39, oxygene 6 · 1, and carbonic acid 5 · 2 inches. From theſe data Mr. Davy inferred that 56 · 3 cubic inches of the oxide has been abſorbed, and 16 inches of azote produced.(a) Upon this experiment it may be remarked, that the 32 cubic inches ſuppoſed to be in the lungs at the commencement, is probably much leſs than the actual quantity which they contained. It is not expreſsly ſtated by Mr. Davy, that he made a complete expiration before the experiment, but granting this to be the caſe, we may conjecture, that above 100 cubic inches would be ſtill left in the lungs; now ſuppoſing the whole of this quantity to be of the ſame compoſition with the part examined by Mr. Davy, it will be compoſed of about 72 parts azote, 15 oxygene, and 13 carbonic acid; if to theſe we add 100 cubic inches of nitrous oxide, we ſhall have the whole amount of gaſes before the experiment. After the experiment, ſuppoſing that a complete expiration was made, there will ſtill be 100 inches left in the lungs; ſuppoſing with Mr. Davy, that the reſidual

(a) *Reſearches, p.* 412 *&* *ſeq.*

refidual gas in the lungs, is of the fame compofition with that in the apparatus, thefe 100 inches will be compofed of about 46 inches nitrous oxide, 41·5 azote, 6·6 oxygene, and 5·9 carbonic acid. If we add thefe quantities to the gafes left in the apparatus, the whole amount of the refidue after the experiment, will be nitrous oxide 75 inches, azote 67·2, oxygene 10·7 and carbonic acid 9·1 inches. Thefe numbers compared with the refpective quantities at the commencement of the experiment, will give a deficiency in all the gafes to the following amount; nitrous oxide 25 inches, azote 4·8 inches, oxygene 4·3, and carbonic acid 3·9 cubic inches. In making this calculation, it is affumed, that the 100 inches which are fuppofed to be in the lungs before and after the experiment, are of the fame compofition with the gas in the apparatus, but of this we have no direct proof. It is impoffible therefore, to form any pofitive conclufions from an eftimate founded upon fuch uncertain data, but the refult is unfavourable to the fuppofition, that azote is evolved in thefe circumftances; perhaps if the experiment were repeated with the requifite precautions, it would be found, that the only change which is effected, is an abforption of part of the nitrous oxide.

Before

Before the experiment, the gafes were,

	Oxygene	*Azote*	*Carb. Acid*	*Nitr. ox.*	*Total*
In the lungs	15	72	13		100
In the appar.				100	100
Total	15	72	13	100	200

After the experiment,

In the lungs	6·6	41·5	5·9	46	100
In the appar.	4·1	25·7	3·2	29	62
Total	10·7	67.2	9·1	75	162
loft in the ex.	4·3	4·8	3·9	25	38

Note LI.

The facility with which the greateft part of the oxygene of the atmofphere is abforbed in the various Eudiometrical proceffes, and the difficulty of feparating the laft portions of it, fo as to obtain the azote in a perfect ftate of purity, affords a ftrong prefumption, that the component parts of the atmofphere are united together in a ftate of chemical compofition, and not mechanically mixed, or merely diffufed through each other's fubftance. It appears, that in this cafe, the atmofphere is fubjected to the fame laws of affinity, which the learned

learned M. Berthollet has ſhewn to exiſt in all chemical compounds. *(a)*

Note LII.

As the red particles can only be rendered viſible by the aid of the microſcope, the obſervations that have been made upon them, are deeply tinged with that uncertainty which always attends the uſe of high magnifying powers. In ſome inſtances we are able to point out, upon optical principles, the probable cauſe of the deception; but, in other caſes, it muſt be confeſſed, that there is reaſon to ſuſpect, that the obſerver ſaw what beſt coincided with his previous hypotheſis. A good account of the obſervations which have been made upon this ſubject, accompanied with ſome original experiments, may be found in the appendix to Mr. Cavallo's treatiſe on the factitious airs.

Note LIII.

In the Phil. Tranſ. for 1797, there is a paper on the colour of the blood, written by Dr. Wells, in which the author endeavours to overthrow the generally received opinions upon this ſubject. He conceives, that the alteration induced upon the

(a) *Ann. de Chim.* t xxxvi.

the colour of the blood, as well by the action of oxygene, as of some of the neutral salts, is altogether independent of any change effected by them upon its colouring matter. In order to prove this proposition, he made a solution of the red particles (i. e. the colouring matter) in water, and he found, that the colour of this solution was not affected by the same reagents, which produce a change of colour in the crassamentum of the blood. He also found that an aqueous solution of the red particles, whether taken from the crassamentum of the arterial, or the venous blood, exhibited precisely the same shade of colour. Hence he concludes, that the action of air, nitre, and other substances which redden the blood, is exercised not upon the red particles themselves, but upon the medium which surrounds them. This action consists in rendering the serum more dense, and thus causing it to reflect a greater number of rays of light from its internal surface; and he attempted to prove by experiment, that the blood is in fact rendered more dense, in those cases where its colour is brightened. The reasoning of Dr. Wells is certainly ingenious, but, I think, not conclusive. We know, that by solution in water, the peculiar texture and organization of the red particles is destroyed, so that though no alteration of colour be produced by the air upon these bodies,

dies, when in this ſtate, it does not follow that the change would not be produced, if the air were applied to them while they retained their globular form. Our knowledge reſpecting the arrangement of the minute particles of bodies, is perhaps too imperfect for us to determine with certainty, by what means the changes of colour are effected, which follow from the application of different chemical reagents; an alteration in the denſity of the body, may probably be one among other methods; but ſuppoſing this to take place in the caſe now under conſideration, it is more natural to conceive, that the action ſhould be exerciſed upon the particles themſelves, than upon the ſubſtance which is contiguous to them.

