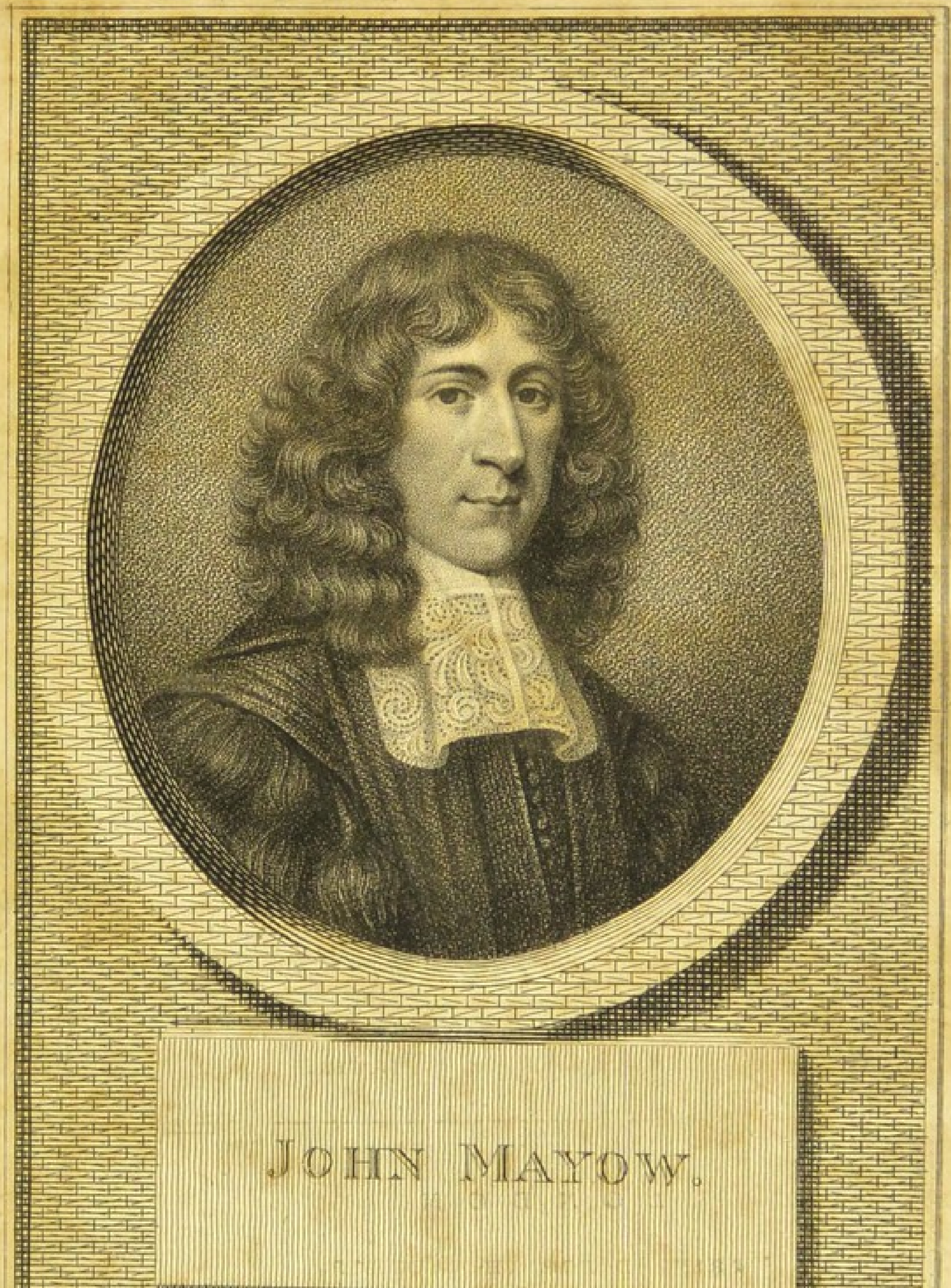
An extremely important work in the history of science. Yeats, like his contemporary Beddoes, drew attention to the merits of Mayow and though, says E. Irving Carlyle, "like most of Mayow's admirers Yeats applauded with too little discrimination, he assisted in the rescuing of his achievements from oblivion."

Yeats was an American born in Florida who took his medical degrees in England. This is his most important work.


JOHN MAYOW.

OBSERVATIONS

ON THE

CLAIMS OF THE MODERNS,

TO SOME

DISCOVERIES IN CHEMISTRY

AND

PHYSIOLOGY.

BY G. D. YEATS, M. B.

OF HERTFORD COLLEGE, OXFORD, MEMBER OF THE
ROYAL MEDICAL SOCIETY OF EDINBURGH,
AND PHYSICIAN AT BEDFORD.

De verbis autem cavillisque cum nemine unquam libuit altercari, quærenti derisori scientia ipsa sese abscondit, sed studioso veritatis obviam venit et se præbet conspiciendam. WALÆUS.

Tros Tyriusve mihi nullo discrimine agetur. VIRGIL.

LONDON:

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AND SOLD BY J. DEBRETT, OPPOSITE BURLINGTON-HOUSE, PICCADILLY.

1798.

TO

DR. MAYO, DR. VAUGHN,

AND

DR. P. MAYO,

PHYSICIANS TO THE

MIDDLESEX HOSPITAL,

THE FOLLOWING WORK IS INSCRIBED,

AS A MARK OF ESTEEM,

BY THEIR SINCERE FRIEND,

THE AUTHOR.

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When I became acquainted with the more recent theories in chemiſtry and phyſiology, I was taught to believe, that they had been excluſively broached by the philoſophers of the preſent day; but having applied myſelf to the reading of authors of more early date, I was ſurpriſed to find, that not only many of the modern theories had been delivered and explained, but alſo, that a conſequent practice had been adopted and followed. The more I examined into their writings, the more was I confirmed in the belief, that the labours of many ingenious men had been unaccountably overlooked, or greatly miſrepreſented. Chemiſtry, in particular, which has blazed with ſuch ſuperior luſtre from the Continent, forms a diſtinguiſhed portion of the effects of modern inveſtigation.

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INTRODUCTION

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The diſcovery and decompoſition of the different gaſes, will give a permanency to that branch of ſcience; and which, with ſome theories in phyſiology, very much contribute to revive and confirm the opinions of many eminent men, of the laſt and preſent century. This aſſertion will, no doubt, cauſe ſurpriſe in ſome; but if they will candidly and impartially examine the ſubject, they muſt be convinced of the fact. It is certainly, *primâ facie*, a matter of aſtoniſhment, how it happens, that theories and experiments, clearly and fairly explained, ſhould have eſcaped the obſervation of medical men, till more recent inveſtigation attracted their attention to the ſubject. It is difficult to account for the prejudice of the human mind, with its conſequent rooted attachment to eſtabliſhed ſyſtems; but there, certainly, is evident, in the popular mind, a proneneſs, at particular periods, to the reception of certain ſyſtems, in like manner as there exiſts in the body, a prediſpoſition to diſeaſe, at different times. That cuſtom powerfully operates to confirm prejudices, however abſurd, and to enſlave the mind to theories, however ridiculous, is evident from many

diſtinguiſhed

diftinguifhed æras in the annals of philofophy. When men have fpent, probably, a long life in perfuading themfelves that a particular fyftem is true, they receive, with reluctance, any alterations from the more fuccefsful labours of cultivated genius. The reliance upon a certain remedy, becaufe it has fometimes fucceeded; the attachment to a particular theory, becaufe they have been taught to believe it; the envy, perhaps, of not being themfelves the difcoverers; all confpire to raife the fpirit of oppofition, againft the intrufion of philofophical innovation. ' Multi ob fauftos eventus femel aut bis obfervatos ex aliquo remedio, vel potius ob innatam quendam proclivitatem tum laudendi tum fingendi, ad libitum medicamentorum vires ita erga remedium aliquod efficiunter, ut putent, illud unum ad curandos quofque morbos fummam vim ac veluti imperium obtinere *.

I need fcarcely obferve, how lamentably often this has happened, and how long it obftructed the progrefs of medical knowledge. Notwithftanding the difcovery of the valves of the veins, by Fabricius,

* Baglivi de Praxi Medica. P. 18.

Fabricius, Cesalpinus, and others, so wedded were medical men to the Galenic doctrine, that they not only had no idea of their use, but laboured to explain them, as subservient to that theory; and the liver still maintained its pre-eminence in the system. Circumstances, like these, seem long to have retarded the knowledge of the circulation, and after our eminent countrymen demonstrated, with the precision of mathematical truth, his brilliant discovery, the partial admiration of Galen produced so violent an opposition, that it prevented the diffusion of the discovery, with that celerity, which its great importance required.

These thoughts naturally pressed themselves upon my mind, when I reflected, with what coolness the beautiful experiments of Mayow were received, upon their publication. Retired from the world, he planned and executed, in the cloisters of a college, experiments the most elegant and decisive, that the greatest genius could contrive. Unassisted by the labours of others, not encouraged by the adoption of his opinions, his aspiring genius soared into the regions of truth, amid the obstacles of surrounding opposition. Every one

who

who is acquainted with Mayow's writings, and the ſpirit of his expreſſions, muſt acknowledge and regret, that his early death was a great loſs to ſcience*. Had he lived, he would, no doubt, have preſſed his accumulated truths upon the minds of men, with reſiſtleſs evidence.

It is an obſervation, juſtified by the pages of philoſophy, that the adoption of ſyſtems, is the reſult of the preparation of the minds of men, by a continued attention to the phænomena of nature. The labours of an individual, however ſucceſsful, will not ſuffice to convince the erroneous, or to reclaim the bigotted. It appears, that a particular ſtimulus is wanting, to direct, to the particular object, that general attention, which the brilliancy of genius cannot always command. A revolution in ſcience, is always attended with a revolution in terms; and theſe are often applied, from a particular theory: hence the difficulty, without a painful and laborious attention, of arriving at the ſpirit in the expreſſion of the diſcoverers. But when experiments are made, by ſeveral philoſophers, quickly

* Vide Wood's Athenæ Oxon.

quickly ſucceeding each other, the mind is prepared, by a *partial* acquaintance with terms; and, therefore, receives, with leſs reluctance, the full completion of the diſcoveries. The quick ſucceſſion of ingenious experiments, from the induſtry of Hales, of Prieſtley, and of Black, paved the way, and cauſed a ready reception, for the beautiful ſyſtem of Lavoiſier; and had not anatomy directed the attention of medical men, to the diſcovery of the valves of the veins, immediately previous to Harvey, the knowledge of the circulation would, probably have remained, for ſome future examiner into the operations of the animal œconomy. When it remains for an individual, whoſe acute obſervations is not ſatisfied with the contradictions in an eſtabliſhed ſyſtem, to plan and execute a revolution in ſcience, he may expect a cold reception, and for a time, a rejection of his opinions. When, however, from the evolution of events, and a more minute acquaintance with natural phænomena, the mind is, in ſome degree, diveſted of its prejudices, the new ſyſtem begins to undergo inveſtigation, and if conſiſtent with truth, acquires its votaries from candour and im-

partiality.

partiality. This ſeems to have been preciſely the caſe with Mayow. His ſyſtem ſuddenly burſt upon the literary world, with all that beauty and energy, which adorn the pages of the modern chemiſt. His terms perplexed, becauſe they were not properly ſtudied, and his doctrines were neglected, becauſe, at that time, ſevere attention was neceſſary to underſtand them.

When a ſyſtem of ſcience, which future diſcoveries prove to be falſe, has been adopted and believed, for ages, we are naturally ſurpriſed to find, that errors, ſo groſs, ſhould have been maintained, or that facts, ſo ſimple and obvious, ſhould have eſcaped obſervation. The unprejudiced mind, engaged in its purſuits, beholds, with a mixture of ſurpriſe and curioſity, the diſcoveries of the ingenious, and contributes its exertions to examine theſe new opinions. Novelty, indeed, with ſome, independent of the importance of facts, has a conſiderable effect in enchanting the mind; and when aided by the charms of eloquence, and enforced by the powers of argument, it enſlaves us often to prejudices and contradictions. But when time has wiped away the force

of

of novelty, and the effervefcence, caufed by the promulgation of the difcoveries, has fubfided, the mind retires, to examine into their authenticity, and to award its juftice. When we find, from enquiry, that the new difcoveries have arifen from objects, daily prefented to our obfervation, we are naturally led to examine, how far the ingenious, of paft ages, were acquainted with them; and if they poffeffed the knowledge, what obftacles prevented its completion, or retarded its tranfmiffion to pofterity. Speculations, like thefe, prompt us to trace, in authors, the opinions of paft ages; and free from the bias of party zeal, we can make a candid enquiry, and give an impartial view of the merits of the difcoverers. The fimplicity which adorns the late difcoveries in chemiftry, will naturally fuggeft fuch reflections, and will induce us to imagine, that phænomena, fo ftriking and obvious, muft have arrefted the attention of many ingenious men, who flourifhed in paft ages; accordingly, we find in many, the moft pointed remarks, and the cleareft expreffions, accompanied by proofs, by which we cannot but believe, they underftood a confiderable part of modern chemiftry.

The doctrine of the convertibility of air into other ſubſtances, and *vice verſâ*, is very antient. Even as far back as the time of Pythagoras, we trace a remarkable co-incidence between his doctrine, and that of modern times. It is needleſs for me to mention, that we have no writings, either of Pythagoras himſelf, or of any of his immediate ſcholars: of courſe, our notion of his philoſophy, muſt be taken from the occaſional mention of him and his opinions, in the antient philoſophers and poets. The doctrine of this ſage, cannot be better explained to the reader, than by quoting the paſſage, concerning it, from the fifteenth book of Ovid's Metamorph.

Hæc quoque *non perſtant*, quæ nos *elementa* vocamus.
Quaſque vices peragant, animos adhibete, docebo.
Quatuor æternus genitalia corpora mundus
Continet. Ex illis duo ſunt onerofa, ſuoque
Pondere in inferius, tellus atque unda, feruntur;
Et totidem gravitate carent: nulloque premente
Alta petunt, aer atque aere purior ignis.
Quæ quanquam ſpatio diſtant, tamen omnia fiunt
Ex ipſis, et in ipſa cadunt: reſolutaque tellus
In liquidas roreſcit aquas, tenuatus in auras
Aeraque humor abit; dempto quoque pondere rurſus

In ſuperos aer tenuiſſimus emicat ignes.
Inde retro redeunt, idemque retexitur ordo.
Ignis enim denſum ſpiſſatus in aera tranſit,
Hic in aquas, tellus glomeratâ cogitur unda.
Nec ſpecies ſua cuique manet: rerumque novator,
Ex aliis alias reparat natura figuras.
Nec perit in tanto quidquam (mihi credite) mundo,
Sed variat, faciemque novat, naſcique vocatur,
Incipere eſſe aliud, quam quod fuit ante: morique
Deſinere illud idem. Cum ſint huc forſitan illa
Hæc tranſlata illuc; ſumma tamen omnia conſtant.

V. 237.

The ingenious Dr. Gregory, who fills, with ſuch diſtinguiſhed merit, the practical chair at Edinburgh, in anſwer to a letter which we wrote to him, makes the following obſervations. ‘ The co-incidence between the doctrine in this paſſage, and ſome of the moſt modern chemical diſcoveries, eſpecially thoſe about air and water, appears to me very ſtriking. I reckon it merely accidental. But alluding to the admiration of the antients, which prevailed for two centuries and more, after the revival of letters, and the eagerneſs which ſome learned men ſhewed, to refer to the antients every modern diſcovery and improvement, I ſaid, in my jectures, 'There was a time when, upon the faith of that coincidence, the credit of our moſt important modern diſcoveries in chemiſtry, would have been transferred from the real author of them to Pythagoras, or perhaps to Hermes.'

The convertibility of different ſubſtances into each other, was attended to, we ſee, by the Pythagorean ſchool. Lucretius, too, has mentioned this doctrine, in his work De Rerum Natura; and ſays, that air is conſtantly changing into other ſubſtances, which are again decompoſed into air; and were it not for this continual round, every thing, at laſt, would remain converted into air.

Aera nunc igitur dicam, qui corpore toto
Innumerabiliter privas mutatur in horas,
Semper enim quodcumque fluit de rebus, id omne
Aeris in magnum fertur mare, qui niſi contra
Corpora retribuat rebus recreetque fluenteis,
Omnia jam reſoluta forent et in aera verſa.
Haud igitur ceſſat gigni de rebus, et in res
Recidere aſſidue, quoniam fluere omnia conſtat.*

 This

* Titi Lucretii Cari, de Rerum Nat. Lib. V. v. 274. Francofurti, 1583.

This idea, no doubt, first presented itself to this sect, from observing water to escape in a state of vapour, and from attending to its subsequent condensation. The different fermentations shewed to them, that solid bodies were reducible into fluid masses, and that, during this conversion, a considerable quantity of *vapour* escaped; hence they concluded, and not without reason, that these substances were changed into air. Water, they saw, in a solid, fluid, and vaporific state, according to the degree of heat; but they were not aware of the cause of these different states. It was imagined, then, by the Pythagoreans, to speak in modern language, that all substances were composed of the same elementary principles, in different states of combination. By spontaneous changes, these were resolvable, reciprocally, into each other, according as circumstances varied. A spontaneous decomposition was continually going on, which varied the face of the creation; nature was employed, in one continued circle of operations; and the various objects which presented themselves to their observations, were changed, modified, and renewed; and thus origi-

nated

nated the doctrine of the Samian sage. These, were facts acquired from the experience of a long course of years, and, by no means, the result of a particular enquiry. A philisophy like this, too, would have been the first to be adopted, by an attentive observer, as it arose from facts, the most obvious and the most striking; accordingly, we find the principles of it traced to the earliest period.*

With notions like these, it is not difficult to understand the origin of metempsychosis. The Pythagoreans contended, that nothing was perishable: substances, which appeared to be destroyed, by no means suffered a total annihilation; it was only a modification of form. A transition, from the material to the immaterial part of the creation, was easy; the analogy was striking, the conclusion inevitable. This doctrine was regular and systematic, in every branch of it; it comprised a complete and uniform plan of philosophical principles; and as it wore the appearance of truth and simplicity, and was delivered out with the authority of

* It is said, that Pythagoras borrowed the principles of his doctrines from the Gymnosophists, or Indian Brahmins.

age and experience, it required numerous votaries, and enslaved the mind, for many subsequent centuries.

That admirable philosopher, Mr. Boyle, imagined, that water might be converted into air. He relates an experiment, which he says he made, in consequence of effects produced by a machine, which Kircher had invented, and by which he supposed the water was changed into air. He took a glass vessel, capable of containing about three ounces: He filled it with oil of vitriol, diluted with half its quantity of water, and having thrown in some pieces of iron, he inverted it into another vessel, containing the same fluid. Air was soon disengaged, in considerable quantity; and the acid, of course, would be proportionably depressed. This air, he says, very soon filled the inverted glass; and it is worthy of remark, he adds, that when he applied his warm hand to the glass, containing the air, it expanded very rapidly. This experiment, he says, although it does not absolutely prove, that air is produced from water, yet it shews, beyond a doubt, that air may be generated *de novo*. The reader knows, that it was hydrogen which Boyle

here

here procured, one of the conſtituent principles of water. What induced him to add water to the acid, in this experiment? Was he aware, that the oil of vitriol did not act upon iron ſo rapidly, as when united with water? Certain it is, that the water was added, from Boyle's own words, with a view to decompoſition. How beautifully has this truth been confirmed, by the experiments of the Honorable Mr. Cavendiſh, who firſt diſunited the component parts of water. Little has it been imagined, however, that ſimilar ideas were entertained, previous to the preſent age. The ingenious Mr. Boyle made many experiments on animals, with his air-pump. It will be proper to ſay ſomething of them, as they involve many important facts, and as Mayow quotes them in his work.

It is well known that the Honorable Mr. Boyle was conſidered, as one of the firſt experimentaliſts of the age in which he lived. He invented a variety of ingenious contrivances, for carrying on his experiments. The moſt ingenious, as well as the moſt uſeful, was the air-pump, which he conſiderably improved from Otto de Guerick, its celebrated inventor, and which firſt inconteſtibly

 proved,

proved, that animals could not live, one moment of time, without the presence of air. He found, that a variety of insects, as flies, bees, worms, &c. were immediately deprived of the power of motion, by exhausting the air-pump, and that mice, sparrows, &c. died, when placed in the same situation. He made the experiments with fish, with the same result; and observed, that they turned upon their backs*. Such remarkable phænomena could not pass without a comment, from so acute a philosopher as Boyle; he subjoins some observations (doubts as he calls them) on respiration, so intimately connected with these experiments. He modestly confesses, that his experiments are not sufficiently numerous or satisfactory, to warrant positive assertions; but they have suggested to me, he says, some new ideas, with respect to the use of air, by which my former distrust of the general received opinion, is much strengthened; I am, therefore, inclined to seize the present opportunity, and relate my doubts.

That

* This fact is very ingeniously explained by Mayow, as the reader will see, by turning to Sect. XIV. P. 255.

That respiration is not intended to cool and condense the blood, as many philosophers of the first respectability imagine, is evident, from many facts; but particularly from observing, that to animals of cold blood, and old people, whose circulation is languid, and heat deficient, respiration is necessary; also, in some diseases, animal heat is so much exhausted, that were respiration to cool the blood, much injury would be done. There are others, of the first note, who imagine, that the very substance of the air enters the lungs, and passes to the left ventricle, not only to cool the blood, but also to prepare the way for the generation of animal spirits. This opinion he does not admit, although it comes nearest to the truth of any. It is the opinion of others, again, that respiration is intended to pass the blood, from one ventricle to the other, by which it is enabled to discharge much noxious effluvia*, which keeps up the vigour of the circulation, in a similar way, as

* Even after the revolution of more than a century, this opinion continued to prevail; and Dr. Priestley endeavoured to confirm it, after his discoveries Vide Vol. III. P. 55; and Kirwan on Phlogiston.

as a free egreſs to the ſmoke cauſes a fire to burn well. He agrees, in part, with this opinion; and relates an experiment, in confirmation of it. He confined a bird, under a glaſs veſſel, and it died in about three quarters of an hour; this happens, he ſays, becauſe the exhalations from the body have ſo infected the air, as to render it unfit for reſpiration. This is not to be wondered at, he obſerves, becauſe the inſenſible perſpiration exceeds the other diſcharges of the body. Notwithſtanding this, it may be reaſonably ſuſpected, that the air ſerves other purpoſes, beſides carrying off effluvia from the lungs; we may therefore conclude that there is ſome other uſe of the air, not yet diſcovered, which makes it ſo uninterruptedly neceſſary to the life of animals. Paracelſus, indeed, obſerves, that as the ſtomach digeſts the food, and converts one part to the uſe of the body, and rejects the other; ſo the lungs conſume one portion of the air, but does not admit the other. It would appear, therefore, that we ſhould agree with this philoſopher, that there is a certain vital elixir in the air, neceſſary for reſtoring our vital ſpirits.

I take the preſent opportunity, ſays Boyle,

of

of relating the invention of Cornelius Debrell, a man deſervedly celebrated, on account of his ſkill in chemiſtry and mechanics. It is related by many of the firſt credit, that he built, for our late King James, a ſmall boat, ſo conſtructed, as to be rowed under water, which experiment was made, with wonderful ſucceſs, in the river Thames. This boat carried twelve rowers, beſides paſſengers, one of whom is living at this day, and related the ſtory to a great mathematician, from whom I got it. I alſo mention it, ſince neither curioſity has been wanting on my part, nor have I let ſlip any opportunity, of making a particular enquiry among the relations of Debrell; but chiefly I enquired of an ingenious phyſician, who married his daughter, how it could poſſibly be effected, that men ſhould remain ſo long under water, with perfect impunity. I was told, that Debrell imagined, that it was not the whole body of air, but a certain quinteſſence, or ſpirituous part of it, which ſerved reſpiration, and which being conſumed, the remaining part of the air became effete, or lifeleſs, ſo that it was unable to maintain the vital flame, reſiding in the heart. Therefore (as far as I could

procure

procure intelligence) besides the mechanical contrivances of a vessel, he had a certain chemical liquor, which was the great secret in this submarine navigation; for as often as he perceived the purer part of the air was consumed, or too much injured by respiration, and infected with the effluvia of the navigators, he opened the vessel containing this fluid, which suddenly restored to the injured air so much of its vital parts, as enabled it to serve the purposes of respiration, for some time longer. Boyle says, that Debrell never told any, of what this curious liquor was composed. I have related this story, says this ingenious philosopher, not because I think it altogether true, but because it is not mentioned by any author, nor is it generally known. With respect to respiration, I am sometimes inclined to agree with those, who imagine, that the air is necessary to maintain the vital flame in the heart; for in our experiments with the air-pump, when the air is exhausted, flame will not continue much longer than the life of an animal. Although our experiments shew a *new* similarity between life and combustion, yet this opinion is opposed by many

 difficulties;

difficulties; for although in the hearts of animals the blood is warm, it is impoſſible to conceive, how the air can arrive at the heart, and if it did, by what means it can increaſe the heat; for although the air increaſes the heat of burning coals, yet being blown on warm liquors, it cools them.

Such are the doubts which Boyle has propoſed, without determining for any one opinion. How near was he to the truth, when he obſerved, that there was a ſimilarity between life and combuſtion. Here experiments were wanting; and had Boyle varied his, he might have diſcovered the cauſe of animal heat. He did nothing more than prove, that animals were killed, and flame extinguiſhed, in an exhauſted receiver. This indeed demonſtrated, that air was abſolutely neceſſary to animal life and combuſtion; but in what way it anſwered theſe great purpoſes, continued as great a myſtery as before. It remained, too, to be ſhewn, that only one part of the air entered the blood, during reſpiration, and was conſumed by combuſtion.— Here it was that Mayow, who was cotemporary with Boyle, but when Boyle was far advanced in years, commenced our knowledge of pneumatic

chemiſtry.

chemiſtry. His invention of a pneumatic apparatus, the happy manner in which he executed his experiments, and his application of his diſcovery of vital air, to explain the wide influence of its effects in reſpiration and combuſtion, muſt give us a high idea of his genius. The works of this ingenious phyſician, certainly contain a great part of thoſe noble truths, which have given reputation to the names of Prieſtley, Scheele, Lavoiſier, and others. The reader, we truſt, will be convinced of this, from peruſing this publication, a conſiderable portion of which is taken up in examining the opinions of Mayow.

We are aware, that the attention of the medical world has been already directed to the works of this author, by two very eminent men, Dr. Scherer and Dr. Beddoes. The analyſis of the former is accurate, but deficient, as not containing the beſt and moſt noble of Mayow's diſcoveries: the extracts, too, are publiſhed, without pointing out Mayow's opinions, as they quadrate with thoſe of the moderns. Much laudable zeal is, however, diſplayed by Dr. Scherer, to reſtore to Mayow his long loſt honours; but the analyſis is incomplete, and does not impreſs the mind of the reader with the extent of Mayow's diſcoveries. We have aſſerted, as the reader will ſee in the work, that Mayow was not encouraged, by the adoption of his doctrines, during his own life time. This is contrary to the opinion of Dr. Scherer, who ſays, the Royal Society of Arts in London, Henry Munday, Phyſician at Oxford, and ſome others, received and adopted his doctrine, with pleaſure. An account of Mayow's experiments was, indeed, given in the Phil. Tr. previous to their publication; but, ſays Dr. Beddoes, the ſullen reviewer in the Tranſactions, could never once prevail on himſelf to ſmile upon the fair features of this new-born offspring of ſcience. I know not, upon what authority Dr. Scherer attributes the adoption of Mayow's opinions to Munday. I can ſay, from a peruſal of this Author's work, that he does not mention him*. The ingenious Dr. Beddoes was the

* Βιοχρηστολογια, ſeu Commentarii, de Aere Vitali de Eſculentis, de Potulentis cum Corollario de Parergis in victu; Auct. Hen. Munday, Med. Oxon. 1680. I heard of this work in town. I knew, from conſulting Wood's Athenæ, that

the firſt, who held up the merit of Mayow to the literary world. The abſtract, however, which he publiſhed, was rather haſtily done. He wiſhed only to let the world know, that there was ſuch a book in exiſtence, which merited the attention of the literary. His profeſſional, as well as other important avocations, hurried him too much, in detailing the beauties of Mayow; and, indeed, the ingenious Dr. Beddoes, with a candour and liberality peculiar to himſelf, has written me, that he is ſenſible his extracts from Mayow have been publiſhed in too curſory a manner *.

Notwithſtanding the opinions of Mayow did not meet with the immediate reception which a candid examination would have produced, yet he was afterwards taken notice of, by ſeveral authors of credit and reſpectability. Many, however, by not under-

that he was cotemporary with Mayow. I, therefore, went immediately to Oxford, to examine the Bodleian Library, with full hopes of finding an adoption and explanation of Mayow's opinions. I was, however, miſerably diſappointed, not only in this, but in not diſcovering any thing worth attending to. Appendix No. VI.

* Appendix No. I. and III.

underſtanding him, rejected his opinions; and others attributed to him doctrines, which he never held. Jocher, in his Lexicon, ſays, that Mayow's opinions did not meet with approbation; Dr. Beddoes imagines, that he aſſerts this upon the authority of Morhof. In the Polyhiſtor. no notice is taken, of what reception the nitro-aerial doctrine met with, in Mayow's own time: but he is repeatedly quoted by Morhof, on different ſubjects, and his opinions adopted*.

It is not neceſſary to ſay much about the nature of the following work; the table of contents ſufficiently ſhew that†. It was undertaken with a view of examining, how far the recent publications on chemiſtry and phyſiology, are entitled to the claims of novelty. I have long accuſtomed

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* Dr. Beddoes ſays that he wiſhed to ſee Morhof's account of Mayow, but unfortunately the Polyhiſtor had no index. The edition in his poſſeſſion muſt be very imperfect, for the one I was fortunate enough to procure in London has a very copious one.

† It was originally the intention of the author, to have given a large and full table of contents; but he found, that it would unneceſſarily increaſe the ſize of the work.

myſelf to, and been peculiarly delighted with, reading the works of the laſt century; an age diſtinguiſhed for the production of many learned men, in every department of ſcience. I have often ſtole an hour from my more immediate profeſſional engagements, that I might dedicate it to this ſort of reading. Little did I imagine, at firſt, that ſuch employment would be the ſubject of publication; and as little did I conceive, that I ſhould trace out facts, ſuppoſed to be but very recently diſcovered. I could not be ſurpriſed, too, when I obſerved theories, founded upon theſe facts, very ſimilar to thoſe we now entertain. I mentioned them, during converſation, in the philoſophical circles which I had the pleaſure to attend, during my reſidence in London. Some believed, more diſbelieved, and many ſuppoſed, that from the glow of converſation, exaggeration might creep in. Having collected a ſufficient number of facts, to juſtify my obſervations, I imagined they would afford an hour of entertainment to the curious reader.

It is a ſource, too, of rational and agreeable amuſement to the cultivated mind, to examine the

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ſimilarity of opinions, in diſtant periods, and to trace the progreſs of ſcience. It is curious to obſerve the co-incidence, that ſometimes take place, between the philoſophical ſyſtems of great men. As reſearches into nature ever afford a pleaſing employment to rational minds, and as her operations are all directed by fixed and immutable laws, it is not inconſiſtent to expect, that even in periods the moſt diſtant from each other, the philoſophical purſuits of the literary will converge to the ſame point. The ſame obſervations, with reſpect to facts, have, no doubt, repeatedly occurred to every enquirer; and reſults, which ariſe from the conſideration of general principles, have preſented themſelves, in a ſimilar point of view, to the examination of philoſophers. But it is not a looſe hint thrown out, nor a ſyſtem founded upon conjectural philoſophy, that entitles a man to the merit of a diſcoverer. An arrangement of facts, the reſult of experimental enquiry and cloſe reaſoning, alone, claims that honourable appellation. Laborious, indeed, is the inveſtigation, which requires it, and tedious the courſe. How carefully, therefore, ought we to avoid wreſting from the brow of a discoverer, the dear-bought crown of literary fame. It affords a grateful pleasure to a feeling mind, to observe each man, possessing the rewards due to his labours, and however difficult it is, perhaps impossible, to award the *suum cuique*; yet investigation, under almost every circumstance, contributes to that end, and promotes the dispensation of distributive justice.

OBSERVATIONS, &c.

PART THE FIRST.

SECT. I.

Of Fire-air Particles.

I shall now enter into the principal object of this Publication, which is to ſhew, that what many of the moderns have given out to the world as their own, may be clearly traced in earlier authors. I ſhall begin with Mayow, the great chemical luminary of the laſt century, whoſe works every where abound with the moſt obvious traces of genius and ability. His publication commences with the hiſtory of nitre. He was aware, he ſays, that many authors had written upon this ſubject, as if it had been ordained, that nitre ſhould make as much noiſe in philoſophy as in war; but the truth, obſerves Mayow, ſeems ſtill obſcured, notwithſtanding the multitude of writers; and the properties of nitre are ſtill concealed from our knowledge.* When the wonderful phenomena produced by the combuſtion of gun-powder became more generally known, they would certainly arreſt the attention of the philoſopher; and it is probable, the chemiſt would more particularly direct his enquiry to the properties of nitre; but nothing ſatisfactory could be known on this ſubject till its elementary compoſition was detected.† On this ſubject, therefore, did

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* In the year 1670, four years before Mayow publiſhed the Collection of his Tracts, a Sir William Clarke, Phyſician to the King, publiſhed a book on Nitre, entitled, A Natural Hiſtory of Nitre, or a Philoſophical Diſcourſe on the Nature, Production, Situation, Artificial Extraction, Virtues, and Uſe of Nitre. Morhof ſays, it is a trifling work, *verum parum præſtitit pro dignitate hujus ſubjecti; nam in naturâ ejus indagandâ* exilis *admodum ipſius opera eſt.* The great Lord Verulam had directed the attention of the phyſician to nitre, by maintaining, in his Hiſtoria Vitæ et Mortis, that its daily uſe would prolong life.

† The diſcovery of gunpowder is commonly attributed to Barth. Schwartz, a German Jeſuit; but it is clear, that Roger Bacon, who lived ſome ages before, was the original diſcoverer.

did our ingenious author employ his time; and upon the refult of his beautiful experiments with nitre, were all his difcoveries founded. The happy manner in which he conceived all his experiments, and the neatnefs and accuracy with which he feems to have executed them, could not fail of producing the moft unequivocal refults. It is evident, he fays, from analyfis and fynthefis, that nitre is compofed of an alkaline falt and an acid, the former derived from the earth, the latter, in part, from the atmofphere: for if we diftil nitre, the acid fpirit will come over into the receiver, the alkali remaining in the retort; and by adding nitrous acid to an alkali, nitre will be formed. Mayow did not reft fatisfied with this rough mode of explanation; he was determined to difcover the component parts of each, and examine what gave activity to the nitrous acid. This quality, he proves, is derived from the atmofphere, and is the common principle

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difcoverer. It is a curious circumftance, that both Morhof and Boerhaave give to Bacon the honour of the difcovery, whereas Cambden attributes it to the Jefuit. Vide Boerhaave's Chemiftry, Shaw's Edit. p. 130.—Morhof's Polyhift. p. 422.

of acidity, and procured from the ſame ſource. In order that his ideas on this ſubject may be more clearly underſtood, he explains his theory of combuſtion, in which the reader will find a beautiful doctrine, and perfectly analogous to that of the modern diſcoverers. (A) In the firſt place, I think it will be granted, that an aerial ſomething, whatever it may be, is abſolutely neceſſary to the production of flame. The experiments of Boyle place this beyond a doubt;* for it appears from theſe, that a lighted candle is much ſooner extinguiſhed *in vacuo*, than when confined under a glaſs filled with air: an evident proof that the flame is not choaked, as ſome imagine, by the ſmoke, but goes out for the want of an aerial ſupply. He adds, (B) Beſides, any combuſtible body placed *in vacuo*, cannot be inflamed by burning coals, ignited iron, nor by the ſun's rays collected in the focus of a lens; ſo that we muſt allow, that certain aerial particles are neceſſary to the production of fire. Having premiſed this, he

* Theſe experiments are related by Boyle, in his Nova Experimenta Phyſico Mechanica, de vi aeris elaſtica, et ejuſdem effectibus.

he goes on to obſerve, (C) But we are by no means to imagine, that the igneo-aerial ſupply is the whole air employed, but its more active and ſubtile part; for a candle under a glaſs will ceaſe to burn, although a conſiderable quantity of air remain. Having made theſe obſervations, which not only the experiments of Boyle, but alſo more recent inveſtigations have ſhewn to be perfectly agreeable to truth, he proves, by an experiment the moſt deciſive and convincing, that the active and igneous part of the air exiſts in nitre; (D) For we muſt obſerve, that nitre mixed up with ſulphur, will burn perfectly well, either *in vacuo* or under water, as is evident from the following experiment.

Let well levigated gun-powder be reduced into a pretty firm maſs, with a little water; with this fill a tube, the one end of which muſt be cloſed, ramming it tight with a piſton; then ſet the powder on fire at the open extremity of the tube; then invert it, and plunge it into water; in this ſituation the powder will completely burn away. The ſame powder, thus managed, burns equally well *in vacuo*, when all other burning bodies are extinguiſhed, for the want of the aerial ſupply; a clear proof that nitre contains the igneo-aerial particles, ſo neceſſary to combuſtion; hence there was no occaſion to ſupply them from the atmoſphere.*

That the phenomena of combuſtion are produced by the fire-air particles preſent in nitre, and not from any ſulphureous (phlogiſtic) matter, is proved by a variety of means. It is evident, that fire-air particles exiſt in nitre from its deflagration, when theſe particles are rapidly thrown off; for I cannot agree with Willis, that any ſulphureous matter exiſts in nitre.† For although the fire-

* Vide Mem. de Mathematique, &c. XVI. 686—1786. 4to. for an experiment of Lavoiſier's, very ſimilar to this. Bernoulli, in the laſt century, aſcertained the development of air by the deflagration of gun-powder. He placed *four* grains of powder in a recurved tube of glaſs, plunged the tube in water, and ſet fire to the powder by means of a burning glaſs; after the combuſtion, the interior air occupied a larger ſpace, ſo that the ſpace abandoned by the water, was ſuch as would have contained *two hundred grain* of gun-powder.—Hiſt. de l'Academie des Sciences de Paris, 1696, tome ii.—Memoire de M. Varignon ſur le Feu & la Flamme.

† Quod ſulphur copioſè inſit nitro, præter deflagrationem ejus, ſatis teſtatur ipſius geneſis; naſcitur enim præcipue

fire-air particles are abſolutely neceſſary to combuſtion, yet of themſelves they are not capable of inflammation, unleſs ſome combuſtible ſubſtance be added. (E) To the inflammation of any combuſtible matter, it is neceſſary that fire-air particles ſhould be ſupplied from the atmoſphere, or from nitre mixed with it; hence the reaſon why ſulphur will not burn *in vacuo* without nitre. On the contrary, there is no neceſſity that fire-air ſhould be ſupplied from the air for the combuſtion of nitre, as it will deflagrate ſufficiently well *in vacuo;* but for its inflammation, it is abſolutely neceſſary that ſome combuſtible body be mixed with it. If nitre too be thrown into an ignited crucible, it will not inflame; nor will this effect be produced by the flame of a candle, or rays of the ſun, except ſome combuſtible body be mixed with it, a clear proof that nitre contains no ſulphureous matter whatever: and hence it follows, that flame produced by combuſtion with nitre, ariſes from its fire-

cipue in locis ubi ſcatent animalium excrementa ſulphurea.— De Ferment. p. 77.

air particles. (F) This is the reaſon why the flame of nitre differs ſo greatly from that which is excited in any burning body; for the combuſtible body burns in conſequence of the fire-air particles derived from the atmoſphere, but nitre inflames on account of the fire-air particles thickly condenſed in itſelf: hence it happens, that the flame of nitre is ſo very violent.

Having thus taken notice that combuſtible matter will not burn *in vacuo*, except nitre be preſent, and that flame is extinguiſhed under a glaſs, although a conſiderable quantity of air remain, he denominates that part of the atmoſphere which ſupports combuſtion, Fire-air Particles*. Since alſo theſe

* The coincidence between this term of Mayow, and that of the Swediſh Chymiſt, is very remarkable. Mr. Scheele having collected ſome air from nitre, mixed it with three parts of air, in which fire would not burn, and it afforded an air ſimilar to the common ſort. He obſerves, ' Since this ' air is abſolutely neceſſary for the generation of fire, and ' makes one-third of our common air, I ſhall henceforth ' call it, for ſhortneſs ſake, Fire-air.'—Experiments on Air and Fire, p. 35. There is no foundation, ſays Dr. Beddoes, for the vague poetical term *empyreal*, the compound word *feuer-*

fire-air particles exiſt in nitre, in common with the atmoſphere, he calls them alſo by the compound appellation of Nitro-aerial Particles, and Nitro-aerial Spirit.* He maintains, that the nitrous acid is a compound containing fire-air particles; but notwithſtanding this, it will not produce inflammation, becauſe theſe particles are ſurrounded with moiſture. Had he not been hurried on by an ardour of mind, conſcious of the truth of his doctrines, he might have been led to the ſtriking experiment of mixing nitrous acid with oil; for he himſelf afterwards ſhews, that oils, fat, &c. are principally compoſed of combuſtible matter. Mayow attributes heat too to the motion of fire-air particles. The doctrine that motion is the cauſe of heat muſt have been the firſt that preſented itſelf to thoſe who conſidered the ſubject. The common experiment of friction, the motion

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feuer-luft ſignifying preciſely fire-air.—Letter to Dr. Goodwin, p. 28.

* We ſee here Mayow forming a nomenclature for himſelf, the juſt reſult of his experiments, and which preſented itſelf to him from the new facts which occurred in the proſecution of his ſubject.

tion and extrication of heat, which took place in the union of different falts, &c. naturally produced this idea in the mind. The genius of Black had not yet explained the doctrine of latent heat, and Mayow was not aware of *that* which caufed the folid and aeriform ftate of bodies. This was, therefore, the moft obvious theory that could occur to him, when he obferved motion in all his experiments in which caloric was difengaged: hence he adds, the reafon why combuftible bodies are as neceffary to produce the phænomena of combuftion is, becaufe they are fitted to ftir up a very rapid and igneous motion with the nitro-aerial particles. We fhould fay, in modern times, that the oxygen, under a certain temperature, acquires a greater affinity for the elementary principles of the combuftible body; hence it gives out heat and light. Mayow therefore attributed the phænomena of combuftion to the motion, the moderns to the condenfation of fire-air. He explains himfelf more clearly afterwards. Having mentioned thefe properties of his nitro-aerial particles, he further examines into combuftion and heat, as arifing from them. He obferves, (G) If we ferioufly attend

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to the nature of flame, and carefully examine what change the igneous particles are undergoing, while they are in a ſtate of inflammation, we ſhall conclude, that the inflammation of the igneous particles is produced in no other way, than by their very rapid motion. To prove that the combuſtible, and not the nitro-aerial particles are ignited, and alſo that both are neceſſary to the production of flame, he adduces another experiment. (H) If nitre be thrown into an ignited crucible, it will preſently be melted, but will not take fire; but if oil be injected into this crucible it will immediately inflame. That the igneous particles, collected in the ignited crucible, are of a nitro-ſaline nature (i. e. contain nitro-aerial particles) is evident, becauſe any combuſtible matter injected into this ignited crucible, immediately burſts into a flame; but combuſtible particles are by no means thrown into that rapid and igneous motion, except through the medium of nitro-aerial particles. Here it may be obſerved, our ingenious author makes no diſtinction between heat and fire-air. He ſaw the crucible ignited; ignition he maintained was owing to the motion of fire-air; any

combuſtible

combuſtible ſubſtance, therefore, thrown into the crucible, would inflame, in conſequence of motion being communicated to it by this air.* Beſides, he obſerves, that particles of a combuſtible nature, are too groſs to penetrate ſolid bodies, as iron, glaſs, &c. for if a poliſhed plate of metal be held for a while in the flame of a candle, it will be penetrated and heated by the igneous particles; but that theſe are fire-air, and not combuſtible particles, is evident, becauſe the latter adhere to the ſurface of the metal, in the form of ſoot, and by no means enter it. In order to form the ſubtile and penetrating nature of his fire-air particles, he mentions a very remarkable fact, and till

* Dr. Pearſon, the ingenious lecturer in chemiſtry, thinks that the blue appearance at the bottom of the flame of a candle, is owing to the combuſtion of hydrogen and carbon. Mayow thinks, that the blue flame, which is obſerved during the combuſtion of ſulphur, is owing to the leſs rapid motion of fire-air particles. In burning phoſphorus under a bell-glaſs, I have frequently obſerved, that at firſt, when the combuſtion is *very rapid*, the flame is *yellow*, but as ſoon as the inflamation begins to decreaſe in violence, the oxygen being *more ſlowly* applied, the flame becomes blue.

till of late not at all underftood. Having obferved that antimony is calcined in the focus of a burning glafs, as if nitrous acid had been poured upon it, or as if it had been expofed to the flame of nitre, for in this the fire-air particles are thickly condenfed, he emphatically adds, [1] I muft by no means omit to obferve, that antimony calcined in the focus of a lens, is not a little increafed in weight, as is known from experiment; nor can we conceive whence that increafe of weight is derived, except from the *fixation of fire-air particles*, during the calcination. In all thefe proceffes too, it acquires equally a diaphoretic quality. Here is an obfervation, that medicines act as explained by the modern phyfiologift, and we know it to be conformable to truth.

