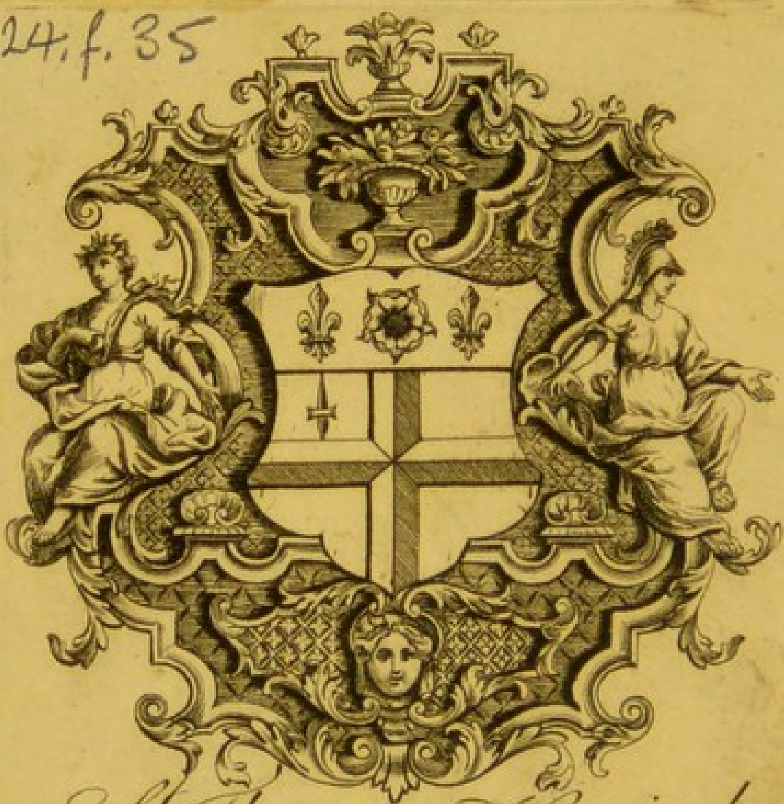

A DISSERTATION ON NATURAL AND SUSPENDED RESPIRATION.

By EDWARD COLEMAN,

PROFESSOR OF THE VETERINARY COLLEGE,
VETERINARY SURGEON GENERAL TO HIS MAJESTY'S
CAVALRY, AND MOST HONOURABLE BOARD
OF ORDNANCE, &c.

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1802.

DEDICATION.

To HENRY CLINE, Esq.

LECTURER ON ANATOMY, AND SURGEON TO ST. THOMAS'S HOSPITAL.

DEAR SIR,

THAT diſtinguiſhed eminence you have ſo deſervedly attained in the medical world, and that gratitude you might ſo juſtly claim from all your pupils, particularly from one who is indebted for his chirurgical and phyſiological knowledge, not only to your public but private inſtructions; would alone prove ſufficient inducements for me to addreſs theſe firſt-fruits of my profeſſional ſtudies to you.

But, however powerful theſe motives, allow me to add, there is another yet more cogent, and which flows more immediately from the heart—

That friendſhip with which you honoured me while reſident under your roof, and which you have kindly continued ſince I quitted that hoſpitable manſion, to enter the buſy ſcenes of life, will for ever live in my recollection, and awake the moſt grateful emotions of a feeling mind.

Permit me then to hope you will receive this Dedication as a ſmall but ſincere teſtimony of that ſenſe I entertain of

your

your eſteem; to merit and to enjoy which, to the lateſt period of my exiſtence, is the higheſt ambition of,

Dear Sir,

your much obliged

and moſt humble ſervant,

EDWARD COLEMAN.

Veterinary College.
April 20, 1802.

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INTRODUCTION.

OF all the exertions of human ſkill, there is, perhaps, none which affords more ſolid and laſting gratification, than the reſtoring to life thoſe who are apparently dead; none, ſurely, more eminently ſhews the dignity and fruitfulneſs of Philoſophy, or more clearly evinces the benefits that may be derived from the well-directed efforts of human underſtanding.

This art (if ſuch it might be called in ſo rude a condition) was, in former ages, guided chiefly by blind prejudice; the knowledge of the animal œconomy, and of life, was not ſufficiently extended, to afford maxims of any value to the practitioner; and the caufes of death were too incorrectly marked, to fhew, with any degree of precifion, the means of recovery.

Accidental recoveries had, indeed, fhewn that it was practicable; but Phyfiological fcience was unable to explain or prefcribe the mode. It was referved for the eighteenth century, to exhibit, on a large fcale, any practical fpecimens of this mode of benevolence, and to approach, in fome refpect, to the fcientific folution of thofe principles by which it muft be guided. Many focieties were formed on the continent of Europe, for the purpofe of promoting this kind of knowledge; and their reports afforded the moft mortifying reply to thofe who had declaimed with fuch triumph on the vanity of natural fcience, and the impotence of human art. Their multiplied fucceffes, in fo untried a path, awakened a general

general ardour on this ſubject, which was not a little foſtered by a cotemporary improvement in natural knowledge: I allude to the philoſophy of elaſtic fluids, which has, during the laſt part of the preſent century, received ſuch incredible acceſſions. The doctrine of *airs* was ſo intimately connected with the ſubject of reſpiration, that it could not fail to fix the attention of Philoſophers on thoſe caſes where its ſudden ſuſpenſion was the cauſe of death. It were ſuperfluous to enumerate the various theories offered by the Chymiſts and Phyſiologiſts of this recent period. Suffice it to remark, that the Humane Society of London deemed the ſubject ſo perplexed with diſcordant theories, and ſo ſuſceptible of farther experimental elucidation, that they publiſhed, in 1787, a queſtion on *the nature of the diſeaſes produced by ſubmerſion, ſuſpenſion, and noxious airs.* Two diſſertations, of peculiar merit, they honoured with prizes: thofe of Dr. Goodwyn and Mr. Kite. The fame enlightened and benevolent body purfued this inquiry, by propofing a queftion—"*Whether Emetics,* "*Venefection, or Electricity, be proper in* "*fufpended Animation, and under what* "*Circumftances?*"

To this queftion, I am about, in the following Differtation, to attempt an anfwer. It may be thought, that, as this queftion is purely *practical*, any inveftigation of the *proximate caufe* of the malady is fuperfluous and impertinent, and that our views ought to be limited to the remedies employed in its cure; or it may, perhaps, be fuppofed, that fuch inquiry is precluded by the fuccefsful labours of Dr. Goodwyn and Mr. Kite: but reafon, which forbids us to abandon any

any thing fo important to blind empiricifm; the example of thefe Gentlemen, who had from their pathology deduced their cure, and the repugnance of their inferences to each other, which countenanced a doubt refpecting the accuracy of either,—feemed to prove the neceffity of reinveftigating, by experiment, the nature and caufes of the difeafe, previous to the delineation of any plan of cure. One of thefe Gentlemen attributes death in thefe cafes to the quality of the blood in the left fide of the heart, which has not received from the air, that ftimulant power which fupports the action of that organ. The other attributes it to apoplexy. I was induced, fince the appearance of thefe effays, to attempt a feries of experiments on the fubject, which perhaps I fhould not have cultivated with fo much ardour, had I not been animated by the example of Mr. Kite, from whom I received the rudiments of my medical education, and for whom, in combating his opinions, I truſt I ſhall not be deficient in that reſpect which his talents demand. Theſe experiments preſented reſults which contradicted, in many important particulars, received opinions: but I ſhould not, at ſo early a period of my life, have preſumed to offer them to the public, had I not been emboldened by the approbation of the Medical Society of London, who voted me the Humane Society's Medal.

Dr. Goodwyn has juſtly and ingeniouſly remarked, that the expreſſion, "Suſpended "Animation," is objectionable. Reſpiration and circulation may be ſuſpended; but the principle of life, or the ſuſceptibility of action, which is the ſource of theſe functions, may ſtill remain. Life, therefore, can with no propriety be ſaid to be ſuſpended, when the vital principle is preſent. The animal muſt either retain the principle of life, or be abſolutely and irrecoverably dead. There is no intermediate ſtate between life and death. The diſtinction between the actions and powers of life, which, with ſo many other admirable obſervations in Phyſiology, we owe to the ingenious Mr. Hunter, clearly illuſtrates the impropriety of the language to which we object. He has proved that in many caſes, theſe powers remain when the actions are ſuſpended. The preſence of theſe powers alone conſtitutes life, and forms the ſole diſtinction between inanimate and animated matter. When they ceaſe to be preſent, life is *not ſuſpended, but deſtroyed.* Inſtead therefore of employing the term Suſpended Animation, we ſhall adopt that of *ſuſpended* *respiration*, which only simply expresses a fact, and is equally applicable to those cases which terminate in death, as to those of which the event is favourable.

The necessity of inflicting a painful death on so many animals will ever be felt by minds of sensibility, as a cruel alloy to the pleasure of Physiological research. By no other mode, however, than that of experiments on living animals, can any important advance be made in this subject. Such experiments, in a question of mere curiosity, are certainly indefensible; but where, as in the present case, the advancement of truth conspires with the interest of humanity, we must impose silence for a while on the remonstrances of sensibility.

In the conduct of the experiments which

which form the basis of the following dissertation the most solicitous accuracy has been every where studiously sought.

To those who are in the habits of Physiological experiment, nothing is more familiar than the perplexing variety and repugnance of their results; two experiments, though made in the same manner on the same order of animals, will rarely in every particular agree; for it is not only true, that different species of animals, but that different individuals of the same species, possess various degrees of irritability. In some, irritability may be excited for several hours after apparent death, others lose it in less than one. The cause, however, of these variations, where they have been in any respect considerable, we have generally discovered to be some accidental and extrinsic circumstance, and by multiplying

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and varying experiment, we have attempted to difcriminate between what is made the foundation of *general* principles, and what is the effect of peculiar and fortuitous circumftances. But the enthufiafm which we acquire in the purfuit of a favourite refearch, and our anxiety to fupport a cherifhed opinion, ought ever to make the experimental inquirer diffident of the correctnefs and impartiality of his *own* views. A bias unconfcioufly taints his judgment, againft which the only remedy is, the vigilant eye of acute and intelligent friends, who feel more anxiety for his reputation, than tendernefs for his prejudices; and who have no motives either to make tortured inferences, or to hide unfavourable refults.

The fame good fortune that has bleffed my private life with the friendfhip of fuch men, I have alfo eminently felt in my

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ſcientific purſuits. Their acuteneſs has reſcued me from my prejudices; and their aid has given me a confidence in the correctneſs of the experiments, which diſtruſt in my own individual ſkill would otherwiſe never have permitted me to entertain. I have to mention with particular gratitude, Mr. Aſtley Cooper, whoſe anatomical and phyſiological knowledge needs no comment; and Mr. Keir, a gentleman of diſtinguiſhed ingenuity, who favoured me with his occaſional aſſiſtance. And it affords me no ſmall gratification, that my much reſpected friend, Dr. Haighton, Teacher of Phyſiology, in the Borough, has made many experiments which corroborate moſt of the opinions here advanced.

Though ſubmerſion be the moſt frequent, it is by no means the only caſe of

apparent

apparent death worthy the inquiry of the Phyfiologift, or the attention of the medical practitioner. Nor is the benevolent zeal of the Humane Society confined to it alone; as every cafe of apparent death, arifing from a fudden fufpenfion of refpiration, partakes equally of its bounty; and indeed, agreeable to this extenfive view of the fubject, the queftion before us is propofed.

The fufpenfion of vital action from ftrangulation and noxious airs exhibits phænomena fo nearly fimilar, and requires a treatment fo ftrictly congenial, that any inquiry into the nature of fubmerfion would be narrow and imperfect, unlefs illuftrated by the inveftigation of thefe kindred difeafes. To them, therefore, we have thought it expedient to extend our refearches; and from inductions founded

on

on a ſeries of experiments and obſervations on theſe different modes of death, we flatter ourſelves with the hope of having eſtabliſhed a general doctrine on premiſes leſs ambiguous and unſtable, than thoſe which have been the baſis of former theories.

To aſcertain phænomena is the firſt duty of every inquirer into nature. We ſhall, therefore, preface any inquiry into the nature of ſuſpended breathing, by the Phyſiology of the organs which are its ſeat; thus delineating their natural actions, before we examine their morbid condition. The Phyſiology of the heart and lungs therefore will conſtitute our firſt ſection.

We ſhall then proceed to deſcribe the

phænomena

phænomena of departing life, the appearances on diffection, and to confider that peculiar condition of the animal which forms the *proximate caufe* of the difeafe.

The remaining part, to which the preceding fections are but preliminary, will be devoted to the confideration of the cure: and in order to inveftigate more at length the efficacy of thofe means which have been either fuggefted by fpeculation, or fanctioned by experience, we fhall dedicate a fection to each clafs; by which we fhall be enabled to form a juft eftimate of their comparative efficacy and importance.

Emetics, Venefection, Electricity alone or combined with artificial Refpiration, Warmth,

Warmth, Frictions, and Clyfters, will be fairly examined by the tefts of experiment and of reafon; and our laft fection will confift of conclufions drawn from the whole.

SECTION I.

Phyſiology of the Heart and Lungs.

It is by no means my deſign to extend this inveſtigation to every advantage that reſults from reſpiration, as the voice, ſmell, &c. but merely to take a ſuperficial ſurvey of thoſe functions more immediately connected with *life*.

On this ſubject Dr. Goodwyn has beſtowed no ſmall ſhare of attention; and though the reſult of my obſervations does not permit me to yield aſſent to many of his opinions, yet the reſources of his ingenuity and perſpicuity of arrangement muſt ever claim admiration and applauſe.

But before we inquire into that particular connexion which fubfifts between breathing and life, our firft object is briefly to confider the manner in which refpiration is performed in health.

The expanfion of the thorax in ordinary infpiration is effected by the intercoftal and other mufcles, and its cavity lengthened by the defcent of the diaphragm; but in laborious infpiration, the *ferratus major anticus, pectorales*, &c. bear a confiderable part.

Expiration is faid to be both an active and paffive procefs: it is active when the abdominal mufcles comprefs the vifcera, and draw the ribs downward and backward, or in quadrupeds upward; and paffive, from fome of the mufcles of infpiration at this time relaxing.

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The lungs themfelves are fomewhat elaftic; but are paffive in refpiration. They may not unfitly be compared to a pair of bellows, and the mufcles of refpiration to the power that works them. In their ftate of expanfion, or when the mufcles of infpiration act, the cavity of the cheft is enlarged; but when compreffed, by the action of the mufcles of expiration, the cavity is leffened and the air expelled. Thus, by this alternate dilatation and contraction of the thorax, the procefs of refpiration is fupported.

The action of thefe mufcles in a ftate of health is involuntary, and is lefs influenced by the will than moft of the other mufcles in the body: we are able, however, for a fhort interval, to check or increafe their action, but that it fhould not be wholly fubfervient to the will, is very wifely ordained; for otherwiſe the powers of reſpiration muſt ceaſe whenever the ſenſes are ſuſpended by ſleep or inſanity.

It has been generally ſuppoſed that one of the natural functions annexed to the lungs was that of aſſiſting, by their alternate dilatation and contraction, in propelling the blood from the right to the left ſide of the heart; but in health they ſeem to poſſeſs no ſuch power; for if circulation depended on their mechanical action, ſuſpending the breath for one twentieth part of a minute would produce a ceſſation of the heart's motion, and the heart of the human ſubject would then have but one pulſation to one expiration, whereas in health we have four. Some animals have ſix pulſations to one expiration; the horſe in ordinary breathing has only two.

It is therefore very probable, that the pulſation of the heart is not governed by the action of the lungs in natural reſpiration; and from an experiment in the next ſection it will appear that the right ſide of the heart, unaſſiſted by the action of the lungs, is capable of ſending blood to the left, even after the reſpiration has ceaſed. If then the heart, in a ſtate of debility, can perform this function independent of the lungs, can it be ſuppoſed unequal to it in the vigour of health? Groundleſs therefore is the ſuppoſition that attributes this office to the lungs in ordinary reſpiration.

But a ſubject more delicate and abſtruſe, a ſubject that of late years has been warmly and ably controverted, now claims our attention; I mean the alteration induced on the blood in the lungs; the cauſe on which this alteration depends; and the effect it produces on the animal œconomy.

To inveſtigate the peculiar change which the air may undergo in the lungs, is not of much conſequence to our preſent inquiry; but before we can obtain any knowledge of the *proximate cauſe* of death in ſuſpended reſpiration, it is abſolutely neceſſary to trace and aſcertain the effects produced by the air on the blood.

We are inclined to the opinion of the late celebrated Dr. Crawford, that a principal advantage derived from reſpiration is *animal heat*; that when the blood returns from all parts of the body to the lungs, it has loſt a quantity of its latent heat*, and imbibed ſome

* According to Locke's definition, heat is a ſenſible quality; and if this definition be admitted, then, properly ſpeaking, there can be no ſuch thing as latent heat, as that

ſome noxious quality; that in the lungs it meets with atmoſpheric air, containing a portion of oxygene, which is known to poſſeſs heat in a latent form; that it abſorbs part of this heat, and at the ſame time imparts to the air which remains its impurity.

That the blood having thus robbed the air contained in the lungs of a portion of its latent heat, and rendered that which remained ſenſibly warm, the air is expelled, and freſh air taken in to undergo a ſimilar proceſs.

Dr. Crawford, in the courſe of his experiments, had occaſion to obſerve that animals immerſed in a warm, did not ſo ſoon con-

that muſt ceaſe to be heat when once it becomes inſenſible; but as the term appears to convey the idea we wiſh, that of a principle or quality exiſting in a body which cannot be meaſured, but under certain circumſtances can produce ſenſible heat, we have preferred it to others.

ſume a given quantity of oxygene as thoſe immerſed in a cold medium: nor is the reaſon inevident; for when the blood arrived at the lungs, it had not loſt ſo much of its temperature, conſequently did not require to rob the air of ſo much of its purity; whereas, in the other caſe, the animals being immerſed in the cold medium, were obliged to generate more heat: but to effect this, they muſt abſorb a greater quantity of oxygene than thoſe in the warm medium. It is alſo obſerved, by the ſame author, that the difference between the colour of the venous and arterial blood was diminiſhed by expoſing animals to heat, and increaſed by expoſing them to cold.

The object of theſe experiments was to prove, that in proportion as the atmoſphere is cold, more or leſs fire is abſorbed from the air, to keep up an equilibrium of heat;

and it is remarkable that the animal in the warm medium died firft, notwithftanding the blood was florid, and the furrounding air more pure than that which the animal breathed when in the cold medium.

The one dying fooner than the other probably depended on debility; that the one in the warm medium, from being obliged to generate cold, or more properly *refift heat*, was rendered weaker than the other, from this procefs being more expenfive to the fyftem than generating heat; for there appears fuch a tonic power in cold, that an animal will allow of its natural heat being diminifhed feveral degrees without inconvenience, but cannot fuffer its fenfible heat to be increafed more than fix degrees at moft of Fahrenheit, without death taking place. Hence it would feem that although the fluids of the one contained more of the

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ſtimulating quality than the other, yet from the ſolids not being ſo ſuſceptible of action, life could not continue ſo long: and it appears probable, that if the animal in the cold medium could have exchanged its blood with that in the warm one, the difference in the duration of life would have been ſtill greater.

The objections adduced againſt Dr. Crawford's truly ingenious theory ſeem to poſſeſs but little weight. It is urged by ſome, that if breathing be the ſource of animal heat, how can it happen that the inhabitants of the northern climates breathe no quicker than thoſe of the ſouthern; and yet nearly the ſame degree of animal heat is preſent in both? The reaſon appears obvious; there is always exiſting in the atmoſphere a much greater proportion of oxygene than is conſumed by any animal

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in one inſpiration; ſo that thoſe in the colder climates, although they breathe no quicker, nor take in a larger volume of air, yet they rob that air of more of its latent fire.

The cold atmoſphere, bulk for bulk, muſt be ſpecifically heavier than the warm, and, weight for weight, the bulk will be leſs; ſo that any given quantity of air, in proportion as the temperature is diminiſhed, its volume muſt decreaſe; and vice verſa. Hence in a cold atmoſphere, although the volume of air taken in at each inſpiration be the ſame, yet in that volume a greater number of particles of air are received into the lungs; and it alſo ſeems probable, that, weight for weight, this atmoſphere ſhould contain more oxygene than the warmer, ſince it is generally allowed that in proportion as its warmth is increaſed, it becomes a better menſtruum for foreign matter of all kinds.

Dr. Crawford ſuppoſed that heat is given out in the capillaries only; but there is reaſon to believe that heat is alſo evolved during the whole of the circulation; for in amputating a limb, where the tourniquet has been for ſome time applied, the firſt blood iſſuing from an artery aſſumes a venous colour; and Mr. Hunter found, from tying up the carotid artery of an animal, that the blood became black; from which it may be concluded that the blood is capable of undergoing the ſame proceſs in the larger arteries as in the ſmaller veſſels. In ordinary circulation, however, the change muſt be leſs in degree, from the circulation being here quicker, and a greater quantity of blood being in contact with fewer ſolids.

It ſeems alſo more than probable that the blood ſtill retains a quantity of fire in a latent form after it has paſſed through the capillaries and entered the veins; for, on applying preſſure to a vein in common bleeding, the longer a ligature is applied the darker the blood becomes; and at the concluſion of the operation its colour aſſumes nearly a florid hue; which corroborates the opinion that it may poſſeſs a conſiderable portion of the latent principle after it has entered the veins; and that this blood is capable of continuing the ſame proceſs, ſo long as it contains any heat to evolve. In fever, the venous blood is ſometimes nearly florid; and Dr. Crawford found that when animals were immerſed in a warm medium, the blood paſſed through the capillaries without undergoing the uſual change: both which circumſtances tend to prove, that the blood contains more or leſs of that principle ab-

ſorbed

ſorbed in the lungs after it has entered the veins; indeed, the circulation in fiſhes appears to decide the queſtion, for the heart of theſe animals is a ſingle one, conſiſting of one auricle and one ventricle, both of which contract from the ſtimulus of black blood; and as the blood in the coronary veſſels is of the ſame quality, its heat and nouriſhment muſt be kept up by that blood only which has paſſed through the capillaries.

Hence it appears, that if this black blood did not poſſeſs a quantity of latent fire, the warmth and life of the heart could not be ſupported: notwithſtanding, therefore, that the blood, when it paſſes through the capillaries, evolves the greateſt part of its heat, yet there ſtill remains a portion of it in a latent ſtate even after it has entered the right ſide of the heart: and however inconſiderable this may be, yet if it is equal to the

the demand, the temperature of the whole animal muſt be the ſame. With a view to aſcertain the comparative temperature of arterial and venous blood the following experiment was made.

