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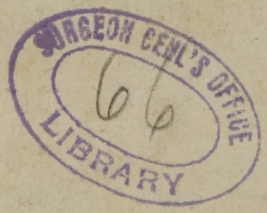

Jackson (S.)

The Professor

AN ESSAY

ON

SUSPENDED ANIMATION.

BY SAMUEL JACKSON,

HONORARY MEMBER OF THE PHILADELPHIA MEDICAL, AND MEMBER OF THE PHILADELPHIA LINNEAN SOCIETIES.

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PHILADELPHIA.

1808.

AN INAUGURAL DISSERTATION,

FOR

THE DEGREE

OF

DOCTOR OF MEDICINE,

SUBMITTED

TO THE EXAMINATION

OF THE

MEDICAL PROFESSORS,

OF

JAMES M'DOWELL, LL. D. PROVOST,

AND OF

THE TRUSTEES

OF THE

UNIVERSITY OF PENNSYLVANIA,

ON THE 28TH DAY OF APRIL, 1808.

398639

TO

CASPAR WISTAR, M. D.

ADJUNCT PROFESSOR OF ANATOMY AND MIDWIFERY

IN THE

UNIVERSITY OF PENNSYLVANIA;

A MAN,

WHOSE AMIABLE QUALITIES HAVE ENDEARED HIM

TO A LARGE CIRCLE OF ADMIRING FRIENDS;

A PHYSICIAN,

WHOSE TALENTS HAVE GAINED HIM

THE CONFIDENCE AND RESPECT OF A COMMUNITY,

THIS EPHEMERAL PRODUCTION

IS INSCRIBED,

AS A

SMALL TRIBUTE OF RESPECT

FOR EVERY VIRTUE WHICH ADORNS AND EXALTS THE HUMAN CHARACTER,

AND AS A MARK OF HEARTFELT GRATITUDE,

FOR MANY INSTANCES OF ALMOST PARENTAL KINDNESS,

EXPERIENCED WHILE UNDER HIS TUTELAGE,

BY HIS AFFECTIONATE PUPIL,

SAMUEL JACKSON.

INTRODUCTION.

OLD Age, if not the only natural outlet of death, as Bacon asserts, before Man forgot the God that made him, and became the unrestrained follower of vice, was most probably the principal portal through which life issued. How changed the scene! Existence has become the tenure of uncertainty; each gale bearing on its wings the seeds of destruction, while death suspends its sword each instant o'er our heads. For although youth may lead us exulting with the fulness of life, and bounding with joy, along its verdant paths, though Health circles our brow with its rosy wreaths, we are uncertain but that a moment separates us from eternity. This enemy of man lurks in secret around; he appears not always in the horrid garb of disease, to warn us of his approach; but while the soul revels in the luxuries of anticipating fancy; while the soft smiling countenance of hope attracts our regards; or ambition, as a dazzling meteor, absorbs attention, his icy hand touches our hearts; we cease, ere our day has run its course, and still warm with imagination's glowing fire, are inclosed in the cold and silent tomb. So enervated has life been rendered, and so numerous are the deathful instruments, which disease now marshals in its ranks, from the follies, the vitious propensities, and the crimes of men!

To diminish this accession to disease, and to guard these avenues of death, that man may live nature's originally allotted span, is the object of medical science, the constant, the unvarying effort of every philanthropick physician. But while he regards the more dreadful battalia, he must not neglect the sudden, unexpected, and therefore too frequently fatal attacks

of his watchful foe. Those casualties, which are so often irremediable, if they cannot be prevented, he should endeavour to render innocent. Of these, few are a more frequent cause of destruction to existence, than that of drowning. Each year does society lament that it is, by this means, deprived of its proudest boasts, its brightest ornaments, or its humble supports. A family, in a few fatal moments, too frequently loses the support and protection of a father, is precipitated from the comforts of competence, to the distresses of poverty, and left to the cold charity of a pitiless world, from his term of being, prematurely, and untimely ending by this too common accident. Often does a fond mother, while distraction paints her looks, despair of the coming of her wandering child. He returns no more, or is given to her arms a lifeless corpse, the victim of the rapacious wave.

As the physician has not fulfilled his duty, when he has only defended from the aims of death; or stretched forth his hand, and raised a suffering being from a bed of disease, of anguish, and of pain; but as he is to render secure the life he has protected; as he is to make it a blessing to its possessor, that it may not be a burden to a "soul heavy laden" with distress, he is to close this avenue of death, and to dry this source of misery.

AN ESSAY, &c.

IN the first dawn of physiological learning, it might perhaps have appeared vain and presumptuous, to attempt restoring to animation an apparently dead body. But as we now know that the characteristick signs of life and death, are obscure, (if we except putrefaction) and that no one has been able certainly to pronounce, this is vitality, or that is death; as we are also absolutely certain, that the more obvious appearances of life may be suspended, without its being annihilated, it becomes an act of criminalness to neglect any effort to resuscitate in cases of the sudden suspension of vital action. Shall we refuse to rake the warm ashes for a spark, which may rekindle to a genial blaze, and give new vigor to a sinking frame?

That life can exist, as it were, in a latent state, for sometime, and still remain capable of being roused into action, and to produce all its phenomena, is established by facts so numerous and incontestible, as to render the attempt to prove it superfluous. It was, from an observation of the spontaneous recovery of drowned persons, which, in Holland, first led to the practice of endeavouring to revive those, who suffered by this acci-

dent. The success, which crowned the first attempts, induced the philanthropists of that country, to institute humane societies. This laudable example was soon imitated in the different nations of Europe, by which means, has humanity frequently gained a triumph over death.

It must, however, be acknowledged that all efforts at resuscitation, are often ineffectual; even in cases in which submersion has not long existed. It becomes, then, a question of much interest and importance, to be answered, whether in these instances, the failure is not to be attributed to a defect in the method now employed for recovery, or to a total extinction of life.

The Humane Society of this and other cities, and also many respectable physicians, appear to be of the opinion, that more persons might be recovered, and that a plan of greater efficacy, than the present might be adopted to effect this noble object. Actuated by this sentiment, the Philadelphia Humane Society, highly to its honour, and with a zeal and disinterestedness, which philanthropy ever inspires, has, by offering medals, endeavoured to excite an investigation of this subject; but the result of this commendable spirit is as yet unknown. It is to be feared, however, until that distant day shall arrive, when the principle of life will be less obscure, its cause ascertained, or itself made known, all efforts will be fruitless. For it appears to proceed from a difference in the nature of this unknown cause of the phenomena of life, that some persons sooner fall victims to submersion than

others, though not remaining in that state, a greater length of time, than those who recover; and that some succumb under diseases, which others resist for years. A belief in this opinion, might be supposed to preclude all other inquiry, and induce us to rest contented with our present advantages, hopeless of attaining to a greater good. But while an opinion is overshadowed with a doubt, the true philosopher relies not on it fully; he trusts to it, but with the cautious steps of the wanderer of the dark. He would not, by this opinion, be induced to remit an exertion, or let one experiment remain unassayed, which would more illume the subject, or serve to extinguish that light which twinkled but to deceive.

In the following pages, I shall treat this interesting subject to the extent of my abilities. If the smallest germe of success should bud from my exertions, to be afterwards nurtured with more refined culture, my expectations will be more than fulfilled, and myself rewarded. I shall occupy them,

I; with considering respiration, and as far as our limited views will enable us, in inquiring into the manner in which it supports life;

II; with ascertaining the proximate cause of the disease, produced by drowning; examining the state of the vital organs; and the indications of cure, which a knowledge of these presents;

III; with the mode of recovery, founded on these indications, promising the greatest success.

SECTION I.

OF RESPIRATION.

Than respiration, no function of the animal economy, is more important, or more interesting: important, as it is connected with life; interesting, as the manner of this connexion is unknown.

Until the late discoveries and improvements in chemistry, which have formed so brilliant an epoch in the history of physicks, the knowledge of respiration, was, in a great measure, vague, conjectural, and uncertain. Ignorant of oxygen, of the composition of the atmosphere, and its effects on the blood, physiologists, before this time, had no data from which to reason, no guide to direct their steps. Like mariners without the compass, their course was devious and wandering. If sometimes, by the mere force of genius, they approached the confines of truth, they were, however, lost more frequently in the mists of error. But although chemistry has elucidated many of the phenomena, which before eluded investigation; although its discoveries have been a luminary to respiration, and enabled us to behold with a more extended ken, yet there remains a region of truth to be explored, still is the most useful part of the knowledge of this function unknown, or viewed through the delusive medium of conjecture.

The manner in which respiration is performed, is well known to every physiologist; it will be, therefore, unnecessary that it should be here detailed. Its more important parts demand our consideration.

The change which is produced in the air inspired; the alteration of colour in the blood, with the manner by which this change is effected; and what purposes it answers to the system, are the points of view in which respiration attracts attention.

Atmospheric air, composed of oxygen 22, azote 78 parts, when inhaled into the lungs, and again expired, is ascertained, on examination, to be in a different state, from that in which it was, when first taken into those viscera. The greater part of the oxygen disappears, and in its place, a quantity of carbonic acid gas is found. Mr. Davy has lately published to the world, that part of the azotic portion of the air is also consumed. His experiments, confirmed by those of Priestley, previously made, render the opinion probable, if not certain.

Before proceeding to explain the cause of this change in the air inspired, the alteration which is experienced by the blood must be noticed. They are so intimately connected, so mutually dependent, that to understand the one, the other must be known.

The blood passes into the lungs from the right side of the heart, through the pulmonary artery, of a dark purple or black colour. After having circulated through this artery, which ramifies into almost inconceivable

minuteness, and in which it is exposed to the action of the air contained in the lungs, it is returned to the left side of the heart, of a bright vermillion hue, and is thence distributed throughout the system, which it nourishes and supports. The change in the constituents of the air, which I have mentioned, being discovered, and the alteration that the blood undergoes in the lungs, into which the air is received, being known, led to the supposition, that the change of the one, was the cause of that of the other. The experiments of Cigna, but more particularly those of Priestley, proved this supposition to be correct. They found venous blood, when exposed to atmospheric air, to acquire a bright scarlet colour, and that the air lost its oxygen, while carbonic acid gas was formed; and that oxygen gas produced the same effects, but with more rapidity. Priestley removed all objections which might be urged against this opinion, by showing, that these changes occured, even when the air was separated from the blood, by the intervention of a bladder; a demonstrative proof, that the membranes of the lungs could be no hindrance to their mutual action.

Having made this advance in the discovery of respiration, philosophers endeavoured, next, to render it complete, by explaining in what manner these different changes were effected.