Dr. Thornton has informed me, that the breathing of oxygen, produces an increafed exhalation from the fkin; and indeed that gentleman, with a liberality which fhould every where be infeparable from the Medical Profeffion, has permitted me to be prefent when he receives his patients, and to afk queftions, with refpect to the operation of pneumatic remedies.—From every enquiry I have

made,

made, I have found, that an atmoſphere of a higher than ordinary ſtandard promotes the cuticular diſcharge. The additional weight acquired during oxydation, is, by Mayow, attributed to the right cauſe. How has this excellent obſervation been thrown away upon the Stahlians? How has not philoſophy been tortured to explain the contradiction of increaſed weight with diſengaged phlogiſton? How have they not been obliged to recur to the imaginary cauſe of repulſive power and principle of liberty, by neglecting the obvious and ſimple explanation of Mayow? They ought to have recollected, too, the rules of the great Lord Verulam, the firſt who rejected hypotheſes not founded on direct experiment, that to reaſon from occult cauſes, is an inexhauſtible ſource of error. From what has been here delivered, it is evident that Mayow met with oxygen, which he never, however, collected in a ſeparate ſtate, and from the manner of his expreſſions, we can have no doubt that he diſcovered that active principle, which he has ſo admirably employed in the explanation of his experiments. If the reader, however, ſhould be inclined to

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with-hold his aſſent, we think he will be convinced from what will be afterwards detailed; but before we proceed any further, it will be neceſſary to explain, what Mayow means by his Salino-ſulphureous particles, which are hereafter ſo often mentioned.

SECT. II.

Of Salino-sulphureous Particles.

From experiments made by different chemiſts, it appears, that oils, fats, and the food we eat, are compoſed principally of hydrogen, azot, and carbon. When hydrogen and carbon are combined together, without the intervention of caloric, to bring the hydrogen into the ſtate of gas, they form oil, which is either fixed or volatile, according to the proportions of hydrogen and carbon in its compoſition. By means of calculation applied to the products of experiments, we find, that fixed air is compoſed of twenty-one parts, by weight, of hydrogen, combined with ſeventy-nine parts of carbon. Animal ſubſtances, being compoſed of nearly the ſame elements with cruciferous plants, give almoſt the ſame products in diſtillation; with this difference, that, as they contain a greater quantity of hydrogen and azot, they produce more oil and more ammoniac*. It is from these elements of hydrogen, azot, and carbon, by different combinations, that various substances, apparently very dissimilar, are formed. It is by these that the animated creation has its life and being. From these we derive our daily nourishment, and by their union with oxygen, taken in by the lungs, animal heat is disengaged, the chief stimulus to the irritability of the animated fibre. Again it is by the union of these with oxygen, that combustion is supported. We shall see something very like this in Mayow. By the by, I must observe, that he uses sulphureous and salino-sulphureous, indiscriminately, to express the same thing. The word sulphur was one of the most vague terms made use of by the older chemists. It was applied to the inflammable principle of bodies, and all substances capable of inflammation, were said to be sulphureous; hence oils, resins, animal fat, &c. were by some called sulphurs. Sulphur, however, properly so called,

* Elements of Chemistry by Mr. Lavoisier, p. 165-7-82. 3d Edit. by Kerr.

was ſuppoſed to be a compound, conſiſting of concentrated oil of vitriol and phlogiſton*; and ſulphur, being a very inflammable ſubſtance, the terms phlogiſtic and ſulphureous became ſynonimous. Whenever, therefore, Mayow uſes the terms ſulphur, ſulphureous, and ſalino-ſulphureous particles, he means ſimply combuſtible matter, capable of producing flame, by a rapid motion with fire-air. We wiſh the reader to recollect this, whenever he meets with theſe terms. He ſays that the fat of animals is formed of theſe ſulphureous particles; we may learn, from a variety of expreſſions, that this was his opinion, ſuch as the ſulphureous particles of the blood, of which the fat is compoſed, ſulphureous particles producing fat, &c. (K) Our food, he maintains, is principally compoſed of ſuch matter; moreover our nouriſhment is replete with ſalino-ſulphureous particles: hence thoſe ſubſtances which copiouſly contain a volatile ſalt and ſulphur, are beſt adapted to

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* Vide Neumann's Chemical Works, p. 165;—and Lewis's Mat. Med. p. 559.

to recruit the ſtrength worn out by daily labour*. The following paſſage from Mr. Hewſon, ſhews how accurately Mayow had attended to the animal œconomy; ſpeaking of the abſorption of fat for nouriſhment, he adds, 'This circumſtance was clearly proved by my valuable and ingenious friend, the late Dr. Stark, who, in a courſe of curious experiments made by weighing himſelf, after living for ſome time on different ſorts of food, diſcovered that a leſs quantity of ſuet was ſufficient to make up for the waſte of his body, than of any other ſort of ordinary food; and that when compared with lean part of meat, its nutritive power was, at leaſt, as three to one'†. If any one

* We know, ſays Mr. Lavoiſier, that animal ſubſtances are compoſed of hydrogen, carbon, azot, phoſphorus, and *ſulphur*, all of which, in a ſtate of quintuple combination, are brought to a ſtate of oxyd, by a larger or ſmaller quantity of oxygen.—Elem. of Chem. p. 202.

† Experimental Enquiry into the Properties of the Blood, p. 127-8. The ingenious Dr. Stark, at an early age, fell a victim to his zeal for experimental enquiry, his death having been occaſioned by a courſe of experiments (made upon himſelf) on the effects of various kinds of food. Vide Med. Communications, Vol. I. art. 24.

ſhould ſtill doubt that theſe expreſſions, made uſe of by Mayow, were merely vague and looſe terms, thrown out without an expreſs intention, the following paſſage, in which he mentions, ſtill further, what ſubſtances contain the ſalino-ſulphureous particles, muſt convince, that he attached a preciſe idea to theſe terms; for he obſerves, that *ſpirit of wine*, and the chemical *oil* of vegetables, are fluids replete with a volatile ſulphur. It was thought neceſſary to premiſe theſe obſervations, that the reader might not imagine, the expreſſions of Mayow were tortured to a different meaning, when it is ſaid, that his ſalino-ſulphureous particles are what we now find to be the compoſition of different ſubſtances, and to which the French chemiſts have given the appellation of hydrogen, azot, and carbon. We do not mean to ſay, that Mayow *exactly* meant this; or that he maintained his ſulphureous ſubſtances were compoſed of different elementary principles; but when he mentions the ſubſtances, containing the ſalino-ſulphureous particles, the component parts of which are now known to be hydrogen, azot, and carbon, we may fairly conclude that the chemical agency

which

which Mayow afcribes to the former, is analogous to that of the latter; and that certain principles, acting in thefe fubftances, produced chemical effects, during their operation, fimilar to thofe now attributed to the elements of the more combuftible bodies. All febaceous fubftances, indeed, were, by Stahl and his followers, termed phlogiftic, as containing the principle of inflammability, which continued in ufe, till the late happy revolution in chemiftry, by detecting the ambiguity of fuch expreffions, has given to fubftances appropriate names.

SECT. III.

Of Acids.

On the conſtitution of the acids, the ingenious Mayow is no leſs curious; and we diſcover a ſtriking analogy between his facts and reaſonings, and thoſe of the modern chemiſts, on this ſubject. The reader will, probably, be ſurpriſed to find the following account of the formation of the acids, in ſo old a writer; for it is exactly ſimilar to the explanation which the new diſcoveries have given riſe to. He has already, as we have ſeen, deſcribed, that active principle in the atmoſphere, which forms a part of the nitrous acid; he now wiſhes to account for its conſtituents: but before he does this, it is neceſſary to lay down a general theory of the acids, for, as he obſerves, there is a great ſimilarity among all of them. He rejects the opinion hitherto entertained, that an acid exiſts, ready formed, in ſulphur, becauſe in its union with alkalis, no effer-

veſcence

vefcence takes place, and we cannot detect it by any chemical means. The fulphuric acid, he fays, is formed by the union of fire-air particles with the fulphur, during combuftion. He fuppofed fulphur to contain particles of an alkaline nature, and the following idea which he gives, of the manner in which the fire-air particles unite with the fulphur, in no wife effects the truth of his theory, as it is a mode of expreffion which he borrowed from the language of the times. (L) During the burning of fulphur (*i. e.* whilft the fulphureous particles and fire-air are hurried into a motion conftituting fire) the faline particles, adhering to the fulphureous, are broken down, and comminuted by the repeated ftrokes of the fire-air, fo that thefe faline particles being frequently bruifed and broken down, are fo attenuated, that inftead of being firm and folid, they become flexile and fluid. Moreover the faline particles of the fulphur, which were before of a fixed nature, after they become fharpened and reduced to fluidity, are converted into a fharp and four liquor, and form, as is very probable, the common fpirit of ſulphur*. The acids of woods are formed in the ſame way, as are alſo the acids of ſugar and honey†. His explanation of the formation of vitriol from martial pyrites (ſulphuret of iron), ſhews how juſtly he conceived this ſubject. (M) For indeed the nitro-aerial ſpirit, effervescing with the metallic ſulphur of the marchaſites, converts their more fixed part into an acid, which immediately attacks and calls forth the metallic particles of the mineral, and unites with them into a vitriol. Moreover the ruſt of iron, which poſſeſſes the nature of vitriol, appears to be produced by the nitro-

* This mechanical explanation has not loſt its effects in our days. Mr. Lavoiſier, when treating on the formation of gas, obſerves, 'It is perhaps more natural to ſuppoſe, that the particles of caloric have a ſtronger mutual attraction, than thoſe of any other ſubſtance; and that theſe latter particles are *torn aſunder*, in conſequence of this ſuperior attraction of the particles of caloric, which *forces* them between the particles of other bodies, that they may be able to reunite with each other.'—Elements of Chemiſtry, p. 72.

† The acid of wood is now termed pyro-lignous; Mr. Goſtling was the firſt who made a particular enquiry into its nature and properties.—Vide Crell's Chemical Journal for 1779.

nitro-aerial particles, attaching to the metallic ſulphur of the iron. In this way, he obſerves, ruſt, or an imperfect vitriol, is produced, in the ſame manner as if an acid had been thrown upon the iron. Has he not here anticipated the modern chemiſts, in their ideas of an oxyde? The ſame principle which conſtitutes acidity, thus unites with metals, to form ruſt; for we muſt obſerve, he adds, that not only in ſolids, but alſo in fluids, an acid ſalt, or ſharpneſs, is produced, by the action of the nitro-aerial ſpirit. Of this he gives an example in the ſouring of wines, which owe their acidity to the union of nitro-aerial particles. (N) On this account, wines or ales, expoſed to the rays of the ſun, or kept warm, in proceſs of time are converted into vinegar; for the nitro-aerial particles being communicated to thoſe liquors, either from the ſolar rays, or the heat, (he imagined heat was owing to the rapid motion of nitro-aerial particles, and that they formed a part of the ſun's rays), effervesce with their ſalino-ſulphureous particles; whence it happens, that theſe ſaline particles are acidified, by the action of the nitro-aerial particles and converted into acid ſalt*. Notwithſtanding, however, that there is a great ſimilarity between the acids, in conſequence of fire-air being the general principle of acidity in all, yet there is a difference obſervable in them. This is owing to the nature of the acidifiable baſe, and the degree of oxygenation of which it is capable†. Sugar, mucus, under which term are included the different kinds of gums and ſtarch, are vegetable oxydes, having different proportions of hydrogen and carbon, as their radicals or baſes. By the addition of

* According to the new chemical doctrine, the acetous fermentation, or oxygenation of vegetable matters, takes place, in conſequence of the increaſed temperature altering the ſtate of affinity; for the chemical attraction which exiſts between oxygen, and various ſubſtances, is varied, in proportion to the degree of temperature. Although Mayow, however, here mentions heat, as neceſſary to the acetous fermentation, he does not ſeem to be aware, that the increaſed temperature produced a change in the ſtate of chemical affinity.

† Thus the baſe may be double or triple, very rarely ſingle. The baſe may be compoſed of carbon and hydrogen, or of carbon, hydrogen, and phoſphorus, combined with ſome oxygen.

of a certain quantity of oxygen, they become acids; and according to the degrees of oxygenation, and the proportion of hydrogen and carbon in their bafes, they form the feveral kinds of vegetable acids*. (o) As to the difference of acid liquors, obferves Mayow, we muft conclude, that it is produced by the diverfity of the falts (bafe) of which they are compofed, *viz.* that the fixed falts are fometimes more, fometimes lefs broken down and acidified by the nitro-aerial fpirit. There is, neverthelefs, a great fimilarity between each of the acids; and the nitro-aerial and fire-air particles are contained in *all* of them, as in a proper fubject.

Such is the confiftent and beautiful manner, in which this great genius applied his difcovery of fire-air, to explain the formation of the acids. From his experiments and acute obfervation, he beheld the neceffity of a principle to give activity to the acids, and without which there could be no acidity. It occurred to him too, that this principle muft be united to fome bafe, before acidity could

* Lavoifier's Elements of Chemiftry, p. 172.

could be formed. Mayow never refted with a fimple fact: it was his ruling paffion, to explain and come at the elementary principles of every thing. He might have been content with faying, that nitre was formed of an acid and an alkali:—this did not fatisfy him, he muft give the conftituent principles of nitre. We know that nitre is formed of three fubftances, *viz.* fixed alkali*, azot, and oxygen. Mayow faw from his experiments, that there muft be a triple combination in the formation of nitre. This falt, he fays, is compofed of three principles, *viz.* 1*ft*, of an active part, derived from the

* The fixed alkalis have never yet been decompofed; and we are ftill ignorant, whether they be fimple or compound bodies. The conftituent parts, however, of the volatile alkali, or ammoniac, have been difcovered by Mr. Berthollet, to be azot and hydrogen.—Vide Memoirs of the Academy, for 1784, p. 316. Analogy has led Mr. Lavoifier to imagine, that all the alkalis are compound bodies, and that azot is one of their elementary principles; but from fome experiments related in the Tranfactions of the Turin Academy, there is reafon for fuppofing, that foda is a modification of magnefia, and this latter fubftance, according to the experiments of Baron Born, appears to be a metallic oxyd.

the atmoſphere ; 2*d*, this uniting to the ſaline vehicle, conſtitutes the nitrous acid ; which, 3*d*, attaching the fixed ſalts of the earth, forms nitre.

The principle, which thus conſtitutes the moſt active part of the atmoſphere, gives acidity to ſubſtances, and is the ſupport of life and flame, is alſo the chief agent in fermentation. (P) The expreſſed juice of vegetables, as the muſt of wine and cyder, &c. effervefce, by the nitro-aerial and ſalino-ſulphureous particles inherent in them ; or the nitro-aerial particles may be ſupplied, *ab extra.* He thinks it very probable, that the putrefaction, as well as the acetous fermentation, is produced by the nitro-aerial particles: (Q) Hence thoſe things which exclude the nitro-aerial particles, preſerve bodies from corruption ; and this is the reaſon, why the fruits of vegetables, as alſo fleſh covered with butter, are long kept from putrefaction, and alſo iron, beſmeared with oil, is not corroded by ruſt.

We know, from the experiments of Pringle and Macbride, that carbonic acid is highly antiſeptic, and that various other ſubſtances poſſeſs this quality ; and ſome curious experiments, made by

Dr.

Dr. Alexander, prove, that meat, expoſed to the exhalations of a common privy, is prevented from undergoing the putrefactive proceſs for a long time, which, as he maintains, was certainly owing to the diſengaged ammoniac. How far theſe ſubſtances act by excluding the fire-air particles, or oxygen, remains for future inveſtigation. One experiment, however, which we made, proves, that if oxygen does not accelerate, at leaſt it does not retard putrefaction. I took three pieces of raw beef, of nearly the ſame ſize. One was confined with oxygen, the ſecond with hydrogen, and the third remained expoſed to the atmoſpheric air, for a ſtandard. After remaining for five days, the pieces of meat were taken out, extremely fœtid. The one in oxygen felt firm, and was of a bright red colour throughout; the other was very lax and dark-coloured, particularly at its upper ſurface, where it had been in contact with the hydrogen, the under part being ſomewhat redder. The ſtandard piece was not putrid; it had become perfectly dry: whereas thoſe in the glaſs were kept moiſt, by the confined evaporation from the water; this no doubt produced its effect. The

 experiment

experiment was performed in the month of April, 1796; the thermometer in the room varied from 40 to 48. We have not had ſufficient time to continue experiments of this kind; but we are juſt about commencing experiments, to diſcover, how far the different gaſes accelerate or retard the vinous, acetous, and putrefactive fermentations.

(A) Primo, concedendum eſſe arbitror, nonnihil, quicquid ſit, aereum, ad flammam quamcunque conflandam neceſſarium eſſe; id quod experimenta Boyliana, extra dubium, poſuerunt; utpote, ex quibus conſtat, lucernam accenſam multo citius in vitro aere vacuo, quam in eodem aere repleto expirare; indicio manifeſto, flammam vitro incluſam, non tam a proprio fuligine, uti nonnullis viſum eſt, ſuffocatam, quam pabulo aereo deſtitutam interire*. P. 11.

(B) Præterea, materia quævis ſulphurea in vitro, ex quo aer exhauſtus eſt, collocata neque carbone,

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* Tractatus Quinque Medico-Phyſici, ſtudio Joh: Mayow, LL. D. et Medici, necnon, Coll. Omn. Anim. in Univ. Oxon. Socii.—An. Dom. 1674.

carbone, aut ferro ignitis, neque radiis solaribus, speculi ustorii ope collectis, accendi potest; ut minime jam dubitandum sit, quin particulæ quædam aereæ, ad ignem excitandum omnino requiruntur. P. 12.

(C) At non est existimandum, pabulum igneo-aereum ipsum aerem esse, sed tantum partem ejus magis activam subtilemque; quippe lucerna, vitro inclusa, expirat, cum tamen copia aeris, satis ampla, in eodem continetur. P. 12.

(D) Quippe annotare est, nitrum sulphuri admixtum, in vitro aere vacuo, item subter aquas, satis prompte deflagrare; uti sequenti experimento constabit.—Nempe pulvis pyrius, minutissime tritus cum aquæ tantillo, in massam duriusculam redigatur, quâ tubulus quivis, cujus altera extremitas obturata est, dense impleatur, materiam subinde cum bacillo fortiter impellendo; dein pulvis iste pyrius in extremitate tubuli apertâ accendatur, tubulusque inversus aquæ immergatur, ibidemque detineatur; ita pulvis pyrius, ad totalem sui absumptionem, subter aquas deflagrabit. Quinetiam pulvis iste, modo prædicto dispositus, in vitro aere vacuo, ardebit, cum tamen ignes alii

ob

ob pabulum aeris ſubductum mox extinguuntur: indicio ſatis manifeſto, ſal nitrum particulas igneo-aereas, ad flammam conflandam requiſitas, in ſe continere; ita ut ad ejus deflagrationem, particulas igneas ab aere ſuppeditari, minime opus ſit. P. 13.

(E) Ad materiæ cujuſque ſulphureæ accenſionem, requiritur, ut particulæ igneo aereæ ab aere, aut a nitro, ei prius admixto, ſuppeditentur: quæ cauſa eſt, quod ſulphur, in loco aere vacuo, flammam non concipiet, niſi nitrum ei admixtum fuerit. E contra, vero, ad nitri accenſionem non eſt opus, ut particulæ igneo-aereæ ab extra ſuggerantur, utpote quod in locis, a quibus aer præcluditur, ſatis prompte deflagrabit; verum, ad ejus accenſionem, omnino requiritur, ut materia aliqua ſulphurea ei admiſceatur. P. 15.

(F) Et hinc eſt, quod flamma nitri valde diverſa ſit ab ea, que a materia quavis ſulphurea deflagrante, excitatur; quippe materia ſulphurea particulis igneo-aereis ab aere ſuggeſtis, nitrum autem particulis igneo-aereis, in ipſo confertim agglomeratis, agmineque denſiſſimo erumpentibus deflagrat; unde fit, quod flamma nitri maxime impetuoſa ſit. P. 16.

(G) Si ad flammæ naturam ferio attendamus, et nobifcum cogitemus, qualem demum mutationem particulæ igneæ fubeunt, dum eœdem accenduntur, nihil aliud certe concipere poffumus, quam particularum ignearum accenfionem, in motu earum perniciffimo, confiftere. P. 25.

(H) Si nitrum in crucibulum ignitum immittatur, idem mox liquabitur, non vero flammam concipiet; cum tamen oleum crucibulo dicto injectum, ftatim accendetur. Quod vero particulæ igneæ, in crucibulo ignito agglomeratæ, indolis nitro-falinæ fint, indicio eft, quod materia quævis fulphurea, crucibulo dicto injecta, a particulis iifdem accenditur; particulæ autem fulphureæ non, nifi particularum nitro-aerearum ope, in motum velociffimum igneumque, fufcitantur. P. 26.

(I) Neque illud prætereundum eft, quod antimonium, radiis folaribus calcinatum, haud parum in pondere augetur; uti experientiâ compertum eft; quippe vix concipi poteft unde augmentum illud antimonii, nifi a particulis nitro-aereis igneifque ei, inter calcinandum infixis, procedat. P. 28.—9.

(K) Sed infuper alimentum particulis falino-ful-

7 phureis

phureis refertum; hinc ea, quæ ſale volatili ſulphure que copioſo conſtant, vires laboribus diuturnis attritas reficiendo, præcipue idonea ſunt. P. 30.

(L) In ſulphuris deflagratione (dum, viz. particulæ ejus ſulphureæ et nitro-aereæ mutuo ſe motu igneo exagitant) particulæ ſulphuris ſalinæ, particulis ejus ſulphureis adhærentes, crebris particularum nitro-aerearum ictibus, verberantur, atteruntur, comminuuntur que; ita ut particulæ eæ ſalinæ, ſæpius attritæ et contuſæ, tandem inſtar gladiolorum exacuantur, et inſuper adeo attenuentur, ut eædem a rigidis ſolidiſque, in flexiles fluidaſque convertantur. Particulæ, vero, ſulphuris ſalinæ, quæ antea indolis fixæ fuerant, poſt quam ita exacuuntur, et ad fluorem perducuntur, in liquorem aereum acidumque convertuntur, ſpiritumque ſulphuris vulgarem, uti veriſimile eſt, conſtituunt. P. 34.

(M) Nimirum ſpiritus nitro-aereus, cum ſulphure metallico marchaſitarum iſtarum efferveſcens, partem earum fixiorem in liquorem acidum convertit, qui mox, ab ortu ſuo, particulas metallicas lapidis dicti adoritur evocatque, tandem-que cum iiſdem in vitriolum coaleſcit. P. 39—40.

(N) Huc etiam ſpectat, quod vina aut cereviſia generoſior, radiis ſolaribus diù expoſita, aut in loco calido detenta, proceſſu temporis in acetum commigrant: nempe particulæ nitro-aereæ, a radiis ſolaribus, aut ab igne, liquoribus iſtis communicatæ, (etenim alibi oſtenſurus ſum, calorem quemcunque a particulis nitro-aereis, in motu poſitis, provenire) cum particulis liquorum eorum ſalino-ſulphureis efferveſcunt; unde fit, quod particulæ ſalinæ, particularum nitro-aerearum actione exacuantur, inque ſalia acida convertantur. P. 41.

(O) Quoad differentiam liquorum acidorum, eam a diverſitate ſalium, e quibus iidem conſtituuntur, procedere, putandum eſt, uti etiam ex eo, quod ſalia fixa nunc magis, nunc vero minus, a ſpiritu nitro-aereo, atterantur exacuanturque: et tamen inter ſalia acida quæcunque, affinitas magna eſt et ſimilitudo; inque iis omnibus particulæ nitro-aereæ igneæque, veluti in ſubjecto idoneo, hoſpitantur. P. 44.

(P) Succus ex vegetabilibus expreſſus, veluti

muſtum

muſtum vini, aut pomacei, idque genus alia, ope particularum nitro-aerearum ſulphurearumque ſibi innatarum, efferveſcunt. P. 60.

(Q) Hinc ea, quæ ſpiritum nitro-aereum excludunt, res a corruptione vindicant; uti etiam carnes butyro coopertæ, a putredine diu præſerventur; item ferrum, oleo illitum, rubigine non corrodatur. P. 62.

SECT. IV.

Of the ANALOGY *between* RESPIRATION *and* COMBUSTION.

From what we have already extracted from the works of our ingenious Author, the reader will clearly perceive, that he was acquainted with an active principle, conſtantly preſent in the atmoſphere, and which was neceſſary to the maintenance of flame. Here he enters more deeply into the ſubject; and by a ſet of beautiful experiments, not only proves it beyond a doubt, but alſo unequivocally ſhews, that the ſame aerial matter is conſumed, both by reſpiration and combuſtion. Theſe experiments are inſtituted, chiefly, with a view of proving, that the elaſticity of atmoſpheric air, in part, depends upon the nitro-aerial particles.

(A) If a cupping glaſs be applied to the ſkin, a partial vacuum will be formed, as ſoon as the flame

flame is extinguiſhed; hence the ſkin will riſe into the glaſs, on account of the preſſure of the ſurrounding air. What is the cauſe of this, ſays Mayow, or whence does it happen, that theſe circumſtances are produced? (B) It is very probable, that the air is cloſely united to the flame, being taken up by it, as a pabulum; ſo that there is not the ſmalleſt portion of flame, in which there does not exiſt ſome portion of air. Hence we may conclude, that the vacuum is formed, becauſe the air is deprived of its fire-air particles, by the combuſtion; as is more clearly proved by the following experiments.

(C) Let a lighted candle be ſo placed, in water, that the wick ſhall riſe about ſix fingers breadth above the ſurface; a glaſs veſſel is then to be inverted over it, as is ſhewn in *Fig.* 1. Care muſt be taken, at the ſame time, that the water be of the ſame level, within and without, by means of a ſyphon, the one leg of which is previouſly placed within the inverted glaſs: this will let out the air, as it is compreſſed by the immerſion of the veſſel. The ſyphon is then to be withdrawn, leſt afterwards the external air ſhould ruſh in. In a ſhort time you will perceive, while the candle is yet burning, the water rifing into the inverted veffel. He will not deny, but that the afcent of the water may be, in part, owing to the included air being lefs rarified and agitated, at the moment the candle is going out. It is by no means owing to this caufe alone; but it is produced by the combuftion of the candle deftroying the nitro-aerial and elaftic particles of the air, fo that it is no longer able to refift the preffure of the atmofphere. Left any one fhould with-hold his affent to the inference, drawn from the above experiment, he inftitutes another, that no doubt may remain, as to the fact he wifhes to eftablifh.

(D) Let a piece of camphor, with fome tinder dipt in melted fulphur, be fufpended in a large glafs veffel, inverted, as is feen in *Fig.* 1. The veffel is to be immerfed, ten fingers breadth, in the water, which is to be brought to the fame level, both within and without, by the means already defcribed. That the view, however, may be more diftinct, fome water may be taken out of the external veffel. Let the height of the water, in the inverted glafs, be marked, by pafting bits of paper

againft

againſt its ſide. After theſe preparations, let the combuſtible matter be fired, by a burning lens. The water, at firſt, will be depreſſed by the rarefaction of the air. When the combuſtion, however, has ceaſed, and the apparatus cooled, you will find the water to have riſen above its priſtine height. After the ſmoke was diſſipated, he attempted to fire, by the lens, a portion of the camphor, which had not been deſtroyed by the combuſtion; but the experiment did not ſucceed: a ſufficient proof, that the air was deprived of its fire-air particles, by the combuſtion; ſo that it was no longer able to ſupport that proceſs. This was ſurely a rigorous experiment, and proved inconteſtibly, that the atmoſphere was compoſed of two kinds of air, the one fit to ſupport flame, the other intirely deſtructive of it. The diminution that took place, in conſequence of the abſorption and condenſation of the fire-air, made Mayow imagine, that the elaſticity of the air depended upon the preſence of that gas. He was unwilling, however, to yield implicit aſſent to this opinion, apparently founded upon fact, as we ſhall ſee in the ſequel. It was his opinion, as has been already ſtated, that the ſame elaſtic, and fire-air particles, were ſubſervient to the purpoſes of reſpiration and combuſtion. To determine this point, the following experiments were made.

(E) Let a moiſtened bladder be tied over the mouth of a circular glaſs veſſel; then let another, containing a mouſe, be applied tight to the bladder, as is ſeen in *Fig.* 2. The edge of the veſſel will ſhortly be ſeen to adhere firmly to the bladder, and the bladder itſelf will riſe into the cavity of the glaſs, in the ſame manner as if a cupping glaſs had been uſed, with a burning body in it; and this will take place while the animal is breathing: hence an animal may be uſed, inſtead of flame, in the operation of cupping. From this it appears, that the air is deprived of its elaſticity by the reſpiration of animals. Not content, however, with merely ſhewing, that one part of the air is conſumed by reſpiration, he inſtitutes another experiment, to determine how much the air is diminiſhed.

(F) Let an animal, confined in a cage, be ſuſpended in a glaſs cucurbit, in a ſimilar manner as the ſmall jar is in *Fig.* 4. The cucurbit is then

then to be inverted over water, which is to be made to afcend into the glafs, by means of the fyphon. Some of the external water is then to be taken out, that we may have a more diftinct view; and the height of the water, within the glafs, is then to be marked, by flips of paper affixed to its fides. In a fhort time you will fee the water gradually rifing, within the glafs, although it fhould feem, that a contrary effect would be produced by the heat and exhalations from the animal: and from experiments made with various animals, I have difcovered, that the air is diminifhed one-fourteenth by refpiration.

(G) From what has been fhewn, it evidently appears, that the refpiration of animals exhaufts the air of certain vital and elaftic particles; fo that we can no longer doubt, that fomething from the air, abfolutely neceffary to life, is taken into the blood by refpiration. It is manifeft too, that the air is deprived of its elaftic power, by the refpiration of animals, in a fimilar manner as by combuftion; and indeed it is rational to believe, that thefe two proceffes deprive the air of the fame particles, as the following experiment decidedly proves.

(H) Let an animal be enclofed, with a burning candle, in a glafs, inverted over water. In a fhort time, the candle will be extinguifhed, nor will the animal long furvive; for I have found, that the animal will not live much above half the time that it would have done, had it been confined without the candle. Left any fufpicion fhould be entertained, that the animal was fuffocated by the fumes of the candle, he made the experiment with fpirit of wine; the animal therefore could not have died from that caufe. Since then the air included in the glafs, is deprived, in part, of the nitro-aerial particles, by the burning of the candle, it could not be long of ufe to the refpiration of the animal; hence not only the candle, but alfo the life of the animal, will be fhortly extinguifhed, from the defect of nitro-aerial particles. He fays, the animal furvives the flame, becaufe the candle requires a continual and rapid fupply of nitro-aerial particles, to fupport combuftion.

SECT. V.

Of the different Gafes MAYOW *met with in his* EXPERIMENTS.

Before we proceed to lay before the reader, Mayow's application of the fire-air particles to the purpofes of the animal œconomy, it may not be improper to enquire, how far he was acquainted with the other kinds of air. The reader muft not expect to find accuracy of diftinction between the airs, by Mayow ; but he will perceive the fame acutenefs of obfervation, and quicknefs of thought. His mind being wholly engaged with the difcovery of fire-air, and his thoughts entirely occupied with the wide extent of its influence, he did not pay much attention to the other gafes which efcaped, during his chemical operations. He, however, collected different kinds of unrefpirable airs, and proved by experiment, that they were unfit to fupport life, and that the fire-air particles were alone adapted to that purpofe. He had

had to invent, too, a method for transferring air, from one veſſel to another, which, like the egg of Columbus, appears ſimple and obvious, after it is explained; but before explanation, required ingenuity, thought, and reflection. To be ſenſible of the merit of the following contrivances, ſays Dr. Beddoes, we have only to recollect, how difficult it muſt have appeared, before a living philoſopher, of whom this country has juſt reaſon to be proud, a ſecond time taught us the art, to confine, divide, remove from veſſel to veſſel, examine, and manage at pleaſure, fugitive, incoercible, and impalpable fluids, like that which we breathe*. Mayow fully exhibits this ingenious invention, in his enquiry, and diſplays a mind, ardent, ſagacious, and obſervant.

(1) I attempted to fire, ſays Mayow, combuſtible matter, under a glaſs, in which an animal had died; but the experiment failed: theretore it is probable, that the ſame air which is unfit for reſpiration, will not ſupport combuſtion. Mayow learnt, from his experiments, that the air, deprived of

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* Honorable H. Cavendiſh's Exp. on Factitious Air. Ph. Tranſ. lvi. p. 142 and the plate.

of fire-air particles, become lighter*; (K) For, he obſerves, if a mouſe, or a bird, be placed in the ſuperior part of the glaſs, it will die ſooner, and the water will not riſe ſo high, as if the animal had been ſituated lower. This is more manifeſt, if two animals be uſed; for the one at the bottom of the glaſs will ſurvive the one at the ſuperior part. (L) From theſe facts we may be allowed to imagine, that the air thrown out from the lungs becomes lighter, becauſe it is deprived of nitro-aerial particles, and therefore aſcends to the top of the glaſs. The ſame exactly happens with reſpect to a burning body; (M) For if a glaſs cucurbit be inverted, and ſuſpended in the air, and a candle be held up under it, it will, in a ſhort time, be extinguiſhed; for the air contained in the glaſs, being made unfit to ſupport flame, by the combuſtion of the candle, and being, by the ſame means, rendered

* Oxygen is more ponderous than the air of the atmoſphere; the cubic foot of atmoſpherical air wighing 720 grains, while the cubic foot of purer air weighs 765—Chaptal's Elem. of Chem. Vol. p. 120. According to Mr. Kirwan, the weight of oxygen, to that of common air, is as 1103 to 1000 nearly.—Eſſay on Phlogiſton, p. 13.

rendered lighter than common air, the furrounding atmofphere preffes upon it on all fides, and prevents it from defcending in the glafs.

Mayow, then, here met with what we now term azot, and diftinguifhed it by the only two properties by which we now diftiftinguifh it, *viz.* its being unfit for refpiration and combuftion. He was not aware, that in refpiration and combuftion, the carbonic acid gas is given out. The chemical reader well knows, that this gas is heavier than common air*; but that a confiderable quantity of caloric being difengaged, both by the animals and burning bodies, the carbonic acid is rendered lighter, and confequently afcends. Mayow did a great deal, in thus decompofing atmofpheric air, and diftinguifhing between the fire-air particles and azot; it was enough for him to have fhewn, that only one part of the air fupported life and combuftion. He admires the wifdom of Providence, in thus carrying

* The weight of fixed air, procured from calcareous fpar, by muriatic acid, is to that of common air, as 1500 to 1000; and that of azot, is as 985 to 1000.—Kirwan, p. 15—17.

ing off from the earth, air unfit for the purpofes of life. How much more would he have praifed the benevolence of the Deity, had he known, that carbonic acid is of greater fpecific gravity than atmofpheric air; but to remedy the evil that would arife to animal life, from its accumulation, it is made the food of plants, and is readily abforbed by water, to which, no doubt, the agitation of tempeftuous weather greatly contributes. Mayow maintained, that the elafticity of the air depended, in a great meafure, on the fire-air particles. He entertained, however, much doubt upon this fubject; and did not deny, that air, deprived of fire-air particles, was alfo elaftic. There is fomething elfe then neceffary to produce this effect. It is, therefore, very probable, he fays, that a fubtile active matter, being interfperfed between the particles of the air, and conftantly keeping them in motion, not a little conduces to the expanfive power of air; and by this means, it would appear, that the elaftic force of the air is caufed from the application of heat. The following experiment was made to determine, how this elafticity was diminifhed.

(N) Let a ſtick, equal in length to the wideſt part of a glaſs cucurbit, be placed within it, ſo that its extremities may reſt upon the ſides, as is ſeen in *Fig.* 4. From this tranſverſe ſtick, ſuſpend a glazed earthen-ware veſſel, capable of containing about four ounces, half full of nitrous acid. Throw acroſs the ſtick, directly over the veſſel containing the acid, pieces of iron, tied to a cord, the other end of which muſt be ſufficiently long to come out from under the cucurbit. The orifice of the inverted cucurbit is to be immerſed about five fingers breadth below the water. After the water is raiſed in the cucurbit, by the ſyphon, ſome is to be taken from the outer veſſel, until that within ſtand about three fingers breadth higher. Let every thing remain thus, till the included air, heated by the hands, return to its former ſtate; the height is then to be marked with ſtrips of paper. The pieces of iron are then let down into the acid, by means of the cord. A violent ebullition with heat will immediately take place, and the water will be depreſſed by the diſengaged gas. In about twenty minutes, or rather when the water has been depreſſed about three fingers

fingers breadth by the gas, the pieces of iron are to be raifed from the acid: you will then foon fee the water rifing gradually in the glafs; and in an hour or fo, it will be elevated far above the height marked by the ftrips of paper, fo that one-fourth of the fpace, hitherto occupied by the air, will now be filled by the water; and indeed, in no long time, the water thus raifed, will defcend to its former level ' I fufpect fome miftake here, fays Dr. Beddoes; the author fays nothing more of this ftrange new depreffion of the water; how can any air be generated in thefe circumftances? Did one of the pieces of iron, at any time come off the bunch, and remain behind in the acid? I wifh the paffage were out of the book; or at leaft, that any one would explain it to me.' It does not appear difficult of explanation; it is well known, that oxygen unites with azot in various proportions, forming the nitrous oxyds. During the folution of metals in the nitrous acid, as the oxygen is robbed from the acid, a gas is generated, which is fometimes azot or nitrous air, with different proportions of oxygen; for it is not the fame quantity of oxygen which unites with azot to form nitrous air; and in proportion to the quantity of oxygen, ſo is its abſorption by the water more or leſs rapid. In this way, we may eaſily account for the new depreſſion of the water. After the pieces of iron were withdrawn, ſome portion of the acid would remain adhering to them, by which azot, or nitrous gas, with a ſmall proportion of oxygen, would be diſengaged; theſe were not abſorbed by the water, and the air could not be again diminiſhed in bulk, becauſe the oxygen had already been exhauſted by the nitrous gas firſt diſengaged, and we know too, that nitrous gas is not rapidly abſorbed by water; hence it muſt have ſuffered a new depreſſion from the diſengaged gas. It is evident, that in this experiment, Mayow procured nitrous air; and it is rather curious, he does not mention the turbid fumes which it produces in its union with fire-air particles. This appearance, as far as we know, was firſt mentioned by Dr. Hales, who procured nitrous air from Walton pyrites and ſteel filings, with the nitrous acid.*

If

* Vide Statical Eſſays, Vol. II. p. 280.

If the pieces of iron be let down a ſecond and a third time into the acid, freſh gas will be diſengaged, but the water will not riſe, as in the firſt caſe. His method of explaining the riſe of the water deſerves attention, notwithſtanding that it is a little confuſed, from the want of diſtinct ideas, with reſpect to the tranſition of bodies, from the aeriform to the ſolid ſtate. (o) The efferveſcing particles, he obſerves, produced in the firſt ebullition, being mixed with the particles of the air, deſtroy them; hence the water not only roſe into the ſpace left by the condenſed gaſes, but alſo into that which the deſtroyed air formerly occupied: but when the air in the glaſs was in a great part deſtroyed by the firſt fermentation, or rather had its elaſticity as much as poſſible diminiſhed, it happened, that after the gas was diſengaged a ſecond time, the water would not riſe into the glaſs, as in the firſt inſtance, except as far as the generated gas was condenſed. We may here perceive, that Mayow makes a diſtinction between the diminution of the air's elaſticity by the gas, and the condenſation of the gas itſelf. In the firſt inſtance, therefore, the water would rife from two caufes, *viz.* the lofs of elafticity in the air, and the condenfation of the gas; but in the fecond cafe, when the elafticity had been already deftroyed by the gas previoufly generated, the rife of the water could be only attributed to the condenfation of the nitrous air. Such is the manner in which Mayow accounted for this extraordinary phænomenon, which paft unnoticed, and neglected for more than half a century,* when it was obferved by Dr. Hales, who although he quotes Mayow on refpiration, took no notice of this curious circumftance. In the difengagement of airs, from Mayow's effervefcing mixtures, he obferved, that the water fometimes did not rife again, after its depreffion. He accounts for this in the following manner: (P) When the difengaged gafes occupy more fpace than is relinquifhed by the loft air, then, although the elaftic power of the air be diminifhed, that circumftance will not take place: hence when gafes are procured from the nitrous acid and a fixed falt (carbonie acid gas)

* The Vice-Chancellor's Imprimatur for Mayow's Tracts, is dated 1673, Sir I. Newton's, as P. R. S. for Hales's 1ft Vol. in 1726—27.

gas) and from ſulphuric acid and iron (hydrogen gas), in the manner already deſcribed, the water will not riſe above its priſtine height. When Mayow found, that part of the air could thus be condenſed, and as it were loſt, he ſet on foot an enquiry, to diſcover whether air could be generated anew. During this inveſtigation, he collected various gaſes, with which he made ſeveral trials, with reſpect to their fitneſs for reſpiration and combuſtion. In the following experiment, the reader will perceive, that he procured nitrous air in a ſeparate ſtate.

(Q) Place equal parts of nitrous acid and water in a large glaſs, then plunge into it a ſmall glaſs goblet, till it be quite full, and after having introduced into its mouth two or three iron globules, invert it on the bottom of the other veſſel, as is ſeen in *Fig.* 3, taking care to prevent the globules from falling out, by applying the finger to the orifice. In a ſhort time, the acid will corrode the iron, and raiſe an effervеſcence; vapours, in the form of bubbles, will aſcend to the top of the glaſs, and there conſtitute a gas, by which the water will be gradually depreſſed. After the glaſs has been filled with the gas, the iron is to be withdrawn, taking care that the orifice of the glaſs does not riſe above the ſurface of the fluid. We ſhall ſoon ſee the gas becoming gradually condenſed, and the liquor riſing into its place. About one-fourth of the gas will remain in the glaſs, which cannot be condenſed by the ſevereſt cold. Some condenſation would, no doubt, take place, after the heat was diſſipated, which was generated during the effervеſcence; but the diſappearance of the nitrous air, is to be wholly attributed to its abſorption by the nitrous acid and water.

(R) If in the place of the nitrous, we uſe the vitriolic acid, mixed with water, or if the effervеſcence produced, as in the laſt experiment, be very ſlow, or if the globules of iron be allowed to remain a day or two in the glaſs, the gas generated in this way will ſuffer no condenſation. The reader will perceive, that hydrogen was procured in the firſt inſtance, which we know is not abſorbed; and in the two laſt caſes, the liquor, on account of the nitrous air paſſing ſlowly through it, would become ſaturated, and no diminution would take place in that which roſe to the top of

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the glaſs: ſome azot, too, would undoubtedly be diſengaged. It is difficult to ſay, obſerves Mayow, whether this gas be really air or not; but it is evident, from the following experiment, that it is equally capable of expanſion.

(S) Take a glaſs tube, of the ſize of a gooſe quill, and about four fingers breadth long, hermetically ſealed at one end; let fall into it a drop of water, and note down the ſpace occupied by the drop, upon paper paſted on the outſide; then let fall ſome more drops, noting down the height; and ſo on. The open extremity of the tube is then to be cloſely cemented, into the orifice of another glaſs, perforated at both ends, as is ſeen in *Fig*. 5. The tube and glaſs veſſel are then to be filled with water, and inverted in another, containing the ſame fluid. The gas is then to be transferred into this apparatus, in the following manner: (T) Let a ſmall veſſel, filled with water, be introduced under the orifice of the other, containing the gas, and be thus removed to the apparatus, containing the veſſel and tube; then bring the mouth of the glaſs, filled with the gas, to the orifice of the one filled with water, and in-

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cline the former in ſuch a manner, that the gas may paſs into the latter, which may be thus filled with gas, although a ſmall quantity will ſuffice for our purpoſe. The veſſel and tube containing the gas, are then to be placed in Boyle's air-pump, by introducing under it a veſſel, ſufficiently large to receive its orifice. (V) When the apparatus was thus adjuſted, he found, by exhauſting the air-pump, that the gas expanded, ſo as to fill the glaſs veſſel; and by comparing the capacity of the whole veſſel, with the ſpace generaly occupied by the gas, and marked on the paper, he found, from frequent repetition of the experiment, that the gas expanded about two hundred times its original bulk ; and had the preſſure of the water been removed, he ſays, it would have made double that expanſion, and common air managed in the ſame way, does not expand more. (W) This experiment, then, decidedly proved the gas to have one property, common with atmoſpheric air. But this does not argue its reſpirability; for air in which an animal has died, or candle been extinguiſhed, is capable of an expanſion, equal to that of any other air; and we know it to be deprived

of

of the nitro-aerial and vital particles. Although therefore, theſe gaſes will expand themſelves, when atmoſpheric preſſure is removed, we muſt not imagine, that they are reſpirable. This analogical reaſoning did not ſatisfy Mayow, he was determined to reſt his opinion upon matter of fact; to clear up therefore all doubt and ſuſpicion, on the ſubject, he made the following experiment.

(X) Let a mouſe, confined in a trap, be placed upon a ſupport, under an inverted glaſs veſſel, as is ſeen in *Fig.* 6. Let all things remain in this ſituation, till the animal dies: the time it lived is to be exactly noted. The mouſe is now to be withdrawn, and another placed in the ſame ſituation, and under ſimilar circumſtances, taking great care, that the ſame quantity of air be uſed in this, as in the firſt caſe. The apparatus being thus adjuſted, ſome gas is then to be thrown up into the veſſel, about twice or thrice the quantity it contains of air. In this caſe it will be found, that the animal will live but very little longer than in the firſt, when the gas was not confined with it. The reaſoning he applies, ſhews how accurately he attended to the reſult of his experiments. If the gas

were

were really air, and fit to ſupport life, the ſecond animal employed ſhould have lived twice as long as the firſt. An obſervation which he adds is extremely nice; the reaſon why the animal lived ſomewhat longer in the ſecond, than in the firſt inſtance is, becauſe the air being diluted with gas, the fire-air particles are more gradually conſumed.