EXPERIMENT.

A Dog was hanged, the ſternum immediately removed, and the lungs inflated until the blood in the left auricle became *florid.*

The contractions of the whole heart at this time were powerful; and Mr. Hunter's thermometer being raiſed to 98° was introduced through an opening in the pericardium, and placed on the right ſide; the mercury roſe to 99° and then became ſtationary; it was removed to the left, and the temperature was the ſame; but on making an aperture into the left auricle, and thruſting the bulb down to the ventricle, the mercury fell to 97°; and on placing it in the ſame manner within the right ventricle, it roſe above 98°.

From frequent repetitions of this experiment it uniformly reſulted that, although the temperature of both ſides of the heart externally was equal, yet the heat of the blood in the right ſide exceeded that of the left, from one to two degrees.

This obſervation may appear rather ſtrange, and at firſt ſeems to contradict the opinion that reſpiration is the ſource of animal heat; but the fact can be readily explained; for the blood, in its paſſage through the lungs, being contained in veſſels that are in contact with air ſo much below its own temperature

temperature, the colder body muſt rob the warmer of ſo much ſenſible heat as is neceſſary to make them nearly equal; and the temperature of the ſubſtance of the left auricle and ventricle is kept up above that of its contents, and equal to that of the right ſide from the heat evolved by the blood in the coronary veſſels; but as the ſenſible heat of the blood in its paſſage through the lungs is only ſlightly diminiſhed, its latent heat muſt be conſiderably increaſed. Indeed when it is conſidered that the blood veſſels in their paſſage through the lungs are always expoſed to the air inſpired, and that the heat of this air is often increaſed or diminiſhed 40° of Fahrenheit, without altering the warmth of the blood even 3°, can there be a doubt but that the blood which warms the air muſt in the lungs *receive a freſh ſupply* of heat? If the pulmonary veſſels were diſtended

with

with water (inftead of blood) at 98° of heat, and the air infpired was only 32°, the contents of the pulmonary veins and left auricle would be much below 98°, whereas the temperature of this blood, whether the air be warm or cold, is generally uniform. The atmofphere at 72°, or at 32°, would make a moft material difference on water at 98°, or any other fluid but living blood. Hence it follows, that while the blood in the lungs is parting with its fenfible heat to the air, it is abforbing, in a concealed form, latent fire.

It has moreover been found that when the change has taken place, the blood in the left fide of the heart retains its heat longer than that in the right, though at firft its temperature be fomewhat inferior.

To eftablifh the fact the following experiment was made.

EXPERIMENT.

A cat was ftrangled, the cheft immediately opened, and the lungs inflated, when the blood in the left fide of the heart became florid; an aperture was then made in the pericardium, and the mercury of a thermometer being raifed to 99°, the temperature both of the right and left fides of the heart was exactly 98°: on opening the left and introducing the thermometer, as in the laft experiment, it fell below 97°; but on examining the right internally, it rofe to near 99°.

So far does this experiment agree with our laft; but the temperature of the blood was re-examined fifteen minutes after, and instead of the right possessing two degrees of heat more than the left, it was found, on the contrary, that the right had four degrees less than the left.

This experiment has been repeated by Mr. Astley Cooper, and in different ways, but the result has been invariably the same; that although the venous blood was superior in temperature at first, yet before coagulation was complete, the arterial became from three to six degrees warmer; this, or nothing, affords a proof, that heat is received by the blood from breathing; for if that blood which has passed through the lungs, is at first inferior in temperature, and soon after becomes superior; from what can this variation arise but the heat received from the air in a latent form, and evolved in a sensible one?

We ſcarcely know of any animal, on whoſe blood the air does not, either directly or indirectly, induce ſome change; and the great object of this change appears to be the ſupport of animal heat; and from the heat evolved the *irritability** of the animal is ſupported.

There are animals whoſe atmoſphere is equal to their own heat; and it has been the opinion of ſome phyſiologiſts that in theſe inſtances their animal warmth is ſupported by the ſurrounding medium. If this be ever the caſe, it probably is in aſcarides, and other animals of the ſame ſpecies, where the temperature of their medium ſcarcely ever varies; but I ſhould much doubt if this be the œconomy of any animal which is placed in an element ſubject to alterations of

* The term irritability is here employed to expreſs nothing more than a *ſuſceptibility of action.*

temperature. Nature has very wifely ordained that animals fhould poffefs a power of retaining nearly an equal temperature for a time, whether they be expofed to excefs of heat or cold; which feems to fhew that their heat cannot be communicated by external temperature; indeed, if animals had not a fource of heat within themfelves, and yet placed in an element liable to variation, life could not be fuftained.

It requires no great ftrength of argument to prove that animal warmth is not produced by the ftomach. The fimple obfervations that, in fevers, when the fenfible heat is greateft, lefs food is taken into the ftomach than in health, and that the infant, as foon as refpiration commences, and before the ftomach receives any nourifhment, is not lefs warm than the adult, are fufficiently convincing that the ftomach is not

to

to be regarded as the ſource of animal heat.

That mere diſtention is the ordinary ſtimulus that excites the action of the heart, is the opinion embraced by ſome phyſiologiſts. Nor is it indeed improbable that a certain degree of diſtention produced by blood of a due temperature, conſtitutes the principal power which ſtimulates the heart to contract; for this power of reaction, when ſtretched beyond a certain tone, ſeems a property inherent in all muſcular fibres.

Nor do we deny that the heart, when void of blood, and ſeparated from the body, retains this action; but this is not peculiar to the heart alone; muſcles, whoſe natural actions depend on the ſtimulus of the will, possess it likewise, though in an inferior degree.

That the different sides of the heart require different stimuli, and that there is something peculiar in florid blood, which alone is capable of exciting the left side to action, we cannot with Dr. Goodwyn admit.

There are several objections that militate against this opinion. Why should the same fibres, nourished by the same vessels, supplied with nerves from the same source, and *performing the same functions*, be excited to action by different causes? Some of these objections the Doctor is aware of, and attempts to remove them by observing that the animal machine offers instances where muscles of similar structure are put into action by different stimuli; but this is not

saying

ſaying that muſcles *performing the ſame functions*, act from diſſimilar cauſes, which it is neceſſary to prove before any analogy can be eſtabliſhed to favour this hypotheſis.

It is far therefore from being certain that the different ſides of the heart derive their action from different ſtimuli: and let us but examine the fœtal circulation, and it will appear that both ſides of the heart contract from the ſtimulus of blood nearly of the ſame quality; that this blood is not florid in either; for even in the umbilical vein it has undergone but a very imperfect change *, if compared with that induced on the blood which paſſes through the lungs of the adult; moreover, that the greater

* We take it for granted that the old opinion, of there exiſting an actual circulation between the blood veſſels of the mother and child, is now exploded, as numerous experiments have been made to prove the contrary.

part

part of the fœtal blood arrives at the heart without paſſing through the placenta at each circulation: that is, the blood in the heart of the *adult* will receive a complete change in the lungs, before it again returns to the ſame place; whereas, the whole of the blood in the fœtal heart will not go to the placenta, to receive the alteration at each revolution; but by far the major part will be ſent to the trunk, the head, and extremities, and be returned to the two cavæ, without having entered the umbilical arteries.

The blood in the umbilical arteries is ſimilar to that in the trunk of the pulmonary artery of the adult circulation; that is, poſſeſſes but a ſmall proportion of latent heat; whereas that of the mother in the cells of the placenta is pure arterial blood. In the minute branches of the umbilical arteries a change is performed; but *only* ſo much heat is imparted to the fœtal, by the maternal blood, as is neceſſary to reſtore the equilibrium of abſolute * heat. The heat therefore which is received by the fœtal blood will be ſmall, in compariſon with that imbibed by the blood of the adult in the act of reſpiration; as *only* that quantity of heat can be imparted from the maternal to the fœtal blood, as can make both their qualities with reſpect to heat *equal*.

When the fœtal blood has undergone this change, it is returned by the umbilical vein; and part of it will paſs through the ductus venoſus † into the inferior cavæ, and mix with the blood brought from the

* By abſolute heat, we mean the latent and ſenſible heat combined.

† The horſe has no ductus venoſus; the blood therefore in the umbilical vein of this animal muſt all paſs through the liver.

lower

lower extremities; but a greater part will paſs through the vena portarum to go to the liver, where by paſſing through capillaries, it muſt aſſume the venous quality before it arrives at the right auricle; it then unites with the blood ſent from the lower extremities and trunk of the body in the inferior cavæ, and on entering the right auricle it mixes with the ſtream of blood coming from the head and ſuperior extremities, none of which has been to the placenta to receive the change.

The right auricle propels part of this blood (which muſt be dark) into the left, and all the blood that paſſes through the capillaries in the lungs alſo enters the left, ſo that the blood which produces the contraction of the left ſide of the fœtal heart muſt be more completely venous than that of the right, as part of the blood in the left auricle,

auricle, without receiving any ſupply of heat, has paſſed through, and imparted heat to the lungs.

If the quantity of blood conveyed to the placenta by the umbilical arteries, be compared with that ſent to the head, ſuperior and inferior extremities, and trunk, it will be found that not one fifth part of the blood goes to the placenta at each revolution, nor can this blood receive but *half the heat* the maternal blood contains; moreover, as all the blood of the fœtal horſe muſt firſt paſs through the capillaries of the liver before it arrives at the heart; and as that of other animals which does not all paſs through capillaries mixes with venous blood, it is obvious that both ſides of the fœtal heart contract from the ſtimulus of black blood, and that the blood of the left ſide muſt be blacker than that of the right.

From

From the blood in the fœtus receiving a degree of change fo inconfiderable, when compared to that produced in the adult by the fame procefs, a doubt might at firft arife whether in both it was deftined to accomplifh the fame end, the fupport of animal heat, and from thence that of *animal irritability*. But the fœtal circulation, far from invalidating, countenances the opinion which derives animal warmth from the act of refpiration.

The experiments of Dr. Crawford have already enabled us to obferve that the quantity of heat abforbed in breathing is proportional to the temperature of the furrounding medium. The obfervation holds equally good in the fœtal circulation; for as the fœtus is furrounded by the liquor amnii and uterus of the mother, the quantity of heat confumed muft be extremely fmall; and the

the temperature being uniform, there is no occafion for more being abforbed than is neceffary to fupply the confumption of fœtal heat. If the whole of the fœtal blood went to the placenta at each revolution, the heat abforbed would exceed the demand, and produce mifchief; for the power of *refifting* heat muft then be called forth to action; and this in the fœtus is very inconfiderable.

On the adult, nature has wifely beftowed two powers for generating cold; that of evaporation from the furface of the body, and a power independent of this; but the fœtus can only poffefs the latter, as no evaporation can take place from the furface of the body; and as the fœtus is deprived of this power, and as the temperature of its furrounding medium, the liquor amnii, is fo much above that of our atmofphere, if an

equal

equal degree of heat were abſorbed in the fœtal, as in the adult circulation, the animal muſt periſh; ſince the act of reſiſting heat for a few minutes is very diſtreſſing, even where the additional power from perſpiration is preſent, to counteract its deſtructive accumulation. Admirable therefore is the proviſion which nature has made, for maintaining a proper degree of heat, both in the fœtus and adult; the former is placed in a warm medium of uniform temperature, which permits but little heat to be conſumed, and the circulation is ſo regulated as only to allow the abſorption of a ſmall and *limited* quantity of heat; ſo that great powers for reſiſting heat are here unneceſſary.

But in the adult, the varying and changeable temperature of the air makes it neceſſary that more or leſs heat be abſorbed, to correſpond with the variation to which it is

 expoſed.

expofed. We are therefore immerfed in an atmofphere fupplied with fufficient oxygene to anfwer our demand; and by evaporation, from perfpiration, &c. we are enabled to refift heat, fo as to prevent its undue and deftructive accumulation. On the other hand, from the warmer medium which encompaffes the fœtus, we may gather the reafon why a fmaller portion of heat fhould be imbibed, and from this being *limited*, why it ftands in no need of evaporation for the generation of cold.

Were the change induced on the blood during circulation intended *folely as a ftimulus to fupport the action of the left fide of the heart*, as maintained by Dr. Goodwyn, then fhould the alteration produced in the fœtus be equal in degree to that produced in the adult; but that this is not the fact we have already, and we hope not unfuccefsfully, endeavoured to prove; and indeed if this was the intention of nature, it is highly improbable that the connexion between the mother and child ſhould take place at the umbilicus, where a great part of the blood which has been at the placenta of the human ſubject, and the whole of the fœtal blood of the horſe muſt firſt paſs through capillaries before it enters the left auricle, and where its purity in the right ſide of the heart muſt be ſuperior to that in the left. We might ſooner ſuppoſe that the umbilical vein would have terminated in or near the left auricle, to ſupply it with blood thus duly altered, than that the blood contained in the *left ſide of the heart* ſhould be *ſimilar in quality* to that in the umbilical arteries which goes to *receive* the alteration; for, in the fœtal circulation, the vein contains blood that has abſorbed oxygene, but the arteries carry blood that is going to receive it.

If therefore the left ſide of the fœtal heart and the whole of the arterial ſyſtem poſſeſs no ſtimulus but that of black blood; if the pulmonary artery in the adult be excited only by this blood; if, in a word, the heart of fiſhes act on no other blood; is it not obvious (at leaſt as far as induction and analogies can prove) that in the adult alſo venous blood can excite the action of the *left ſide of the heart*, and conſequently that the two ſides of the heart do not require to be ſtimulated by diſſimilar cauſes?

From conſidering that one ſide of the heart in the adult circulation contains black blood, and the other florid; and that in ſuſpended reſpiration the left ſide firſt ceaſes to act, when both contain black blood; Dr. Goodwyn, we preſume, was induced to conclude that venous blood, which ſupports the action of the right ſide, was an unfit ſtimulus to keep up the action of the left.

The obſervations, however, we have ventured in ſupport of the idea, that the whole of the heart owes its action to one and the ſame cauſe, oblige us to withhold our aſſent from that of Dr. Goodwyn.

We have already obſerved that when the blood arrives at the right ſide of the heart, it has loſt the greater part of its latent fire: in health it is to receive a freſh ſupply, and is then propelled into the left ſide of the heart, and through the whole of the circulating ſyſtem, to evolve and diſtribute heat.

In conſequence of this proceſs, the left ſide of the heart and coronary veſſels are ſupplied with blood, which diſtribute heat and

and nouriſhment to the whole of the heart; and in ordinary circulation it is probable that the heart derives its heat principally from the blood in the coronary veſſels; but if the motion of the circulating fluid be checked, or totally ſuſpended, then would the blood in the cavities of the heart continue to undergo the ſame proceſs; at leaſt ſo long as it poſſeſſed any heat in a latent form: for it has already been proved, that if blood be delayed in the larger arteries, it is known to aſſume the ſame change and appearances as when it has paſſed through capillaries. The blood within the coronary veſſels not only ſupplies the left ſide of the heart with heat, but alſo the right; and although the blood in the left ſide of the heart may contain 60 degrees of latent fire, when the right poſſeſſes but ſix; yet if the *ſenſible heat evolved* be only equal to two, their temperatures will be the ſame.

The reſult of multiplied experiments authorizes the aſſertion, that immediately after the action of the left ſide of the heart is increaſed by florid blood, the right alſo becomes equally affected; nor is this effect an unnatural or unexpected conſequence; for as the coronary veſſels ſoon receive this blood, and as theſe veſſels are going to both ſides of the heart, the heat and irritability of both muſt be equally increaſed.

One of the ſtimulating powers that keeps up the *action* of the heart, we have already ſuppoſed to be diſtention; but this muſt ceaſe to act as a ſtimulus whenever the quality of the blood becomes incapable of ſupporting the irritability of the heart, by imparting to it a certain proportion of heat.

Dr.

Dr. Cullen imagined that the heart's continuing to act after breathing had ceaſed aroſe from habit; but were that the caſe, why ſhould the action of the right ſide of the heart outlive that of the left; and why ſhould not this influence of habit extend equally to arteries?

Inflating the lungs ſoon after reſpiration has ceaſed, generally increaſes the action of the heart. This in the firſt inſtance depends on the proceſs of circulation being forcibly carried on and aſſiſted by the neceſſary ſtimulus imparted from the air to the blood of the coronary arteries, which increaſes the living powers of the heart and renders it more ſuſeptible of action.

To this opinion, of the action of the heart proceeding from mechanical ſtimuli, Dr. Goodwyn oppoſes this inference: If it were ſo, ſays the Doctor, any aerial fluid would be then equally effectual. But this is not concluſive; for it is agreed on all ſides, that a change in the blood is neceſſary to the life and uninterrupted action of the heart: and although the introduction of noxious air into the cavities of the heart may prove a ſtimulus to the fibres of the heart, yet if the blood ceaſes to receive the change, the irritability of the heart muſt neceſſarily diminiſh. That it is not to be aſcribed to any change immediately induced on the blood *already in the left auricle* is evident; for the right ſide of the heart muſt be excited to action before the left can receive blood that has undergone the change; as no alteration can be given to the blood contained in the auricle.

Dr. Goodwyn is of a contrary opinion; for he obſerves, " that the contractions of

"the left auricle and ventricle are imme-
"diately effected by the quality of the
"blood paffing into them."

We fhall endeavour in the next fection to demonftrate, by experiment, that no alteration can be produced on the blood in the trunks of the pulmonary veins and left auricle, if the communication be cut off from the right fide of the heart: and it muft be manifeft, that if the blood *already* in the left auricle could receive an immediate change from the air in the lungs, the right, which is equally in contact with them, fhould alfo receive it.

That the right fide of the heart continues to act after the left has ceafed, is a phænomenon that has been noticed by many* phyfiologifts; but few, if any, have attempted to unfold its caufe. Indeed Dr.

Dr. Goodwyn appears to be the only one who has feriously endeavoured to explain its rationale, and attempt its illuftration; and though there is no authority to which we would more gladly refer, yet we cannot here adopt his opinion, *that the left auricle and ventricle firft ceafe to act, from the ineptitude of venous blood to excite their contraction*; and *that this is the immediate caufe that fufpends circulation in drowning, &c.*

But in order to explore the true caufe of this phænomenon, let us once more recollect that the blood, when it arrives at the right fide of the heart, has loft the greater part of its latent heat; that in health it receives this fupply in the lungs; but that, *in fufpended refpiration*, the blood paffes through the minute ramifications of the pulmonary artery into the pulmonary veins, without receiving this neceffary quality,

and

and inftead of *abforbing*, muft *evolve its heat.*

An effential difference thus takes place between the blood of the two fides of the heart; the right is diftended with blood that ftill poffeffes latent fire to evolve; but the left has little blood in quantity, and that little has not received but given out heat, to the whole fubftance of the lungs; and as the blood in the one is furnifhed with more heat to evolve than the other, and as the ftimulus of diftention is greateft at the right fide, the action of the one is continued when no effect is produced on the other.

That, in ordinary circulation, both fides of the heart might derive their heat principally from the blood in the coronary veffels, has already been remarked; but as this blood in fufpended breathing contains little or no latent heat, from having evolved it in the lungs, the heart muſt in that caſe imbibe its heat from the blood contained within the cavities; and that this proceſs can be carried on in them we have already ſhewn, ſo long at leaſt as their blood poſſeſſes latent heat to give out, and while the circulation is retarded or totally ſtopt. From which we conclude, that *if the right ſide of the heart in this diſeaſe poſſeſſed the blood of the left, and the left the blood of the right, the difference of irritability would be reverſed.*

If, however, we have ſucceeded in eſtabliſhing as facts, that when the blood arrives at the right ſide of the heart it ſtill contains a portion of heat in a latent ſtate; that this blood in ſuſpended breathing continues to evolve heat in a ſenſible form; that the inferior degree of irritability in the left ſide depends on the eſſential difference in the quality

quality of its blood from that of the right; that moreover this difference in quality proceeds from that of the left having been robbed of a quantity of its heat in its paſſage through the capillaries of the lungs; if, I ſay, theſe facts can be eſtabliſhed, then the temperature both of the right ſide of the heart, and its contents, ſhould be greater than that of the left in this diſeaſe.

The reſult of the two laſt experiments we have mentioned, allowed us to conclude, that both ſides of the heart externally are of the ſame temperature when the blood has received its due change from the air, though the temperature of this blood thus altered is inferior to that of venous; and though the warmth of the blood of the left ſide be at firſt lower in degree, yet its heat ſoon after becomes predominant.

The next experiment was made, to afcertain the temperature of the two fides of the heart, and their contents ; where no change had been given to the blood.

EXPERIMENT.

A Rabbit was ftrangled, and the cheft being opened, a fmall aperture was made in the pericardium, and a thermometer of Fahrenheit's fcale was applied to the right fide of the heart. The mercury rofe to 96°, where it remained ftationary ; it was then removed to the left, where it fell to 94°. On placing it within the cavity of the right auricle, the mercury again rofe to 96°, and when applied in the fame manner within the left, it fell fomewhat below 94°.

This

This experiment was repeatedly made on animals that had been drowned and hanged, both without and within the heart, and there occurred a few inſtances where there was ſcarcely any difference in the temperature of the two ſides at *firſt*; but in all the temperature, both of the heart and its contents, was predominant in the right, before the left ſide had entirely ceaſed to act. It therefore appears, that the blood which paſſes through the lungs into the left ſide of the heart, without receiving from the air the neceſſary change, inſtead of being more tenacious of its heat than the right, on the contrary loſes it ſooner.

Thus we ſee the reſult of experiment ſanctions and juſtifies the predictions of theory, that when blood paſſes from the right ſide of the heart to the left, without having been expoſed to oxygene to abſorb its heat, it muſt evolve in paſſing through the capillaries of the lungs what little heat the blood contained in a latent ſtate: and the left ſide being no longer ſupplied with its due nouriſhment and warmth, either from the blood in the coronary veſſels, or from that contained in its own cavities, muſt have its temperature reduced, its irritability decreaſed, and its action gradually ſuſpended, by the diminution of its ſtimulus of diſtention.

But far different is the condition of the right ſide; for although the blood in the coronary veſſels is incapable of ſupplying it with heat, yet the blood within its own cavities contains a quantity in a latent form, which it continues to evolve; thus is its irritability ſupported, and thus, by continued diſtention, is its action kept alive.

Dr.