Dr. Wells, in the 2d part of his paper, offers ſome experiments to ſhew, that the red colour of the blood does not depend upon the iron contained in the red particles. They certainly deſerve conſideration, but if we admit them in their full extent, they would prove, that no part of the blood contains iron in the ſaline ſtate, a poſition which is directly oppoſed by the experiments that have been made more lately by Fourcroy and Vauquelin, who have not only confirmed the exiſtence of the iron, but have aſcertained, apparently with great exactneſs, the ſtate of combination in which it

exiſts.

exiſts. The hypotheſis with which Dr. Wells concludes his paper, viz. that the red globules are compoſed of two diſtinct ſubſtances, poſſeſſed of different chemical properties, even though it coincides with the microſcopical obſervations of Mr. Hewſon, I cannot but conſider as altogether fanciful. Before we attempt to form any more conjectures on this ſubject, it would be deſirable to repeat and vary the experiments of Fourcroy and Vauquelin.

The imagination of the poet has in ſome degree anticipated the diſcovery of the chemiſts.

When air's pure eſſence joins the vital flood,
And with *phosphoric acid* dyes the blood,
Your virgin trains the tranſient heat diſpart,
And lead the ſoft combuſtion to the heart, &c.*(a)*

Dr. Darwin obſerves in a note, " Dr. Crawford, in his ingenious work on animal heat, has " endeavoured to prove, that during the combination of the pure part of the atmoſphere with " the phlogiſtic part of the blood, much of the " matter of heat is given out from the air ; and " that this is the great and perpetual ſource of the " heat of animals ; *to which we may add, that* " *the*

(a) Economy of Vegetation canto i. *l.* 399—402.

"*the phosphoric acid is probably produced by this*
"*combination*; by which acid the colour of the
"blood is changed in the lungs from a deep
"crimſon to a bright ſcarlet."

Note LIV.

M. Abildgaard, of Copenhagen, made ſome experiments, of which a ſketch is given in the Annales de Chimie, *(a)* in order to determine the comparative quantity of carbone in the arterial and the venous blood. 100 Parts of the venous blood of a horſe, when dried by a moderate heat, afforded 26 parts of a dry pulverizable ſubſtance, whereas 100 parts of the arterial blood treated in the ſame manner, yielded only 25 parts. An ounce of venous blood, dried and decompoſed in a cloſe veſſel, produced 115½ grs. of carbonaceous matter; the ſame quantity of arterial blood produced no more than 87½ grs. Theſe experiments certainly appear favourable to the modern theory of reſpiration, but the account which is given of them is too conciſe to enable us to form any deciſive concluſions.

Note LV.

Dr. Bancroft firſt ſuggeſted the opinion, that charcoal

(a) T. xxxvi. *p*. 91.

charcoal was an oxide of carbone, *(a)* an opinion which has been ſince amply confirmed by the experiments of Guyton and Tennant. It is probable that this black oxide of carbone can only be formed by a degree of heat ſufficient to produce ignition, and conſequently much ſuperior to that of the blood. It may alſo be preſumed, from the whiteneſs of the chyle, that the carbone which it contains, is not combined with oxygene, or at leaſt, not in that ſtate of oxidation, which conſtitutes charcoal. Hence we may infer, that the inflammable matter of the blood exiſts in the ſtate of pure carbone, and conſequently, that the carbonic acid which is emitted from the lungs, is formed by the union of 18 parts of carbone, with 82 parts of oxygene. Dr. Bree ſuppoſes that the dark colour which the mucus of the bronchial glands ſometimes aſſumes, is owing to a portion of the carbone of the blood, which, in aſthmatic patients, is not carried off, in ſufficient quantity, by the formation of carbonic acid. *(b)*

The opinion of Mr. Abernethy, reſpecting the origin of the carbonic acid diſcharged from the lungs, is very different from that generally aſſigned by the modern phyſiologiſts. He obſerves,

(a) On the Phil. of Colours, p. 48. *note.* *(b) On Respiration, p.* 125.

that " at one time, phyſiologiſts believed that the " inſpired oxygenous gas, contributed to the pro- " duction of the carbonic gas, found in the air " expired. This opinion perhaps ſtill prevails in " the minds of ſome people." *(a)* I conceive, however, that the doctrine which is here reprobated, is at preſent almoſt univerſally adopted. Mr. Abernethy ſuppoſes that the carbonic acid emitted from the lungs, is not properly the product of reſpiration ; but is ſimply exhaled from the pulmonary veſſels. His chief objection to the contrary doctrine is, that the quantity of oxygene received into the blood by the lungs, is not ſufficient to generate the carbonic acid which is expelled. But on this point, Mr. Abernethy's opinion is in direct oppoſition to that of M. Lavoiſier, and the moſt eminent chemiſts, who conclude from their experiments, that the quantity of oxygene abſorbed, is conſiderably more than ſufficient to form the carbonic acid.