Mayow does not mention, from what he procured the gas made uſe of in this experiment; but from the reſult we muſt conclude, that it was the hydrogen, otherwiſe the mouſe would have been inſtantly killed by the nitrous gas. From a review of theſe experiments, we cannot but admire the neatneſs and accuracy with which our ingenious author contrived and executed his plans. The method he uſed for transferring the gaſes from one veſſel to another claims our admiration, and muſt have been the reſult of much painful labour and attention. We have ſeen how completely he has anticipated the modern chemiſt, in this mode of experiment. One method, however, which he uſed, is ſufficiently curious, and certainly deſerves notice. He wiſhed to transfer, from one veſſel to another, the air which had been de-oxygenated by reſpiration and combuſtion. To effect this, he had recourſe to the following expedient. A phial is filled with water; round its neck is tied a piece of ſtring; another piece, of ſufficient length to ſuffer both its extremities to come out from under the edge of the glaſs, is thrown acroſs the tranſverſe ſtick in the apparatus, *Fig.* 4; one end of this is tied to the bottom of the phial; by drawing down, therefore, the other end of the ſtring, the phial will be raiſed above the level of the water in the inverted glaſs, and will conſequently be filled with air; again, by drawing down the ſtring to the neck of the phial, it can be withdrawn, and transferred where we pleaſe. He did this with a view of diſcovering, by his meaſuring apparatus, *Fig.* 5, whether the air, in which an animal had died, or candle been extinguiſhed, was capable of expanſion, which he found was the caſe. We may obſerve, that our ingenious author did not diſcriminate between the different gaſes he obtained. One diſtinction, however, he made, but it did not occupy his attention, *viz.* that ſome of his gaſes underwent, what he called, condenſation; but which was their abſorption by the liquors, over which they were procured, while others did not. He ſeems to have conſidered them all the ſame, in their properties; and only knew they were unfit to ſupport animal life and combuſtion. Let it be remembered, that after the lapſe of more than a century, it required the innumerable experiments, and diligent inveſtigation of Prieſtley and Cavendiſh, to diſtinguiſh rightly between the gaſes; and even after this period, the effects of noxious airs upon animal life were not known, the experimentaliſt conſidering the qualities of the unreſpirable airs as ſimilar. This led to the different concluſions which Mr. Kite and Mr. Coleman drew from their experiments*; but the cauſes of the error we have pointed out, in our paper to the Royal Society, on the effects of the different gaſes on the influence, termed by Galvani, animal electricity.

(A) Si cucurbitula, flammâ repleta, cuti applicetur, flammam mox extinctum iri, ſpatiumque

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* Diſſertation on Suſpended Reſpiration, by E. Coleman, p. 24.

que intra cucurbitulam inclufum, pene vacuum fore. P. 97.

(B) Probabile eft, aerem flammæ confertim immifceri, utpote cui in pabulum cedit; ita ut ne minima quidem flammæ pars fit, in quâ aeris aliquantulum non exiftit.

(C) Nempe candela accenfa, ita in aqua collocetur, ut ellychnium accenfum digitos circiter fex tranfverfos aquæ fuperemineat: dein cucurbita vitrea fatis alta, inverfaque, lucernæ ifte fuperimponatur, prout in *Fig.* 1, oftenditur. Curandum eft autem hic, ut fuperficies aquæ intra vitrum inclufæ, altitudinem aquæ exterioris æquet; quo autem id fiat, fyphonis incurvati ejus alterum intra cucurbitæ cavitatem, antequam eadem in aquam demittitur, includatur crure altero exterà eminente. Syphonis iftius ufus eft, ut aer in alembico inclufus, et ab aquâ ei fubftratâ, dum vitrum in aquam demittitur, compreffus, per fyphonis cavitatem exeat. Cum autem aer per fyphonem iftum exire definit, fyphon ftatem eximatur, ne aer poftea per eundem in vitrum irruat. Brevi aquam in cucurbitæ cavitatem, cum adhuc lucerna deflagrat, gradatim affurgentem percipies. P. 99.

(D) Nempe materia quævis combuſtibilis, quæ facile flammam concipiet, in cucurbitâ quam capaciſſimâ, inversa, ſuſpendatur, prout in *Fig.* 1, oſtenditur (ipſe fruſtulum camphoræ, cui lentei ad nigredinem, uti moris eſt, calcinati, ſulphureque liquefacto intincti, tantillum affigitur, ſuſpendere ſoleo). Quo facto, cucurbita ea inverſa, ad digitos circiter 10 aquæ, ita immergatur, ut aqua intra vitrum incluſa, aquæ exterioris altitudinem æquet, quod ope ſyphonis fieri poteſt; ac demum aqua exterior eouſque exhauriatur, donec aquæ interioris, altitudo exteriori ſupereminet, quo melius in conſpectum veniat; tum altitudo aquæ interioris, chartulis hic illic lateribus vitri affixis, notetur. Jam vero cucurbita iſta radiis ſolaribus exponatur, et camphora in eadem incluſa, ope vitri uſtorii accendatur; quo facto, aquam interiorem, ob particularum ignearum exagitationem aeriſque rarefactionem, intus deſcendere percipies. Poſtquam lucerna expiravit, cucurbita una cum vaſculo, cui incumbit, a radiis ſolaribus amoveatur, quo ſe aer in ea incluſus denuo refrigeſcat, et ad priſtinum ſtatum redeat, atque ita aquam interiorem, ſupra metam primo notatam, elevatam eſſe invenies. P. 100—101.

(E) Nempe veſica madefacta, orificio circulari vaſis cujuſvis ſuperextendatur, eidemque alligetur; dein cucurbitula, in quâ animalculum, puta, mus incluſus eſt, veſicæ prædictæ ſtrictim applicetur, ut in *Fig.* 2. delineatur. Cucurbitulam, poſt breve temporis ſpatium, veſicæ iſti firmiter affixam, et inſuper veſicam, qua eadem cucurbitulæ ſubſtrata eſt, ſurſum in vitri cavitatem impulſam videre eſt, haud ſecus, ac ſi cucurbitula ea, cum flammâ ei incluſa, applicata fuiſſet; atque hoc, animale ad huc ſpirante, continget. Et quidem, animalculum cucurbitulæ cuti applicandæ impoſitum, flammæ vicem aliquantulum ſupplere poteſt. Ex quibus liquet, vim elaſticam aeris, per animalis reſpirationem, imminutam eſſe. P. 103-4.

(F) Nempe animal, in carcere idoneo incluſum, in cucurbita vitreâ ſuſpendatur, prout vaſculum in *Fig.* 4. Dein vitrum inverſum aquæ aliquantulum, ita immergatur, ut aqua intra vitrum incluſa, aquæ exterioris altitudinem æquet, quod ope ſyphonis incurvati fieri poteſt. Aqua exterior aliquantulum exhaureatur, quo altitudo aquæ interioris melius conſpici poſſit, quæ chartulis hic illic vitri lateribus affixis, notetur. Brevi, aquam in vitri cavitatem ſenſim aſſurgentem videbis, licet calor ab animalis præſentiâ in vitro iſto excitatus, item halitus ab eodem exeuntes, potius contrarium efficere viderentur; et quidem experimento, cum animalibus variis facto, compertum habeo, aerem in ſpatium ex parte circiter decimâ quartâ minus, quam antea, per animalium reſpirationem redactum eſſe. P. 104-5.

(G) Ex dictis, certo conſtat, animalia reſpirando particulas quaſdam vitales eaſque elaſticas ab aere exhaurire; ut minime jam dubitantum ſit, aereum aliquid, ad vitam prorſus neceſſarium, ſanguinem animalium reſpirationis ope ingredi. Ex quibus manifeſtum eſt, aerem per animalium reſpirationem, haud multo ſecus, ac per flammæ deflagrationem, vi ſuâ elaſticâ deprivari; et utique credendum eſt, animalia ignemque particulas ejuſdem generis ex aere exhaurire, id quod ſequenti experimento, magis adhuc confirmatur. P. 106-7-8.

(H) Nempe animalculum quodvis, unà cum lucernâ, in vitro includatur. Brevi lucernam iſtam expirantem videbimus, neque animalculum diu tedæ ferali ſuperſtes erit. Etenim obſervatione com-

 pertum

pertum habeo, animal una cum lucernâ, in vitro inclufum, haud multo plus, quam dimidium temporis iftius, quo aliàs viveret, fpiraturum effe. P. 108.

(I) Porro conatus fum materiam combuftibilem, in vitro, una cum animale fufpenfam, poftquam animal in eodem fuffocatum eft, ope vitri uftorii accendere; verum experimentum non fucceffit. Verifimile eft autem, aerem, qui vitæ fuftinendæ inidoneus eft, etiam ad flammam conflandam ineptum effe. P. 110.

(K) Si animalculum, veluti mus aut avis, in fummitate vitri includatur, idem multo citius morietur, et aqua fubftrata multo minus elevabitur, quam fi animalculum idem, in inferiori parte vitri collocatum effet. Hoc autem maxime manifeftum erit, fi aves, aut mures duo, fimul, unus in fuperiori, alter in inferiori parte vitri inclufi fuerint; ita enim animalculum, in inferiori vitri parte pofitum, alteri aliquandiu fuperftes erit. P. 124—5.

(L) Verum arbitrari fas fit, particulas aeris, è pulmonibus animalis egeftas, ob particulas nitroæreas partim ex ipfis exhauftas, leviores fieri, eoque ad fummitatem vitri afcendere. P. 125.

(M) Quinimo fi cucurbita vitrea inverfa, in aere ſuſpendatur, et dein lucerna in eandem immittatur, lucernam brevi expirantem percipies; quippe aer, in vitro iſto contentus, ob lucernæ deflagrationem, ad ignem ſuſtinendum ineptus redditur, cum vero idem reliquo aere levior ſit; hinc ſit, quod aer ambiens eum ſurſum impellit, neque facile è vitro deſcendere permittet. P. 126.

(N) Nempe bacillus diametro cucurbitæ vitreæ, qua eadem maxime patet, æqualis, in cucurbitam imponatur, dein idem tranſverſim collocatus, deorſum trahatur, quouſque bacilli extremitates utræque vitri lateribus innixæ, ab iiſdem ſubfulciantur, prout in *Fig*. 4. cernitur. Vaſculum fictile intus vitrefactum, unciarum liquoris circiter quatuor capax, ope unci ferrei ei adaptati, de bacillo iſto tranſverſo ſuſpendatur, idemque ad dimidium plus minuſve ſpiritu nitri impleatur. Porro fruſtula aliquot ferri in faſciculum colligata, ope funiculi de bacillo prædicto ſuſpenſi, e directo ſupra vaſculum illud ſuſpendatur, funiculus autem iſte tantæ longitudinis eſſe debet, ut ejus extremitas altera ad vitri orificium protenſa, foras propendeat. Orificium cucurbitæ iſtius inverſæ ad digitos circiter quinque aquæ immergatur ita—

aqua

aqua exterior eousque exhauriatur, donec aqua interior digitos ciciter tres eidem superemineat; atque ita maneant omnia, donec aer in vitro inclusus manibus tractantis calefactus, ad pristinum statum redeat: tum altitudo aquæ interioris chartulis hic illic vitri lateribus exterius affixis, notetur. Jam vero frustula prædicta, ope funiculi, in vasculum, in quo spiritus nitri collocatus est, demittatur; atque ita brevi æstus admodum intensus excitabitur et aqua interior ab halitibus inde ortis statim deprimetur. Postquam æstus istius modi per tertiam plus minusve horæ partem duraverit; seu potiùs cum aqua interior ad digitos circiter tres ab halitibus excitatis depressa fuerit, frustula ista ferri ope funiculi e vasculo eleventur. Post' brevi temporis spatium aquam interiorem gradatim assurgentem et intra horam unam aut alteram longè supra altitudinem ab initio notatam, elevatam esse percipies; ita ut pars circiter quarta spatii, quod in vitro eo antea ab aere occupatum est, nunc ab aqua intus assurgente teneatur. Et quidem aqua eo modo in vitrum elevata, neque post longum tempus, ad pristinam metam descendet. P. 136-7-8.

 (o) Par-

(O) Particulæ effervefcentes ab æftu priori exeuntes, particulis aereis confertim immixtæ, eas deperdiderunt; unde factum eft, ut aqua non tantum in fpatium ab halitibus condenfatis, fed etiam ab aere abfumpto relictum, elevata fit: cum vero aer in vitro eo ex maximâ parte per primam fermentationem deperditus eft, feu potius quoad vim fuam elafticam, quantum fieri poterat, imminutus eft, hinc fit, ut poft æftum fecundâ vice factum, aqua non nifi, quatenus halitus excitati condenfationem patiebantur, et proinde multo minus, quam in priori cafu in vitrum afcenderet. P. 141.

(P) Quandocunque halitus generati plus fpatii occupabunt, quam ab aere deperdito relinquitur, tunc, utcunque vis elaftica aeris imminuitur, illud tamen in vitro prædicto non comparebit. Atque hinc eft, quod fi æftus a fpiritu nitri et fale fixo commixtis, uti etiam ab oleo vitrioli et ferro, modo prædicto, in vitro excitatus fuerit, aqua tamen, fupra altitudinem priftinam non affurget. P. 145.

(Q) Nempe fpiritus nitri et aqua fontana, in æquali quantitate commixta, vitro fatis amplo imponantur; dein vitrum parvulum mixturæ ifti ita

fubſubmergatur, ut idem liquore eo penitus repleatur, quo facto, globuli duo vel tres, e ferro compoſiti, orificio vitri hujus indantur, idemque inverſum fundo vitri alterius incumbat, prout in *Fig.* 3, delineatur, cavendo tamen, ne globuli iſti è vitro excidant; quo autem hoc præcaveatur, orificium vitri iſtius digito obturetur, donec idem fundo vitri alterius innititur. Poſt brevi temporis ſpatium, menſtruum illud acidum globulos ferreos corrodet, et cum iiſdem inſigniter effervefcet; halituſque ſub bullularum formâ, ab æſtu eo excitati, ad ſupremam vitri partem aſcendent, auramque ibidem component, quæ ſenſim aucta aquam ſubſtratam gradatim deprimet. Poſtquam vitrum iſtoc aura iſtius modi penitus impletum eſt, idem parum elevetur, quo globuli ferrei ex eodem elabantur, qui è liquore eximendi ſunt; cavendo tamen, ne vitri orificium ſupra liquorem attollatur. Atque ita videbimus auram iſtam gradatim condenſari, liquoremque ſubſtratum in locum ejus aſcendere; etenim vitrum ex parte circiter quartâ uſque implebitur, auraque ea, tempeſtate frigidiſſima exiſtente, nunquam tamen in liquorem condenſabitur. P. 161-2-3.

 (R) Si

(R) Si loco ſpiritus nitri, oleum vitrioli cum aqua commixtum ſubſtituatur; aut ſi fermentatio valde remiſſa, modo prædicto, excitetur; aut etiam ſi globuli ferrei, in vitro eo per diem unum aut alterum manere ſinantur; tunc aura, eo modo generata, vix omnino condenſationem patietur. 163.

(S) Nempe vitrum tenue, ad magnitudinem calami anſerini, quatuor plus minus digitos longum, in alterâ extremitate hermeticè ſigilletur; dein gutta una aquæ, orificio ejus alteri aperto, inſtilletur, et in chartulâ vitro exterius affixâ deſignetur, quantum vitri iſtius ſpatium gutta ea occupat; tunc gutta inſuper altera, et ita deinceps aliæ vitro ei, ut prius immittantur, ſpatiumque ab iis impletum, in chartulâ prædictâ notetur. Vitri iſtius extremitas aperta, orificio anguſtiori vitri alterius, in utrâque extremitate perforati, indatur, et dein foramen illud ope cementi idonei ſtrictim obturetur, prout in *Fig.* 5. delineatur. Vitrum hoc, eo modo paratum, aquæ in vaſe idoneo contentæ, ita ſubmergatur, ut orificio ejus ſurſum verſus ſpectante, aer ex eodem totus exeat, et aqua ejus locum ſubeat, curando diligenter, ut etiam vitrum aquâ repleatur; dein vi-

trum,

trum, iſtoc aquâ impletum, invertatur, idemque fundo vaſis alterius incumbat, atque ita ſingula maneant. P. 164-5.

(T) Nempe vaſculum parvulum, orificii tamen vitri, in quo aura exiſtit, capax, eidem tamen ſupponatur; et dein vaſculum illud liquore prædicto repletum, una cum vitro inverſo, in quo aura continetur, ei incumbente, in vaſculum illud transferatur, in quo vitrum primo deſcriptum collocatum eſt, atque orificium vitri, in quo aura exiſtit, orificio vitri alterius aquâ repleti ſupponatur, vitrumque illud eouſque inclinetur, quoad aura in eo contenta prorumpit, et in vitrum alterum aſcendit, quod hoc modo aurâ iſtâ impleri poteſt; quanquam ſatis eſt, ut aeris aliquantulum eidem immittatur. P. 165.

(V) Poſquam aer ex parte exhauſtus eſt, aura in vitro prædicto incluſa, ultra vitri cavitatem ſe expandet. Itaque ſi ſpatium vitri totius, ope aquæ ei guttatim inſtillatæ, commenſuretur, idemque cum ſpatio eo vitri tenuioris, quod aura reliqua adimpleverat, conferatur, compertum erit, in quantum aura prædicta ſe expanderat. Atque experimento *ſæpe* repetito, compertum habeo,

auram

auram iftius modi, plus quam ducenties fe expandiffe; et quidem fi eadem ab aquæ ambientis preffurâ liberata fuiffet, in fpatium circiter duplo majus fe expandiffet, neque quidem aer vulgaris, eodem modo difpofitus, in fpatium majus fe explicabit. P. 166-7.

(W) Quanquam aura ab æftu prædicto generata, vi elaftica non minori, quam aer vulgaris imbuta fit, tamen non exinde fequitur, eam reverâ aerem effe talem, *viz.* qui particulis vitalibus, igneifque donatur; quippe aura ifta, in quâ animal aut lucerna expirarunt, vi elafticâ æque ac aer inviolatus pollet; et tamen eadem particulis nitro-aereis, vitalibufque, deftituitur. P. 169.

(X) Animalculum puta mus in carcere angufto collocatus, in fummâ vitri inverfi cavitate collocetur, fuftentaculo idoneo ei fuppofito, ut in *Fig*. 6, delineatur. Ita maneant omnia, ufque dum animal moriatur; fpatiumque temporis quo animal in vitro eo fpiravit, diligenter obfervetur. Jam vero animal mortuum eximatur, atque animal aliud vivum, in ejus loco fubftituatur, et veluti prius, intra vitrum aquæ immerfum includatur, curando diligenter, ut aeris eadem

quantitas,

quantitas, quæ prius, intra vitrum includatur: quo facto, aura prædicta in vitrum, in quo animal collocatum eſt, tantâ copiâ modo antea oſtenſo transferatur, ut aura ita immiſſa, bis aut etiam ter aerem vulgarem intra vitrum iſtoc ab initio incluſum ſuperet. Comperiemus animalculum, in vitro eo, haud multo diutius ſecundâ vice vitam degiſſe, quam alterum primâ, cum, *viz.* aura prædicta vitro iſti non imponebatur. Sin autem aura iſta revera aer, eademque vitæ ſuſtinendæ idonea eſſet, animalculum ſecundâ vice immiſſum, duplo longius quam præcedens perduraſſet. Quod vero animalculum, aurâ prædicta, vitro in quo idem extitit, impoſitâ, paulo diutius quam alias vixerat, ratio eſſe videtur; quoniam aer, intra vitrum iſtoc incluſus, ob auram ei admixtam magis gradatim, neque adeo confertim ac aliàs ab animalculo hauriri et violari, poterat. P. 169-70-1.

SECT. VI.

Of Respiration.

There is no part of the animal œconomy, which would fooner have excited the attention of the obferver, than the neceffity of continued refpiration to the life of animals. The moft common daily occurrences muft have pointed out, that we could difpenfe with any thing but air for a confiderable time. (A) So great a demand, fays the venerable Hippocrates, have our bodies for air, that we may refrain, with impunity, from all other kinds of food, for two, three, or more days; but if any thing fhould interrupt the communication of the air with our bodies, for a fhort time only, we muft unavoidably perifh, fo neceffary is the prefence of air to our being. A crowded meeting, in places of public amufement, or courts of juftice, muft have preft upon the obfervation of the earlieft philofophers, the neceffity

of

of the prefence of frefh air. The decompofition which daily takes place in different fubftances, from fermentation, prefented to their examination various exhalations; but thefe were all confounded under the name of air, or vapour. They beheld the beauteous ftructure of the animal frame governed by certain laws, which did not exift one moment without the prefence of air; hence it was reafonable to infer a connection between the functions of the fyftem and that fluid. A diftinction was alfo made between the air neceffary, and the external atmofphere; but the manner of its action, and the nature of its effects, were entirely unknown. We are furprifed that they knew fo much, when we confider how little they were acquainted with the operations of the animal œconomy; and for the greateft part of their knowledge on this fubject, they were indebted to the labours of one man, to the acute obfervation and regular attention of the venerable Father of Phyfic. A fpirit, it was faid, pervaded all nature, gave laws to the animal œconomy, and regulated the motion of the heavens, and this fpirit was derived from the air, but was different from

it.

it. Hippocrates mentions, that all animated nature is ſupported by a triple nouriſhment, by meat, drink, and a ſpirit, σιτα, ποτα, και πνευματα. Since exiſtence was inſeparable from this ſpirit, it was conſidered as the ſoul and great mover of all the operations of the œconomy, and became ſynonymous with life.*

Dum memor ipſe mei, dum ſpiritus hos regit artus.†

Afterwards, when the nerves of the body were found to have ſo great a ſhare in its motions, this ſpirit was ſuppoſed to reſide chiefly in them; hence the terms nervous, vital, and animal ſpirits. The word ſpirit was uſed to convey the idea of an immaterial quality, which might exiſt *per ſe*, but without which all the animated creation would be devoid of life and vigour. After the time of Hippocrates,

† The lungs are ſo uſeful to us, as to life and ſenſe, that the vulgar think, our breath is our very life, and that we breathe out our ſouls when we die. Suitably to this notion, both *anima* and *ſpiritus* in Latin, and πνευμα in Greek, are derived from words that ſignify both breath and wind, and *efflare*, or *exhalare animam*, ſignifies to die.—Ray, p. 13, Part II.

* Æneid Lib. VI. V. 336.

Hippocrates, it was the faſhionable philoſophy, that heat or fire was that quality, under the name of ſpirit, which produced harmony and order in the revolutions of the animated world. ' Hic noſter ignis, ſays Cicero, quem uſus vitæ requirit, confector eſt et conſumptor omnium, idemque quocumque invaſit, cuncta diſturbat ac diſſipat. Contra ille corporeus, vitalis eſt et ſalutaris, omnia conſervat, alit, auget, ſuſtinet, ſenſuque adficit'.* In the abſence of heat, it was obſerved, that the creation became torpid and cramped in her operations, ' all nature ſickened,' and the hybernating animals retired to their caves and ſecret places, to remain until this vivifying principle reſtored animation to the face of the world; hence probably, aroſe the idea of fire or heat, being the ſoul and life of created being; and hence we may derive the terms vital flame, igneous ſpirit, &c. It was the breath of life, and impreſt upon the world, *ab origine*, by the Deity.

> Igneus eſt ollis vigor, et cœleſtis origo
> Seminibus†.

In

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* De Natura Deorum, p. 144.

† Æneid. Lib. VI. v. 730.

In the time of Hippocrates then, the ſpirit which animated the human frame, was derived from the air, and afterwards the diſciples of the Platonic ſchool conſidered it as the ſame with fire. They made no diſtinction, however, between life itſelf and its ſtimulus. The body was the machine in which this ſpirit performed its operations; but they did not imagine that, connected with the body, was a principle, independent of this ſpirit, but to which it acted as a ſtimulus. This beautiful illuſtration of the œconomy was reſerved for a diſtant period. No wonder then, that the antients were perplexed in their reaſonings on this ſubject, ſince it required ſo many ages to diſcover a principle, without the knowledge of which, we ſhould have ſtill remained ignorant ſpectators of the phænomena which daily occur in the animated frame. It is a curious co-incidence in the hiſtory of ſcience, that the two qualities which the antient philoſophers conſidered as neceſſary to life, and which they thought were different, ſhould be found combined in a ſingle ſubſtance by the diſcoverers of modern times. We cannot be ſurpriſed to find, that the two moſt powerful agents in nature, air and fire, ſhould have been called forth to explain the phænomena, which occurred to the obſervation of the philoſopher. General obſervations, however, were only made without being particulariſed by any determinate ideas on the ſubject. This could not be otherwiſe, when we conſider how little men were in the habit of making experiments, and how much it was neceſſary that the arts ſhould be improved, before it was poſſible that experimental knowledge could be acquired. So near is the connection between the arts and ſciences, and ſo much does the practice of the philoſopher depend upon the improvement of the workman's art. Ridiculous hypotheſis prevailed, in proportion as the theoriſt was perplexed in his ideas, until the great Lord Verulam arreſted the progreſs of theoretical reaſonings, not founded on direct experiment. Soon after this Mayow appeared, and in 1668, publiſhed his Treatiſe on Reſpiration. The doctrine laid down for this important function, evidently ſhews, the great attention of an ingenious mind, eagerly applied to the object of its purſuit. Unſatisfied with having diſcovered that only one part of the atmoſphere ſupports vitality, he extends his enquiry, to learn what *this* performs in the ſyſtem; and we ſhall ſee how accurately he has ſtruck out the truth. He rejects the prevailing theory of his time, that reſpiration is to cool the blood; on the contrary, he maintains that it produces the oppoſite effect. He denies that it is intended to tranſmit the blood from the right to the left ſide of the heart. (B) Indeed, it is evident, that the blood can paſs through the lungs without their motion; for if blood, or any other fluid, be injected by the means of a ſyringe into the pulmonary artery of a dead animal, it will very readily paſs into the left ventricle of the heart. Any one may experience this in himſelf; for although reſpiration be ſuppreſſed for a time, yet the pulſation of the arteries is ſufficiently firm in the wriſts, which certainly could not happen, unleſs the blood at that time paſſed through the lungs, to the left ventricle of the heart. He does not however deny, that the action of the thorax, aſſiſts the tranſmiſſion of the blood; neither will he allow, that reſpiration is to break down the denſe venous blood, becauſe impure air very well anſwers the purpoſe of giving motion to the lungs, and agitation to the blood, but it does not ſupport life. Having thus refuſed his aſſent to the commonly received doctrine of reſpiration, it was incumbent on him to deliver his ideas on the ſubject, and to urge his objections. Concerning the uſe of reſpiration, therefore, we may reaſonably conclude, that an aerial ſomething, whatever it may be, abſolutely neceſſary to ſupport life, paſſes into the maſs of blood; thence air thrown out from the lungs, from which the vital particles are exhauſted, is no longer fit to ſerve the purpoſe of reſpiration. Mayow ſeldom hazards an opinion without the rigid teſt of experiment, and here he adduces one in proof of his doctrine.

(c) If the lungs of a dog be inflated by a pair of bellows, in ſuch a manner that the air may paſs through perforations made in them, which is immediately to be ſupplied by the bellows, leſt the lungs ſhould collapſe; in this caſe, I ſay, the dog will live. Yet when the lungs are kept in this conſtant ſtate of inflation, no ſhaking or agitation of the blood can take place; and we know too, that if the motion of the lungs be entirely ſuppreſſed, the blood will, nevertheleſs, paſs to the left ventricles. Again, if after a full inſpiration, the mouth and noſtrils be ſtopt, although the lungs remain inflated, the animal muſt die, becauſe it cannot make an exſpiration; and yet, in this caſe, the blood has a paſſage through the lungs, equally as free as in the former; nor can it be ſaid, that in the firſt inſtance, there was a greater breaking down of the blood, ſince in each experiment the lungs were equally diſtended, a decided proof, that reſpiration is not ſo neceſſary, either to the paſſage of the blood through the lungs, nor to break it down, ſince, in the firſt caſe, the animal lives, but in the laſt it dies, becauſe in that there was an occaſional ſupply of freſh air, but in this there was none.* No other concluſion could be drawn from theſe experiments and obſervations, than that which Mayow made; the inferences were perfectly juſt, and warranted the opinion founded upon

* An experiment, nearly ſimilar, has been made, by a celebrated modern phyſiologiſt, whoſe death ſcience muſt regret, to confirm an opinion he had formed, with reſpect to the cauſe of death in drowning.—Hunter, p. 118. See alſo Hales, Vol. I. 253.

upon them. Having thus proved, that refpiration is not intended to break down the blood, nor to pafs it through the lungs, but that this function is intended to tranfmit air into the lungs, let us next inquire, fays Mayow, what that aerial fupply is, which is fo abfolutely neceffary to the fupport of life, that we cannot exift one moment of time without it; and this he determines to be the fire-air particles. Here again, another difficulty prefented itfelf, which he was determined to folve, *viz.* fince the fire-air particles are fo neceffary to exiftence, what purpofe do they anfwer, after being tranf-mitted into the mafs of blood? Not content with having proved the brilliant truths we have de-tailed, he is reftlefs, until he finds out, of what ufe the fire-air particles are to the fyftem. After having thus introduced the nitro-aerial particles into the blood, we muft next inquire, what pur-pofes they fulfil; and indeed, it is our opinion, that the nitro-aerial particles are the principal fource of life and motion in animals. This life and motion, he proves, are produced by heat evolved in the fyftem from the fire-air particles. We fhall prefently fee how beautifully this obſervation is confirmed, and how exaƈtly conformable Mayow's explanation is to doƈtrines lately promulgated.

It will be neceſſary, however, before we proceed any further, to ſay a few words on the modern doƈtrine of animal heat. The atmoſphere, it is well known, is compoſed of two kinds of fluids, azot and oxygen, which are kept in the gaſeous ſtate by caloric. During reſpiration, or the paſſage of the blood from the right to the left ſide of the heart, a great quantity of oxygen is abſorbed, rendering the venous blood arterial. Carbonic acid gas, and water, are formed during the proceſs; hence the blood loſes its hydrocarbonous impregnation, and has its capacity for caloric increaſed.* The oxygenated blood thus paſſes on to

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* To do juſtice to Dr. Duncan, the ingenious Profeſſor of Edinburgh, we muſt obſerve, that ever ſince the year 1774, he has delivered at his Leƈtures, a theory ſimilar to this; but in explaining animal heat, before the new diſcoveries, he employed the term phlogiſton, for which he has now very properly ſubſtituted the hydrocarbonous principle. The coincidence of opinion between the Profeſſor and Mr. Seguin, may be ſeen, by conſulting La Médicine Eclairée, Vol. I. p. 56.

to the different parts of the fyftem, through the medium of the circulation. During this courfe, but particularly in the capillary veffels, a chemical change takes place; the oxygen unites with the other elementary principles, by which caloric is evolved, and the phænomena of animal heat produced. This, we think, cannot be effected by mere chemical affinity; it is evident, that a certain action of the veffels muft take place to produce the effect, and the confequent change of caloric, from a latent to a fenfible ftate, muft arife from a peculiar power, modified by the operation of the vital principle; hence the local heat that very often occurs in different parts of the body. In fevers too, where the action of the vafcular fyftem is greatly increafed and deranged, the difengaged heat is one of the moft troublefome fymptoms to the patient, and to leffen this evolution of caloric, by whatever means it may be effected, is a great ftep towards a cure; hence the good effects that have been produced by the effufion with cold water, and which are certainly not to be explained from the mere reftoration of tone. It is by no means our intention to affert, that there is a power inherent in the body, capable of producing heat, independent of the oxygen abſorbed by the lungs. Something very like this is maintained by the ingenious Dr. Currie, in his valuable publication on fevers.* He ſays, the vibrations of the arterial ſyſtem (in an experiment which he made) were unuſually ſlow, yet animal heat was diſengaged with more than four-fold rapidity.† This obſervation is made to refute an opinion maintained by ſome, *viz.* that animal heat is connected with the action of the heart and arteries. No inſtance, we believe, can be pointed out, where increaſed action of the heart and arteries is not attended with a proportional increaſe of the heat of the ſyſtem. The reverſe of the propoſiton, however, is not true, *viz.* if the action of the heart and arteries be diminiſhed, it will be followed by a proportional decreaſe of animal heat; this is very eaſily explained. It is well known, that the extreme veſſels have an oſcillatory motion, independent of the action of the heart and larger arteries, as has been proved by Whytt and De Gorter;

* Vide p. 177.

† Appendix, p. 35.

Gorter;* the pulfe is therefore no indication of the action that is going on in thefe capillary veffels, and it is in thefe, we think, that the evolution of caloric is principally produced; hence more fenfible heat is indicated by the thermometer in the right than in the left ventricle of the heart.† The fubftances taken into the ftomach, will, no doubt, vary the actual degree of heat in the fyftem; for in proportion as thefe contain their component parts in a loofe ftate of combination, fo will they more readily unite with oxygen, and evolve the caloric; hence the heat produced by ardent fpirits, but more evidently by æther, which has the hydrogen and carbon very loofely combined. We might carry our fpeculations further, did our fubject admit of it;‡ but it is time to return to Mayow. (A) In order to explain his ideas of animal

* Whytt's Phyfiological Effays; De Gorter, De Motu Vitali.

† Coleman on Sufpended Refpiration, p. 42.

‡ Is it not probable, that the coldnefs always felt in digeftion is caufed by the dyfpeptic ftate of the ftomach, forming fubftances, which, by having a greater capacity for caloric, abforb, inftead of evolving animal heat?

animal heat, our ingenious author ſuppoſes, that a vital motion, or fermentation, takes place in the blood, by the union of fire-air with the ſalino-ſulphureous particles. Here he points out an analogy between the animal and vegetable kingdom; for as the nitro-aerial particles, which are abſorbed by the ſoil, ſtir up a gentle and gradual commotion with the ſalino-ſulphureous, upon which vegetable life depends, in like manner, the ſame nitro-aerial particles, being introduced into the maſs of blood by reſpiration, and being ultimately blended with the ſalino-ſulphureous, produce an efferveſcence of ſuch a nature, as is conducive to animal life. The dark and duſky colour of venous blood, is owing to the ſalino-ſulphureous particles;* and as fermentation in general depends upon the nitro-aerial particles, I do not heſitate to ſay, that the motion of the blood is derived from the ſame ſource. This opinion is confirmed, when we conſider, that the blood enters the lungs of

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* We wiſh the reader here to recollect, that the dark colour of the blood was attributed, till lately, to phlogiſton, for which we have now ſubſtituted hydrogen and carbon. Vide Crawford, p. 150.

of a dark colour, but returns of a bright red, as has has been proved by the ingenious Lower. Here Mayow adduces the arguments and experiments of this phyfiologift in confirmation of this fact, and adds, that if the furface of the blood in a cup be rendered fluid by the air, we muft not wonder that that effect fhould be produced in the lungs, fince the air, in this cafe, is diffufed through every particle of blood, and intimately blended with it. That arterial blood contains more air than venous, is evident from the following experiment.

(E) If blood which has been kept in a cup for fome time, be placed in the air-pump, fmall bubbles will rife from its furface by exhaufting the receiver; but if arterial blood, yet warm, be placed in a fimilar fituation, it will expand in a wonderful manner, and throw out an infinite number of bubbles. We muft here obferve, fays Mayow, that as the nitro-aerial particles caufe the fermentation of the blood, fo the fermentation ftrikes out, and attracts the nitro-aerial particles from the air, by which the motion in the blood is again renewed; that is, the nitro-aerial fpirit, mixed

with

with the maſs of blood in the lungs, produces in it a fermentation ſufficiently ſtrong; the greateſt part, however, of theſe particles being ſecreted from the blood for purpoſes in the ſyſtem hereafter to be mentioned, the motion of the blood returning to the lungs, on account of their deficiency, is much diminiſhed and rendered ſlower: the fermentation, however, does not ceaſe ſo much, but that it can be again renewed by an additional ſupply of fire-air particles; and thus it is, that in the animal automaton, the motion of fermentation obſerves one continual round. In like manner as the motion, ſo does the heat of the blood depend upon the union of fire-air with ſalino-ſulphureous particles. For if any ſalino-ſulphureous mineral, as the vitriolic marchaſite, be expoſed to moiſt air, a heat and motion will be produced by the effervefcence of the fire-air with the ſalino-ſulphureous particles. How much greater then muſt be the heat and motion of the blood which abound with the ſalino-ſulphureous particles properly prepared, and with which the fire-air particles are intimately blended by the power of reſpiration? In confirmation, I add, that the great heat obſerved in animals, from violent motion, takes place, because respiration being very much quickened, a larger quantity of fire-air particles are thrown into the blood, by which a greater than usual heat and motion are produced: in violent exercise we cannot artribute so much heat to the friction of parts, and indeed, if any one at rest will breathe quick for awhile, he will feel a glow of warmth to pervade his whole body. How has this doctrine been thrown away, says Beddoes, upon Boerhaave and his disciples, even upon Haller, the best of them all. Notwithstanding that Mayow makes use of the word effervescence, he takes care to explain his meaning, and does not admit of that expansion in the blood to which the action of the heart was attributed, previous to the Harveian discovery. He observes, that he by no means allows, that ferment, which has been said to be present in the left ventricle, for part of the blood in the fœtus, does not enter the left side of the heart, but passes directly through the canalis arteriosus in the aorta, which would not be the case if a ferment was so necessary to the blood in the left ventricle; how much less probable then is it,

that

that the pulſation of the heart depends upon a rarefaction, and expanſion of the blood in its ventricles. (F) If this were the caſe, the heart ſhould be conſiderably dilated at every pulſation, as a bladder is rendered larger by inflation; but it is evident, from diſſecting living animals, that the heart contracts at its pulſation, and the blood is expelled during contraction, and by no means during dilatation. Moreover, it appears, that the motion of the heart does not depend upon the rarefaction of the blood, for hearts cut from animals are obſerved to pulſate, although all the blood be ſqueezed out of them. Indeed I know, ſays Mayow emphatically, from repeated experiments, that if a ſolution of opium or cold water be injected into the jugular vein, the motion of the heart becomes quicker, which certainly does not ariſe from the expanſion of the blood. Whence it would appear, that the heart is a muſcle, which does not differ in its action from any other muſcle. The effervefcence therefore which Mayow meant, as he expreſsly aſſerts, was not an expanſion or rarefaction in the blood, but a motion which was communicated by the air to the blood in the

lungs

lungs, and which was continued as well in the whole vafcular fyftem as in the heart. As this vital motion in the blood depends upon the abforption of fire-air by the lungs, it was an inenevitable conclufion, that from a fuppreffion of refpiration, the blood would lofe thofe properties, by which it gave vigour and activity to the whole frame. But the blood is expelled with fufficient force from the right venticle, when it is not yet impregnated with air. Blood, therefore, with different qualities, is equally capable of motion, fince we find that in one ventricle it has acquired air, and, in the other, it is devoid of it. How then does it happen, fays Mayow, that death fo foon takes place from fuppreffed refpiration, fince blood without air, as happens in the right ventricle, is capable of motion? He explains himfelf in the following manner. (G) Life confifts in a diftribution of the animal fpirits; to fupply which, it is abfolutely neceffary that the heart fhould pulfate, and that there be a continual afflux of blood to the brain; for it appears, that the motion of the mufcles, and that of the heart, depends chiefly on this aerial falt. But it is neceffary to obferve, that the ſudden contraction of muſcles is produced, by the union and mixture of two kinds of particles, the one derived from the air, by reſpiration, the other ſupplied from the maſs of blood. Wherefore, when reſpiration is ſuppreſt, this aerial ſalt, ſo abſolutely neceſſary to motion, is withheld; the pulſation of the heart, and conſequently the afflux of blood to the head, muſt be interrupted, and death enſue. That this is the uſe of reſpiration, is further confirmed, by a variety of circumſtances. In the more violent exerciſes, we are obliged to breathe more quick and laborious; not that the blood may have a freer paſſage through the lungs, for we have ſhewn, that this can take place without their motion; but ſince, from the various effervеſcences produced in the action of muſcles, a very great waſte of the nitro-aerial ſalt is cauſed, ſo that the venous blood returns to the heart, very much impoveriſhed and grumous (as is obſerved more evidently in epileptic convulſions), therefore that the impoveriſhed blood may reſtore its waſte, there is a neceſſity for quicker reſpiration. Beſides, in the violent motions, on account of the more abundant flow of

 blood,

blood, the pulſations of the heart are increaſed, which could not be continued, (eſpecially as the blood is now deprived of air), without a more plentiful ſupply of nitro-aerial ſpirit: *hence we may conclude, that the chief uſe of reſpiration is, to cauſe and keep up the motion of the heart and muſcles.* We may further add, that if after the motion of the heart has ceaſed, on account of ſuppreſſed reſpiration, air be blown through a tube, inſerted in the *vena cava*, the motion of the heart will be gradually reſtored; whence it appears, that the air is that property, without which the motion of the heart cannot be maintained: nor is it of any conſequence, by what means air is introduced into the maſs of blood; whether through the lungs, or in any other way*. The doctrine which Mayow has thus laid down, is ſo exactly ſimilar to the theory ſuggeſted by the late diſcoveries, and

* This nice obſervation has been confirmed, by an experiment of ours, in which the motion of the heart was kept up, independent of reſpiration, oxygen being ſupplied to the blood, through the coats of the veſſels. Needham, in the laſt century, reſtored to life a ſtrangled dog, by inflating the thoracic duct with air.—Vide infra, P.

is ſo beautifully illuſtrated by himſelf, that it ſcarcely requires a comment. That the blood acquires a vital air in the lungs, for the purpoſes of animal heat, and loſes it again, on its return to the right ventricle, is now, we believe, not doubted by any. That this idea impreſt itſelf, with full force, on the mind of Mayow, we think every one will allow, after what has been delivered. So convinced, indeed, was he, of the truth of his doctrine, and ſo clearly did he ſee into the operation of his fire-air particles, that he extends their influence, to various functions in the animal frame. Although he does not ſeem to have diſtinctly underſtood, how ſubſtances paſt from an aeriform to a condenſed ſtate, yet he ſaw and proved, the abſorption of fire-air, by the blood, in the lungs.

One apparently great objection to his doctrine, however, ſtill remained to be refuted, which was, that the fœtus in utero, and chick in ovo, lived, were nouriſhed, and acquired growth, without the function of reſpiration. In the explanation of this ſeeming contradiction, we ſhall ſee Mayow purſuing one uniform and ſyſtematic plan, and applying his doctrine, in a moſt beautiful

manner.

manner. The moſt obvious, and therefore the moſt commonly received opinion, with reſpect to the uſe of the veſſels, was, that they were formed, for the ſole purpoſe of conveying nouriſhment from the mother to the child. Mayow denies, that this can be the ſole object of their formation; and refutes a variety of other opinions, brought forward on the ſubject. (H) Wherefore, ſince the functions, which have been aſſigned to the umbilical arteries, do not ſeem to be conſiſtent with the truth, we may be allowed to conclude, with the divine old man, Hippocrates, that the umbilical veſſels ſupply the office of reſpiration to the fœtus, which is alſo the opinion of the learned Everard*. We may, therefore, conclude, that the fœtal blood, conveyed through the umbilical arteries to the placenta, not only carries along with it a nutritious juice, but alſo a ſupply of nitro-aerial particles to the fœtus; whence the

* Although Mayow thus agrees with the premiſes of Everard, he does not aſſent to his concluſion, which is, that the circuit of the blood, through the long and tortuous courſe of the umbilical veſſels, is intended to cool it.

blood of the fœtus, by circulating through the umbilical veſſels, becomes impregnated with nitro-aerial particles, in the ſame manner as in the lungs. We muſt, therefore, no longer denominate the placenta, the liver, but the lungs of the uterus.

That blood is only of uſe, in proportion as it is impregnated with fire-air, is a fact which evidently arreſted the attention of Mayow. He ſaw, that reſpiration was quickened in violent motion, for the purpoſe of ſupplying that which was expended in the different functions; hence he concluded, that the fœtus muſt derive this aerial ſupply from ſome ſource. (I) Indeed it is very probable, ſays Mayow, that if arterial blood, which is impregnated with the nitro-aerial ſpirit, could be conveyed to the heart, inſtead of venous, there would be no occaſion for reſpiration. In confirmation of this, he mentions a very curious and remarkable experiment, which, if juſtly appreciated, cannot fail to convince the reader, that Mayow's ideas, with reſpect to the agency of fire-air in the body, are exactly ſimilar to thoſe of the moderns. (K) Whilſt arterial blood is tranſ-

fuſed

fufed from one dog into another, although the dog, into whom the blood is injected, is panting and breathing deeply, yet upon receiving the arterial blood into his veins, he fcarcely feems to breathe. The animal having received, fays the ingenious Beddoes, from an unufual fource, the fubftance which is probably expended in mufcular action, it was, therefore, not neceffary to inhale it rapidly.