Dr. Goodwyn having obferved that in this difeafe all the cavities of the heart contain black blood, was induced to conclude that its other qualities were exactly fimilar; but had it been confidered that in thefe circumftances the blood, in its paffage through the lungs, fuffers a deprivation of its remaining heat, without the acceffion of a new fupply, the caufe whence originates the difference of irritability in the two fides of the heart would have no longer remained obfcure, nor would the Doctor, to explain this phænomenon, have been reduced to the fuppofition that the fame mufcular fibres were excited to action by different caufes, and that the blood of the fame quality that ftimulated the right fide to contract, was incapable of producing the fame effect on the left; but this difference would have been difcovered to arife from the left having loft a greater portion of its heat, and the ſtimulus of diſtention being diminiſhed beyond that of the right.

The advantages derived from this property of the right ſide of the heart, which ſupports its action after that of the left is ſuſpended, ſeem to have eſcaped the notice and eluded the reſearch of phyſiologiſts, yet no proviſion of nature more deſervedly claims our admiration and inquiry; for in no department of the animal œconomy has ſhe adopted a wiſer precaution for the preſervation of life, than, after the laſt expiration of the animal, prolonging to the right ſide of the heart a ſtimulus and power of action ſuperior to that of the left.

Let us but ſuppoſe the reverſe, that the left had the irritability of the right, and the

right

right the irritability of the left; as it is found neceſſary to the effecting a recovery, that the right ſhould firſt contract, and ſupply the lungs with blood to receive a change from the air in order to excite the left ſide of the heart to action; the right, in this ſuppoſition, would ſoon be incapable of performing this function; whereas, from the right continuing to contract after the left is motionleſs, it is thus capable of propelling blood through the lungs into the left auricle, which being once reſtored by the arrival of duly prepared blood (even though it ſhould have ceaſed to act from the ſtimulus of its own) is enabled, by the freſh ſupply of this ſtimulating quality, to revive, and the action of the whole heart is increaſed; but if the irritability of the left ſide were at firſt predominant, it would get rid of its own blood, and the feeble action of the *right ſide* be incapable of ſupplying it with more.

Thus at the very origin of the circulation, where the freſh ſtimulus is laſt applied, Nature, ever wiſe in her operations, has prudently placed a ſuperior degree of irritability; while in the left, where the irritability is inferior, the increaſe of ſtimulus is firſt received: nor will this be deemed the reſult of chance, if we but recal an obſervation we have already mentioned, that in the fœtal circulation, the ſtimulating quality of the blood is greater in the right ſide of the heart than that in the left, and that in the adult it is reverſed.

But although the blood in theſe two ſtates of the animal poſſeſs this difference of ſtimulus in the different ſides of the heart, yet, if an injury threaten the life either of the fœtus or of the adult, the right ſide of the heart will be found to contain blood of a ſtimulating quality ſuperior to that of the left,

left, and confequently greater irritability; for let us fuppofe that, at the time of birth, the umbilical chord is prevented from carrying on the circulation to and from the placenta, the blood that runs to the left heart, from its being obliged previoufly to pafs through the capillaries of the lungs, is deprived of a portion of its ftimulus: and thus, in the morbid ftate, is the fame provifion made for the fœtus as for the adult, though their natural circulation be widely different.

There is reafon to fufpect that in man there does not exift fo much irritability as in animals of more fimple conftruction; for it feems that in the more perfect or complicated, as man, whofe fentient powers are greateft, the vital are leaft; and we believe this will hold good in gradation with all the inferior animals, that, in proportion as the *ſentient powers abound*, the *vital diminiſh*, and *vice verſa*.

This is ſtrikingly exemplified in the polypus, which has been obſerved to regenerate into as many different polypi as divided into pieces; and theſe animals have neither brain or ſpinal marrow.

It appears therefore not improbable to be the intention of the great Creator, that thoſe animals, whoſe powers for perceiving danger are leſs acute, ſhould be capable of receiving greater injuries without the deſtruction of life, than thoſe that are armed with this faculty in a ſuperior degree.

All impreſſions made upon ſuperior animals are immediately conveyed to the brain, and

and this being the great ſenſorium, the whole animal receives the alarm, and an immediate effort is made to remove the cauſe of the injury. But inferior animals, that are unprovided with nerves and brain, that are conſequently deſtitute of ſenſation, and whoſe powers of inſtinct are but feeble, Nature, we find, to compenſate for this want of ſenſation, has enabled them to withſtand injuries to a greater degree than thoſe that are furniſhed both with brain and nerves. Animals alſo that are endowed with ſuperior ſagacity, poſſeſs but a ſmall degree of irritability; and it ſeems to be juſtly remarked, that the irritability of animals decreaſes as they advance in age. This was certainly intended for the ſame excellent purpoſe, that of ſupplying the defect of ſagacity while young; but when the ſentient powers become adequate to the neceſſity, this exquiſite irritability, which was ſo wiſely beſtowed on them while young, is no longer required.

In different ſpecies of animals, we have ſometimes obſerved that after reſpiration is ſuſpended, from drowning, &c. &c. ſcarce any action remained in the right ſide of the heart; but in ſeveral experiments the cauſe of this phænomenon was found to ariſe from an over diſtention of the right auricle and ventricle; for when a ſmall puncture was made in the anterior cava, and a portion of the blood contained in the right ſide of the heart expelled, its contraction became extremely powerful.

Here, then, was indirect debility brought on from over-diſtention; and there is reaſon to ſuſpect that this morbid accumulation of blood may frequently happen

from

from the methods of recovery ufually adopted, and in various other difeafes.

There remains a fufceptibility of action in almoft every part of the body for fome time after the fufpenfion of the fentient powers; but as animals, whatever may be the caufe of their deftruction, begin to die firft at the extreme and exterior parts; fo, in fufpended refpiration, from drowning, &c. we find the irritability of the heart outlives that of any other part of the body. One exception, indeed, has occurred, where the heart and extremities ceafed to act nearly at the fame time.

From confidering the length of time the heart may be made to contract after breathing has ceafed, there can fcarce be any doubt, if electricity be unable to excite it to action, but that life is irrecoverably loft; for,

for, with Mr. Hunter, we imagine life and the power of action to be intimately connected. If, therefore, we are incapable of calling forth this power into action, by the ſtimulus of electricity applied to the heart, there does not remain the moſt diſtant probability that the effect can be produced by the application of any other ſtimulus.

In our attempts, however, to reſtore the life of the apparently dead, we are furniſhed with no criterion for determining when this power of action is thoroughly extinct; for the exterior parts may have loſt this degree of irritability, and the heart ſtill retain it. In ſome inſtances, the heart of young animals has been made to act by electricity from ten to fourteen hours; and a gentleman, on whoſe veracity I can rely, has informed me he has ſeen it contract even twenty hours after reſpiration was ſtopped, and which is many hours longer than we have been able to excite action in any external part.

At firſt view it ſeemed probable that irritability and animal heat might be coequal; that, from the latter being preſent or abſent to a certain but unknown degree, we might be able to draw a prognoſtic of the preſence or extinction of the other; but ſubſequent obſervations diſcovered this to be erroneous; for, as there are few whoſe ſolids are not very differently excited to action by the ſame cauſe, ſo the quantity of heat evolved from the blood, that would ſupport irritability in the one, would produce no effect on the other.

This opinion is confirmed by the following experiments:

EXPERIMENT.

EXPERIMENT.

A ſmall Puppy was drowned, and on examining the temperature of the two ſides of the heart in the pericardium, the right was 98°, the left 96°. The right ſide of the heart continued to act for more than two hours; and during the laſt ten minutes its temperature was 60°, that of the left 57°; the warmth of the air in the room 55°.

EXPERIMENT.

A full-grown Dog was hanged, the pericardium opened, and the temperature of the right ſide of the heart was 100°, the left 99°. The right continued to act not quite ten minutes, when its warmth was 90°, that of the left 87° and one-half: the temperature of the room was alſo 55°.

Here,

Here, then, action continued in the one more than twelve times longer than in the other, though with a degree of heat much inferior. We have alfo had a farther opportunity of being convinced that heat and irritability do not always co-exift, from the bodies of two perfons that had been executed. A powerful electrical fhock was given, without producing the fmalleft external action, although three hours after execution the temperature of one was 80° externally, and the other 82° at the expiration of two hours and one-half.

This fuperior degree of heat, above that of the atmofphere, does not proceed from the prefence of life; for the longer or fhorter continuance of fenfible heat of any animal muft always be proportionate to the quantity of latent fire the blood contains, and the temperature of the furrounding medium;

medium; whereas the difference of irritability much more depends on the readineſs with which the ſolids act when this ſtimulus is applied, than on the quantity of heat evolved.

Why the fibres of one animal of the ſame ſpecies ſhould more readily act than thoſe of another, from the ſame cauſe, and how we are to diſcover the different degrees of this ſuſceptibility of action in each particular animal, is a queſtion not leſs important than intricate to unravel. As we have endeavoured to prove that *heat and irritability* do not neceſſarily co-exiſt, this may at firſt ſeem to militate againſt the opinion of heat being eſſential to the ſupport of irritability; but in reality it does not, for although the fibres of one animal ſhall act with its temperature at 60°, and the fibres of another ſhall ceaſe with its temperature at 90°: yet this only proves that the ſolids of the one act from a ſlighter cauſe than thoſe of the other, and not that the ſtimulus of heat is wanting. A certain quantity of inebriating liquor ſhall produce violent effects on one perſon, when a much greater quantity ſhall have no effect upon another.

Diſſimilar effects alſo take place from the ſame temperature; for although the heat of one animal may exceed that of another, and where the inferior degree of heat is preſent, the greater effect be produced; yet the *ſtimulus* in *quality* is the *ſame*, and the difference of action depends on the moving powers of the one being more readily excited to act than thoſe of the other. Nevertheleſs, though no deciſive prognoſtic can be drawn of the preſence of *irritability*, from the preſence of *any known degree of heat*, yet the nearer the degree of heat

approaches

approaches to its ſtandard the greater muſt be the irritability of that particular animal. But it will ever be better to fix no criterion of life, and make uſe of every poſſible means of recovery, in every inſtance, than to form a hazardous prognoſtic, that may prove fatal to hundreds.

Having now ſuperficially examined the functions of the Heart and Lungs, we ſhall, in the next Section, endeavour to aſcertain the common effects of ſuſpended reſpiration from Drowning, Hanging, and Suffocation, and then proceed to aſcertain the immediate cauſe of death.

SECTION II.

On the common Effects of Drowning.

The general effects of submersion have been described by others; and the result of our observations will be found nearly similar to that obtained by those who have already written on this subject. But, as it was necessary first to examine the appearances of animals under that circumstance, before any clear idea could be formed of the proximate cause of the disease, we shall begin with a description of the visible effects that usually arise from drowning.

As ſoon as an animal is immerſed in water, air is expelled from its lungs, and immediate attempts are made, apparently with great difficulty, to inſpire; in which a ſmall quantity of water is taken into the noſtrils and mouth. The animal betrays increaſing uneaſineſs; again expels air, and takes in more water. The duration of theſe ſymptoms varies from one minute to four: when the muſcles of reſpiration ceaſe to act, and all ſtruggling is at an end. Some involuntary motions, however, generally ſucceed. On opening the cheſt, we find the two *venæ* cavæ, right ſinus venoſus, auricle, ventricle, and pulmonary artery, loaded with blood; the left auricle and the left ventricle about half diſtended; the aorta and its branches containing a quantity of blood, which, in all its appearances, reſembles venous. The lungs are diſcovered in a ſtate of collapſe, containing a very ſmall

quantity

quantity of water, in the form of froth. The ſtomach, on examination, preſents alſo a little water, which probably paſſes into the œſophagus when the rima glottidis becomes cloſed by the epiglottis; for, as the water contained in the mouth is then refuſed admittance into the trachea, it ſhould ſeem, that, at that moment, it makes its way into the ſtomach; ſo that, as ſoon as the animal attempts to inſpire, water enters the orifice of the trachea; but this organ, as if conſcious of not receiving its due element, rejects the water, which is then allowed to paſs into the œſophagus. Air is again emitted, and new efforts made to inſpire, when, upon the ſame ſenſation being produced, ſimilar effects ariſe; and, after the laſt expiration, no more water enters the lungs, or ſtomach. If the rima glottidis as ſoon as irritated was not cloſed by the epiglottis, there would be found as much water in the lungs as the animal had expelled air; and if the ſtomach or lungs continued to admit water after reſpiration had ceaſed, we ſhould often find them fully diſtended: but, whether our examination be made immediately after the laſt expiration, or after the ſpace of one hour, we find no variation in the quantity.

It has been ſuppoſed that no water enters the windpipe until the animal is dead: but the entire reſult of our experiments tends to prove that water does get into the lungs during the act of drowning; and that no animal, provided with lungs, can be drowned without this circumſtance taking place. Indeed Dr. Goodwyn has proved this to be the fact, in a manner ſo convincing and ſatisfactory, that we need only mention, that the whole of our experiments to afcertain this point have uniformly agreed with his.

It has been mentioned by Dr. Goodwyn, and other Phyfiologifts, that the right auricle and ventricle are found FULL; but there feems to be fome degree of impropriety in the expreffion; for by the term FULL is generally underftood, a *cavity replete without vacuity*; and if fo, the left ventricle may be faid to be *full* when diftended only to half its natural fize, as it adapts itfelf to the volume of blood it contains, and in proportion as the quantity increafes, the cavity enlarges, until it attains a certain degree of diftention, when it re-acts. If the heart, therefore, contain but a fmall quantity of blood, the fides of the auricle or ventricle will be in contact with it, and the cavity be thus obliterated. Hence we prefer the term *diftention* to that of *fullnefs*.

The following experiments were made with a view to determine the exact proportion which the quantity of blood contained in the right side of the heart, bears to that in the left, after drowning.

EXPERIMENT I.

A Cat was drowned, and as soon as the usual struggles attending submersion had ceased, the chest was opened, the two cavæ, pulmonary artery, and aorta were secured, and the blood contained in the heart carefully collected. The proportions of the right were to the left as 12 to 7.

The next experiment was made to ascertain, whether, after the action of the heart had ceased, the proportions were altered.

EXPERIMENT II.

A Cat was drowned, and when the heart had ceafed to vibrate, the two cavæ, pulmonary artery, and aorta were fecured as before. The proportions of the right were to the left as 2 to 1.

Thefe experiments were repeated, and the quantities varied; fometimes being as 7 to 4, at other times as 5 to 2, or as 12 to 7. So that, at a medium, the proportions of the right are to the left as about $3\frac{1}{8}$ to $1\frac{6}{8}$. The lungs were uniformly in a ftate of collapfe.

Dr. Goodwyn has made fome experiments to afcertain the precife quantity of air contained in the lungs after death,

 and

and they were conducted in the following manner.

" A dead body of ordinary ſtature was
" procured, and a cloſe compreſs applied
" upon the ſuperior part of the abdomen to
" fix the diaphragm in its ſituation ; a ſmall
" opening was then made into the cavity
" of the thorax on each ſide, and upon the
" moſt elevated parts. The lungs imme-
" ately collapſed, and water was intro-
" duced at theſe openings, till the cavity
" was filled. The volume of the water
" contained was 272 cubic inches.

" The perſon on whoſe body this ex-
" periment was made had been hanged.
" In four ſimilar experiments, where death
" was natural, he found the medium was
" 109 cubic inches of air after complete
" expiration."

Theſe

These experiments, however, are by no means conclufive; for whatever may be the caufe of death, the animal dies with an expiration, and the tendinous part of the diaphragm is thruft up as high as the fourth, and fometimes as high as the fifth inferior rib; and therefore the application of a compress round the abdomen does not feem adequate to prevent the diaphragm from defcending. Could this event be effected, as the ribs cannot be kept at any fixed point, and as the air contained in the lungs was not collected, the experiment can by no means authorife any legitimate conclufion.

Dr. Goodwyn obferves, that atmofpheric air, by means of its gravity, will enter into the cheft, and, by its preffure on the external furface of the lungs, oblige them to collapfe. This obfervation, we prefume, ſume, is inaccurate; for according to a well-known law in hydroſtatics, air and all fluids preſs equally in every direction. However great, therefore, the quantity of air may be in the lungs after the laſt expiration, the preſſure of the external air cannot be ſuppoſed to aſſiſt in expelling it. This appears obvious on a common bladder inflated, which the preſſure of the external air by no means contributes to collapſe, but in the ſame manner as the lungs, where the preſſure is equal, its evacuation will depend on its own *elaſticity* and *weight*.

Thoſe who die a natural death muſt always have a portion of air remaining in the cheſt, ſince the lungs cannot be thoroughly evacuated by one expiration; but in drowning, &c. repeated expirations are made with ineffectual attempts to inſpire. What, therefore, Dr. Goodwyn has advanced on this head, appears neither eſtabliſhed by argument, nor countenanced by fact. But to determine the point, the following experiment was attempted.

EXPERIMENT.

A Cat was drowned, and after the uſual ſtruggles had ceaſed, the trachea was ſecured by a ligature, the cheſt opened, and the lungs taken out. A glaſs tube, divided into drachms and half drachms by meaſure, was filled with water, and inverted in a baſon containing the ſame fluid. The trachea was then placed under the tube and divided, and the lungs being preſſed, not half a drachm of air eſcaped. The ſame lungs when diſtended contained 16 drachms of air.

This experiment was ſeveral times repeated on different animals, and ſometimes ſcarce a bubble of air was collected ; in no inſtance was the proportion of air in inſpiration, to that remaining in the lungs after expiration, ſo ſmall as 10 to 1. The Heart has frequently been obſerved to contract, or more properly to vibrate, for more than two hours after reſpiration was ſuſpended, and that from no other ſtimulus but its own blood ; while in other experiments the vibrations did not continue one tenth part of that time. The right ſide of the heart preſerves its action much longer than the left, and the auricles longer than their correſponding ventricles.

The periſtaltic motion of the inteſtines does not continue as long as the contractions of the heart ; and on opening the head, the

the veins, as in ordinary death, are found rather diſtended, but without the leaſt appearance of extravaſation. Our next inquiries will be directed to the effects of hanging.

SECTION

SECTION III.

Common Effects of Hanging.

WHEREAS, in the lungs of animals that are ſuſpended by the neck, there is always preſent a certain quantity of air; the idea has been ſuggeſted, that they poſſeſſed no power to expel it; and that, as the lungs would then be more or leſs diſtended, the diſeaſe ariſing from it muſt differ from that produced by drowning. To aſcertain this point, the following experiment was made.

EXPERIMENT.

A dog was ſuſpended by the neck. As ſoon as the ſtruggles became violent, the fœces

fœces and urine were difcharged. In lefs than four minutes he ceafed to move. The air-tube was tied, the cheft opened, and we difcovered the fame appearances after hanging as after drowning; the lungs collapfed; the right fide of the heart over-loaded with blood; the left auricle and ventricle about half-diftended. The aorta and its branches contained blood, in quantity and colour fimilar to that from drowning.

Hence it appears, that, when an animal is fufpended, the mufcles of expiration are capable of performing their functions; nor are the mufcles of infpiration deprived of their action: but, as the preffure of the cord overpowers that of the external air, and clofes the opening of the trachea, the lungs are not found expanded, but collapfed.

Our

Our next objeƈt was, to attempt aſcertaining the exaƈt quantity of air that remained after hanging.

EXPERIMENT.

A dog was hanged; and, when all ſtruggle and motion had ended, a ligature was made on the trachea, in the ſame manner as in thoſe animals that had been drowned; the lungs were then removed; and the orifice of the trachea being placed under the glaſs tube filled with water, the ligature was taken off. On preſſing the lungs ſomewhat more than a drachm of air eſcaped. Theſe lungs, when inflated, contained forty-three drachms and one half of air. This experiment was often repeated; and ſometimes ſcarcely any air could be expreſſed from the lungs. At other

other times, the proportions in inſpiration were, to thoſe in expiration, as 11 and 12 to 1: but, in all inſtances, the quantity of air that remained was very inconſiderable.

In the next experiment, we endeavoured to aſcertain the exact proportion of the blood in the right ſide of the heart to that in the left, after hanging.

EXPERIMENT.

A dog was ſuſpended by the neck till he ceaſed to move. The two cavæ, pulmonary artery, and aorta, were ſecured by ligatures; and, after a careful inſpection of the heart, it was found, that the proportion of blood in the right, was, to that in the left, as 2 to 1.

The ſame experiment was repeated on a cat, and the proportions were as 5 to 3. On a repetition of theſe experiments, it appeared that in ſome the proportions were as 9 to 4; in others as 5 to 3, and as 7 to 4. So that the medium ſtands as 28 to 15.

The contractions of the heart and the periſtaltic motion of the inteſtines continue nearly as long after hanging as after drowning, the veins of the pia mater ſeem more diſtended, but without any extravaſation.

SECTION IV.

Common Effects of Noxious Airs.

It has been generally fuppofed, that when animals were immerfed in any air unfit for refpiration, it was both taken into their lungs, and again expelled. During which procefs a deleterious effect was produced on the fyftem that terminated in death.

This fuppofition is, however, fupported neither by argument, experiment, nor analogy; for we find the lungs equally *collapfed* in thofe animals deftroyed by noxious air, as in thofe which have been drowned. In both cafes, the firft expiration is by no means fufficient to exhauft the lungs.

The animals attempt to inſpire; when they become conſcious of receiving an improper element, and the epiglottis cloſes. Air continues to be expelled, and new attempts are made to inſpire, when the trachea being again irritated by the noxious air, little or none enters the lungs, and after the laſt expiration they admit no more.

In order to diſcover the preciſe quantity of air now retained, we made the following experiment.

EXPERIMENT.

A kitten was immerſed in nitrous gas, and when it had ceaſed to breathe, the trachea was ſecured, and the lungs removed. The air was then collected as before, in the glaſs tube; and it amounted only

only to $\frac{1}{2}$ of a drachm *; whereas, in the diftended ftate, thefe lungs contained 14 drachms and $\frac{1}{4}$.

In the repetition of this experiment, different kinds of impure air were employed; and the proportion of it in the diftended to that of the collapfed ftate was frequently as 40 and fometimes even 50 to 1; and in every inftance the quantity of remaining air was very inconfiderable.

The next object was to determine the exact quantity of blood in the right and left fides of the heart.

To afcertain this, the following experiment was repeated.

* We here mean the fame bulk occupied by half a drachm of water.

EXPERIMENT.

A rabbit was deſtroyed by nitrous gas; after which, the two cavæ, pulmonary artery, and aorta, were ſecured. The blood, in the right and left heart, was then collected. The proportion of the former was to that of the latter not quite as 3 to 2.