The opinion of M. Burdin, concerning the formation of the carbonic acid emitted from the lungs, is in ſome reſpects ſimilar to that embraced by Mr. Abernethy, and is in like manner very different from the one generally adopted. He imagines that all the ingredients of the acid exiſt in

(a) Essays, p. 146.

in the venous blood, previous to its return to the lungs, and that when it arrives at this organ, they are united, and ſeparated from it by ſecretion. He ſuppoſes that oxygene is at the ſame time abſorbed by the blood, but that it has no immediate ſhare in the formation of the carbonic acid.*(a)* In oppoſition to this hypotheſis it may be urged, that the abſorption of oxygene and the production of carbonic acid, take place in the ſame manner, when a piece of craſſamentum is placed in a jar filled with oxygene, as when the venous blood is acted upon by the air contained in the veſicles. From this circumſtance it follows, that the effect produced depends altogether upon a chemical affinity between the oxygene, and one or more of the conſtituents of the blood, and not upon the operation of a function which can only ſubſiſt during life.

Note LVI.

In forming this concluſion, I decide not only in oppoſition to the opinion of M. Lavoiſier, but of nearly all the phyſiologiſts who have treated upon this ſubject ſince the publication of his memoirs. The opinions of the French chemiſts, as far as I am acquainted with their works, almoſt

(a) Course of Med. Studies, v. iii. *p.* 343.

uniformly coincide with that of their illuſtrious countryman.*(a)*

Among the Germans I find the ſame doctrine ſupported by Blumenbach, Jacquin, and Gren,*(b)* and in our own country by Crawford, Cavallo, Higgins, Currie, Thomſon, Rutherford and Allen. *(c)*

It is to be obſerved, that the formation of water is equally owing to reſpiration, whether in conformity with the opinion of M. Lavoiſier we ſuppoſe it to be immediately generated in the lungs, or in paſſing along the ſanguiferous ſyſtem, according to Haſſenfratz's hypotheſis. In both caſes it is produced by the union of oxygene ab-
ſorbed

(a) Guyton-Morveau, Enc. Meth. Chimie, t. i. *p.* 727. 765. — *Seguin, Ann. de Chim. t.* xxi. *p.* 225. *& Med. Eclaireé.* — *Fourcroy, Systeme, &c. t.* x. *p.* 373. *t.* i. *p.* 56. *Hassenfratz, Ann. de Chim. t.* ix. *p.* 261. — *Vauquelin, Ann. de Chim. t.* xii. *p.* 290. — *De la Metherie, Journ. de Phys. t.* xlvi. *p.* 106. — *Cuvier, Leçons d'Anat. comp. t.* i. *p.* 92. *Richerand, Phys. p.* 149.

(b) Blumenbach, Inst.Phys. p. 115. — *Jacquin, El. of Chemistry,* § 956. — *Gren, Ann. de Chim.* xxiv. *p.* 196.

(c) Crawford on Animal Heat, p. 154. — *Cavallo's Elem. of Nat. Phil. v.* ii. *p.* 540, 1. — *Higgins's Minutes of a Society, p.* 163. — *Currie's Med. Reports, p.* 192. — *Thomson's Chemistry, v.* iv. *p.* 497. — *Rutherford in Marcet de Diabete, p.* 31. — *Allen in De la Rive de Cal. Anim. p.* 35.

ſorbed by the blood in the lungs, and hydrogene previouſly exiſting in that fluid.

Note LVII.

M. Lavoiſier, in the two memoirs on reſpiration, written in conjunction with his friend Seguin, to which I have already ſo frequently referred, employs no arguments to prove the diſcharge of hydrogene from the blood in addition to thoſe ſtated above: as however theſe papers contain an account of the moſt elaborate experiments that have ever been performed upon the ſubject of reſpiration, and are the laſt works which this great philoſopher lived to complete, it may be deſirable to examine them with ſome degree of minuteneſs.

After ſtating ſome general propoſitions reſpecting the nature of combuſtion, and the compoſition of the bodies which are produced by reſpiration, they proceed to obſerve, that this function may be conſidered as a ſlow combuſtion of carbone and hydrogene, and that its effect is to abſtract from the blood a quantity of theſe ſubſtances, and to depoſit there a portion of caloric. There is, however, one circumſtance, in which reſpiration is materially different from combuſtion;