The difcovery made by Mayow, that fire-air is abfolutely neceffary to the fœtal œconomy, and the fource from whence it is derived, have now, we believe, no opponents. This function of the umbilical veffels, Mayow extended to the chick in ovo. It is our opinion, he fays, that the chick breathes through the umbilical veffels, in a fimilar manner as the fœtus in utero. If it fhould be objected, that the air contained in the liquors of the egg, is not fufficient to fupply refpiration, during the whole time of incubation, I anfwer, that the air contained in the egg is not common air, but that aerial matter, felected from the common air, by the power of refpiration. Befides, the fœtus in utero and ovo, have occafion for only a fmall quantity of nitro-aerial particles, becaufe the fœtus, in either cafe, has no motion, except that of the heart; therefore a ſmall ſupply of nitro-aerial particles, from the arterial blood of the mother, and the liquors of the egg, is abundantly ſufficient for the purpoſe. The gentle warmth, too, excited in the egg by incubation, ſupplies the place of nitro-aerial particles, upon which depends the heat of the ſyſtem: for as the nitro-aerial particles enter the earth, and effervefce with the ſalino-ſulphureous, from which are derived the reſpiration and life of vegetables; and as the ſame particles, mixed with the blood in the lungs, cauſe a motion neceſſary to animal life; ſo being blended with the liquors of the egg, by the influence of the gentle warmth, they induce a vital fermentation, and animal motion in them, and ſupply the place of reſpiration. We are at a loſs, which moſt to admire, the beautiful facts Mayow diſcovered, or his cloſe and acute reaſoning upon them. Nothing can be more conſiſtent, than the manner in which, he ſays, fire-air is ſupplied to the fœtus in utero and ovo; and the reaſon he gives, for the fœtal ſtate not requiring ſo much fire-air, is admirably explained by him, as the reader will ſee in the ſection on muſcular motion.

SECT. VII.

Opinions *of* Authors, *contemporary with* Mayow, *concerning* Respiration.

About the time, that the ingenious Mayow gave to the world his ideas of reſpiration, two treatiſes were publiſhed, on the ſame ſubject; the one by Swammerdam, the other by Thruſton. It was the opinion of Swammerdam, that ſomething was mixed with the blood, during reſpiration, and which was neceſſary to its perfection; and alſo, as the blood paſt through the lungs, vapours were thrown off, as noxious to the ſyſtem. He aſks himſelf the queſtion, Of what uſe is reſpiration to life? and he anſwers, that it is of utility to the heart, and that, for the ſake of the preſervation of life, which we think conſiſts in the final perfection of the blood, in the left ventricle of the heart. He maintained, that the more ſubtile part of the air was abſorbed; and by his expreſ-

 ſions

ſions ſeems to think, that the vapours which are exhaled, make room for the abſorbed air. He held, neverthelcſs, the common opinion of the times, that the inſpired air was intended to cool and condenſe the blood, become heated and rare-fied by fermentation, in the right ventricle. He mentions, however, that the heart is ſtimulated to action, and nouriſhed, by the fine part of the air, taken in during reſpiration. Much confuſion and perplexity of ideas pervade the whole of his treatiſe; and he ſeems occaſionally at a loſs, to find terms to convey to the mind of the reader, his notions of the effect of air on the blood, re-ſulting, no doubt, from a want of experiment, by which alone, a knowledge of this kind is to be acquired. He relates, however, a curious ex-periment, which he made. A long flexible tube was fixed in an opening, made in the trachæa of a dog: the animal was then immerſed in water, and he lived for a conſiderable time, by breathing through this tube, the upper extremity of which roſe above the water, on the ſurface of which, the reſpiration of the dog produced a beautiful un-dulation.

In

In Thruſton's publication, we find ideas very ſimilar to thoſe we at preſent entertain, with reſpect to the uſe of reſpiration.* He ſeemed to be aware of the great connection between air and life, and of the dependence of the one upon the other, and quotes, in confirmation of this, the paſſage in Geneſis, 'He breathed into his noſtrils the breath of life.' In order to ſhew in what manner

* I cannot avoid remarking, as very ſingular, that in every work I have conſulted, publiſhed in the time of Mayow, no notice is taken of him. Thruſton's work was originally a theſis, which he wrote for his degree at Cambridge. It is a cuſtom in the Univerſities of Cambridge and Oxford, for the candidates to write a paper, on a medical ſubject, in which they are queſtioned by the Profeſſors, in the preſence of a numerous audience. Thruſton's theſis was firſt made known, when he took his degree, in 1664. Anno enim 1664, Cantabrigiæ in ſcholis medicis circumfuſa, virorum doctiſſimorum et juventutis florentiſſimæ, frequenti corona comparuit. Mayow's treatiſe was publiſhed about four years after, *viz.* in 1668; but Thruſton's was not delivered to the public, for ſeveral years after he took his degree: yet he makes no mention of Mayow, notwithſtanding he maintains, that reſpiration is to warm the blood; an opinion firſt proved by Mayow, in his beautiful experiments, and in oppoſition to the prevailing theories of the times.

manner respiration is so necessary to animals, he first considers motion as absolutely indispensable to the blood, while it is possessed of vitality. He brings forward the arguments of Harvey, for the circulation; and maintains, that the cause of it is heat, derived from the air absorbed by the blood in the lungs. He makes some observations upon the nature of air, which, he says, possesses nitrous particles, for which he refers us to Gassendi, Ent, and Digby. It is very probable, says Thruston, that as the more pure, more subtile, and more penetrating part, is absorbed by the lungs, and is there, as it were, lost, so the expired air, becoming replete and vitiated with the exhalations, and deprived of its better part, is, in some degree, rendered effete and useless: moreover, I believe, it loses, very considerably, its elasticity, being, in a great measure, deprived of its more active matter. How similar, with some exception, is this, to the reasoning of Mayow. Were the experiments of Mayow, before publication, noised abroad, so as to suggest these ideas to Thruston; or did opinions, like these, beginning to prevail at that time, from observation, rouse the genius

of

of Mayow, to inftitute experiments to confirm the truth of them? Certain it is, that at this period, immediately, after Charles was reftored to the crown, the genius of medical men began to enquire, more minutely, into the fecrets of nature. Harvey had but juft immortalized his name, by the difcovery of the circulation; Sydenham was in all the glory of his medical career; and the different branches of medical fcience were enriched, by the elegant publications of Willis, Gliffon, Mayow, und innumerable others. When this noble, but alas! much abufed profeffion, was adorned by fuch able fupporters, every attention would be paid to the phænomena of the animal œconomy, and every theory would be brought forward, that induftry could examine, and abilities explain; and it appears, that a hiftory of the ftate of phyfic, during the laft century, would afford an interefting account to the medical reader.—— But to return.

The intromiffion of air, into the blood, by the lungs, according to Thrufton, was neceffary to its fluidity and heat. (M) The particles of the air, being mixed with the blood, conduce not

only

only to its fluidity, but alfo to the prefervation of its heat. The heat is owing to the nitrous particles of the air, which, he fays, he could eafily prove, by a variety of experiments, unlefs he ftudied brevity; and to explain its *modus operandi*, in producing animal heat, he has recourfe to fermentation. It was obferved, too, by him, that the blood acquired its florid colour, by its union with air; whence it is rendered more fit for the functions of the circulation. His theory had been taken notice of by fome, without mentioning his name; and he adds, it is with peculiar pleafure I find, that the experiments of Lower confirm my ideas. (N) It is our opinion, that this is the peculiar office of the lungs, *viz.* to tranfmit air into the blood, by which it acquires a red and florid colour; whence it happens, that it becomes not only more florid, but more fluid; and the more fluid it is, the more eafily will it be fitted for the functions of the circulation. Thofe two celebrated philofophers, Hook and Lower, feem to have placed this matter beyond a doubt, by a very ingenious experiment. And I do not a little rejoice, and return them my hearty thanks, that

they

they have produced a complete confirmation of my theory, which has been publicly attacked by ſeveral, without mentioning my name. It was an opinion, held by medical men, for ſome time after the diſcovery of the circulation, and till the nature of reſpiration became better underſtood, that ſanguification was performed by the heart alone. This is entirely rejected by Thruſton: the office of the heart, he ſays, is to circulate and diſtribute the blood, through all parts of the body, and with it, the vital heat. If we at all conſider the nature of ſanguification, and examine into the effects produced in the blood, we ſhall not fail to attribute them to the lungs. No phæ-nomena in the ſyſtem, ſince the late diſcoveries in chemiſtry, have excited greater attention, than the ſuſpenſion of reſpiration, in drowning, hang-ing, and ſuffocation. It is generally believed, that the cauſe of death, in theſe caſes, has been but lately diſcovered. Various have been the conjectures offered, and as various the publica-tions. It is evident, that unleſs medical men had a true idea of the function of reſpiration, no adequate explanation could be given, of the

cauſe

caufe of death, in hanging, drowning, and fuffocation.

Among the writers who have adorned phyfyology, by their publications on this fubject, that of the ingenious Mr. Coleman feems to have been conducted with moft accuracy.* This gentleman has fhewn, that irritability is more or lefs connected with animal heat, which the blood acquires in its paffage through the lungs; that the difference of irritability in the two ventricles, is owing, in drowning, hanging, &c. to the blood having loft its heat, in confequence of not undergoing the neceffary change in the collapfed lungs. It is moft affuredly true, that the blood in the right ventricle, during fufpended refpiration, muft poffefs more heat, than when it arrives at the left; becaufe it has traverfed through the lungs, where it muft have loft a confiderable quantity, to fupport the life of thofe organs. The proximate caufe of death, in the cafe of drowning, &c. according

* To do juftice, however, to Dr. Fothergill, of Bath, we muft fay, that from fome accidental circumftances, we have not read his work on this fubject; it is a pleafure we promife ourfelves very fhortly.

according to Mr. Coleman, is a want of latent heat in the blood, with obſtruction in the pulmonary veſſels, from the collapſed lungs. It appears to me, that want of the latent heat alone, is the proximate cauſe. I regard the want of latent heat, as an effect produced by the collapſed lungs. Mayow, and others, have long ago ſhewn, that fluids can paſs from the anterior to the poſterior cavities of the heart, when the lungs are collapſed. The only intention of nature, in diſtending the lungs, during reſpiration, is, that more points of contact may take place, between the blood and atmoſpheric air, for the abſorption of oxygen; and were it poſſible, that the ſame quantity of oxygen could be abſorbed by the lungs, when they are collapſed, as in the other caſe, I have no doubt, that there would be no occaſion for the alternate contraction and dilation of the thorax. I ſpeak abſtractedly of reſpiration, for every one allows the utility of preſſure, &c. in reſpiration, producing conſiderable effects, in promoting the flow of the fluids of the body.

The ſame idea, with reſpect to the cauſe of death, in drowning, &c. preſented itſelf to the

ingenious

ingenious Thrufton; and probably Mr. Coleman will be furprifed to find his theories and experiments, in part, anticipated, as far back as the year 1664. It occurred to Thrufton, that the death produced by hanging, drowning, and fuffocation, arofe from one and the fame caufe. Many and various experiments had fully proved, that in thefe cafes, air was prevented from uniting with the blood; hence its motion was ftopped, and life deftroyed. He had obferved, that the anterior cavity was always greatly diftended with blood; for from the fufpenfion of refpiration, there was nothing which could convey that principle to the blood, by which it acquired its heat, fluidity, and vitality. (o) Having premifed thus much, fays Thrufton, it is fcarcely neceffary to enquire, why animals die fo quickly, in hanging, drowning, and fuffocation: we muft repeat, that air being withheld from the blood, its motion ceafes, and life is deftroyed. That this is the cafe, is proved by a variety of obfervations and experiments. Harvey fays, that in a body juft hanged, he found the right auricle and lungs very

very much diftended and infarcted with blood*. Refpiration ceafing, there was nothing which could propel the blood into the left ventricle; nothing which could properly blend it together, and communicate to it vitality, fluidity and heat. Experiments were inftituted, to fhew how neceffary the p refence of air is to the vitality and motion of the blood. Hook had, before this, exhibited to the Royal Society, the experiment of affording and with-holding life, by the motion of a bellows in the trachea of a dog; and Profeffor Croon, at London, before the Prefident and Fellows of the College of Phyficians, reftored to life a ftrangled fowl, by blowing air into its lungs. Dr. Needham too, at Oxford, in the prefence of Boyle and others, recovered a dog, which had been hanged, by inflating the thoracic duct with air. (P) The *infpired* air, obferves Thrufton, united with the blood, and promoted its fluidity, by which means it reftored the circulation, which had ftagnated in the right ventricle, and in the lungs. He adds, that bleeding is often found of

H ufe,

* Epift. ad Riolanum.

ufe, in cafes of hanging, &c. and it could only produce its good effects, by relieving the right ventricle and lungs of their load of blood; hence the great ufe of that remedy, in fome cafes of difficult refpiration. The remarks which Thrufton has made on this fubject, are very judicious; and the coincidence, which had taken place, between his obfervations, and thofe of Mr. Coleman, is curious and worthy of notice. The heart has been found, fays this gentleman, to be loaded. If, therefore, from the right fide of the heart, while thus in a ftate of violent plethora, a fmall quantity of blood could be taken, experiment and obfervation tell, that its power and actions would be inftantly re-invigorated*.

Although it had been afferted and proved, by many, that the prefence of air, was abfolutely neceffary to the life of animals, yet feveral very able men denied, altogether, its union with the blood in the lungs†. It was urged, that no experiments had decidedly proved it; that the fame pores

* P. 163-4.

† Harvey, Highmore, Diemerbroeck, &c.

pores which gave entrance to the air, would ſuffer the blood to eſcape. Willis, however, imagined, that the air got acceſs to the blood, immediately before it paſſed from the artery into the vein, when it is changed from a dark to a florid colour*. They did not know, that air could unite with blood, through a medium as denſe as a bladder. Experiments had not been made to prove the abſorption of air by the blood, when Harvey and Highmore denied it; and, had Thruſton attended to the experiments of Mayow, he would have been able to have urged more powerful arguments, in favour of the abſorption of air. Lower too, ſoon after, as we ſhall preſently ſee, inconteſtibly proved, by his experiments on animals, the union of air with the blood in the lungs.

* Pharmaceut. Rat. P. 17. By the way, we may obſerve, that this ingenious phyſician maintains, that the uſe of reſpiration is to give vitality to the blood, by its union with air. He takes notice of the difference between arterial and venous blood; and ſays, that the blood, in paſſing through the lungs, to the pulmonary vein, is animated by the air; and adds, that this vein is exactly ſimilar to an artery, and that you may inject it from trunk to branch.

Notwithſtanding, however, that Mayow's experiments had decided, beyond the ſhadow of the doubt, that atmoſpheric air is compoſed of two kinds of air, the one the preſerver, the other the deſtroyer of animal life; and Lower had clearly ſhewn, by additional experiments, its abſorption by the lungs; yet we find an ingenious anatomiſt denying its union with the blood; ſo much does prejudice pervert the judgment. How air could be brought from the elaſtic, to the unelaſtic ſtate, was a difficulty, which could not be ſolved by the philoſophers of the laſt century; and they did not attempt to explain it; becauſe they never imagined ſuch a change. From different facts and obſervations, they could underſtand, how air might *mix* with the blood in the lungs; but beyond this, they could not extend their ideas, as the powerful and neceſſary aid of experiment was wanting. They ſaw the fœtus in utero, living and growing without reſpiration; they obſerved, that all the blood did not paſs through the fœtal lungs, and that nature, to ſupply this defect, had formed other channels, to tranſmit the blood from the right to the left ſide of the heart, *viz.* the

the foramen ovale, and canalis arteriofus. Hence they concluded, that as air could not arrive at the fœtus, and that as the temporary channels were obliterated in the adult, refpiration was of no other ufe, than to circulate the blood through the lungs. It was thus imagined, that the moft powerful argument againft the abforption of air by the lungs, was the event of refpiration in the fœtus in utero. (Q.) If air be fo neceffary to the adult, why fhould the fœtus be deprived of that privilege? But in the fœtus, no union of air can take place, becaufe it cannot breath in the uterus; yet in it, equally good and fine blood is prepared as in the adult who breathes.

Of this objection, Thrufton was aware; and although he mentions that the mutual heat and umbilical veffels fupply the place of the lungs and refpiration, yet he feems to lay hold of the idea, left he fhould be obliged to give up his theory, of the abforption of air by the lungs, and not as if he underftood the nature and office of the placenta. Had Diemerbroeck, with his ufual penetration, attended to the experiments and obfervations of Mayow and Lower, how foon would he have been convinced of his error? He would there have ſeen, that the placenta ſupplied the place of the lungs and reſpiration, by oxygenating the fœtus with the maternal blood. He only mentions Mayow once, and then in ſuch a manner as if he did not at all underſtand the ſublime truths he had diſcovered. Maurocordatus had publiſhed at Conſtantinople 'De Motu et uſu Pulmonum,' in which he denied, that reſpiration was of any uſe, except to tranſmit the blood from the right to the left cavities of the heart. This opinion, ſays Diemerbroeck, John Mayow has refuted by a celebrated experiment. The experiment, I ſuppoſe, for he does not quote it, of injecting a fluid into the pulmonary artery, and finding it in the left ventricle.

The difference between the colour of arterial and venous blood, formed a no leſs ſubject for ſpeculation, than the function of reſpiration. Hippocrates knew nothing of the diſtinction between artery and vein, and confounded both under the name of φλεψ. Eraſiſtratus, who had been invited by the Ptolemies into Ægypt, where he made ſome diſſections, was the firſt who diſtinguished veins from arteries, which he called smooth, in contradistinction to the aspera arteria. He imagined, however, that they contained air. Galen next asserted, that arteries were not air vessels, which he discovered by the obvious experiment, of passing ligatures round the vessels. He observed, that arterial blood was differently coloured from that of the veins, which, he said, was owing to the air, which mixed with it in the arteria venosa. He imagined, however, that it united with the air, by passing through the septum cordis. Columbus maintained, that the blood was mixed with air, in its passage through the lungs; and this was, also, the doctrine of Servetus, which he published in his book 'De Restitutione Christianismi.' These observations, however, that blood acquired its colour from the air, were made without actual experiment, and was a mere theoretical opinion. Neither was it adopted by physiologists in general: and the doctrine which obtained the most credit among medical men was, that the colour of the blood was owing to the heat it acquired in the heart; little imagining, that both the heat and colour are originally owing to one and the ſame cauſe. The obvious experiment of obſerving the blood in the pulmonary artery and vein, had not *directly* occurred to any one, till the middle of the ſeventeenth century. The doctrines which began to prevail at this time, reſpecting the laws of the animal œconomy, bear a ſtriking analogy to thoſe which are at preſent adopted. It was an age, indeed, remarkable for accuracy of obſervation and elucidation of facts. Certain it is, that at this period, opinions, ſuppoſed to be modern, were whiſpered in literary circles, and ſuggeſted experiments, upon which to build the foundation of rational ſyſtems. The prejudices of mankind, and a ſuperſtitious veneration for the dead, were now conſiderably obliterated, by the diffuſion of knowledge. The neceſſity of the ſtudy of anatomy, was perceived by men not connected with the profeſſion, and whoſe minds had been emancipated from the ſhackles of prejudice, by a liberal education: hence diſſections became more frequent; and in proportion as theſe prevailed, diſcoveries were made, which refuted or confirmed the theoretical opinions of their predeceſſors.

Among thofe who by their induftry and attention, acquired and merited their medical celebrity, was the ingenious Lower. This phyfician, by his accurate and well conducted experiments, convinced the unprejudiced mind, at once, that the change produced in the blood, was owing to the abforption of air. We are by no means, to imagine, that the blood derives its colour from the heat which it acquires in the heart. This opinion is fufficiently refuted, by obferving, that the colour is different in each ventricle; and fince their functions are fimilar, why fhould not the change take place, equally in one as in the other? for every one will allow, that blood, drawn from the pulmonary artery, is venous, in every refpect. (R) Experimental enquiry clearly proves to us, that the new rednefs is not acquired in the left ventricle; for if we infert a cork into the divided trachea of an animal, and draw blood from the cervical artery, immediately upon fuffocation, it will be completely venous, in every refpect, as if it had flowed from an opened jugular. However, that no room might be left for doubt upon the fubject, the following experiment was made on a dog,

deprived

deprived of all fenfation and motion by fuffocation, to prove the effect of air upon the blood, in its paffage through the lungs. (S) The lungs, which were perforated, were inflated by air blown into them; the event anfwered expectation completely; for the blood was received into a cup (from the pulmonary vein) equally as florid, as if it had been taken from the artery of a living animal.*

(T) The experiments which prove, that the blood acquires its red colour from aerial particles, is confirmed by the common obfervation, that the furface of venous blood becomes florid, when received into a cup, by being in contact with air. If this be removed by a knife, the next ftratum will acquire the fame colour, from a fimilar caufe. It is, therefore, very evident, that the bright red colour of the blood, is owing to the particles of air infinuating themfelves into it; the whole mafs is rendered florid in the lungs, becaufe in them, the

* The lungs were perforated, that they might be conftantly diftended, and that the air might have free egrefs. Lower ufes the terms *floridus*, *purpureus*, *rutilus*, & *coccineus*, indifcriminately.

the air being diffufed through all the particles of the blood, is intimately blended with it; and if blood be kept conftantly agitated, as it flows from a vein, it will become florid throughout. If you afk, fays Lower, in what manner the nitrous fpirit of the air paffes into the blood, do you fhew me, by what pores the fame nitrous fpirit of fnow, paffes through veffels and cools fluids*. (V) We may, therefore, rationally conclude, that the blood, in its paffage through the lungs, abforbs air, to which its red colour is altogether owing; afterwards, however, when the air has efcaped from the blood, into the *ftructure of the fyftem and parenchyma of the vifcera*, and has tranfpired through the pores of the body, it is perfectly agreeable with reafon to fay, that the venous blood, being deprived of it, appears, on that account, more dufky and black. Can any reafoning be more clofe or more accurately applied? or where fhall we find induction, from experiment, better underftood, or more clearly perceived? It is, however, remarkable, and rather fingular, that Lower no where

* This was a branch of the atomical philofophy, which confidered frigorific particles as the caufe of congelation.

where mentions his ideas, with reſpect to the *uſe* of the inſpired air. He contents himſelf with a ſimple relation of facts, and probably did not wiſh to hazard his reputation upon theory, particularly as there appears to have been a jealouſy entertained againſt Mayow, for the new doctrines which he had ſo boldly and ſo confidently aſſerted. It appears extraordinary, that Lower ſhould not have quoted Mayow's experiments and obſervations, in confirmation of his own opinion, that the blood loſt the florid colour it acquired in the lungs, in conſequence of the air being diſengaged from it in the ſyſtem, particularly too, when that opinion was very much doubted by phyſiologiſts of the firſt reſpectability.* Lower, however, throws out ſome obſervations upon the ſubject, which had long before partly occurred to medical men. (W) From obſerving the effects produced upon the blood, during reſpiration, we may eaſily conjecture,

* Lower's Treatiſe on the Heart was publiſhed ſeveral years before Mayow wrote; but the fourth edition of the Tractatus de Corde came out above ſix years after Mayow gave to the world his Experiments and Obſervations on Reſpiration.

conjecture, how much the health of the blood depends upon its union with the air, and of what consequence it is to respire it pure; and how greatly they are in the wrong, who deny this intercourse between the air and blood, without which it would be equally as possible to enjoy health in the filth of a prison, as in the pleasantness of country air. He adds this remarkable observation, which shews how gradually knowledge was stealing upon the mind,—for wherever a fire burns freely, there also we breathe with most ease. Little did Lower imagine, how intimately connected this observation was with the admirable experiments of the neglected Mayow, and how beautifully illustrated by them. Nothing can afford a more pleasing amusement to the rational mind, than to observe the growth of knowledge, in succeeding generations: and little praise is due to that man, whose mind would not grow warm from the contemplation of science, or whose enthusiasm would not encrease, from discovering the beauties of an author. Surely the mind must be deeply interested, when she observes the gradual display which genius makes in the advancement of know-

 ledge;

ledge; and when ſhe beholds the efforts of aſpiring abilities ſtumbling upon truth, but again falling from it, for the want of experiment.

(A) Τοσαύτη δε τυγχανέι πᾶσι χρειη τοῖσι σώμασι τȣ πνευματος ἐȣ̃σα, ὥςτε των μεν ἄλλων ἁπαντων ἀπεχόμενος ὁ ανθρωπος, και σιτίων και ποτων, δύναιτ᾽ ἂν ἡμέρας δυο και τρεῖς καὶ πλείονας διάγειν: ἢν δέ τις επιλάβοι τας τȣ πνεύματος εἰς το σῶμα διεξοδȣς, ἐν βραχεῖ μέρει ἡμέρης, ἀπόλλοιτο ἄν, ὡς μεγίςης χρείης εȣσης τω σωματι τȣ πνεύματος. De Flatibus.

(B) Imo certe conſtat, poſſe ſanguinem, ſine ipſorum motu pulmones pertranſire: etenim ſi ſanguis, aut liquor quivis alius, ope ſyringæ, in arteriam pulmonalem animalis mortui injiciatur, idem in ſiniſtrum cordis ventriculum prompté ſatis permeabit. Et utique quiſque in ſe experiri poteſt, quanquam ſuppreſsâ ad tempus reſpiratione, pulſum tamen arteriarum in carpis ſatis validum eſſe, quod vero non contingeret, niſi ſanguis, per pulmones, ad ſiniſtrum cordis ventriculum, tunc temporis tranſiret. P. 298.

(C) Nempe ſi ope follium aſperæ arteriæ adaptatorum,

adaptatorum, animalis cujusvis, veluti canis, infufflentur pulmones, ita tamen ut per foramina in eorum extremitatibus hinc inde facta, etiam exeat aliquid aeris, qui iterum per folles fupplendus eft, ne pulmones concidant, in hoc cafu vivet animal. Et tamen in pulmonibus incontinenter inflatis, iftius modi, fanguinis conquaffatio locum habere nequit; quin et pulmonum motu prorfus ceffante, fanguis, nihillominus, per eofdem, ad finiftrum cordis ventriculum tranfmittitur. Sin autem alias, infpiratione inftitutâ, fprituque in pulmones haufto, os et naris alicui obturentur, quamvis inflati maneant pulmones, mori tamen neceffe erit, quia non licet expirare. Et tamen, in hoc cafu, æque patet fanguini tranfitus per pulmones, ac in priore, neque enim in illo, major effe poteft fanguinis comminutio, cum in utroque cafu, pulmones fe pariter diftentos habeant; certo utique argumento, neque ad fanguinis tranfitum per pulmones, neque ad conquaffationem, adeo neceffariam effe refpirationem; ideo autem in priore cafu vivit animal, in pofteriore tamen moritur; quia in illo novus eft fubinde aeris recentis acceffus, in hoc vero nullus. P. 299-300.

(D) Nimirum

(D) Nimirum quemadmodum particulæ nitro-aereæ, terræ ſpiracula lente ſubeuntes, ibidem cum particulis ſalino-ſulphureis, iis verô immaturis, æſtu obſcuro congredientes, a quo vegetabilium vita dependet; ita particulæ eædem nitro-aereæ, magis confertim in cruoris maſſam pulmonum miniſterio introductæ, particuliſque ejus ſalino-ſulphureis, ad juſtum rigorem evectis, quoad minima admixtæ, fermentationem ſatis inſignem qualis ſc. ad vitam animalem requiſita eſt, efficiunt. P. 47. Cruoris maſſa a particulis ſalino-ſulphureis, ad juſtam volatilitatem evectis, conflatur, unde fit quod ei color atro-purpureus ſuppetat. Enimvero fermentationes rerum naturalium fere quaſcumque, a particularum nitro-aerearum motu procedere, et utique ſanguinis æſtum ab eâdem causâ provenire nullatenus dubito. P. 148.

(E) Si ſanguis in vaſe aliquandui ſervatus, in vitrum collocetur, ex quo aer per antliam aeream exhauritur, ſanguis iſte in ſuperficie, quà idem colorem floridum obtinuit, leniter efferveſcet et in bullulas aſſurget. Sin autem ſanguis arterioſus adhuc incaleſcens, in loco aere vacuo

poſitus

pofitus fuerit, idem mirum in modum expandetur et in bullulas pene infinitas elevabitur. P. 149. Annotandum eft hic loci quod ficut particulæ nitro-aereæ fanguinis fermentationem efficiunt, ita fermentatio ifta particulas nitro-aereas alias, ex aere excutit, elicitque, a quibus æftus in fanguine de novo inftauratur. Nimirum fpiritus nitro-aerei fanguinis maffæ in pulmonibus immixti, effervefcentiam fatis intenfam in eâdem excitant; iidem vero mox in corporis habitu maxima ex parte a fanguine in ufus infra dicendos fecernuntur; ita ut æftus cruoris ad pulmones reverfi, ob eorundem defectum multum imminutus et remiffius fiat; neque tamen fermentatio adeo penitus ceffat, quin eadem particulis nitro-aereis ex aere pro fui renovatione eliciendis valet. Atque ita demum in automato animali motus fermentationis perennatur. P. 150-1. Quemadmodum fanguinis fermentationem, ita etiam illius incalefcentiam a particulis nitro-aereis cum particulis cruoris falino-fulphureis exæftuantibus, oriri exiftimo. Etenim fi minera quævis falino-fulphurea, cujufmodi funt marchafitæ vitriolicæ aeri humido exponantur, eædem brevi æftum caloremque intenfum con-

 cipient;

cipient; in quantum fc. particulæ aereæ cum particulis mineræ falino-fulphureis congreffæ, effervefcentiam infignem excitant. P. 151. Quanto ergo major fanguinis æftus fervorque erit, qui particulis falino-fulphureis, rite evectis abundet et quibus particulæ aereæ confertim et quoad minima, pulmonum minifterio admifcentur? Quibus infuper addo, calorem iftum adeo intenfum, quo animalia in motum violentum concita afficiuntur, ex eo partim provenire, quod in motibus violentis refpirationem magnopere intendi neceffe fit; unde fit, ut particulæ nitro-aereæ majori copia in fanguinem introductæ, effervefcentiam, caloremque folito majorem efficiant: neque enim in motibus maxime violentis membrorum attritio tanta eft, quæ calorem adeo fervidum excitare poffit. Imo fi quis etiam in quiete aliquantifper intenfius refpiraverit, idem fe calore, folito majore, mox fuffufum effe fentiet. P. 152.

(F) Etenim fi cordis pulfatio a fanguine in finubus ejus exæftuante perficeretur, tunc pulfante corde, illius ventriculi a fanguine ifto maxime dilatarentur; haud aliter quam vefica in figuram maxime capacem inflatur. At vero ex vivi fec-

tionibus

tionibus conſtat, cordis ventriculos in pulſu ſuo contrahi et non a ſanguine rarefacto ſive exploſo dilatari, item ſanguinem e corde ſe contrahente, non vero reſoluto, proſilire. P. 302. Et ulterius adhuc conſtat motum cordis a ſanguine rarefacto non fieri, quippe corda nonnumquam execta, quanquam expreſſo ex ventriculis eorum ſanguine, nihilominus pulſare obſervantur. Enimvero ſi opii diſſolutio, aut aqua frigida per venam jugularem injiciantur, cordis motus ſtatim crebrior erit, ut ſæpe expertus ſum, at hoc propter crebiorem accenſionem fieri non poteſt. Plane ut cor nihil aliud eſſe videatur, quam muſculus, in actione ſua e cæteris parum diſcrepans. P. 303.

(G) Vita ni fallor, in ſpirituum animalium diſtributione conſiſtit, quibus ſupplendis cordis pulſatione ſanguiniſque ad cerebrum affluxu omnino opus eſt. Enimvero veriſimile eſt, ad quemvis muſculorum motum ſal hoc aereum omnino neceſſarium eſſe. P. 304. Etenim arbitrari fas ſit, ſubitam illam muſculorum contractionem a diverſi generis particulis invicem commixtis procedere. Particulas nitro-ſalinas ex aere inſpirato hauſtas particularum motivarum alterum genus conſtituere, quæ particu-

 lis

lis aliis ſalino-ſulphureis a ſanguinis maſſa ſuggeſtis, effervefcentiam eam excitant, a quâ muſcularis contractio procedit. Quapropter ſuppreſſâ reſpiratione, cum ſal illud aereum, ad motum quemvis requiſitum, deficiat, cordis pulſationem et conſequenter ſanguinis ad cerebrum affluxum interrumpi, mortemque ſequi neceſſe erit, 304-5. Et hic reſpirationis uſus ulterius confirmari poteſt. Etenim in exercitiis, motibuſque violentis intenſiore utique, et crebriore reſpiratione opus eſt; non tam ut major ſanguinis affluxus liberius per pulmones tranſeat; hoc enim ceſſante reſpiratione fieri poſſe oſtendimus; ſed quoniam per varias iſtas effervefcentias, in muſculorum contractione factas, maxima ſit ſalis nitro-aerei impenſa, ita ut ſanguis venoſus multum jam depauperatus et grumoſus ad cor redeat (uti etiam poſt motus convulſivos in epilepſiâ factos evenire certum eſt) quapropter ut ſanguis effætus diſpendia reſarciat, intenſiori reſpiratione omnino opus eſt. Præterea in motibus violentis ob uberiorem ſanguinis affluxum, cordis pulſationem citatiorem eſſe neceſſe eſt, quod ſine liberiore ſpiritus nitro-aerii acceſſu (præſertim ſanguine effoeto jam exiſtente)

istente) fieri vix potest. Plane ut præcipuus respirationis usus esse videatur, ut musculorum et præcipue cordis motus instituatur. Et ulterius adhuc, si postquam cordis motus ob suppressam respirationem cessaverit, aer pertubulum venæ adaptatum insuffletur, motum cordi ex postliminio restitutum videatur. Ut videatur, aerem esse illud, sine quo cordis motus institui plane nequit, neque multum referre, quâ ratione, utrum per pulmones, an aliâ quâvis viâ aer in sanguinis massam trajiciatur. P. 306-7.

(H) Quocirca, cum munera arteriarum umbilicalium, quæ hactenus assignata sunt, minus propria, veraque videantur, liceat, cum divino sene Hippocrate, statuere, umbilicum embryo respirationis vicem supplere; quæ etiam doctissimi Everardi sententia est. P. 318. Statuimus, sanguinem embryi per arterias umbilicales ad placentam sive carunculas uterinas delatum, non tantum succum nutritium sed una cum eodem particularum nitro-aerearum portiunculam commeatu suo ad fœtum devehere; plane ut sanguis infantuli, per circulationem suam in vasis umbilicalibus factam, eodem modo ac idem in vasis pulmonalibus, particulis nitro-aereis impregnari videatur. Proinde, ut placenta non amplius jecur, ſed potius pulmo-uterinus, nuncupanda ſit. P. 319-21.

(I) Enimvero veriſimile eſt, ſi ſanguis arteriosus, qui ſpiritu nitro-aereo imbutus eſt, loco venoſi ad cor accederet, nullâ omnino reſpiratione opus eſt. P. 321.

(K) Et hoc inde confirmari videtur, quod dum ſanguis arterioſus ex uno cane in alterum, noto jam experimento, tranſmittitur, canis in quem ſanguis transfertur, quanquam antea anhelus et intenſe reſpirans, ſanguine tamen arterioſo intus recepto vix omnino reſpirare videtur. P. 321.

(L) Veriſimile etiam eſſe, puriorem, ſubtiliorem, magiſque penetrabilem aeris partem, dum ille inſpirando pulmonibus hauritur, ibidem quaſi deperdi, adeoque aerem expiratum, utpote fuliginibus et vaporibus implicitum iniquinatum, orbatumque potiori parte ſua, vapidum quodammodo atque effœtum reddi; quinetiam (ut facile crediderim) eidem elaſticitatis ſuæ vim magna ex parte perire, nempe mobiliori illa materia plurimum ſpoliato. P. 34-5*.

(M) Porro

* De Reſpirationis Uſu primario Diatriba. Auctore Malachia Thruſton, M. D. Lugd. Batav. 1671.

(M) Porro particulæ aeris ſanguini commixtæ, non ad fluiditatem ſolum, verum etiam ad colorem ejus conſervandum non parum valent. P. 52.

(N) Tandem hoc pulmoni proprium ac peculiare eſſe volumus. Quod intromiſſis particulis aeris, colore coccineo, rutiloque ſanguinem donet; unde non floridior tantum, ſed etiam fluidior ut ſit, neceſſe eſt; quo autem fluidior eſt, eo facilius circuitu etiam ſuo defungitur. Celeberrimus Hookius et Lowerus, jam aliquoties laudatus, nobiliſſimo experimento, rem hanc difficillimam confeciſſe videntur, nec ſinunt nos diutius de eadem dubitare. Ego vero non parum lætor, illiſque gratias ago quod theoriæ meæ (a nonnullis publice ſed ſuppreſſo meo nomine impugnatæ) rem ipſam teſtem dederint. P. 128-9.

(O) Hoc præmiſſo, non opus erit ut quæramus, cur ſtrangulati tam ſubito morte pereant, ſive illi ſuſpendantur laqueo, ſive aquâ ſubmergantur, ſive catarrhis ſuffocentur? repetendum enim eſt, aeris uſu interdictis ſanguinem a motu ceſſare, proindeque et vitam tolli. Atque hanc rationem veram eſſe, multæ variæque obſervationes, et experimenta, teſtantur. Harveius (in Epiſt. ad Riolanum) ſe in cadavere humano, noviter ſtrangulato, auriculam cordis dextram et pulmones ſanguine plurimum diſtentos, atque infarctos reperiſſe teſtatur. Nempe ceſſante reſpiratione, nihil erat, quod ſanguinem e pulmonibus in ſiniſtrum cordis propelleret, nihil quod fluidum ſatis, calidumque, ac ſpirituoſum, adeoque rite commixtum et rutilum prœſtaret. P. 61-2.

(P) Nimirum inſpiratus aer et ſanguini commiſcebatur, ejuſque fluiditatem promovebat, ac totum ejus flumen, quod jam in dextro cordis ventriculo, atque in pulmonibus ſtagnarat, forti impulſu ad progreſſum et circulationem ſollicitabat. P. 63-4.

(Q) At in fœtu nihil aeris admiſceri poteſt, cum in utero non reſpiret; et tamen in eo, non minus bonus ac ſpirituoſus ſanguis conficitur, in quam homine nato et reſpirante. P. 455*.

(R) Quinimo nec a ſiniſtro cordis ventriculo, novum hunc ruborem ſanguini impertiri, certiſſimo hoc experimento confici poteſt; ſi nimirum aſpera

* Iſbrandi De Diemerbroeck Medicinæ et Anatomes Profeſſoris Anatome Corporis Humani. Genevæ. 1679.

aſpera arteria in collo nudata diſcindatur, et immiſſo ſubere arctè deſuper ligetur, ne quid aeris in pulmones ingrediatur, ſanguis ex arteria cervicali ſimul diſciſsâ effluens (ſaltem qui aliquandiu poſt præfocatum pulmonem erumpit) totus venoſus pariter, et atri coloris apparebit, non aliter quam ſi vena jugulari pertuſa profuſus fuiſſet. Hoc ego ſæpius expertus ſum. P. 119*.

(S) Itaque propulſo ſanguine atque inſufflatis ſimul, necnon perforatis pulmonibus; expectationi eventus optime reſpondebat; quippe æque purpureus in patinam excipiebatur, ac ſi ex arteria viventis effuſus fuiſſet. P. 119.

(T) Præterea colorem hunc rutilum particulis aeris ſeſe in ſanguinem inſinuantibus omnino deberi, ex eo ſatis perſpicuum eſt; quod ſicut in pulmonibus per totum floridus redditur, quia in illis aer per omnes ſanguinis particulas diffuſus, cum ipſo intimius permiſcetur, ita ſanguinis venoſi in vaſe excepti ſuperficies, et pars ſumma, quatenus

* Tractatus de Corde, item de Motu et Colore Sanguinis et Chyli in eum tranſitu. Authore Ricardo Lower, M. D. Coll. Med. Lond. Socio. Editio quarta. Londini, 1680.

quatenus aeri expofita fit, coccineum quoque colorem acquirit; quæ fi cultello auferatur, proxima quæ fubjacet a fimili aeris contactu, in eundem brevi mutabitur. P. 121.

(V) Quare fanguinem in fuo per pulmones tranfitu aerem haurire, ejufque admixtioni floridum fuum colorem omnino debere, maxime verifimile eft; poftquam autem in habitu corporis et vifcerum parenchymatis aer rurfus a fanguine magnâ ex parte avolavit, atque per poros corporis tranfpiravit, fanguinem venofum illo privatum obfcuriorem et nigriorem illico apparere, rationi pariter confentaneum eft. P. 122.

(W) Ex quo conjicere facilè eft, quantum fanguini beneficium ab admixto aere accedat, quantumque interfit, eum falubrem femper et ferenum haurire; quantumque aberrent illi, qui aeris hoc cum fanguine commercium omnino intercedant; abfque quo fieret, ut poffet aliquis non minus falubriter verfari in fætore carceris, quam inter ameniffima vireta; ubicumque fc. ignis fat commode ardere poteft, ibi et nos æque commode refpirare. P. 123.

SECT.

SECT. VIII.

MAYOW'S *Theory of* RESPIRATION *adopted by* WOLFERSTAN *and* VERHEYEN.

When the myſtery of reſpiration began to be unfolded by men of abilities, the truths which every where adorn the works of Mayow, ſoon caught the attention. Obſervations were made, which the neglected experiments of Mayow had already confirmed, and opinions ſtarted, which his doctrine had long before beautifully illuſtrated. The ſtudy of air, too, became a favourite purſuit, and branched out into multiplied experiments, in order to account for the various phænomena, which accompany the important function of reſpiration. As Mayow, from his laborious and ingenious experiments, had explained this ſubject in the moſt accurate and ſatisfactory manner, it was natural, when truth aſſerted her dominion, to bring forward him whoſe labours moſt certainly

 entitled

entitled him to credit and reſpect. In a book which we accidentally met with, the author adopts the theory of Mayow, and explains himſelf, very clearly, on the ſubject*. He rejects, as abſurd, the idea of fermentation in the heart; and ſays, that the chyle coming to the right ventricle, is more intimately blended with the blood in the diaſtole; yet they ſtay not to make that buſtle ſome have thought, but by the ſyſtole, or contraction of the heart, are ſoon driven out again, the chyle accompanying the blood, to the greateſt amongſt all the the viſcera for *ſanguification, I mean the lungs*, where they both receive a *florid red*, and by the admiſſion of *new particles*, are more perfectly aſſimilated, and united with each other†: and he ſays, he very much queſtions, whether

* An Enquiry into the Cauſes of Diſeaſes, in general, and the Diſturbances of the Humours in Man's Body, wherein the Nature of the Blood, of the Air, and of a peſtilential Conſtitution, are briefly conſidered, with ſome Obſervations on the Venom of Vipers, by Stanford Wolferſtan, M. A. London, 1692. I find no account of this author in Wood's Athenæ, neither is he mentioned by Morhof.

† P. 7.

whether any thing elfe is left in a healthy air, to join with the blood, but the aerial falt.

His obfervations, on the ufe of the aerial falt, are very ftriking. It is apparent, he obferves, that the air does enter the veficulary cells, and there, whether by adhefion or diffolution, does depofite, at leaft, *fome part* of this aerial falt, where meeting with the occurring humours, it is directly carried to the heart, and by its pungent particles, molefting and irritating the ftreight and oblique fibres of the heart, does ftir them up into a fyftole, or contraction, and fo, by ftreightening its ventricles, to drive forth the blood contained in them, in the arteries. And *this I take to be the true original of all pulfation, and firft motion of the heart*, and circulation of the blood throughout the body.* He fays, he cannot fuppofe, that the aerial falt enters the pores of the body: neither, when I obferve the difference between the florid, *aerial* coloured blood, returning from the lungs, and the atro-purpureal blood contained in the veins, can I believe this aerial falt does enter by the

* P. 24.

the pores; or is mixed with the venal blood, when I ſee the venal blood both changed in colour and conſiſtence, from what I either find in the lungs or arteries.* He maintains, that the effects of the aerial ſalt on the ſyſtem is evident, in many caſes: it produces hunger, by entering the ſtomach and lungs; hence the appetite in taking the freſh air: that it is highly uſeful to the motion of the ſtomach:† it prevents too the coagulation of the blood, and promotes the ſecretion in our bodies, and facilitates the ſeparation and expulſion of almoſt every humour. The reader will recollect, that this was alſo the opinion of Thruſton, who maintained, that the inſpired air gave fluidity to the blood.

That oxygen renders the blood more fluid, by its union with it, we think is evident, from the following

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* P. 28.

† My very worthy and ingenious friend, Dr. Ingen-Houſz, found by his eudiometer, that there was leſs oxygen in the marſhy lands of Holland, than at Vienna; and he attributes the *keenneſs* of *appetite*, and *quickneſs* of *digeſtion*, in the latter place, to this circumſtance. Mayow alſo maintains, that the fire-air particles are eſſential to digeſtion.

following experiment, which we made. After a frog had remained ſeveral hours in nitrogen gas, he was taken out, and barely ſhewed ſigns of ſenſation, when pinched. He was now opened; the heart was turgid with dark blood, and did not act, except when pricked. In this ſtate, he was placed in a glaſs jar, containing oxygen gas. The heart began inſtantly to act, and the blood became very quickly florid, through all the veſſels. I obſerved, when opening him, that the blood in the veſſels, particularly in a large one, which runs along the internal ſurface of the abdominal muſcles, was dark, firm, and coagulated, and did not flow, but only left a ſpotted mark on the ſciſſars, where they divided the veſſel; as ſoon as it became florid, from the admiſſion of oxygen, it flowed freely through all the veſſels, and in conſiderable quantity, giving a bright red colour to the ſurface of the water.* It is far from us to build theory or practice upon diſtorted facts; ſuch

* Experiments ſimilar to this, the Royal Society have done us the honour to receive. We have there proved, that oxygen is not the principle of, but ſtimulus to irritability, contrary to the opinion of Girtanner and others.