From a repetition of this experiment we learned, however, that, although the proportion was ſometimes not ſo much as 3 to 2, yet, in one inſtance, it was more than 2 to 1.

As a medium, therefore, the quantity of blood contained in the right, may be to that in the left as 5 to 3.

We

We here alſo remarked, that the irritability of the heart continues but little longer than the periſtaltic motion of the inteſtines; and that, in theſe experiments, they both ceaſed ſooner than in animals deſtroyed by drowning or hanging. Nor was this irritability in any one inſtance manifeſt from artificial ſtimuli after reſpiration had been ſuſpended one hour and five minutes. In ſome rabbits, deſtroyed by nitrous gas, the heart ceaſed to contract, from its own ſtimulus, in leſs than four minutes.

From the uniformity of theſe effects from various airs, we are authorized to conclude, that the airs in which the animals were immerſed contributed by their ſpecific quality to deſtroy their irritability.

I ſhall not deny that this effect is to be attributed to the noxious quality of the air entering the lungs; but ſhould rather ſuſpect the bulk of this air, taken into the cheſt of ſuffocated, does not more than equal that of the water admitted by drowned animals: for as the latter at each inſpiration receive only a ſmall quantity of water, it is probable the former admit only the ſame quantity of noxious air, which, mixing with what remains in the lungs, is at length nearly all expelled by repeated expirations; and a ſimilar collapſe takes place to that which we have already obſerved accompanies hanging and drowning.

It has been remarked, that animals deſtroyed by impure air do not grow rigid, but remain pliant and flexible. We have, however, in the courſe of our experiments, met with ſtriking examples of the contrary.

Animals killed by nitrous gas become fooner rigid than thofe deftroyed by drowning; and in two inftances, the rigidity of the extremities was remarkable, even before the heart had ceafed to vibrate.

On examining the head, the veins were found in a fmall degree diftended.

From this brief inquiry into the vifible effects arifing from hanging, drowning, and fuffocation, thefe trifling variations were obferved :—that in one inftance water enters the lungs; in the other noxious air: that this air poffeffes a greater tendency to deftroy the action of the heart than either hanging or drowning, and that, after the former, more blood is found in the head, though its proportions in the different fides of the heart are nearly equal.

The lungs in all theſe are in a ſtate of *collapſe*. Theſe conſiderations, eſpecially the *laſt*, incline me to believe, that the cauſe which produces death in one inſtance, operates alſo in the others.

SECTION

SECTION V.

An attempt to ascertain the proximate cause of the disease produced by Submersion, Strangulation, and Suffocation.*

To investigate and establish the proximate cause of the disease in suspended respiration from drowning, hanging, &c. is a task that has engrossed the attention, and exercised the pens, of several eminent physiologists; but there has been little coincidence of opinion; each seeming to have started, and embraced an hypothesis of his own.

* By proximate cause I mean, the condition of the parts diseased, and, which being removed, the disease ceases.

It

It has been the idea of ſome, that the air contained in the lungs becomes highly phlogiſticated, and that, from its deleterious influence originates the diſeaſe. Others attribute it to a congeſtion of blood formed in the heart and lungs; while another claſs ſuppoſe death to be produced by apoplexy.

To none of theſe opinions does Dr. Goodwyn incline; to him it appears that, from the privation of the uſual ſtimulus ſupplied by the air, the blood *contained in the left auricle and ventricle* is rendered incapable of exciting their contraction; and hence he derives the *immediate cauſe of the ſuſpended circulation.*

From an authority we ſo highly reſpect, it is with diffidence I diſſent; but the reſult of experiment tends to prove this opinion to be erroneous.

If

If the prefence of black blood in the left heart was the *proximate caufe* of circulation ceafing, then we fhould certainly find it *fully diftended* from the action of the right; but Dr. Goodwyn himfelf admits that this is by no means the fact: and, indeed, if the left auricle and ventricle were *fully diftended*, and it were neceffary for the reftoration of life that the blood *already contained* in the left auricle fhould undergo a change before it was enabled to empty itfelf, then every animal would be irrecoverable as foon as this black blood had once diftended the auricle; for we can appeal to the teft of experiment to prove, that no alteration can be produced on the quality of the blood contained in the trunks of the pulmonary veins and left auricle.

To afcertain if any fuch change could be effected, the following experiment was made.

EXPERIMENT.

EXPERIMENT.

A Dog was ſuſpended by the neck until he ceaſed to move; on opening the cheſt, both ſides of the heart were obſerved to contract; but the left ceaſed in eight minutes, while the right continued to act ſtrongly. The pulmonary artery being carefully ſeparated from the aorta, and ſecured by ligature, we proceeded to inflation, which was continued fifteen minutes, without being able to empty the trunks of the pulmonary veins and left auricle, or produce any apparent alteration on the quality of the blood.

This experiment was repeated on a cat, during the action of the left ſide of the heart, which became leſs diſtended, but no alteration

alteration in the colour of its blood could be produced. The change therefore which the blood undergoes in its paſſage through the lungs, is effected before it enters the *trunks of the pulmonary veins and left auricle*, and as the air cannot come in contact with *this blood* to produce any chemical alteration, it muſt be propelled through the ſyſtem unaltered, whenever an animal recovers; for ſuppoſing the blood within the lungs to have undergone its uſual change from inflation,, as the trunks of the pulmonary veins and left auricle are here underſtood to be *full*, and as *this blood* can receive no chemical change, the left auricle muſt act on its *black blood*, and receive the contents from the trunks of the pulmonary veins (which we have ſaid has not undergone the change) before the left heart can contain blood duly prepared by the air. We were, at firſt induced to believe that the collapſe of the lungs after inflation might have the power to empty the left auricle mechanically, by propelling the contents of the pulmonary veins onward, and by the preſſure thus applied from without, to the blood within the auricle, to ſtimulate its muſcular fibres to react, and ſo expel a portion of its contents. But there ſeems an objection to this ſuppoſition; for if the lungs by their collapſe had any ſuch power, they muſt have exerted it at the laſt expiration, and then thoſe veſſels which are affected by this action would be ſo far emptied as to require a freſh ſupply of blood from the right ſide of the heart before the lungs could, by their collapſe, have any mechanical effect on their contents; and the next experiment proves, that, after reſpiration is ſuſpended, very little blood is left within the lungs.

EXPERIMENT.

A Cat was drowned, and, when all motion had ceafed, we opened the cheft and fecured the pulmonary artery. A fmall ligature was then paffed round the trunks of the pulmonary veins, as they enter the left auricle, and both auricle and ventricle were then opened; the blood being all taken up by a fponge, the trunks of the pulmonary veins were divided, and on preffing the lungs very little blood efcaped, except that contained in the trunks. The repetition of this experiment afforded the fame refult. We muft therefore look elfewhere for reafons to account for the action of the left auricle in recovery, as experiment proves, that by inflation no chemical change within the trunks and auricle can be produced, nor by the mechanical action of the lungs, if the communication be cut off from the right ſide of the heart, empty the trunk; as this, I ſay, cannot be effected, it would ſeem that when the right ſide of the heart acts during inflation, there is a quantity of blood ſent within the lungs; and this contraction, aſſiſted by a collapſe of the lungs, propels a portion of the contents of the pulmonary veins onward, and thus produces ſuch a vis-a-tergo on the blood within the auricle, as to excite it to contract. It has been before obſerved that the right ſide of the heart *in health* performs this function independent of any mechanical action of the lungs, and it is likewiſe capable of the ſame action for ſome minutes after reſpiration is ſuſpended; but where the contraction of this

organ

organ is inſufficient to propel blood through the lungs, producing an *artificial collapſe* will have the ſame effect. This however can only happen where a freſh ſupply of blood has been ſent to the lungs by the contractions of the right ſide of the heart; as experiment ſhews that the quantity of blood remaining in the lungs is too ſmall to enable their mechanical action to have any effect on their contents.

It has been mentioned by Haller and other celebrated phyſiologiſts, that where the lungs are collapſed, an obſtruction to the paſſage of the blood through them will be the conſequence; but they have not proved *that the lungs are in ſuch a ſtate of collapſe in Drowning, Hanging, and Suffocation.*

I have endeavoured to ſhew that Dr.

Goodwyn's experiments to determine this point were objectionable; and our inquiries presented results very opposite to his, that instead of the lungs being distended that they were collapsed, and contained but very little air. In order, however, to prove that *this degree of collapse* was sufficient to produce a mechanical obstruction in the lungs in Hanging, Drowning, &c. we compared the quantities of blood in the different sides of the heart, where the collapse was removed, to that where the collapse existed.

The experiments were conducted in the following manner.

EXPERIMENT.

A Dog was suspended by the neck, and in less than a minute the fæces and urine

were

were difcharged; his ftruggles continued for little more than three minutes, when he ceafed to move; the trachea was then laid bare, and divided, and the lungs fully diftended with warm water (about blood heat) through the medium of a funnel; the trachea being fecured fo as to permit no water to efcape, the cheft was opened, and, contrary to all experiments made before, there was found a much lefs quantity of blood in the right finus venofus, auricle, ventricle, and pulmonary artery, than in the left, which was loaded with blood, part coagulated, and the whole quite black. The experiment was repeated, and yielded nearly the fame refult, with this variation, however, that the right fide of the heart had a little more blood than before, but the left was again fully diftended.

It then appeared evident, that if by an

artificial diftention of the lungs only, without the admiffion of air to produce any chemical change on the blood, the right fide of the heart was capable of diftending the left, and of expelling a part of its own contents; that in fufpended refpiration there exifts fuch *a mechanical obftruction in the interior* pulmonary veffels from collapfe of the lungs*, as prevents the right fide of the heart from getting rid of its contents.

The experiment was therefore repeated with fome alteration.

EXPERIMENT.

A Cat was drowned, and after the ceffation of all ftruggles, an aperture was made

* By interior pulmonary veffels is meant thofe that ramify within the lungs, and are influenced by the air; and by the trunks we mean thofe veffels that arife from the auricle, and are attached to the furface of the lungs.

made in the trachea, and the lungs diſtended with air, which was retained. On opening the heart we found the contents of the left ſide were to that of the right as five to four.

EXPERIMENT.

A Dog was drowned; when he ceaſed to move, cold water was introduced into the lungs. On examining the heart we found the proportions of the blood in the left were to that in the right as ſix to five.

Theſe experiments were repeated, and ſometimes the proportions were as ſix to four; but in one, where the irritability was trifling, the blood was a little predominant in the right. On the contrary, in another, where great irritability was preſent, the proportions were as two to one.

It may be urged by ſome as an objection to the above experiments, that water may act as a ſtimulus to the pulmonary veſſels, ſo as to excite them to act; but it has been obſerved, that there remains very little blood within the lungs after the laſt expiration; and if water acted on them as a ſtimulus, it could not produce the ſame effect on the trunk of the pulmonary artery, right auricle and ventricle, which we find in part emptied.

It has been obſerved that animals under the common method of ſuſpenſion, retain the power of expelling air from the lungs; but it was found not impoſſible ſo completely to compreſs the trachea, as to prevent any air from eſcaping: with this view the following experiment was tried.

EXPERIMENT.

The trachea of a Kitten was laid bare, and a ligature paſſed round it, that the whole of the air might be confined within the lungs. The animal ceaſed to move in four minutes and a half; and on opening the heart we found the proportions of blood in the left ſide were to that of the right as nine to ſeven.

The ſame experiment was repeated on a Rabbit, and the proportions were as eight to ſeven.

In theſe experiments, therefore, where the muſcles of expiration had not ſufficient power to overcome the compreſſure of the cord, and expel air from the lungs, the blood accumulates to a greater quantity in the

the left fide of the heart, becaufe no collapfe takes place, and confequently no obftruction to the paffage of the blood through the lungs.

The next experiment was made on an animal that had been fuffocated, by diftending its lungs with nitrous air.

In order to perform this experiment a common bladder was produced, and a pipe affixed to its neck, fmall enough to be inferted into the trachea of a rabbit. This pipe was introduced through a cork adapted to the fize of a wide mouthed bottle, which contained copper with diluted nitrous acid. The nitrous vapour arifing from this folution was collected in the bladder, and when a fufficient quantity was obtained, we attempted the following experiment.

EXPERIMENT.

A ſmall Rabbit was deſtroyed in nitrous gas, and as ſoon as it diſcontinued to expire air from its lungs, we removed it from the medium in which it was plunged. A ſmall aperture was then made in the trachea, the bladder taken from the bottle containing the nitrous gas, and the pipe introduced into the trachea in order to diſtend the lungs; which being effected, the air was prevented from eſcaping, by tying the trachea. On examining the heart, the proportions of blood in the left were to thoſe in the right as ſeven to ſix.

The experiment was again repeated by deſtroying an animal in hydrogene, and diſtending the lungs with nitrous air; and the

the proportions in the left were to thoſe in the right as thirteen to twelve.

But theſe laſt experiments did not always produce the ſame reſult, a larger portion of blood being found in the right than in the left ſide of the heart, from the ſlight degree of irritability that remained after reſpiration had been ſtopt by noxious air.

Our next attempt was to aſcertain if more blood were found in the lungs of an animal whoſe reſpiration was ſuſpended, and then the collapſe removed by a fluid, than where this ſuſpenſion took place without the removal of the collapſe.

We could deviſe no method to enable us to eſtabliſh this point with accuracy, but ventured, however, on the following experiment.

EXPERIMENT.

EXPERIMENT.

A Rabbit was drowned, and the lungs immediately diſtended with air; after tying up the trachea the cheſt was opened, the pulmonary artery and aorta ſecured, as alſo the trunks of the pulmonary veins. The left ſide of the heart was then opened, the blood removed, and pulmonary veins divided, the ligature was taken from the trachea, and the air expreſſed from the lungs. A large quantity of blood flowed from the pulmonary veins, and in a few minutes, by alternate expanſion and collapſe, the lungs were emptied of their contents. No accurate compariſon, however, could be drawn between the quantity of blood preſent in this experiment, and that which they contained in the collapſed ſtate; but it was evidently leſs in the latter, which tends to confirm the opinion of the collapſe of the lungs preventing a free circulation through them; for if more blood be found when they are diſtended than when collapſed, this it would ſeem muſt ariſe from the preſence of an obſtruction in the one inſtance, and its removal in the other.

Theſe, together with the former experiments, conſpire to prove that the collapſe forms an impediment to the circulation; for if in an animal that is drowned, hanged, or ſuffocated, the blood be found to predominate in the right ſide of the heart, while in another deſtroyed by the ſame means, the contrary takes place merely from the introduction of a fluid into the lungs which can have no chemical effect on the blood; from what can this variation and

and difference of quantity originate, if not from the mechanical obſtruction in the firſt caſe, and its removal in the ſecond?

It ſhould however be obſerved, that, although repeated experiments prove mechanical obſtruction to exiſt in ſuſpended breathing; yet it appears that the right ſide of the heart is capable of propelling a ſmall quantity of blood through the lungs during the collapſed ſtate of thoſe organs for ſome little time after reſpiration has ceaſed, and the left of getting rid of *its black blood*; an opinion that is ſtrongly countenanced by the following experiments.

EXPERIMENT.

A Kitten was drowned, the cheſt immediately opened, and the aorta ſecured, without including the pulmonary artery; when the

the heart had ceafed to contract, the quantity of blood in both its fides was examined, and it was found that the left contained nearly as much as the right.

This experiment was frequently repeated, and fometimes the quantity of the blood was greater in the left fide of the heart than in the right; but in all the experiments the quantity of blood in the left fide of the heart was increafed by tying up the aorta.

In the animals, therefore, fubjected to thefe experiments, the blood muft have paffed through the lungs in the collapfed ftate; and if no ligature had been applied, this *black blood* would have been propelled into the aorta, fince the period of examination of the heart after refpiration has ceafed,

makes

makes no alteration in the proportions of blood in their different cavities.

Thefe experiments afford a refult in direct contradiction to the opinion fupported by Dr. Goodwyn, that the left fide of the heart is incapable of acting from the ftimulus of black blood: for they prove that as the right fide of the heart is capable of fending blood through the lungs in the collapfed ftate, the left is alfo enabled to contract from the ftimulus of *black blood.*

The fame experiments may alfo at firft feem to invalidate the opinion that fuppofes the prefence of collapfe. But whenever the right fide of the heart has the power of propelling blood through the lungs in the collapfed ftate, the quantity is not fufficient to diftend completely the left auricle and ventricle. And as the lungs in this difeafe contain but very little air, and are nearly in the fame ftate as the fœtal lungs; and as only a fmall quantity of blood in the healthy ftate of the fœtus can be propelled through that vifcus; it appears that the blood paffing through the lungs during the collapfe in the adult, would not be fufficient for the demand, as very little more blood can be fent through the cheft, after the laft expiration, than in the fœtal circulation; with this material difference however, that in the fœtus a change has been given to the blood (in the placenta), while in the adult it can receive none.

Now as the left fide of the heart foon ceafes to poffefs a ftimulus that can enable it to difcharge its contents; fo alfo the right cannot long continue to propel blood through the lungs in their *contracted ftate:* for if the right fide of the heart continued

to

to ſend blood through the lungs when the left was incapable of getting rid of its own, we ſhould then find the blood predominate in quantity in the left.

Were Dr. Goodwyn's aſſertion true, that after the laſt expiration in drowning, &c. &c. the lungs contain a greater quantity of air than in *hydrops pectoris*, then an objection would ariſe to the ſuppoſition of their collapſe forming an impediment to the free paſſage of the blood; but we have already attempted to detect the inſufficiency of thoſe experiments which he imagined authorized this concluſion.

It muſt however be confeſſed, that Dr. Goodwyn's experiments ſeem ſo ingeniouſly deviſed, and the concluſions drawn from them ſo ſpecious, that at firſt they ſuſpended inquiry; and it was only by

 ſubſequent

ſubſequent examination that we were able to detect the fallacy of thoſe particular ones, which he adduces to aſcertain the quantity of air remaining in the lungs after the laſt expiration. But, by purſuing a mode of inquiry different to his, we obtained a reſult extremely unfavourable, and indeed contradictory to his concluſion, viz. that inſtead of the lungs containing a large quantity of air after drowning, hanging, or ſuffocation, the reſiduum is very inconſiderable.

To this concluſion ſucceeded an obvious reflection, that if the circulation could be properly carried on during a *collapſe* of the lungs, why ſhould the fœtal circulation differ from that of the adult? And indeed it appears evidently to be the intention of Nature, that only a ſmall portion of blood ſhould ever paſs through the lungs

in

in their ſtate of *collapſe*; for ſhe, ever uniform as wiſe in her operations, would never have provided a different circulation for the fœtus, if the veſſels of its lungs could have admitted through them a free and uninterrupted paſſage to the blood; but as a collapſe of the lungs was neceſſary to the fœtal œconomy, it was indiſpenſable that it ſhould be furniſhed with a foramen ovale, ductus arterioſus, &c. to compenſate for the ſmall allowance of blood that is ſent through them.

In drowning, &c. &c. as very little air remains in the lungs after the laſt expiration, the diſeaſe muſt exhibit nearly the ſame phænomena as the fœtus, whoſe muſcles of reſpiration have not been excited to act; for, in this caſe, it is nature that effects what we endeavour to attain by art; that is, to remove the collapſe of the lungs, and this by the introduction of a fluid that will give the neceffary change to the blood.

Haller, Cullen, and others, were of opinion that the ftate of full infpiration was as unfavourable to the tranfmiffion of blood through the lungs, as that of expiration; but this fuppofition appears to be but ill-fupported by fact; as experiment proves that, when the lungs were *completely diften-ded by water*, the blood freely paffed from the right fide of the heart to the left; and the action of the heart, under this circum-ftance, muft have been feeble, if compared to that which it exerts in a ftate of health.

It has alfo been the generally received opinion that where the *motion of the lungs* is by any caufe impeded, the circulation, from want of *their mechanical action*, is alſo ſuſpended; and that the accumulation of blood, which takes place in the right ſide of the heart, from drowning, hanging, and ſuffocation, originates from the ſame cauſe.

This opinion, however, has been contradicted by experiment, where, from the mere removal of the collapſe, independent of any *mechanical action* of the lungs, the circulation through them was reſtored; whence it is obvious that the accumulation of blood in the right ſide of the heart does not proceed from want of *motion*, but from the *collapſe* of the lungs.

In drowning and in ſuffocation from foul air, it has been found that the veins of the head are not more diſtended than in natural death: and that apoplexy does not take place, as has been ſuppoſed from hanging, is equally true; for if ſuch were the caſe, a recovery could not be effected; ſince our endeavours to remove common apoplexy, even while the proceſs of reſpiration and circulation proceed, frequently prove unſucceſsful.

Were it really true that apoplexy took place either in drowning, hanging, or ſuffocation, we ſhould conceive more ſanguine hopes of recovery after breathing had ceaſed in ordinary apoplexy than when it aroſe from drowning, &c. for theſe latter cauſes produce their fatal effect in a few minutes; while common apoplexy, even where a prediſpoſition exiſted, is generally many hours, and ſometimes days, before death takes place. If, therefore, the two diſeaſes be of the ſame ſpecies, that which ariſes from drowning, &c. muſt be much the more violent in degree. Were this indeed literally the fact, we should then, from drowning, &c. find great extravasation, and no recovery could be effected, and we should have reason to expect a recovery in those cases, where the cause was so slight as to require several hours to stop the natural actions: but as we are able to recover long after breathing has ceased in that disease, which, according to this theory, must be the most violent; and as we frequently fail of recovering from common apoplexy, even during respiration; it certainly proves that this disease, and that which takes place from drowning, are essentially different.

It has been advanced by some authors, that the mere distention of the vessels, without any extravasation either of blood or or ſerum, is ſufficient to produce apoplexy; and this is the ſpecies of apoplexy which has been ſuppoſed to be produced in drowning, &c. as it is acknowledged that no extravaſation takes place in the head: but were congeſtion alone, in theſe caſes, the cauſe of death, then muſt it be ſuppoſed that the diſtention alone of the veſſels acts much more violently than when attended with actual extravaſation; but this is an opinion not only diſcountenanced by probability, but alſo flatly contradicted by Valſalva and Morgagni on the ſtubborn faith of numerous facts. The latter obſerves "that thoſe caſes are the moſt violent, and "much the ſooneſt mortal, which have "their origin from *extravaſation* within "the cranium, we not only have daily "proofs of ourſelves, but it has alſo been "frequently obſerved by others."

