A DISSERTATION ON RESPIRATION.

TRANSLATED FROM THE LATIN OF

DR. MENZIES.

WITH NOTES,

BY CHARLES SUGRUE,

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PREFACE.

PHYSICIANS were, till very lately, totally ignorant of the effects of refpiration on the fyftem: Modern chemiftry, by difcovering the component principles of atmofpheric air, and that portion of it which underwent a change in the lungs, induced philofophers to examine the fubject with the degree of attention it merited. The names of Black, Crawford, Lavoifier, &c. will be tranfmitted to pofterity, as the firft who have elucidated this important point. The following Differtation will be found to contain experiments made with the greateft degree of accuracy; and the conclufions are fuch as evidently flow from them. This, together with the importance of the fubject, will, it is prefumed, be a fufficient excufe for giving it to the public in an Englifh drefs.

If

If we confider that the better the functions of any organ are afcertained, the more effectually we fhall be able to prevent or cure its difeafes, and that no organ is fubject to more dangerous or obftinate difeafes than the lungs, it will be evident that whatever throws light on fo interefting a topic is worthy of our attention.

Two other motives, which influenced the Tranflator, are, that the Latin edition is extremely rare; and that the experiments and conclufions of Dr. Menzies have met with the approbation of fuch of the Profeffors of this Univerfity, as treat on the fubject in their lectures.

The notes added by the Tranflator, are marked with capitals; the others are chiefly references to authors.

TO

JOSEPH BLACK, M. D.

PROFESSOR OF CHEMISTRY IN THE UNIVERSITY OF EDINBURGH.

&c. &c. &c.

SIR,

I COULD not long heſitate to whom I ought to dedicate this diſſertation, when I conſidered the numerous diſcoveries with which you have enriched ſcience, particularly that of animal heat, of which your ingenuity had, twenty years ago, diſcovered the ſource. This, Sir, has ſpread your reputation throughout the world, and makes you be conſidered as the Father of modern Chemiſtry.

I ought not, among other motives, to omit the ineſtimable advantages I have derived from your lectures; nor ſhall I ever forget the friendſhip and politeneſs you have ſhewn me.

I remain with all due reſpect,

SIR,

Sep. 6th,—1790.

Yours, &c.

ROBERT MENZIES.

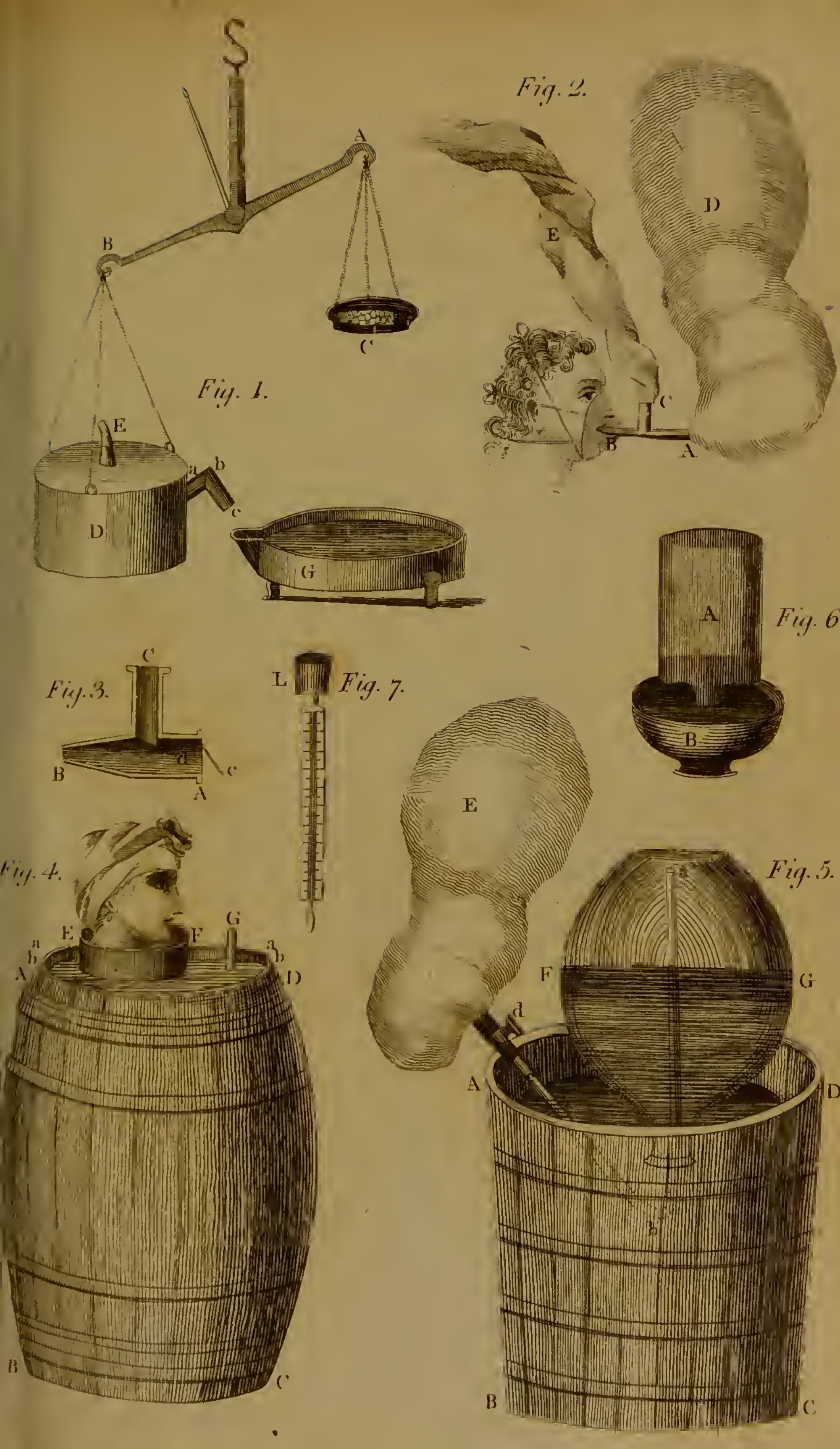
Fig. 1.
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Fig. 2.
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Fig. 3.
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Fig. 4.
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Fig. 5.
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Fig. 6
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Fig. 7
L

ON RESPIRATION.

RESPIRATION is a function ſo neceſſary to life, and ſerves ſo many important purpoſes in the animal œconomy, that whatever may contribute to throw the ſmalleſt light on ſo difficult a ſubject, ſeems worthy of the attention of the phyſician. This conſideration will, I hope, be a ſufficient excuſe for making it the ſubject of a diſſertation.

I have had no idea of attempting to clear up all the difficulties which ſurround this important branch of phyſiology. I have chiefly endeavoured to inveſtigate the quantity of air uſually reſpired by an adult, and to conſider reſpiration as the chief ſource of animal heat.

 These

Theſe ſubjects have of late occupied the attention of the moſt celebrated philoſophers of the age.

In this inveſtigation I ſhall take the liberty of paying no attention to any hypotheſis, however ſpecious, if it be not founded on the ſure teſt of experiment: Syſtems and men may paſs away, but the laws of truth are eternal.

SECT. I.

By reſpiration we mean that function in which, by the alternate dilatation and contraction of the thorax, a quantity of air is received into the lungs, and afterwards expelled from them. The firſt of theſe actions is called inſpiration, the latter expiration.

Inſpiration is chiefly performed by the action of the intercoſtal muſcles, and of the diaphragm; for the ribs and ſternum are elevated and puſhed forwards by the contraction of the

intercoſtal

intercoſtal muſcles; which, joined to the contraction of the diaphragm, dilates the cavity of the thorax in every direction. Thus the lungs, which cloſely adhere to the ribs and diaphragm, and paſſively obey their motions, are extended, and the external air ruſhes into them through the trachea, by which the equilibrium is preſerved. During expiration, all the muſcles contracted during inſpiration are relaxed; the cartilages of the ribs and the mediaſtinum, from their inherent elaſticity, gradually return to their former ſituation; hence the ribs are puſhed down, the diaphragm elevated, and the cavity of the thorax diminiſhed. This is the cauſe why the portion of air remaining in the lungs is expelled. Beſides, the contraction of the abdominal muſcles, and perhaps the elaſticity of the lungs, aſſiſt expiration.