In these lowly pages, which assume not to the form of a regular treatise, I think it unnecessary to pass in review the various theories on this subject, which have been

promulgated, to point out their merits or correct their faults. I shall only consider that, which at the present day, can boast of the most numerous followers, and examine to what degree it merits our assent. Lavoisier, whom Science extols as the most illustrious, while she weeps his fate, as the most unfortunate of her sons, soon after the publication of Priestley's paper* on respiration, presented to the publick, a theory replete with ingenuity, which, for some time, possessed, almost universal sway over the philosophick world, and still retains, perhaps above all others, the greatest number of disciples.

Carbonic acid gas, appearing in the air expired, supplying the place of the oxygen, which has disappeared from the air inhaled, and this gas being known to consist of carbon and oxygen, its formation in the lungs, Lavoisier thought, would satisfactorily explain the source of the consumption of oxygen; and the carbon being extracted from the blood, would account for the change, which that fluid sustains. In some accurate experiments, at a period subsequent to the publication of this theory, he ascertained that more oxygen is lost to the air inspired, than is necessary to the formation of the carbonic acid gas, which is found in the air expired. To obviate the powerful objections, which might be argued against his doctrine, from this fact, he now, for the first time, noticed the elimination of water from the lungs, which

* Philosophical Transactions, Vol. 66, part 1.

before he had treated with neglect. His discovery of the constituent parts of this fluid, which has impressed on his name the seal of immortality, came now with friendly aid to relieve him from this difficulty. Influenced undoubtedly by this discovery, and never having admitted that oxygen entered into the blood, he concluded instantly the portion of this gas, for the disappearance of which, he was to account, united to hydrogen thrown out from the blood, and with it, formed the water contained in the air of expiration. This theory, simple, plausible, and ingenious, was for a while supposed to have completely explained the phenomena of respiration; accounting for the consumption of oxygen, the formation of the carbonic acid gas, and the change in the colour of the blood. But this hypothesis, though specious in appearance, and the authority of a name so commanding as that of Lavoisier, could not entitle it to universal adoption, unexamined and uninvestigated. It has passed the ordeal of publick opinion, and has been found defective. It lives in remembrance, not from its own intrinsick worth, but from the merits of its author.

Lavoisier fell into an error, at his commencing the consideration of respiration, which affected all his future reasonings. After appearing undecided, whether oxygen is absorbed into the blood, or united to carbon, in the cavity of the lungs, he finally adopted the latter opinion as correct; and believed that the change in the blood, is produced by the loss of carbon and hydrogen, which are

rejected from the vessels, uniting to the oxygen exteriorly thereto, forming carbonic acid gas and water.

Bostock, in his very valuable essay on respiration, contests this opinion; and his arguments must prove it fallacious. He very justly concludes, as also does Burdin,* that there are no solid grounds, on which to rest the opinion, that water is formed in the lungs. The supposition of hydrogen in the blood, is entirely gratuitous, no one fact, but the escape of water from the lungs, countenancing the notion, and this is to be explained on another principle, more analogous to the general operations of the œconomy, than to that of the union of its constituent parts, in these viscera. For we find every cavity of the body, moistened continually by a watery, or serous fluid, to preserve them pliable and soft, to obviate the bad effects which friction would otherwise occasion; and we must suppose the same fluid would be secreted on the surface of the bronchiæ. The air, carrying part of this off in solution, or in vapour, explains its appearance in expiration. We are no more authorized to conclude, that hydrogen and oxygen unite to form water in the lungs, than that they do so in the cavity of the abdomen; for we find, when opened, while the animal heat yet remains, a watery vapour exhales from it.

Crawford was led into the same error, from a mistake under which he laboured, owing to the nascent state of Chemistry in his day. He says, there are two kinds of

* Medical Studies, Vol. III, page 341.

inflammable air, one obtained from metals, the other from animal and vegetable substances, which he calls lambent inflammable air. This last produces carbonic acid gas in its combustion. He supposed this air was extricated from the blood, united to oxygen, and produced the carbonic acid gas. But as water is the product also of the union of oxygen and hydrogen, it must therefore result, that water is formed in respiration;* such is the train of reasoning he pursues. This lambent inflammable air, of which he speaks, is carbonated hydrogen gas; but that it exists in the blood, or is ejected into the lungs, is an opinion not founded on fact, and discarded by reason.

Lavoisier and Crawford, were not more correct in their belief, that oxygen did not enter the blood, and that its loss was owing to its uniting with carbon and hydrogen, in the lungs. The numerous and decisive proofs of its absorption into the blood, are indicated,

1st, By some experiments of Girtanner, who found arterial blood to evolve oxygen; for this blood being exposed to azote, and hydrogen airs, and this gas, being discoverable in them afterwards, it must have escaped from the blood; these experiments are confirmed by others of Priestley;†

2, Mr. Davy, when he exposed arterial blood to heat, informs us that carbonic acid gas and oxygen, or "phos oxygen," as he terms it, were driven off.

* Crawford, on Animal Heat, page 153-4.
† Priestley on air, Vol. III, p. 76-7.

3d, From the fact that air is separated from the blood, in the living body. It must of course be a secretion from the arterial blood, and is the separation of air contained in, or held in solution by it; for it is impossible, for a moment, to entertain the idea that it proceeds from a decomposition of the blood, or of any part of the system. Hunter mentions several circumstances in support of this belief;* and also the case of a tumor, from which air was discharged.† In the disease called Tympanitis, a vast quantity is at times secreted. No account has as yet been published of this air having been examined, we therefore cannot determine of what kind it is; but in the sword-fish, the air bladder is ascertained to contain oxygen, while in the carp, it is found to be azote.

4th, Morgagni mentions his having inflated the lungs of a subject, in consequence of which, air passed into the pulmonary veins.‡ Bichat relates an experiment he performed on a living dog, which must place the fact beyond all dispute. He inflated, with some force, the lungs, and on dividing the crural artery, the blood issued forth, mixed with bubbles of air. He distended, afterwards the lungs of another dog, with hydrogen gas, and while the air escaped from the crural artery, as in the preceding experiment, he inflamed it with a taper.§

* See Animal Economy. Observations on digestion, p. 164-5.
† Same, p. 166-7. ‡ Vol. 1, p. 117.
§ Recherches Physiologiques, sur la Vie et la Mort, p. 354-5

5, From the fact that air has been found in the vessels after death. Morgagni in his dissections gives several instances of this circumstance, which occurred to himself and others, that render it incontestible.* I had an opportunity of witnessing this incident, in the bodies of two men, whose deaths were sudden and violent; having been destroyed by strangulation. The right side of the hearts of each, were considerably distended; and on making an opening into them, air rushed out with the blood. The coronary veins of one were perfectly transparent, from the air, which they contained. It was also detected in the brain of the other, in the vena galena. The diaphragm in the agonies of death had been raised to a considerable height, and compressed the lungs. It may be supposed, as it was by a person present at the examination of the bodies, that the air proceeded from putrefaction. I will not assert it did not; indeed I must confess there proceeded from one of the subjects, a fetor; but the viscera did not exhibit signs of putrefaction, and I am confident I have dissected, and seen dissected and examined, subjects much farther advanced in putrescence than these were, yet have not before seen or heard of this appearance.

But if the oxygen does not enter the vessels, what, I would ask, becomes of that portion of it, which Lavoisier has found, is lost to the air, more than is necessary, to the formation of the carbonic acid, which is produced. It does not certainly unite to hydrogen, as he supposes,

* Morgagni on Diseases, Vol. 1, Letter 5.

to form the aqueous vapour. If we do not admit that it enters the blood, we cannot account for its disappearance; and as we find facts, which so strongly countenance the opinion that it does, so we cannot hesitate to believe it true, or confess it plausible. It more readily meets our assent, we are impelled on to conviction, when we know that air, in a certain quantity, can be injected into the vessels, without injury to the animal. Dr. Hartshorne, mentions in his inaugural thesis, that he injected 1-8 of a cubic inch of oxygen gas, into the femoral artery of a dog, which suffered from it, no inconvenience. I repeated this experiment, on the jugular vein of a dog, to the amount of the 1-4 of a cubic inch of oxygen, with a similar result. Girtanner met, indeed, with very different effects in his experiments; almost instant death being induced; but he employed a "considerable quantity," so that the heart must have been acted on by a powerful and unaccustomed stimulus; for the distance, from the place at which the air was injected, and the heart, being but small, and the quantity considerable, the air must have been emptied into the right auricle and ventricle, unmixed with the blood. Now it is not my intention to contend, that air, in any quantity, not in union with the blood, can circulate without injury to the animal; but that it is received into the blood, in the capillary vessels of the lungs, and circulates with it, either held in solution, or minutely divided.

An opinion has lately been advanced by Mr. Thompson, that all the component parts of the air are absorbed into the blood, and that the greater part of the azote, is again rejected from it, while the oygen is retained. It appears, that he was led to this supposition, to account for the disappearance of a portion of the azote, which is lost in respiration, ascertained by Priestley and Davy. This occurrence, is however satisfactorily explained by Bostock, without admitting this solution; and as it is supported by no other circumstance, I place no confidence in its truth. It is, on the contrary, invalidated by the facts;

1st, That between blood and azote, as Mr. Thompson informs us expressly himself, there is no affinity.

2nd, That a strong affinity exists, between blood and oxygen. I have ascertained, that blood exposed to oxymuriatic acid gas, becomes oxygenated as rapidly as in atmospheric air, and reduces it to the state of common muriatic acid gas.

3d, That it has never been observed, in the numerous experiments which have been made, by exposing blood, drawn from the system, to atmospheric air, that any of the azotic portion disappeared.

This opinion, cannot be admitted, then, unsustained by any decisive facts. I believe, on the other hand, that the oxygen is solely absorbed, but owing to the affinity, existing between oxygen and azote, there is carried along with it, into the blood, a small portion of this last air.

From the facts which have been enumerated, and the arguments which have been inferred from them, it is concluded

1st, That the oxygen gas of the air disappears, by entering into the blood, in the pulmonary vessels.

2nd, That the carbonic acid gas is generated in the lungs, by the oxygen, combining with, what Allen* calls, an oxide of carbon, formed during the circulation, from the oxygen forsaking the blood, to unite intimately, with the carbon it contains: and

3rd, That the water of expiration, arises from the exhalation of the pulmonary secretion.

This theory of Respiration, nearly that of La Grange, as modified and improved by Allen,† appears to me to possess more plausibility, and a greater support from facts, than any other of the many which have been constructed.