combuſtion; this latter operation proceeds with more rapidity in proportion to the greater purity of the air employed, whereas the quantity of oxygene which an animal conſumes in reſpiration, is very nearly the ſame in whatever proportion it exiſts in the air received into the lungs. This propoſition was proved by a ſeries of experiments performed upon guinea-pigs. Theſe animals were confined for ſome days in air of different compoſition, from pure oxygene on the one hand, to air which only contained $\frac{1}{15}$ part of this ſubſtance. We are expreſſly told, that under theſe circumſtances, the reſpiration and circulation were not affected, and that the heat continued the ſame; in ſhort the only perceptible alteration in the ſtate of the animal was a degree of drowſineſs, when too large a proportion of azote had been employed. There appears no reaſon to doubt the accuracy of theſe experiments, the concluſions from them are probably different from what we ſhould have been led to expect; they however demonſtrate the abſolute inefficiency of the attempts which have been of late made with ſo much confidence to introduce at pleaſure into the ſyſtem, a greater or leſs quantity of oxygene, merely by varying the compoſition of the air preſented to the lungs. The authors ſtate, that during reſpiration there is neither any diſengagement

gagement nor abſorption of azote; they conſider this ſubſtance as entirely paſſive, and aſſert that any other gas mixed with the oxygene, which was not immediately noxious, would anſwer the ſame purpoſes with the air of the atmoſphere; they employed hydrogene, and found no inconvenience to ariſe from its uſe. It had been obſerved, that the quantity of oxygene conſumed in a given time, was different in different ſtates of the animal œconomy; the authors proceed to inveſtigate the amount of theſe changes, and for this purpoſe M. Seguin himſelf became the ſubject of the experiments. The circumſtances which they noticed as affecting the proceſs of reſpiration were, temperature, exerciſe, and the ſtate of digeſtion. As a ſtandard for the reſt of the experiments, they began by taking an average effect produced by a man at reſt, with the ſtomach empty, and at the temperature of 82°; under theſe circumſtances he conſumed in the ſpace of an hour 1210 cubic inches of oxygene. At the temperature of 57°, the other circumſtances remaining as before, he conſumed 1344 inches. During the proceſs of digeſtion, the conſumption of oxygene was increaſed to 18 or 1900 inches. While the ſtomach was empty, and the body in a ſtate of violent exerciſe, 3200 inches were conſumed in an hour, and when he

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ufed the fame exertion after taking food, the quantity was increafed to 4600 cubic inches in the fame fpace of time. Notwithftanding this great difference in the quantity of oxygene confumed, the temperature of the body was fcarcely affected, but the circulation, and the refpiration, were confiderably quickened. The authors obferve, that thefe numbers muft only be confidered as indicating proportions, becaufe it is probable, that individuals differ in the abfolute quantity of the changes which are effected by the refpiration. The apparatus by which thefe experiments were performed, is not fpecified. It muft be obferved that the effects produced upon the refpiration by a change of temperature, had feveral years before been diftinctly pointed out by Dr. Crawford; and M. Jurine had alfo afcertained that mufcular exertion, and the ftate of the ftomach, produced very important effects upon this function, yet neither of thefe authors are noticed in the memoir now under confideration.

Taking into account all thefe different circumftances, the authors conclude, that about 1 cubic foot, or 1728 inches, may be confidered as the average quantity of oxygene confumed in an hour, and confequently 24 cubic feet in the courfe of a day. They next proceed to point

out

out what proportions of this oxygene are to be refpectively affigned to the production of the carbonic acid, and the water. It is not expreffly ftated, in what manner the fum of the carbonic acid was obtained, but this we may fuppofe to be fufficiently accurate. It is then affumed, that this carbonic acid is compofed of carbone, which previoufly exifted in the blood, in the fame ftate of oxidation with charcoal, and the additional quantity of oxygene is eftimated, which is required to form it into complete carbonic acid. The remainder of the oxygene is fuppofed to be employed in the production of water, and by calculating how much water can be formed by this fuperabundant oxygene, the amount of the aqueous exhalation from the lungs is eftimated. It will not be neceffary to follow the authors through their details; I have endeavoured to fhew that their calculations are built upon the affumption of data, one of which is improbable, and the other at leaft incapable of proof. The memoir concludes with fome general reflections deduced from the experiments. With refpect to the increafed confumption of oxygene in a low temperature, the authors are inclined to attribute it to the greater denfity which cold air poffeffes, and to fuppofe, that the principal circumftance which moderates the heat of the body in a high temperature,

temperature, is the increafe of tranfpiration, which carries of part of the caloric that had been previoufly evolved, by the formation of the water and carbonic acid.

Before I leave this memoir, I muft remark, that in the eftimate which is made of the quantity of water formed in the lungs, the figures which denote the weight of the hydrogene difcharged, and of the water produced, do not correfpond. The whole quantity of oxygene fuppofed to be confumed is 19080 grs.; the carbonic acid produced is 21600 grs.; this will contain 15553 grs. of oxygene. There will then be left for the formation of water 3528 grs., which will require 622 grs., or 1 oz. 46 grs. of hydrogene, and the quantity of water produced will be 4150 grs., or 7 oz. 1 dr. 46 grs. In the memoir under confideration, the quantity of hydrogene is ftated to be 1 oz. 5 drs. 51 grs. and the water only 5 drs. 51 grs.