In this manner the alternate motions of reſpiration are carried on, from the moment of our entrance into the world, till the vital flame is extinguiſhed. It ſerves various uſeful, though

ſecondary,

(*R*) fecondary, purpofes; fuch as the formation of the voice, the expulfion of the fæces, urine, fœtus, &c. This action is in a great meafure fubject to the influence of the will, though Providence has wifely ordained that it fhould not be totally dependant on that faculty; when neceffary,

(*R*) Phyficians have differed much with refpect to the final caufe of refpiration. Finding it lefs laborious to build hypothefes than to make experiments, fome imagined the blood to be cooled in the lungs; others, that it abforbed nitre in its paffage through them. In the fecond part of this differtation we fhall find the author prove, as clearly as any phyfiological fact can be proved, the opinion of Black, Crawford, &c. that the lungs are the fource of animal heat. Dr. Goodwin remarked alfo the florid colour the blood receives in its paffage through the lungs; and found that when it poffeffed this florid colour it ftimulated the left auricle to contraction, but otherwife not. He does not venture to pronounce what change the blood undergoes, but fuppofes, "That the chemical change which the blood undergoes in "the lungs by refpiration, gives it a ftimulating quality, by "which it is fitted to excite the left auricle and ventricle to "contraction;"—and concludes from hence, the chief indication for recovering drowned perfons to be inflation. But I think he ought not to recommend dephlogifticated air for this purpofe; as, if the principle of accumulated irritability be allowed, its effects would be too violent.

neceſſary, it is aſſiſted by the action of all the muſcles attached to the ribs.

With reſpect to the different theories on the cauſe of the firſt inſpiration, and of the alternate motion of the diaphragm and intercoſtal muſcles; as I have nothing to ſay which would ſerve to elucidate the ſubject, I ſhall paſs theſe opinions over in ſilence; nor ſhall I venture, as many ingenious * authors have done, to explain them on mechanical principles. I prefer adopting the opinion of the celebrated Monro, "that it is the work of an all-wiſe Being." †

Although the ſtudy of primary cauſes is abſurd, and although the ſource of primary motions is beyond our comprehenſion; nevertheleſs, an experimental inveſtigation of their mode of action muſt contribute to the advancement of ſcience.

Thus, though we can never hope to be acquainted with the primary cauſe of the motion of the

* Boerhaave, Martin, Whitt, Haller, &c.

† Nervous Syſtem, p. 101.

the heart and lungs; any fact, which may throw light on the laws by which they are governed, merits our utmoſt attention. To this claſs, in my opinion, belongs the exact eſtimation of the quantity of air reſpired, in a healthy ſtate of the ſyſtem. Though this, at firſt ſight, appears inſignificant; yet, if we conſider how intimately it is connected with the various theories on reſpiration, on the paſſage of the blood through the lungs, on animal heat, and with the cure of (*P*) diſeaſes, which can never be underſtood without

(*P*) The moſt violent and obſtinate diſeaſes of the ſyſtem are ſeated in the lungs; ſuch are pneumonia, aſthma, and the different ſpecies of phthiſis. Dr. Beddoes has lately aſſigned a theory of the latter diſeaſe, very different from that before adopted by phyſicians. Laying it down as a principle, that oxigene is received into the ſyſtem during reſpiration; he ſuppoſes phthiſis to depend on a certain ſtate of the lungs, by which too much oxigene is received into the ſyſtem. He ſupports his opinion by ſeveral ingenious analogies; and by the good effects phthiſical patients have experienced from re-reſpiring carbonic gas. We have not, however, as yet, a ſufficient number of facts to enable us to determine in what caſes the different gaſſes may be adminiſtered with advantage, nor in what proportion they ought to be combined.

Many

without this previous knowledge, it will appear worthy of all the attention we can beſtow.

Since

Many celebrated philoſophers deny the abſorption of oxigene: They think that the combination of oxigene with hydrogene and carbone in the lungs, and the conſequent evolution of heat, explain in a ſatisfactory manner the phenomena of reſpiration. Dr. Girtanner has made ſeveral experiments, with a view of aſcertaining this important fact; and their reſult ſeems to favour the opinion of the abſorption of oxigene: They prove, at leaſt, the preſence of oxigene in arterial, and of carbone and hydrogene in venous blood. Dr. Girtanner, not content with this, ſuppoſes oxigene to be the principle of irritability: But one conſideration, I think, overturns this hypotheſis. In the fœtus the blood is of a darker colour than in the adult; yet the more frequent contractions of the heart and arteries, and the greater effect produced by ſtimuli, prove the greater irritability of the fœtus. The irritability of the ſyſtem gradually decreaſes from the firſt moment of life to that of its extinction; yet, till the moment of birth, the fœtus receives oxigene only from the mother; and that the quantity it receives is ſmall, appears from the dark colour of the placenta. After birth, the lungs perform their functions imperfectly for ſome time. This is evident from the colour of infants being darker than that of adults; and ſtill more ſo from the ſtory of the blue boy mentioned by Dr. Beddoes; the right ventricle of whoſe heart communicated with the left one, and conſequently only a ſmall portion of blood

Since the middle of the laſt century to the preſent time, phyſiologiſts have endeavoured to aſcertain the quantity of air inſpired, or how much

blood paſſed through the lungs: nevertheleſs, for the firſt year the boy felt no inconvenience; but at the end of that period the dreadful ſymptoms commenced, which gradually increaſed till he died, in his thirteenth year. Does not this caſe prove, that the change the blood undergoes in the lungs is not ſo neceſſary the firſt year, but that it becomes more and more ſo as we advance in years? Is it not rational to ſuppoſe that oxigene, if it be abſorbed, acts as a ſtimulus on the ſyſtem? This will explain why Nature has permitted only a ſmall portion of it to be received into the ſyſtem during infancy; becauſe the irritability being then greater, a ſmaller proportion of ſtimulus is required; but, in the ſame ratio as the irritability is diminiſhed, the ſtimulus is increaſed, till we arrive at the acme of vigour, when the quantity of irritability and ſtimulus may be ſuppoſed to be on a par, and the lungs come to their full growth.

But though it is not yet ſufficiently proved that oxigene is abſorbed; this is not an argument againſt Dr. Beddoes' method of cure in phthiſis. As it is demonſtrated that the blood receives a ſupply of heat in the lungs; the phenomena of phthiſis, aſcribed to the abſorption of oxigene, may be equally well explained by the abſorption of heat. This we ſhall be the more convinced of, if we conſider, that oxigenous gas alone undergoes any change from reſpiration; which perhaps depends on the more looſe combination of caloric with much the thorax is dilated in each ordinary inſpiration *. A late author has come very near the point. As many experiments have been made with this view, and as a great deal has been written on the ſubject; we ſhall give, in a few words, an account of the moſt important experiments, and their reſult.

The firſt experiment we ſhall mention, is that made by Borelli † in the middle of laſt century. Having breathed through a glaſs-tube, of

 which

with oxigene, than with any of the other baſes: and the good effects of the combination of hydrogene gas, &c. with atmoſpheric air, muſt in this caſe be explained by a ſmaller quantity of heat being given out.

Whatever opinion be adopted, it is evident, that the effects of the air reſpired on the ſyſtem muſt vary with the proportion of its conſtituent principles. As that ſtate of the ſyſtem called phlogiſtic, or ſthenic diatheſis, is always accompanied with an increaſe of heat, and as it is proved that the lungs are the ſource of this heat; may we not diminiſh the heat of the ſyſtem, by diminiſhing the quantity of it evolved in the lungs? How far this may be effected in pneumonia, &c. muſt be determined by future experience.

* Goodwin on Animation.

† Borelli on the Motion of Animals, p. 119 & 133.

which the volume was aſcertained, and of which one end was immerſed in ſome bubbles of ſoap, he found the quantity of air received into the lungs in one inſpiration, to be about 15 cubic inches; and towards the end of the experiment, to be between 18 and 20 cubic inches.

It is however clear, that beſides the friction and other cauſes which render this experiment inaccurate, it is liable to objection, as being only the meaſure of one inſpiration. For as reſpiration is a function in ſome degree ſubſervient to the will, it is evident, we can draw no concluſion from the meaſure of a ſingle inſpiration.