It then follows, that the change induced in the air by respiration, is produced;

1st, By the oxygen uniting to, or entering into the blood.

2nd, By its acquiring carbonic acid gas, from a portion of the oxygen, combining with an oxide of carbon, in the pulmonary vessels, from which it is eliminated.

* I am not certain I am correct, in mentioning this as Mr. Allen's opinion. The work he has published on this subject, cannot be obtained here; but I am led to believe, this is the modification of La Grange's theory by Mr. Allen, of which Bostock speaks, page 115.

† See note, page 29.

3d, By its losing a small portion of azote, which is absorbed into the blood, in consequence of the affinity, between it and the oxygen.

The cause of the alteration of colour, which the blood experiences in the lungs, comes now to be considered.

By Crawford, Lavoisier, and the generality of physiologists, it is attributed to the escape of hydrogen and carbon, from the blood. As no proof of the existence of hydrogen in the blood can be advanced, it becomes needless to consider how far its absence, may have this effect.

It only remains to demonstrate that it cannot be explained on the supposition of the ejection of the carbon from this fluid.

If the deprivation of carbon to the blood, have the effect of changing its colour to the vermilion hue, it must follow, that it would not lose this tint, when once acquired until it should be again impregnated with carbon. How does this accord with fact? Arterial blood exposed to the air, whence it could derive no carbon, will, after sometime, lose its vermilion colour, which proceeds from the oxygen more closely combining with the carbon, which the blood still contains. If blood, which has this colour be exposed to an air, which does not contain oxygen, or be placed in vacuo, in neither of which situations, it can possibly receive carbon, it takes on, however, the dark colour of venous blood; and if again ex-

posed to oxygen, or atmospheric airs, it again assumes the vermil hue.

It might be objected to these well-known facts, that the blood is not in a natural state, and of course, they cannot be depended on to draw any conclusions of what occurs, in the living body. To obviate this objection, of little force, we have only to refer to the experiment of Hunter, which proves that arterial blood, confined in the arteries, becomes as black as venous blood. It is also a fact, familiar to every surgeon, that the blood, which first flows from an artery in an operation, after the circulation has been prevented in it by the tourniquet, for a short time, is perfectly black. It will be in vain to assert, that here the blood might have received carbon. No well-informed physiologist, at this day, will support the opinion of Crawford, which his warmest admirers have deserted, that the blood receives its carbon, in the capillary vessels. The only medium, by which it can enter the vessels, is the absorbents, and of course, it is utterly impossible any could have been carried to the carotids, on which Hunter experimented, or to any artery, secured with a tourniquet.

This fact alone, that the blood receives its carbon by means of the absorbents only, which is perfectly undeniable, and which Lavoisier himself was obliged to confess true, completely refutes his doctrine. Bostock decisively proves, from it, that Crawford and Lavoisier

are in error.* For as the blood, according to these philosophers, becomes black only in consequence of acquiring carbon, (for hydrogen is put out of the question) and as it gains the bright red colour, in the lungs, characteristick of arterial blood, by the loss of this principle, then, it should follow, that the blood having passed the lungs, would retain this colour, until it arrived at the junction of the right and left subclavian veins, with their respective jugulars; for it is at this place alone, that the absorbents of the system pour their contents into the blood vessels. Now, as the blood cannot obtain carbon, until it comes to this situation, it should not before then, acquire the black colour. This is known, however, to the world, not to be fact. The alteration of the colour of the blood, during the circulation, is not, then, dependant on the presence or absence of carbon.

When under certain circumstances, we behold a certain effect produced invariably and universally, the human mind almost involuntarily and most justly, attributes those circumstances as the cause.

It is not, then, without surprize, we find that so much difficulty and dispute, have existed for so long a period, on this subject. For as blood becomes always of a florid red colour, when oxygen is present; as it always loses this colour, when deprived of contact with this air; as this colour when lost is restored again by exposing the

* Essay on Respiration, p. 111-12.

blood to this gas; as the gas is always found diminished in quantity, or entirely consumed, according to the greater or less space of time they have been in contact; and as the degree of the colour of the blood always corresponds with the quantity of oxygen present, or consumed; it would appear to be unquestionably proved, that the absorption of oxygen, was the cause of the blood's florid colour.

The preceding portion of this section, established the fact that oxygen was absorbed into the blood, and as it is now shown, that the change of the blood's colour in the lungs, is not the consequence of the escape of carbon from it; and also, that this effect occurs whenever oxygen is present, the conclusion is, the combination of oxygen with the blood, is the cause of its vermilion hue.

The gradual change of this colour, to the purple or black of venous blood, is the event of the oxygen combining gradually, with the carbon of the blood, and forming an oxyde of carbon; and thus, by the loss of oxygen to the blood,* and perhaps the peculiar state of the carbon, produced by its combination with the oxygen, is the dark colour to be attributed.†

* Bostock, p. 115-6. † This theory, which Bostock mentions, as that of La Grange, modified by Allen, appears to me to be supported by strong and conclusive proofs. The facility with which it explains the hitherto anomalous facts of animal heat, inexplicable by any other, must tend greatly to its establishment and reception.

As the change of colour in the blood, to the bright vermilion, is produced by an absorption of oxygen; and it being a well-established and incontestible fact, that death is the speedy and unavoidable consequence of the defect of this change, it remains to inquire into the manner in which oxygen supports life.

I have now approached a part of my subject, which, like a rugged, desolate, untrodden tract, appearing after a passage through a land meliorated by the hand of industry, almost deters from traversing its bounds. I paused on its borders, as the wandering youth, when first he meditates to forsake the beaten way; tempted by luxuriant fruit——

> "And unknown regions dares descry,"

like him, I may return, gladdened with the object of my pursuit, or roam lost in mazy windings.

A strong conviction, that thebranch of medical science I am treating, can alone be improved by an elucidation of this part of physiology, impels me to this undertaking. Too long have those, who are best calculated to penetrate its deepened gloom, and overcome the obstacles of its ways, avoided the arduous task. They have proceeded on, and neglected, or viewed it with a cursory glance, leaving its wide extent to be explored by feebler steps. Yet ere Elysium can be gained, must

Acheron be passed. A few, however, have entered and have sought the riches of the soil. Where some before have trodden, I may, then, be permitted to wander, nor suffer reprehension. Perhaps some sparkling gem, whose "mild and modest ray," lay concealed from their view, may meet my more fortunate eye. Drawn from its obscurity, and polished by a master's hand, the long neglected and once worthless stone, becomes the valued jewel.

As the oxygen combines gradually with the carbon of the blood, its caloric is eliminated, and it is thus, the cause of the phenomenon of animal heat. But does it not perform some other office to the system, more immediately conducive to life? I believe it does. I found this belief,

1st, On the absolute necessity, of the continual oxygenation of the blood, in every class of animals which is known.

2nd, On the sudden suspension of the actions, and phenomena of life, in animals, cut off from communication with this gas.

For if it produce no other effect, but merely the production of animal heat, those animals and insects, whose animal heat is not greater than that of the surrounding medium, ought not to be readily, if at all, destroyed, when deprived of oxygen. For as the heat of their bodies, is not superior to that of the circumambient air, and sometimes not so great, it could not be lost, and of

course, the animal would continue to live: and if oxygen only supported, and was only connected with life, by producing animal heat, this principle, quality, or whatever it be, should not be so suddenly destroyed, as we find it is, especially in warm blooded animals, when they cannot obtain oxygen; for death is mostly induced, before the animal heat is diminished. If this were also, the only connexion between them, we would find a man, or any other animal, deprived of oxygen, in a temperature warmer than their animal heat, would require a greater length of time, in which to be destroyed, than when placed in a cold medium, under similar circumstances. Yet we find no difference produced in the period, in which life ceases, in these two conditions.

3rd, From the same necessity existing in the fœtus, as in the adult, of the uninterrupted oxygenation of its blood—Situated as the fœtus is, it is securely guarded against any change of its temperature being effected, by external causes. In truth, it scarcely seems necessary, it should in its own system evolve any caloric, and of course, if oxygen be only intended to produce this effect, it is here perfectly superfluous. And surely, as the animal heat of the fœtus cannot be lost, even when it no longer receives oxygen, it would continue to live, if oxygen were not necessary to its existence in some other manner.

But can the fœtus exist better than the child which breathes, independent of oxygen? No, not more cer-

tain does night succeed day, than death follows the cessation of this effect on the blood.

The sad experience of every accoucheur tells, how fatal are the consequences to the child, of compression on the umbilical cord, which prevents the fœtus from receiving oxygenated blood. Neither is the opinion of Goodwyn, that it is thus needful to life, by stimulating the left side of the heart, more correct. The arguments of Coleman, and the experiments of Bishart, have consigned this theory to the number of those, which are to be held in remembrance as having been, but are no more.

In what manner, then, is the oxygenation of the blood connected with life? This is the club of Hercules.

While physicians believed, and were the advocates for the doctrine of a self-existing power, an "*anima medica*," presiding over the system, we are not astonished, that the slightest shadow of suspicion never crossed their minds, that life was supported, much less produced, by any external cause. But since this opinion has been held from the pale of physiology, and excluded from communion with medicine; and as the theory of the forced state of life, founded on fact, supported by reason, and countenanced by analogy, has extended its empire over the fairer portion of the medical world, we cannot but admire, a circumstance so obvious as this must be, has been neglected, nor met with that attention, which its importance, surely commands. For if excitability, that quality on which stimuli act, and produce

the actions of life, is ever varying, and seldom rests at one point, according to the difference, in the number or strength of the stimuli acting upon it, there must be some source within the system, from which it may be generated when deficient. It cannot be, the system is endowed with a certain quantity, at the commencement of existence, and that this is to continue during life: if so, a portion once exhausted, is irretrievably lost, and existence has suddenly hastened to its termination. But we well know, each hour witnesses in it, some accumulation or diminution.