ſuch is the experiment, and the reader is at liberty, to draw what concluſions he pleaſes from it. Theſe ſuggeſtions, however, of Thruſton, and Wolferſtan, confirmed by our experiment, hold out the hopes of reſolving tumours, by the inhalation of oxygen, and accordingly this idea has been put to the teſt of experiment; and the tumours I have ſeen reduced by this remedy, affords the pleaſing conſolation, that when obſervations ſhall have been matured by further experimental enquiry, caſes of this kind will be as little dreaded by the practitioner, as the moſt trifling: and it gives me pleaſure, that the knowledge, on this ſubject, daily increaſes; for among the many caſes which come under the care of Dr. Thornton, he often meets with glandular ſwellings, particularly about the neck, which yield ſurpriſingly to the inhalation of vital air.

After having enumerated theſe effects of the aerial ſalt, Wolferſtan forms a theory, which few will imagine was entertained by ſo old a writer. He ſays, that the plague, and peſtilential fevers, muſt ariſe from either a partial defect, or partial contamination, or depravation of this ſalt. When

reaſoning

reaſoning on the partial want of the nitro-aerial particles, he adds the following remarkable obſervations; Yet even in theſe partial defects, when only one part of one lobe of the lungs is ulcerated, yet in how ſhort a time is the motion, colour, and conſiſtency of the blood, changed from what it was? *

In caſes of peſtilential fevers, for the want of this combining ſalt, the particles of our blood are diſlocated, its craſis made lax, and prepared very ready to receive and combine with this aerial depravation†. The reader will, probably, here recollect, the reaſoning that has been applied, in the cure of typhus, by oxygen. The analogy pointed out between the blood, in the ſecondary ſtage of fever, and ſea ſcurvy, is very ſtriking. The ſame idea preſented itſelf to the penetrating mind of

K Wolferſtan,

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* P. 45-6. † P. 48. Wolferſtan ſeems to think, that only a *certain* quantity of nitro-aerial particles preſerves the proper, or if I may ſay, the vital conſiſtence of the blood. That different ſtates of aggregation, in the component parts of the blood, takes place, to a certain extent, during life, I believe is allowed; but it would be deſirable to know, how far oxygen is concerned in ſuch alterations.

Wolferſtan, when he obſerved, that the craſis of the blood was lax, for the want of the combining ſalt. Some of the moſt curious paſſages, in this valuable little treatiſe, relate, if I may apply the expreſſion to ſo early an author, to the de-oxygenation of the blood. He applies Mayow's theory, to account for the effects produced by the bite of the adder. He obſerves, that the different parts of adders produced no bad effects, when eaten by dogs, cats, &c. but when a living creature was bit, the place always ſwelled, looked black, and the blood ſeemed to coagulate.* Not being able to find, by tedious diſſection, a ſtructure for the ſecretion of poiſon, nor

* P. 62. Baglivi gives a ſimilar account of the bite of the tarantula. Demorſa pars quandoque ſtupore inſigni prehenditur *circulo livido, nigro*, aut *ſubflavo ſtatim* coloratur. And an obſervation which he adds is ſo remarkable and curious, that I muſt quote it. Generaliter loquendo ſymptomata tarantalorum, primo veneni inſultu ſimilia ſunt iis, quæ in malignâ febre ex coagulatione obſervari ſolent, qualia ſunt angor cordis ſpirandi difficultas, omnimodo fere pulſus oppreſſio, animalium vitaliumque actionum repentina, ac pene fatalis jactura. Diſſert. de Anat. &c. et Tarant. P. 23-5. How ſimilar this, to the deſcription of ſcurvy!

nor the poiſon itſelf; but above all, he ſays, having obſerved, that during biting, the adder kept its jaws ſo exactly cloſe, that no breath or water could paſs out, and that the animal, at the time, made an expiration, and that no ill effects were produced from the bite, if he prevented the animal from making an expiration; and after having quoted Mayow, to ſhew, that expired air is no longer fit for animal life, he concludes, that it is *the introduction of this unwholeſome breath into the ſyſtem, which produces the conſequent evils.** The ingenious Wolferſtan, draws another concluſion from theſe facts, to explain the ſymptoms attending peſtilential fevers. 'Tis as eaſy to conceive, he adds, as in the adder, how the ſame deficiency is made in the air we breathe in, and what effect that deficiency will have upon the humours of our body.†

It is ſuppoſed now, by ſome, that various poiſons act by de-oxygenating the ſyſtem. May not many animals poiſon, by introducing into the ſyſtem, expired air? It is known, from experi-

 ments,

* P. 70. † P. 74.

ments, that carbonic acid and azotic air, injected into the blood veſſels of animals, produce a dark colour of the blood, and ſpeedily deſtroy life.* It is found, alſo, that the preſence of the deleterious gaſes, to any conſiderable extent, more or leſs prevents the union of oxygen with different ſubſtances. Hence, probably, the reaſon, why ſo ſmall a quantity, introduced into the circulating ſyſtem, produces death. The celebrated Italian experimentaliſt, Fontana, has ſhewn, that the venom of the viper, mixed with the blood of a fowl, rendered it black, and preſerved it in that ſtate, notwithſtanding its expoſition to atmoſpheric air; and we know, that under this latter circumſtance, its ſurface ſhould have acquired a florid colour. Hence the venomous influence, prevented the abſorption and uſual agency of oxygen.

The doctrines of Mayow, thus elucidated, in ſome degree, by Wolferſtan, began now to make their way on the Continent, and to acquire further able ſupporters. The clearneſs and preciſion, with which they pointed out the neceſſity of

* Girtanner.

of the preſence of an aerial fluid, to the vitality of the blood, demonſtrated, that reſpiration was not intended for thoſe futile purpoſes, to which it had been hitherto applied. Some of the beſt ideas on this ſubject, after Mayow, were thoſe publiſhed by Verheyen. We cannot avoid mentioning ſome remarkable obſervations of this author, as they clearly ſhew, that he perfectly underſtood this important function; particularly, too, as he has attended, with more accuracy, than any one, to Mayow's experiments, and has altogether adopted the reaſoning and doctrine founded upon them. He rejects, with Mayow, that reſpiration is to cool the blood, or to tranſmit it through the lungs; other more important uſes muſt be ſought for. (A) From conſidering, with attention and accuracy, every circumſtance, it appears, that reſpiration is performed, for the expreſs purpoſe of continually ſupplying air to the ſyſtem, to keep up the motion of the blood; for it is evident, that our life and health altogether depend upon this cauſe. Now, when we obſerve, that this motion fails, when reſpiration is impeded, it is rational to conclude, that ſomething is ſupplied from the air, through the medium of refpiration; more efpecially, too, when we recollect, that the blood acquires its beautiful crimfon colour in the lungs. The blood, however, he remarked, loft this colour, in its circulation through the fyftem, for it is obferved to be dark in the veins. (B) Therefore, fince the veins receive the blood from the arteries, it follows, that its bright red colour is loft, either in the capillary branches, or in its paffage from the arteries to the veins; and fince the bright crimfon colour is reftored to the blood, after it has circulated through each ventricle, and the intermediate lungs, we muft conclude, that the colour originally loft, is reftored to it, during its paffage from one ventricle to the other. Here the experiments of Lower are quoted, in confirmation of his opinion, and he makes the following curious remarks. (C) The laft portion of blood drawn, during venæfection, has very frequently the appearance of being arterial. It is owing to this caufe; as the blood in the veins flows flowly, it oppofes fome refiftance to the circulation of the arterial blood, which, when moderate, is of ufe, for the fafe and eafy fecretion of various

principles,

principles, from the maſs of blood; but during the flow of blood from an opened vein, that reſiſtance being removed, the arterial blood circulates more freely, and therefore does not depoſit, in ſuch quantity, thoſe principles, which it otherwiſe would have done, and *among which we muſt take in the matter ſupplied by the air.*

Verheyen was not ſatisfied with aſſerting, that air entered the blood, during reſpiration; like Mayow, he muſt enqire what that aerial matter is, which is ſo neceſſary to life. Here he quotes this author, and adopts his opinion, that the nitro-aerial particles is the matter ſupplied. He agrees with him, too, in thinking, that that part of the air is copiouſly condenſed in nitre; hence it is, that combuſtible matter readily burns in vacuo, when combined with that ſalt. (D) I could adduce, adds Verheyen, many more arguments, drawn both from reaſon and experience, in favor of the opinion, that ſuch an aerial ſupply is acquired by reſpiration; but the want of time, and a wiſh to conſult the convenience of the reader, prevent me: if any one, therefore, is deſirous of more knowledge, on this ſubject, he may conſult the treatiſe, which the ingenious Mayow has publiſhed, on nitre and the nitro-aerial ſpirit. Verheyen goes on to prove, that the nitro-aerial particles are taken into the ſyſtem, for the purpoſe of producing heat and motion, which are cauſed by the mutual union of the nitro-aerial and ſulphureous particles. We now know the reaſon, he ſays, why reſpiration is ſo much quickened by exerciſe, and why animals die in the vacuum of an air pump; for together with the air, are exhauſted thoſe particles ſo neceſſary to life. (E) Indeed it is very probable, that together with the air, are included other particles, of a more ſubtile nature, differing very much in their nature and properties. And it is certain, that animals die in vacuo, on that account, becauſe the flame and ignition of coal, a candle, and ſuch like ſubſtances, confined in the glaſs, are immediately extinguiſhed, on the exhauſtion of the air, if we are to credit the experiments of Boyle; but gun-powder, which beſides ſulphureous, contains nitrous matter, burns and flames almoſt equally as well as in the open air.

This author, by attending to the experiments

of

of Boyle and Mayow, acquired a perfect knowledge of the function of respiration; and his ideas on the motion and heat of the blood, are no less accurate. These effects, he says, are produced, by the union of the particles, taken in by the lungs, with the phlogistic matter, which is conveyed to the system with our food.

SECT. IX.

MAYOW'S APPLICATION *of* FIRE-AIR *to* VEGETATION.

The new difcoveries in chemiftry, have thrown as much light on the œconomy of plants, as on that of animals. It is now evident, that the food of vegetables is chiefly air. The ingenious Prieftley, the world well knows, inftituted a variety of experiments, to difcover what air was beft adapted to the food of plants; and at laft determined, that plants grew the moft vigoroufly, in noxious and putrid airs. All the noxious airs, however, were not equally favourable to vegetation; nitrous air, he obferved, prefently deftroyed it. The experiments of Prieftley difcover much contradiction; in fome cafes, the plants thrived perfectly well in putrid air, but in others they died.* Dr. Prieftley alfo found, that plants have a power of correcting bad air; but did not know the condition, neceffary to

* Vide Vol. I. for 1781, p. 86.

to make plants perform ſuch an office; for both he and Mr. Scheele obſerved, that the growth of plants very frequently injured the air. Theſe contradictions could not be accounted for, by the diligent enquiries either of Prieſtley, of Senebier, or of Scheele, till my accurate and learned friend, Dr. Ingen-Houſz, clearly pointed out the error. He diſcovered, that not only the ſame plant did not, at all times, give out the ſame kind of air, but alſo that different parts of the ſame plants, afforded different kinds of air; and that theſe variations were owing to the influence of the ſun's light upon vegetation. His experiments too, led him to draw very different concluſions from thoſe of Prieſtley. This philoſopher imagined, from his experiments, that oxygen was invariably injurious to vegetable life. This opinion, however, Dr. Ingen-Houſz has refuted, and maintains, that plants ſhut up in vital air, live ſo much the longer, as the air is ſuperior, in purity, to atmoſtpheric air.* The uſe of oxygen is, to form

* Eſſay on the Food of Plants, p. 2. Is it not probable, that the food of different plants vary, like that of animals?

form with carbon, fixed air, the chief food of plants. Being perſuaded of this, from his experiments, he concluded, that the ſoil was continually engaged, in forming carbonic acid, for the nouriſhment of vegetables; and that, for this purpoſe, it muſt abſorb oxygen from the atmoſphere, being already ſupplied, by a variety of ſubſtances, with carbon: hence the neceſſity of fallow. To prove this, he made the following experiments; He expoſed eight cubic inches of good mouldy ground, to eighteen of atmoſpheric air, for three days, but conſtantly hid from the ſun's light, by covering the apparatus with a flower-pot, and found the air ſo much deprived of oxygen, that a wax taper could not burn well in it; and when the experiment was made in the light, the remaining air was almoſt pure azot.*

This,

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animals? Thoſe which grow in marſhy grounds, it is probable, have their food principally compoſed of hydro-carbonate, which is diſengaged, in conſiderable quantity, in ſuch ſituations. Dr. Prieſtley found, that the willow thrived the beſt of all in putrid air; and this plant, we know, conſtantly grows in marſhy ſoils.

* Ibid. p. 16-17.

This, ſurely, has let in conſiderable light upon agriculture, and ſhews the great influence of oxygen, in the wide extent of the creation. This uſe of fire-air, did not eſcape the penetration of the ingenious Mayow. We have ſeen, how admirably he has applied this air, to the purpoſes of the animal body; his explanation of the vegetable proceſs, upon the ſame principle, is no leſs curious and remarkable. He ſuppoſed, however, that nitre formed the chief nouriſhment of vegetables; but the manner in which he has explained it, diſcovers how accurately he marked the agency of oxygen, in the vegetable kingdom. It was his opinion, that the fire-air particles united with various ſubſtances in the ſoil, but chiefly with the alkaline baſe of nitre.

(F) The nitro-aerial ſpirit ſlowly and gradually effervefcing with the earthy matter, varies its ſulphureous part to a proper volatility; and moreover unites with the ſaline part into nitre; the elementary principles are formed in ſuch a manner, as to promote the growth of vegetables. It will be evident, that this is the caſe, if we conſider at what ſeaſon of the year, chiefly, the nitro-

aerial

aerial ſpirit effervеſces with the earthy matter, and forms nitre. Now, we know, that nitre is produced, in greater quantity, in the ſpring, than at any other time: for, in the winter, the nitro-aerial particles and earthy ſulphur, are bound by the froſt; but on the approach of ſpring, the nitro-aerial ſpirit is brought into motion, by the warmth of the ſun, and the ſoil is opened, the froſty weather diſappearing: hence the nitro-aerial ſpirit, being now more active, deſcends deep into the earth, and there meeting with ſalino-ſulphureous particles, produces a violent effervеſcence, whence nitre is copiouſly generated, and vegetation comes on rapidly.* That oxygen unites with other matters beſides carbon, to form the food of plants, is evident, from a variety of circumſtances. The many heterogenious ſubſtances, which enter into the compoſition of common mould, muſt contain various elementary principles, ready to

* Vide Appendix, No. IV. and V. where Dr. Ingen-Houſz has given a full account of his diſcovery, that plants give out oxygen, by the influence of the ſun's light, and where the coincidence between his opinion and that of Mayow is further mentioned.

to unite with oxygen, in various propotions, according to the degree of temperature. It is needlefs to particularize any one bafe, with which it will unite, as the reader can fupply many from his own obfervation. It is evident, that it will form acids, and then neutralize falts, *ad infinitum*, according to the fubftances it meets with : and in a letter to Dr. Ingen-Houfz, we have mentioned it as probable, that the great quantity of oxygen abforbed by the foil, contributes to form thofe vaft refervoirs of water, found in the bowels of the earth.

The neceffity of air, then, to the growth of plants, has been fully proved, by Hales, Prieftley, Ingen-Houfz, and Scheele. A plant fhut *in vacuo*, foon dies, fays Dr. Ingen-Houfz; and it is probable, obferves Hales, that the air freely enters plants, not only with the principle fund of nourifhment by the roots, but alfo through the furface of their trunks and leaves, efpecially at night, when they are changed from a perfpiring to a ftrongly imbibing ftate.* Dr. Ingen-Houfz, too,

* Vegetable Statics, Vol. I. p. 59. The abforption of air, by plants, was proved by a very beautiful experiment. P. 155. Expert. 47.

too, has not only proved, that plants can exist altogether upon air, by being hung up to the ceiling of a hot-house, but has also shewn, as we have seen, that the aerial fluid is so necessary to their existence, that it is absorbed in great quantities by the soil, for the purposes of nourishment. It would appear, from the positive manner in which Mayow expresses himself, that he had made experiments to prove the very same thing. (G) So necessary indeed, he observes, is this aerial salt to every kind of life, that not even plants can vegetate in earth, from which the access of air is precluded; but if this earth be exposed to the air, it becomes impregnated with this fœcundating salt, and is again rendered fit for the nourishment of vegetables: which proves, that the very plants have a kind of respiration, and are under the necessity of imbibing the air.

(A) Itaque omnia rem istam concernentia exacte perpendens, existimo, respirationem eo potissimum collimare, ut quædam materia sanguini, pro motus illius continuatione jugiter ex aere subministretur;

miniſtretur; enimvero evidenter conſtat, vitam noſtram, adeoque et ſanitatem, motu ſanguinis omnino dependere. Cumque videamus hunc motum, ob defectum reſpirationis ſtatim deficere, non inepte concludimus, ejuſmodi materiam ex aere, mediante reſpiratione, ſuppeditari, maxime ſi attendamus, ſanguinem ſplendorem ſuum, et intenſum ruborem, præcipue nanciſci in pulmonibus. Vol. II. P. 326*.

(B) Itaque cum venæ recipiant ſanguinem ex arteriis, ſequitur, colorem ejus intenſe rubrum deperdi, vel in ramis capillaribus, vel in tranſitu ex arteriis in venas; et cum idem ille ſanguis fuſcus, poſtquam utrumque cordis ventriculum et pulmones intermedios peragravit, redeat totus rutilans et coccineus, conſequens eſt, colorem illum ante deperditum ipſi in tranſitu eſſe reſtitutum. P. 327.

(C) Pro quo notandum, quod ſanguis in venis tarde fluens, aliquatenus reſiſtat tranſitui ſanguinis adventantis per arterias, quæ reſiſtentia

L moderata,

* Corporis Humani Anatomia, autore Philippo Verheyen, in Univerſitate Lovanienſi Art. et Med. Doct. et Profeſſore. Amſtelodami et Lipſiæ. 1731.

moderata, utilis eſt, ut ea, quæ a ſanguinis maſſa ſeparari debent, facilius feliciuſque ſeparentur: at vero dum aperta vena ſanguis effluit, ſublata dicta reſiſtentia, ſanguis arterioſus liberius ſubſequitur, adeoque minus deponit illa, quæ alias fuiſſent deponenda, inter quæ eſt ipſa materia ex aere deſumpta P. 329.

(D) Poſſum, pro eadem reſpirationis materia, ejuſque neceſſitate, afferre plura argumenta, tam ab experientia quam a ratione petita; ſed temporis anguſtiæ et lectoris commoditati conſulendum duxi: ſi quis ulteriora deſideret, legere poterit tractatum, quem prælaudatus D. Mayow, de ſale nitro et ſpiritu nitro-aereo, conſcripſit. P. 331-2.

(E) Etenim maxime veriſimile eſt, inter particulas aere incluſas, alias aliis eſſe ſubtiliores, atque inter ſe natura et indole omnino diverſas. Et certe animalia in loco aerem non continente, ob eam rationem interire, inde confirmatur, quod ignis carbonum, candelæ, et ſimilium, magno vitro incluſus (ſi experimentis Boyleanis fidendum eſt) extracto aere confeſtim extinguatur: pulvis tamen pyreus, qui præter ſulphur materia nitroſa abundat, fere æque ac in libero aere accendatur, ardeatque. P. 339.

 Spiritus

(F) Spiritus nitro-aereus, cum materia terrestri motu obscuro effervescens, partem ejus sulphuream ad justam volatilitatem perducit; et insuper cum parte ejus salina in nitrum coalescit, principia verum naturalium in tali constituuntur, qualis ad vegetabilia formanda requiritur. P. 52. Rem modo prædicto, in vegetabilium ortu se habere, indicio est: quod quo tempore vegetabilia præcipue a terrâ oriuntur, spiritus nitro-aereus cum materia terrestri maxime exæstuat, et salnitrum præcipue in terra generatur: quippe experientia constat, nitrum majori copia ineunte vere, quam reliquis anni temporibus in terra nasci. Nimirum tempore hyberno, particulæ nitro-aereæ et sulphur terrestre glacie constipantur; ineunte autem vere, spiritus nitro-aereus, æstu solis intensiori in motum concitatur, terræque compages, glacie jam liquefacto reseratur: hinc spiritus nitro-aereus, in motu positus, in altam tellurem descendit, ibidemque cum particulis ejus salino-sulphureis minutim perfractis congressus, effervescentiam satis intensam efficit; unde nitrum copiose generatur, et vegetabilia confertim excrescunt. P. 52-3.

(G) Adeo enim ad vitam quamcunque ſal iſtoc aereum neceſſarium eſt, ut ne plantæ quidem in terra, ad quam aeri acceſſus præcluditur, vegetari poſſint; ſin autem terra iſta aeri expoſita, ſale hoc fæcundante denuo impregnetur, ea demum plantis alendis iterum idonea evadet. Plane ut vel ipſæ plantæ aliqualem reſpirationem, aeriſque hauriendi neceſſitatem habere, videantur. P. 52-3.

SECT. X.

Of Muscular Motion.

The beautiful discoveries of the ingenious Mayow, enabled him to extend his theories and observations, to several functions of the animal œconomy. To a man who contemplates, with serious attention, the structure of the human frame, the cause of muscular motion must present many perplexing ideas. A self-moving animal, is a machine of wonder to the eye of the beholder; and nothing but custom, has familiarized our minds to such objects. That an animal should become loco-motive by volition, or a simple exertion of will, has afforded matter for speculation and dispute, in almost every age.* The laborious

 anatomist

* Does there appear any principle, in all nature, more mysterious, than the union of soul and body, by which the spiritual part possesses such an influence over the material, that it is able to direct the motion of any, or even sometimes a part of a muscle? *Hume.*

anatomiſt could not diſcover the cauſe in tedious diſſection; and it was equally concealed from the reaſoning of the ſpeculative theoriſt. It was little imagined, that reſpiration had any connection with muſcular motion, or that a conſtituent part of the atmoſphere, at all aſſiſted in this extraordinary function. When the mind was fatigued with reaſoning, and nothing ſatisfactory appeared, it is curious to obſerve, that it was attributed to a *cauſe*, originally impreſt upon the works of the creation. As it has been proved, that all matter is in a ſtate of perpetual motion, originally impreſt upon it by nature, it ſeems moſt agreeable to the analogy of nature, to refer muſcular motion to an original law of animated matter, for which no cauſe can be aſſigned, any more than for gravitation, cohesion, or chemical affinity.* It is with diffidence, we diſſent from ſo high an authority as that of Dr. Blane. The univerſality of motion is evident to all; but ſuch an explanation of muſcular action, is equally applicable to dead as to living matter. The Deity has, no doubt, impreſſed certain determinate

* A Lecture on Muſcular Motion, read at the Royal Society, by Gilbert Blane, M. D. F.R.S.

determinate and immutable laws on inanimate matter; but if that were the caſe with the animated creation, to what purpoſe has he given the living principle? That the muſcular fibre is every way fitted for that motion, to which it is applied, is a queſtion which no one can deny; but that the cauſe of this motion is ſimilar to that of gravitation, coheſion, or chemical affinity, is a poſition, which an acquaintance with the animal œconomy entirely rejects, as unfounded.

The difficulty of this ſubject, did not deter the boldneſs of Mayow's genius to attempt an explanation. When we conſider, that his nitro-aerial particles are the ſame with oxygen, when we reflect, that his ideas of reſpiration, and its application to the purpoſes of the œconomy, exactly coincide with thoſe of modern times, we may expect to find his ſpeculations on muſcular motion, to form a ſtriking analogy with thoſe of ſome recent authors. When the nerves were found to have ſo great a ſhare in muſcular motion, it was a natural concluſion, that what was denominated animal ſpirits, ſhould be called in to aſſiſt, in the explanation of the motion of animals. A difficulty,

 however,

however, ſtill preſented itſelf, which required conſiderable labour and inveſtigation to ſurmount; this was the nature of the nerves themſelves, a ſubject which always occaſioned much perplexity, and is not to be developed or unravelled by ſpeculations alone. Of late it has been imagined, that the diſcovery of animal electricity, opened a wide field, to acquire a *certain* knowledge of the cauſe of muſcular contraction. An ingenious author, on this ſubject, takes conſiderable pains to prove, how fully he is convinced, that the nervous electricity is the animal ſpirits, and that to the influence of this cauſe, is owing the contractile power of the fibre. It is remarkable, that the ſame reaſoning which Mayow applied to his nitro-aerial particles, has been called in to aid the explanation of the nervous electricity, by Valli. It is time, that we ſhould now examine what Mayow has ſaid, upon this ſubject, and we ſhall be as conciſe as poſſible.

(A) I maintain, that muſcular contraction is produced by different particles, uniting and effervеſcing, in the ſtructure of a muſcle. We cannot doubt, but that an influx of animal ſpirits is neceſſary, becauſe if a nerve be cut or obſtruct-ed, the muſcle to which the nerve is diſtributed cannot contract. But we are not to imagine, that muſcular contraction ariſes from the animal ſpirits alone; in as much as to the performance of which, it is abſolutely neceſſary, that other particles ſhould be ſupplied from the maſs of blood. Since, indeed, the arterial blood flows to the muſcles, in a continual ſtream, and that in a greater quantity than is diſtributed to other parts, or than is ſufficient for their nutrition, eſpecially in violent actions, it is rational to conclude, that arterial blood, during its paſſage, depoſits ſomething neceſſary to muſcular contraction. To prove this, he relates an experiment of Steno's, who, by tying up an artery, found, that the muſcle to which it was diſtributed, was unable to undergo contraction*. (B) As to the nature of the *motive* particles, ſecreted from the maſs of blood, we are of opinion, that they are of a ſalino-ſulphureous nature. For we muſt obſerve, that not a ſmall loſs of fat takes place,

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* This aſſertion, ſays Valli, has been proved to be void of foundation, by the obſervations of Kaaw, Boerhaave, Langruiſh, Pozzi, and ſeveral others.

place, in violent exercife, and it is almoft altogether confumed, in laborious employments long continued: but, on the contrary, animals indulging in eafe, and ceffation from labour, become corpulent, and fat is depofited, in confiderable quantity, upon the mufcles; whence we conclude, that the fulphureous particles of the blood, claim fome fhare in performing mufcular contraction.

Having thus explained one fet of particles, neceffary to mufcular action; he next enquires into the nature of others, *viz.* animal fpirits, which are alfo neceffary to this function. Concerning the nature of animal fpirits, we may conclude, that they are formed from the nitro-aerial particles. To underftand this, he fays, we muft recollect, that the ufe of refpiration is, to tranfmit nitro-aerial particles into the blood, and alfo, that they are neceffary to motion. (c) Hence animals, in the more violent exercifes, as in fwift running, are obliged greatly to quicken their refpiration, which feems to take place, becaufe the nitro-aerial particles are fecreted from the mafs of blood, and confumed during mufcular contrac-

tion.

tion.* For I think, that the nitro-aerial particles, coming from the brain into the moving parts, and there meeting with the falino-fulphureous particles, they effervefce, and by thefe being thrown into agitation, mufcular motion is performed.† Hence it is abfolutely neceffary, for the continuation of animal motion, that a fupply of falino-fulphureous, and nitro-aerial particles, fhould never be wanting in the mafs of blood; and in proportion as mufcular contraction is increafed, as in the more violent exertions, fo is there a greater

* As oxygen is neceffary to the contraction of the mufcles, it is probable, that it is confumed; or, more properly fpeaking, enters into fome new combination, in confequence of which it is eliminated out of the body; for we cannot but fuppofe, that the quantity employed, correfponds to the vaft quantity imbibed. Hence we may underftand the final caufe of quickened refpiration, during great exertions of the mufcles.—Beddoes on Scurvy, &c. P. 50.

† Does not mufcular contraction, or intumefcence, really depend upon the combination of oxygen with hydrogen and azot (feparately, and combined in various proportions) in confequence of a fort of explofion, produced by the nervous electricity?—Beddoes' Remarks on Girtanner's Effay on Irritability, P. 258.

greater waſte of nitro-aerial and ſulphureous particles; to reſtore which, not only reſpiration muſt be quickened, but alſo aliment, replete with ſalino-ſulphureous particles, muſt be taken in greater quantity: hence thoſe ſubſtances, which conſiſt principally of a volatile ſalt and ſulphur, are the moſt proper for recruiting the ſtrength, worn out by continual labour. Whence are the nitro-aerial particles ſupplied in muſcular motion? is a queſtion, which Mayow aſks himſelf. (D) Concerning this, I was ſometime in doubt, whether the nitro-aerial particles did not immediately paſs from the blood, into the moving parts; but when I had applied more attention to the ſubject, it appeared more probable, that the *motive* particles, ſupplied by the blood, were of a ſalino-ſulphureous nature: whence it follows, that the nitro-aerial particles are derived from the brain, and, conſequently, that they are the ſame with the animal ſpirits. I think it may be allowed, too, that in the more perfect animals, the animal ſpirits are only formed in the brain, and thence diſpenſed through the ſpinal marrow, and nerves ariſing from it. Why ſhould not the animal ſpi-

rits

rits be rather derived from the atmofphere, than from our food? It is apparent, that the air is impregnated with extremely active and fubtile particles, and fo great is the demand of the fyftem for air, that we cannot exift one moment of time without it. And indeed it does not feem poffible, that the immenfe wafte of animal fpirits, can be fupplied from any other fource than from the air.

Such is the admirable manner, in which our author reafons upon this fubject. The attention that has been beftowed on this department of phyfiology, points out, at once, its difficulties. The elaborate inveftigations of Valli, of Pugh, and of Blane, with all their experience, from the time of Mayow, have fcarcely been fufficient to the tafk. The explanation of the mechanical philofopher, is rejected by the chemift, who thinks fomething more is neceffary, to produce the phænomena. Had all the mufcles been fubject to volition, lefs difficulties would have perplexed the fubject; as it is, mufcular action depends on fomething exclufive of that caufe; and it is as certain, too, that the action of volition alone, is not adequate to produce the effect. The will employs its agent; and what that agent is, has been the dispute for many ages. The æther of Sir I. Newton, the fire-air particles of Mayow, with the oxygen and nervous electricity of modern times, have perplexed, divided, and confounded the medical world. The doctrine which seems most adequate to explain the phænomena, is that founded upon the curious discovery of Galvani. Here too, insurmountable difficulties attend; and notwithstanding the ingenious and plausible endeavours of Valli, to reduce under the principle of the electric fluid, the general effects produced by the influence of the nervous system, there are still innumerable exceptions.

Although I hold it highly injurious to science, to say the Deity wills it, when we are unable to explain certain phænomena, it is, nevertheless, allowed by all, that there are certain bounds placed to our knowledge, beyond which it is not permitted us to explore; and it would be no difficult matter to prove, that were our intellectual faculties more refined or intuitive, or our sensibilities more acute,

* The asylum ignorantiæ of Spinoza.

acute, we ſhould be very unfit inhabitants of this globe. In contemplating man, ſays an ingenious philoſopher, as at the head of thoſe animals with which we are acquainted, a thought occurred, that no ſentient being, whoſe mental powers were greatly ſuperior, could poſſibly live and be happy in this world. If ſuch a being really exiſted, his miſery would be extreme. With ſenſes more delicate and refined, with perceptions more acute and penetrating, with a taſte ſo exquiſite, that the objects around him could by no means gratify it, obliged to feed upon nouriſhment too groſs for his frame, he muſt be born only to be miſerable, and the continuation of his exiſtence would be utterly impoſſible. Even in our preſent condition, the ſameneſs and inſipidity of objects and purſuits, the futility of pleaſures, and the infinite ſources of excruciating pain, are ſupported with great difficulty, by refined and cultivated minds. Increaſe our ſenſibilities, continue the ſame objects and ſituation, and no man could bear to live.* Hence the Heathen ſuperſtition, attached ſomething præternatural to the mode of life, nature, and food of

* Smellie's Philoſophy of Natural Hiſtory. P. 526.

of their gods, as we find in the defcriptions of their antient philofophers and poets. Far be it from me to damp the ardour of inveftigation; and it is impoffible for us to fay, what can or cannot be known, as we have not yet arrived at, or acquired the full completion of *time*; but it appears, that mufcular motion depends upon an immaterial caufe, little cognifable by our fenfes, and originally implanted in animal nature by Creative Wifdom, to direct the inftinct and the reafon of the rational and irrational part of the creation.

SECT. XI.

Of the Conspiring Action *of the* Intercostal Muscles.

The accuracy of Mayow, as an anatomiſt, is no leſs conſpicuous than his ſkill as a chemiſt. His attentive obſervation of the animal œconomy, from the ſlow-paced labour of diſſection, led him, no doubt, to the knowledge of the oblique co-operation of muſcles; a diſcovery which Dr. Monro has ſo excluſively claimed to himſelf, that many will be juſtly ſurpriſed, when they read the quotations from Mayow. Firſt, we ſhall briefly lay before the reader, Dr. Monro's doctrine, with his reaſoning; and after, by bringing paſſages from Mayow, we ſhall be the better able to judge of the circumſtances. Dr. Monro, after giving an account of muſcular obliquity, in general, adds, ' but in treating on particular parts, I dwelt chiefly on the ſtructure and effects of the intercoſtal muſcles, as a variety of opinions, concerning their operation, has, in the courſe of the laſt hundred years, been propoſed; and as *no author had explained the reaſon of the obliquity of their fibres, nor of their being diſpoſed in two layers of decuſſating fibres*, after fully explaining the ſtructure, I endeavoured to prove, as Haller had done, but with ſome additional arguments, that both rows of intercoſtal muſcles, conſpired to elevate the ribs, or that they were muſcles of inſpiration.'* After having aſſerted this, he propoſes his reaſons and illuſtrations. That the firſt rib is ſo much fixed, as to be almoſt immoveable; and that the ſecond rib is more fixed than the third, and the third than the fourth, and ſo on, downwards.†

Now let us ſee, what Mayow has ſaid upon this ſubject. (E) It is a received opinion, that the external intercoſtal muſcles ſerve to dilate, and the internal to contract the thorax; but it appears to me more rational, that the thorax is dilated,

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* Obſervations on the Muſcles, and particularly on the Effects of their Oblique Fibres, by A. Monro, M. D. Edinburgh, 1794. P. 15.

† P. 16, 17.

lated, at the ſame time, by both. And we may aſſert, that the thorax is dilated, when the ribs are raiſed, and on the contrary, contracted when they are depreſſed. Let us ſuppoſe here (of what any one may be ſatisfied by examining the ſkeleton) that the ribs are not articulated with the ſpine and ſternum, at right angles, but that the angle below the ribs is ſomewhat leſs than a right one, ſo that if a rib be raiſed, its articulations with the ſpine and ſternum approach to right angles. Moreover we maintain, that the thorax is dilated, by the ribs being brought to right angles.* Whenever a muſcle attached to two bones contracts, the one bone approaches the more fixed; therefore, ſince each lower rib is leſs fixed than the upper one, it follows, when the intercoſtal muſcles contract, that each lower rib muſt be drawn upwards. And the ſame takes place, during the action of the

 internal,

* Haller mentions ſomething ſimilar to this. Coſtæ, circa ſuas articulationes rotatæ, angulos et cum ſterno et cum vertebris *majores* faciunt, in mediis vero arcubus aſcendunt, marginem ſuum inferiorem antrorſum erigunt. Primæ Lineæ Phyſiol. P. 162. It muſt be remembered, that Haller quotes Mayow, in his Elements of Phyſiology.

internal, as well as external intercoftal mufcles; nor is it of any confequence, that they are inferted into the ribs at different points, as appears in *Fig.* 1, where the interior mufcle, *a. a.*, during its contraction, draws up the lower and more moveable rib, equally as well as the exterior mufcle, *b. b.* We muft here obferve, that the ribs are articulated, in fuch a manner, with the fpine, that when they are raifed by the action of the intercoftal mufcles, they eafily afcend, and are thrown out, fo as to defcribe a circle.*

Can expreffions and reafoning be more ftrikingly fimilar? In fome places it is almoft a literal tranflation. Dr. Monro further obferves, that from the origin and infertion of the intercoftals in an upper and lower rib, they act with a longer lever upon the upper rib;† and fays, it is evident, that the obliquity of the fibres here, is not intended to increafe their number or ftrength of the mufcle; becaufe the fibres would have been more numerous, if they had paffed directly from one rib to the other, or had been inferted into the ribs

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* Vide the quotation from Haller, in note, P. 179.

† P. 17.

ribs at right angles.* Let Mayow ſpeak for himſelf. After obſerving that both ſets of intercoſtals conſpire to raiſe the ribs, he adds, (F) The oblique and oppoſite ſituation of the intercoſtal muſcles, is a farther proof of this. Although an inſertion at right angles would have been better adapted, for directly raiſing or depreſſing the ribs, yet nature ſeems to have inſerted thoſe muſcles obliquely into the ribs, becauſe the intercoſtal ſpaces are ſo ſmall, that if thoſe muſcles had been inſerted at right angles, they would have been ſhorter than their nature allows; therefore, that they might have their proper length, it is neceſſary that they ſhould be inſerted obliquely into the ribs. But ſince this oblique poſition is leſs fit for raiſing the ribs, therefore nature, that excellent mechanic, has formed muſcles of various inſertion, that, whilſt they at different points obliquely raiſed the ribs, the ribs ſhould, nevertheleſs, aſcend directly upwards, as is ſeen in *Fig.* 2, where the exterior muſcle, *a.a.*, and the interior, *c.c.*, acting at the ſame time, the inferior and more moveable rib

 aſcends

* P. 21.

afcends directly upwards, in like manner as if it had been acted upon, by a mufcle inferted at right angles. From this it is plain, that both the internal and external intercoftal mufcles, acting at the fame time, raife the ribs, by their joint co-operation, and thus dilate the thorax. Mayow further maintains, that the ribs are thrown out, during infpiration; and obferves, that they are united, by a double articulation, to the vertebræ. His words are, (G) It is to be remarked, that the ribs are not, as is commonly believed, united to the fpine by a fingle, but by a double articulation; and thefe articulations are fo obliquely placed, and are formed with fuch art, that it is impoffible that the ribs fhould be raifed by the intercoftal mufcles, but that they muft, at the fame time, be thrown out, for the more ample dilatation of the breaft, as is evident from *Fig.* 3. In which let *a. e. i.* be a portion of a rib, whofe round head *a.* enters the finus *c.* in the fpine, which articulation is fuperior and anterior. On the contrary, in the other articulation, *viz.* the inferior and exterior, the finus, which is fmaller, is hollowed out in the rib, at *e.*, which is articulated with the

 fpine,

ſpine, at the protuberance *b*. Now if we ſuppoſe *a*., the head of the rib, to be placed in the ſpinal cavity *c*., and the coſtal ſinus *e*. to reſt upon the ſpinal protuberance *b*., and then if that rib, connected to the ſpine by theſe double articulations, be moved upwards, it is eaſy to conceive, that that rib would be thrown to the left, or which is the ſame, with reſpect to the ſituation of the thorax, would be extended outwards.* An ingenious and accurate anatomiſt of London, takes conſiderable pains to impreſs upon the minds of his pupils, the effects produced by the double articulation of the ribs. He maintains, that the ribs are, thereby, much aſſiſted, in rolling upwards and outwards. It is unneceſſary for me to mention, how far this is the opinion of Mayow.

The acute Mayow offers another reaſon for

* The celebrated Author of the Conſpectus Medicinæ, has the following obſervations. Hæ (ſc. coſtæ) autem, propter obliquum quem cum ſpina dorſi habent ſitum et nexum, elevari nequeunt, quin ſimul protrudantur ipſæ et ſternum quoque protrudant et latera pectoris oppoſita a ſe invicem aliquantum dimoveant. Vol. I. p. 295.

his opinion, that the ribs are thrown out, during inſpiration, and which, we think, is an irrefutable fact. (H) Nor is it to be neglected, that the cartilages, through the medium of which the ribs are articulated with the ſternum, are united to the ribs with a remarkable obliquiry, as is ſeen in *Fig.* 4, in which *a. c.* is the rib, *c. e.* the cartilage between the rib and ſternum, *e.* the angle formed by their union. This obliquity is ſo formed, that the ribs may be extended and thrown out, deſcribing a circle. The clear and explicit manner in which Mayow has expreſſed himſelf, the full and ſatisfactory reaſoning he has applied to the facts, and the juſt inductions he has drawn, muſt convince the moſt obdurate ſceptic, that he has completely anticipated both Haller and Monro, in their ideas of the operation of the intercoſtal muſcles. We are, by no means, ſo prejudiced in favour of our author, as not to admit, that Dr. Monro has very ably extended the principle of obliquity to the action of muſcles in general. We only wiſh it to be obſerved, that the principle is clearly explained by Mayow, and therefore he muſt be conſidered as the discoverer of this important fact in physiology. He had, also, many opponents to struggle with, in promulgating this doctrine. It was an opinion generally received, previous to his time, that the external intercostals, were intended to dilate, and the internal, to contract the thorax, during respiration. This doctrine acquired strength, and seemed firmly established, on account of its being countenanced by the first physiologists of the times, as the reader will see, by consulting the twenty-third and twenty-fifth pages of the Pharmaceut. Rational. of Willis. Mayow was aware, that he had to oppose not only a prevailing doctrine, but one which seemed sanctioned by the authority of time and experience; for he opens the subject with saying, that he shall not pay so much regard to the authority of writers, as to truth itself. Notwithstanding, however, this general prevalence of opinion, that the two layers of intercostals performed different offices, Mayow's doctrine, not long after it was published, acquired its advocates, and those of the most powerful kind. In a system of anatomy, published in 1685, the author adopts the opinion, of the co-

operation

operation of the intercoftals, in elevating the ribs.* It is a received opinion, fays he, that the outward intercoftal mufcles are minifterial to the dilatation, and the inward to the contraction of the thorax; but as I humbly conceive, it is more agreeable to reafon, that both the outward and inward intercoftal mufcles, do affift each other, in the dilatation of the thorax; by reafon both kinds of mufcles have their extremities implanted into the margents of the next upper and lower rib, whereupon the moft loofely faftened doth approach the more ftrongly, by the contraction of the intercoftal mufcles; wherefore the lower rib, having a more lax articulation than the upper, muft neceffarily be drawn upward, by the motion of the mufcles, toward the upper more fixed rib, as

* A Syfteme of Anatomy, treating of the body of Man, Beafts, Birds, Fifh, Infects, and Plants, illuftrated with many Schemes; by Samuel Collins, Doctor in Phyfick, Phyfician in Ordinary to his late Majefty, of bleffed Memory, and Fellow of the King's moft famous College of Phyficians, in London, and formerly a Fellow of the Royal Foundation of Trinity College, in the moft flourifhing Univerfity of Cambridge. London, 1685.

as to the center of motion; whence it may be reaſonably deduced, that both the external and internal intercoſtal muſcles, though they have different inſertions into the ribs, yet, in their contractions, they aſſiſt each other, at the ſame time, to lift the ribs upwards, toward the throat.* This poſition is proved, by the very ſtructure and ſituation of theſe muſcles; their obliquity is contrived, to give them greater extent of motion; for had they been inſerted at right angles, they would have defeated the purpoſe which nature intended, therefore the grand Architect, hath moſt wiſely contrived the divers ſituations of the external and internal intercoſtal muſcles, that both pulling obliquely, with equal force, might mutually concur to the carrying ribs upwards.† It is rather ſingular, that although theſe paſſages have all the appearance of being a tranſlation from Mayow, yet Collins does not take notice of this: he, however, mentions him immediately after, ſhews great reſpect for his authority, and gives him the diſcovery of the double articulation of

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* P. 824-5. † P. 826.

of the ribs. And it is worthy our remark, he ſays, that the ribs have not only one, as is commonly thought, but two articulations, diſcovered by the learned Dr. Mayow, in his Treatiſe de Reſpiratione.*

Notwithſtanding the opinion, that both ſets of intercoſtal muſcles conſpired to elevate the ribs, during their action, was afterwards oppoſed by ſome anatomiſts, yet it had abettors of the firſt reſpectability, who all acknowledged it to be the diſcovery of Mayow. Drake, in his anatomy, mentions this diſcovery, attributes it to its right author, and adopts the opinion. The fibres of theſe muſcles (intercoſtals) run in an order contrary to each other, which has made ſome anatomiſts imagine, that they antagonized one another, fancying that the external drew upwards, and the internal downwards. This erroneous opinion, was firſt ſeverally refuted by the learned Steno, and our ingenious Dr. Mayow, whoſe arguments, however convincing, have, nevertheleſs, failed of ſucceſs, over the prejudices of many

later

* P. 826.

later writers†. He mentions the miſtake of Willis, with reſpect to the action of the intercoſtals, which, he ſays, is ſcarce worth while taking notice of, were it not for the great authority of the man, which is ſufficient to keep that error in countenance even to this very day. Verheyen, an accurate anatomiſt, at the cloſe of the laſt, and commencement of the preſent century, mentions Mayow's diſcovery of the conſpiring action of the intercoſtals, and adds, that his treatiſe on reſpiration, is very learnedly and ingeniouſly written. He obſerves, (1) That the ribs are ſo conſtructed, that when they are raiſed, they muſt, at the ſame time, be thrown out, and thus widen the cavity of the thorax. To elucidate what he means, he gives a diagram, and deſcribes it, in a manner ſimilar to Mayow. With reſpect to the actions of the intercoſtals, as producing this effect, he adopts the opinion of Mayow, whoſe obſervations, he adds,

† Anthropoligia Nova, or a new Syſtem of Anatomy, by James Drake, M. D. Fellow of the College of Phyſicians, and F. R. S. London, 1717. P. 400. Vol. II. We wiſh the reader would conſult the account which Drake has here given, as the paſſages are much too long to tranſcribe.

adds, he always found conformable to truth, by his own diffections. The doctrine, then, that the ribs are thrown out into an arch, while they are at the fame time raifed, during infpiration, and that both fets of intercoftals confpired to produce this effect, as each upper rib is more fixed than the lower, was brought forward by Mayow, and adopted by fucceding anatomifts, before Haller or Monro. It appears fomewhat extraordinary and furprifing, that the obfervations and arguments of Mayow, Collins, Verheyen, and Drake, fhould have efcaped the attention of thefe great anatomifts; and our furprife is encreafed, when we confider, that both Monro and Haller quote the words of thefe authors, who have taken confiderable pains, to confirm Mayow's illuftration of the beautiful mechanifm of the thorax*.