The next author we ſhall mention, who attended to this ſubject is the celebrated Jurin, who came ſo near the truth, that we ſhall give his experiment in his own words. " In conſe-
" quence of an experiment he had made, the
" celebrated Borelli imagined the quantity of
" air expelled, in an ordinary expiration, to be

between

" between 18 and 20 cubic ounces. It is " however, different, not only in different " men, but even in the fame man at different " times. I made the experiment thus. I fuf- " pended a weight to the lower part of a blad- " der, which I had previoufly moiftened, and " having fixed a tube of about an inch diame- " ter in the upper part, I ftopped my noftrils, " and infpired the air of the bladder gently, " during three minutes, the weight remaining " all the while on the table. I then plunged " the bladder, with the air inclofed in it, and " the weight fufpended to it, into water con- " tained in a cylindrical veffel. I marked the " height to which the water rofe. Then hav- " ing fqueezed the air out of the bladder, I " again plunged it into the water with the " weight. The difference of the height to " which the water rofe in both thefe cafes, was " eafily calculated. Having repeated the ex- " periment ten times, and added the quantities " together, the tenth part of the fum total, or

" the

" the proportional difference of the height to
" which the water roſe in both caſes was found
" equal to 35 cubic ounces, which is the vo-
" lume of air contained in the bladder; and
" having added about the twelfth part, or 3
" ounces, on account of the condenſation of
" the air from the coldneſs of the water, as it
" was winter, we ſhall have 38 cubic ounces.
" Beſides, a little muſt be added, both on ac-
" count of the preſſure of the water on the
" bladder, and of the moiſture which is expell-
" ed with the air, and ſoon condenſed by the
" coolneſs of the water and of the bladder. I
" then calculated the quantity of air, expelled
" by a moderate expiration in the ſpace of
" three minutes, at 40 cubic ounces. In the
" ſtrongeſt expiration I expelled 125 cubic
" ounces in the ſpace of a minute.' But in a
" very ſtrong expiration, continued till I was
" almoſt ſuffocated, I expelled 220 cubic oun-
" ces from my lungs. From whence it follows,
" that

" that there is more air in the lungs than can
" be expelled by an ordinary expiration *."

This experiment proves that the quantity of air ufually expired is equal to 40 cubic inches. the accurate Hales, Haller, and Sauvage, who have repeated thefe experiments, have agreed that the refult was the fame. But as it is only the meafure of one infpiration, it lies open to the fame objection as the former.

The laft who endeavoured to inveftigate this matter, was the ingenious Dr. Goodwin, he differs very much from thofe who preceded him in this career, and feems to have erred by not fufficiently varying his experiments. But as his reafoning appears, at firft fight, very accurate, it is neceffary to give his own words. "We
" muft now eftimate the quantity of air ufual-
" ly refpired. This can be pretty accurately
" done, if we endeavour to breathe from a vef-
" fel full of air, joined by means of a tube, to
" another

† Motte's Abridgement of Philof. Tranfact. v. i. p. 415.

" another full of water; ſo that a certain vo-
" lume of water, equal to the volume of the air
" inſpired, may get into the place of the latter
" after each reſpiration; for the volume of wa-
" ter, ſubſtituted to that of the air inſpired,
" muſt be equal to the volume of air conſumed
" in inſpiring.

" With this view, I thought on the machine
" A B C D G, Fig. 1. of which the veſſel D
" (which I call the pneumatic one) is capable
" of containing more than 100 cubic inches of
" air. The tube *a b c* is immerſed in water;
" whilſt the tube E is received into the mouth
" in order to breathe the air incloſed in the
" veſſel. But as the veſſel D is ſuſpended to
" A B; if the tube *a b c* be immerſed into the
" veſſel G filled with water, whilſt ſome per-
" ſon breathes through the tube E; then a
" volume of water, preciſely the ſame with
" that of the air reſpired, will get into its place.
" On the other hand, the weight of the vo-
" lume of water will be ſhewn by the other

" ſcale

" ſcale C; and having calculated, we ſhall find " the number of cubic inches of water, which " entered into the pneumatic veſſel during re- " ſpiration; but it is plain that the number of " cubic inches of water, which have paſſed in- " to the veſſel, will be equal to the number of " cubic inches of air reſpired." . . . .

" An adult, of a middle ſize, and in good " health, endeavoured to reſpire, as naturally " as poſſible, from the pneumatic veſſel.

" 1ſt time, he inſpired 3 } cubic inches.
" 2d time, he inſpired $2\frac{1}{2}$ }

" Another perſon, of the ſame ſtature, en- " deavoured to breathe out of the veſſel.

" At the firſt inſpiration,
" he breathed $3\frac{1}{2}$ } cubic inches."
" At the ſecond, $2\frac{1}{4}$ }

But as the difference of the reſult of both theſe experiments may be ſuppoſed to proceed from the different degree of attention, he thus varied it. " The ſame man inſpired and ex- " pired thirty times out of the ſame veſſel, and

" as

" as nearly as possible with the same degree of " exertion; and, having calculated, we found " the average quantity of air of each respira- " tion to be $2\frac{1}{4}$ cubic inches.

" The same experiment being repeated with " the greatest care, the average quantity of " each respiration was found to be 3 cubic in- " ches.

" Another man, of the same stature, breath- " ed thirty times from the same vessel, and in " the same manner; and the average quantity " was $3\frac{1}{4}$ cubic inches. Hence it follows, that " the greatest quantity of air, received into the " lungs, during each natural respiration, does " not exceed $3\frac{1}{6}$ cubic inches; much less than " what Hales and Jurin had calculated."

But in the last dissertation, published by the same author, it is mentioned, that a dull kind of pain was felt in the chest, before the man had finished the number of inspirations; and, having removed the tube from his mouth, it was

was neceſſary to make a deep inſpiration, which ſeems to prove, that the quantity of air, received into the lungs, was not ſufficient for the purpoſes of reſpiration. This defect is, however, attributed by the author to the imitation of natural reſpiration, which could not counterbalance the difficulty of raiſing water contrary to its natural gravity.

As an attempt to breathe in the open air, would not give the meaſure of an ordinary inſpiration from the machine, it was neceſſary to aſſiſt the action of the lungs; by which the quantity received was much increaſed, as is evident from the following experiment. "Three "perſons of ordinary ſtature, inſpired from the "pneumatic veſſel thirty times ſucceſſively, and "took in as much air at each time, as the ſen- "ſations in the breaſt ſeemed to require. The "average quantity of air taken into the lungs "at a ſingle inſpiration,

" By the firft, was	12	
" By the fecond,	14	cubic inches."
" By the third,	11*	

Thus Dr. Goodwin concludes the quantity of air infpired to be equal to 12 cubic inches, which are dilated by heat to 14; and as the quantity of air remaining in the lungs, after an ordinary expiration, is near 109 cubic inches; he concludes the proportion of the dilatation of the lungs, before and after a healthy infpiration, to be as 109 to 123.

Although Dr. Goodwin has candidly mentioned fome of the caufes, which render his experiments inaccurate; there are, however, others which clearly explain the difference between the refult of his experiments and thofe of Jurin. For befides the refiftance of the column of water to be raifed contrary to its natural gravity; as the fpace of time neceffary for the water to flow from one veffel into another, does not

* Goodwin on the Connexion of Life with Refpiration.

not differ much from the time of a common infpiration, it is evident that the air in the pneumatic veffel D, fig. 1. muft be in fome meafure expanded, and confequently, the moment the mouth is removed from E, the external air will rufh in to reftore the equilibrium, and will drive back the column of water in the tube *a b c*, even before a proper quantity can have paffed into the veffel D. If the tube *a b c* be large, the quantity of water driven back will be confiderable; and if it be fmall, the fpace of time neceffary for the water to pafs from one veffel into another being longer, and the refiftance made by the air rufhing into the pneumatic veffel, will make the error ftill greater.

Thefe objections may however be, in a great meafure, obviated by the ufe of valves, and by raifing the water to the fame height in both veffels. However, in the different experiments I made on this fubject, I found that no confidence could be placed in this method, on account of the many inaccuracies to which it is liable; although it may ſometimes be ſerviceable. I freely confeſs that it was by chance I firſt diſcovered how inaccurate it was.

SECT. II.

As a ſhort time before I had filled ſome allantoids firſt with inflammable air, and afterwards with my breath, the idea ſtruck me, that by means of a tube ſufficiently large, and with good valves joined to it, I could pretty accurately aſcertain the quantity of air uſually reſpired; as, on account of the large volume of the allantoid, the average quantity of the number of reſpirations could be eaſily taken; and on account of the thinneſs of the membrane, the air could never be much condenſed.

EXPERIMENT I.