It is far from being my intention to assert confidently, that oxygen is the principle, from which is produced the excitability; I presume not thus; I forget not that I am one, whose step is yet scarce o'er the threshold of the temple of medicine, on whose head, experience but now begins to shed its illuminating rays; and whose acquaintance with the phenomena of life, as exhibited in health and disease, is contained within a circumscribed bourne. This opinion, however, advanced by Girtanner, has been silently acquiesced in by some, while it has been openly denied but by a few. The theory which he has connected with it, the doctrine of Brown, travesty with a chemical garb, has deterred many from bestowing on it that attention, to which, perhaps, it is entitled. Yet it becomes philosophers, more especially medical philosophers, not to reject hastily, even a conjecture, somewhat patronized by facts. Medicine con-

tains within its records many truths, which were once but suppositions, and which are now confirmed facts. And if we but for a moment reflect on the continually fluctuating state of excitability, it must be evident, it is originated in the system; and if so, there must be some source, from which it can be discovered to proceed. Is the brain, as some suppose, this fountain of life? If the excitability be secreted in that organ, there must still be some principle, from which it is formed. This, however, can be conveyed to the brain, by the blood alone. But it is blood in a peculiar state only, which can circulate to the brain, without causing the destruction of its function, and the extinction of life, and which of course, can alone produce the excitability; and this blood is arterial. As blood, then, which contains oxygen is alone proper to generate this principle, and as no other kind of blood will answer for this purpose, it still does not, if this theory be true, invalidate the opinion of Girtanner, which therefore, begins to appear less improbable, than it did at first glance.

But the brain is not the organ, which produces or secretes excitability. The fact is well known, that when the communication between this viscus, with parts not vital, is cut off, those parts still retain their excitability, and irritability, for although their sensation and voluntary motions are destroyed, yet they are alive to the action of stimuli.

If the opinion, of the production of excitability from oxygen, should for a moment be admitted, could a conjecture, anyways satisfactory, of the place and manner of the production of this quality from oxygen, be given? It evidently receives its existence in the system. Why not, then, trace the streamlet to its source?

The blood passes into the capillary vessels, possessed of oxygen; in these vessels, it is continually changing, being converted into the animal fibre, and returns from them devoid of this principle. Now, what becomes of the oxygen, when the blood is thus deprived of it? It does not return with the superfluous blood, nor does it escape in the form of gas, or we should always be emphysematous. Does it not unite to the animal fibre? Would not its combination with the animal fibre, in a manner yet unknown, but which "*dies doceat*," possess it with the property or quality, of being susceptible to the action of stimuli. That this is positively the mode of the origination of excitability, I certainly do not mean to assert; but that it arises in the capillaries, and from oxygenated blood, can admit of no question, is demonstrably proved by experiment.

And we are now familiarized to the singular and wonderful properties, which oxygen gives to different substances, by combining with them. Even a difference in the proportion of the oxygen, united to the same base, endows it with properties wholly unallied and totally distinct.

Could we but devest our minds of those habits of thinking, which were imposed on them in our youthful days, when feeble and just evolving from their germe, they bent before each breath, received each insect touch, and no longer behold life, as a mysterious and ethereal ens, which it is "*scire nefas*," but view it as exhibited by the wild and daring genius of Brown, the more correct and discerning Rush, as it truly exists—the creature of necessity, the offspring of the world, we shall not start, amazed at the avowal of a belief, excitability depends on, flows from the combination of oxygen, and the animal fibre. But too frequently does error retain its strong hold of our minds, and like the shirt of Nessus, even Hercules cannot shake it off.

Yet no opinion, however plausible it may appear, however much it may tend to supply some deficiencies, in a theory otherwise complete and correct, and, therefore courting our acceptance, ought to be admitted, or for a moment entertained in the mind, unless supported, in some degree, by facts. Are there any facts, experiments, or circumstances, which stamp the opinion with credibility, that oxygen is the source of excitability. I think it rendered probable,

1st, From the consequences, resulting from black or unoxygenated blood, circulating in the arteries. When, by any means, respiration is obstructed, so, that the blood no longer receives oxygen, it passes into the arteries of a dark colour, or in the state of venous blood.

In warm blooded animals, if this continue for four or five minutes, the actions of life diminish, and are soon entirely destroyed. If the most powerful stimulants, which before this diminution of life has been induced, would have excited the most violent commotions in the system, be now employed, they produce very little and frequently no effect. Coleman mentions, that having injected one drachm of tart. emet. into the stomach of a dog, which he had immersed in water until nearly drowned; it produced no effect for sometime, and then caused vomiting, while the same quantity in another dog, not treated in the same manner, caused, in a few minutes, violent vomiting, inflammation and gangrene of this viscus. Whence could this difference in the effect of the same medicine proceed? It was from the excitability being nearly destroyed in the one, while in the other it remained in a natural state. But to what shall we attribute the loss of the excitability of the first dog? We know it is worn down by the powerful, or the long continued action of stimuli; but in this case, there were no powerful or long continued stimuli applied, by which it could be reduced. Nor can I coincide in the belief of some, that the exertions made in drowning, could exhaust this quality. The actions of drowning animals are by no means violent, and surely, we each day, make exertions much more powerful, at the same time that other strong stimuli are exciting us, without expending the whole of the excitability, though it is diminished. When we examine the system, for an ex-

planation of this fact, the only cause to which it can be attributed, is the blood having circulated through the capillaries, destitute of oxygen; and of course whatever effect the oxygen there produced must have ceased. But the excitability and irritability, have been suddenly lessened, or have entirely disappeared. Does this proceed from the want of oxygen in the blood? No other cause of solution presents itself to our view. In what manner can these qualities of the fibre be generated from oxygen? To throw some gleams of light upon this darkened spot, which may prove that the want of irritability depends on the absence of oxygen, an experiment of Bichat occurs. He injected blood, fresh taken from the veins, into the crural artery of an animal. The motions of the limb became very weak, and it was often rendered paralytick.* This probably would have been more frequently the result, but in such an experiment, it is almost impossible to prevent, more or less oxygenated blood passing into the limb, through the anastomosing branches.

When this experiment was performed on one of the carotids, instant death was produced. The same effect was caused undoubtedly, by the blood in both cases; but as an indispensable necessity exists, that for the continuance of life, the functions of the brain should be perfectly performed, and its vitality remain uninjured, it was, in this experiment, mortal. In what manner did venous

* Recherches Physiologiques, p. 327.

blood prove thus deadly, and why should it not be as congenial to life, as arterial?

We cannot suppose life to be different in the brain, from what it is in other parts. It must be specifically the same, throughout the system, and consist in the possession of excitability. In this experiment, the excitability disappeared, when venous blood was present in the arteries. This could have occurred but in one of two ways. It must either have been, that the venous blood, by the force of its stimulant action, destroyed the excitability, or, if this quality proceeds from the blood, it must have been destitute of that principle, on which, excitability may possibly depend.

But it is the universal opinion, founded on observation, that venous blood is not as great a stimulus as arterial, of course it could not have ended life in the first mode. May we not conclude, then, that death was produced in the second mode, or that venous blood, was improper to form the excitability of the brain, being devoid of that principle from which it is generated. We know of no other principle in the blood, which adapts it for this purpose but oxygen.

2d, From the results, arising from injecting oxygen and other gasses into the blood vessels. The experiment of introducing oxygen into the vessels, has been frequently performed, but we have to lament the want of system, with which it has been pursued. A large or a small quantity of oxygen have been employed; the

one always effected the destruction of the animal, the other was of no disservice. It would more tend to raise the curtain for discovery, were it to be ascertained, what quantity could be employed without killing the animal, and by careful observation, note the consequences of it in the system.

When death has been induced, by a large quantity being forced into the vessels, all agree that the irritability of the heart and of the muscles, is much increased; and that it continues in them a greater length of time, than when death has been suddenly occasioned, by any other cause. If any other air be injected, death is as suddenly, and sometimes more so, produced; yet the irritability is found to be nearly destroyed, or does not exist. The constant occurrence of these effects, in the experiments, cannot permit them to be doubted. How are they to be explained? Do they not point to a connexion, existing between irritability and oxygen? Or why should the irritability be increased by this substance alone, while it is diminished by every other?

3d, From the fluctuating state of the excitability, most clearly proving it to be generated in the system; and in connexion with this, the variation in the quantity of oxygen consumed. That the quantity of oxygen which disappears in respiration is not uniform, is proved by the difference which exists, between the calculations and experiments of different physiologists, made to ascertain the amount, which is destroyed by one person. Their

contradictions, can only be reconciled upon the principle, that different quantities, are consumed at different periods. It has much tended to retard our progress, in the knowledge of this branch of physiology, that the European chemists, who, from their opportunity of being supplied with every necessary apparatus, are best calculated to pursue this subject, have in their experiments neglected to determine, whether disease produced any alteration in the quantity of air, used by the system; whether there were any difference, in different times of the day; and whether the quantity, which disappeared, corresponded in any degree, with the stimulability of the subject of the experiment. From this inattention, we are obliged to remain in doubt. But we are not entirely destitute of some guides, which may direct, though obscurely, our way. For, 1st, during the night, the excitability becomes equalized, and is accumulated for this purpose, and also to answer the wants of the coming day. This is the object of sleep,

——repairer of decay,
Whose balms renew the limbs to labours of the day.
DRYDEN.

In this state of being, more oxygen is consumed, than in an equal space of time when awake.* We are inclined to believe this assertion correct, from observing those who sleep, make full, deep, and long inspirations, completely distending the lungs, by which means, more air

* Rush, M. L.

is brought into contact with the pulmonary vessels. 2d, In the first attacks of violent inflammatory fevers, when the system possesses a superabundance of excitability, and more of it is also exhausted, than in a healthy state; we observe the respiration more full, hurried, and laborious, than usual; and, of course, more oxygen must be absorbed by the blood. 3, In diseases in which the excitability is deficient, and stimuli can with difficulty produce action, the respiration is slow and weak, or performed, with quick and small inspirations and expirations. The blood, then, must receive less oxygen, than when in health, from the respiration being different. But the excitability exists in small quantity. Does this arise from the less oxygenation of the blood? It might be supposed, if this were true, that a contrary effect, ought to be produced. For if the excitability proceed from oxygen in the blood, the deficiency of this quality, should occasion the respiration to be increased, that the consumption of oxygen, may be augmented, to answer the exigencies of the system. This reasoning appears certainly specious, and seems to prove, that no connexion, exists between oxygen and excitability. But view it in another light, and it presents a different aspect. When the excitability is exhausted, by any means, the actions of the animal economy are weakened, and the functions of each particular part, are performed in a defective manner. The heart propels a less quantity of blood, and with less momentum to the lungs; the muscles of respi-

ration are debilitated, and can no longer produce the same enlargement of the thorax, as in a natural state; from these causes, less blood is sent to the lungs to receive the genial influence of the air; and less air is respired, than when health diffuses equability of life and strength over the frame. In these states, the respiration or consumption of oxygen, bears a ratio to the excitability.

4, That when a part, becomes inflamed, in which case, more oxygenated blood circulates through it, than usual, we observe its vital phenomena, which depend on an increase of its excitability, to be exalted. Is not this the consequence of the increased quantity of oxygen it receives?