The 2d memoir written by M M. Lavoifier and Seguin, is expreffly on the tranfpiration of animals. Tranfpiration is defcribed as confifting in the lofs of a certain moifture, which requires a quantity of heat to reduce it to vapour, in order to carry it off from the body; it proceeds both

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from the ſkin and the lungs, and according as it is derived from the one or the other of theſe ſources, it is ſtiled the cutaneous, or the pulmonary tranſpiration. Upon the greater or leſs quantity of this tranſpiration, together with the degree of denſity in the inſpired air, the authors conceive, that the uniformity of temperature depends, which the animal preſerves in all the different degrees of external heat to which it is expoſed. In addition to the aqueous vapour derived from the cutaneous and pulmonary tranſpiration, there is alſo the water ſuppoſed to be generated in the lungs from the immediate union of oxygene and hydrogene; it remained therefore to contrive a method by which the effect of theſe operations might be kept diſtinct and preciſely aſcertained; for this purpoſe the following apparatus was employed. A perſon was encloſed in a ſilk garment covered with a varniſh of elaſtic gum, he had a tube adapted to his mouth, and by this method it is ſtated, that the effects of tranſpiration and of reſpiration were kept diſtinct. But in this ſtatement there appears to be an inaccuracy; the effects of cutaneous tranſpiration would certainly be kept diſtinct from the effects of pulmonary tranſpiration and reſpiration, but it does not appear how theſe two were to be ſeparated.

By

By weighing the body juſt after entering the apparatus and juſt before leaving it, they endeavoured to aſcertain the loſs of weight which the body experiences by reſpiration, ſeparate from that by tranſpiration. By weighing the body immediately after entering the apparatus, and immediately after leaving it, the total loſs which it experiences by all the different ſources is aſcertained. The authors conjecture, that there exudes into the bronchia a humour which is ſeparated from the blood, compoſed chiefly of hydrogene and carbone, and which filtres through the membranes of the lungs. This humour, by being very much attenuated as it paſſes through the ſmall exhalants, is in part conſumed, and by decompoſing the oxygene with which it is in contact during inſpiration, water and carbonic acid gas are produced. The authors obſerve, that the carbonic acid is removed from the lungs by expiration, but that the water would be accumulated, were there not a method provided by nature to prevent this circumſtance. The air enters the lungs cold, but iſſues from them warmed nearly to the temperature of the blood, it will, therefore, diſſolve a part of the water, and remove it from the lungs. From this hypotheſis, which ſuppoſes that the combuſtible matter, diſengaged during reſpiration, is, in the firſt inſtance, tranſ-

mitted

mitted through the pores of the lungs, in the form of an aqueous exhalation, it might be ſupposed quite ſuperfluous to aſſign any other ſource for the water of expiration; but it is immediately added, that the water exhaled is derived from two ſources, 1ſt. the water of pulmonary tranſpiration which we have been deſcribing, and 2d. that produced by the immediate union of oxygene and hydrogene, or what is ſtiled the water of reſpiration. We are informed, that in a future memoir the method will be detailed which was employed to ſeparate the water derived from theſe two ſources, and to aſcertain their reſpective amounts; but this memoir appears never to have been publiſhed, in conſequence, it may be preſumed, of the untimely fate of M. Lavoiſier.

The authors proceed to inform us, that by aſcertaining what quantity of water was ſent out of the lungs, and what quantity of carbonic acid was produced, and afterwards, by comparing theſe quantities with the quantity of oxygene conſumed, an eſtimate was formed of the water generated in the lungs, and of that which was evaporated from them. From the expreſſion employed there is reaſon to infer that the water exhaled was by ſome method, which is not mentioned in the memoir, actually collected and meaſured,

ſured, and not as in the former inſtance ſimply calculated by a compariſon with the oxygene conſumed. But, ſuppoſing this to be accurately performed, before the concluſions could be obtained which are formed by Lavoiſier and Seguin, it would be neceſſary to prove that all the oxygene was employed in the formation of carbonic acid and water, and that the carbone had previouſly exiſted in the ſtate of charcoal. They ſeem to have been aware of the difficulty reſpecting the carbonic acid, and add, that if the carbonic acid be produced by digeſtion, the oxygene muſt be employed in the formation of the water of reſpiration, which will diminiſh the quantity ſuppoſed to be derived from the pulmonary tranſpiration; but it is ſtill taken for granted, that all the oxygene is employed in uniting with the combuſtible matter diſcharged from the blood. The mean reſults of the experiments are then detailed, and are as follows:—the loſs of weight experienced by the body in twenty-four hours is 2lb. 13oz.; this includes the loſs by reſpiration, and by both the cutaneous and the pulmonary tranſpiration; of this quantity the cutaneous tranſpiration takes away 1lb. 14oz., and the reſpiration 15oz.; it appears by a ſubſequent part of the calculation, that this eſtimate of the loſs by reſpiration includes alſo the loſs by the pulmonary tranſpiration;

tranſpiration; it appears, indeed, that these two effects were neceſſarily confounded from the nature of the apparatus. The mean conſumption of oxygene in twenty-four hours is 2lb. 1 oz. 1dr. 10gr.; this oxygene is employed partly in the formation of carbonic acid, and partly in the formation of water. The weight of carbonic acid diſengaged in twenty-four hours is 1lb. 1 oz. 7dr. 4gr.

conſiſting of oxygene	. .	12oz. 0dr. 4gr.
carbone	. .	5oz. 7dr. 0gr.
		1lb. 1 oz. 7dr. 4gr.