Having meafured the volume of the allantoid D in two different manners, firft by the quantity of water it was capable of containing, and afterwards by the diameter and area of it, I found it to be nearly equal to 2400 cubic inches; and having joined to it the machine A B C, fig. 2d and 3d, confifting of two pretty large tubes joined at right angles, in which are feen the two valves *d* and *c*, fo that the air, without any unufual effort, can be infpired through C, and expired through A; the tubes are large, the valves are made out of an allantoid, and are fo thin that the fmalleft force moves them. I then began to expire, and did not remove my mouth from the tube till I had filled the allantoid; taking care to ftop my noftrils during expiration. The allantoid was filled, in repeated trials, by about 56 expirations as natural as poffible. So that if you divide

2400 cubic inches, the volume of the allantoid by 56, you will have 42.8 cubic inches as the average quantity of air usually expired.

Besides the allantoid E was filled with atmospheric air through C, and the quantity inspired was found nearly the same.

Several persons, of the middle size, repeated this experiment with nearly the same result. The difference was scarcely ever more than one or two cubic inches *.

I always took care that the valves were properly fixed on, so that a man may breathe through A B C when joined to the allantoid.

Surprised at the difference between the result of mine and Dr. Goodwin's experiments, I took two broad vessels, which communicated with each other in the middle, by means of a tube supplied with valves, constructed in such a peculiar

* The volume of air must necessarily be increased or diminished, according to the degree of heat, or of pressure, to which it is exposed.

culiar manner as to permit the water to paſs only into the veſſel from which I inſpired. And in order to be able to make many inſpirations without taking off the mouth, and to prevent the external air from ruſhing in, the machine A B C was put into the place of the tube E: on account of the breadth of the veſſels, the perpendicular column did not vary much. By pouring water into the open veſſel, it was preſerved in both at nearly the ſame altitude. The veſſel, from which I inſpired, was ſuſpended in a broad but accurate ſcale; but the reſult of theſe experiments made by me and many of my friends, was ſo different, ſometimes 4, ſometimes 10, ſometimes 20 cubic inches, and the effort was ſo painful, that we agreed to prefer the allantoid.

But as it may be objected to this method, that the attention muſt be neceſſarily directed to what is going on, and that the allantoid cannot always contain the ſame quantity of air, I determined to vary the experiment, ſo as either

to

to confirm the former refult, or to difcover if there had been any miftake. A method which did not appear liable to any objection, and which feemed accurate, was that propofed by Boerhaave, of plunging a man into the water up to his chin, and judging of the dilatation of the lungs from the afcent and defcent of the water. I know of no perfon who has tried this method. But as it is neceffary that the man fhould be placed in fuch a manner, as not to be obliged to move any part of his body above the furface of the water; and as it is requifite that the afcent and defcent of the water fhould be accurately marked; it is evident, that if a man be put into the hogfhead A B C D, fig. 4. in the top of which there is a hole large enough to permit his head to pafs out, and if the cylindrical veffel E F, be well fitted about his neck up to his chin, the hogfhead being filled with water, a quantity of this water, equal in volume to the air refpired, will alternately afcend and defcend into the veffel E F.

This

This quantity may be eaſily known by multiplying the altitude to which the water riſes by the area of the cylindrical veſſel, leſs the area of the neck; or if the water between *a a* and *b b* be taken out and meaſured, it will give the meaſure of a common reſpiration. And as a man may remain ſome hours in warm water without any inconvenience, there will be time enough, not only to mark the height to which the water riſes, but alſo to avoid any errors. I thus made the experiment.

EXPERIMENT II.

A healthy man, 5 feet 8 inches in height, three feet and ſome odd inches about the thorax, was cloſely ſhut up in the hogſhead A B C D, fig. 4, which was filled with water heated to the 90th degree of Fahrenheit's thermometer, as far as that part of the neck which was beſt ſuited to meaſure the difference of aſcent and deſcent. This difference was about the height of 1.25 inches. His pulſe, both before and after immerſion, beat 64 or 65, and his reſpirations were 14 or $14\frac{1}{2}$, in the ſpace of a minute. And they remained the ſame during the two hours and upwards he remained in the hogſhead, without any inconvenience in any reſpect. During all that time, the difference between the aſcent and deſcent of the water was $1\frac{25}{100}$ inches at leaſt. But when he made a deep inſpiration, ſo much air ruſhed into the lungs, that the water paſſed out through the cylindrical veſſel. But as the area of the cylindrical veſſel was 55.41 ſquare inches, and the area of the neck 18; (*Z*) $55.41 - 18 \times 1.$

(*Z*) It is evident, that if the area of the neck be ſubtracted from the area of the cylindrical veſſel, it will give the area of the ſpace between the neck and the ſides of the veſſel: now, if this area be multiplied by 1.25 inches, the altitude to which the water riſes, it will give the area of the ſpace which the water fills between the neck and ſides of the veſſel; conſequently the volume of the water itſelf, and conſequently that of the air reſpired will be found out.

25=46.76 cubic inches as the quantity of air ufually refpired by this man. This experiment was thrice repeated with the fame refult. But leaft there fhould be any miftake, I made him breathe through the allantoid.

EXPERIMENT III.

The fame man breathed through the machine A B C, fig. 2, into the allantoid D, which was found to contain 2700 cubic inches of air*; and, in many trials, filled it with 58 expirations, which gives 46.55 cubic inches as the quantity of air expired; this calculation is very near the preceding one. But as it was manifeft

* 1ft, A cow's bladder filled with air was found to drive out 370 cubic inches of water, and the allantoid contained $7\frac{1}{3}$ as much air as the bladder; confequently 2715 cubic inches of air. 2dly, The average area was equal to 70 fquare inches, the length to 39. Thus 39×70=2730. The average number of this=2722.5. but we fhall ufe the round number 2700.

manifeſt to me, that the reſpirations of this man were never more than 14 and 14½ in a minute †, it appeared probable that he inſpired more air than other men of the ſame ſtature. In order to aſcertain this, and to be able to eſtimate the average quantity of air reſpired, it was neceſſary to examine the reſpiration of a man of ſmall ſtature.

EXPERIMENT IV.

ANOTHER man, only 5 feet and an inch in height, was ſhut into the ſame hogſhead, except that the cylindrical veſſel was ſomewhat changed. The pulſe beat 72 ſtrokes, and the number of reſpirations was 18 in a minute. The water was heated between the 85th and the 90th of the thermometer of Fahrenheit. The

† In the laſt experiment, the number of reſpirations in each minute, was equal to 16 of the ſame man; the quantity of air reſpired was however nearly the ſame, viz. 46.72.

The difference between the aſcent and deſcent, during a long time he remained there, was 0.95 of an inch, or, in common fractions, the $\frac{95}{100}$ parts of an inch. The area of the cylindrical veſſel made uſe of in this experiment, was equal to 57.012 inches, and the area of the neck was equal to 14.0837 inches. Hence (as will be eaſily underſtood from the calculation in the former experiment) $57.012 - 14.0837 \times 0.95 = 40.781$ cubic inches, as the quantity of air taken into the lungs, by a common inſpiration.

This was confirmed by his breathing into the allantoid, which gave us from 38 to 40 cubic inches as the meaſure of a common inſpiration. So that if you take half the quantities of the 2d and 4th experiments, you will have 43.77 cubic inches as the average quantity of air reſpired. The aſcent and deſcent of the water were carefully marked by means of the glaſs-tube G, fig. 4, on which a ſcale of degrees was cut; this was not however quite ſo uſeful as

may

may be expected, on account of the ſhort time the water remained ſtationary. But when ſomething had been ſubtracted *, on account of the attraction between the water and the ſides of the tube, the meaſure of the aſcent and deſcent was pretty accurate. So that if it may ſeem neceſſary to ſubtract any thing † more on account of the dilatation of the air by the heat of the lungs, we may ſtill compute the quantity of air inſpired, at 40 cubic inches.

Dr Goodwin ſuppoſes, that only 109 cubic inches of air remain in the lungs after an ordinary expiration, and that the proportion of their dilatation after an ordinary expiration is to

* Viz. $\frac{1}{20}$: For although the aſcent and deſcent was in reality 1.3, inches in the 2d experiment, and in the 4th experiment equal to 1 inch; yet, on account of the attraction between the water and the ſides of the veſſel, the firſt was only eſtimated at 1.25, and the latter at 0.95.