It

It would therefore appear that though the veſſels of the head were fully diſtended in drowning, hanging, and ſuffocation, this diſtention could not here be conſidered as the immediate cauſe of death, ſince at moſt it can produce but a very mild ſpecies of apoplexy; for even when extravaſation follows, the actions of life generally continue for hours; while in drowning, &c. it is needleſs to repeat, the natural functions are in a few minutes aboliſhed.

There ſtill remains one obſervation, which proves the improbability of apoplexy happening from drowning, &c. and that is, that no accumulation of blood can be formed even at the right ſide of the heart, prior to the commencement of the collapſe of the lungs; but as ſoon as this obſtructs the circulation, then the blood receives but an imperfect change; and is therefore, in a great meaſure, deprived of its eſſen-tial quality. From this circumſtance it will no longer be capable of keeping up the full and natural action of the heart and arteries; and as the carotid and vertebral arteries will alſo have their action proportionably diminiſhed, the impetus of the blood to the head muſt thereby be checked, and conſiderably enfeebled. Theſe conſiderations induce me to believe that apoplexy can only happen where the blood receives its proper ſtimulus from the air to ſupport the action of the heart and arterial ſyſtem, and where an obſtruction exiſts to its free return.

In apoplexy that proceeds from diſtention of the ſtomach, and other cauſes, the blood continues to receive its due ſtimulus from the air; while, for want of a ſufficient expanſion of the lungs, (the diaphragm not being allowed a proper defcent,) an obftruction arifes to the free return of the blood, which occafions the difeafe. But, even in this fuppofition, death might not be the confequence, at leaft for many hours, if at all; although the veffels of the head might have been fully diftended, and that by the natural action of the carotid and vertebral arteries; but as in drowning, &c. thefe veffels are foon deprived of their wonted ftimulus, no injury whatever can happen to the brain.

From thefe obfervations, we truft it has been proved not unfatisfactorily, that apoplexy never happens in drowning, &c. But there is an experiment, which muft always fuperfede argument, that feems to difprove the exiftence of apoplexy.

This experiment has been mentioned before,

before, to prove a different fact; but as it is one that serves our present purpose, the repetition of it will therefore be excused.

EXPERIMENT.

The trachea of a dog was laid bare, and secured by a ligature, and this was endeavoured to be performed at the instant an inspiration was made; in less than four minutes he ceased to struggle. On examining the heart, we found the quantity of blood in the left, when compared to that of the right, as thirteen to twelve. A portion of the cranium was removed, and the veins of the head were evidently less distended than *natural*.

Here, then, there being no obstruction to the passage of the blood through the lungs,

lungs, it could not be collected in the right side of the heart, and consequently no accumulation was found in the head, and yet this animal died as soon as other animals from ordinary hanging; which carries conviction to my mind, that apoplexy forms no part of the disease.

As a further testimony, however, in favour of this opinion, the following experiment was made.

EXPERIMENT.

The two carotids of a Dog were secured *, and in half an hour after this operation

* This experiment of tying up the carotids in Dogs has been made both by Dr. Haighton, and Mr. Cooper, in order to ascertain the effects. I have also often taken up both carotids in horses, to remove staggers and other diseases of the head, without the smallest inconvenience to the natural functions of the animal.

operation he was hanged. In lefs than four minutes he ceafed to move; on removing a large portion of the cranium, the veffels were found much lefs diftended than in *ordinary death.*

From this experiment, as the principal fource of fupply was cut off, inftead of the veffels of the brain being in a ftate of congeftion, the quantity of blood they contained muft have been lefs than natural, and confequently no fpecies of apoplexy could follow. Yet this animal died as foon as other animals which had undergone no fuch operation.

Dr. Crawford's experiments evince, that when an animal is placed in a warm medium, the venous blood becomes nearly florid.

With a view to afcertain if an animal could be drowned, and the blood in the left fide of the heart ftill retain a florid appearance, the following experiment was made.

EXPERIMENT.

A Kitten was immerfed in a warm medium, a little above its own temperature, and permitted to breathe under a large glafs-bell for twenty-four minutes: it was then drowned in the fame medium.

On opening the cheft, it was found that the blood in both fides of the heart was fomewhat florid, and yet this animal died, which, according to Dr. Goodwyn, fhould not have happened. But the caufe of this animal's death can be readily explained; for the collapſe of the lungs was here of courſe the ſame as in common drowning, and from it aroſe the *immediate cauſe* that ſuſpends the circulation. But there was ſtill another power operating upon this animal to deſtroy life; for, from the intenſe heat and denſity of the medium in which the animal was placed, it was compelled to have recourſe to the proceſs of generating cold, in order to reſiſt the effects of this exceſſive ſtimulus; and the act of repelling heat invariably renders the powers of the animal leſs ſuſceptible of action: moreover, the power of generating cold by *evaporation* was here denied. Notwithſtanding, therefore, that the blood in the left ſide of the heart might be florid, yet the ſuſceptibility of action being feeble, the quality of this blood was inſufficient to ſupport irritability.

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It is worthy of remark, that in this and in every ſimilar experiment, the heart had leſs action than uſual, although the blood had this florid appearance; which clearly demonſtrates, that much heat diminiſhes irritability, and this effect is probably produced by the quick action which exceſſive heat invariably excites, and the debility conſequent on the endeavours to reſiſt heat. Hence it follows, that although the blood might poſſeſs latent fire in abundance, and what in health when rendered ſenſible would have been a proper ſtimulus, yet, from the ſolids not being ſuſceptible of action, life could not be ſupported. The *ultimate effect* of all violent ſtimuli muſt be that of a *ſedative*; thus heat, (which is one of the moſt powerful ſtimuli in nature,) when applied to a certain degree, acts as a perpetual ſtimulus; but if this be carried to exceſs, the final effect will be extreme *de-*

bility and death. This is likewife the ef-
fect of the ufe of fpirituous liquors, &c. a certain quantity will produce a ftimulating effect, without diminifhing the powers of the animal; but increafe it beyond this, and *debility* will be the confequence.

It has been feveral times remarked, from the refult of repeated experiments, that where the collapfe of the lungs was removed after breathing had ceafed, the circulation went on freely through the lungs, and diftended the left fide of the heart; but when the collapfe exifted, the left was not diftended: which evidently proves that the *collapfe of the lungs is the immediate caufe of the ceffation of circulation*, and not, as Dr. Goodwyn fuppofes, *the prefence of black blood in the left fide of the heart*; nor, as has been fuppofed by others, *from want of motion in the lungs.*

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We do not, however, confider the collapfe of the lungs as forming the whole of the *proximate caufe* of the *difeafe*; as, by the removal of the collapfe, the right fide of the heart is merely enabled to empty itfelf, and, by the *vis à tergo*, to produce an action in the left. But before the procefs of circulation can be completed, the animal muft be provided with blood poffeffing an increafed quantity of latent fire, as not only the left fide of the heart, but the whole fyftem, wants blood of this quality; fince in the fœtal circulation, the change is received before it reaches the heart, and both fides have a like ftimulus. As the heart, however, in the adult muft be the origin of circulation, fo it is neceffary that the alteration fhould be made immediately, before the blood enters one of thefe cavities; whereas, in the fœtus, the heart not being the origin of circulation, the change is given to the blood in the cells of the placenta.

There would appear a ſtriking impropriety in ſaying, that *the black blood in the left ſide of the heart and arterial ſyſtem was the proximate cauſe of the diſeaſe*, as *this blood* cannot be changed until it has run the courſe of the circulation, and returned to the lungs; and that cannot be effected without a previous removal of the obſtruction formed by the *collapſe*, and exciting the left to contract on its *black blood*; but even if the neceſſary change could be given during the exiſtence of *collapſe*, the lungs could not allow a ſufficient quantity of blood to paſs through them, to keep up the natural functions of the animal.

The proximate cauſe, therefore, of that diſeaſe produced by *drowning*, *hanging*, and *ſuffocation*, ſeems to be *mechanical obſtruction in the interior pulmonary veſſels from collapſe of the lungs, with a want of latent fire in the blood.* Remove this collapſe, and induce the neceſſary change on the blood, and you cure the diſeaſe.

Having thus far attempted to eſtabliſh the proximate cauſe, we are naturally led to inquire into the uſual remedies employed in this diſeaſe; and to ſelect ſuch as appear to be the beſt calculated to produce a ſalutary effect.

SECTION VI.

Effects of Emetics in suspended Respiration.

THE proximate cause that results from the suspension of respiration in drowning, hanging, and suffocation, we have supposed to be mechanical obstruction in the lungs, with a decrease of stimulus in the blood. The remedies employed to remove the disease are as numerous and different as the theories advanced to explain it; but, of them all, emetics, with which we begin, are perhaps the most ineffectual; their administration must even be attended with no inconsiderable injury, if had recourse to before the action of the vital functions is restored, and even then should be regulated by a ferious and vigilant regard to particular circumftances.

No falutary effects can be expected from vomits, but in cafes where the proceffes of refpiration and circulation have been re-eftablifhed, and where inquiry informs us that the ftomach has been overburdened either with food or fpirituous liquors. In thefe cafes there may be no impropriety in emptying the ftomach to facilitate the defcent of the diaphragm in infpiration; but to *commence* by the exhibition of emetics muft be highly improper, as the action and energies of the heart, from its fympathy with the ftomach, muft thereby be confiderably debilitated. And even admitting no fuch debilitating effects took place, every attempt to empty the ftomach muft neceffarily be futile until the nervous energy be reftored in a very ſenſible degree, when they may be exhibited to more advantage.

To aſcertain, however, with ſome degree of preciſion, the effects of a powerful emetic, the following experiment was made.

EXPERIMENT.

A Puppy was drowned, and after all ſtruggling had ceaſed, one drachm of emetic tartar, diſſolved in two ounces of water, was injected into its ſtomach. The lungs were then inflated, and other means of recovery employed, until the animal made an effort to inſpire; ſoon after which it appeared perfectly recovered.

In ſeven minutes from its apparent recovery it began to vomit; in twelve to purge;

purge ; and continued frequently to vomit and purge for one hour and feventeen minutes, when it died.

On examining the ftomach, it was found empty, but without the fmalleft appearance of inflammation.

As a temporary recovery was effected in this animal where fo ftrong a dofe of poifon had been adminiftered, and that without producing any inflammation, it was deemed requifite to introduce the fame quantity of emetic tartar into the ftomach of another puppy during the healthy actions of the animal, in order to determine if the effects were fimilar.

The experiment was made in the following manner.

EXPERIMENT.

EXPERIMENT.

Into the ſtomach of a Puppy of the ſame litter as that of the laſt experiment, was introduced one drachm of emetic tartar, while its natural actions remained unimpaired; in two minutes it appeared faint; in leſs than four vomited; in eleven purged; and in fifty-three minutes died.

The ſtomach, as in the laſt experiment, was found empty, but the *whole internal coat was nearly in a ſtate of gangrene.*

The reſult of theſe experiments exhibits a truly remarkable circumſtance, that an animal may be drowned, afterwards have poiſon injected into its ſtomach, and yet be recovered and continue to live longer than

than another of the ſame order and age, which had received the ſame quantity of poiſon in full health. It tends, however, to evince and aſcertain one fact, that medicines introduced into the ſtomach do not produce the ſame effect when reſpiration and circulation are *ſuſpended*, as when *theſe functions* are duly carried on: and this circumſtance ſomewhat accounts for a phenomenon which before to me appeared extraordinary, that a recovery ſhould ſometimes be effected, even after emetics, tobacco, &c. have been adminiſtered in quantities ſufficient utterly to deſtroy the life of the ſame ſubject if given in full health.

It may, however, at firſt be doubted, whether medicines that poſſeſs a ſedative property, like tobacco, would not produce their greateſt effect on an animal whoſe

powers

powers were weakeſt, and conſequently deſtroy the irritability of an animal already debilitated by drowning, &c. much ſooner than an animal, the vigour of whoſe powers remained undiminiſhed.

To aſcertain this point the following experiment was made.

EXPERIMENT.

A Puppy of about a fortnight old was drowned, and after all motion had ceaſed, a ſtrong infuſion of tobacco (one drachm to two ounces of boiling water, and ſuffered to cool) was thrown into its ſtomach; the uſual means of recovery were then employed: in fifteen minutes it made an effort to inſpire, and ſoon breathed tolerably well, but, in leſs than ten minutes after, it died.

EXPERIMENT.

An equal quantity of an equally ſtrong infuſion of tobacco was introduced into the ſtomach of another Puppy of the ſame age; it immediately fell motionleſs on the ground, and in leſs than four minutes expired.

Theſe experiments ſeem to prove that, whether medicines have a powerful ſtimulant or narcotic quality, their effects are *diminiſhed* in proportion as the powers of the animal are *decreaſed.*

That medicines, however, do produce ſome effect before reſpiration is reſtored, has been confirmed by the following experiments.

EXPERIMENT.

EXPERIMENT.

A ſmall Puppy was drowned, and the cheſt being immediately opened, the heart was obſerved to contract ſtrongly. Six drachms of laudanum were thrown into its ſtomach, and there followed almoſt an inſtantaneous diminution of the action of the heart.

This experiment was repeated, by injecting white vitriol, emetic tartar, infuſion of tobacco, &c. into the ſtomach, at a time when the heart was expoſed to view; and theſe were alſo found to check the force and frequency of its contractions, but particularly *tobacco*. As it therefore appears that in this diſeaſe ſympathetic effects continue to ariſe from the application of im-

preſſions

preffions to the fympathifing organs, it will at once appear obvious, that any medicine introduced into the ftomach which is likely to leffen the power of the heart, muft be attended with confequences highly detrimental; and that brandy, on the contrary, or any other warm cordial, which is known to increafe the action of the heart, (probably in thefe circumftances without diminifhing its power,) fhould only be employed.

To confirm this opinion, we proceeded to the following experiment.

EXPERIMENT.

A Dog was hanged, and the heart being expofed to view, one ounce of brandy was thrown into its ftomach; the actions of the heart were ſoon quickened, and each contraction appeared more forcible than before the exhibition of this ſtimulus.

This experiment we frequently repeated, by increaſing the quantity of ſpirit to ſix ounces and upwards; and it was found that ſo large a quantity quickened the actions of the heart extremely, but they were feeble and of ſhort duration.

From theſe experiments, however, we can draw only this inference, that a ſmall quantity of ſpirits here increaſed *both the power and action of the heart*, while a large quantity quickened the *action*, and *exhauſted the powers*. But the analogy will not hold good with the human ſubject in this particular inſtance; for, as the ſtomach of the brute is not accuſtomed to receive ſo ſtrong a ſtimulus as that of brandy, its effects will be different *in degree.* Indeed, from obſerving that all medicines produce a leſs effect after reſpiration has ceaſed than during health, it is probable that ſix, or even eight ounces, thrown into the human ſtomach would not increaſe the action of the heart beyond its powers; and thus a cordial of ſome kind becomes one of the neceſſary remedies in this diſeaſe.

SECTION VII.

Effects of bleeding.

We do not consider bleeding as a dangerous remedy in every case of suspended respiration from drowning, hanging, or suffocation; and, were it possible to take blood from the part where we know it superabounds, bleeding would prove one of the most immediate and efficacious means of recovery.

The right side of the heart has been found to be loaded with blood. This universally obtains in this disease; and I mentioned one or two instances in particular, where we had an opportunity of observing that the heart ceased to act from over diftention: but that, when relieved from a portion of its burden, its contractions were immediately renewed.

If, therefore, from the right fide of the heart, while thus in a ftate of violent plethora, a fmall quantity of blood could be taken; experiment and obfervation tell that its power and actions would be inftantly re-invigorated.

But as this lies beyond the reach of art, the taking of blood from any other part of the body can rarely be productive of any confiderable benefit, nor can it be employed with advantage but in fuch cafes, where, from an acquaintance with the complexion and habits of the patients, we may prefume that, previous to the accident or difeafe, a general plethora prevailed.

It may, then, be ſerviceable to diminiſh the exceſs of blood that loads the ſyſtem; for, when the right ſide of the heart has got rid of its preſent diſtention, if an accumulation of blood preſſes in every direction on the orifices of the two cavæ, and thence on the right auricle, it muſt tend not a little to enfeeble or wholly deſtroy its action.

A certain quantity of blood is requiſite to the ſupport of the proper action of the heart; but any thing above or below the ſtandard, will produce debility.

Bleeding, then, ſhould be only employed where the fluids appear too abundant. When the operation is to be performed, I concur with Mr. Kite, in adviſing the blood to be taken rather from one of the jugulars; not, however, that we expect much advantage

advantage to be gained by taking blood from the head after drowning and ſuffocation; but as there is here a near connection with the ſuperior cava, the heart would ſooner be relieved, than where it is drawn from the arm.

After hanging there will be a much more frequent occaſion for blood-letting than after drowning or ſuffocation, ſince the cord muſt in ſome meaſure prevent the free return of blood by the veins; and although we have endeavoured to prove that apoplexy can never happen, yet in theſe caſes as there is more than the natural quantity of blood in the head, it may be of ſervice to leſſen it. But the quantity of blood in the head will much depend on the weight of the patient; and, as bulk, weight, and general plethora, are united frequently in the ſame perſon, bleeding in ſuch caſes is very neceſſary ; whereas, if the patient be tall and thin, the diſtance from the heart to the head conſiderable, and the ſyſtem rather to want blood, bleeding, even after hanging, would perhaps do more miſchief, by debilitating the ſyſtem, than advantage could be gained, by relieving the local plethora of the head and heart ; for if the removal of the local plethora tends to increaſe the general debility, this laſt diſeaſe is more dangerous than the one we endeavour to remove.

We ſhall next inquire into the effects of electricity, together with thoſe of artificial reſpiration, both ſingly and combined.

SECTION VIII.

Effects of Electricity and Artificial Respiration.

FROM electricity, as it has hitherto been recommended and employed, confiderable indeed muft have been the mifchief that enfued. Agreeably to the method that was to direct its application, it was to be adminiftered as a local and general ftimulant, to be tranfmitted through every part of the body, the heart, brain, and fpinal marrow; and in all cafes where electricity has been the remedy principally relied on, it feemed to fuperfede moft of the other curative operations, but particularly that of expanding the lungs. From attending, however,

however, to the nature of the difeafe produced by fufpended refpiration in drowning, hanging, and fuffocation, it will evidently appear, that ftimulating the heart, without at the fame time endeavouring to remove the obftruction occafioned by a collapfe of the lungs, muft be one of the moft ill-judged and moft dangerous plans of recovery.

I repeat, there is a mechanical impediment to the paffage of the blood through the fmall veffels of the lungs from a collapfe of the air cells. This alone points out the danger of ftimulating the heart, while there exifts a caufe that muft impede its action. We are deftroying its irritability, without deriving any advantage, as the circulation can go on to no effect, unlefs the obftruction in the lungs be firft removed.

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We are, by this plan of treatment, absolutely taking away life.

Thofe who conceived *that the ftoppage of the motion of the lungs* was the immediate caufe of the ceffation of the circulation, and that the lungs were not in a ftate of *collapfe*, were led to recommend fhocks of electricity to be paffed through the heart, &c. without the lungs being at the fame time expanded. It has been advifed, that artificial refpiration, as well as electricity, fhould be frequently *interpofed*; and that, when the body is electrified, *all the other operations fhould ceafe.*

But as it has been proved, by experiment, that in this difeafe the lungs are in a ftate of *collapfe*, and that the circulation is ftopped from this caufe, and not from the want of *motion* in the lungs; it appears that electri-

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fying the heart without expanding the lungs muſt be highly detrimental. Had it been aſcertained that a *collapſe* of the air cells exiſted, I am perſuaded electricity applied to the heart during ſuch a ſtate of the lungs would never have been recommended. When electricity has been employed, the lungs have ſometimes been firſt expanded and *collapſed*, and ſhocks then paſſed through the heart, brain, and ſpinal marrow; but in this caſe the lungs being at the ſame time contracted, every electrical ſhock muſt diminiſh the power of the heart. Artificial reſpiration has alſo been employed after electricity; but this ſecond effort promiſes leſs probability of ſucceſs than the firſt; for the heart having before received a ſtimulus ſo great as that of electricity, it is not likely that the minor one, viz. that of the mechanical action of the lungs, ſhould have the ſmalleſt effect. And as the heart may not naturally act more than once or twice in a minute, there are many chances to one that theſe contractions do not happen at the inſtant the obſtruction is removed.

Inflating the lungs, and immediately after preſſing the cheſt, is ſaid to be imitating natural reſpiration; but it appears evident, that if the heart has not been excited to action during the expanſion of the lungs, this mode of proceeding is very improper.

Neither is this proceſs an imitation of nature; for, in health, the lungs always contain a large quantity of air, and we only expel a little, and receive in proportion. But, if all the air be diſcharged as ſoon as received, it is probable that the heart may act when the

the lungs are contracted, and which action can produce no falutary effect.

Whatever view the operator may have, who purfues this plan of treatment; whether he fuppofes a change to be produced *in the blood within the auricle*, or whether he expects to propel the blood within the lungs into the left fide of the heart, he will be equally difappointed. For we have obferved that no change can be produced in the trunks of the pulmonary veins; and we have alfo found that if any alteration in the quality of the blood be made within the lungs, there is not a fufficient quantity remaining for their mechanical action to propel this blood into the left auricle.

The advantage we may expect from inflation is this; *that the right fide of the heart may act at the fame time the lungs are diftended;*

distended; but, surely, suffering them to collapse as soon as inflated, is very unlikely to ensure success, when the heart has not been stimulated by electricity during the expansion of the lungs. Moreover, as the air can only become vitiated but by the action of the heart propelling blood into the lungs, there appears no necessity of performing a complete expiration after every inspiration, unless electricity has been at the same instant employed.

The plan of treatment likely to be useful appears to be, first, to expand *the lungs*; *and, when the collapse is removed, to stimulate the heart by a shock of electricity. The heart from this stimulus may be made to contract*; *there is a free passage for the blood and air in the lungs to produce a change*; and, if any irritability remains, the contractions of the heart will force some blood to enter the lungs,

lungs. We now perfectly collapse the lungs to convey the blood into the trunks of the pulmonary veins and left auricle. The lungs should be again immediately distended, and kept so, until another shock be passed as before.

It has been observed, that the hearts of some animals in suspended respiration have for a time the power of sending blood through the lungs, without removing the collapse; and probably in man, the heart may possess a sufficient degree of irritability to perform the same functions on being stimulated by electricity. But without considering the powerful stimulus required to effect this, and the debility which must necessarily ensue, let us inquire, what advantages can be possibly gained by propelling blood, during the collapsed state of the air cells of the lungs, from the right side of the

the heart to the left. Allowing this could be effected, there is no air in the lungs to produce any chemical alteration on the *quality* of the blood; and if the left auricle and ventricle were in part emptied and again distended with a fluid equally foreign to the wonted ſtimulus, their power muſt every time be diminiſhed, and conſequently the right, at each contraction, require a ſtronger ſtimulus to produce the ſame effect; and the left finding an increaſing difficulty in propelling its contents, the right would be leſs capable of overcoming the collapſe.