There is then left a quantity of oxygene equal to 1lb. 4oz. 2dr. 10gr. which, in order to be formed into water, would require 3 oz. 3dr. 10grs. of hydrogene, which will produce a quantity of water equal to 1lb. 7oz. 5drs. 20grs.

The 2lbs. 13oz. which are ſuppoſed to be loſt in twenty-four hours, will be found then to conſiſt of the following quantities:

	oz.	*dr.*	*gr.*	*lb.*	*oz.*	*dr.*	*gr.*
Cutaneous tranſpiration				1	14	0	0
Carbone in the carb. acid	5	7	0				
Hydrogene in the water	3	3	10				
Pulmonary tranſpiration	5	5	62				
Loſs by reſpiration				0	15	0	0
				2	13	0	0

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In this calculation it may be obſerved, that the two firſt quantities only are the direct reſult of experiment. The hydrogene was eſtimated from the oxygene left after the formation of the carbonic acid gas, and the pulmonary tranſpiration was a quantity aſſumed to ſupply the difference between the weight of the hydrogene and the carbone, and the total amount of the loſs by reſpiration. It has been already noticed, that the different experiments of Lavoiſier himſelf, indicate very different quantities of oxygene conſumed, and of carbonic acid produced in the proceſs of reſpiration.

Upon the whole, we may conclude, that theſe memoirs ſtill leave us much in doubt reſpecting the nature of the changes produced upon the blood by reſpiration, ſtill leſs can they be conſidered as aſcertaining with exactneſs the amount of theſe changes. At the ſame time we muſt acknowledge, that they exhibit an ingenuity in the contrivance, and a perſeverance in the execution of experiments which ſo juſtly entitle their illuſtrious author to the firſt rank among chemical philoſophers.

As my object in the analyſis of theſe papers was, rather to preſent a view of the method of reaſoning employed by Lavoiſier, than to enter into

into an examination of his refults, I have retained the figures ufed in the original, which indicate the weights and meafures of France before the revolution.

Note LVIII.

I have referved the difcuffion of the fubject of animal heat to a fubfequent part of the effay; I fhall, however, in this place remark, that the doctrine which I have attempted to eftablifh concerning the non-emiffion of hydrogene by the lungs, will materially affect our opinions refpecting the influence of refpiration upon the temperature of the living body. According to the generally received theory, the whole, or a part of the water which is difcharged from the lungs, having been formed in the fyftem by the union of the gaffes which form its component parts, produces the extrication of caloric in the fame manner as during the generation of the carbonic acid gas. But if, on the other hand, we fuppofe that the water is generally received into the fyftem by the ftomach, in the fluid ftate, and is afterwards emitted from the body in the form of vapour, its difcharge will be neceffarily attended with a confiderable expenditure of caloric. This removal of caloric from the body will take place in exact proportion to the quantity of water evaporated, either from the ſkin or the lungs, and conſequently we muſt conſider the diſcharge of aqueous vapour, either by the pulmonary or the cutaneous tranſpiration, as in all inſtances counteracting or moderating the effects produced by the diſcharge of carbone from the blood. According to this view of the function of reſpiration, the proceſs by which the body is cooled in high temperatures, is not confined to the operations of the ſkin, but is effected in an equal or probably greater degree by the lungs, ſo that we muſt regard this organ as poſſeſſing the power of imparting or abſtracting heat to and from the ſyſtem, according to the temperature of the ſurrounding medium, or the occaſional demands of the animal. The application of this hypotheſis to the operations of the living body, as well in the ſtate of health as of diſeaſe, is obviouſly of great extent and importance, but I ſhall not enter into any ſpeculations concerning it until I have endeavoured, by experiment, to eſtabliſh its validity. I ſhall only remark, that I think it will materially aſſiſt in the elucidation of thoſe anomalous facts in animal heat, which ſome eminent modern phyſiologiſts have conceived to be inexplicable upon the theory of Dr. Crawford. I allude particularly to the experiments and obſervations of the late Mr. Hunter, and to thoſe more recently made by my much reſpected friend, Dr. Currie.

Note LIX.

An hypothesis similar to this of M. Cuvier's, has been more lately proposed by Dr. Thomson; *(a)* it is advanced by him, as an original suggestion, and from the candor which pervades his writings, we must conclude, that he had not seen the work of M. Cuvier when he wrote the article of Animal Substances, in the *Supp.* to the *Enc. Brit.* The *Leçons d'Anatomie Comparée*, were published in 1800 (ann. 8), Dr. Thomson's essay in 1801. M. Cuvier and Dr. Thomson, in conformity with the prevailing opinion, conceive that both carbone and hydrogene are removed from the chyle when it is converted into blood, but we have no proof that the proportion of hydrogene is greater in chyle than in fibrine; the experiments that have been performed upon this subject only prove, that azote exists in greater proportion in blood than in chyle.