† But as atmoſpheric air is dilated by every degree of heat about $\frac{1}{471.5}$; 40 cubic inches, heated 40° will be increaſed in volume by 3.38 cubic inches. For $\frac{40}{4715} \times 40° = 43.38$. Thus $43.77 - 3.38 = 40.39$.

to that of their dilatation after an ordinary infpiration as 109 to 123 *. But I remarked that many men, efpecially when fhut up in hogfheads, after an ordinary expiration, could ftill expel 70 cubic inches of air from their lungs. Hence after fuch an expiration, only 39 cubic inches of air will remain in the lungs. Without doubt then, as appears from another experiment †, Dr Goodwin fuppofes that 109 cubic inches of air remain in the lungs after an extraordinary expiration. But as we have found 70 cubic inches to be the difference between an ordinary and an extraordinary expiration, this number added to the former or to 109, will give 179 as the quantity of air remaining in the lungs after an ordinary expiration. Now the former experiments fhew that the quantity of air expelled by an ordinary expiration is equal to 40 cubic inches, confequently

* Connexion of Life with Refpiration, p. 37.

† Id. p. 46.

quently if this be added to 179, we ſhall have 219 cubic inches as the quantity of air contained in the lungs before expiration: Hence the dilatation of the lungs before and after an ordinary expiration, will be as 219 to 179; or in other words, the thorax will be increaſed by a quantity of air nearly equal to a cube of 3½ inches which, if we conſider how much the thorox and abdomen are dilated by an ordinary inſpiration, ſeems very trifling. The difference between an extraordinary expiration, or one made with ſome effort, and an extraordinary inſpiration, I often found to exceed 200 cubic (*T*) inches.

SECT.

(*T*) Dr Goodwin expelled the air from the lungs of three dead bodies of an ordinary ſize, and in one found the quantity of air 272 cubic inches, in another 250, and in the third 262, but theſe ſubjects were hanged; and, as he remarks, perſons under the influence of fear make a deep inſpiration, and cannot afterwards expel the air on account of the cord tied round the trachea.

SECT. III.

Although then the lungs, before and after expiration, are much more dilated than Dr Goodwin had ſuppoſed; this by no means invalidates his objections againſt Haller's theory on the facility or difficulty of the paſſage of the blood through the lungs in the different ſtages of reſpiration. And although I ſhould by no means pretend to decide on the ſubject; yet it is evident from Dr Goodwin's experiments, particularly when he induced an artificial hydrothorax (*S*), that the blood can paſs through the

(*S*) The chief uſe of reſpiration, according to Haller, is to dilate the lungs ſo as to permit the free paſſage of the blood from the right ventricle to the left auricle; but as Dr Goodwin computed, that only 14 cubic inches of air were taken into the lungs by each ordinary inſpiration, and as he found, that though he diminiſhed the cavity of the lungs $\frac{1}{3}$ of its ſpace, by creating an artificial hydrothorax, the animal felt no great inconvenience; he hence juſtly concludes, that though reſpiration ſhould be ſuſpended, the free paſſage of the blood from the right lungs, whilſt they are in a greater ſtate of collapſe than ever happens during natural reſpiration. I cannot then heſitate in coinciding with him in opinion, "That the dilatation of "the lungs is not the final cauſe of reſpiration."

ANIMAL HEAT.

I SHALL next venture to examine how the aſcertainment of the quantity of air uſually reſpired may throw light on the generation of animal heat, and on the quantity of heat generated in a given time.

Mayow was, I believe, the firſt who hinted that

right to the left ſide of the heart would continue, if there had not been ſome other cauſe. Dr Menzies's experiments, though they prove the quantity of air inſpired to be much greater than Dr Goodwin had ſuppoſed, do not deſtroy his arguments againſt Haller's theory on the uſe of reſpiration; for Dr Menzies proves that between $\frac{1}{4}$ and $\frac{1}{5}$ of the air contained in the lungs is taken in by a ſingle inſpiration, and Dr Goodwin proves that the lungs may be reduced to $\frac{2}{3}$ of their volume, with preventing the paſſage of the blood.

that animal heat was generated in the lungs; this was during the laſt century. Since his time, many other hypotheſes were framed and died away; ſuch as, that it depended on the friction of the fluids againſt the ſides of the veſſels, on fermentation, &c.; and that the air rather ſerved to cool the blood.

But the celebrated Dr. Black made it appear probable, more than 20 years ago, that animal heat was generated in the lungs, and ſupported his opinion by many ſtrong arguments; viz. that air is changed in the ſame manner by reſpiration as it is by combuſtion, which he beautifully demonſtrated by paſſing air changed by each mode, through lime-water; next that the degree of heat in different animals is in proportion to the quantity of air they mephitize; and that the fœtus in utero does not generate heat till it has begun to breathe.

The ſame philoſopher alſo, by remarking the different capacities of mercury and water for heat, and by eſtimating as much as it can be

done,

done, the heat of various bodies, laid the foundation of the doctrine of comparative heat. Drs. Jurin and Crawford prosecuted the subject farther. The latter, assisted by Dr. Black's discoveries, clearly proved, in my opinion, that animal heat is derived from the change induced on the air in the lungs (*Y*), by which its capacity for heat is diminished, and a quantity of sensible heat is given out. Dr. Crawford has laid it down as a principle, that the quantity of heat, which is yielded by pure air, when it is converted into fixed air, and aqueous vapour, is such

(*Y*) Dr Crawford concludes from his experiments, that animal heat depends on elective attraction; oxygenous gas is received into the lungs; the blood is returned from all parts of the system impregnated with carbonated hydrogene; carbonated hydrogene has a greater affinity for oxygenous gas than for the blood; consequently a double decomposition takes place, the hydrogene unites with the oxygenous gas, and the heat separated from the oxygenous gas unites with the blood. The arterial blood having thus received a supply of caloric, gives it out in the course of circulation, and receives hydrogene in the capillary vessels.

ſuch (as if it were not diſſipated) could raiſe the air or vapour ſo changed to 4 times *.

On account of the nicety of his experiments with reſpect to the different capacities for heat of the different kinds of air, and on account of the uncertainty of the loweſt degree of heat, which he by no means imagines to have aſcertained †, many objections have been made to his doctrine, and ſome perſons have totally rejected it.

But if it can be proved that animal heat is not only generated in the lungs, but that the quantity of it thus generated can be eſtimated independently of any theory of the nature of heat

* This paragraph is rather obſcure in the original, but the words of Dr Crawford himſelf are, "The quantity "of heat yielded by pure air, when it is converted into "fixed air, and aqueous vapour, is ſuch (if it were not "diſſipated) as would raiſe the air and vapour, ſo changed, "to more than four times the exceſs of the heat of red hot "iron above the common temperature of the atmoſphere."

† Crawford on animal heat, p. 375.

heat itfelf, and without any calculation of the different capacities of the different airs or of the loweft degree of heat; it will ferve to confirm the conclufions of the ingenious Dr. Crawford, as the refult will be found the fame, though arrived at by a different mode of reafoning.

1ft, The following circumftances render it probable, that animal heat is generated in the lungs; thofe animals alone, which have lungs, and breathe air, can preferve themfelves in a degree of temperature fuperior to that of the furrounding bodies (*W*); and their degree of heat

(*W*) Another queftion intimately connected with this, is the power of animals to generate cold, or to fpeak more philofophically, to preferve themfelves below the temperature of the medium they live in, if it be exceffive. The increafed evaporation had been fuppofed the caufe, but as it happens equally in a dry or a moift atmofphere, Dr Crawford concludes that fome other caufe muft contribute to it; his experiments prove that lefs air is mephitized by refpiration in a warm than in a cold medium, in other words, that lefs hydrogene is given out by the lungs in the former than in the latter cafe; but this hydrogene he fuppofes to be combined with the blood in the capillary heat is in proportion to the volume of their lungs, and to the quantity of air inſpired in a given time. Thus birds, whoſe lungs are proportionably larger than thoſe of other animals, and who mephitize more air in a given time, are found to have very warm blood. On the contrary, fiſhes and amphibious animals have their blood more or leſs warm according to the quantity of air they require.

2dly, It will appear that animal heat depends on the change the blood undergoes in the lungs from the action of the air, if we attend to what takes place in combuſtion. For in the combuſtion of wax, of coal, which cannot be kept up without vital air, we find the air

capillary veſſels, and heat conſequently given out; hence leſs hydrogene is combined with the blood in a warm temperature, and conſequently leſs heat given out; in other words, a degree of cold is produced. By evaporation the ſurface of the body is cooled, and by the union of a ſmaller proportion of oxygenous gas with hydrogene in the lungs, a ſmaller quantity of heat is given out there.

air in which the combuſtion had taken place to be deſtructive to animal life, incapable of ſupporting the flame of a candle, and a part of it to be converted into fixed air. The very ſame, as Dr. Black has proved, happens in reſpiration. Beſides from Lavoiſier's accurate analyſis of atmoſpheric air, 73 parts of it conſiſt of nitrogene gas, and 27 of oxygenous; and the latter of theſe alone is changed by combuſtion or reſpiration (*X*). But, in the combuſtion of coal, it is proved that this part of atmoſpheric air is entirely converted into fixed air, and that a pretty large quantity of heat is generated. As in reſpiration the air undergoes a ſimilar change,

(*X*) This is proved by the following experiment. If an animal be placed in a jar inverted over water or mercury, in ſome time it will periſh; the air will be diminiſhed in volume, and the portion remaining will be found to conſiſt of phlogiſticated or nitrogenous gas, with a ſmall portion of oxygenous gas diffuſed in ſuch a manner as to be unfit for the purpoſes of reſpiration.

change, we are authorifed to conclude, that an equal quantity of heat is evolved.