(A) Mufculorum contractionem, a particulis diverfi generis, in mufculi compage invicem commixtis,

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* Mayow is quoted by Haller, Elem. Phyf. 3-206. and the reader is referred to feveral paffages in Drake's Anatomy, by Monro, in his publication de Venis Lymph. Valv. p. 80. Edin. edit.

mixtis, mutuoque effervefcentibus, provenire ftatuam. p. 8. Tractat. Quartus. Spirituum animalium influxum, ad functionem motivam obeundam neceffarium effe, minime dubitandum eft: in quantum fc. nervo præcifo vel obftructo, mufculus, in quem idem diftribuitur, contractionem inire prorfus nequit, cæterum non eft putandum, mufculorum contractionem a fpiritibus animalibus folis dependere; utpote cui peragendæ particulis infuper aliis a fanguinis maffa fuggeftis, omnino opus eft. Quandoquidem enim, fanguis arteriofus continuo gurgite, eoque pleniore, quam ad cæteras partes, aut quam eorum nutritioni fufficit, præfertim in motibus violentioribus, ad mufculos appellit, liceat concludere, fanguinem arteriofum nonnihil ad mufculorum contractionem neceffarium, in tranfitu fuo deponere. P. 15-17.

(B) Quod fpectat ad naturam particularum motivarum, a cruoris maffa fecretarum, noftra fert opinio, eafdem indolis falino-fulphureæ effe. Etenim anotare eft, pinguedinis jacturam haud modicam in exercitiis violentioribus fieri, eamque in laboribus diuturnis pene totaliter abfumi: cum tamen e contrà, animalia otio indulgentia, et a laboribus

ribus vacantia, admodum obesa evadunt, eorumque musculis adeps copiâ satis amplâ accrescit. Unde colligimus, particulas sanguinis sulphureas, è quibus pinguedo componitur, in musculari contractione obeundâ partes aliquas obtinere. P. 22 3.

(C) Animalia, in exercitiis violentioribus, veluti in cursu valde concitato, omnino necesse habeant respirationem quam maxime intendere; quod propterea fieri videtur, quoniam in motibus violentis, particulæ nitro-aereæ, pro musculorum contractione instituendâ a sanguinis massa secernuntur et absumuntur, p. 24. Nempe existimo, particulas nitro-aereas, a cerebro in partes motrices prosilientes, ibidem cum particulis salino-sulphureis iis in occursum datis effervescere; a quibus mutuo se exagitantibus, contractio muscularis perficitur. Et hinc est, quod pro motu animali continuando omnino necesse est, ut pabulum salino-sulphureum, particulæque nitro-aereæ, in saguinis massa nunquam deficiant; et quanto intensius contractio muscularis instituitur, veluti in laboribus violentioribus, tanto majora particularum nitro-aerearum et sulphurearum dispendia fiunt; pro quibus resarciendis, non tantum respi-

ratio

ratio intenditur, fed infuper alimentum, particulis falino-fulphureis refertum, copiâ majori fumendum eft, hinc ea, quæ fale volatili fulphureque copiofo conftant, vires laboribus diuturnis attritas reficiendo, præcipue idonea funt. P. 30.

(D) Super hoc, in dubiis aliquandiu hæfitaverim, an non particulæ nitro-aereæ immediate a fanguinis maffa in partes motrices cederent: verum cum feriò ad rem attenderim, magis probabile vifum eft, particulas motivas, a fanguine fuggeftas, indolis falino-fulphureæ effe; unde fequitur, particulas nitro-aereas a cerebro provenire, et confequenter ipfos fpiritus animales effe. P. 32-3. Concedendum effe arbitror, fpiritus animales, in animalibus perfectioribus, non nifi in cerebro elaborari, atque eofdem ab eo fonte ad fpinalem medullam, nervofque ab eâ oriundos diffeminari. P. 38. Quidni ergo fpiritus animales potius ab aere, quam alimentis ingeftis ducantur? Certè aer particulis maxime activis, fubtilibufque imprægnatur; illiufque hauriendi tanta neceffitas eft, ut ne momento quidem temporis, fine eodem, vivere poffumus. Et quidem immenfa fpirituum animalium difpendia aliundè quam ab aere inftauriri, vix poffe videntur. P. 45.

(E) Recepta opinio eſt, muſculos intercoſtales tantum exteriores dilatando, interiores autem contrahendo pectori inſervire. At mihi videtur rationi magis conſentaneum ab iis utriſque ſimul pectus dilatari. Nempe affirmare fas ſit, coſtas ſurſum tractas pectoris ſpatium dilatare, deorſum autem retractas idem contrahere. Supponimus enim hic (quod cuivis in ſceleto videre datum eſt) coſtas cum ſpina et ſterno non ſecundum angulos rectos articulari, ſed angulos infra coſtas eſſe paulo recto minores: ita ut ſi coſta ſurſum trahatur, ejus articulationes cum ſpinâ et ſterno, verſus angulos rectos accedant. Afferimus inſuper, a coſtis ad angulos rectos elevatis, pectus dilatari. P. 278-9. Quandocunque muſculus duobus oſſibus affixus contrahitur, os minus fixum ad alterum magis fixum accedit: quapropter, cum inferior quæque coſta minus fixa eſt quam ſuperior, neceſſe eſt, ut muſculis intercoſtalibus etiam interioribus ſe contrahentibus, coſtæ ſingulæ inferiores ſurſum trahantur. Imo ſimilis planè ratio obtinet in interioribus muſculis ac in exterioribus; neque obſtat, quod illæ diverſa poſitione coſtis inſeruntur, uti *Fig.* , patet, ubi muſculus interior

interior, *a. a.*, in contractione ſuâ, æque ſurſum trahet coſtam inferiorem, mobilioremque, ac muſculus exterior *b. b.* Advertendum eſt hic loci, quod coſtæ cum ſpinâ ita articulentur, ut coſtæ a muſculis prædictis tractæ, facile aſcendant, et in orbem eleventur. P. 281.

(F) Et hoc ulterius adhuc oſtendit muſculorum intercoſtalium obliquus et contrarius ſitus. Ideo enim videtur natura muſculos illos oblique coſtis inſeruiſſe (quanquam iiſdem ſurſum aut deorſum movendis recta inſertio melius conveniret) quia coſtarum interſtitia adeo minuta ſunt, ut ſi muſculi iſti rectis angulis insererentur, breviores eſſent, quam ipſa muſculorum natura patitur; quapropter, ut dicti muſculi juſtam longitudinem obtinerent, eos oblique, ut fit, coſtis inſertos eſſe oportet: cum tamen obliqua hæc poſitio ad coſtas ſurſum movendas minus idonea ſit, ideo natura, machinatrix ſapientiſſima, diverſi ſitûs muſculos conſtituit, ut dum hinc inde æquali nixu oblique coſtas trahunt, coſtæ interim rectæ ſurſum aſcendunt, prout in *Fig.* , oſtenditur; ubi muſculis exterioribus *a. a.*, et interioribus *c. c.*, ſimul ſe contrahentibus, coſta inferior, mobiliorque non oblique fed rectà furfum afcendet; perinde ac fi a mafculo, rectis angulis ei affixo, traheretur. Plane ut videantur mufculi interiores, fimul et exteriores, eodem tempore fe contrahere, et fociato nixu coftas furfum ducere, pectufque ampliare. P. 282-3.

(G) Advertendum eft hic loci, quod coftæ non unicâ, uti vulgò creditur, fed duplici articulatione cum fpinâ conjungantur; articulique ifti adeò oblique collocentur, talique artificio formentur, ut coftæ a mufculis intercoftalibus furfum trahi nequeant, quin eædem fimul extrorfum pro majori pectoris dilatatione trahantur, prout in *Fig.* . manifeftum eft. In quâ *a. e. i.* coftæ portio fit, cujus caput rotundum *a.* finum in fpinâ excavatum *e.* ingreditur, quæ articulatio fuperior et interior eft. E contra vero, in articulatione alterâ, fc. inferiori et exteriori, finus, is autem minus confpicuus in coftâ ad *e.* excavatur, qui cum protuberantiâ fpinæ, ad *b.* extante articulatur. Jam vero fi fupponamus caput coftæ iftius *a.* in cavitate fpinæ *c.* collocari, et finum coftæ *e.* protuberantiæ fpinæ *b.* incumbere, et dein coftam eam, binis iftis articulationibus fpinæ connexam, furfum moveri, facile eft conceptu, coftam eam ad lævam, five quod idem eft, refpectu pectoris extrorfum latum iri. P. 284-5.

(H) Neque illud prætereundum eft, quod cartilagines iftæ, quarum interventu coftæ cum fterno conjungantur, non fine obliquitate notabili coftis inferantur, prout in *Fig.* . delineatur, in quâ *a. c.* cofta fit, *c. e.* cartilago, cujus interventu cofta ea cum fterno combinatur, *e.* angulus ex commiffura ambarum conftitutus. Obliquitas autem ifta eo fpectat, ut coftæ extendantur, et extrorfum in orbem trahantur. P. 285.

(I) Verum rem penitus infpicienti et arcuatam coftarum figuram, earumque inarticulationem confideranti manifeftum eft, coftas attolli non poffe, nifi fimul recedant extrorfum, atque ita thoracem amplificent. P. 310, Vol. II.

SECT. XII.

Of Digestion *and* Use *of the* Spleen.

When we reflect upon the many diseases, idiopathic and sympathetic, to which the stomach is so often subject, we are not surprised, at the almost innumerable dissertations that have been published, to explain the nature and cause of digestion; that grand function, upon which depends the life and activity of the whole frame. To an attentive observer, nothing appears more curious, than digestion, by which substances the most heterogeneous, are converted and assimilated into a bland and nutritious fluid. One of the first causes to which it was attributed, was fermentation, obviously because this operation was the first that presented itself; in which it was perceived, that the qualities of substances were spontaneously and completely changed. It was natural, therefore, to have recourse to a ferment, which seemed so

satisfactorily

ſatisfactorily to explain this important function. The term fermentation, was applied almoſt to any internal motion, which took place in various ſubſtances; it was applied, uniformly, to the decompoſition of all bodies, and was equally extended to the effervefcence, produced by the union of an acid with an alkali. Decompoſition, certainly, takes place in every fermentation; and in this ſenſe of the word, it is with equal propriety applied to digeſtion.

The knowledge of reſpiration, has beautifully explained the connection between that function and digeſtion. That animal food increaſes the neceſſity, and cauſes a greater abſorption of oxygen by the lungs, is now placed beyond a doubt, and will certainly lead to important regulations, in the regimen of patients, in different diſeaſes. Certain it is, that the union of oxygen with hydrogen, &c. produces conſiderable effects in the ſyſtem; and by this union, provided the oxygen be taken in by the lungs, reſults animal heat, the *ſine qua non* of animal life. If the oxygen be thrown in by the ſtomach, in the form of acids, &c. it is in a fixed ſtate, ſeparated from its caloric; of courſe this method of oxygenating the fyftem, is not fo ufeful, when we wifh to increafe the generous and benign ftimulus of animal heat, as in chlorofis, dropfy, &c. But in fevers of the putrid kind, when the heat is very great, from the rapid decompofition of oxygen, and when, at the fame time, we wifh, under certain circumftances, to throw in that fluid, the ftomach feems, at leaft, as good a medium as the lungs, for oxygenating the fyftem. I, therefore, cannot agree with my ingenious friend, Dr. Rollo, that it is proper, in every cafe, to oxygenate the fyftem by the ftomach. Befides, when we oxygenate by the lungs, we aid digeftion more powerfully; and in dyfpepfia, where acidity always more or lefs prevails, oxygen is prefent in fufficient quantity; but it requires animal heat to give vigour, and apply the oxygen to the purpofes of the œconomy; and cold extremities is a never failing fymptom in dyfpepfia. Acids, too, are cooling, and they appear to me to produce this effect, by leffening the abforption of oxygen with caloric by the lungs.

Let us now examine, what Mayow has faid on this fubject: He will not admit the opinion of

his

his time, that digeftion is owing to an acid fer-ment in the ftomach. The acid eructations are produced by bad digeftion, and not by any natural acid in the ftomach. Iron, indeed, is corroded, and milk coagulated in the ftomach. How is this to be accounted for? In the firft place, I fuppofe, that digeftion is performed by the animal fpirits, carried through the nerves, which are fo numer-oufly fupplied to the ftomach; and according as the animal fpirits are fupplied, in greater or lefs quantity, to the chylopoietic organs, the digeftion of our food is more flowly or more quickly per-formed. (A) Hence if any one, after food, ap-plies to ftudy, or employs his thoughts on any abftrufe fubject, fo that the animal fpirits are, in a great meafure, detained in the brain, on account of the attentive contemplation, and greater agita-tion of the mind, the office of digeftion is not rightly performed, becaufe there is a want of animal fpirits: but our food remaining undigefted in the ftomach, caufes a heavinefs and anxiety in that organ, as any one may put to the teft of experiment in himfelf. But, on the contrary, when the mind is not engaged in thought, as in

fleep,

ſleep, the digeſtion of our food goes on perfectly well; for then the animal ſpirits, not being employed in carrying on other functions, are copiouſly ſupplied to the organs deſtined to digeſtion: hence it happens, that immediately after a full meal, we feel a great propenſity to ſleep. It is a fact, which no one will now diſpute, that the brain ſhould be continually ſupplied with well oxygenated blood, in order that its functions may be exerted vigorouſly, and unimpaired. Certain it is, too, that oxygen powerfully aſſiſts digeſtion; but whether it produces this effect by giving vigour to the nervous ſyſtem, or that the oxygen is immediately carried to the ſtomach, for this purpoſe, from the aorta, through the cæliac and gaſtric arteries, I cannot determine. Mayow, himſelf, had his doubts upon this point. (B) I will not, he ſays, poſitively aſſert, whether in land animals,* beſides the nitro-aerial ſpirit communicated by the nerves, there be alſo an aerial ferment, directly ſupplied from the maſs of blood to the ſtomach. I ſhould

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* He had juſt quoted Dr. Gault. Needham, to prove, that there is a communication between the ſtomach and air-bladder of fiſhes.

I ſhould think, it was neceſſary in both caſes. The ſtomach, we know, is very vaſcular; no function is performed by the blood, without the aſſiſtance of nerves. Is it then inconſiſtent to ſay, that the arterial blood, which follows ſo copiouſly to the ſtomach, is intended to ſupply oxygen, as a component part of the gaſtric juice, and alſo animal heat, as a ſtimulus to the ſtomach;* and that the nerves are intended to give vital energy to the whole, by the ſupply of nervous electricity, or any other power, the reader chuſes to give the nerves? From obſerving, that the brain muſt be ſupplied with fire-air particles, and that digeſtion is injured, if after a full meal we are employed in deep thought; (c) hence, I would conclude, ſays Mayow, the far-famed ferment of the ſtomach, is chiefly compoſed of nitro-aerial particles, which being depoſited, through the medium of the nerves, or the membranes of the ſtomach, it is highly probable, are there mixed with a proper fluid, ſecreted from the blood by glandular membranes,

* The ingenious Mr. Coleman has ſhewn, that it is animal heat, and not oxygen, which is the ſtimulus to the fibre.

branes, of which the ſtomach is compoſed; both theſe being carried, by proper veſſels, into the cavity of the ſtomach, from its fermenting liquid.* And we may now know, why a clear and pure air cauſes appetite, and promotes the digeſtion of a ſurfeit; while, on the contrary, a denſe atmoſphere, polluted with noxious vapours, ſoon produces anxiety at the ſtomach. (D) If the ſtomach is entirely empty, it is very probable, that its inner membranes are irritated by the nitro-aerial particles; hence hunger is produced. Such is the admirable and conciſe manner, in which the accurate Mayow reaſons on this ſubject. Who can with-hold their applauſe, when we find him, in every

* Although Mayow had denied, that digeſtion was owing to an acid ferment; yet we ſee, he makes uſe of this term, becauſe the phyſiological vocabulary was not ſufficiently enlarged, to allow a better. This is evident, from what he ſays in the ſentence immediately preceding: Although the nitro-aerial ſpirit is, of itſelf, not acid, yet iron is corroded by it, and by being united with other ſubſtances, it becomes almoſt an univerſal menſtruum: hence the far-famed, &c. &c. Has he not here anticipated Stevens, Spallanzani, and Hunter, in their ideas of the gaſtric ſolvent?

every cafe, ftriking out fome noble truths, and wonderfully anticipating the modern pneumatifts?

After fome obfervations on the fuccus pancreaticus, which, he fays, contains fire-air particles, the next fubject which occupies his attention, is the fpleen; the ufe of which has bafled the inveftigation of phyfiologifts. Mayow obferves, that after this part of his work was written, De Graaf's publication, De Succo Pancreatico, came into his hands, in which the fame ufe was affigned to it by De Graaf, as by himfelf, *viz.* by its union with bile, to form a proper digeftive for the chyme. But we cannot, adds Mayow, agree with him, that the pancreatic juice poffeffes any acidity; for in that cafe, inftead of forming a homogeneous fluid, it would produce an indiffoluble coagulum, which acids are known to precipitate from bile*. The fubferviency of the fpleen to the liver, has been long noticed by anatomifts, and confidered as its principal, if not its only ufe; notwithftanding that the idea is contradicted by the remarkable and

* For a confirmation of this fact, fee Maclurg's Experiments on Human Bile, p. 11; and Saunders on the Liver, p. 102-3.

and well authenticated fact, that animals can live, perfectly well, after the spleen has been cut out, as is observed by Mayow, Baglivi, and innumerable others. The ingenious Dr. Saunders has made an experiment, which decisively proves, that the functions of the liver are, altogether, independent of the spleen. He examined the bile of a dog, from which the spleen had been removed for several weeks, and found no difference between it and bile under the usual circumstances*. When it was found, that the animals could live without a certain viscus, no wonder that the medical world was perplexed about its use. It is imagined, that a physiologist, now a lecturer in the Borough, has discovered (if it is discovered) the true use of the spleen. It is considered by this gentleman, as a reservoir for blood, which is supplied to the chylapoietic organs, during digestion. The stomach being distended with food, presses upon that viscus, and prevents the blood from flowing freely through the splenic artery; consequently a greater than usual quantity will be carried to the stomach and pancreas, into which branches from the sple-

nic

* P. 42.

nic artery are diftributed : hence a greater fecretion takes place, from thefe vifcera producing their falutary effects. If this be the whole of the theory, Dr. Haighton will find, that he has been anticipated, many years ago. We fhall firft examine what Mayow has faid upon this fubject.

That it may be underftood, what opinion we entertain, refpecting the ufe of the fpleen, we muft here repeat, what has been before mentioned, *viz.* It is probable, that to fupport animal life, it is neceffary that the animal fpirits, *i. e.* the nitro-aerial particles, muft be continually paffing through the brain, or, at the leaft, the cerebellum; hence it happens, that if refpiration be fuppreffed, or the pulfation of the heart be interrupted, but for a moment, the more perfect animals are inftantly deftroyed. The nitro-aerial particles are carried by nerves from the brain to the different parts. The organs of digeftion, require a greater fupply of nitro-aerial particles at one time, than at another. It is, therefore, probable, that the fpleen is formed to mix, intimately, the nitro-aerial particles with the blood. The functions, therefore, of the fpleen are thefe: (E) 1. That the nitro-

 aerial

aerial particles, which inceſſantly flowing through the brain, are not all exhauſted in the natural and animal functions, might be carried back into the maſs of the blood, and intimately united with the ſame. 2. That the nitro-aerial particles might be properly and regularly ſupplied to the digeſtive organs; for as theſe organs are, more or leſs, full, ſo they require a greater or leſs ſupply of fermentative particles. 3. That the nitro-aerial particles, endued with a motion and activity, and by being intimately blended in the ſtructure of the ſpleen, with the ſalino-ſulphureous particles, might produce, in the maſs of blood, an effervefcence, of ſuch a nature, as ſhall exalt its ſalino-ſulphureous particles to a proper volatility. Mayow, we ſee, conſidered the ſpleen, as ſubſervient to digeſtion, not mentioning a ſingle word of its connection with the liver. He makes uſe of the term nitro-aerial particles, inſtead of arterial blood, as it is only in it, that the fire-air is conveyed to the different parts of the ſyſtem.* In a work publiſhed in 1723, entirely on the ſpleen, we

* The fire-air, carried by the nerves, was modified, and became the animal ſpirits.

we find the author maintaining, that it is intended to affift digeftion*. It is the fulleft and beft account of the fpleen we have ever read; and the author feems to have beftowed very confiderable pains upon the fubject. After mentioning the opinion of the antients, refpecting its nature and ufe, and giving a very accurate defcription of it, he then offers, very fully his ideas, as to the functions it performs. From the ftructure, he calls it an animal fponge, the great refervoir and magazine of blood. Mentioning the numerous veffels ramifying through it, he obferves; this contrivance in nature, can ferve no other purpofe, than moft effectually to form a fponge, which, when filled by arterial blood, can, at pleafure, upon its contraction, throw it all out again, into the great channel of the fplenic vein, or back into the artery, if it be not too full†. The great quantity of blood, which flows to this vifcus, muft have been intended, by nature, to anfwer

* Of the Spleen, its Defcription and Hiftory, Ufes and Difeafes, by William Stukely, M. D. C M L. and S R S. London. 1723.

† P. 19.

ſome important purpoſe. The ſpleen, then, muſt be ſaid to officiate to the ſtomach, by pouring in upon it, as it ſwells in eating, a new quantity of blood, beſides that derived in the ordinary courſe of circulation, which is to procure the required heat and neceſſary juices in concoction. And that this is not only neceſſary, but likewiſe in a very high degree, a little conſideration will perſuade us*. It would occupy too great a portion of the preſent work, to mention the facts and arguments, which Stukely brings forward, in confirmation of his opinion; ſuffice it to ſay, that it was his opinion, that it ſupplied blood, during digeſtion, to all the chylopoietic organs, ſtomach, pancreas, &c†. and adds, Mayow's opinion quadrates with ours, only putting blood, inſtead of his favourite nitro-aerious particles, which in due plenty, and with a certain regimen, are carried to the bowels dedicate to the concoction of the aliment‡.

(A) Hinc ſi quis mox a paſtu ſtudiis incumbat, aut ſuper materiâ difficili cogitabundus fuerit, ita

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* P. 33. † P. 61. ‡ P. 42.

ita ut ſpiritus animales in cerebro, ob contemplationem attentam et majorem mentis agitationem, magnâ exparte detineantur, digeſtionis munus, ob ſpiritum animalium defectum, rite non perficitur; ſed cibus in ſtomacho indigeſtus manens gravedinem et moleſtiam ventriculo facit, uti quilibet in ſeipſo experiri poteſt; cum tamen, e contra, dum animus e cogitationibus vacat, uti etiam in ſomno, ciborum concoctio optimè peragitur; in quantum ſc. ſpiritus animales, non jam in functionibus aliis obeundis verſati, ubertim per viſcera digeſtioni dicata deferuntur. Huc etiam ſpectat, quod mox a prandio, ſive cœnâ pleniore, dormiendi cupiditate teneri ſolemus. P. 54-5.

(B) Utrum vero etiam in animalibus terreſtribus, præter ſpiritus nitro-aereos nervorum ductu advectos, fermentum inſuper aereum immediate a ſanguinis maſſa in ventriculum faceſſat, nihil certi ſtatuere poſſum. P. 56.

(C) Quapropter ſtatuendum eſſe videtur, fermentum ventriculi adeo decantatum, a particulis nitro-aereis præcipue conſtare, quæ nervorum ductu in membranas ventriculi depoſitæ, ibidem, uti veriſimile eſt, liquori idoneo, membranarum glandulosarum, quibus ventriculus constat, ministerio, a sanguine secreto, commiscentur; e quibus utrisque, per vasa peculiaria, in ventriculi cavitatem ingestis, liquor ejus fermentativus componitur. P. 55-6.

(D) Si stomachus cibis prorsus vacuus fuerit, illius membranæ interiores a particulis nitro-aereis, uti verisimile est, vellicantur; unde fames oriri videtur. P. 57.

(E) 1° Ut particulæ nitro-aereæ, quæ cerebrum serie continua pertranseuntes, in functiones naturales aut animales non erogantur, in sanguinis massam reportentur, eidemque rite admisceantur. 2° Ut particulæ nitro-aereæ debitâ copiâ et cum regimine quodam ad viscera ciborum coctioni dicata ferantur; prout enim viscera ea alimento referta, aut vacua fuerint, ita eadem, majori aut minori, particularum fermentativarum affluxu opus habent. 3° Ut particulæ nitro-aereæ, in motu et vigore positæ, cum particulis sanguinis salino-sulphureis in lienis compage quoad minima permixtæ, effervescentiam istiusmodi in cruoris massa excitent, quæ ad particulas ejus salino-sulphureas ad justam volatilitatem perducendas idonea est. P. 62-3.

SECT. XIII.

MAYOW'S EXPLANATION *of ſome* DISEASES *from his* DOCTRINE.

When a principle has been diſcovered, which produces conſiderable effects in the phænomena of nature, it becomes a matter of courſe, not only to recur to this principle for explanation, in deviations from regularity, but alſo, upon the ſame foundation, to extend our enquiry by analogical reaſoning. Thus, ſoon after Dr. Prieſtley had made his diſcovery of dephlogiſticated air, he immediately extended his ideas to reſpiration; and the diſcoveries of the great Lavoiſier, in France, were followed up, by their application to the irritability of the fibre. When Mayow, therefore, obſerved the univerſal agency of his fire-air particles, his penetrating mind immediately perceived, that they would aſſiſt him in explaining the ſymptoms of diſeaſe. In our days, upon the ſame foundation, and in an almoſt ſimilar manner, a ſyſtem of phyſic has been formed, which certainly explains ſatisfactorily, ſome of the moſt obſcure laws of the animal œconomy. Upon our knowledge of the airs, a ſuperſtructure is now daily riſing, which will add further dignity to the profeſſion of phyſic, and will render it a purſuit, more philoſophical and engaging: and we would fain indulge a pleaſing thought, that by a more general diffuſion of its principles, the credulity of mankind will not be ſo much impoſed upon, by that numerous herd of empirics, which every where inſult the common ſenſe of the metropolis. The materia medica has been ſo much enriched, by the new diſcoveries, that the ſtudy of chemiſtry muſt now form an indiſpenſable part, in the education of a medical practitioner. The beautiful and uſeful manner, in which the new diſcoveries illuſtrate the nature of diſeaſe; and the remedies they have ſuggeſted, in caſes hitherto incurable, will render the doctrine, founded upon the pneumatic theory, a moſt powerful aid in the hands of the phyſician.* The innumerable caſes, already publiſhed,

* I here allude, more particularly, to the cure of diabetes,

publiſhed, by Dr. Beddoes, afford a convincing proof of the utility of the airs, in a variety of complaints, which had reſiſted the effects of the uſual remedies; and in the hands of a ſkilful and cautious practitioner, I have no doubt, that this branch of ſcience, in conjunction with remedies by the ſtomach, will be made ſubſervient to the moſt uſeful purpoſes in the practice of phyſic. The manner in which the airs act upon the animal frame, did not eſcape the all-inveſtigating eye of Mayow. His explanation of their effects upon the body, is, in ſome caſes, perfectly analogous to that of modern times. The chapter, in which he handles this ſubject, is entitled, 'Of the diſeaſes, to which the animal ſpirits are ſubject.' The firſt diſeaſe which arreſts his attention, is phthiſis; and this, we know, was the firſt complaint, in which the theory and practice of pneumatic chemiſtry was applied. He explains it, as ariſing from the want of fire-air: this forms, however,

 no

diabetes, by the hepatiſed ammonia, &c. and the venereal diſeaſe, by the nitrous acid of oxygenated muriat of potaſh, for which ſee the ingenious work publiſhed by Dr. Rollo, and Mr. Cruikſhank of Woolwich.

no objection to the juſtneſs of his remarks, as the reaſons which he ſubjoins are concluſive, and which occurred to Dr. Beddoes, in his obſervations on that diſeaſe. (A) In order that animal life may be properly ſupported, it is of great conſequence, that the lungs poſſeſs a ſound diathesis, that the nitro-aerial particles may be ſupplied to the blood, in ſufficient quantity, and be intimately united with its ſulphureous particles. Hence, if the lungs ſhould become conſumed or tabid, the blood, on account of the want of nitro-aerial particles, is not rightly fermented; and becauſe the *motive* particles of both kinds are deficient, there will follow a proſtration of ſtrength, and a waſting. This theory, we ſee, is oppoſite to the one which Dr. Beddoes propoſed, ſome years ago;* but let it be remembered, that the firſt trial made, in this complaint, was by the inhalation of oxygen gas. It ſpreads flowers, ſays Mr. Chaptal, on the borders of the tomb, and

* I however think it probable, that inſtances have occurred, where the loſs of a conſiderable part of the ſubſtances of the lungs, has checked the progreſs of the diſeaſe. Obſ. on Calculus, &c. p. 146.

and prepares us, in the gentleſt manner, for the laſt dreadful effort of nature.*

That oxygen is one of the moſt powerful ſtimuli, we can apply to the irritability of the fibre, is, I believe, now very little doubted. When we know, that an animal cannot exiſt one moment of time, without a ſupply of this neceſſary fluid, we cannot but conclude, that it is concerned, in a variety of alterations which occur in the body, both in health and diſeaſe. That its quantity may be increaſed or diminiſhed, by certain alterations in the ſtate of the ſyſtem, affecting the lungs, is as probable and certain, as that the abſorption by the ſkin, undergoes theſe variations, from a multiplicity of cauſes. It is probable, too, that even ſhould a ſufficient quantity of oxygen be abſorbed by the lungs, yet, from certain cauſes, it may not be ſupplied to different organs of the body, in the quantity which the part may require. The brain is one of thoſe organs, which require a continual ſupply of well oxygenated blood, to ſupport both mental and corporeal vigour. (B) If the nitro-

* Elements of Chemiſtry, Vol. I. p. 138-9.

nitro-aerial particles, says Mayow, are not supplied to the brain in sufficient quantity, or are unable to pass obstructed nerves, a deranged state of the animal œconomy must be the consequence; and it would seem, that from this cause, not unfrequently are produced apoplexy, palsy, and diseases of that kind. If there be a fluid, necessary to the functions of the nervous system, it is highly probable, that a deranged structure of the nerve will impede, or totally prevent its transmission. The influence, termed animal electricity, is, we know, varied in its transmission through the nerves, by a variety of causes. If, therefore, we agree with Valli, in supposing the animal electricity to be the same with the nervous power, may we not be allowed to imagine, that a nerve may be so affected, as to become a non-conductor to that fluid? Is any one air a better conductor than another? Air, we know, is only a conductor, in proportion to the moisture it contains; but has it been proved, that none of the airs are possessed of a conducting power? As charcoal is a conductor, it is probable, that the carbonated hydrogen possesses that quality. Experiments, instituted

upon

upon these queries, would probably be more than curious. That oxygen is of as great use to the nervous, as to the muscular parts of our bodies, is evident, from the great quantity distributed by the carotid arteries. A sixth-part of the whole mass of blood is driven to the brain, from the heart, in an oxygenated form, and quickly returns thence, de-oxygenated, and is as speedily supplied by fresh oxygenated blood. The oxygen, therefore, which is carried to the brain, may, previous to its being applied to the nervous system, undergo, let me say, from morbid action, combinations, inconsistent with the healthy functions of the brain. Does not something, analogous to this, evidently take place in diabetes? Is there not here, diseased action, forming combinations, from the elementary part of our food, inconsistent with health? If, in other parts of the body, oxygen enters into morbid combinations, it is probable, that the same effect may be produced in the brain. It was maintained by Mayow, that as fire-air was so indispensable to the health of the brain, so any quantity of another principle, would produce opposite effects. He imagined, that a greater than usual quantity of

salino-falino-fulphureous particles, might get admiffion into, and injure the brain. Whatever obftructs oxygen, will, in fome degree, increafe the other principles; for as oxygen is withdrawn, the other principles will be more or lefs difengaged, and thus an accumulation take place, as happens in fcurvy. We do not mean, that they are completely difengaged in the vafcular fyftem, but that they form a combination, into the compofition of which, oxygen does not enter, and thus produce the falino-fulphureous particles of Mayow.

As the nitro-aerial particles, fays our author, are abfolutely neceffary to the functions of the brain, fo the falino-fulphureous ought to be excluded from it. (c) If it fhould fo happen, that they get entrance too copioufly into the brain, the animal œconomy is difturbed, as happens in intoxication, epilepfy, and fimilar difeafes: for liquors replete with a volatile fulphur, as fpirit of wine, and the chemical oil of vegetables, too inconfiderately taken, not unfrequently produce thefe difeafes. Intoxication, we know, is produced quickeft by the ftrongeft fpirits, that is, by thofe which have the fmalleft quantity of oxygen in their

their compofition, and their other principles in the loofeft ftate of combination. The degree of intoxication, independent of the quantity of fpirit taken, will, no doubt, be relative to the excitability of the fyftem. It is not the mere increafe of vafcular action, nor the ftimulus, abftractedly confidered, applied to the ftomach, which caufes drunkennefs. That a de-oxygenated ftate takes place, is evident, from the palenefs and laffitude which fucceed a debauch. With refpect to epilepfy,* we know it to be produced by a variety of

* The mention of this difeafe, brings to my recollection, a cafe which occurred, under my own obfervation. At the commencement of the winter, 1796, when I was in London, a man applied for advice for fits, to which he was fubject. I mention this, as one of the innumerable impofitions practifed by the empirics. I found the complaint to be epilepfy. The fubject was a fhoemaker, ætat. 23: had been under the care of an apothecary in town; but being impatient, and allured by the deceitful hand-bills daily diftributed in the ftreets of London, he applied for relief to the author of one of them, who told him, that the difeafe was trifling, and fhould be cured in a week; after fome months, he difmiffed him uncured, not, however, before he had

of caufes, affecting the nervous fyftem; but more particularly, that great fource of nervous energy, the brain. We are very little acquainted with the precife nature of difeafes, caufed by affections of this organ. Its excitement and collapfe, to ufe the words of the great Dr. Cullen, depend fo much upon, what appear to us, contradictory caufes, that they have divided and perplexed the medical world. Spafmodic affections of all kinds, are frequently very much beyond the reach of medical art, notwithftanding the elucidation that has been given to the fubject, by an ingenious phyfician. Pathology, fays Dr. Beddoes, is, on no account, more indebted to the author of Zoo- nomia,

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had taken from him twenty guineas!!! When he applied to me, he had a fit almoft daily, attended with confiderable ficknefs and vomiting at times. I concluded, that the dif- eafe was owing to a diminifhed energy, connected with dyfpepfia; but I knew, that although I removed the original caufe, the difeafe would be continued from habit. I, there- fore, ordered an æther draught to be taken, whenever he, in the leaft, fufpected a fit was coming on. This, together with occafional vomits, a proper regimen, and pills formed of the calx of zinc, and tartarized iron, reftored him to health.

nomia, than for the light he has thrown upon the obfcure origin of convulfive difeafes. The reader would do well, to confult this part of Dr. Darwin's publication, with the cafes publifhed in the fourth part of Confiderations on Factitious Airs, with the ingenious fpeculations of Dr. Beddoes on the fubject.

We have mentioned it, as the opinion of Mayow, that an accumulation of thofe principles in the fyftem, which exclude oxygen, proves highly injurious to the well being of animal life. Some obfervations which he adds, are extremely curious, and are analogous to a fimilar fubject, mentioned in Medical Extracts. I wifh the reader to attend to this explanation, as it decidedly points out, Mayow's ideas of chemical combinations, which fometimes takes place in our frame. (D) To the fame caufe, may be referred a fact, which has more than once come under my obfervation, *viz.* I have known feveral fubject to be feized by maniacal and convulfive fits, whofe faliva, when thrown upon the fire, during thefe paroxyfms, has burft into a flame, like oil, or rather like gunpowder. It is probable, adds Mayow, that thefe

phænomena

phænomena are owing to the ſalino-ſulphureous particles; the reader will recollect, that theſe are the principles which Mayow had maintained, united with the nitro-aerial particles, or oxygen, to produce fire or flame. In the firſt Vol. p. 92, of Medical Extracts, are ſome curious facts, mentioned with a ſimilar idea. Some remarkable caſes are there quoted, from authors of the firſt reſpectability, of perſons who have been conſumed by fire, in conſequence of dram-drinking; ſo eager, ſays the ingenious Dr. Beddoes, are the principles, of which ſuch perſons are compoſed, to combine with oxygen. The reader may attach what credit he pleaſes to ſuch facts. Perſons, who conſider the ſubject with a vulgar eye, will probably turn from it with contempt and ridicule; but the philoſophic enquirer, although he may have his doubts, reſts not his diſbelief upon the appearance of improbability. A fact, ſomewhat ſimilar to thoſe we have referred to, I find mentioned by Baglivi: On diſſecting a dog, the vapors which exhaled from the viſcera, crepitated like ſea-ſalt, when a candle was applied*.

* Diſſert. de Experim. Anat. Pract. p. 110. The hog

had

It appears, from ſome obſervations of the ingenious Mayow, that it was his opinion, that the functions of the brain might be injured, not only by directly with-holding the nitro-aerial particles from that organ, but alſo indirectly, by any changes which might be produced, with this principle, in other parts. Is not this evident, from the following expreſſions? (E) Melancholy and madneſs, are owing to the ſalino-ſulphureous particles being detained in the ſpleen, and there effervefcing, for a long time, with the nitro-aerial particles, produce the atra-bilious and gloomy diſpoſition. The manner of his expreſſion here, and the mode of explanation, are conformable to the idea he entertained, of the nature and functions of ſpleen; and the atrabilis was a conſtant attendant of the melancholia, according to the ancients. The curious ſympathy, that exiſts between the brain

had been, previouſly, fed, with a view to experiment. The lacteals were copious and tinged upon the meſentery, and alſo ſome lymphatics; the fluid in the latter, I taſted, ſays Baglivi, which was very ſharp, and left a very diſagreeable impreſſion upon the tongue; eratque mordaciſſima, et linguam graviter feriebat. The animal was very much emaciated, and laboured under the mange.

and abdominal viſcera, is known to the moſt ſuperficial medical obſerver; but why an affection of theſe parts, produces a concomitant change in the functions of the brain, is difficult to prove. When the chylo-poietic organs are debilitated, or deranged in their actions, who has not obſerved a great diſpoſition in the patient to magnify his complaints? Every medical practitioner muſt have frequently obſerved, that a cathartic has removed that gloomineſs, which overcaſts the minds of thoſe, who are ſubject to what is called the melancholic temperament.

It is evident, that animal heat, the reſult of this decompoſition of oxygen, is abſolutely neceſſary to the energy of the brain. As this is the caſe, we may be indulged in an obſervation. When any of the chylo-poietic organs are in a deranged ſtate, a diſpoſition to the generation of acidity is always preſent; hence oxygen is abſorbed, in leſs quantity, by the lungs; animal heat is, conſequently, diminiſhed, the vaſcular ſyſtem becomes weakened in its action, for the want of a ſufficient ſtimulus, the brain is deprived of its neceſſary quantity of animal heat, and the

train

train of thought will be affected, in proportion as the organ undergoes any modification, from an *altered* fupply. This is entering more into fpeculative theory than we wifh; but when principles cannot be rendered cognifable to our fenfes, it is natural and reafonable, to hazard conjecture; and from conjectures, facts often arife, which are perfectly reconcilable to truth and experience. It is not my wifh nor intention to vindicate every theory, Mayow may have thought his difcoveries fuggefted: but his hints, as well as his pofitive facts, ought to be taken into the account; and from the remarkable co-incidence between his difcoveries and obfervations, and thofe of modern times, it would be an injuftice not to pay as particular attention to his fpeculations, as to thofe of a more recent theorift upon the fame fubject.

Notwithftanding that oxygen is fo eafily taken in by the lungs, it is not decompofed from the blood by fimple chemical affinity alone. The heat of the body is, by no means, fufficient to form the different combinations, according to the laws of chemiftry. We muft, therefore, look out for fome other aid, to account for thofe various

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decompofitions, which we obferve to take place daily in our body. We can refer it to nothing, but the principle of life. A certain action is, therefore, neceffary in the veffels, to prepare the different elementary principles, in order to form the various compounds which the fyftem may require. Our food, which confifts principally of hydrogen, azot, and carbon, muft, therefore, be modified, by the powers of the fyftem, in fuch a manner, that when decompofed, the elementary parts may readily unite with oxygen; a circumftance abfolutely neceffary to animal heat and motion. The fame idea prefented itfelf, to the all-grafping mind of our moft accurate author. (F) It is abfolutely neceffary to the fupport of life, fays Mayow, that the falino-fulphureous particles* be fo managed by the powers of the fyftem, as to be brought to that ftate, in which they readily unite with nitro-aerial particles. As it is evident, that fuch an action muft belong to the fyftem, and as that action may be rendered irregular, by a variety of caufes, it follows, that the

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* The reader will recollect, that Mayow faid, that our food is compofed of thefe particles.

the elementary principles may be formed into compound, inconſiſtent with the health of the ſyſtem. (G) If the ſalino-ſulphureous particles, ſays our author, are not properly prepared, on account of ſome morbid diatheſis of the muſcular fibre, an impediment to motion will be the conſe-quence; and, therefore, it is probable, that the ſpontaneous laſſitude and diſinclination to move, which take place in ſcurvy and jaundice, ariſe from this cauſe. Is there not here, ſomething more than a hint of that theory, propoſed by Dr. Beddoes, and confirmed by Dr. Trotter? and do not the remarkable inability, and diſinclination to motion in ſcurvy, confirm the ideas of Mayow*? with reſpect to jaundice, every medical obſerver is acquainted with the languor, that attends an obſtructed flow of bile into the inteſtines. We have mentioned, that when a debilitated action takes place in the abdominal contents, a conſe-quent effect is produced upon the brain; and, in proportion as this is affected, a dimiſhed energy

* Vide Lind, Roupp, Blane, &c. A diſinclination to exertion, is ſo prominent a ſymptom, that Cullen mentions aſthenia in his Definition. Synop. Noſolog. Method. P. 294.

is communicated to the whole nervous ſyſtem; or, to uſe the words of Dr. Saunders, a torpor is diffuſed, by ſympathy, over every part of the ſyſtem, and langour and laſſitude prevail.* As a certain degree of vigour is neceſſary in the body, to eliminate and apply the elementary principles, taken in by the ſtomach and lungs, it follows, that whatever leſſens that neceſſary degree of power, prevents the proper decompoſition of our food; the caloric will, therefore, not be diſengaged from the oxygen, and the ſyſtem will want that indiſpenſable ſtimulus to its excitability: hence follow the cold extremities, langour and ſallowneſs of countenance, obſerved in thoſe who are, from any cauſe, affected with torpor of the alimentary canal. There ſeems, obſerves Dr. Saunders, much ſympathy between the brain and liver; and in maniacal perſons, there is generally a defect in the ſecretion of bile.†

Whatever

* P. 123.

† P. 154. Does not this explain, the good effects that are ſometimes produced by ſtrong purgatives in mania? Does the ſtramonium act in any other way, than by producing a flow

Whatever part of the works of Mayow we examine, we always find fufficient caufe for admiring the fertility of his genius, and the accuracy of his remarks. (H) As it appears, that with debility, as in fcurvy, there is a deficiency of fire-air, therefore, fays Mayow, we now know, the reafon why exercife of the body contributes fo much to the cure of fcurvy and jaundice, and to keep off the paroxyfms of intermittents: for, during exercife, refpiration is fo much quickened, that the nitro-aerial particles are thrown into the blood, in much greater quantity. How beautifully has the truth of this theory been illuftrated and confirmed, in fome of the moft popular and recent publications. The reader has only to confult the works of Beddoes, Thornton, Trotter, &c. to find analogous theories explained and confirmed. In every cafe of mufcular contraction, there is an expenditure of oxygen, and the wafte is in proportion to the quantity and violence of exertion. It is probable, too, that the exertion

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a flow of the different fecretions into the inteftines, and thus relieving their torpor, and ultimately the brain?—Vide Rufh's Effays.

of the mufcles may be fo violent, as to expend or eliminate out of the body, more oxygen than the lungs are able to convey to the fyftem, efpecially if they be, in any degree, morbidly affected. This feems to explain, fays Beddoes, an obfervation, that has fo frequently been made at fea; that the fcurvy makes its appearance after a ftorm, when the feamen, having undergone violent exercife, have expended a great part of the oxygen of the folids.* Animals, during violent motion, obferves Mayow, breathe very quick; becaufe the blood, returning to the heart, is deprived of the nitro-aerial particles, and very much fo in the brain, for the formation of animal fpirits. (1) From this we may fee, the reafon why blood, drawn during a convulfive paroxyfm, is apt to be thick, and in fome degree grumous: for in the more violent mufcular contraction, there is not only a very great wafte of nitro-aerial, but alfo of falino-fulphureous particles (upon the proper union of which, the fermentation and fluidity of the blood depend); and on this account it is, that the blood

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* Obfervations on Calculus, &c. P. 50.

is more difpofed to coagulate. This happens too more readily, in thefe cafes, if the parts, fubfervient to refpiration, become convulfed, then on account of refpiration being almoft fupprefs, the wafte of nitro-aerial particles, caufed by the convulfions, cannot be fo quickly reftored, as in other violent motions, when refpiration is free. The great difpofition which the blood fhews to coagulate, in a de-oxygenated ftate, and the curious, but extraordinary cafe, furnifhed to Dr. Beddoes, by the Leyden Profeffor, are a confirmation of thefe admirable obfervations of Mayow. The reader will recollect, too, how quickly oxygen communicated fluidity to the coagulated blood, in an experiment which we made with a frog.