5, That in weak, debilitated habits, in which life seems to be put partially unfolded, syncope is frequently brought on in crowded rooms, where the air is vitiated by the breaths of many persons, and the burning of candles. Our places of worship, and assembly rooms, bear witness to the truth of this remark. This state of the system, is occasioned by a deficiency of excitability. Does not its diminution in these cases, proceed from the want of oxgen in the air?

6, From the state of the excitability in hibernating animals, during the winter; and the quantity of oxygen, consumed by them, bearing an equal proportion. At this period, the excitability is extremely low, the most powerful stimuli, but weakly affecting it, and then, being but small in quantity, it is soon exhausted, and the animal

falls a victim, to the curiosity of the experimenter. In these animals, while in this state, the consumption of oxygen is extremely small. But soon as the returning spring, revives and animates all drooping nature, they are roused in their hibernal caves by its inciting influence; their respiration becomes stronger; with this, their excitability increases, and now they forsake their habitations, and wanton in the full possession and vigour of life. Do not these circumstances tend to impress us with a belief, that the two, are connected by an indissoluble link?

7, From the affections brought on the system by a rarified atmosphere. When the medium, is in this condition, the quantity of oxygen, received into the lungs at each inspiration, is much smaller, than when it is dense. In great altitudes, the atmosphere, is in a high degree of rarifaction. The accounts, which bold and enterprising philosophers, relate of their sensations; of the state of their systems; the effects which they suffered; and the symptoms, they exhibited, when on the summits of the most elevated mountains, to which they had ascended, induce the persuasion, that these consequences of a rarified atmosphere, were occasioned by a want of excitability. These are best described in the words of De Saussure. "When we came near the top," (of Mont Blanc) "I could not walk 15 or 16 paces without stopping to take breath; and frequently perceived myself so faint, that I was under the necessity of sitting down from time to

time; and in proportion as I recovered my breath, I felt my strength renewed." Again he proceeds, "I was obliged almost at every instant, to desist from my labour and turn all my thoughts to the means of respiration." And afterwards, "While I remained perfectly still, I experienced but little uneasiness more than a slight oppression about my heart; but on the smallest bodily exertion, or when I fixed my attention on any object for some moments, and particularly when I pressed my chest in the act of stooping, I was obliged to rest and pant for a few moments." His assistants, in digging into the snow were obliged to relieve one another incessantly, raising a few shovel-fulls completely fatiguing them. They were all affected with great thirst. Spirits and wine, but aggravated their symptoms. These were, indubitably, signs of a rapid exhaustion of the excitability, from the action of moderate stimuli. But why should this quality be sooner lost in a rare, than in a dense atmosphere? The mere weight or levity of the air, I am inclined to believe, would have but little influence in producing this effect.

And when we reflect, that oxygen is indispensably necessary to life, and as it is on the summits of lofty mountains, and in a rarified atmosphere, received in less quantity into the blood, than when the air is dense, we cannot avoid attributing these symptoms, the consequences of a defective excitability, to the diminished quantity of oxygen in the blood.

8, From the melancholy phenomena, which are exhibited in those persons, who labour under a mal-conformation of the heart. The cause of the symptoms, which afflicts these unhappy and irremediable sufferers, is beyond doubt, the want of duly oxygenated blood. Nor do the terrible, awful paroxysms, attacking these wretched victims of Nature's sport, occur, but when some cause stimulates the system, and hurries on the circulation, so rapidly, that the blood cannot receive the same quantum of oxygen, as before, and which merely sufficed to support a flame just quivering to its end. It is then, that convulsive actions distort the whole frame, the pulse is weak and tremulous, and the body is of a blue colour; the thorax is greatly agitated, and dilated to produce its greatest enlargement, that the utmost quantity of air, it is capable of containing, may be inhaled. It is not until by such respiration, agonizing to the patient, and dreadful to the beholders, the blood becoming more oxygenated, that the paroxysm declines, and the sufferer, to whom death would have been a blessing, awakes to life; and to be subject to a similar torture, whenever the circulation shall again become accelerated. That the restoration in this case, is produced by the greater oxygenation of the blood, is proved by the colour of the body, which, during the paroxysm, was blue or black, gradually becoming of a more florid hue than usual.

9, By the circumstances of the fœtal circulation. If the fœtus, in utero, possessed the same quantum of ex-

citability as the child which is born, it would scarcely ever behold the light; it would only be removed from its maternal tomb, to be consigned to the dust. For as the stimuli, which can exhaust the excitability of the fœtus, are few in number, it would so accumulate, that the smallest adventitious stimuli, or even the action of those, which are natural to it, would excite the most violent commotions in the system, or prostrate it at once into the arms of death. The taper would be extinguished by a superabundance of its oil. Now, if there be any dependence of excitability on oxygen, it should follow, the fœtal blood would receive less oxygen, than that of the adult, or a breathing child. It would be an insult to my medical readers, were I to inform them this is the fact.

Does not this source afford another corroborative fact? When the fœtus is born, is given to the world, that moment it is acted on by powerful, and unaccustomed stimuli. If then, there be no original within the system, from which excitability is produced, no fountain, from which this principle of life flows to supply the deficiency that occurs, and to accommodate it to the force of the stimuli, by which it is suddenly expending, it would quickly be exhausted; and the infant would sink under their agency, and perish. But if the child which is born, breathe, it continues to live, and of course, its excitability, instead of being expended, has been augmented. We look around in vain, for any other cause

to which we may attribute the instantaneous increase of this quality, when a child is born and respires, than to that of a greater accession to the body of oxygenated blood. As soon as the fœtus arrives in the air, it must breathe, or it ceases to exist; even though the circulation which continues through the umbilical cord, still affords it a portion of oxygenated blood. How is this to be explained? We see, that if the child does not breathe as soon as it is born, its excitability is quickly expended, and of consequence it dies; but if it breathe, its excitability accumulates, and continues so to do, until it equilibriates with the acting stimuli, and life continues, and goes on to be evolved. In what manner does respiration thus produce excitability? Is it by imparting to the blood any principle from which the excitability may deduce its origin? There is no substance received into the blood by breathing, but oxygen. Is not oxygen then the matter, from which excitability is created? And on what other principle can we explain the death of the child which expires, as soon as born, if, from some cause or other, it does not respire? If the doctrine I am supporting be true, it affords an easy solution of this difficulty. For as the child becomes acted on immediately by stimuli, of no small force, if its waste of excitability be not supplied, it will speedily be expended, and death supervene. As but a small quantity of oxygen, is received (the child not breathing) through the cord, and which was sufficient only in the fœtal state, to accommo-

date this quality to the few and weak stimuli which could diminish it, the small portion the fœtus thus obtains by the *funis* could by no means produce excitability sufficient to answer the great demand now made upon it, and of course, the infant dies from the total expenditure of its excitability.

10, From the account which the Sacred Volumes of Truth, give of man's formation, and his first spring of life. Man, by the hand of the Deity, was made perfect and complete in all his parts and organs; the machine was fitted to act; but until the breath of life was breathed into his nostrils, the motions of life were absent. We here behold life beginning its career in the lungs. Was this by the air stimulating them, or entering into and acting on the blood. It could not have been by a mere stimulant action, for a thousand stimuli were operating upon its frame at the time, yet without the aid of breathing, could produce no action. But what effect, then, did the air produce on the blood? Did it endow this fluid with vitality? Did it render the blood capable of communicating a principle of existence to the system? or in other words, Did it produce the excitability? for no other vital principle can be admitted. It is evident, no excitability was in the system, until air was received into the lungs. We cannot for a moment suppose, that the air was a specifick stimulus, alone calculated to excite the excitability, and to produce action. If this supposition be rejected, as it must be with contempt, we

have no other resource but to believe, that air was the efficient cause of excitability. For as all nature was created, before man rose to being, the stimuli which now propel his wheels of life, then existed, they would have acted on his system, and developed its latent animation, if it had possessed the quality, which gives to it, a capacity for their action. But it was destitute of this quality, until air entered the lungs. Does not this also lead us to infer, the dependence of excitability on oxygen?

11. From the irritability of the heart, after the cessation of all the apparent phenomena of life, being supported, and even increased, by the presence of oxygen. In a late French work, by Berger, on the proximate cause of the disease produced by drowning, a number of experiments are related, in which the heart of drowned animals contracted often, when the thorax was opened, for many hours, and never less than two or three. But in a few others, which he details, when the thorax was not immediately opened, but attempts were made to recover them for the space of an hour, and then the sternum was raised, no contraction of the heart was perceptible. A considerable contradiction certainly obtains, between the results of these experiments, which I was unable to reconcile, unless the contact of the air, was admitted to have had an influence, in supporting the irritability of the heart. To ascertain how far this idea was correct, and whether oxygen was the agent concerned, I instituted the following series of experiments:

Experiment 1st. I submerged a kitten 20 minutes after 11 o'clock A. M., and when the signs of life had ceased, I withdrew it from the water, and immediately laid open its thorax, by raising the sternum, and cutting it off. The heart contracted 36 times in a minute. In 15′ the contractions of the right auricle and ventricle, were to the number of forty in a minute; the left auricle and ventricle acted more feebly than the right. In 20′, the left auricle intermitted for a time, its motion; it being covered by the left lung, which I raised up and permitted the auricle to be exposed to the air; when in the course of 4 or 5′, it commenced again spontaneously to move. During the experiment, I repeated, several times, covering and exposing the auricle, and always found, that when cut off from contact with the air, its contractions ended, and when exposed to it, they again commenced. In 35′ the right auricle, the blood of which was evidently of a redder hue than before, appeared to have lost the power of moving; but in 45 minutes it recommenced, without irritation, to contract, and more forcibly than before, but a few moments terminated its action, and it was not observed again to move. In one hour, the two ventricles were contracting. In 1° 30′, the number was 19 in a minute. In two hours, the contractions were nearly the same in number, but weaker. I was now obliged to leave the experiment, and when I returned 25′ after 3 P. M., or four hours after the thorax was first opened, the right ventricle was still contracting. It was distinctly perceived by the gentleman who assisted me in the experiment.

Being engaged in the next succeeding experiment, particular attention was not paid to its contractions, and when we inspected the heart, 15′ afterwards, no motion could be observed.