But this is not fupported by analogy alone. For the experiments of Crawford *, Lavoifier and De la Place †, prove that when equal quantities of air are vitiated by refpiration and combuftion, equal degrees of heat are evolved. The celebrated Crawford found, that if an animal was fhut up in a veffel furrounded with water, and protected from the accefs of air by very foft wool, 100 meafures of air, each containing an ounce, vitiated either by this animal's refpiration, or by the combuftion of wax or coal, communicated the following quantities of heat to 31‡ lb. 7 oz. of water.

* On animal heat.

† Memoire fur la chaleur.

‡ The weight meant here, and in other parts of the differtation, is that ufually called troy weight.

		Degrees.
100 meafures of vital air vitiated by the	combuftion of coal, gave	19.3
	———— wax,	21
	refpiration of a guinea-pig,	17.3*

But as each degree of Crawford's thermometer makes only one tenth of a degree of Fahrenheit's, it is very evident, if a thoufand fuch meafures were vitiated by the combuftion of wax, and the refpiration of a guinea-pig, that the difference between the quantities of heat given out to 31 lb. 7 oz. of water will make 3.7 degrees of Fahrenheit's; the difference between the quantities given out by the combuftion of coal, and the refpiration of a guinea-pig, will be equal to 2° of Fahrenheit's thermometer, or the heat communicated to the water by the guinea-pig, will be two degrees lefs than that communicated by the combuftion of coal.

But from the feries of experiments of Lavoi-
fier

* On animal heat, p. 351.

ſier and De la Place *, it appears, that the heat communicated by the reſpiration of a guinea-pig, when equal quantities of air were vitiated, was greater than that generated by the combuſtion of coal, in the proportion of 13 to 10.3. I am ſorry I cannot repeat theſe experiments with the Calorimeter †, the only method of doing it accurately. But as cotemporary authors have ſuppoſed the degree of heat ſaid to be communicated by the guinea-pig, to be ſomewhat exaggerated, and as the reſult of their experiments was almoſt the ſame, we think we may ſafely take the average number as a rule, ſince the difference probably proceeded from the different conſtruction of the inſtruments employed, and we conceive it to be ſufficiently demonſtrated, that when equal quantities

* Memoire ſur la chaleur, lu 1783.

† An inſtrument which meaſures the degree of heat by the melting of ice, and is deſcribed in Lavoiſier's Elements of Chemiſtry.

tities of air are vitiated, whether by refpiration or the combuftion of coal, nearly equal quantities of heat are generated.

As the quantity of heat generated when a given quantity of fixed air is produced by the combuftion of coal, has been lately demonftrated by Lavoifier *; and as the quantity of air ufually refpired has been afcertained, it is evident that the quantity of heat (*F*) generated in the

* Elements of Chemiftry, p. 101.

(*F*) The experiments of Monf. Lavoifier prove that $\frac{4}{5}$ of the oxygen which difappears in refpiration are confumed in the formation of carbonic acid gas by the combination of oxygen with the carbone of the blood, the remaining $\frac{1}{5}$ is either abforbed by the blood, or is expended in the formation of water with the hydrogene of the blood; the latter opinion feems probable to Monf. Seguin, (Vid. Médicine eclairée par les fciences phyfiques, tom. 1.) who thinks that the arterial blood becomes venous in the extremities of the arteries by abforbing hydrogene, and that vice verfa the venous blood becomes arterial by giving out its hydrogene in the lungs; this hydrogene he imagines to hold a confiderable quantity of carbone in folution. Monf. Seguin's opinion of the caufe of animal heat fcarce differs from Dr Crawford's; oxygen gas he thinks is decompofed in the lungs, on the lungs, in any given time, could be alſo found out, were we able to eſtimate the quantity of fixed air, in air which has been once breathed. The following experiments were made with this view.

EXPERIMENT I.

A quantity of air only once reſpired, was preſſed out of the allantoid E, by means of the curved tube *a b c*, in which there is a cock at *d*, into the bottle F G, till it was filled. Leſt any part of the fixed air ſhould have been abſorbed by the water, ſome oil was poured into the bottle before the air was introduced. The bottle was then removed from the veſſel A C, and

on account of the affinity of carbonated hydrogene for oxygene being greater than that of oxygene for caloric, and of carbonated hydrogene for the blood; water and carbonic acid gas are formed; caloric conſequently given out, which unites with venous blood, the capacity of which for heat is increaſed by loſing its carbonated hydrogene, and thus venous blood becomes arterial.

and was inverted into a veſſel, fig. 6, filled with cauſtic alkali; the barometer was in the mean time attended to, and the degree of heat marked by the thermometer, fig. 7. The air in the bottle was ſhaken, and left in contact with the cauſtic alkali till all the fixed air was abſorbed. Then the bottle was put into the veſſel A C, and plunged into the water, till the cauſtic alkali in the bottle, and the water in the veſſel were at the ſame height. Then having ſtopped the bottle with a cork, and placing it on its bottom, it contained a quantity of cauſtic alkali, which accurately weighed gave the quantity of fixed air abſorbed, after ſome corrections being made on account of the aſcent and deſcent of the mercury in the barometer, and the difference of temperature in the bottle.

Thus the bottle contained 2038.5 cubic inches; the temperature of the air of the bottle was 59° of Fahrenheit's thermometer, the altitude of the mercury in the barometer was 29.87 inches; the temperature of the air after two

days,

days, was 57.5°; the altitude of the mercury in the barometer was 29.37 inches; the cauftic alkali of the bottle weighed 5 lb. $9\frac{1}{3}$ oz. equal to 131.2713 cubic inches. But on account of the afcent of the barometer, it is neceffary to fubtract 20.9 cubic inches; and 6.471 cubic inches, becaufe the air in the bottle was rendered 1.5° of Fahrenheit's thermometer, colder.

For as the volume of elaftic fluids is in the inverfe proportion of the weight preffing on them, $29.37 : 29.07 = 2038.5 : x = 2017.6$; and $2038.5 - 2017.6 = 20.9$.

And as atmofpheric air is expanded by $\frac{1}{472.5}$ of its volume for every degree of Fahrenheit's thermometer, $\frac{2038.5}{472.5} \times 1.5° = 6.471$. So $131.2713 - 20.9 - 6.471 = 103.9$, the number of cubic inches of fixed air in 2038.5 inches of air once refpired; a quantity lefs than $\frac{1}{19}$th of the whole; for $\frac{2038.5}{103.9} = 19.6$.

But leaft any part of the fixed air fhould be abforbed, oil was ufed inftead of water.

EXPERIMENT II.

The bottle A, fig. 6, was filled with air once refpired and paffed through oil: A contains 179.812 cubic inches.

The thermometer and barometer were as in the laft experiment. The cauftic alkali after two days, weighed $6\frac{1}{20}$ ounces, and meafured 11.451 cubic inches. But 29.37 : 29.07 = 179.812 : x = 177.973. And 179.812 — 177.973 = 1.839; and $\frac{179}{472} : \frac{812}{5} \times 1.5^{\circ} = 0.5707$.

Thus 11.451 — 1.839 — 0.5707 = 9.042, the quantity of fixed air found in 179.8 cubic inches of air once refpired; for $\frac{17}{9}.\frac{9.812}{042} = 19.8$.

The fame experiment being repeated with the fame bottle, 3 oz. and 1 drachm only of cauftic alkali, meafuring 5.9148 cubic inches, were found in the bottle. But as during this time the mercury in the bottle has fallen the $\frac{6}{10}$ of an inch, 3.817 cubic inches muft be added to that quantity. And as the air in the bottle

bottle was found $\frac{8}{10}$ of a degree colder towards the end than at the beginning of the experiment, 0.3044 parts of an inch are to be subtracted from it.