Having now, in a great meafure, finifhed our extracts from Mayow's works, we cannot but exprefs much furprife, that fuch noble and confirmed truths, fhould have lain buried in oblivion for near a century. Every candid reader muft yield his affent, to this very extraordinary anticipation of modern difcoveries. I feel a degree of veneration for his name, when I reflect upon his genius; and I have derived the fame fatisfaction

from

from the perufal of his works, that was felt by the ingenious Profeffor of Gottingen. Acutiffimi ingenii medici Joh. Mayow, fays Blumenbach, tractatum Oxon. 1674 editum, magnâ cum voluptate legi et relegi.*

The philofophy of Mayow was publifhed in an age, remarkable for its production of great literary characters. Contemporary with him were Boyle, Newton, Lower, Willis, and innumerable others, whofe labours have confiderably improved the fciences.† Harvey had but lately difcovered the circulation; the philofophic world was engaged in examining the Newtonian and Cartefian doctrines; and the theory of Stahl was juft adopted by chemifts, and was in the full career of its glory. Germany was now famous, all over Europe, for its production of able chemifts; and men were much attached to the chemical labours of that country, which had given birth to Becher, to Van Helmont, and to Stahl. It was impoffible for a man, who died at the early age of thirty-two, however

* Inftit. Phyfiologicæ, P. 114. 1787.

† We may mention Sydenham, Gliffon, Thrufton, Bartholin, De Graaf, and Stahl.

ever true his theory, or juſt his ſyſtem, to oppoſe, with ſucceſs, ſuch accumulated obſtacles. No champion would be found, hardy enough to ſtem the tide of popular opinion, with theories and experiments not his own; and it is not likely, that many would riſk their literary reputation, in defence of doctrines, which appeared to meet with general diſapprobation. Thus it was, that the opinions of Mayow did not attract notice, till many years after his death, when his works were read and admired by ſome of the firſt philoſophers of the age. His opinions, however, were not generally underſtood, ſo as to form a ſyſtem; the public mind was not ripe for improvement; and thus it was, that his theories were loſt, in the great maſs of diſcordant doctrines. The beſt mode, ſays Dr. Scherer, which led to the perfection of chemiſtry, and which, although now adopted, does not remain entirely exempt from controverſy, was forgotten, and ſuffered to lie in duſt and oblivion, for upwards of a century. It is but a few years ago, that Wiegel, Blumenbach, and latterly, Dr. Metſger, all friends of antient literature, have called the attention of the chemiſt

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and phyfiologift, to the penetrating genius of our author.*

The name of this great man is now reftored to the annals of fcience; his genius is acknowledged, his abilities admired, and his opinions confirmed. The unequivocal concurrence of refpectable teftimonies, have added luftre to his literary fame; and may the laurel of merited reputation continually flourifh, unfaded, which the fatigues of difcovery have wreathed about his brow.

(A) Ad vitam animalem ritè inftituendam, imprimis neceffarium eft, ut pulmones diathefi fanâ potiantur, quo particulæ nitro-aereæ copiâ fatis amplâ in cruoris maffam trajiciantur, et cum particulis ejus fulphureis quoad minima permifceantur.

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* Thefe phyfiologifts, however, did not fufficiently appreciate the merits of Mayow.—Vide Appendix, No. VI. The gentleman who favoured us with this letter, is not the Dr. Scherer who made extracts from Mayow's works. Monf. de la Metherie, has alfo given a fhort account of Mayow's chemical opinions, with the plate of his pneumatic apparatus. Vide Obfervations fur la Phyfique, fur l'Hiftoire Naturelle, & fur les Arts, par M. l'Abbe Rozier. P. 154.

ceantur. Hinc ſi pulmones abſumpti ſint, aut etiam tabidi, flaccidique fuerint, ſanguinis maſſa, ob particularum nitro-aerearum defectum, non ritè fermentatur: unde fit, quod particulæ motivæ utriuſque generis deficiant, eòque ſumma virium dejectio, corporiſque imbecillitas contabeſcentia ſequantur. P. 73.

(B) Si particulæ nitro-aereæ aut copiâ non debitâ in cerebrum deferantur, aut nervos obſtructos pertranſire nequeant, œconomiam animalem et functionem motivam perturbari neceſſe eſt. Atque ab hâc causâ apoplexia, paralyſis, idque genus morbi alii, haud raró originem trahere videntur. P. 73.

(C) Hinc œconomia animalis valde perturbatur, uti in ebrietate, epilepſiâ, id genus aliiſque morbis, evenire veriſimile eſt: quippe liquores ſulphure volatili referti, veluti ſpiritus vini, oliaque vegetabilium chymica, nimis propere ingeſta, morbos prædictos haud raro inducunt. P. 74.

(D) Huc ſpectat obſervatio, haud ſemel a me facta. Nempe quoſdam noveram paroxyſmis veluti maniacis et inſuper convulſionibus corripi ſolitos, quorum ſputum, dum iſtius modi paroxyſmis laborarunt, igni immiſſum, inſtar olei, ſeu potius pulveris pyrii ritu, in flammam prorumperit. P. 74.

(E) Quoad melancholiam et maniam, probabile eſt particulas ſanguinis ſalino-ſulphureas, in lienis parenchymate detentas, ibidemque cum ſpiritibus nitro-aereis diu effervefcentes, indolem atribilariam malignamque acquirere. P. 75.

(F) Ad vitam inſuper animalem et functionem motivam inſtituendam requiritur, ut maſſa ſanguinea particulis ſalino-ſulphureis, iiſque debitè evectis imprægnetur, quo viz. particulæ nitro-aereæ iis admiſtæ, effervefcentiam juſtam in cruoris maſſâ excitent. P. 75.

(G) At vero ſi particulæ ſalino-ſulphureæ, ob fermentationem ſanguinis nimis imminutam non rite exaltentur, aut ob carnis muſculoſæ diatheſin vitiatam non ſatis prompte a cruoris maſſa ſecernantur, functio motiva vix inſtitui poteſt ; et utique probabile eſt, laſſitudinem eam ſpontaneam, et ad motum ineptitudinem, quæ ſcorbutum et mortum ictericum comitantur, ab hac causâ procedere. P. 76.

(H) Ex dictis ratio aſſignanda eſt, cur corporis

 exercitium

exercitium ad morbum ictericum, fcorbutumque, item ad febrium intermittentium paroxyfmos præcavendos tantum valet: nempe in exercitiis, particulæ nitro-aereæ majori copia per refpirationem intenfiorem, in fanguinis maffam tranfmittuntur P. 76-7.

(1) Ex quibus ratio petenda eft, cur fanguis, in paroxyfmis convulfivis emiffus, valde craffus et aliquantulum grumofus effe folet: nempe in contractione mufculari violentiori, non tantum particularum nitro-aerearum, fed etiam falino-fulphurearum (a quibus mutuo æftuantibus, fanguinis fermentatio et fluiditas dependent) difpendia quam maxima fiunt, eoque cruoris maffam aliquantulum coagulari neceffe eft. Hoc etiam tum præfertim contingit, cum etiam partes refpirationi infervientes, convulfiones patiuntur, tunc enim ob refpirationem fere fuppreffam, particularum nitro-aerearum jactura in motibus convulfivis facta, non uti alias in motibus violentis per refpirationem inftauratur. P. 44.

SECT. XIV.

Mayow's Works *attended to by various* Authors, *after his* Death.

It is a trite, but juſt obſervation, that the works of an author often lie neglected, till the author himſelf has paid the debt of nature. It then not unfrequently happens, that many exert themſelves to bring his works into notice. It requires time, too, to unfold the reaſoning of the mind; and as moſt people have not paid that attention to the ſubject which the author himſelf has, they will not ſo readily underſtand his experiments. As obſervations are made, they confirm the facts, or refute the theory advanced by him, whoſe labours have been over-looked. If the ſubject is of ſufficient importance, at all to engage the attention, the author, in either caſe, is in time taken notice of; and we are often ſurpriſed to find, that truths ſo noble, and the reſult

of

of tedious experiment, ſhould have been neglect-ed, for a conſiderable length of time. This ſeems to have been the caſe with our ingenious author; and it will be a purſuit of pleaſing curioſity, to examine how far his writings were taken notice of, when he was unable to feel the honors paid to his ſuperior genius. The comprehenſive abilities of Mayow, not to be reſtrained within the narrow limits of ordinary minds, graſped, at once, the whole animal œconomy, and endeavoured to explain all its laws, from his admirable diſcoveries. If in the ardour of enquiry, he is ſometimes found to be wrong, we are not to blame the genius of the man, ſo much as the fallibility of humanity. When the mind has been long employed, in inveſtigating the cauſes of natural phænomena, it feels an inexpreſſible pleaſure, in detecting the operations of nature: it gives a ſpur to induſtry, and a reſt for further enquiry. Wholly taken up with the object of its purſuit, and exulting in the ſatisfaction of not being employed in vain, it is apt to ſtray from the principles with which it ſet out, and fall into error, from premature concluſions. It is thus, that many great abilities have been led into miſtakes, by a favourite doctrine.

The opinion, that air held a conſiderable ſhare in the functions of the ſyſtem, had been mentioned, but not underſtood, before Mayow wrote. We may, therefere, expect, that this part of his works would be firſt attended to: accordingly, in publications already examined, ample teſtimony is given, of the confidence placed in the accuracy of Mayow's experiments, and his authority quoted, in confirmation of obſervations on the aerial doctrine. The manner in which the ingenious Wolferſtan explained the effects produced by the bite of the adder, ſhewed, that he had very particularly attended to Mayow's writings. The equivocal expreſſions of, *particulæ nitro-aereæ*, *nitro-ſalinæ*, &c. which Mayow uſed, is a reaſon, why he was ſo eaſily miſunderſtood, by thoſe who did not thoroughly examine into his expreſſions; and alſo afforded an opportunity, for others to aſcribe to him opinions, which he never publicly avowed. Science was not yet ſufficiently improved, to allow of better; and as far as chemiſtry was advanced at this period, no terms could be more

aptly

aptly applied. Had Mayow's life been ſpared to an older age, his terms would, no doubt, have been altered; as by a variety of experiments, he would have extended his diſcovery of oxygen, more generally, and have ſeen the neceſſity of *inventing* expreſſions, leſs equivocal.

About thirty years after the death of Mayow, a book was publiſhed at Amſterdam, on the ſubject of nitre. Schelhamerus, the author, makes Mayow ſay, that the air is full of nitre, and the nitrous particles is that part of the air, which is ſo neceſſary to the preſervation of life. Another author, of more recent date, attributes the ſame opinion to Mayow. He ſays, that Mayow pretends, ſome ſaltpetre flies about in the atmoſphere*. This is entirely a miſtake, which it is *now*, we hope, unneceſſary to correct. The doctrine, that atmoſperic air was replete with nitrous particles, was not attributed to Mayow by Schelhamerus

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* Tranſlator of Scheele's Eſſay. Should it be aſked, ſays Dr. Beddoes, if the author of this aſſertion had read Mayow, and ſhould it be replied, yes; then let it again be aſked, if he underſtood him; will that queſtion be alſo anſwered in the affirmative ?—Letter to Dr. Goodwin, p. 38.

hamerus alone. It was believed to be his, and eagerly adopted by the philoſophers of Europe: and the manner in which Schelhamerus mentions this circumſtance, ſhews ſome jealouſy of Mayow's reputation. Speaking of the abſorption of nitrous particles by the lungs, he adds; (A) Which opinion, *as the preſent age is ſo fond of novelty*, was *greedily* received by the phyſiologiſts of England, France, Germany, and Holland. This is the account which Schelhamerus gives. Are we warranted in ſaying, that, becauſe Schelhamerus thought Mayow mentioned, that the air contained nitrous particles, he attributed the adoption of the ſame opinion to others, who underſtood him in the right ſenſe? Is it not agreeable to think, that of the many who ſubſcribed to his doctrines, ſome muſt have entered into the *true ſpirit* of Mayow's enquiry? Morhof, we ſhall ſee, perfectly underſtood him. Among thoſe, too, who adopted the opinions of Mayow, the celebrated Baglivi of Rome.

The neceſſity of applying a name to *that*, which is abſorbed during reſpiration, muſt have been clearly perceived by all, when that function was

better

better underſtood. The anatomiſt who examined into the ſtructure of the fibre, or the phyſiologiſt who theoriſed upon the nature and effects of reſpiration, muſt have acknowledged, after this period, the efficacy of air, in the functions of the animal frame. The great diſcovery of the circulation had now been made, and acknowledged by all Europe; the full importance of the abſorbent ſyſtem was underſtood; no wonder then, that the beautiful doctrine of Mayow, as it ſo well illuſtrated and *completed* the diſcovery of the circulation, ſhould have gained ſo general a reception in the medical world: and had not the madneſs of politics diſtracted the attention of mankind, at this time, the diſcoveries of Mayow might have led to thoſe brilliant truths, which are now univerſally adopted. It is a remarkable coincidence in the hiſtory of ſcience, that the great diſcoveries in chemiſtry and phyſiology, of the laſt and preſent century, ſhould have been attended with a correſponding revolution in the politics of Europe. The name of Harvey was firſt immortalized amid the broils of civil war, and notwithſtanding Mayow publiſhed his work after the Reſtoration, policits ſtill engroſſed the attention of the public, and the effervescence produced in the minds of men, by the political mania of the times, did not ſubſide, till 1688, when James abdicated the throne of theſe kingdoms. Even after that period, the claims of the Pretender found advocates among many, and politics ſtill intruded upon the peaceful labours of the Muſes. Scarcely had the beautiful ſyſtem of Lavoiſier excited the attention, and claimed the acknowledgment of the chemiſt, when the fury of politics, and the madneſs of war, hurried all things into one undiſtinguiſhed confuſion. How muſt the heart of every man feel, when he reflects upon the murder of that illuſtrious philoſopher. When ſhall we again find combined, his ſpirit, induſtry, and great abilities, which an ample fortune enabled him to employ to their fulleſt extent? It is a loſs, which a century will not repair. Singular infatuation! that even the hoary head of ſcience ſhould be made to bend, without a crime, to the fury of democratic tyranny *. But to return.

Notwith-

* A great northern philoſopher, whoſe labours have much contributed to our preſent knowledge of the gaſes, in

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Notwithftanding the exertions of Schelhamerus, to difcredit the doctrines of Mayow, which he did not underftand, his truths made their way into the writings of authors of the firft refpectability. His affertion, that the philofophers, throughout Europe, attributed to Mayow the doctrine, that nitrous particles are abforbed by the blood, is remarkably contradicted in the Polyhiftor. of Morhof. Morhof feems to have been better acquainted with the writings of Mayow, than the other authors of his time. He was a man of extenfive reading, accurate judgment, and deep penetration. He travelled to different Univerfities, for the improvement of his mind. (B) He paft over into England, immediately upon the Reftoration, and celebrated the inauguration of Charles, at Oxford, in Latin verfes. He alfo refided at this Univerfity for fome time,

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an introductory lecture on the Hiftory of Chemiftry, when he came to the difcoveries of Lavoifier, was unable to proceed; the generous feelings of a humane heart, recurring to the cruel circumftance of his death, prevented utterance. It is faid, that when Lavoifier was informed, by letter from Dr. Black, of his converfion to the antiphlogiftic doctrine, fo great was his joy, in having acquired fuch able fupport, that he publifhed it in all the newfpapers in Paris.

time, to enjoy the benefit of reading, in the exten-ſive library of the Bodleian. As Morhof made it a point to cultivate the acquaintance of the literary, in the different Univerſities in which he ſtudied, I had imagined, that he might have met with Mayow, when he was at Oxford; but upon adverting to chronology, I find, that was impoſſible, as Morhof was at Oxford in 1660, and, according to Wood, Mayow did not enter the Univerſity, till September 1661, ætat 16.

The aſſiduous application of Mayow, had not only endeavoured to inveſtigate the laws of the animal œconomy, but he employed his diſcoveries to account for various phænomena, which occur in natural philoſophy. He had extended his doctrine to the explanation of combuſtion, reſpiration, the chemical union of ſalts, &c. &c. In moſt of his opinions, he is followed by Morhof, who, on every occaſion, takes an opportunity of paying him a compliment. In various parts of this author's intereſting miſcellany, we find the Tractatus Quinque, as a book of reference. Among other ſubjects, which Mayow had conſidered, that of combuſtion claimed his attention. He imagined, as we

have

have feen, that it was owing to the rapid motion of fire-air particles. In this part, he is quoted by Morhof, who fays, that Mayow's opinion is fingular; and adds, after having defcribed his theory,—this, to fpeak the truth, is very ingenioufly and curioufly imagined by him. On the fubject of nitre, Mayow had very ably employed his genius and his pen; and before his time, the nature of this neutral falt was very imperfectly underftood. The clear and explicit manner, in which he points out it's compofition, claims our admiration, when we confider, through what darknefs and confufion he had to wade, in his application to this fubject. The promifes, which Glauber had made, were not realized, in the fubfequent pages of his Novum Mundi Miraculum, and his dreams had impofed both on himfelf and others; neither was any elucidation given to this fubject, from the exprefs publication of Clarke. After perufing the treatife, De Sal-nitro, by Mayow, Morhof exclaims; (c) John Mayow has very learnedly, and with much perfeverance, enquired into it's compofition, in a whole treatife upon the fubject; and has fhewn, that there is a certain fpirit, which he terms nitro-aerial, difperſed through the earth and are, whence air derived the production and fermentation of all things. As Mayow had applied both his attention and experiments, to diſcover the nature and properties of atmoſpheric air, he thought it his buſineſs, to explain the phænomena that are produced by the agency of this great maſs of fluids. The ſingular appearances, cauſed by water-ſpouts, attracted his curioſity, and he determined to exert his abilities, in the explanation of them; and the maſterly manner, in which he has executed his determination, is another laurel, which the hand of Genius has wove into his crown. The vacuum formed by converging winds, did not eſcape his obſervation; and the water riſing on the principle of the Toricellian tube, is the explanation of the accurate Mayow*. Morhof mentions him, as the author who has given the beſt theory of theſe phænomena.

As

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* Electricity, certainly, has its influence in the production of water-ſpouts; their ſudden appearance and diſappearance, their occurrence in thunder ſtorms, the flaſhes of light that are ſometimes ſeen iſſuing, the cuſtom that ſailors have, of preſenting pointed ſwords to diſperſe them, all tend to confirm this idea.—Nicholſon's Philoſophy, Vol. II. p. 361.

(D) As to thofe aerial whirl-pools, termed by the Englifh, fpouts, no one has more accurately explained them than John Mayow, who has alfo defcribed them by plates.

The chemical accuracy of the indefatigable Mayow, is no where more confpicuous, than in his examination of Bath-waters. Notwithftanding his doctrines did not meet with a kind reception, at firft, yet we learn, from Wood, that he became noted, from his practice in phyfic, and efpecially in the fummer time, in the city of Bath. The waters of this celebrated place of fafhionable refort, would not efcape the prying eye of one, fo eager to examine into the operations of nature, as Mayow; and the refults of his experiments and obfervations, form a curious coincidence with thofe of Dr. Falconer. It is proper, Mayow obferves, before we enquire into the caufe of the heat of thefe waters, to examine their impregnations. The experiments of Mayow, produced the following refults. The waters become turbid, from the addition of the fixed and volatile alkalis, and they form a curd in boiling milk *. Hence, he concludes, that

* Falconer on Bath-Waters, Vol. I. p. 240-8.

that an acid is prefent in thefe waters, in a combined ftate. It appears, from the experiments of Dr. Lucas and Dr. Falconer, that felenite, or fulphat of lime, is contained in them. They are therefore rendered turbid by the precipitation of the lime, the alkali feizing upon the fulphuric acid. According to Mayow, thefe waters contain no nitre, becaufe the refiduum, after evaporation, when thrown on burning coals, does not deflagrate like that falt *. Dr. Falconer imagined, that the Bath-waters contained fulphur, in the form of a calcareous hepar. This ingenious gentleman, triturated quicklime and flowers of fulphur, with diftilled water, and by adding the different alkalis, in their mild and cauftic ftate, obferved, that the fame appearances were produced as in the Bath-waters. An objection has been made to this experiment, by a writer on the materia medica, who, had he exerted his chemical abilities, would have difcovered, that this objection was founded in error. He maintains, that fulphur and quicklime, triturated with cold water, produce a fimple lime-water, without fulphur. This is a miftake; for the

* Vide Falconer, p. 239.

the heat generated during the union of the quick-lime and water, is ſufficient to produce a ſolution of ſome of the ſulphur. Dr. Lucas, however, maintains, that they contain no ſulphur; Mayow alſo thinks ſo, and gives a curious reaſon, why it was imagined they contained ſulphur. It is a prevailing opinion, he ſays, that ſilver is tinged yellow by theſe waters; but this is by no means the caſe, for the deception has ariſen in the following manner. It is a cuſtom with the keepers of the baths, to colour pieces of money with the ſulphureous filth of privies, and then ſell them to ſtrangers for a moderate compenſation. According to our author, theſe waters contain no ſal ammoniac; and the chalybeate impregnation, which they are ſaid to poſſeſs, was detected by him. (F) The Croſs and Hot-Baths, he ſays, contain no iron; but the King's Bath ſtrikes a black colour, with infuſion of galls. This, too, accords, in ſome meaſure, with the experiments of Dr. Falconer, who ſays, that they all ſhew the preſence of iron; but, beyond all doubt, the chalybeate impregnation is moſt perceivable in the King's Bath water*, and

* P. 232.

and Dr. Lucas detected moſt iron in the water from the ſame Bath.

Mayow mentions a very curious circumſtance, unnoticed by other authors; and as he is very accurate in other reſpects, I cannot doubt it. (G) He ſays, that the ſabulous matter, which riſes with the water from the ſpring, ſhews no impregnation of iron, upon the addition of infuſion of galls; but if the vitriolic acid be added, a briſk efferveſcence takes place, and then the infuſion will ſtrike a black colour. Hence, he concludes, that this ſand contains a metallic impregnation, convertible into a vitriol, by the addition of the acid. It appears, at firſt ſight, a remarkable circumſtance, that authors ſhould have varied ſo much, in their analyſis of mineral waters; but when we conſider the fallacy that often obtains in theſe nice experiments, and alſo the variety of minutiæ, which eſcape the attention, we muſt be ſatisfied, with the imperfect knowledge of their impregnations. It appears to me highly probable, that from the continual decompoſitions which nature is undergoing, the quantities and qualities of the different impregnations muſt vary, at different times, producing no very ſenſible alteration, except by the nice analyſis of chemical teſts. Hence, probably, the reaſon, why the reſults have ſo often varied. I think it rational, alſo, to conclude, that they are continually undergoing a change, and repeatedly return to the ſame qualities, by ſucceeding recompoſitions, from various cauſes, which we daily ſee done artificially by the hands of the chemiſt. The coincidence between the experiments and obſervations of Mayow, and thoſe of Dr. Lucas and Falconer, is curious and remarkable. In him were combined the rare qualities of induſtry, genius, and perſeverance. Undiſmayed by the difficulties of purſuit, his ardent mind, with a perception attendant only on great abilities, examined into the ſecret operations of nature. His clear explanation of complicated experiments, his indefatigable induſtry, and conſequent diſcoveries, have handed down to poſterity, the name of Mayow. It was impoſſible for a mind, ſo vigorous as his, to reſt ſatisfied, with giving a ſimple explanation of the contents of Bath-waters; their heat, alſo, arreſted his attention, and he was determined to explain the caufe. It was an opinion, pretty generally adopted, in the time of Mayow, that the heat of fprings was owing to fubterraneous fire. Dr. Jordan was one of the firft who rejected this doctrine, and maintained, that from fermentation, was derived the caufe of this never-failing heat. It was not fufficient for Mayow to know, that fermentation produced this phænomenon; the caufe of *that*, alfo, muft come under his examination. It is owing, therefore, to the decompofition of martial pyrites. The fulphuret of iron, being moiftened, becomes heated, and Mayow fays, this is produced by the fermentation, which takes place between the air of the water, and the fulphureous part of the marchafite. Will the martial pyrites, moiftened *in vacuo*, produce heat? Is it not owing, according to Mayow, to the decompofition of atmofpheric air? It is a well known fact, that iron and fulphur, kneaded together, with a little water, will abforb oxygen from the atmofphere. That water contains air, Mayow fays, is proved, from the bubbles which are emitted, during the exhauftion of an air pump. In further confirmation of this, he relates an experiment,

which

which deſerves a deſcription. A veſſel, filled with water, is immerſed, with its mouth downwards, into a larger one, containing the ſame fluid. The water is made to boil, and afterwards ſet by to cool: we ſhall then find a quantity of air, collected in the upper part of the inverted veſſel; and this he found, by his experiments, to poſſeſs the elaſticity of common air. In peruſing many paſſages of this moſt ingenious author, my mind is ſometimes unwilling to believe, that they were publiſhed as far back as 1688; ſo aſtoniſhingly ſimilar are they, to the chemical doctrines, and mode of experiment, lately promulgated; ſo ſtrikingly great is the analogy between them. That air, too, is contained in water, is evident, from the life of marine animals; for fiſh ſoon die in water, from which the air has been exhauſted. (H) In this caſe, they turn on their backs; becauſe the air in their air-bladder exerts its elaſticity, and the abdomen is diſtended, the atmoſpheric preſſure being removed. He will not determine, whether the contents of the air-bladder enter the maſs of blood, as in reſpiration, but he thinks it probable; becaufe fifhes will live longer than other animals, in an exhaufted receiver.*

So juft and able account of the heat of mineral waters, would not fail to acquire the affent of the accurate Morhof: no wonder, then, when he found Mayow fo clear in his ideas, and fo well arranged in his facts, that he rejects the doctrines of others, and prefers Mayow's in this, as in other inftances. (1) More agreeable with truth, appears to be the opinion of John Mayow, who following Jordan, imagines the heat of mineral waters to arife from fermentation, produced in the bowels of the earth; *viz.* the particles of air defcending into the earth, with the rains, and there meeting the falino-fulphureous mineral, produce heat in it, and the fprings arifing from the mineral, thus heated, conftitute the warm mineral waters. The ingenious

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* My very worthy friend, Dr. Brodbelt, of Jamaica, has difcovered, that the air, in the air-bladder of the fword-fifh, is pure oxygen, and which, he very juftly obferves, is to ferve the purpofes of life, when the animal is far below the furface of the water.—Vide Letter to Dr. Duncan, in Med. Annals.

ingenious author of the Polyhiſtor. takes notice of Mayow, on ſeveral other occaſions. The abilities he had diſplayed, in his treatiſe on reſpiration, did not eſcape the attention of Morhof, who recommends his work, on this ſubject, with thoſe of Schwammerdam and Thruſton. It is evident, from what has been detailed, that Morhof ſet a value on the labours of Mayow; whether, however, he clearly perceived what Mayow meant, by his nitro-aerial particles, may perhaps be doubted. If we are to judge, merely, from the manner in which he quotes Mayow, it would appear, that he entered into the ſpirit of his author; on the contrary, when we find no explanatory account of theſe quotations, a ſuſpicion may be entertained, of his complete knowledge of their meaning*.

It requires more than a curſory reading, to under-

* The authority of Mayow, as a writer of credit and reſpectability, was referred to and relied upon, in other branches, beſides that of chemiſtry. Hanſen, in his Inaugural Diſſertation on Rickets, publiſhed at Gottingen, 1762, quotes him in the honourable company of Gliſſon, Petit, Heiſter, Boerhaave, Hoffman, Sydenham, &c. and he is alſo frequently mentioned, by the commentatro on Van Horne's Microcoſ.

underſtand his works, eſpecially when we conſider, that at this period, the language of the chemiſt was not altogether free from the jargon of alchemy. As ſoon, however, as the compoſition of the atmoſphere, and the knowledge of the different gaſes began to be developed, by the labours of Hales, of Prieſtley, and of Black, Mayow's works immediately arreſted the attention of the philoſophic world. In confirmation of the truth of this obſervation, we need only remark, that almoſt directly upon the ſuppoſed diſcovery of fire-air, by Prieſtley and Scheele, a celebrated writer on phyſiology immediately referred to Mayow, as the original diſcoverer *.

It is well known, with what enthuſiaſm Dr. Beddoes publiſhed his extracts from the works of Mayow. The conciſe manner, in which he treated the ſubject, barely did juſtice to the genius of the author. If we have at all filled up the deficiency, or, in any wiſe, impreſt the chemiſt and phyſiologiſt, with the reſpect due to the abilities of ſo great a man, we ſhall feel the moſt ſenſible ſatisfaction; and happy are we, from the inveſtigation we have

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* Blumenbach.

gone through, in being able to contradict the too hafty conclufion, that his name, as it never was echoed by popular applaufe, was foon forgotten among men, and his memory obliterated.

(A) Quæ fententia, ut eft novitatum amans hoc feculum, ftatim ab omnibus, vel tantum non omnibus Anglis, Batavis, Gallis, Germanis, avidè arrepta eft. P. 100 *.

(B) Exeunte eodem, A. 1660, in Bataviam et Angliam abiit, viros utriufque celebres falutavit, Caroli II, Britanniæ regis, inaugurationi interfuit, fuumque applaufum carmine illi teftatus eft gratulatorio, et in academia Oxonienfi, ut bibliotheca hujus illuftri uteretur, aliquandiu fubftitit. Prolegom. P. 10 †.

 (C) Longe

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* Guntheri Chriftoph. Schelhameri, de Nitro Commentatio. Amftelodami, 1709.

† Danielis Georgi Morhofi, Polyhiftor. Literarius, Philofophicus, et Practicus, Edit. à Johanne Mollero, Flenfb, Lubecæ, 1714.

(C) Longe doctius and magno molimine, nuper in ejus materiam inquisivit toto libro Johannes Mayow; qui ostendit, per totam terram et aerem spiritum quendam, quem nitro-aereum vocat, esse dispersum, unde omnium rerum fermentationes et germinationes deducit. Lib. II. Part 2, Cap. 38.

(D) Quod ad vortices illos aereos attinet, qui Anglis *spout* dicuntur, eos nemo accuratius, iconibus etiam additis descripsit, quam Joh. Mayow. Lib. II. Part. 2. Cap. 23.

(E) Balneorum custodibus familiare sit, nummos argenteos fimo salino sulphureo, qualis in cloacis passim reperire est, tingere et veluti deaurare, dein eosdem, tanquam aquis balneorum tinctos, peregrinis pro quæstu modico venditare. P. 251.

(F) Quod ad vitriolum denique spectat, balneum, vulgo dictum Balneum Crucis, item alterum Præfervidum nominatum, vitriolum plane nullum continere videntur; etenim si gallæ contusæ aquis thermarum dictarum infusæ fuerint, aquæ istæ colorem purpureum aut nigrum nequaquam habituræ sunt. Quod ad Balneum Regis, istoc vitrioli tantillo impraegnari videtur, quippe si gallæ contuſæ, ejus aquæ injiciantur, eodem colore atro-purpureo leviter tingetur. P. 151-2.

(G) Si ſabulum iſtoc balneorum liquore acido imprægnatum, infuſioni gallarum injiciatur, liquor mox colorem atro-purpureum acquiret; cum tamen, ei infuſio gallarum ſabulo iſti recens a balneis exempto affundatur, ea nequaquam colorem purpureum obtinebit. P. 252.

(H) Illud hic loci obiter annotare liceat, quod ſi piſcis aquæ in vaſe idoneo contentæ impoſitus, intra vitrum includatur, ex quo aer exhauritur, idem non ut antea pronus, ſed e contra ſupinus natabit; quin et venter ejuſdem ex parte aliquâ ſupra aquæ ſuperficiem elevabitur, quorum ratio eſſe videtur, quod aer in veſicâ natatoria incluſus, quamprimum aeris externi preſſura aufertur, ob vim, quâ pollet, elaſticam ſe ſtatim extendit, ita ut veſica prædicta, uti etiam cavitas abdominis ab aere iſto inflentur. P. 259-60.

(I) Rectior eſſe videtur ſententia Johannis Mayowii, qui Jordanum, popularem ſuum, ſecutus, thermarum incaleſcentiam a fermentatione, in terræ viſceribus alicubi excitata, provenire exiſtimat, ſic, ut particulæ aereæ, una cum aquis

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pluvialibus, in altam tellurem defcendentes, ibidem mineræ falino-fulphureæ occurrentes, æftum admodum intenfum in eadem excitent, et demum aquarum fcaturigines e minera, eo modo exæftuante, profluentes, thermas conftituunt. Lib. II. Part. 2. Cap. 20. Mayow. 261-2.

CON-

Conclusion *to the* First Part.

Having ſhewn, how accurately different authors were acquainted with the operation of air upon our bodies, during the laſt century, we preſume it will form no unintereſting part of the preſent work, to trace the progreſs of opinion, on this ſubject, from that period to the preſent times. We have ſeen, how extenſively diffuſed Mayow's doctrine was, although greatly miſunderſtood by ſome phyſiologiſts. We are, therefore, not ſurpriſed, to find his expreſſions adopted, without knowing to whom they belong, and his doctrine affecting the arguments of the phyſiologiſt, to the diſcoveries of Prieſtley and others. Certain it is, that almoſt immediately after the time of this moſt ingenious phyſician, we find, in a variety of authors, the reaſoning of the chemico-phyſiologiſt more clear, diſtinct, and conſiſtent with the truth ; but occaſionally, as is natural, we diſcover deviations from the ſpirit and meaning of the original diſcoverer. There is a tide in diſcoveries, which, like a ſtream, flows

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neglected and unperceived from its source, and occasionally is encreased or diminished, in proportion as it is confined, or receives it tributary waters, till at last it bursts into a broad and extensive plain : it then arrests the attention, and obliges us, by its magitude, to reflect on its source.

When it was known, that neither fire nor life could be supported, without the presence of air, it was natural, from analogy, to transfer the term, vital flame, to animal existence ; it was a just inference, too, to say, that this flame resided in the air. That this was actually the case, we find, in various publications, since the time of Mayow ; and which we shall now examine. Fire, flame, or heat, abstractedly considered, was regarded as the grand stimulus to all the motions of the animated frame ; it was the vital flame, residing in the blood*. No clear distinction, however, was made between the matter of heat and fire, or flame. The want of this discrimination, tended very much to confuse the ideas and perplex the reasonings of the

* The Wisdom of God, manifested in the Works of the Creation, by John Ray, F. R. S. London. 1692. P. 61.

the phyfiologift, till the beautiful theory of Dr. Black was publifhed. The air, fays Ray, ferves us and all animals, to breathe in, containing the fewel of that vital flame we fpeak of, without which it would fpeedily languifh, and go out*. This general proportion, involves in it facts of great extent and importance. We all know, how near it is to the truth; yet the nature and qualities of that fluid, which fupplied the vital flame, were as unknown to Ray, as to the predeceffors of Mayow. He faw, however, that fomething different from common air, but contained in it, fupported life; but its properties he found it difficult to underftand. The neceffity of an aerial pabulum was felt, and ackowledged by him; for he maintains, that the blood, in its paffage through the lungs, receives air, as the fewel of the vital flame, and thus impregnated, returns to the heart†. Animal heat was the effect, too, of this acquifition of air: hence animals of hot blood have two ventricles, by which that fluid has a more frequent communication with the air in the lungs, fo requifite to mufcular motion.

* P. 62. † P. 48.

tion*. The idea of animal heat being produced from the air, imbibed by the lungs, occurred to Ray, from the confideration that horfes, fheep, &c. remained with impunity in the open air, during the coldeft weather. Confidering with myfelf, by what means they were enabled to do this, and to abide and refift the cold, it occurred to me, that the extremities of their toes were fenced with hoofs, which in good meafurue fecured them; but the *main thing* was, that the cold is, as it were, its own antidote; for the air being fully charged and fated with nitrous particles (which are the great efficients of cold, and no lefs alfo the pabulum of fire) when infpired, doth by means of them, caufe a great accenfion and heat in the blood (as we fee fewel burns rafhly in fuch weather) and fo enable it to refift the impreffions of

* P. 100. Did Ray ever confult the works of Mayow? He no where quotes him; but we fuppofe had an *inkling* of his writings, from the following expreffions. When fpeaking of the damps in mines, he fays, indeed though there were no damps, yet the *nitrous part* of the air being fpent, and confumed by the breathing of the miners, the remaining part would be rendered altogether unfit for refpiration, unlefs new and frefh air could fucceed. P. 69-70.

of the cold, for so short a time as its more nimble circulation exposes it thereto, before it comes to another heating *.

The apparent want of air to the fœtus in utero, has often been the grand objection urged by those, who maintained the non-absorption of air by the blood in the lungs. This formed no obstacle to Ray, who explains it in a manner similar to Mayow; for he says, that the fœtus does receive as much as is necessary for it, in its present state, from the natural blood, by the placenta uterina or cotyledons. That the respiration of the dam did serve the fœtus also, or supply sufficient air to it, I have met with in books; but the explicit notion of it, I owe to my learned and worthy friend, Dr. Edward Hulse †. It was a doctine then more prevalent, at that time, than we

* P. 102. Here, by not making a proper distinction, he falls into error, which would not have happened, had he read and understood Mayow. From him he would have learnt, that it is only one of the principles of nitre, which by decomposition produces heat; and that the neutral salt, only during solution, produces cold.

† P. 65.

we are apt to imagine, but which owed its origin to Everard, and was more clearly elucidated and improved, by the experiments and obſervations of Mayow. The chief uſe, therefore, ſays Ray, of the circulation of the blood, through the placenta uterina, ſeems to be the impregnation of the blood with air.

When the principles of a ſcience have been developed, and have rendered familiar and eaſy the explanation of phænomena, we are, in ſome degree, aſtoniſhed to find, in older writers, facts laid down, and partly explained, but which required a much greater improvement, in that branch of knowledge, perfectly to underſtand. No one will be bold enough to aſſert, that Ray was acquainted with that principle, we now call oxygen; yet it is evident to conviction, that he knew there was an abſorption of a vivifying quality from the atmoſphere, and which was ſupplied to the fœtus, through the medium of the mother. He not only underſtood, that ſomething was imbibed by the lungs, but maintained, that it was the ſupport of the vital energy. This, ſurely, was a great point gained; and the doctrine would have received more firm

ſupport,

ſupport, and would have been more eagerly adopted, had Mayow's writings been thoroughly examined, and more generally underſtood. The fluctuation of opinion, with reſpect to a particular doctrine, ariſes, frequently, as much from the want of a general knowledge of the ſubject, as from a prejudiced attachment to a favorite theory. In examining into the works of ſucceeding authors, we are ſurpriſed to find one, who ranks high in the ſcale of the medical world, totally unacquainted with the nature of reſpiration, although this function had been examined, and its object pointed out, in the moſt unequivocal manner, by men of the firſt literary abilities*. It is a curious circumſtance, that notwithſtanding Liſter did not underſtand this important function, yet he diſtinguiſhes between atmoſpheric and vital air; but in a manner which confounded, inſtead of elucidating the ſubject. He maintained, that notwithſtanding air was neceſſary to life, yet it did not unite with the blood, in the lungs, but entered with the chyle †. There is a difference, he ſays, between pure air or æther,

* Lower, Mayow, Thurſton, &c. &c.

† Diſſertatio de Humoribus, P. 29-30.

æther, and common or atmoſpheric air : the former is, every where, clear, pellucid, inviſible, and poſſeſſed of a certain degree of elaſticity; the latter is compoſed of two kinds of *halitus*, and contains fire and water. The one *halitus* is moiſt, and evidently the vapour of water, produced by evaporation; the other is a warm and dry ſpirit. Of theſe two kinds of *halitus*, therefore, is compoſed atmoſpheric air, which, on that account, is to be conſidered both as dry and moiſt. Æther, therefore, or pure air, by which animals are ſurrounded, being replete with a dry and aqueous vapour, conſtitutes vital air *. This vital air, deſcending into the ſtomach with our maſticated food, is the cauſe of digeſtion; and its ſulphureous part, after it enters the lacteals, produces animal heat, by agitation with the chyle in the heart. The following is a remarkable obſervation, which he makes, with reſpect to animal heat; and he would have been able to have explained the fact, had he underſtood the nature of reſpiration. Our food, he ſays, is either a cauſe of animal heat, or an indiſpenſable condition to its generation †.

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* P. 88. † P. 97.

A certain fact, which the chemical doctrines have beautifully illustrated. I feel myself totally inadequate, to give any clear account of the distinction which Lister has made, between atmospheric and vital air. The confused manner in which he has handled this part of his work, shews, that he himself possessed no distinct ideas of the subject. He seems to have taken his notions from the doctrine of Hippocrates; but rendered more confused, by adopting the language of the Pythagorean School. He applied the term vital, to that part of the air, which Hippocrates called *spiritus*, and which was necessary to the nourishment of animals. I should have deemed it unnecessary to have mentioned this work, did I not wish to point out, that although certain laws of the animal œconomy could not be understood, without having recourse to the air, yet that so remarkable a deviation should be made from the more simple and obvious explanation of Mayow, Ray, and others.

Soon after the commencement of the eighteenth century, many ingenious men investigated the nature and properties of air, with its effects on the human frame. The labours of Hales, of Arbuthnot, and of Bryan Robinson, principally claim our attention, till we arrive to within a very few years of the late chemical diſcoveries, when our admiration is due to the genius of a great phyſician, the late celebrated Dr. Hugh Smith. The laborious experiments of Hales, furniſhed ample matter for future inveſtigation, by ſhewing how much might be done. The *quantity* of air, which he diſengaged from a variety of ſubſtances, ſeemed to take up his attention, much more than their properties. No juſt or uſeful diſcrimination was made, between the various airs which eſcaped, during his chemical operations. They all appeared to him to poſſeſs nearly the ſame quality; and when they were rendered unfit for reſpiration or combuſtion, it was owing to the loſs of their elaſticity*. The term, loſs of elaſticity, has been uſed by Hales and others, to expreſs that diminution, which takes place in the bulk of air, from reſpiration and combuſtion; not knowing, that every kind of air, when heated, is capable of nearly the ſame degree of expanſion. Hales imagined, that elaſticity was a quality, of which the air might, *in toto*, be deprived. I made ſome attempts, both by

* Statical Eſſays, Vol. I. P. 258-75.

by fire, and alſo by fermenting and abſorbing mixtures, to try, if I could deprive all the particles of any quantity of elaſtic air, of their elaſticity; but I could not effect it*. Owing to this idea of elaſticity, Hales maintained, that combuſtion was not maintained, nor animal life ſupported, by the vivifying ſpirit of air. The candles and matches ceaſing to burn, ſoon after they are confined in a ſmall quantity of air, ſeems not to be owing to their having rendered that air effete, by having conſumed its vivifying ſpirit; but ſhould rather be owing to the great quantity of acid fuliginous vapours, with which that air is charged, which deſtroy a good deal of its elaſticity, and very much clog and retard the elaſtic motion of the remainder †.

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* P. 312. The term elaſtic, is very improperly applied, in theſe caſes; becauſe it is owing, altogether, to the interpoſition of another matter, *viz.* caloric; and the degree of expanſion will be, in proportion to the quantity of caloric applied: for the ſame reaſon, iron, when heated, may be ſaid to be elaſtic.

† P. 275. A very conſiderable ſtep was made, by Hales, in improving our knowledge of the airs. He accidentally

As a difciple of the Boerhaavean fchool, Hales attributed animal heat to friction; and maintained, that the blood was cooled, in its paffage through the lungs. He quotes Mayow in two places, yet pays no attention to his difcovery of pure air, and his application of it to the animal œconomy *. There is a very particular experiment in the Statical Effays, extremely fimilar to one of Mayow's, which proves, that air, once breathed, is unfit for refpiration. The coincidence is fo ftriking, that I could wifh to quote the paffages, were they not too long †. Dr. Hales found, that he could breathe air, through cloths fprinkled with vinegar, as long again, as the fame quantity of air not thus managed ‡. I could point out, were it

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tally hit upon an experiment, by which he produced nitrous air. This he obferved to form red fumes with atmofpheric air, with a diminution of its bulk. He procured it from Walton pyrites, iron-filings, and mercury, with nitrous acid. We cannot call it his difcovery, as Mayow had already done the fame; and it is furprifing, that he fhould not take notice of this circumftance in that ingenious author.

* P. 234-36.

† Compare P. 98-299 of Mayow, with Stat. Eff. P. 169-255.