The hypothesis which is adopted by M. Richerand, to account for the more complete animalization of the chyle, appears singularly perplexed. He supposes, that, as it passes through the lungs, after being mixed with the venous

(a) Chemistry, v. iv. *p.* 497.

blood, a great part of it is depoſited, "par une "ſorte de perſpiration interieure," into the parenchymatous ſubſtance of theſe viſcera. In this ſituation it is oxidated by the contact of the air, and being re-abſorbed by a number of inhalent veſſels, it is carried to the bronchial glands, where it depoſits part of its carbone, "et de fuligineux," and thus communicates to them their black colour. Being thus purified, it is returned into the thoracic duct, and poured again into the ſubclavian vein, whence it is brought back to the lungs, and again ſubjected to the action of the air. "De maniere qu'il ſe fait," ſays the author, "à travers ces organes, une veritable circulation "lymphatique, dont l'object eſt de donner au "chyle un degré d'animaliſation plus advancée."*(a)*

Note LX.

In ordinary experiments, oxygenous gas is obtained from a metallic oxide, heated in contact with the ſulphuric acid. But theſe oxides not unfrequently contain a quantity of carbonic acid, which will neceſſarily be expelled by this proceſs, and mixed with the oxygene; this is particularly the caſe with the red oxide of lead, a ſubſtance which frequently is made uſe of for the

(a) Phys p. 160, 1.

the difengagement of vital air. The black oxide of manganefe, which is often employed for the fame purpofe, is not unfrequently mixed with the carbonate of lime, which will give out its carbonic acid during the procefs.*(a)* When the heat has been applied for a confiderable length of time, and been in any degree violent, part of the fulphuric acid itfelf will be converted into vapour, and be mixed with the gas. It is to accidental impregnations of this nature, that we muft attribute the painful fenfation which fome perfons have experienced in attempting to refpire oxygene.

Note LXI.

Some perfons have inferred from Dr. Prieftley's experiments, that the refpiration of oxygene is productive of injurious effects to the animal conftitution.*(b)* This opinion was, I imagine, founded upon the obfervation of this philofopher, that after an animal had expired in oxygenous gas, a 2d could continue to live for a fhort time in the fame air. Dr. Prieftley, however, attributed the death of the firft animal to the effect of cold, in confequence

(a) Cavallo on Fact Airs, p. 203. *(b) Beddoes on Fact. Air, part* i. *p.* 13.

ſequence of its paſſing through the water by which the gas was confined, and he accordingly was able to prolong the life of the animal for ſeveral hours, until the air was completely deoxidated, by placing it near the fire. "*This expe-* "*ment*," he adds, "*fully satisfied me, that it* "*was nothing in the dephlogisticated air itself,* "*that was the reason that mice could not live in* "*it*."*(a)* The deficiency of external heat appears evidently to have had an important effect upon the life of the animal, but there is another circumſtance, which I am inclined to regard as a ſtill more immediate cauſe of death, viz. a quantity of carbonic acid gas, which would be generated, and be confined in the apparatus. A freſh and vigorous animal would be able for ſome time to bear with impunity a quantity of this gas, which had proved fatal to one whoſe powers were exhauſted by the long continued expoſure to it. In confirmation of this opinion, I may remark, that the very ſame circumſtance has been found by Cigna, Jurine, Morozzo, and others, to occur in the reſpiration of a limited quantity of atmoſpherical air. *(b)*

M. Lavoiſier

(a) Exper. on Air, v. ii. *p.* 165. *(b) Journal de Phys. t.* xxv. *p.* 105, 6. *Chaptal's Chemistry, v.* i. *p.* 131, 2. *Enc. Meth. Art. Med. t.* i. *p.* 496.

M. Lavoiſier, in ſome of his earlier experiments made the ſame obſervation, viz. that after the death of the firſt animal, the air was left in ſufficient purity, to ſupport for ſome time the life of a ſecond. The ſame explanation may be had recourſe to in theſe, as in the experiments of Dr. Prieſtley; indeed, as M. Lavoiſier confined his gas by mercury, there would be a greater proportion of carbonic acid mixed with the oxygene, than when water was employed, as this fluid would neceſſarily abſorb part of the acid. When animals have been confined in oxygene, and the carbonic acid has been carefully removed by a pure alcali, or by lime water, the animal has completely abſorbed the whole of the oxygene without any inconvenience. *(b)*

We may remark, that the experiments of Dr. Prieſtley, in which he found an animal confined in oxygenous gas, to ſuffer ſo much from the effects of cold, afford a ſtrong preſumptive argument againſt the increaſe of temperature, which ſome ſpeculative chemiſts have imagined, muſt be produced by the reſpiration of oxygene.

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(a) Higgins's Minutes of a Society, p. 132.

The only other obſervations upon this ſubject, which I find in the writings of Dr. Prieſtley, are contained in the following paragraph. " From the greater ſtrength and vivacity of the " flame of a candle, in this pure air, it may be " conjectured, that it might be peculiarly ſalutary " to the lungs in certain morbid caſes, when the " common air would not be ſufficient to carry " off the phlogiſtic putrid effluvium faſt enough. " But, perhaps, we may alſo infer from theſe " experiments, that though pure dephlogiſticated " air might be very uſeful as a *medicine*, it " might not be ſo proper for us in the uſual " healthy ſtate of the body; for as a candle " burns out much faſter in dephlogiſticated than " in common air, ſo we might, as may be ſaid, " *live out too fast*, and the animal powers be " too ſoon exhauſted in this pure kind of air." *(a)* It can ſcarcely be conceived, that Dr. Beddoes has built his poſitive aſſertion on the mere ſpeculation of Dr. Prieſtley; a ſpeculation ingenious indeed, and, at the time when it was formed, plauſible, but which has been ſince controverted by the direct experiments of M. Lavoiſier.