Having thus ascertained a certain period of time, during which the heart of an animal drowned contracts, when exposed to the air, by which the results of the succeeding experiments, might be compared; I proceeded to perform

Experiment 2nd. A kitten was prepared as the first had been, the heart of which contracted 20 times in a minute. It was conveyed directly into a bottle containing nitrous gas, over water, and in 4′ the contractions were 18 and weakened. The blood, which was effused, became of a bright vermilion, which created a suspicion in the minds of the gentlemen present, and in my own, that the blood having a great affinity for oxygen, might even take it from the nitrogene, decomposing the nitrous air. If this had been so, the conclusion to be drawn from the experiment, would have been destroyed. The animal was not retained in the gas more than 15′. But in less than an hour after the thorax had been laid open, the heart had entirely ceased acting, nor could it be excited by the application of sulphuric acid.

Experiment 3rd. A kitten being treated as those in the preceding experiments, the heart was ascertained to contract 33 times in a minute. It was then placed in a vessel of hydrogen gas. In 5′ the contractions were re-

duced to 23, in 8′ to 15, and were very feeble, in 20′ to 9, and in 45′ all action had ceased. The auricles ceased the first, the two ventricles ended their action at the same time. It was then taken from the gas, and the heart irritated, but without any motion being excited.

Experiment 4. A large cat was drowned. In its struggles it twice got its head above the water, and respired. When all appearances of life had disappeared, it was taken out, and the thorax was opened. I observed the muscles possessed more irritability in this animal, than in the kittens; for they contracted powerfully under the knife. When the heart was exposed, the right auricle alone contracted. The fibres of the diaphragm were very irritable and contracted spontaneously. In 10′ after opening the thorax, the right ventricle began to contract. The auricle acted very irregularly, now contracting with frequency, and now very slowly. In 20′, the right ventricle contracted 4 times in a minute, the auricle propelled a wave of blood through its extent 4 or 5 times, and then ceased to move. The right ventricle was acting at the end of an hour. Its actions had been throughout always slow, but sometimes forcible. The left side had not been observed to move once, during the experiment. I had intended to have placed this cat in azote, but as the action of the heart was so very different in this, from what it had been in the other experiments, I thought no correct conclusion could have been inferred from it, and therefore did not pursue my intention. I left the experi-

ment at the termination of an hour, and when I returned, which was 4 hours from the time that the chest had been first opened, I touched the right auricle, which to the surprize of those present and myself, now contracted with more force, than any which I have yet seen, except the last experiment, appearing to close completely on itself, and continued to be thus capable of excitement for an hour.

Experiment 5. A kitten was drowned, and its thorax was then opened. The heart beat 13 strokes in a minute. I conveyed it into azotic gas. In 5′ the right side beat 9 in a minute, the left ventricle the same number, the left auricle was not seen, being hid by the lungs. In 20′ the contractions were 6. In an hour they were extremely feeble, appearing to be the motion of one or two fibres. In 1° 10′ being taken out of the gas no motion was perceptible, nor could any be produced by irritation.

Experiment 6th. A kitten was drowned, and opened as in the other experiments. The whole heart was in motion, and contracted 43 times in a minute. The animal was placed in carbonic acid gas. As I had neglected in the foregoing experiments, to increase the temperature of the water, through which I was obliged to pass the animal, with its thorax opened, to place it in the gas, to that of the animal heat, I thought it might perhaps have been a source of error, by the water abstracting heat from the heart; I therefore, obviated, in this and the following experiments, this objection, by raising the heat of the water to the ani-

mal temperature. Six minutes after the animal had been in the gas, the contractions were reduced to 27, and were very feeble; in 15′ to 14, and still weaker; in 28′ both ventricles were quiescent, the auricles contracted feebly. In 35′, no action was visible, nor could any be excited by irritation. To compare with this experiment I instituted

Experiment 7th. A kitten from the same litter, from which was obtained the subject of the preceding experiment was drowned with it, and permitted to lay in the water until the heart of the first had ceased to act in the gas. The sternum was raised 35′ after the kitten was drowned; but no motion could be perceived in the heart. After being exposed 5′, the right ventricle began to contract very weakly, the corresponding auricle followed, and the ventricle and auricle of the left side, next in succession. The contractions increased in force, especially those of the right auricle, which nearly obliterated its cavity each time. The left auricle ceased the first, the ventricle of the same side became quiescent, in two hours after the opening of the thorax. At three hours, the auricle and ventricle of the right side were acting with but little diminution of force, though less frequently, being but 4 times in a minute. Night now closing in I was compelled to retire.

Experiment 8. A kitten was prepared, as all the preceding had been. When the heart, was first exposed, the two auricles alone contracted, not more than 6 times in a minute, and with but little force. After having

waited a few minutes, and having found the ventricle remained quiet, I placed the kitten in a glass vessel, containing oxygen gas, over water of 99 or 100°. In the space of 4′, the right auricle became perfectly red, from the blood within it, becoming oxygenated. It was of as florid a colour as arterial blood. The left auricle was concealed by a lobe of the left lung; its motion could, however, be perceived by the agitation of the lung. The action of the auricles, became increased a little in frequency, but much more in force. For 15′, no sign of motion could be seen, in either of the ventricles; once it was thought, there was a slight oscillation of the right, but it was not decisive. Twenty minutes after the animal had been in the gas, both ventricles gave suddenly and spontaneously a powerful contraction. At first there was not more than one, in a minute; in 25′ they contracted twice in a minute, in 30′ 4 times, the auricles also, but 4 times. The contractions of the heart, were now, undoubtedly, and to the conviction of the gentlemen, who witnessed this, as the preceding experiments, much more powerful, than any of the others had been, even when the thorax was first opened. The apex of the heart, was drawn up towards its basis, between one and two lines. The coronary vessels of the ventricles, were of a beautiful scarlet colour. In 40′, the ventricles beat 12 times in a minute, and with so much force, that the blood, began to ooze out from the divided vessels of the lungs, the edges of which I had cut off, to expose the heart.

Although I had done this in the preceding experiments, the blood had never been seen to issue from them, except at the moment of excision. In one hour, the ventricles and auricles beat 8 times in a minute, and with a force apparently greater than before. In 1° 20′ the ventricles beat 32 in a minute, the right auricle 16, the left auricle appeared to have ceased contracting, for the lobe of the lung which covered it, could not be seen to move, as before. In two hours, the ventricles contracted 27 times in a minute, but more feebly, the left contracted the most forcibly. The right auricle appeared to be enfeebled. I was now obliged to leave it, and returned 3° 30′ after the animal, had been placed in the air. The ventricles contracted 11 times in a minute, the right with less strength, than the left. The auricle, however, beat much stronger, than when I left it, but only 4 times in a minute. I remained in the room, inspecting the heart of the animal at intervals, until 20′ before 7 o'clock, which was precisely 7 hours after the animal had been placed in the gas. The two ventricles were acting feebly, compared to the force of their contractions at first, but much stronger, than in most of the former experiments, after they had continued a short time. The right auricle contracted slowly, but with force. It growing dark, I was under the necessity of leaving the experiment. But I am confident, that, from the manner in which the heart was acting when I retired, it must have continued at least two hours longer, if not more.

From considering these facts, circumstances, and experiments unrelated, yet coefficient, I cannot avoid consenting to the belief, that oxygen and excitability, are intimately connected, if the one be not the original of the other. I will not assert, that the manner of its production, at which I have hinted, is true, or in what other mode, they are connected. The task is beyond my limited powers. But life, will not remain forever, the secret ens of nature. The quill of fame, shall enrol upon the page of time, the day, when some medical NEWTON will be born, whose genius winged by intuition shall pass the barriers, to which this "jealous goddess" limits our research; when its springing shoot shall be discovered; and its laws shall be unfolded from the deep, the dark obscurity, with which they are now enveloped.

SECTION SECOND.

ON THE PROXIMATE CAUSE OF THE DISEASE, &c.

THERE is no subject in the range of medicine, which has more excited the disputes, and contentions of physicians, than that of the proximate causes of diseases. Theory has risen on theory, system has been overthrown by system, until medicine, has appeared the wreck of reason, the Egypt of science, where the works of ge-

nius and of ignorance, lay alike mouldering in scarce distinguishable ruins. But this period has passed away, this day of barbarism, when despotick power assumed the right to manacle the godlike faculties of man, and with its iron hand to crush the expanding wing of aspiring intellect, has now declined; and its productions, the offspring of cramped and confined minds, which dared not to deviate from the dictates of authority, to listen to the voice of truth, or to view the form of nature, inviting with "*simplex munditiis*," but only as represented in the canvass of daubing copists, appear too hideous to have had birth, while their existence can scarce command the belief, that they are the children of intelligence. But now, that reason roves far as the wide extended realms of nature, uncontrolled by power, unfettered by adherence to systems, because they are venerable from the continued belief of wasted ages, though unsanctioned by truth, our science has emerged from this unintelligible disorder, to lucid method, and arrangement. The temple of medicine, from this confused mass, erects its noble front, the proudest boast, the honour of our race. It no longer threatens to overwhelm those who approach it, with cumbrous and tottering magnificence, but invites to its entrance with simple majesty. While disease, within this dome, has now lost the hydra form with which it has been represented, and its proximate cause beheld with piercing ken, is no longer confounded with its effects, but appears as morbid excitement.

However true the opinion, that morbid excitement is the proximate cause of disease, may be, with respect to maladies in general, yet it cannot be applied to the disease (if it may be so called) in consideration. In other diseases, such as fevers, &c. there is a peculiar action, specifically different from that of health, which is either above or below the natural standard; suddenly reducing the excitability, or slowly wasting it away. But in this disease, there is an almost immediate suspension of the actions of life, from the capacity, which the system possesses, of supporting and preserving them in existence, being suddenly lost. In the one, the wheels of life are worn down by the powers which propel them, in the other, the wheels being abstracted, the machine must cease to move. It, of course, must have a proximate cause peculiar to itself.

Almost every author, who has written on suspended animation, has advanced an opinion on this subject. I shall not retard my progress to consider them, for they are all erroneous, having been founded on theories of respiration, which are untrue, and are now rejected. They, therefore, could not be correct. This remark is evidenced, by the facility with which each author controverted the opinion of the one, who preceded him, while his own became a victim to his successor. It would be ungenerous, it would be criminal, to rake the remains of learning and talents from the dust, to triumph over their errors, for they are now harmless.

I would present, with much more hesitation, than I do, an opinion of my own on the proximate cause of this disease, if any investigation had been expressly made into it, since respiration has been better understood, and its final cause began to be investigated. I will not undertake to declare I am correct in my notion; for that which satisfies me, may not content a more discerning mind. But if I have gone astray, I hope to be presented with some Mentor's hand, which may restore me from my wandering.

If the opinion, I have endeavoured to render probable, in the preceding section, approach the truth, or if it be only admitted, that oxygen, is immediately conducive to life, more than as a mere stimulus, even if it be so, in some manner of which we are ignorant, we shall then have approached to a view of that, which we seek.