For 29.2 : 29.82 = 179.812 : x = 183.629; and 183.629—179.812 = 3.817; and $\frac{179}{471}$. $\frac{8}{1}\times 0.8$ = 0.3044.

So that 5.9148 + 3.817 — 0.3044 = 9.427.

And $\frac{179}{9}$: $\frac{812}{427}$ = 19.07.

In frequently repeating those experiments through the large and small bottles, whilst the state of the thermometer and barometer were carefully attended to, the greatest variation was 20.1. So that the average number will be 19.6.

Some experiments were also made with a view of discovering the quantity of fixed air in the room where these experiments were made; but the quantity of fixed air in 2038.5 cubic inches of air respired, was found so small as to be scarce perceptible. If any calculation be ventured on, the quantity of fixed air may be eſtimated at the $\frac{1}{10}$ or $\frac{5}{100}$ part of air once reſpired.

SECT. IV.

THUS, if the quantity of air commonly inſpired, be eſtimated at 40 cubic inches, and the number of reſpirations at 18 in a minute, 720 cubic inches will be inſpired in the ſpace of a minute; of which quantity only the $\frac{27}{100}$ or 194.4 cubic inches conſiſt of vital air, the only conſtituent of atmoſpheric air changed by reſpiration. But the $\frac{5}{100}$ parts only of atmoſpheric air are changed in each reſpiration. Hence 36 cubic inches of fixed air are generated in the ſpace of a minute in the lungs of a middle-ſized man, or 51840 cubic inches in the ſpace of a day; a quantity of air weighing 22865.5 grains, or 3.9697 lb. troy weight. And as the celebrated Lavoiſier has accurately calculated, that for every pound of fixed air generated by the

the combuſtion of coal, a quantity of heat was evolved which would melt 27.02024. lb. of ice; and as the ſame quantity of heat is generated in air, vitiated either by reſpiration or by the combuſtion of coal, it follows, that the quantity of air vitiated daily in the lungs of an ordinary man, will give out nearly as much heat as would melt 107.2 lb. of ice. For 27.02024 $\times$ 3.9697=107.2622. But as a portion of this heat is carried off in the air expired under the form of ſenſible heat, and as a portion is employed in the formation of vapour, or is rendered latent, theſe quantities can be calculated in the following manner.

As a cubic * inch of atmoſpheric air weighs 0.32112 parts of a grain, 40 cubic inches will weigh 12.8448 grains. But the $\frac{5}{100}$ parts of this quantity being converted into fixed air, if the whole remains free from moiſture, it will gain 0.19794, ſo that it will weigh 13.04274 grains.

* Lavoiſier's Elements of Chemiſtry.

grains. Hence the air expired during the ſpace of a minute will weigh 234.7693 grains, or that expired during an entire day will weigh 338067.82 grains, or 58.692 lb. But if air of this kind be ſuppoſed to have the ſame capacity for * receiving heat as water, it follows that the ſame degree of heat which would raiſe 58.69 lb. of water ſome degrees, would alſo raiſe the air to the ſame degree. But the quantity of heat requiſite to raiſe 58.69 lb. of water to the 66th degree of Fahrenheit's thermometer, is equal to that which would melt 27.6692 lb. of ice; for ſince 140 degrees of heat become latent in the formation of each pound of water, $\frac{58.69 \times 66^{\circ}}{140} = 27.6692$.

Conſequently the ſame degree of heat will be daily evolved in the lungs under the form of ſenſible heat.

The important diſcovery of latent heat ſhews that a great quantity of heat is abſorbed during the

† Crawford ſuppoſes it to have leſs capacity for heat.

the formation of vapour, without an increafe of temperature. And it appears from the experiments of Mr. Watt, that the heat thus abforbed or rendered latent, would raife the temperature of a body of the fame fpecific gravity and capacity for heat as water, although it could not be converted into vapour, 960 degrees more than before.

I weighed accurately a large allantoid, and then fixed it on the above-mentioned machine; the allantoid being empty, I filled it, by means of the machine, with air expired. After the allantoid being left fome time to cool, and then carefully weighed, it was found to gain two grains by every expiration; which coincides remarkably with an experiment made by Dr. Hales, who when he had breathed through fome moiftened diaphragms, found they gained fix grains in the fpace of three minutes*. Although in the laft experiment, in which he breathed

* Vegetable Statics, vol. 1. p. 268.

breathed through burning coals, which abſorbed the fixed air of the lungs and the moiſture, he concludes the humidity to have been greater. Thus if you ſubtract two grains of aqueous vapour every minute, without attending to what may be ſubtracted on account of the difference between the weight of fixed and vital air, you will ſubtract 6 oz. or 0.5 of a lb. daily. But if the temperature of a body incapable of being evaporated, of the ſame capacity for heat as water, weighing 0.5, be increaſed 960°, this heat will diſſolve 3.42854 lb. of ice. For $\frac{960 \times 0.5}{140} = 3.4285$. But as Dr. Crawford computes the relative heat of aqueous vapour at 1.55, a quantity of heat will be ſubtracted which would diſſolve 1 lb. and 0.8856 hundredth parts of ice more than if its capacity for heat had been the ſame with that of water; or in other words, all the heat daily conſumed in the lungs in the formation of aqueous vapour, would diſſolve 5.3141 lb. of ice. So that all the heat, daily evolved in the lungs, under

under the form of fenfible heat and vapour, would diffolve 32.9833 lb. of ice. For 27.6692+5.3141=32.9833.

But as it was before demonftrated, that in the fpace of a day a quantity of heat was evolved, by the change induced in the air, in the lungs, which would diffolve

	107.2622	lb. of ice.
Subtract from thence	32.9833	
There will remain	74.2789	lb. of ice, which

would be the quantity capable of being diffolved by the heat daily evolved in the lungs of an ordinary man. But it is neceffary the blood fhould abforb the heat, becaufe it is expofed to its action in the lungs for the fpace of fome hundreds of fquare feet *, and becaufe this fluid is admirably calculated to diffufe heat through all the body.

The degree to which the blood is heated in its

* Hale's Statical Effays, vol. 1. p. 239, and the celebrated Dr Monro's lectures.

its paſſage through the lungs, may be eſtimated in the following manner. For as the quantity of blood, which paſſes through the lungs in the ſpace of a minute, may be eſtimated at 8 lb. if we ſuppoſe the heart to propel an ounce and a half in each ſyſtole; and if we ſuppoſe blood to have an equal capacity for heat with water, it is evident that the quantity of heat which would make 8 lb. of water riſe to any given point, would alſo make 8 lb. of blood riſe to the ſame point. But as the quantity of heat abſorbed during the ſpace of a day by the blood, has been demonſtrated ſufficient to diſſolve 74.27 lb. of ice; the quantity abſorbed each minute could only diſſolve the 0.05158 of a lb. of ice. Beſides, ſince 140° of heat are requiſite to liquefy 1 lb. of ice, or become latent, $1 : 140 = 0.05158 : x = 7.22°$, or the quantity of heat requiſite for diſſolving 0.05158 of a lb. of ice, would increaſe the temperature of a lb. of water by 7.22°, or 8 lb. by 0.90265 of a degree of the thermometer of Fahrenheit. Thus the

the heat generated each minute in the lungs of an ordinary man, would raiſe the temperature of the blood paſſing through them in that ſpace of time, by 0.90265 of a degree, if blood had an equal capacity for heat with water.

But as Dr. Crawford concludes that the (V) comparative heat of venous blood bears the ſame proportion to that of water as 0.8928 to 1, it is evident, that the ſame degree of heat which would raiſe venous blood one degree, would raiſe the ſame quantity of water only 0.8928 of a degree, or that their temperatures are in the inverſe proportion of their capacities for heat.

Thus the quantity of heat which would raiſe 8 lb. of water 0.90265 of a degree, would raiſe

H the

(*V*) Dr Crawford's experiments prove that the blood which paſſes from the lungs to the heart by the pulmonary vein, contains more abſolute heat than that which paſſes from the heart to the lungs by the pulmonary artery. This amounts to a demonſtration that the lungs are the ſource of animal heat.

the ſame quantity of venous blood 1.01103°; for $0.8928 : 1.0000 = 0.90265 : \frac{.90265 \times 1}{0.8928} = 1.01103$.

And as he lays down, that the capacity of venous blood for heat is increaſed by its becoming arterial, in the proportion of 0.8928 to 1.03; if venous blood is raiſed 1.01103° by the heat evolved in the lungs, it follows that arterial blood will be raiſed only 0.8763: for as $1.03 : 0.8928 = 1.01103 : x = 0.8763$.