This power, therefore, could only continue for a ſhort time, and during its exiſtence no better effect could be produced from blood paſſing through the lungs *without receiving a change from the air*, than when propelled by friction from any other artery into a vein.

If no electrical machine can be procured, the manner of carrying on artificial reſpiration ſhould be altered; the lungs are to be expanded; and, inſtead of compreſſing the air out as ſoon as received, they are to be kept in a ſtate of moderate expanſion for about a minute; ſo that, if the heart acts during this period, there may be no obſtruction to the paſſage of the blood.

It has been already obſerved that the lungs in ordinary reſpiration have no active power in propelling blood through them in health. But it ſeems in the recovery they may aſſiſt by their action; for when the heart poſſeſſes only power ſufficient to ſend blood within the lungs, without being able to propel it to the left heart, producing an *artificial collapſe* will empty the interior pulmonary veſſels of the blood they have received, and excite the left auricle and ventricle

ventricle to contraction. That the lungs will here produce this effect there can be no doubt, ſince we find a greater quantity of blood in them when diſtended than collapſed; and hence, by compreſſing the lungs, they muſt act upon all the blood they have received ſince the laſt expiration.

Care, however, ſhould be taken, that the collapſe is never ſuffered to *continue*; for the heart may act at this period, and *then* without effect; ſo that the act of inſpiration in every inſtance ſhould be performed immediately after the laſt complete expiration.

During the whole proceſs of the treatment, from the firſt attempt to effect a recovery, the lungs ſhould never be ſuffered to remain collapſed, that other curative means may be employed. Without this precaution we render abortive all our endeavours to remove the caufe of the difeafe; for this end not previoufly attained, what rational hope or dependance can be placed in the application of any remedy?

Inftances of recovery have not been wanting where the lungs were not inflated; but in fuch it muft be attributed to an unextinguifhed energy of the living principle, which continued in fome degree to enable the mufcles of infpiration to act fo as to afford admittance to a portion of air.

Does it not appear probable that the difference of fuccefs which marks the cafes reported by the Humane Society, in which the fame method of cure was obferved, may depend in a great meafure on the heart's acting, or not acting, during the *expanfion* of the lungs? Some patients were irrecoverable

irrecoverable after refpiration had been ftopped for only one, two, and three minutes; whilft the recovery of others, who had remained more than half an hour under water, was effected by a fimilar mode of treatment.

The variation of the degrees of irritability in the fame order of animals is found to be confiderable; but it appears improbable, that one fhould be deftroyed from a caufe which, thirty times multiplied, is infufficient to take away life from another apparently under the fame circumftances. In one cafe where I was prefent as a fpectator, the body had not been long under water; yet all the endeavours to reftore life proved unfuccefsful. The failure of fuccefs, however, in this unfortunate cafe was evidently occafioned by the means and method purfued to obtain a recovery. The ſmoke of tobacco blown up the rectum, frictions, and inflations of the lungs, were firſt employed for about ten minutes, when the two latter were ſuſpended to allow the adminiſtration of electricity. This ſtimulus was applied by paſſing ſmart ſhocks through the heart, brain, and ſpinal marrow; in fact, the whole body was electrified. The muſcles through which it was conducted contracted powerfully. The ſhocks were repeated with ſanguine hopes of ſucceſs, but the contractions gradually became more feeble, and in about two hours were totally aboliſhed.

Artificial reſpiration, with frictions, was again attempted, but to no effect. It is obvious that in this caſe a conſiderable degree of the vital energy was preſent, but abſolutely deſtroyed by the means employed to re-eſtabliſh it; for as the proximate cauſe

of

of the difeafe was not removed, every action of the heart increafed the debility. But had the collapfe of the lungs been taken away when the heart had been ftimulated, far different indeed might have been the effects; no impediment would then have exifted to the paffage of the blood through the lungs, and it would have imbibed from the air its neceffary portion of heat.

Inflating the lungs and electrifying the heart at the fame inftant, may at firft view be thought difficult; but it will be found that proper inftruments, conftructed for the purpofe, will make this an eafy procefs.

We have obferved before, that *collapfing* the lungs, as foon as diftended, is not imitating natural refpiration. Air blown from the mouth of another muft be highly improper, as being robbed in fome meafure of its purity; and if a pair of bellows be uſed, it will employ three perſons, one to inflate, another to ſecure the noſtrils and mouth, and a third to preſs on the cricoid cartilage and cheſt in expiration. Nevertheleſs, where no other inſtruments are at hand, a pair of bellows may be found very uſeful.

But there are two diſadvantages attending every inſtrument introduced into the noſtrils; firſt, the epiglottis obſtructs the free paſſage of the air; and part of the air, which cannot be prevented by preſſing on the cricoid cartilage, will often enter the ſtomach; for although preſſure applied here may prevent moſt liquids from paſſing, yet ſo ſubtle a fluid as air blown with force, may make its way into the ſtomach. The air is not likely to produce miſchief from its quality, but from the mechanical effect

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of preventing the lungs from expanding. Refpiration in health is often impeded in confequence of the diaphragm in the act of infpiration being obftructed by the ftomach; and the other mufcles not being able of themfelves fufficiently to enlarge the cheft, the right fide of the heart is prevented from acting with its ufual eafe; and hence a diftention of the ftomach from air muft be attended with the fame effect *.

Mr. Hunter has contrived a double pair of bellows with two valves, fo that one fhall perform the office of infpiration, and the other that of expiration, and thefe are

* From want of proper inftruments I once faw the ftomach and the whole inteftinal canal very much diftended, and a rupture, under which the patient laboured, was alfo confiderably enlarged; but the major part of the air may at any time be difperfed, by preffing on the abdomen.

adapted to an inſtrument which is to be introduced into the trachea, after bronchotomy has been performed.

This is certainly a good contrivance, but, from want of portability, they have rarely been employed.

Dr. Monro has invented an inſtrument to be introduced into the trachea, in the form of a common male catheter. This is mentioned by Mr. Kite; but its uſe is only recommended on particular occaſions: and it would ſeem that the inſertion of this inſtrument into the windpipe could not anſwer the purpoſe ſo well as at firſt might be expected; for, when introduced, the inferior orifice would be thruſt againſt one of the ſides of the trachea, and the curve preſſing on the other would form an obſtruction to the air.

There alſo ariſes a great difficulty in introducing this inſtrument, more eſpecially to thoſe who have not been in the habit of employing it, as no guide is given by which we may know whether it be inſerted into the larynx or pharynx; and as the aperture of the latter is ſo much larger than that of the former, it would rather glide into the œſophagus than into the trachea, and thus inflate the ſtomach inſtead of the lungs. The ill conſequences ariſing from ſuch a miſtake are ſufficiently obvious; and, to guard againſt ſo fatal an error, the following inſtrument is recommended.

As it has been deemed requiſite to introduce ſome ſtimulating cordial into the ſtomach, a vegetable bottle (Fig. 7.) is contrived for this purpoſe, which is to be attached to the flexible tube, (Fig. 6. at B.) and introduced down the œſophagus; and on this tube

tube is placed a conical piece of ivory, (cc) that is moveable, to ſerve as a director for the introduction of the pipe into the trachea.

The vegetable bottle being filled, the tube is to be inſerted three or four inches into the œſophagus, and the conical piece of ivory is then to be carried onward by the aſſiſtance of the forefinger, ſo as to cloſe the ſuperior aperture of the œſophagus.

Having proceeded thus far, the tongue is to be brought as forward as poſſible, and the inferior end of the curved pipe (Fig. the 1.) paſſed to the farther part of the mouth, until it meets with the ivory director. The pipe being then brought a little forward, the ſuperior extremity is to be elevated, by which means the inferior will be depreſſed,

and

and with eaſe enter the trachea: for, as the entrance of the œſophagus is ſituated immediately behind the larynx, and as the pipe is prevented from entering here by the ivory director, it muſt paſs into the air-tube; ſo that the vegetable bottle and its appendages anſwer a double purpoſe, that of injecting fluids into the ſtomach, and as a guide to the introduction of the other inſtrument.

The * pipe for the trachea is much larger and longer than thoſe uſually employed, and the great curve is given to the ſuperior, inſtead of the inferior part; from which reſults this advantage, that when it is fixed in the trachea, it will be nearly in a ſtrait line with that tube; and, for the more

* It may perhaps be adviſable, that the ivory director be continued in the œſophagus during the whole proceſs of the treatment, as this will prevent air from regurgitating into the ſtomach.

eaſy introduction of the inſtrument, the pipe is made conical, and that there may be no impediment to the paſſage of the air, two lateral openings are made at the inferior extremity (B).

The application of theſe inſtruments are not likely to embarraſs any profeſſional man. If, however, any impediment ſhould prevent the inſertion of the pipe into the air-tube, bronchotomy ſhould be immediately performed; but the place, and manner of performing this operation, agreeable to the method generally recommended, do not appear the moſt eligible.

We are adviſed by authors, to begin it by a longitudinal inciſion, three or four rings below the cricoid cartilage, and when the trachea is met with, to divide it between the rings.

The performance of this operation, according to this plan, can ſcarcely be attended with danger, when attempted by a ſkilful anatomiſt; but it may be embarraſſing to a medical aſſiſtant, who is obliged haſtily to perform it, when perhaps he may not perfectly recollect the ſituation of all the veſſels; and it is to be remembered that haſte is always particularly neceſſary on theſe occaſions. Allowing, however, that the operation is ſuccefsfully performed, great inconvenience muſt follow from the ſituation of the wound; for, in the recovery of the drowned, hanged, and ſuffocated, the head is, and always ought to be, kept a little elevated; the conſequence of which muſt be, that the aperture in the trachea then becoming the moſt depending part, the flow of blood that follows the operation will principally enter it, and thus prevent artificial reſpiration from being properly carried on. This is not a theory founded on hypothefis, but on facts; as I have feen two cafes wherein this accident actually happened. The fuperficial veins of the trachea from this operation were divided and bled fo confiderably, as to get into the bronchia and occafion fuffocation; and I have heard, from good authority, that the fame effects have been known to happen in feveral other cafes.

Another inconvenience attendant on this mode of operating is, that, from the trachea at this part being fo much covered, the pipe for inflating the lungs is with difficulty fecured; and fhould a recovery be effected, the patient muft be under the neceffity of keeping his chin directed conftantly downward, in order to approximate the cartilages, a pofition that is not only difagreeable, but to be continued very unfavourable to the union of the parts.

In order therefore to render the operation more fimple, lefs dangerous, and to prevent blood from entering the air-tube; I conceive it more eligible to divide the thyroid cartilage: and that, inftead of the incifion firft being longitudinal, and then tranfverfe, both the integuments and cartilage fhould be cut through longitudinally at once.

Several are the advantages derived from this mode of operating. Firft, no danger can then arife from the want of anatomical knowledge. Secondly, the covering being here very fuperficial, little blood will be loft, and the little that does efcape, cannot get into the windpipe. Thirdly, the curved pipe can be very well fecured, in order to carry on inflation and collapſe. Fourthly, if our attempts to recover be ſuccefsful, keeping the head naturally erect will be the beſt poſition to approximate the divided cartilage; and laſtly, that the recurrent nerves and ſuperficial veins are in no danger of being divided. The only inconvenience to be dreaded from this operation, is that of committing an injury on the ſacculi laryngis, and thus to incommode the voice; but theſe are ſecured from danger, by cutting through the middle of the cartilage; and an union will be more eaſily effected, than if the trachea itſelf had alone been divided.

The ſurgeon ſtanding at the right ſide of the patient ſhould perform the operation by putting the integuments on the ſtretch with the thumb and forefinger of the left hand, a longitudinal inciſion is then to be made immediately over the thyroid cartilage,

lage, into which may be inſerted the curved pipe that was intended to be introduced into the trachea by the mouth.

Whether this operation has or has not been performed is of little conſequence to the recovery, if an inſtrument be introduced into the windpipe, that is connected with the other apparatus.

To the curved pipe for the trachea is to be fixed one extremity of the flexible tube, (Fig. 2. A) ; and the other end (B.) be attached to the inſtrument, (Fig. 3. C.) which may be fixed to the nozzle of any pair of bellows.

Every thing being prepared for inflating the lungs, one aſſiſtant is to have the direction of the bellows, and to ſtand at the head of the patient, whilſt the other prevents any air from escaping at the nostril and mouth ; or from the aperture if any has been made in the trachea.

The bellows are now to be employed, until the chest is elevated ; and the Medical Assistant, having the electrical machine prepared, is to place one director between the fourth and fifth rib of the left side, and the other between the second and third of the right ; so that the electrometer may discharge the jar, and the shock be made to pass from the apex of the left side of the heart to the basis of the right.

When the electrical stroke has been once more repeated, the assistant, who has the care of the mouth and nostrils, is now to remove his hands, and press strongly upon the chest ; the bellows are again to be immediately employed, and, another shock being

being prepared, the heart is to be thus ſtimulated twice or thrice, and the lungs collapſed as before.

If the heart retains any irritability, the collapſe of the lungs being removed, the contractions of the heart will probably be renewed; a free paſſage is opened for the blood, and air admitted to give it the neceſſary change. But as the actions of the heart may not be ſufficiently powerful to propel the blood completely through the lungs, it becomes neceſſary to have recourſe to the artificial collapſe, in order to effect this. We therefore, after having inflated the lungs, and electrified the heart, preſs upon the thorax, in order to expel moſt of the air contained in the lungs; for ſuppoſing the lungs to have received but one ounce of blood from the contraction of the heart, a certain degree of collapſe will get rid of a portion of this blood; and if the collapſe be increaſed, the quantity of blood acted upon will alſo be greater. This plan of treatment appears a matter of importance, for the greater the quantity of blood ſent from the right ſide of the heart to the left, (if at the ſame time it has received the wonted change from the air,) the greater undoubtedly is the probability of its exciting the left to action.

If *natural reſpiration* be imitated without attending to the *collapſe* of the lungs, there is leſs probability of ſucceſs, even ſhould the heart be electrified during the expanſion of the lungs. For, if the pulmonary veſſels are diſtended with blood by the action of the right ſide of the heart, without producing a collapſe of the lungs ſufficient to enable them to act mechanically in emptying theſe veſſels, there will ariſe nearly as much obſtruction to the action of the heart as when the collapſe exiſted; for the pulmonary veſſels muſt then be emptied as well as diſtended by the action of the right ſide of the heart alone, which by this diſeaſe is ſoon rendered ſo enfeebled, as to be wholly inadequate to ſuch an exertion.

By exhauſting the lungs, after the heart has been made to act during inſpiration, the *collapſe* will in ſome meaſure ſupply the abſence of powerful action in the right ſide of the heart; for all the blood the lungs have received is by that means carried to the left, by which we not only gain the advantage of ſending blood which has received its due heat from the air into the left auricle and ventricle, but moreover the pulmonary veſſels are again put in a fit ſtate to receive more blood from the action of the right, and even a feeble contraction of the heart may be capable of ſending blood into the pulmonary arteries, though a more powerful one might be inſufficient to propel it into the pulmonary veins and left auricle.

Mr. Field, a very ingenious mathematical inſtrument-maker, has invented an inſtrument (Fig. 4.) which may be fixed to the nozzle of a common pair of bellows for the double purpoſe of inflating and collapſing the lungs. (For a deſcription of which ſee the explanation of the plate.) But, in order to produce this effect, it is neceſſary that the valve hole of the bellows be cloſed by the inſtrument (Fig. 5.) by which means all the air employed muſt paſs through the ſmall aperture (d. in Fig. 4.) Hence the operation of inflating and collapſing the lungs neceſſarily becomes a ſlow and tedious proceſs, which may be conſidered as an imperfection in this inſtrument, particularly if the bellows to which it may happen to be fixed be not air-tight, in which caſe the external air will find a ready entrance, and its intention as an air-pump will be defeated.

If oxygene were at hand, there can be no doubt but that it would be far preferable to any other air for inflating the lungs; but to procure it in ſufficient quantities at ſo critical a period is generally impracticable, we muſt therefore make uſe of atmoſpheric air as pure as can be obtained.

If the jar be not charged to give the electrical ſhock, as ſoon as the lungs are expanded, no miſchief or inconvenience enſues; for we need only ſuffer a ſmall quantity of air to eſcape at the mouth after

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every inſpiration, and immediately throw freſh in by the bellows; and this proceſs is to be continued for about a minute; when, if the ſhock is not yet ready, we let go the mouth, and empty the lungs. The heart from this may have been irritated by the repeated inſpirations, while the lungs have not been ſuffered to obſtruct the free paſſage of the blood, and a freſh ſupply of air has been introduced to give it the neceſſary change; ſo that if the heart has acted during this period, collapſing the lungs will now convey the blood they have received into the trunks of the pulmonary veins and left auricle. This proceſs is therefore to be purſued where any circumſtance prevents the ſhock of electricity being given as ſoon as the lungs are expanded, or where no electrical machine can be procured; but, as the irritability of the heart cannot long be excited to action by

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the mere diſtention of the lungs, we think it of the higheſt importance that electricity ſhould be employed.

It ſhould, however, be remembered that every ſhock given to the heart during the collapſed ſtate of the lungs, tends to rob it of its vital power, without promoting in the leaſt the recovery; and let it alſo be repeated, that the lungs from the beginning are never to be ſuffered to remain collapſed for a ſingle minute; as the heart may act at that very inſtant, and in this caſe without effect; for as every contraction is an expenſive operation to the heart, if it has got rid of no portion of its burden, the utmoſt care ſhould be taken that the lungs be expanded at every ſyſtole of the heart; and this can rarely happen from the uſual method of inflating the lungs without at the ſame time ſtimulating the heart. When the heart has been once emptied, occaſional ſhocks may be tranſmitted through other parts of the body (care always being taken that the heart partakes of their influence, and that the lungs be expanded); for ſtimulating the extremities may probably produce ſome degree of contraction in the arterial ſyſtem; but it ſhould be ever remembered that the heart is the origin of the circulation, and whilſt other parts of the body are electrified, care ſhould be taken that the heart at the ſame time partakes of the ſtimulus.

In order to compare the difference of the effects produced by electricity on the heart, when the lungs are collapſed, and thoſe that reſult from it when the lungs are in a ſtate of expanſion, the following experiment was made.

EXPERIMENT.

A Cat was ſtrangled, and five minutes after the laſt expiration the cheſt was opened; the lungs were then alternately expanded and collapſed for five minutes, the heart acted rather powerfully, but no alteration could be obſerved in the blood of its two ſides; either as to quantity or quality.

The heart was now electrified by ſmall ſhocks, during the exiſtence of the collapſe, and this was continued for five minutes, when, upon examination, it was obſerved that its action was evidently leſſened; the left ſide became rather more diſtended than before, but the blood was black in both auricles and ventricles.

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The lungs were now expanded, and the heart at this inftant electrified: after two fhocks had been given, they were collapfed, again expanded and electrified; and this procefs was likewife continued five minutes. On examining the heart, both fides were now found lefs diftended, their action quickened, and the blood in the pulmonary veins, left auricle and ventricle, completely florid.

The refult of this experiment not only proves the advantages of the ftimulating power of electricity on the heart, beyond that of *fimple inflation*; but alfo evinces the fuperiority of adminiftering it in the diftended, over the collapfed ftate of the lungs.

Whatever will excite the heart to expel its black blood, and fupply the left fide

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and the whole arterial ſyſtem with blood that has imbibed its natural heat from the air, muſt be the means of cure the moſt efficacious that can be employed; and this laſt experiment ſeems to confirm the opinion, that electrifying the heart during the expanſion of the lungs, and then collapſing them, is beſt calculated to produce this deſired effect.

With reſpect to the electrical machine, the more compact, and at the ſame time the more powerful it is, the better; for as the quantity, neceſſary to be applied, muſt be determined by the jar and electrometer, the more ſpeedily it can be filled the better. The ſize of the jar neceſſary for the purpoſe ſhould be about thirty inches of coated ſurface; and the electrometer placed a little more than one third of an inch from the jar; the diſtance of which may be gra-

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dually increafed. It is better that the glafs of the jar be thin, as the fhock will then be pungent; for if the glafs be thick, the ftroke will be large and denfe ; and it appears probable, that the pungent ftimulus would excite greater action in the heart than one that is denfe, without being fo liable to deftroy its powers.

All that appears neceffary in thefe cafes, for the purpofe of applying electricity, is, the cylinder, a conductor, jar, electrometer, wire and directors, in order to convey the fhock to the particular parts we wifh; and all thefe may be comprifed in a box of twenty inches in length, and twelve in width ; and as every medical man may have occafion to make ufe of electricity for other purpofes, the expence will not be thrown away, even fhould he never meet with this moft fatisfactory employ of it, the attempting or perhaps the actual reftoring of the apparently dead to life.

There appears no neceffity for making ufe of the inftrument invented by Dr. Goodwyn, for the purpofe of extracting water from the lungs, as thofe who have recovered from drowning muft all have taken in water, without its having produced any ferious inconvenience; and as the extracting it would take up a confiderable time, we think it better as foon as poffible to proceed to the diftention of the lungs.

We fhall now inquire into the effects produced by the application of warmth.

SECTION IX.

Effects of Warmth.

It has been the uniform opinion of thofe who have turned their attention and their pens to the fubject of fufpended refpiration, from drowning, hanging, and fuffocation, that the application of heat is abfolutely neceffary, and that it ought to be made with gradual, and nearly infenfible increafes. This idea feems to have been fuggefted, by attending to the good effects of warmth on torpid animals; and the manner that nature prefcribed, was that of its being applied in the moft gradual manner; for, where the body has been frozen, a fudden application of heat

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has been found deſtructive, whereas a leſs degree has proved beneficial.

It would be preſumptuous to deny that theſe obſervations and precautions ſeemed well grounded. But it muſt, however, be confeſſed, that the detection of any ſtrict ſimilitude between the two diſeaſes, would be attended with no ſmall difficulty. In the one, the vital principle is attacked merely by a ſedative power; in the other, it is endangered by a collapſe of the lungs, which not only prevents the free paſſage of the blood, but at the ſame time deprives it of that due degree of heat, which it borrows from the air.