Believing this opinion to be true, it appears to me, that the proximate cause, consists in the defect, or absence of that principle, or quality, which oxygen bestows upon the system or animal fibre, to which perhaps it unites; or the cessation of that effect, which oxygen produces in the animal economy, immediately and directly connected with life; owing to a want, or a deficiency of oxygen in the arterial or nutritive blood. Or, in other words, if oxygen be the principle which produces excitability, irritability, or any other effect immediately vital, yet hidden from our view, then, the proximate cause, is the absence of either of these from the system, produced by the defect of oxygen in the blood.

I shall not consume any of the little time allotted for the preparation of a thesis, in endeavouring to maintain this supposition. It rests upon the truth of the doctrine I have supported, that oxygen is immediately vital, or a sustainer of life, otherwise than as a stimulus. If that be false, this cannot be true; if that should stand the test of reason, it will sanction this. I shall, therefore, without delay, proceed to examine the effects of drowning, on the vital organs.

When an animal is immersed in water, it immediately throws out air from its lungs, and attempts to inspire, by which means, water passes into the trachea. It then struggles with violence, and endeavours to raise itself to the surface of the water; again ejects air from its lungs and assaying to breathe, receives water again into the trachea, part of which probably passes into the lungs, while some of it goes into the stomach. The animal now becomes more agitated and convulsed, than before; a few bubbles of air escape from its lungs, and it may be seen to expel a small stream of water from its mouth, mixed with, and clouded by mucus. The struggles, then, diminish in force, the head sinks down, the mouth is wide open, the tongue somewhat projecting, the eyes are open and protruding, and the pupils mostly dilated. The animal is now no longer convulsed, but at intervals feebly moves its extremities, and the body curves, which is produced seemingly by a contraction of the diaphragm. These also quickly terminate, and the animal is taken out of the water apparently lifeless.

If the thorax be immediately opened, the heart will be found contracting weakly, especially the ventricle and auricle of the right side, which are distended with black blood; while the left contains but a small portion, which through the thin sides of the auricle appears also black. The cavæ and all the veins are also distended with black blood. These appearances, show that the circulation has been entirely suspended; and from the blood being collected in the right side of the heart, that a remora had occurred to its passage, through the lungs. These viscera are generally found in a state of greater or less collapse. At one time, I have observed it to be small, at another great. When they are cut into, a frothy fluid, which seems to fill very nearly, the extreme bronchial cells, escapes; demonstrating that water has penetrated into them. The experiments of Berger, however, decide this contested fact. In all his experiments, he found the lungs of animals drowned, in coloured water, to be more or less tinged. The stomach always contains water; sometimes I have found it in considerable quantity. The brain, whose function is early suspended in this affection, appears to be nearly natural. A little more congestion is observed, in the veins of the pia mater, than in subjects who have died of diseases, which did not materially affect this organ, though not sufficient to have produced any evil consequences. Even in the death from hanging, the brain is not as much engorged with blood, as we might suppose. I assert this from

having once had an opportunity, of examining and comparing the brain of one hanged, with that of a person who had died with disease, between which, but little difference existed.

There can be no doubt, that the death, induced, either by drowning or hanging, does not proceed from an affection of the brain, and indeed, I believe death, in either case, to be entirely independent of the brain, and that the greater or less engorgement of this organ, does not hasten or retard it one moment.

If the proximate cause of the disease induced by drowning, which I have proposed, be true; and as the appearances of the system, after death, evince a total stop being put to the circulation, and the vital effects of the blood, there arise, then, from this knowledge, two indications of cure.

1st, To restore the blood to that state, in which it is salutary to the system, and essential to its life, by impregnating it with oxygen.

2d, To restore the circulation, that this effect of oxygenated blood may be produced on the system.

The means of fulfilling these indications, form the subject of the next section.

SECTION THIRD.

THE MODE OF CURE.

The practice, which has been adopted, for the recovery of drowned persons, has varied at different periods. The remedies, which have been employed, have frequently tended more to the total extinction of the portion of life that might have remained, than to promote the recovery of the patient. It has, indeed, been too often marked by empiricism; the invariable consequence of principle, being deserted or despised in practice.

It was my intention to have entered into the consideration of the different modes, recommended by authors, and to have pointed out, how far they were beneficial or injurious. But I am obliged to abridge this part of my design. Those remedies, however, they have advised to be employed, which I thought useful, I have adopted, those which I thought injurious, I have rejected.

There is, however, a treatment yet pursued, occasionally, by the vulgar, which I must notice, and which cannot be too strongly reprobated, nor too quickly prohibited. I allude to the practice of rolling and shaking persons just drawn from the water.

I have heard of an instance of suspended animation, which occurred near this city, a short time since, in which this practice was the destruction of a person. It was a woman, who had sunk but for a few moments, and

exhibited manifest symptoms of life, when taken out, but which entirely disappeared, by rolling her on the gravelly bank; while another, who owed her safety to being thought dead, and was therefore permitted to lie on the shore in the sun, recovered. It is therefore the duty of every well informed individual, to discourage a treatment of such an injurious tendency.

To fulfil the indications of cure, mentioned in the last section, the remedies divide themselves into,

1, the preparatory, or the means to be employed, in preparing the subject, for the further more effectual treatment.

2, those which oxygenate the blood.

3, Those which stimulate the system, and at the same time oxygenate the blood.

4, those which merely stimulate the system.

The means under the first head to be resorted to, are those which, from necessity, must be employed by the persons present, when the body is obtained from the water; and consist, merely in conveying the body, with the least motion to it possible, to the nearest habitation, and placing it in an airy room, in which a small fire may be kindled. The clothes are then to be taken off, as speedily as possible, and the body wiped dry, with warm cloths or flannel, but with no more agitation or friction to the body, than is absolutely unavoidable. It is to be placed on a table overspread with blankets, or with a feather bed, and the body is also to be covered

with warmed blankets. While these preparations are making, medical assistance should be sought, and the apparatus belonging to the Humane Society, should be brought from the nearest place of deposit.

No other treatment should be pursued, until a physician shall be present, if there be a possibility of one being speedily procured. If there be no probability of obtaining the assistance of a physician in a short time, some careful and judicious person, may endeavour to pursue the directions which are here related.

To the ardent and officious zeal of bystanders, proceeding certainly from the most laudable motives, but ill and fatally directed at the commencement of the process for cure, too often, is every future effort, rendered unavailing.

When a physician has arrived at the spot, he should proceed immediately to fulfil the first indication of cure, by the employment of the

2d class of remedies, which is the oxygenation of the blood; and which is to be obtained by the inflation of the lungs. This is to be considered, 1st, as to the instruments to be employed: 2d, as to the agent by which accomplished. 1, The apparatus which it appears to me is best calculated to attain the desired object, is the following:

There should be a small ivory tube to be introduced into the trachea, curved at the extremity, which goes into the fauces; such a one as Coleman describes; and having

the end which passes into the trachea small, and round, or flattened on its sides. I prefer having this of ivory, or any other hard, or smooth substance, as a firm body can be passed with more ease into the trachea, than one which is flexible. The end of this tube, which projects from the mouth, should be made to screw to the end of a leather tube, whose opposite end, is to be made so as to be attached to the pipe of a double bellows.

This is more eligible than the common single bellows employed, for with it an artificial respiration, almost as complete as natural, can be supported. But with the single bellows, the use of the tube to enter the trachea cannot be had; for it would be necessary, each time compression is made on the thorax, in order to effect an expulsion of the air it contains, to remove the ivory, or unscrew the leather tube. The inconvenience resulting from this is obvious. And if the tube be not employed, the lungs cannot be so completely, nor so easily inflated; for notwithstanding all our efforts, great part of the air will pass into the stomach; and into the whole tract of intestines.

This apparatus, I think, can be employed by a physician with ease; and this method of inflation being more complete, than that in common use, I should suppose is certainly to be preferred. The artificial respiration, by this means, makes an approach to the natural; the lungs are fully distended, and again emptied, with little violence to the system; and at each inflation, fresh air is forced into them.

But whoever has seen the operation performed with a single bellows, in the common way, must have been struck with its insufficiency. The compression on the thorax, to expel from it any air, must be violent, and even then cannot effect completely the purpose for which it is intended; for the greater part of the air, in the inflation, does not enter the lungs, but passes into the stomach. The irritability of the heart, the last lingering spark of life, would soon be, as I fear it too frequently is, entirely extinguished by this treatment.

The principal objection to this apparatus is, that there is some difficulty in introducing, and keeping the tube in the glottis. But a physician who is acquainted with the structure, and situation of the parts, cannot fail succeeding to place it in the situation he wishes; and a little attention will answer to preserve it there. The following directions, however, may assist to attain a knowledge of the proper manner of introducing it. The mouth should be opened to its utmost extent, and the tongue drawn forwards. The epiglottis can then be distinctly perceived. The tongue will mostly remain in this situation, if not, it may be retained by an assistant. The fore-finger of the left hand, (if standing on the right side) is to be introduced into the fauces, and is to be placed over the epiglottis, which should not be pushed down on the opening of the glottis: then, holding the tube in his right hand, the physician passes it along the finger of the left, until it has passed over the epiglottis; and

with the same finger, he can direct and place the extremity of the tube, into the opening of the glottis. The tongue is, then, to be put back, into its natural situation. An assistant, standing at the head of the patient, should hold the tube, to prevent its falling from the glottis, and rather push it down and forwards. The flexible leather tube attached to the bellows, should be screwed to the ivory tube, and inflation should be immediately commenced.

That this apparatus may be employed, the Humane Society should have it left, at the different places of deposit. If this cannot be obtained, necessity will obligate the use of that, which is at hand, and can be directly employed.

A common bellows is to be procured, the pipe of which is to be fixed into one of the nostrils, which is to be compressed round it, by an assistant, who with the same hand closes the other nostril; keeping the mouth shut with his other hand. Another assistant presses on the cricoid cartilage, compressing the œsophagus, and thus preventing the air from going into the stomach. A third and fourth, raised on stools, one on each side the patient, are to make compression with their hands on the thorax, whenever it is completely distended; while the physician, or an assistant blows the bellows. The first apparatus, I think, is to be preferred, if it can be obtained.

2. The agent, which accomplishes the intention of inflation, is atmospheric air, which ought to be as pure as it can be obtained. To this end, no more persons should be permitted to remain in the room, than are wanting for assistance. If it should be night, as few candles should be in the room as are necessary; a patent lamp, if it can be procured, is to be preferred to candles, as it affords no smoke. If the weather be not too cold, the windows may be raised, to permit a free circulation of air.