Note LXII.

" On ſait que la combuſtion, toutes choſes égales

(a) Exper. on Air, v. ii. *p.* 168.

" egales d'ailleurs, eſt d'autant plus rapide, que
" l'air dans lequel s'opère, eſt plus pur. Ainſi, par
" example, il ſe conſomme dans un temps donné
" beaucoup plus de charbon ou de tout autre com-
" buſtible, dans l'air vital, que dans l'air de l'atmo-
" ſphère. On avoit toujours penſé, qu'il en etoit
" de même de la reſpiration ; qu'il devoit s'accé-
" lérer dans l'air vital, and qu' alors il devoit ſe dé-
" gager ſoit dans le poumon, ſoit dans le cours de
" la circulation, une plus grande quantité de calo-
" rique. *Mais l'experience a detruit toutes ces opi-*
" *nions qui n'etoient fondées que sur l'analogie.*
" Soit que les animaux reſpirent dans l'air vital pur,
" ſoit qu'ils reſpirent ce même air, melangé avec
" une proportion plus ou moins conſiderable de gaz
" azote, la quantité d'air vital qu'ils conſomment,
" eſt toujours la même, à de très-lègeres differences
" près. Il nous eſt arrivé pluſieurs fois, de tenir
" un cochon d'Inde pendant pluſieurs jours, ſoit
" dans l'air vital pur, ſoit dans un mélange de
" quinze parties, de gaz azote & d'une d'air vital,
" en entretenant conſtamment les mêmes propor-
" tions ; l'animal dans les deux cas eſt demeuré
" dans ſon état naturel ; ſa reſpiration & ſa circu-
" lation ne paroiſſoient pas ſenſiblement, ni acce-
" lerées, ni retardées ; ſa chaleur étoit égale, &
" il avoit ſeulement, lorſque la proportion de gaz
azote

" azote devenoit trop forte, un peu plus de difpo-
" fition à l' affoupiffement." *(a)*

Note LXIII.

Soon after it was difcovered that oxygenous gas poffeffes the appropriate power of fupporting animal life, the idea was fuggefted of employing it for medicinal purpofes. As it was known, that in phthifis the lungs are much injured in their texture, and probably rendered incapable of performing their functions to the full extent, fome of the French chemifts conceived that the refpiration of oxygene might prove advantageous in this difeafe, by permitting the fyftem to acquire its due proportion of this element with greater facility. The idea was at leaft as plaufible as the generality of medical hypothefes, and the fubjects of the firft experiments, in confequence of the happy delufion which fo frequently attends this fatal complaint, were infpired with ftrong hopes of the fuccefs of the new practice. *(b)*

Shortly after this period, a fpeculation of an oppofite

(a) Lavoisier, Mem. Acad. 1789. *p.* 573. *(b) Chaptal's Chemistry, v.* i. *p.* 138. *Jurine, Enc. Meth. art. Medecine, t.* i. *p.* 500. *Fourcroy, Ann. de Chim. t.* iv. *p.* 83. *&c seq.*

oppofite nature was formed in this country. It was imagined that the fyftem, when affected by phthifis, had in fome way acquired too large a proportion of oxygene in its compofition; and it was confequently concluded, that benefit muft be derived from the refpiration of a gas which contained a fmaller proportion of oxygene than the air of the atmofphere. The objections which were urged againft this hypothefis were indeed numerous, and the defenders of it were unable to point out by what means the fyftem could acquire this fuperabundant quantity of oxygene. But all oppofition was for fome time filenced by the apparent fuccefs which attended the employment of the pneumatic medicines, and fo fully perfuaded were the profeffion at large of the advantage likely to refult from their ufe, that a public fubfcription was actually commenced, in order to form an inftitution, for the purpofe of curing confumption by the employment of the different gafes. But the infatuation has at length fubfided, and the plan feems to be abandoned by the projector himfelf, who cannot be fuppofed to want either zeal or induftry in the promotion of his favorite fcheme.

Note LXIV.

Few perfons, I conceive, in the prefent day, will

will object to this expreffion. The pertinacity with which the oppofite opinion was maintained by the determined followers of the Brunonian doctrine, appears to me one of the moft fingular contefts of theory againft experience, which has occurred in the hiftory of modern fcience. Dr. Smith, in his valuable differtation "de actione "mufculari," ranks "vapores mephitici" among the chemical fedatives. *(a)*

(a) p. 33.

END OF THE SECOND PART.

In collecting the materials for this work, I have frequently found great difficulty in obtaining the necessary information, in consequence of the imperfection or inaccuracy of references. In order to prevent the recurrence of this obstacle to any future inquirer into the subject of respiration, I have noted my references with as much accuracy as lay in my power; and to remove the difficulty which is sometimes produced by the employment of different editions of the same work, I have subjoined a list of the books and treatises to which I have referred, marking the particular editions that were employed.

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