Dr. Goodwyn has particularly inſiſted on this gradual application of warmth; but his plan of treatment does not coincide with our opinion. He obſerves, "that, to fa-

" vour the recovery moft effectually, the " application of heat fhould be conducted " on the fame plan nature has pointed out " for torpid animals. It fhould be applied " very gradually and uniformly, and it " may be raifed to 98, but not further " than 100. When the body is warmed " uniformly, and the heat of the interior " parts about 98, we direct our attention " to the ftate of the thorax, and if the " patient make no attempt to infpire, we " proceed to inflate the lungs." Nor does this practice appear to be altogether in unifon with the Doctor's own theory on the nature of the difeafe, for external warmth can produce no chemical change on the blood; and as he afferts that the heart cannot act until a change has been produced, what great expectations can we form of its being attended with fuccefs. Moreover, this gradual application of heat muft engrofs no

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inconſiderable portion of time, already too precious, before the external heat can be much increaſed, and the action alſo of the muſcles of reſpiration could rarely be reſtored before that of the heart.

We alſo are obliged to withhold our aſſent from Dr. Goodwyn's opinion, where he ſays, that, whilſt the circulation of the blood continues, the temperature of the body may be raiſed many degrees above the natural ſtandard without inconvenience. To this aſſertion is oppoſed the reſult of Dr. Fordyce's experiments, which prove that upwards of two hundred degrees of external heat of Fahrenheit's ſcale could not raiſe the animal heat three degrees; and it may be a doubt, whether in theſe caſes *internal* animal warmth where life is preſent can ever be raiſed to 98 or 100 by the application of external heat.

The warmth of the body in health may be decreafed many degrees without much inconvenience; but never can be raifed more than three or four above the natural ftandard, without producing pernicious effects: nature therefore has prudently provided two powers for refifting heat, while fhe has given only one for generating it. That warmth is effential however I admit, and that in its application it fhould neither be violently nor irregularly increafed; but we can on no account deem it allowable to wait for any *increafe* of heat in the interior parts before the lungs are inflated, as it feems impracticable to increafe the living heat before this end is firft accomplifhed.

To regulate the application of this remedy, it does not appear neceffary to afcertain the degree of heat on the external furface of the body, and of the rectum,

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ſince we can always judge of the warmth of the atmoſphere within five or ſix degrees; and as water, whilſt in a fluid ſtate, muſt have its temperature nearly equal, we need only be cautious that the warmth of the room is not at firſt much greater. But as it may be ſome ſatisfaction to the Surgeon to know the degree of heat remaining in the body, (ſince the greater the degree of heat, the greater muſt be the irritability,) it may not be improper or unſatisfactory to be furniſhed with a thermometer, and Mr. Hunter's ſeems the only one that is well adapted to the purpoſe; ſince aſcertaining the heat of any part of the body, except in canals, cannot be of the ſmalleſt utility.

A thermometer ſeems alſo neceſſary for regulating the increaſe of heat, ſince our ſenſations are more likely to deceive us afterwards than at firſt; and it is of importance that the warmth be not very conſiderable: perhaps 70 degrees of Fahrenheits's ſcale is as much as ſhould ever be applied, ſince to ſupport any degree above this produces a waſte of ſtrength.

Warmth thus applied is certainly highly expedient, and its effects on the ſyſtem are probably theſe, that the blood, in drowning, &c. deprived of the greater part of the latent heat it imbibes from the air, becomes inſufficient to ſtimulate the ſolids; but, by the application of ſenſible heat to the ſurface of the body, the heat of the animal is prevented from being ſo ſoon carried off; and thus in ſome meaſure ſupplies the place of that latent heat which naturally is abſorbed by the blood; for, although

though heat be abſorbed from the air in a latent form, it is given out to the ſyſtem in a ſenſible one. Let it not, however, be underſtood that warmth is to effect a cure of itſelf; for we have repeatedly mentioned that the collapſe of the lungs has cauſed an obſtruction to the paſſage of the blood; and, before circulation can go on, this obſtruction muſt be removed, and the blood furniſhed with its uſual ſtimulus and change.

Various are the modes of applying heat to the body; warm bath, warm grains, &c. but theſe are means more eaſily directed than procured or put in execution; and there is only one advantage attending them, that of applying heat more univerſally. Even this is counterbalanced by a greater objection, as it prevents us from having recourſe to frictions, and permits ſuch a

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length of time to elapſe before either warm bath or grains can be procured.

The more adviſable method therefore appears to be, to place the patient on a mattraſs or bed at a proper diſtance from the fire, where every other operation that is thought proper can be carried on at the ſame time; and the readineſs with which warmth can be thus applied, muſt certainly be a convenience.

We propoſe next to inquire under what circumſtances frictions may be uſeful.

SECTION X.

Effects of Frictions.

It is with great propriety Mr. Kite has limited the uſe of frictions; at the *commencement* of the curative operations they muſt be productive of great miſchief; for the right ſide of the heart being already overloaded with blood, we are by the uſe of frictions increaſing its quantity: and it ſcarcely can be doubted but that this practice has contributed in many inſtances to fruſtrate the moſt ſuccefsful treatment, by producing an over diſtention, and conſequently indirect debility, of the right ſide of the heart. With a view to aſcertain the

the effects of early frictions, the following experiment was made.

EXPERIMENT.

A Cat was ſtrangled; and after it had ceaſed to breathe, the body and extremities were thoroughly rubbed for ten minutes, the cheſt was then opened. On examining the heart, the right ſide was found *fully* diſtended, and the left rather more ſo than uſual, without any ſign of action in either.

An opening was then made in the inferior cava, ſo as to let out a portion of blood; and the action of the right ſide of the heart was ſoon renewed.

This experiment was repeated, and it invariably reſulted, that the more the right ſide of the heart was diſtended, the

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weaker was its aἁion, and, even where no aἁion was evident during the diſten-tion, it was generally renewed by remov-ing part of the blood from the anterior cava.

It is, however, with friἁion as with electricity; if made uſe of at one time, it may tend to deſtroy life; and, at another, it may greatly aſſiſt in the recovery.

In our ſurvey of the common effeἁs of ſuſpended reſpiration, it was obſerved that the aorta and arterial ſyſtem contained a quantity of blood; this point being aſcer-tained, and it being likewiſe known, that the aἁion of the aorta and arterial ſyſtem is ſuſpended from a decreaſe of the due ſti-mulus in the blood, and that the veins have little or no contraἁile power of their own; when once the right ſide of the heart has been

been enabled to rid itſelf of a portion of its contents by the plan already mentioned, we ſhould then proceed to frictions, as a ſubſtitute to the natural action of the arteries in health, viz. that of propelling the blood onward, and producing a *vis a tergo* on the blood in the veins. The right ſide of the heart being thus in part emptied, is again diſtended by the application of frictions, which ſhould be continued as long as electricity is employed; but when, from any cauſe, we are prevented from electrifying, we ſhould be ſparing and cautious in the uſe of frictions, leſt by over diſtention we deſtroy the action of the heart. From frictions made uſe of as a general ſtimulant, little or no advantage can be expected.

The excoriations produced by the application of ſalt, brandy, volatile alkali, &c.

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may be exceedingly troublefome after recovery. This objection however fhould have but little weight were any real advantage to be derived from their ufe; but the application of ftimuli to the eyes, noifes to the ear, acrid liquors to the tongue and palate, fternutatories to the noftrils, fcarifications to the fkin, and the actual cautery, tend to extinguifh the little that remains of animal life, rather than to rouze or re-eftablifh it into action; for their effect on the nervous fyftem muft be fimilar to that of electricity when applied to the heart during the collapfe of the lungs, viz. the deftruction of irritability. The idea that fuggefted fuch applications muft have arifen from fuppofing the animal powers to be only in a ftate of torpor, without confidering that there exifted a caufe, without the removal of which all thefe attempts muft not only prove fruitlefs and abortive, but even de-

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ſtructive to life, much ſooner than if no remedy at all had been employed.

There appears to be an objection to the uſe of vitriolic acid with oil, or any application that produces an unknown and partial degree of heat. It may be preferable as a medium for friction, to make uſe of a little common oil or lard, which will rather prevent than occaſion excoriations, at the ſame time that it anſwers every other intention; for the principal end to be obtained by frictions is by means of their mechanical preſſure; and any medium that will facilitate this, appears preferable to thoſe applications which ſtimulate and generate heat. As much warmth as is deemed requiſite may be applied to the body by more certain and leſs diſagreeable means. Nor ſhould it be forgotten that the

the circulation even in health, is moſt languid at the remote parts of the body; conſequently the frictions ſhould be chiefly applied to the upper and lower extremities, and the body ſhould be occaſionally rubbed, where it does not interfere with the electric ſhock.

We ſhall next examine into the effects of Enemas.

SECTION XI.

Effects of Enemas.

As tobacco thrown up the rectum in the form of ſmoke was one of the firſt remedies employed in ſuſpended reſpiration; and as we ſee, to our great regret, that it is ſtill too frequently made uſe of; we ſhall endeavour, by a few animadverſions on its effects, to proſcribe its continuance.

Mr. Kite, I believe, was the firſt who reprobated the uſe of tobacco; and the arguments he adduces in ſupport of his opinion are truly ingenious.

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The hiſtory of medical errors ſcarce affords an inſtance of a more blind and obſtinate prejudice, than that which ſtill induces the Humane Society to recommend a mode of practice ſo obviouſly deſtructive. It is actually exhibiting a poiſon, that acts, as moſt other vegetable poiſons do, by producing ſuch an extreme degree of debility as no powers of life can ſupport; and there can be ſcarce any raſhneſs in affirming that ſuch quantities of tobacco have been adminiſtered in this diſeaſe in the form of ſmoke, as would have deſtroyed any man in full health. And indeed can there be any thing more evidently improper than ſuch a practice? We might with as much propriety recommend tobacco in ſyncope, or in a typhus fever, as in ſuſpended reſpiration from drowning, &c. nor can there be the leaſt doubt entertained of the effects it would produce in either of theſe diſeaſes.

When we conſider the effect that a drachm of tobacco in infuſion has upon the ſyſtem, when given ſolely to produce a temporary debility in hernia, &c. one would ſcarcely credit that any perſon acquainted with this effect, could ever think of adminiſtering eight or perhaps twelve times this quantity, when the powers of life are reduced to their loweſt ebb. It is really an indelible ſtigma on the profeſſion, that, while we cannot but obſerve the deleterious tendency even of a ſmall quantity of it, in one diſeaſe, where we wiſh to reduce the ſtrength; we nevertheleſs employ it by wholeſale in another, where ſcarce a ſpark of life remains unextinguiſhed. With headſtrong inattention we have perſevered in its uſe, without ever aſking ourſelves this neceſſary queſtion— What are we rationally to expect from

ſuch

ſuch a remedy? This indeed, is quackery in the higheſt degree.

When examining the effects of medicines thrown into the ſtomach after reſpiration had ceaſed, it was found that their action was far leſs powerful than when adminiſtered in full health; and it is a fortunate circumſtance indeed, that their operations are regulated by ſuch a law; for, if medicines produced the ſame effect in this diſeaſe as during the unimpaired vigour of the natural functions, it may without heſitation be declared, that no one could ever have been recovered where tobacco had been employed in quantities equal to what has been recommended. Tobacco injected into the ſtomach will of courſe produce more violent effects than when thrown up the rectum; but when the quantity employed is perhaps equal to three or four ounces, the effects muſt be as violent, if not more ſo, than a ſixteenth part injected into the ſtomach.

In order more accurately to determine the effects of tobacco enemas, the following experiment was made.

EXPERIMENT.

A full grown Cat was drowned, and the cheſt being immediately opened, the heart was obſerved to act ſtrongly; ſix drachms of tobacco were thrown up the rectum in the form of ſmoke, but before the herb was half conſumed, there remained ſcarcely any action in the heart; and after the whole had been injected, the contractions totally ceaſed. Mr. Kite has ſubſtituted in the place of tobacco, ſome aromatic herb; and, if we are to make uſe of gliſters at all, herbs of this nature are certainly preferable: but no very great advantages can rationally be expected from any applications to the rectum.

If the diſeaſe is not removed by the means before laid down, we may with as much confidence expect a recovery from injecting a little warm milk and water into the ſtomach, as from the injection of enemas of any kind.

It ſhould alſo be remembered that enemas ought to be ſmall in bulk, in order to render them innoxious; for ſmoke and fluids of all kinds, when given in large quantities, will diſtend the inteſtines; the reſult of which will be, that their mechanical effect in preventing the eaſy deſcent of the diaphragm, will neceſſarily be productive of miſchief. Warm enemas may have the ſalutary effect of ſlightly ſtimulating the inteſtines; and the heart alſo, from ſympathy, may poſſibly have its action in ſome ſmall degree increaſed; but if tobacco be employed, the oppoſite effect muſt ariſe. And as ſympathy is ſuppoſed to be greater between the heart and the ſtomach, than between the heart and inteſtines, it were better to inject ſome warm aromatic into that viſcus, than into the rectum; but inflation, electricity, and frictions, ought by no means to be neglected to make room for ſo ineffectual a remedy.

Having examined the merits of the remedies uſually employed in ſuſpended reſpiration, and recommended ſuch as are

countenanced

countenanced by inquiry and experiment, it may not be deemed unneceſſary to ſubjoin an account of the method of conducting the treatment.

SECTION XII.

Method of Cure.

THE plan of treatment generally to be purſued has been laid down ſomewhat at large in ſeparate ſections; but it may not be unſatisfactory to the practitioner, in theſe caſes, to ſee the whole contracted into an abridged form, and placed in a nearer and cloſer point of view.

As a few minutes in this diſeaſe make a material difference as to the probability of recovery, we think it of ſufficient importance to remark, that the electrical machine and the apparatus for artificial reſpiration, ſhould be kept always at hand, and in readineſs.

No

No more ſpectators ſhould be allowed to be preſent than are abſolutely neceſſary; which we conceive may be eight or nine in all, including the Medical Aſſiſtants; two to have the direction of the cheſt, one to turn the electrical machine, one to direct the ſhock, four to apply the frictions, and the other to aſſiſt occaſionally. This number will be ſufficient for anſwering every purpoſe, and a greater number would rather embarraſs, and only contribute to render the air leſs fit for reſpiration.

The body, if wet, ſhould be gently dried with cloths, but in ſuch a cautious manner, as to prevent the mechanical effect of the friction from propelling the blood towards the heart.

Having prepared the bed, or mattraſs, on on a table of convenient height, the body is to be placed on it with the head a little elevated. Five or ſix ounces of brandy, rum, or ſome other warm aromatic, ſhould be thrown into the ſtomach, by means of the vegetable bottle and pipe; and the ivory director paſſed to the farther part of the mouth, ſo as to cloſe the ſuperior aperture of the œſophagus.

If the patient ſeems plethoric, and more particularly if the diſeaſe has been occaſioned by hanging, bleeding ſhould be employed, and that as one of the firſt remedies; nor ſhould the application of a proper degree of warmth be neglected.

The curved pipe being then introduced into the trachea, and ſecured by an aſſiſtant, and the flexible tube, &c. being attached, the lungs ought as ſoon as poſſible

to

to be inflated; and the electrical machine being prepared, one director is to be applied between the fourth and fifth rib of the left fide, and the other between the fecond and third of the right; when the electrometer is to be placed a little more than one third of an inch from the jar, and the ftroke given. The electrical fhock is to be repeated once or twice, and the affiftant, who prevented the air from efcaping by the noftrils and mouth, then fhould remove his hands, and prefs the cheft, and immediately after expand the lungs, for the heart to be again ftimulated.

If any impediment fhould prevent the introduction of the pipe down the trachea, the thyroid cartilage fhould be immediately divided in the manner defcribed in Section the Eighth, and the curved pipe inferted into the trachea at this aperture.

When

When the lungs have been three or four times expanded and collapfed, frictions are to be had recourfe to; thefe, together with the procefs of expanding the lungs, (and at the fame time electrifying the heart,) and then again collapfing them, are to be continued four hours without intermiffion, unlefs natural refpiration be reftored.

In fome cafes where the living powers are remarkably languid, it may be advifable to continue the ufe of electricity, and gentle frictions, even after refpiration is renewed, as there have been inftances of momentary and tranfient recoveries: the ill fuccefs of which may be conceived to arife either from an improper ufe of tobacco, or the heart not poffeffing fufficient irritability to carry on the circulation, or from want of a fupply of blood to the right fide of the heart after it has been once emptied. Thefe obftacles may probably be removed by affifting the heart and arteries to perform their refpective functions, after the mufcles of refpiration have been ftimulated to action.

If unfortunately no electrical machine be at hand, the method of performing artificial refpiration fhould be altered. When the lungs are expanded, the affiftant, who has the charge of the mouth and noftrils, fuffers a fmall quantity of air to efcape, while the other affiftant continues to throw in a frefh fupply : this procefs fhould be protracted for about a minute, when the hand is to be removed from the mouth, and the cheft preffed, to complete the collapfe. It cannot be too frequently inculcated, that the lungs are never to be fuffered to *remain* collapfed ; for all our endeavours and attempts to effect a recovery, fhould the lungs

be

be permitted to continue in that ſtate, muſt ultimately prove fruitleſs.

We cannot better conclude the preſent diſſertation, than by briefly recapitulating the principles and obſervations which form its baſis and ſupport.

CONCLUSION.

FROM what has been obſerved it appears,

1. That in *ordinary* reſpiration and circulation the lungs are paſſive.

2. That the principal advantage derived from reſpiration, is that of its being the ſource of animal heat; and this heat, by being evolved in a ſenſible form, keeps up the irritability of the whole animal.

3. That the blood imbibes leſs or more latent heat, in proportion to the degree of ſenſible warmth applied to the ſurface of the body.

4. That the temperature of the florid blood in the left ſide of the heart is at firſt lower than that of the right; but its ſenſible heat ſoon becomes greateſt.

5. That this circumſtance favours the idea of heat being abſorbed from the air in the act of reſpiration.

6. That as ſoon as the blood has undergone the change in the lungs, it is rendered fit to ſupport the heat and irritability of the animal.

7. That heat is not only evolved from the blood as it paſſes through the capillaries, but that the ſame proceſs continues throughout the whole circulation.

8. That the ſtimulus which excites the heart to act, is the ſame in all its cavities; and this principally is diſtention.

9. That

9. That in the fœtus both ſides of the heart act from the ſtimulus of black blood.

10. That the intent of the fœtal and adult change is the ſame, viz. that of ſupporting animal heat and irritability.

11. That this change is effected in the fœtus, by the blood paſſing through the cells of the placenta, and the veſſels coming in contact with the maternal arterial blood.

12. That ſo much of the venous quality is imparted to the maternal from the fœtal blood, and *only* ſo much latent fire evolved from the maternal to the fœtal, as is neceſſary to reſtore the equilibrium of abſolute heat.

13. That as the fœtus is ſurrounded by the warm medium of the liquor amnii and mother, very little heat can be conſumed, and therefore an abſorption of heat equal to that of the adult is not neceſſary.

14. That the fœtus poſſeſſes only one power of reſiſting heat, and as the heat to be imbibed by the fœtal blood is always limited, and as it is always ſurrounded by an uniform degree of temperature, the fœtus ſtands in no need of the power of reſiſting heat, or generating cold by evaporation.

15. That the fœtal heart contains only a ſmall portion of blood that has been to the placenta; and as this blood can receive only a partial change, and as even the greater part of that ſame blood muſt firſt paſs through the capillaries before it arrives

at

at the left auricle and ventricle; and as that which does not paſs through capillaries mixes with venous blood, it follows that the left ſide of the fœtal heart contracts from the ſtimulus of black blood.

16. That as all the blood which paſſes through the lungs muſt enter the left auricle, the latent heat of the fœtal blood in the right ſide muſt exceed that of the left.

17. That the blood in the umbilical arteries which is to receive the change, being of the ſame quality with that in the left ſide of the heart, is an additional proof that this blood muſt be black.

18. That although the blood in the fœtal heart and arteries be black, yet, like the blood of the adult in the right ſide of the

 heart

heart and pulmonary artery, it muſt ſtill poſſeſs a portion of latent fire, which it continues to evolve, to keep up the temperature and irritability of the parts, through which it paſſes.

19. That during the act of drowning the animal emits air from its lungs, and in its attempts to inſpire, a ſmall quantity of water enters the lungs and ſtomach.

20. That, after the laſt expiration, the lungs are found nearly collapſed, containing a ſmall quantity of froth, but very little air.

21. That the quantity of blood found in the right ſide of the heart is nearly double that contained in the left.

22. That the blood contained in both

ſides of the heart is of the colour of venous blood.

23. That, whether the heart be examined during its contractions, or after they have ceaſed, no perceptible difference is found in the proportions of blood in the two ſides of the heart.

24. That the action of the heart ſurvives the periſtaltic motion of the bowels.

25. That the veſſels of the head exhibit no extravaſation, nor even diſtention.

26. That where reſpiration is ſuſpended, from ordinary hanging, the animal has the power of expelling air from its lungs.

27. That although the muſcles of

expiration perform their office, no power can be applied 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.

29. That the quantity of blood in the two fides of the heart bears nearly the fame proportion in hanging as in drowning.

30. That there is very little difference in the continuance of the irritability of animals after hanging, from its continuance after drowning; but the veffels of the head are fomewhat diftended in the former.

31. That animals immerfed in impure

air do not appear to make a full inſpiration, but like animals immerſed in water reject it, as ſoon as a ſenſation is produced in the trachea, which ſeems to make them conſcious of not breathing their uſual element.

32. That the muſcles of expiration continue to act till they have expelled all the air from the lungs, which they have the power of acting on.

33. That the ſame collapſe of the lungs is produced from ſuffocation, as from drowning or hanging; and the contents of the right ſide of the heart bear nearly the ſame proportion to thoſe of the left.

34. That animals deſtroyed in impure air are ſooner deprived of their irritability

than

than when refpiration is fufpended from drowning or hanging.

35. That animals deftroyed by nitrous air foon grow ftiff and inflexible, fometimes even before the heart has ceafed to vibrate.

36. That the veffels of the head contain lefs blood after fuffocation from impure air, than after hanging.

37. That in fufpended refpiration from any caufe, the right fide of the heart continues to act after the left has ceafed.

38. That the caufe of this difference is not that the left fide of the heart is incapable of being ftimulated by black blood; but from this blood being effentially dif-

ferent

ferent in *quality* and *quantity* from that of the right.

39. That this difference of *quality* in the blood of the left ſide of the heart depends on its having paſſed through the lungs, and imparted to them a conſiderable portion of its heat, without receiving a ſupply from reſpiration; while the blood of the right poſſeſſes a quantity of fire in a latent form, which it ſtill continues to evolve.