Thus 0.13468 parts of a degree of Fahrenheit's thermometer, equal the quantity of heat which becomes latent, although the capacity of the blood for heat is increaſed in the ſame proportion; for $1.01103 - 0.87635 = 0.13468$.

Therefore the blood, in its paſſage through the lungs, gains 1.01103, or more than one degree of Fahrenheit's thermometer, and its temperature is increaſed 0.8763 of a degree. From whence it follows that the blood in the left ſide of the heart is warmer than that in the right by $\frac{8}{10}$ of a degree. This ſeems confirmed by an experiment

experiment of Hunter's on a live dog, which I ſhall give in his own words.

" The ball of a thermometer being introdu-
" ed two inches within the rectum, the quick-
" ſilver roſe to 100° and a half exactly. The
" cheſt of the dog was then opened, and a wound
" made into the right ventricle of the heart,
" and immediately, on the ball being introdu-
" ced, the quickſilver roſe to 101° exactly.
" A wound was next made ſome way into the
" ſubſtance of the liver; and, the ball being
" introduced, the quickſilver roſe to 100° and
" three quarters. It was next introduced into
" the cavity of the ſtomach, where it ſtood at
" 101°. All theſe experiments were made in
" the ſpace of a few minutes.*"

SECT.

† Animal œconomy, p. 102.

SECT. V.

THUS it has been ſhewn, not only that animal heat is generated in the lungs, but that the quantity ſo generated can be determined by a method, which has no connexion with any theory on animal heat, nor with the different capacities of fixed and vital air for heat.

This method is founded on the two following propoſitions, which we preſume, have been demonſtrated by our experiments.

1ſt, That nearly equal quantities of heat are evolved, when equal quantities of vital air are vitiated whether by the combuſtion of coal or by the reſpiration of animals.

2dly, That the quantity of fixed air generated in the lungs in any given time, can be eaſily determined by knowing the quantity of fixed air in air once reſpired.

But if the quantity of air commonly refpired had been fo fmall as Dr Goodwin had fuppofed, it is evident, that fo fmall a proportion of it would have been changed in the lungs, that this organ could not be confidered the fource of animal heat. And in fact, feveral objections were made to Dr Crawford's theory on account of the experiments of Dr Goodwin, and fome others made by the celebrated De la Metherie, who eftimates the quantity of air commonly infpired at 8 or 10 cubic inches, and fuppofes therefore, that not more than ½ an inch of fixed air is generated.

But as Monf. De la Metherie meafured only one refpiration, and that without much accuracy, there is no neceffity of dwelling any longer on this topic. But from the above experiments and calculations we neceffarily conclude, that the quantity of heat generated in the lungs is fufficient to compenfate for its continual lofs. We cannot therefore fufficiently admire the infinite wifdom of the fupreme Being,

who

who has made heat be generated in the lungs from that very element, which draws off heat from every other part of the body. We cannot but admire alſo the diffuſion of heat through the entire ſyſtem by means of the blood. Hence we ſee the reaſon of filling the lungs of drowned perſons with air; whether filling the lungs with air be the moſt efficacious method of reſtoring the proper degree of heat to the vital parts, or whether it be a ſtimulus to the heart; the motion of which perhaps ceaſes in a great meaſure from the loſs of heat. This is rendered probable, not only by the blood being hotter in the lungs, and therefore in the left ſide of the heart, than in other parts of the body; but alſo by an experiment of the ingenious Dr Gardiner, which I ſhall give in his own words. " Some years ago, I cut out " the heart and part of the large veſſels of a " turtle, with a view to examine the ſtructure " of the parts, and the circulation of the blood " in that animal. Having wiped off the blood " and

" and other moiſture, the heart was wrapped
" up in a handkerchief; but engagements in
" the way of my profeſſion obliged me to poſt-
" pone my curioſity till about 6 or 7 hours af-
" ter it was cut out. When I examined it,
" there appeared not the leaſt ſigns of life. It
" was much ſhrivelled and dried. But, upon
" putting it in water nearly milk warm, it
" plumped up, and ſome parts of it acquired a
" tremulous motion. Laying it on the table,
" and pricking it with a large needle, it palpi-
" tated ſeveral times. The palpitation renew-
" ed as often as the needle was puſhed into its
" ſubſtance, until it became cold, when it
" ſeemed to be inſenſible to every ſtimulus.
" But after warming it again in water, it reco-
" vered its irritability, and repeated its palpita-
" tions on the application of the needle.
" Though no motion could be excited in it by
" any ſtimulus when cold, yet it moved ſeveral
" times after being macerated in warm water.
" This evidently ſhews the neceſſity of heat

" and moiſture for maintaining the full powers
" of the living principle *.

As the air may be made to act on blood drawn out of the body and quite cold, it is probable that heat may be generated in the lungs of men who had been a long time immerſed in water.

As in ſome inſtances, men who had been only a ſhort time (*N*) under water, or expoſed to

* Animal œconomy, p. 46.

(*N*) Mr Coleman differs widely from Dr Goodwin about the cauſe of death from ſuſpended reſpiration: According to his opinion, the effects are nearly the ſame whether the animal periſh from hanging, drowning, or breathing a noxious gas; in all theſe caſes the lungs were found collapſed, and the proportion of blood in the right ſide of the heart was to that of the left, as 13 to 7. It is worthy of remark, that the heart was much leſs irritable in thoſe who periſhed in any of the noxious gaſſes, than in thoſe who periſhed from hanging or drowning.

The proximate cauſe of death from hanging, drowning, or reſpiring noxious gaſſes, he ſuppoſes to be an obſtruction in the minute pulmonary veſſels, ariſing from a collapſe of the lungs: This, he thinks, happens in every inſtance of death from any of theſe three cauſes.

Although Mr Coleman's experiments are numerous and highly ingenious, yet Dr Goodwin's opinions on the ſubject are more generally adopted, particularly in this Univerſity.

to mephitic air, did not recover, though every attempt was made of inflating their lungs in the ufual manner; and as it appeared probable that their death was owing to a fmall quantity of water or mephitic air, which had infinuated itfelf into the pulmonary vesicles, fo as not to be got out by the common method; would it not be proper, in fuch a dreadful inftance, to make one fide of the lungs collapfe by a puncture into the thorax? For thus the water or mephitic air would be expelled by a few inflations, and pure air would be clofely applied to the pulmonary vesicles. It is alfo evident, that little danger would occur from the air being admitted between the lungs and pleura, if we took care to force it out, as appears from many cafes of emphyfema. We muft not however indulge in conjectures, when it is poffible to afcertain the fact by experiment: as in this entire differtation we have cautioufly avoided theory, except it was of manifeft advantage. Many corallaries may be deduced from the above-mentioned calculations, ſuch as the increaſed heat of the body from exerciſe, and whatever propels a greater quantity of blood through the heart in a given time. But as we have already tranſgreſſed the bounds preſcribed for this kind of diſſertations, we ſhall ſay no more on the ſubject.

F I N I S.

E R R A T A.

Page 30. line 3. of the 2d Note, *loco* $\frac{40}{4725}\times 40^{\circ}$, *read* $\frac{40}{472.5}$ $\times 40^{\circ}$.

48. 9. *loco* $\frac{8}{5}{}^{12}$ *read* $\frac{8\,12}{5}$.

12. *loco* $\frac{17}{9}\cdot\frac{9}{0}\cdot\frac{8\,12}{42}=19.8$, *read* $\frac{179.812}{9.042}=19.8$.

49. 5. *loco* $\frac{179}{472}\cdot\frac{8}{5}\times 0.8$, &c. *read* $\frac{179}{472}:\frac{8}{5}\times 0.8$.

50. 2. *read* may be eſtimated at $\frac{1}{20}$ or $\frac{5}{100}$ part of air once reſpired.

52. 13. *loco* 276692 lb. of ice, *read* 27.6692 lb. of ice.

58. line the laſt, *read* by $\frac{8}{10}$ of a degree.

Speedily will be Published,

A

PHYSIOLOGICAL DISSERTATION

ON THE

FUNCTIONS OF THE PLACENTA.

Translated from the Latin of DR. JEFFRAY.

" From these Facts and Observations we may infer, that the
" Placenta is an organ for giving due oxigenation to the
" Blood of the Fœtus; which is more necessary, or at least
" more frequently necessary, than even the supply of Food."

ZOONOMIA, Pag. 477.

EDINBURGH—*MDCCXCVI.*