When inflation has been pursued for a short time, so as to render it probable, that the blood, in the vessels of the lungs, is oxygenated, the second indication of cure is to be fulfilled, by the employment of the two remaining classes of remedies, and of these as the more preferable is the

3d, Consisting of those agents which stimulate the system, and at the same time, oxygenate the blood. They may be considered, 1st, as atmospheric air, impregnated with stimulant fumes, or vapours. Vol. sal. ammon., &c. have been recommended, but the best I believe, is the vapour of oxygenated muriatic acid gas. This may either be procured, by pouring sulphuric acid on powdered oxid of manganese and salt, in an oil flask; or the muriatic acid, on the manganese, and applying to the flask a gentle heat. The mouth of the flask is to be held under the air-hole of the bellows, which will thus receive the gas as it is discharged.

The assistants, to avoid being affected by it, should have their handkerchiefs wetted with Aqua Ammon., passed round their mouths; the gas will thus be neutralized, passing into the nostrils.

This gas readily parts with its oxygen to the blood,* and at the same time, it powerfully stimulates the lungs, between which, and the heart a sympathy exists. This circumstance points out the utility of this class of remedies. No danger need be apprehended from the use of this gas; the low state of the excitability, in persons suffering by submersion, will prevent its usual effects; as also will its being diluted with atmospheric air.

I once inhaled a considerable quantity of it, in performing an experiment, yet to be mentioned. It excited violent coughing for a considerable time, and increased the action of my whole arterial system which continued for some hours attended with headache, This is a proof of its stimulant qualities, and of course of its promising good in this disease; and that when diluted, it may be breathed without much danger.

2. The employment of gasses, unmixed with atmospheric air. These however can seldom be used. It is almost impossible, to have the apparatus, necessary to obtain the gasses, at the places of deposit, and indeed if it were, more time would be necessary to procure them, than could be spared; nor can they be kept ready prepa-

* See page 24.

red, without great difficulty and expense. If they could be had, however, some of them might be of advantage.

The first, as the best of these, is oxygen gas. The rapidity with which it produces its desired influence on the blood, and the stimulating effects it exerts on the lungs, render it the most efficacious remedy that can be employed; if means for obtaining it readily and easily could be devised. When this, or the other gasses can be had, they are to be conveyed into a large bladder, or varnished silk bag, fitted with a stop cock, which should be made to screw, into a perforated block of wood, over the air-hole of the bellows.

The second is the oxygenated muriatic acid gas. Mr. Potel, in a late French work, which I have not been able to procure, recommends its use. The Annual Register,* gives an extract from it, informing that a number of drowned rats, which lay on a table where this gas was used, were recovered by it; that the experiment was repeated on other rats with a similar result; and that at last the young chemist "procured the suspension of his apparent animal functions," and was restored by this means.

I must confess, the latter part of the information inclined me to doubt the truth of the whole relation. I, however, determined to make the experiment. For this purpose I drowned a young dog, having first prepared a

* Vol. 2, page 259.

sufficient quantity of the gas, in a gas-holder. It was 15 minutes after he was drowned, before the inflation of his lungs could be commenced. For the more sure and easy performance of the experiment, I performed the operation of bronchotomy, and introduced the pipe of a leather flexible tube, which was connected to the pipe of the bellows, into the trachea. The gas was transferred into a bladder, and retained in it, by a stop cock, which screwed to the air-hole of the bellows. The lungs were distended with the gas, but it escaped in so large a quantity from the mouth of the dog, and so much was received into my lungs, that I was obliged to drop the bellows, and run into the air; I however, immediately returned, and attempted another bladder-full, when the same circumstance occurring again, in a still greater degree, than before, I was obliged precipitately to fly. The coughing it excited, was so violent and long continued, that I could not resume the experiment, nor have I since then been able to repeat it.

I cannot fully rely on it, as the dog, for a first experiment, remained in the water too long, and besides this he had the appearance of being unhealthy, which might have had an unfavourable effect.

The third air which has been recommended, is the nitrous oxid. No experiments, however, having been made with it, its efficacy can only rest on conjecture.

Under this class, may be mentioned, injecting air into one of the jugular veins. It would slightly alter the

blood, but its principle effect would be to stimulate the heart. The quantity of 1-4, or 1-2 of a cubic inch, or even more, might probably be used, not only with safety, but with advantage.

The 4th class, or those which merely stimulate the system, may be employed in conjunction with either of the two preceding classes.

When the inflation of the lungs, is supposed to have oxygenated the blood, the earlier the heart is excited into action, the greater success does it promise. As soon, then, as the blood has experienced this effect, this class of remedies may be employed. In their use, regard should be had, to the part to which they are applied, as connected with the heart or diaphragm, by sympathy: for on this, in a great measure will depend their salutary influence.

The strongest stimuli should be used, for as the excitability is extremely deficient, if weak stimulants only be employed, no action will be excited. Indeed, in this state of the excitability, their employment cannot be excused as doing no injury, for they will, by degrees, exhaust it, without inducing any marked excitement.

They may be divided into, 1st, Internal, and 2d, External. The Internal, are applied to the stomach and intestines; the External, to the skin and nares.

The Ist, or Internal, are,

1st, such as are applied to the stomach, as brandy, and other spirits, which should be given very

warm, or nearly hot; and also volatile alkali. Laudanum is recommended; but I am doubtful, whether its secondary sedative effects, should recovery be obtained, would not prove injurious; for it might, then oppress the system, the actions of which remain for some time, feeble.

Caution is necessary in pouring these fluids down the throat, lest any of them should get into the trachea; but if they be gradually discharged, from a vessel with a spout to it, and the head of the patient be a little elevated, this inconvenience may be avoided:

2d, such as act on the intestines. For this end, different kinds of enemata have been recommended; their utility however is questionable. The connexion, which exists, between the heart and intestines, is weak, even in the full possession of life; and in the asphixia from drowning, it must be still more feeble, and of course, any action exerted on them, would not be extended to that organ, and therefore it could be of no service, but would produce harm; for it would exhaust the little remaining excitability they retained, and not excite the heart. The experiments of Dr. Legare, related in his inaugural thesis, in some measure authorize this conclusion. He found, that although the peristaltic motion of the intestines of drowned dogs, was excited, and the vessels brought into action by the fumes of tobacco, it proved but momentary, and could not afterwards be awakened. If oxygen, or oxy-muriatic acid gas, were to be injec-

ted into the intestines, might they not oxygenate the blood in their vessels? for this principle acts speedily on the blood, through membranes, and even through the sides of the heart;* and as they would stimulate the intestines, the blood of whose vessels would also be oxygenated by them, they might produce partially, beneficial effects.

IId, The External, are,

1st, Electricity. A small electrical machine ought to form part of the apparatus, which the Humane Society deposits at different convenient places. The bottle should be discharged so as to affect directly the heart, or diaphragm, to excite them into action. The charge should be powerful from the commencement, and frequently repeated.

2d, Frictions to the extremities, either with or without stimulant substances.

3d, heat. The objections of Hunter to this remedy are invalid. They are founded on an opinion, which is not established to be true; and for this reason they are entitled to no weight.

4th, Those which affect the nares, as vol. salts, &c. Sternutatories might be of service, to excite the action of the diaphragm. For if natural respiration can be excited, recovery is almost certain. The most powerful of the sternutatories, should be employed; snuff, to one

* See page. 52 & 57.

unaccustomed to its habitual use, will answer well, or the assarabacca of our shops.

These means should be persisted in, for an hour at least; but their continuance for a greater length of time would be proper. It is impossible to determine accurately, the moment when life has entirely ceased, so as to be no longer recalled. While, therefore the most distant hope, while the faintest glimmer of possibility remains, that life can be restored, it is an act meriting the severist censure, it is a measure, almost rising to a crime, to desist from exertion. For is it not the performance of a duty, which man owes to man, to society, to his God? And shall he go and leave this duty unfulfilled, shall he depart and become an accessary to the destruction of a being, shall he hasten away and permit a brother to sink to eternity, " unanointed and unaneled."

Behold the man, who has been the successful agent in restoring one from asphixia by drowning. Does he not feel the glowing consciousness, of having been the instrument of safety, from inevitable destruction? for he has snatched a being from the very embraces of death. Does he not hear the prayers, the blessings of a grateful family, " ascending to the recording angel" of heaven, for his prosperity, his happiness, his health? for he has restored to them a brother, a son, a father, a husband.

These, physicians, are your rewards! Will they not excite you to exertion?

I cannot terminate this essay, without acknowledging the many obligations, under which, I labour to the Professors of the University, for the instruction I have derived from their truly valuable lectures; their willingness to impart information; and personal benefits which, from some of them, I have received. That they may long continue the ornaments of the chairs which they now fill, with annually increasing fame to themselves, and advantage to their pupils, is my most ardent, my most fervent wish.

Ere I finally lay aside my pen, I must request the kind indulgence of my reader, for the errors which may stain these sheets. Let a shade of oblivion conceal them from remembrance, or the hand of lenity blot them from the page. I thus deprecate the censure of the scrutinizing eye, from being conscious that many faults seem to be fostered here, from a want of time necessary for their eradication. I should not urge this apology, if it were not, the period is limited, in which a thesis is to be prepared, of which the greater part was consumed in the performance of my experiments; nor do I presume, this production merits to be considered in any other light which might induce to the procrastination of its appearance. For once, then, may I indulge the hope, that necessity, will plead and will gain a departure from the critick's law,

—— Reprehendite, quod non
Multa dies et multa coercuit, atque
Præsectum decies non castigavit ad unguem. HOR.

IN consequence of the haste, in some measure unavoidable, with which this impression proceeded from the Press, several mistakes have been committed. They are, however, contained, with the exception of a few in the punctuation, in the following list of errata; by a reference to which they can be rectified.

Page 14, line 19, add *it* after behold.
21, — 17, insert *analogous* before experiment.
23, — 3, dele *so*.
33, — 9, for *Bishart* read *Bichat*.
52, — 9, for *being* read *was*.
16, — 25, dele *to*.

IN consequence of the Author, in committing the
voluntable, with which this impression
Press, several inaccuracies have been
are, however, counteracted, with the exception of a few in
the punctuation, in the following list of errata, by a re-
ference to which they can be rectified.

Page 14, line 10, add it after [illegible]
21, — 17, [illegible] manifestations before [illegible]
28, — 3, dele [illegible]
35, — 9, for [illegible] read [illegible]
53, — 9, for [illegible] read [illegible]
[illegible], — 25, dele [illegible]

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