40. That as the blood in the right ſide of the heart contains a portion of latent fire, while that of the left is exhauſted; and as the ſenſible heat both of the right auricle and ventricle muſt conſequently predominate, its irritability of courſe will likewiſe be greater.

41. That the ſtimulus of diſtention being greater in the one than in the other, will tend to produce a difference of action.

42. That as the right ſide of the heart poſſeſſes more irritability in this diſeaſe than the left; and as the ſtimulus of diſtention is alſo more powerful at the right ſide than at the left, its action will be continued when no effect is produced on the other.

43. That although the heart may derive its heat in health principally from the blood in the coronary veſſels; yet the blood in the cavities of the heart will be alſo capable of evolving heat, and more eſpecially when ſtagnation takes place in ſuſpended reſpiration.

44. That *if the right ſide of the heart*

poſſeſſed

possessed the blood of the left, and the left the blood of the right, the degree of irritability would probably be reversed.

45. That if the right side of the heart in suspended respiration had the irritability of the left, and the left the irritability of the right, a recovery could scarcely be effected in any instance.

46. That as soon as the action of the left side of the heart is increased by the stimulus of florid blood, the right also acts more powerfully.

47. That this depends on the coronary vessels being supplied with blood, that has received a quantity of latent fire from the air, which these vessels distribute alike to the right and left side, and consequently give

give an equal increaſe to the irritability of both.

48. That the heat and irritability of the heart, being then the ſame, the ſtimulus of diſtention will produce an equal action.

49. That the heart can be made to act after reſpiration has ceaſed, from the ſtimulus of electricity, while no action can be excited in the external parts from the ſame cauſe.

50. That as electricity is capable of producing action in the heart, when it has no effect on the exterior parts, and as life actually exiſts at this period; it might lead to moſt pernicious conſequences to conclude that life was totally extinct, from

no

no external aƈtion being produced by elec-tricity.

51. That as the difference of irritability in animals of the ſame order depends more on the ſpecific ſtate of the ſolids than on the quantity of heat evolved from the fluids, no deciſive prognoſtic can be drawn of the preſence of irritability, from the heat of the animal being above that of the atmoſphere.

52. That as eleƈtricity has been found capable and incapable of producing external aƈtion during the preſence of various de-grees of heat, it proves that animal heat and evident irritability are by no means coequal.

53. That although no ſpecific degree of heat will determine the preſence or abſence

of

of irritability, yet the greater the degree of heat, the more will be the irritability of *any particular animal.*

54. That as the heart is confidered as the origin of circulation, there is a proba-bility of recovery, fo long as the heart can be made to act; although external irri-tability may not be manifefted by the teft of electricity.

55. That it will ever be better to have no criterion to judge of the abfence of life, and make ufe of every means of recovery, in every inftance, than to rely on an im-perfect and hazardous prognoftic.

56. That when the lungs are inflated foon after the laft expiration, both fides of the heart will immediately act.

57. That this probably proceeds from the irritability of the heart being ſtill ſo great as to be ſtimulated to action by the mechanical preſſure of the lungs, as, in proportion to their expanſion, will their ſurface preſs upon the two ſides of the pericardium.

58. That in ſuſpended reſpiration, from drowning, hanging, and ſuffocation, as the collapſe of the lungs begins, the impediment to the paſſage of the blood through them commences.

59. That when the laſt expiration is made, the interior pulmonary veſſels are collapſed, and contain but a ſmall quantity of blood.

60. That if even a change be produced on the quality of this blood, the quantity

is ſo ſmall, that unleſs the right ſide of the heart be firſt excited to action the motion of the lungs alone will be unable to propel this blood into the left.

61. That by inflating the lungs, we cannot alter the quality of the blood in the trunks of the pulmonary veins and left auricle.

62. That the right ſide of the heart can propel blood to the left, immediately after the laſt expiration, independent of the mechanical action of the lungs.

63. That as the heart can perform this function after reſpiration has ceaſed, it is probable that the lungs have naturally no active power of propelling the blood onward.

64. That part of the black blood contained in the left auricle and ventricle in this diſeaſe, muſt be propelled through the ſyſtem unaltered whenever a recovery is effected, and as a quantity of blood of the ſame quality has already paſſed into the arterial ſyſtem, it follows that the left auricle can and does act from the ſtimulus of black blood.

65. That as an animal, when immerſed in warm water, may be drowned with its blood ſomewhat florid, it neceſſarily furniſhes an objection to the opinion, that the action of the left heart ceaſes from the preſence of black blood.

66. That as in drowning, &c. the impetus of blood to the head is checked immediately after the obſtruction to its

return takes place, no injury whatever can happen to the brain.

67. That if apoplexy did actually take place, we ſhould never be able to bring about recovery after reſpiration had once ceaſed, ſince we frequently fail of removing common apoplexy during the exiſtence of reſpiration; and as, in drowning, &c. we find no extravaſation in the head, if apoplexy were to exiſt, it muſt be ſolely attributed to the diſtention of the veſſels.

68. That as mere diſtention is capable of bringing on only a very mild ſpecies of apoplexy, which does not for many hours, and ſometimes for many days, produce its fatal effect; and as, on the contrary, apparent death from drowning, hanging,

and ſuffocation, takes place in a few minutes; it certainly follows that this diſeaſe and apoplexy are as eſſentially different as any two diſeaſes to which the human body is expoſed.

69. That the immediate cauſe of the ſuſpenſion of circulation is not *the preſence of black blood in the left ſide of the heart*, neither is it the *want of motion in the lungs*; but a *collapſe of the air cells of the lungs, which produces a mechanical obſtruction to the paſſage of the blood in the ſmall branches of the pulmonary veſſels.*

70. That the proximate cauſe of the diſeaſe may be ſaid to conſiſt in a collapſe of the lungs, producing a collapſe of the pulmonary veſſels, with want of latent heat in the blood, ſince, unleſs both theſe ef-

fects are removed, the diſeaſe will ſtill continue.

71. That the mechanical obſtruction from collapſe muſt firſt be removed, before the chemical effects can take place.

72. That emetics in this diſeaſe, before the circulation is re-eſtabliſhed, are improper.

73. That even then they ſhould only be exhibited where the ſtomach is known to have been overloaded previous to the accident that produces the diſeaſe.

74. That all medicines introduced into the ſtomach, produce a leſs effect after reſpiration has ceaſed, than during the healthy actions of the animal; and that, in

in this difeafe, it appears neceffary to inject fome warm cordial, fuch as brandy, &c. into the ftomach.

75. That as in fufpended refpiration, from hanging, there is fome degree of plethora in the head as well as in the right fide of the heart, bleeding will then be more frequently neceffary than after drowning and fuffocation.

76. That the degree of plethora in the head will greatly depend on the weight and bulk of the fubject.

77. That as in drowning and fuffocation the plethoric ftate of the right fide of the heart cannot be relieved by bleeding, this operation fhould never be performed unlefs it appears that there is too much blood in the fyftem.

78. That when bleeding is judged requiſite, it ſhould be performed on the jugular veins in preference to any other.

79. That when bleeding is deemed expedient, it ſhould be one of the firſt remedies employed.

80. That ſhocks of electricity paſſed through the heart, brain, and ſpinal marrow, without the collapſe of the lungs being at the ſame time removed, muſt tend rather to deſtroy than reſtore the actions of life.

81. That imitating natural reſpiration without frequently producing a collapſe of the lungs is improper, as the diſtention of the pulmonary veſſels occaſioned by the action of the right ſide of the heart

will

will form great obſtruction to the paſſage of the blood.

82. That the uncertainty of ſucceſs which has hitherto attended the caſes reported by the Humane Society, has probably been in a great meaſure occaſioned by the method of conducting the artificial reſpiration.

83. That the advantages to be derived from artificial reſpiration are, to procure a contraction of the right ſide of the heart when the lungs are dilated, and, by collapſe, to excite the left auricle to get rid of a portion of its contents, and ſupply it with blood that has received its ſtimulus from the air.

84. That in order thoroughly to accompliſh this end, we are to expand the

lungs, and, when expanded, to ſtimulate the heart by a ſmall ſhock of electricity; we are then to collapſe them, and again to inflate.

85. That from this mode of proceeding, if any irritability remain in the heart, the right auricle and ventricle may be ſtimulated to act, and propel ſome of its blood into the lungs, where it meets with no obſtruction, and air to impart to the blood its due ſtimulus and heat: the blood thus duly changed may, by means of the expiration, excite the left auricle to get rid of its burden, and furniſh a freſh ſupply endued with a proper ſtimulus.

86. That if no electrical apparatus be in readineſs, the method of conducting artificial reſpiration ſhould be altered.

87. That

87. That the lungs ſhould be diſtended, and, after allowing a ſmall quantity of air to eſcape, the inſpiration ſhould be repeated; and this proceſs, of ſuffering, after each inflation, a little air to eſcape, (or, in other words, imitating natural reſpiration,) ſhould be continued about a minute, when the lungs are to be collapſed; ſo that only one complete expiration ſhould here be made to ſeveral inſpirations.

88. That the intention of this practice is, that as the heart may poſſibly not contract more than twice or thrice in a minute, the blood may find a free paſſage whenever it happens to act, and a freſh ſupply of air to produce the neceſſary change; and likewiſe that theſe ſeveral inſpirations may act as a ſtimulus to the heart, while the collapſe helps to remove the blood the lungs have received.

89. That

89. That during the whole procefs of the treatment the lungs fhould never be fuffered to *remain in a collapfed ftate* for a fingle minute.

90. That electricity fhould never be employed on any account without a concomitant expanfion of the lungs.

91. That if the heart be excited to act during the collapfe of the lungs, very little more blood can pafs through them than paffes in the fœtal circulation, and even this fmall portion receives no benefit from the air.

92. That the application of warmth is neceffary, and that when firft applied fhould be about fix degrees above that of the open atmofphere, if this be below 70 of Fahrenheit.

93. That

93. That we are on no account, as advifed by Dr. Goodwyn, to wait for any increafe of animal heat before the lungs are inflated.

94. That placing the body on a mattrafs or bed, at a proper diftance from the fire, is the beft mode of applying warmth; as it neither embarraffes nor prevents any other procefs that may be found expedient.

95. That the principal advantage to be expected from the application of warmth is, to prevent fo much fenfible heat being evolved from the blood, and thus in fome meafure fupply the defect of latent heat that fhould have been abforbed from the air.

96. That frictions made ufe of as a primary

primary remedy are highly improper, as they tend to deftroy the action of the heart, by promoting an over diftention of the right fide.

97. That frictions fhould never be employed before the lungs have been feveral times expanded and collapfed.

98. That after the heart has been in part emptied, frictions are abfolutely neceffary.

99. That a little common oil or lard, as a medium for the frictions, is preferable to either falt or fpirits, or any other ftimulating application.

100. That the principal effect to be expected from frictions is, their mecha-

nical

nical aĉtion in propelling blood towards the right ſide of the heart.

101. That tobacco in any form is highly pernicious; and were this medicine to produce the ſame baneful effeĉts in caſes where reſpiration is ſuſpended, as in a ſtate of health, it is more than probable that no one could ever have been recovered where this remedy had been applied.

102. That enemas of any kind are only to be conſidered ſerviceable, in as much as they co-operate with more important remedies; and, if employed at all, warm ſtimulating ones ſhould be preferred.

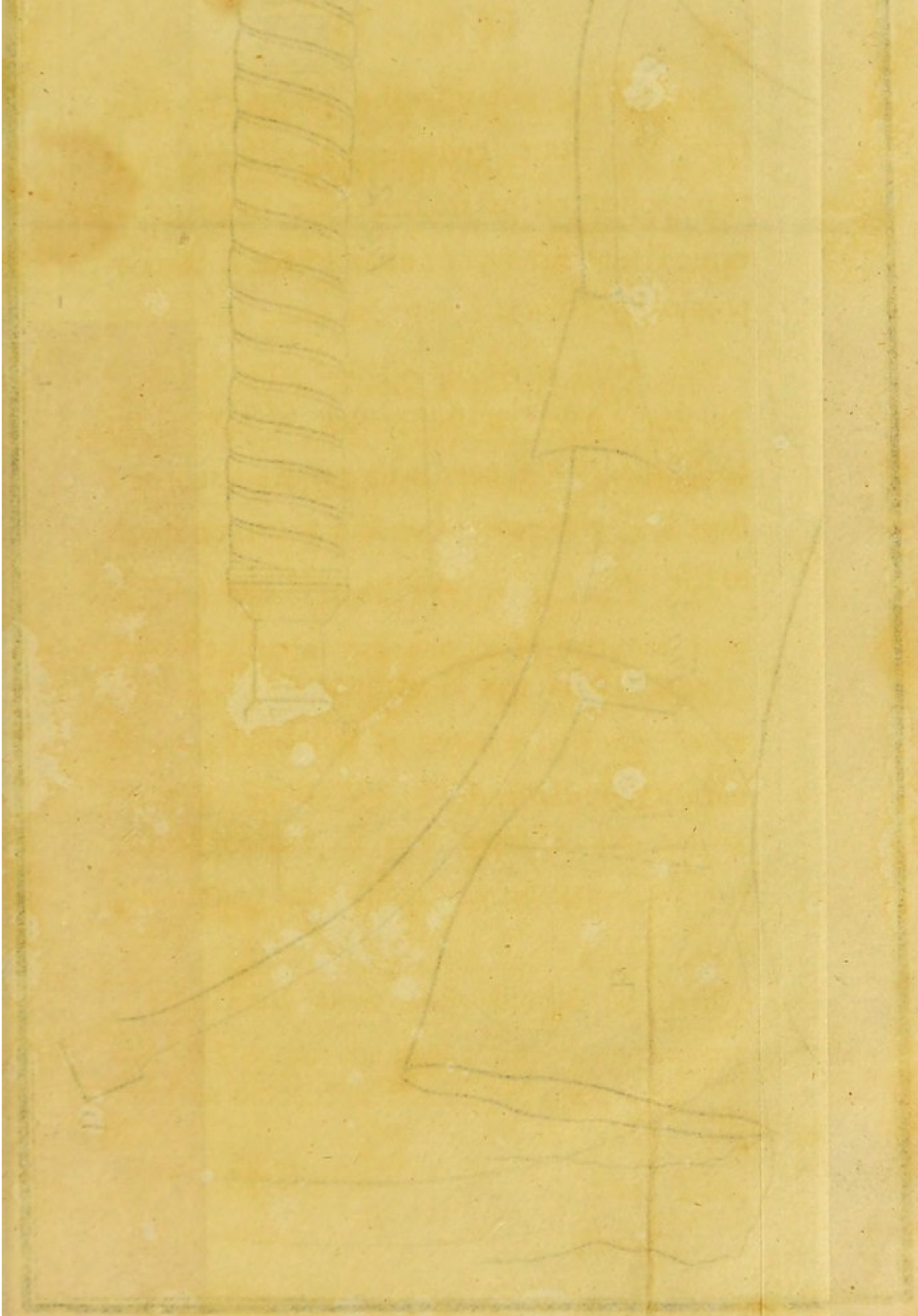
103. That their bulk ſhould be ſmall, leſt by their mechanical aĉtion they prevent the free deſcent of the diaphragm.

104. That

104. That inflating the lungs, electrifying the heart, collapfing the lungs, and the application of frictions, are to be continued four hours, if our endeavours be not previoufly crowned with fuccefs.

105. That electricity and frictions are to be continued even after refpiration is reftored, if the powers of life feem unequal to the tafk.

106. That the final intention of the whole plan of treatment is, *to imitate the natural circulation*.



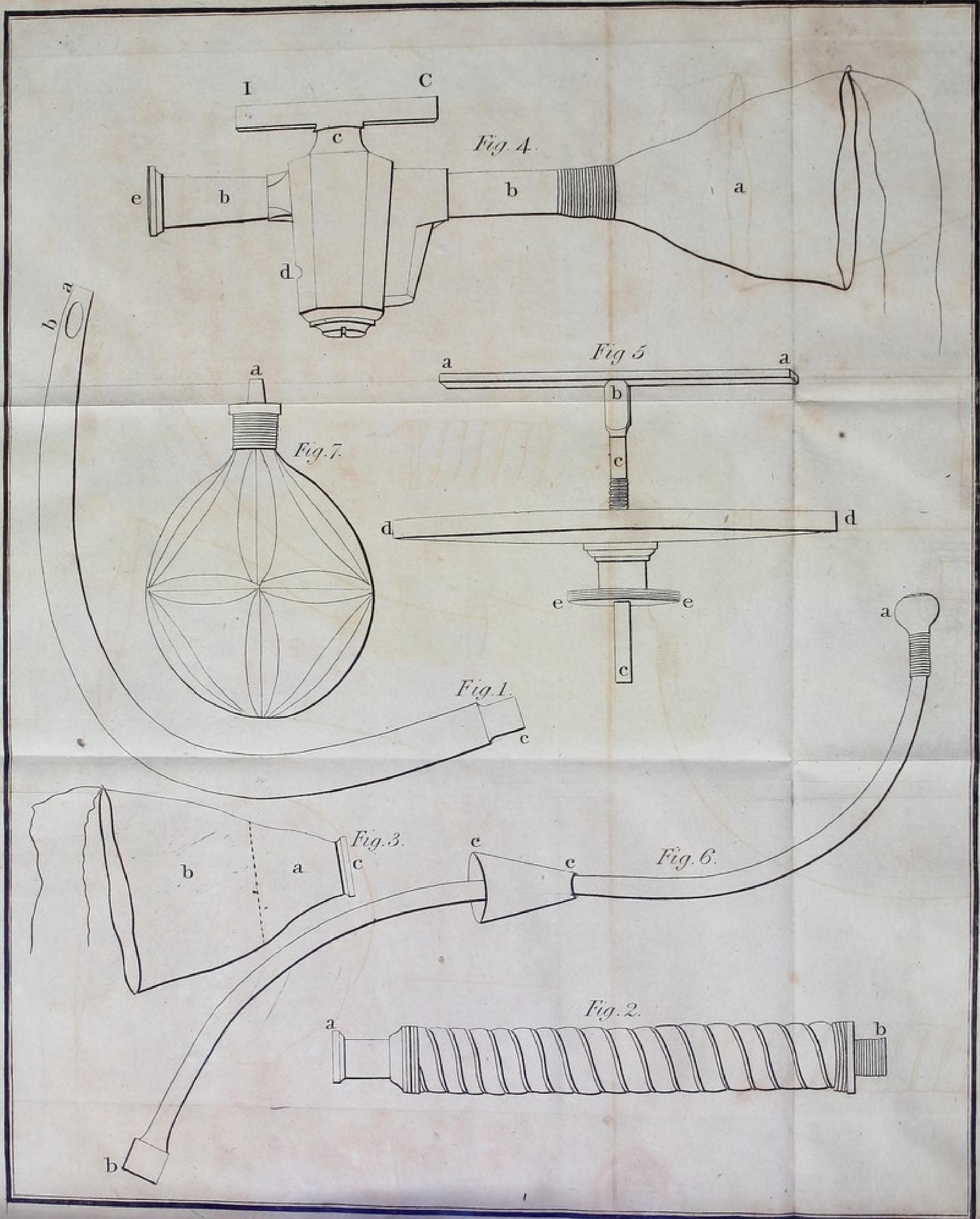


Fig. 4.
I
C
c
e
b
b
a
d
Fig 5
a
a
b
c
d
d
e
e
c
Fig. 7.
a
b
a
Fig. 1.
c
Fig. 3.
b
a
c
Fig. 6.
c
c
a
b
Fig. 2.
a
b

DESCRIPTION OF THE PLATE.

Fig. I.

A large ſilver conical pipe to be introduced into the trachea, either by the mouth, or by an opening made in the thyroid cartilage. A. the inferior extremity; B. two lateral openings for the paſſage of the air; C. the ſuperior end of the pipe.

Fig. II.

A ſhort flexible tube for conveying air into the lungs. A. the inferior extremity to be attached to the ſuperior one of the ſilver pipe; B. the other extremity to be connected with the contrivance (Fig. 3. at C.) or attached to Mr. Field's inſtrument (Fig. 4. at E)

Fig. III.

Repreſents a ſhort conical braſs tube connected with a conical piece of leather, to receive the nozzle of any pair of bellows, and by means of packthread to retain it in its ſituation. A. the braſs; B. the leather portion; C. a female ſcrew to admit the ſuperior extremity of the flexible tube.

Fig. IV.

Mr. Field's inſtrument for inflating and collapſing the lungs. A. is a conical leather tube to be attached to any pair of bellows; B B. is a braſs tube; C. is a ſtopper to the cock, in which there are two valves opening in contrary directions; D. is an aperture through which all the air is to paſs to and from the bellows, (the valve of the bellows being previouſly cloſed by another

other

other inſtrument repreſented in the next figure); E. the inferior extremity of the braſs tube to be connected with the ſuperior end of the flexible tube (at B.). When the ſtopper ſtands as is here repreſented with I (ſignifying Inflation) pointing to the inferior extremity of the tube, the lungs will be expanded; and when the ſtopper is turned half round, ſo that C (meaning Collapſe) will be placed in the ſame direction, the lungs will be exhauſted.

In the one inſtance, by the action of the bellows, air is received at the aperture D, and thrown into the lungs, but prevented from regurgitating on account of the valve: in the other, air will be received from the lungs into the bellows, and thrown out at the aperture D.

Fig. V.

Is the invention for cloſing the valve of any bellows. A A. is a piece of iron to be inſerted into the valve-hole of any bellows, which, being placed acroſs, prevents its being drawn out; B. is a pivot on which the iron part C C. turns; D D. is a circular flat piece of wood (lined with leather) to cover the valve-hole, with an aperture in its centre, to admit the iron C C. through it; E E. is a braſs nut which is made to ſcrew on the iron C C. to retain the piece of wood in its ſituation.

Fig. VI.

A flexible tube (of the ſame compoſition as flexible catheters) to be introduced into the œſophagus, for conveying ſpirits, &c. into the ſtomach. A. the bulb and inferior extremity; B. the ſuperior; C C. is a conical piece of ivory, to be paſſed

paſſed a ſmall way down the œſophagus by the forefinger, to prevent air eſcaping into the ſtomach, and as a guide for the introduction of the ſilver pipe into the glottis, when bronchotomy has not been performed.

Fig. VII.

Is a vegetable bottle for injecting fluids into the ſtomach through the flexible tube. A. the mouth of the bottle to be attached to the extremity of the flexible tube at B.

THE END.

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