‡ For the Note, Vol. I. P. 266.—Vol. II. P. 321.

neceſſary, in ſeveral parts of the Statical Eſſays, expreſſions and obſervations, preciſely ſimilar to thoſe of Mayow, as the fermentation of pure air with ſulphureous particles, &c. I am ſure it is unneceſſary for me to mention, that I have not the ſmalleſt ſuſpicion of plagiariſm by that admirable philoſopher; a character which all reſpect and admire. I only wiſh it to be obſerved, that had Dr. Hales applied his abilities, with proper attention, to Mayow, he would have been leſs perplexed, in accounting for the multiplied variety of his experiments, and thus, as it were, by beginning where Mayow finiſhed, he would have conſiderably advanced the progreſs of knowledge. Had he, too, properly weighed the importance of Mayow's diſcoveries, would he then have broke out into the following exclamation? 'If thoſe who unhappily ſpent their time and ſubſtance, in ſearching after an imaginary production, that was to reduce all things to gold, had, inſtead of that fruitleſs purſuit, beſtowed their labour in ſearching after this much neglected volatile Hermes, who has ſo often eſcaped through their burſt receivers, in the diſguiſe of a ſubtile ſpirit, a mere flatulent exploſive matter,

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they would then, inſtead of reaping vanity, have found their reſearches rewarded, with very conſiderable and uſeful diſcoveries.'* The varied and multiplied experiments of Hales, inſtead of deterring, ſeemed to ſtimulate others to undertake the ſame ſubject. The great quantity of air diſcovered by him, in almoſt innumerable ſubſtances, obviouſly pointed out, that its preſence was intended to anſwer ſome uſeful purpoſe. The alteration produced in the function of reſpiration, by different ſtates of the air, ſhewed, that it had it's effects upon the œconomy. The various phænomena that occurred from modified airs,† but above all the apparent connection between epidemical diſeaſes and a ſtate of the atmoſphere, pointed out this ſubject, as one very fit for the examination of the phyſician and phyſiologiſt. Accordingly, it attracted and merited the attention of Dr. Arbuthnot, who, in 1733, publiſhed an Eſſay on the Effects of Air on Human Bodies; a work replete with judicious

* Vol. 1. P. 317.

† By modified air is meant, an alteration produced by impregnation with different matters, as may be ſeen in the experiments of Hales, and others, previous to this time.

judicious and accurate remarks. His obſervations are a collection of facts, which experience had ſuggeſted, but which he could not explain, for the want of chemical knowledge. He knew, that a portion of air, once breathed, was rendered deleterious to animal life; this he learnt from Hales; but he was entirely unacquainted with the cauſe. He was led into error, by attending too minutely to Hales's experiments, which did not diſcriminate between the effects produced by different kinds of air. After the manner of that philoſopher, Arbuthnot attributed the effects, produced by reſpiration and combuſtion, to the action and re-action of elaſtic air-particles. He, however, was obliged to acknowledge, that air had many uſes and effects, in the animal œconomy, beſides what was deducible from mechanical principles; and that there is in it ſome other vital principle, which makes it ſo neceſſary for the life of all animals*. We come now to an author, whoſe genius and abilities have conſiderably improved the ſcience of medicine. The name of Bryan Robinſon, is well known in the medical circles; and his Treatiſe on the Animal

* P. 115.

mal Œconomy, will not detract from his merit by being examined. In this work we find, the mind returning into the path of truth, after it had been mutilated, for upwards of fifty years, by the crude speculations of Lister, and the unconnected experiments of Hales *.

There is nothing which so much retards the progress of knowledge, as the neglect of the labour of our predecessors. Harvey would, probably, never have discovered the circulation, had he not made himself acquainted with the knowledge of the antients; and it is not improbable, that the writings of Hales, and others, led Priestley and Cavendish to their discoveries. The mind of an individual is not sufficiently vigorous to investigate, in its fullest extent the minutiæ of science; something must be previously and repeatedly performed, otherwise *all* the knowledge, which we now possess, would have

* We hope the reader does not imagine, that we mean to depreciate the merit of this ingenious experimentalist. The manner in which the subject was treated, could not have afforded any other result. We are rather surprised, however, that he did not attempt to make any discrimination, between the variety of airs which escaped, during his chemical operations.

have preſented itſelf to the experiments and obſervations of philoſophers, many centuries ago. A lucky train of thought, an accidental experiment, or a caſual explanation, have led to ſome of the moſt brilliant diſcoveries, which adorn and enrich the different ſciences. Chemiſtry and phyſiology, had received conſiderable and important improvements from the labours of Mayow, Lower, Wolferſtan, and others; but from neglect and inattention, theſe noble truths were buried in oblivion. Their merit, indeed, had acquired votaries, both at home and abroad; but from ſome unfortunate circumſtances, their diſcoveries were not followed up, which would have prevented that confuſion, which has crept into the experiments and ſpeculations of ſucceeding chemiſts and phyſiologiſts. After peruſing the writings of Mayow, Lower, Gliſſon, &c. what a great falling off do we obſerve, when our reading leads us to the publications of Boerhaave, Hales, and innumerable others. Notwithſtanding this, however, ſcience is indebted to the latter, for conſiderable improvements; they ſet that inveſtigation on foot, which had been neglected, in the writing of their predeceſſors, and

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which, but for their exertions, would have been longer consigned to accompany musty records, in the dusty corner of antiquated libraries.

It is with pleasure we peruse the works of Bryan Robinson, who, by going systematically to work, and arranging accurately his facts, has struck out some of those truths, which are now generally received. The following is the position he lays down to be proved. The life of animals is preserved, by acid parts of the air, mixing with the blood in the lungs; which parts dissolve, or attenuate the blood, and preserve its heat, and by both these, keep up the motion of the heart *. This, he proves, in the following manner: Animals die, when they are deprived of air; and they also die soon, in a small quantity of air, closely confined. Under similar circumstances, a candle goes out; glowing coals, and red-hot iron, cease to shine. Animals, also, die in air, rendered effete, by burning coals or candles in it till they are extinguished; and *vice versâ*. If air be rendered effete, or blown on live coals, it puts out the fire; but if fresh air be admitted, the coals begin to burn, and

* P. 190.

and ſhine afreſh: and he adds this remarkable fact, that common air, by being paſſed through red-hot iron, red-hot charcoal, or the flame of ſpirit of wine, becomes unfit to preſerve life, and the ſhining of fire or flame. From theſe unequivocal and well authenticated facts, he could not but conclude, that the very ſame parts of the air, which preſerved animal life, by reſpiration, alſo maintained flame, during combuſtion. He concluded, that this part of the air was acid, from the well known experiment of mixing nitrous acid with unctuous ſubſtances. He performed Mayow's experiment, of burning ſulphur and nitre, under water, without acknowledging the author; and tranſlates pages from Lower, concerning the red colour which the blood acquires in the lungs, but does not mention his name. He takes conſiderable pains to prove, that it is not the elaſticity of the air, which preſerves life and flame; becauſe the air which remains, after a candle is extinguiſhed, or an animal dies, is itſelf elaſtic. The air is, alſo, the cauſe of animal heat; for ſpirit of vitriol, which contains acid air, diſſolves iron-filings, with great heat and ebullition; and the acid of the air, diſſolves ſulphureous and unctuous fubftances, with the heat of fire and flame. Hence, we may conclude, that fince the acid of the air unites with the blood in the lungs, and attenuates it, the fame effect muft be produced there *. That the motion of the heart is owing to the fame caufe, is evident; becaufe, if the acid air be with-held from refpiration, the pulfe becomes fmall and quick. The fame reafon, which induced Mayow to give the name of fire-air particles to oxygen, alfo perfuaded Robinfon to call that gas, acid air; and from a fimilar caufe, have the more recent difcoverers given it its prefent appellation. The moft rigid fceptic could not have with-held his affent, to the clofe reafoning of Robinfon; for when chemiftry was not yet analyfed into its elementary principles, his obfervations were perfectly confiftent with truth. Dr. Robinfon faw

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* By a fubftance being attenuated, I underftand its being rendered more fluid. If a fubftance, we fhall fay, be compofed of two kinds of particles, the fimple interpofition of a third, muft make an alteration in the ftate of agregation, *i. e.* remove the original particles at a greater diftance from each other, or render the fubftance more fluid. A chemical interpofition would produce different effects, according to the degrees of affinity between the elementary principles.

ſaw the air united with the blood in the lungs; the ſame he knew to be a conſtituent principle of the acids; and, as the acids produce heat, in diſſolving various ſubſtances, it was natural to conclude, that when the acid air came into contact with the blood in the lungs, it cauſed animal heat in the ſame way. The genius of Black had not yet illuſtrated, by his beautiful doctrine of latent heat, ſome of the moſt important chemical phænomena; neither was it ſtrictly aſcertained, that atmoſperic air (for Mayow's experiments were not ſtudied) could be analyſed into two component parts *. The concluſions of Robinſon were, therefore, perfectly juſt; and if the reader will take the trouble of examining Prop. 24, from P. 190 to P. 206, in his treatiſe on the animal œconomy, he will find, with what a remarkable accuracy of obſervation his experiments and facts were illuſtrated.

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* Pure atmoſpheric air is compoſed of twenty-eight parts of oxygen, to ſeventy-two of azot; the other gaſes, which are found in it, are accidental, and vary according to local circumſtances.

The period was now faſt approaching, when the whole doctrine of the humoral pathology began to be ſuſpected; for experiment and obſervation now taught, that neither viſcid lentor, nor error loci, could, in every caſe, be the cauſe of fever or inflammation. The doctrines of Hoffman had made their way into the hands of the ſtudents of Edinburgh, and found a more ready reception than thoſe of Boerhaave; at this time, the humoral pathology was at its acmè, being taught by the profeſſors, who had been all pupils of Boerhaave.

The ingenious Dr. Gregory, of Edinburgh, has the following obſervations: I conſider Hoffman, as the original and great author of the revolutions in the theory of medicine, for theſe laſt fifty years paſt. The Boerhaavean doctrine was then taught, in this place, by the profeſſors, who had all been the pupils of Boerhaave; and it is curious, that the revolution was firſt began here by the ſtudents, ſome of them having got hold of the works of Hoffman. The Boerhaavean doctrine was firſt overthrown, in the Medical Society, fifty years ago; this I have, upon the authority of my father, who was a member of that

ſociety *.

ſociety*. When the mind has once perceived error, in a particular ſyſtem, it becomes ardent in the purſuit of fallacy; for ſomething is then promiſed to the fatigues of labour and inveſtigation. The doctrine of the importance of air, in the functions of the animal œconomy, was gradually ſtealing upon the minds of men, at this time, and in every part of the world it ſeemed to engroſs the general attention†. In proportion, therefore, as

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* The Royal Medical Society of Edinburgh, of which I have the honour of being a member, is one of the uſeful ornaments to this celebrated ſchool of phyſic. Compoſed principally of ſtudents, who have no object in view but the improvement of ſcience, the opinions of profeſſors are canvaſſed with the warmth of youth, and candour of impartiality. Some of the firſt medical characters in the kingdom, have derived their knowledge from this ſociety; and it is with great pleaſure I ſeize the preſent opportunity, of paying my tribute of reſpect and eſteem, to the abilities I have ſeen diſplayed in this learned ſeminary.

† Dr. Prieſtley and Mr. Cavendiſh, in England; Dr. Black, in Scotland; Dr. Macbride, in Ireland; Mr. Scheele, in Sweden; with Meſſrs. Lavoiſier, Foureroy, and others, in France, were all employed in making thoſe beautiful diſcoveries, which have ſince ſo much added to the improvement of ſcience in general.

as the fufpicion of the truth of former fyftems increafed, fo would a greater importance be attached to the doctrine of air, now become more ferioufly the fubject of confideration. In 1778, the late Dr. Hugh Smith, publifhed his Syllabus of Philofophical Lectures, in London, four years after Dr. Prieftley's difcovery of dephlogifticated air. We find, in this work, remarks the moft curious, and an aerial fyftem of phyfic beginning to be formed. The nature of Dr. Smith's theory, is fomewhat analogous to the ideas entertained by Hippocrates, and adopted and enlarged, but confufed by Lifter. He makes no diftinction between the air, except between what he calls vital and atmofpheric, what he thus diftinguifhes: Air rarified, in motion, detained in animal bodies by glandular fecretions, or circulating with the fluids in the vafcular fyftem, permit us to call vital air *. The firft circumftance, which led this ingenious phyfician to the theory of air, was his attention to the caufe of the circulation of the blood. He ftudied Harvey, whofe doctrine, that the motion of the blood was owing

* P. 32.

owing to itſelf, he adopted. But the cauſe of this motion, Harvey did not underſtand, becauſe he knew not the principle; and this cauſe, Dr. Smith pronounces to be vital air.

The hypotheſis of life exiſting in the blood, certainly owes its origin to Moſes, though firſt adopted, philoſophically, by Harvey, who goes ſo far as to ſuppoſe, the blood to be the ſeat of the ſoul. Dr. Harvey's doctrine deprived, altogether, the muſcular fibre, of its ſhare in the functions of the vaſcular ſyſtem. He attributed every thing to the blood; and ſeemed to think, that its motion was owing to a power, inherent in itſelf, without any aſſiſtance from the arterial ſyſtem. Nor is the blood, ſays Harvey, to be called an original and principal part only, becauſe in it and from it, motion, and the beginning of pulſation, ariſe; but alſo, becauſe in it animal heat firſt is bred, the vital ſpirit is produced, and the ſoul itſelf reſides. How infinitely near, adds Dr. Smith, does he approach, and yet does not ſeem to have the moſt diſtant conception, that air was the unknown principle, yet wanting, to produce all thoſe wonderful phænomena, that thus claimed his moſt curious attention? I cannot help confidering this great man, as my valuable and good ally; but we have a formidable opponent to contend with, one who has endeavoured to overturn the whole of Dr. Harvey's theory, in order to eftablifh his own; I mean the celebrated Boerhaave, who abfolutely affirms, that the caufe impelling the blood, from the heart into the arteries, and from the veins into the heart, is not in the blood, but to be fought for in the heart itfelf. At this time, the Boerhaavean doctrine of the circulation, was generally received, throughout the medical world; and Smith laments, that his hypothefis fhould have obtained the preference over Harvey's. From our obfervations, perhaps, it will appear, that the re-action of the veffels, which Boerhaave and his followers make the firft caufe, is, in reality, only the fecondary caufe of the circulation. Both thefe great men, however, obferves Dr. Smith, though they differed fo widely in their theories, were neither of them far from the truth. One difcovered motion in the vafcular fyftem, but knew not the caufe; the other difcovered motion in the vafcular fyftem, and miftook it for the firft

caufe.

caufe. It was neceffary to collect thus much from Dr. Smith's writings, in order to make his doctrine intelligible, which we fhall now briefly examine.

By a feries of experiments, Dr. Smith proved, that vital air, heat, and motion, were infeparable from animal life. Upon this his theory is built, upon this has he founded all his reafonings, and from this are all his conclufions drawn. It will be fufficient to mention his leading facts. Vital air is the material caufe of motion in animal life; it produces its effects by rarefaction from animal heat. His words are curious, and mark a mind attentive to the fubject, though fomewhat confufed, from the want of a clear and diftinct idea of terms. In the degree of heat, proper to animal life, this air muft be in a rarefied and active ftate. The propelling force of air, thus rarefied and confined in the vafcular fyftem, is the firft caufe of the circulation of the blood; and the *reaction* of the vafcular fyftem (his own words) we prefume, to be the fecondary caufe, in conjunction with the former. Here we find a part of that doctrine, adopted by Cullen, and afterwards pub-

 lifhed,

liſhed, in a more viſionary manner, by Brown The re-action of the veſſels was produced, by the diſtenſion cauſed by the rarefied air; or to ſpeak in more modern language, they acted from the ſtimulus of diſtenſion. This mode of expreſſion is certainly proper; for when the blood is driven from the left ventricle into the aorta, conſiderable effect will be produced, both as to reſiſtance and propulſive force, by the elaſticity of the artery, independent of the contraction produced by the ſtimulus being applied to the irritability of the fibre. It is evident, that Dr. Smith imagined, that air was in a diſengaged and free ſtate in the vaſcular ſyſtem, and that it produced its effects upon mechanical principles. This was deviating, very materially, from Mayow, who, although he did not preciſely underſtand, how fluids paſſed from an aeriform to a condenſed ſtate, yet maintained, that air *united* with the blood, and produced its effects from chemical laws. He drew his concluſions, indeed, from a very different combination of facts, the reſult of cloſely connected experiments. Theſe, although juſtly conceived, and admirably executed, were neglected by many, notwithſtanding the ſame idea was entertained and delivered to the Public, under different expreſſions. The vivifying ſpirit of one, with the vital and acid air of others, all meant the fire-air particles of Mayow; and there was only wanting a comparative view of experiments and obſervations, to confirm the beautiful ſyſtem publiſhed by that ingenious author. Recently has this doctrine been revived, in a full and ample manner. The attention of medical men ſeems now principally directed to the agency of air in the human frame; and from the trials that have been made, it would ſeem, that great benefit will reſult to the practice of phyſic.

INTRODUCTION

TO

PART THE SECOND:

COMPRISING THE HISTORY OF THE ABSORBENT SYSTEM.

THE nature and functions of the abſorbent ſyſtem, is a ſubject which ſtill conſiderably engages the attention of the phyſiologiſt; and it is ſuppoſed, that, but lately, this part of our frame has been rightly underſtood. Immediately upon the diſcovery of the lacteals, the office aſſigned to them was the reſult of the idea entertained, of the nature of the circulation. But as ſoon as that great light was let in upon phyſiology, by the immortal Harvey, and when the diſcoveries of Rudbeck, of Bartholin, and of Jolyffe, were known, the firſt abilities were exerted, in explaining this important part of the animated fabric, and the futility of abſorption by red veins, and of the lacteal carrying chyle to the liver, was ſeen and acknowledged.

The diſcovery of the lacteals, by Aſellius, forms an æra in the hiſtory of medicine. But, notwithſtanding that he undoubtedly has a ſole right to this diſcovery, yet we find, that others, about the ſame period, maintained, that they ſaw the lacteals, without knowing of Aſellius's diſcovery. Highmore puts in this claim, but, at the ſame time, gives up the right of diſcovery to Aſellius, as Bartholin ought to have done that of the lymphatics to Rudbeck.* When diſſecting a dog, ſays Highmore, I accidentally ſaw theſe veſſels, without having read of them in any one author; at firſt, I could only trace them to the pancreas, but afterwards, I acquired a more perfect knowledge of them, from Aſellius.

Aſellius

* Circa eadem tempora, in Suecia, ſimiles aquoſos ductus delexit et poſtea deſcripſit Olaus Rudbeck. In Anglia de Jolivio quoque ſuo gloriantur amici. Quin nobiſcum aliis hinc inde viſa ſunt, negare nolim ſed nobis qui primi in arenam deſcendimus, nullus mortalium viam monſtravit. Anat. Renov. p. 622. Upon the ſame grounds, Highmore has a right to the diſcovery of the lacteals.

Asellius discovered these vessels, in brutes, in 1622; and Highmore says, that an Oxford physician mentioned to him, that he saw them in the human subject, in 1637, and that he himself saw them, also, in the human subject, in 1639, and shewed them, distended with chyle, to a number of pupils; but Veslingius, according to Haller, first saw the lacteals in men, in 1634. Mr. Cruikshank, in his ingenious work, points out, that absorption was maintained, but not understood, both by Hippocrates and Galen; and that Herophilus, according to the latter, had seen the lacteals. Highmore mentions the same thing, and quotes a remarkable passage from Fallopius, who certainly saw the lymphatics, from the accurate description he has given. On the flat side of the liver, are certain very small ducts, which terminate in the pancreas,* and neighbouring glands, and which convey a white oleaginous juice. The lacteals then, were first seen by Herophilus; but their nature and functions were not understood by anatomists, until Asellius again held them up to

* Pancreas aselli, the mesentery.

to the notice of the medical world, when Veſlingius, Highmore, and others, notwithſtanding that the exiſtence of ſuch veſſels was denied by the great Harvey, by ſeeing them in men, confirmed the diſcovery. The lymphatics appear to have been firſt ſeen by Fallopius; it is evident, that Aſellius alſo ſaw them, but miſtook them for lacteals. This ſubject remained confuſed, until Rudbeck and Jolyffe again diſcovered them, when Willis, Gliſſon, Bartholin, and others, completed the ſubject. The thoracic duct, according to Mr. Cruikſhank, was firſt ſeen and deſcribed, in 1563, by Euſtachius, a Roman anatomiſt; but this diſcovery was alſo neglected by anatomiſts, till Pecquet and Van Horne tacitly cenſured this neglect, by confirming the truth of it. Such, conciſely, are the revolutions, which the knowledge of the abſorbents ſuffered, for many years, till the middle of the laſt century, when the ſubject underwent a full inveſtigation, as the reader will ſee in the ſubſequent part of this work.

This ſubject, however, ſeems not to have employed much conſideration, for ſeveral years, previous to the publications of Hunter and Monro.

It is well known, that thefe celebrated anatomifts declared themfelves, the difcoverers of the identity of lacteals and lymphatics; that is, that both thefe fects of veffels abforbed from cavities or furfaces, and at times carried the fame kind of fluid, and therefore, in that point of view, were fimilar, in refpect of office; and that, alfo, they were to be confidered as the fame fyftem of veffels, from a fimilarity of ftructure*. Now it will be no difficult matter to prove, that thefe ideas were entertained and adopted, long before Dr. Monro or Dr. Hunter ever thought upon the fubject. Mr. Cruikfhank, in his valuable treatife on this fubject, maintains, that Dr. Hunter ftill has a claim to the difcovery. Having obferved, that Dr. Hunter maintained his claim fo well, that the greater part of thofe who were entitled to hold any opinion, were on his fide, he adds; when Harvey difcovered the circulation of the blood, his opponents firft attempted to prove he was miftaken; but finding that ground untenable, they then afferted it was known, long

* Vide Lect. on Anat. p. 58; and De Vafis Lymph. Valv. p. 103; and Prolegom. p. 8.

long before: Servetus, Columbus, and Cefalpinus, all knew it*. It is very remakable, that thofe obfervations fhould be brought to invalidate arguments, brought againft Dr. Hunter's pretenfions, when we find that gentleman himfelf ftrongly affering, that the difcovery of the circulation was made prior to Harvey. Speaking of abforption, he adds, Gliffon having been quoted, I confidered what he had advanced upon this fubject, and had the pleafure and mortification to find, that he gave exactly the fame account, both of tranfudation and abforption; fo that I could no longer call it, what I really believed it to be, a new opinion, but Gliffon's revived and confirmed: for in him it was mere opinion, and accordingly was overlooked or rejected, by his fucceffors, as happened

* Introduction, p. 3. We have been unable to procure the works of Servetus and Cefalpinus. The anatomy of Columbus is in our poffeffion; and he certainly makes fome ftriking obfervations, on the circulation of the blood, through the lungs: but notwithftanding this, he maintained, that fanguification was performed by the liver, according to the Galenic doctrine. At fome future day, if time will permit, it is our intention, to examine minutely the doctrines that were entertained, refpecting the circulation, previous to Harvey.

pened to the doctrine of the circulation, in the writings of Servetus and Cesalpinus*. But further, in his introductory lectures to anatomy, we find the following observations: Servetus first, and Columbus afterwards, had clearly given the circulation of the blood through the lungs, which we may reckon, at least, three-quarters of the discovery; and Cesalpinus had, many years before Harvey, published, in three different works, all that was wanting to make the circulation quite complete†. Having mentioned the great merit which is due to Columbus, for the discovery of the Western Hemisphere, and to Copernicus, for that of the Solar System, he adds; In merit, Harvey's rank must be comparatively low indeed. So much had been discovered by others, that little more was left for him to do, than to dress it up into a system; and that, every judge in such matters must allow, required no extraordinary talents. Yet easy as it was, it made him immortal: but none of his writings shew him to have been a man of uncommon abilities‡. It is unnecessary for me to

* Med. Comment. p. 61.

† P. 44. ‡ P. 47.

me to uſe any argument, to prove the comparative merits of Harvey: it is foreign to my purpoſe. As Mr. Cruikſhank aſſerts Dr. Hunter's right to the diſcovery of the lymphatics being abſorbents, I ſhall chiefly confine my obſervations to his work, particularly too, as containing the opinions of Dr. Hunter; and all the arguments and obſervations will be equally applicable, to whatever ſhare of diſcovery Dr. Monro may have claimed, in this important branch of phyſiology. It is, by no means, our intention, to enter into the diſpute between Dr. Hunter and Dr. Monro: our object is to ſhew, that the doctrine of tranſudation and abſorption, was a poſition maintained by the firſt diſcoverers, and conſiderably improved by the anatomiſt who immediately ſucceeded them.

We flatter ourſelves, no apology is neceſſary to the gentlemen, whoſe doctrines we have ſo freely canvaſſed. The moſt oſtenſible modern author, on the ſubject of the abſorbents, is Mr. Cruikſhank, whoſe liberality will not permit him to ſuppoſe, we had any thing elſe in view, than a candid and impartial enquiry, by the freedom of our ſtrictures on his opinions.

PART THE SECOND.

SECT. I.

Of ABSORPTION *by* RED VEINS.

BEFORE we proceed to examine, what different opinions eminent anatomifts maintained, as to the office of lacteals and lymphatics, let us firft fee, what ideas they had, as to abforption by red veins. Bartholin ftrenuoufly maintains, that no abforption takes place by the meferaic veins. His words are. (A) Their ufe, according to the antients, is to fuck up chyle from the inteftines, and to carry it, by the trunk of the vena portæ, to the liver; but the chylous fluid is never found in thefe veffels, they being always filled with blood: moreover, the difcovery of the lacteals and lymphatics, difprove fuch an abforption. That chyle is abforbed by the lacteals alone, is proved, by an experiment of Barbett. He paffed ligatures under fome part of the inteftines of a living dog: between the ligatures,

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the mesenteric vessels, both red and white, came into view; some of these he tied with a thread, and afterwards inflated the intestine with force, through a pipe; but he found, that not the red veins, but only the lacteals became turgid *. And a little lower down, he adds; (B) ' Hence the mesenteric veins can absorb nothing from the intestines. Riolan, however, asserts, that in obstructions of the lacteals, the chyle must, from necessity, be carried through these vessels; but this is by no means the case. Nor do these veins open into the intestines; for then the blood would be effused, and, in my opinion, nourishment would rather cease, as happens in lientery, from obstruction of the lacteals.' When speaking of the lacteals, he observes, (C) ' Their proper use and action is to distribute chyle to the liver, according to Asellius; but rather to the receptacle, and through the thoracic duct to the heart, as Pecquet and ourselves have observed; not by the meseraics, as heretofore believed, through which there is neither a distribution of chyle to the liver, nor of blood to the intestines.' He combats, by powerful arguments,

* This experiment did not succeed with Lower;—vide infra, Sect. III.

guments, the experiments which De Bils had made, to prove abſorption by the meſenteric veins; and obſerves, that from their ſtructure, it is impoſſible for them to abſorb. (D) 'The meſeraic veins are ſo conſtructed, that they cannot take up chyle by their mouths.' In Walœus we find the following obſervations: (E) 'Chyle is never obſerved to enter any vein in the ſtomach, nor any meſenteric vein. The chyle is carried through veſſels, ſuited to the purpoſe rather than through the meſeraic, which contain blood; for then it would be neceſſary, that theſe veins ſhould open into the inteſtines, by which the blood would eaſily eſcape.' We find, alſo, another celebrated anatomiſt, making direct experiments, to diſprove the abſorption by red veins. (F) 'And this is the only way, ſays Lower, through which the chyle paſſes, from the ſtomach and inteſtines, to the blood and heart. Becauſe ſome have fallen into the ſame error with the antients, and ſtrenuouſly maintain, that the meſeraic veins take up chyle from the inteſtines, I have paid particular attention to this ſubject, that I might be certain of the fact; and I at length found, not from one experiment only, that the whole ſupply of chyle is thrown into the blood, by no other paſſage but the chyliferous ducts; for if its paſſage through theſe veſſels be obſtructed, the animal, with whatever kind of food it may be fed, will die in a few days from famine, as I have experienced in two dogs. He lacerated the thoracic duct, and obſerved, that notwithſtanding the ſtomach and inteſtines were diſtended with food, and the lacteals with chyle, yet the dogs died. He was aware, he ſays, that the thoracic duct was ſometimes double; he, therefore, made the inciſion high enough up, in order to be above the junction: he found the chyle effuſed in conſiderable quantity into the thorax from the lacerated duct; (G) An evident and clear proof, he adds, that the chyle does not enter by the meſeraic veins, and that it has no other way by which it is carried to the blood, ſince the animal is ſo certainly deſtroyed, if the paſſage through the thoracic veſſels be cut off.' Surely no candid mind can refuſe its aſſent to the convincing arguments drawn from theſe experiments. It can be no violence to reaſon to obſerve, that both Bartholin and Lower ſaw the *neceſſity* for different functions, in the abforbents and red veins. They beheld and admired the effectual fimplicity of nature, in the animal œconomy; hence they reafoned on the futility of red-vein abforption; and hence they maintained, with a firmnefs which always attends the affertion of truth, and proved by direct experiments, their opinions to the contrary.

In Diemerbroeck, too, we find a ftrenuous affertor of the non-abforption by red veins. When fpeaking of the different ufes that have been affigned to the mefenteric veins, and the objections ftarted againft them, he adds; (H) 'Plempius, perceiving thefe difficulties, explains the fubject otherwife, and fays, that the arterial blood remaining after the nourifhment of the inteftines, flows back by the meferaic veins to the porta, and along with it the chyle. But I wifh Plempius would demonftrate to us, the way by which the chyle enters thefe veins. It is neceffary for the reception of the chyle, that they fhould, in fome manner, open into the inteftines; if this is the cafe, why does not the blood flow through thefe apertures into the inteftines? and again, in the following paffage, his expreffions are remarkably ftrong.

 (I) Befides,

(I) 'Befides, no one could ever fee chyle, nay not any thing which bears the fmalleft refemblance to chyle, in the meferaic veins, but the white chyle is always found in the lacteal and other chyliferous veffels, which were conftructed by the Supreme Architect *to this exprefs purpofe, viz. that they fhould convey the chyle.*' He mentions the experiments of De Bils, who feparated from each other, in a living dog, the mefenteric veins and arteries, and paft ligatures round the latter, which prevented any blood from paffing from the arteries into the veins. He then fewed up the abdomen, and preferved the dog, until he imagined the food, which he had previoufly given the animal, was converted into chyle. He then found the arteries empty, but difcovered the veins filled with a muddy fluid, of a cineritious colour. This experiment of De Bils greatly excited the attention of the phyfiologifts. Some of the firft medical characters of the age entered the lifts, and the difpute was carried on with that acrimony which has, too often, difgraced literary controverfies. (K) All the Bilfianifts, fays Diemerbroeck, regard this experiment as miraculous, and afcribe fo much

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to it, that they are aſtoniſhed if any one ſhould dare to contradict it; moreover, they revile, with the moſt baſe and ſharp contumelies, language unworthy literary characters, thoſe who oppoſe the opinion.

The names of Steno, Bartholin, and Diemerbroeck, are found among the diſputants; and the latter, after having brought forward the experiments and opinions of others againſt red-vein abſorption, cries out exultingly: (L) 'We ourſelves have repeatedly collected blood from the meſeraic veins, and at that time, too, when all the lacteals were replete with chyle, and have compared it with the blood of other veins; but we were never able to diſcover the ſmalleſt difference, in colour, thickneſs, or coagulation. The ſame, alſo, was taken notice of by that moſt accurate obſerver, Nicolaus Steno'. Van Horne, too, one of the diſcoverers of the thoracic duct, did not believe in abſorption by red veins; and Bartholin, in his Structura nova Diaphragmatis, mentions ſome authors who maintained ſuch an opinion, and refutes their arguments*.

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* Notwithſtanding theſe unequivocal experiments and obſervations,

(A) Usus, secundum antiquos, chylum exsugere ex intestinis, et per truncum venæ portæ in hepar deferre; sed nunquam in his lacteus chyli succus repertus, semper sanguine refertis: adhæc inventio venarum lactearum et lymphaticarum isti repugnat. Per solas lacteas chylum sugi hoc experimento probat Barbettius. Prehensum quâvis parte canis vivi intestinum utrinque ligat: in medio hujus exprompta mesenterica vasa, tum alba tum rubra, aliquot itidem filo nectit: post per adaptatam fistulam fortiter flatum injicit; sic non ligata sanguinea, sed sola lactea intumescere vasa comperit*, P. 602-3.

(B) Hinc

observations, by different eminent anatomists, Dr. Hunter, in his Med. Comment. after relating the experiments of John Hunter, to disprove absorption by red veins, says, here is a *new doctrine* proposed in physiology, *viz.* that red veins do not absorb.

* Thomæ Bartholini Anatome ex omnium veterum recentiorumque Observationibus, &c. Renovata. Lugduni Bat. 1673.

(B) Hinc nihil ex inteſtinis meſaraicæ ſugere poſſunt. Neceſſitatis tamen tempore in omnimoda obſtructione venarum lactearum, per hæc chylum deferre concedit Riolanus, ſed nullo argumento. Nec enim hiant in inteſtina, alioquin effunderetur ſanguis, et meo judicio, ceſſaret potius nutritio, quod in lienteria videre eſt, illis obſtructis. Ibid. P. 603.

(C) Actio et uſus propius eſt chylum per ἀνάδοσιν ad hepar diſtribuere juxta Aſellium; ſed ad receptaculum verius, indeque per thoracicas lacteas ad cor, ſecundum Picqueti noſtraſque obſervationes; *non per meſaraicas*, ut hactenus creditum, per quas nec fit chyli ἀνάδοσις ad hepar, nec διάδοσις ſanguinis ad inteſtina. Ibid. p. 613.

(D) Venæ meſeraicæ ita ſunt comparatæ, ut chylum per oſcula ſua non recipiant. Ibid. P. 606.

(E) Nunquam autem chylus animadverſus eſt venam ullam in ventriculi corpore, nec ullam meſaraicam intrare. P. 765*. Per peculiares autem

 venas,

* Epiſtolæ Duæ de Motu Chyli et Sanguinis; annexed to the Anatome Renovata.

venas, potius quam per mesaraicas sanguinem continentes, defertur chylus; quod mesaraicæ sanguinem admissuræ, aperiri osculis in intestina debuissent, per quæ facile sanguis in intestina fuisset elapsus. Ibid. P. 789.

(F) Atque hæc via unica est, qua chylus e ventriculo et intestinis, in ipsum sanguinem et cor infunditur. Verum quia nonnulli in eodem cum veteribus errore etiamnum versantur, venasque mesaraicas chylum ex intestinis excipere confidenter statuant, ipse ut de hac re certior fierem, seriam aliquando impendi operam; atque non uno experimento tandem mihi constitit, totum chyli penum *nulla alia via* quam per ductus chyliferos, in sanguinem infundi; si enim cursus ejus per vasa thoracica impediatur, arimal qualicumque cibo satiatum, intra paucos dies fame penitus interibit, quod in duobus canibus expertus sum. P. 153-4 *.

(G) Claro satis argumento, chylum per venas mesaraicas non intrare, neque ullam aliam dari viam qua sanguini misceatur; cum animal, ex transitu

* Tractatus de Corde item de Motu et Colore Sanguinis et Chyli in eum transitu; Authore R. Lower, M. D. Coll. Med. Lond. Socio. Londini, 1680.

tranſitu per vaſa thoracica impedito, tam certò intereat. Ibid. P. 155.

(H) Plempius, has difficultatés percipiens, rem aliter explicat, et dicit, ſanguinem arterioſum, a nutritione inteſtinorum reſiduum, per venas meſaraicas refluere ad portam, eique chylum ex inteſtinis adjungi vel permiſceri. Sed velim nobis demonſtret Plempius, vias per quas chylus venas illas ingreditur. Illæ enim ut chylum recipiant neceſſario debent alicubi in inteſtinis aperiri; at quare, dum aperiuntur, ſanguis, qui tenuior et ſpirituoſior eſt chylo, per iſtas aperturas non effluit potius in inteſtina? P. 724-5 *.

(I) Præterea, nemo unquam chyli, imò ne minimam quidem chyli ſimilitudinem, in venis meſaraicis obſervare aut videre potuit; ſed chylus albicans ſemper invenitur in vaſis lacteis, aliiſque chyliferis, *eum in finem*, ut *chylum veherent*, a Summo Architecto ſpeciatim conditis. Ibid. P. 725.

(K) Hoc experimentum omnes Bilſianiſtæ, tanquam miraculoſum admirantur, idque tanti faciunt,

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* Iſbrandi de Diemerbroeck Anatome Corporis Humani. Genevæ, 1679.

unt, ut obſtupeſcant, ſi quiſpiam aliquid contradicere audeat; imo turpiſſimis acerrimiſque contumeliis ac convitiis, viris doctis indigniſſimis, quoſcunque contradictores proſcindant. Ibid. P. 725.

(L) Nos enim non ſemel ſanguinem e meſaraicis collegimus, etiam eo tempore, quo omnes lactei ductus chyloſo ſucco turgebant, eumque cum aliarum venarum ſanguine contulimus; ſed nullum manifeſtum diſcrimen in colore, ſubſtantia, aut coagulatione, animadvertere potuimus. Illud ipſum etiam obſervavit oculatiſſimus Nicolaus Steno. Ibid. P. 726.

SECT.

SECT. II.

ORIGIN *of* LACTEALS *and* LYMPHATICS.

It appears, from reading the works of thofe who have written upon the abforbents, that, notwithftanding fome of them maintained, that the lymphatics were continued from arteries, many, alfo, fupported the opinion, that they arofe from furfaces and cavities. Mr. Cruikfhank, in feveral parts of his work, maintains, that Bartholin knew nothing of the origin of the lymphatics. I agree with him in thinking, that he fometimes had his doubts; but fhall we fay, becaufe he hefitated, that he therefore was totally unacquainted with it? But he feems to be tolerably clear, on this fubject, in fome places. (A) ' The origin of the lymphatics is from all parts, and the vifcera, from which lymph is *feparated*, the liver, the gall-bladder, the fternum, mediaftinum, pericardium, heart, lungs, inteftines, ftomach, fpleen, uterus, tefticles, &c. The

ufe

ufe of all the lymphatic veffels is, to carry a limpid fluid or lymph; the lymph is produced from all parts, throwing out a fuperfluous ferum, or from the arteries abounding with lymph.' Can any pofition be more clearly ftated? He firft lays it down, as certain, that the ufe of the lymphatics is, to carry lymph; and he then fays, that the lymph is thrown out, upon different parts. What conclufion are we now to draw? It is fo evident, that it would be doing violence to common fenfe, to mention it. He afcribed to the lacteals and lymphatics, the abforption of medicines, poifons, &c. hence, he fays, we may account for many difeafes, as dropfy, &c. and we can now fee, he adds, the caufe of the confent between the ftomach and heart. He obferves, that thefe veffels ferve to complete the human frame; and concludes, that as to the difeafes he has mentioned, he will treat of them fully, in a book, De Morbis Lymphaticis, a work we have never feen, but which, it is very probable, contains many obfervations and opinions, fuppofed to be modern. In a work publifhed by Bartholin, after the Anatome Renovata, which came into our hands after the above was written, he gives a moft decided opinion, that the lymphatics are not continued from arteries, by continuity of canal. (B) 'Moreover, although we aſſert, that the lymphatic veſſels receive their contents from the arteries, *we by no means allow of a direct anaſtomoſis of the lymphatics with the arteries*, except through the medium of cells, or ſome other ſtructure, into which the blood being effuſed (a curious idea) and having ſupplied to the nutrition of parts and ſecretion of fluids, the lymphatics return the lymph, the veins the blood.' Other celebrated anatomiſts paid particular attention to the ſame doctrine, and by their expreſſions, we cannot doubt, but that they had a clear and adequate notion of the ſubject. Paulus, the editor of Van Horne's Opuſcula, has the following words, when ſpeaking of the liver; (C) 'Indeed thoſe lymphatics, as well as the others of the whole body, do not ariſe ſo much from membranous parts, as from the interſtices where the arteries terminate.'

It was the opinion of Steno, Ruyſch, and Malpighi, that the lymphatics aroſe from glands; and this opinion is adopted by Diemerbroeck. Bartholin denies, that they ariſe ſolely from glands. (D) 'My honoured father, he ſays, who

firſt

firſt gave name and eternity to the veſſels diſcovered by him, believed, that the lymphatics returned the lymph into the blood, from all parts, as well glandular as the extreme joints.' He mentions, that they had been ſeen coming from parts not glandular; and that many laughed at De Graaf, for maintaining, that he had ſeen them upon the uterus. This author, in ſpeaking of the lymphatics of the teſticles, has the following words. (E) 'The lymphatic veſſels, which the curioſity of our age has ſo admirably diſcovered in the teſticles, are ſeen not only to come out from the tunica albuginea, but alſo from the internal ſubſtance of the teſticles.' Theſe lymphatics, as well as the reſt, he adds, have numerous valves to prevent the return of the lymph to the teſticles, and alſo to cauſe it to be propelled, with more eaſe, to the receptaculum chyli. Reverhorſt gives a very particular and accurate account of the lymphatics of the liver, of which, he ſays, there are two ſets; the deep ſeated and the ſuperficial. He mentions, that before they quit the liver, they form themſelves into ſix, ſeven, or eight large trunks, and ſometimes more, paſſing along both ſides of the ſuſpenſory ligament. A circumſtance

cumſtance which he mentions, clearly proves, that the lymphatics could not be continued from the arteries, by continuity of canal. (F) Theſe lymphatics, he ſays, both deep ſeated and ſuperficial, tranſmit lymph, very different from that of the other lymphatics, becauſe it is impregnated with bile, and therefore may be deſervedly called bilious lymph. This is alſo taken notice of by Kirchringius, as quoted by Bartholin, who mentions it to prove, that the lymphatics are not continued from the arteries. (G) This is alſo proved for us by Kirchringius, who, becauſe he ſaw the lymph tinged of a yellow colour, in a lymphatic creeping along the top of the gall-bladder, and perceived a taſte of bile, concluded, that this bitter lymph was taken up, *without the veſſels of the arteries*; becauſe if there were an anaſtomoſis between the arteries and lymphatics, the lymph would not be found bitter, but ſweet, as it is detected in the arteries*.

The ingenious Thruſton, when ſpeaking of the nouriſhment of the fœtus in utero, in the exami-

* De Graaf gives a very neat plate of the uterus, with a deſcription of the lymphatics running along it.—De Mulier. Organ. P. 247.

examination for his degree at Cambridge, ſays, that the fluids, in which it is immerſed, paſs through the ſkin. Dr. Needham, the profeſſor, queſtioned him, as to this particular, to which he gave the following anſwer; (H) ' Nor do I think that you will deny, that ſomething paſſes through the ſkin, in the capillary veſſels, and thus mixes with the blood. I believe, that you (ſo long and ſo ſucceſsfully employed in the practice of phyſic) not unfrequently order fermentations; ſuppoſe you are preſcribing for ſchirroſities of the abdomen, can you doubt, but that ſome part of them enter through the ſkin, into the ſanguiferous veſſels? Here we find abſorption maintained to be performed by the cutaneous veſſels. The diſcovery of the lymphatics was made ſeveral years previous to this period; and the doctrine, that fluids were removed by them, had been for ſome time mentioned and acknowledged: it is probable, therefore, that Thruſton might imagine, this office to be performed by the cutaneous abſorbents; although I do not deny, that his words will bear a different interpretation. If a candid reader, therefore, puts a different conſtruction upon the paſ-

ſage,

fage, we willingly refign our idea, as our object is, not to fupport, unjuftly, the pretenfions of any one man, but to promote, by difpaffionate enquiry, the knowledge of truth.

In a work publifhed a few years after the Anatome Renovata of Bartholin, we find an excellent hiftory of the abforbent fyftem.* Zellerus, the author of this publication, gives the following unequivocal account of the origin of the lymphatics. (l) 'Some believe, that thefe veffels come out from nourifhed parts; others hail all the greater conglobate glands, as the fountain of all of them; others, again, deny this, fince they are found in parts, as the lungs, where no glandular ftructure obtains. Almoft *all of them are in the right.* We pronounce the glandular membrane, which is extended over all the mufcles, in a word, over all parts, and the vifcera as *their commencement*, *fountain*, and *origin*, *fince we may fee them arifing from all parts*, as well from the

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* Bartholin's work came out in 1674; Zellerus's account of the lymphatics is publifhed with the Difputationes Anat. Select. by Waller. Mr. Cruikfhank fays, he never faw Zellerus's publication.

conglomerate as conglobate glands, from the liver, pancreas, tefticles, the vifcera, from the joints, the abdominal lumbar, and intercoftal mufcles, from the diaphragm, pleura, pericardium, and membranes of the brain.'* (I) Long, too, before the time of Zellerus, Van Horne maintained, that abforbents were diftributed over the whole fyftem. (K) The diligent inveftigation of our more recent anatomifts has taught us, that there is fcarcely any part of our body, which wants thefe veffels; for they are found in the lungs, mediaftinum, ftomach, fpleen, teftes, uterus, feptum narium, eye-lids, palate, cheeks, &c.

The feveral paffages here felected, point out, beyond a doubt, that fome of the firft difcoverers of the abforbents, knew their general extent and influence over the whole fyftem. They fhew clearly, too, that the phyfiologifts of the laft century

* Dr. Morelli, Profeffor of Medicine at Padua, and who ftudied for five years under Mufcagni, mentioned to a friend of mine, that he has frequently feen lymphatics, running between the arachnoid coat and pia mater of the brain.—Vide Nuck's Epift. Anatom. P. 150; where he mentions, that he has feen lymphatics paffing from the pineal gland.

tury knew, that the lymphatics arofe from cavities and furfaces, and that they were not returned from the arteries, by continuity of canal; yet Dr. Hunter maintains, that this was the opinion entertained by anatomifts fince the difcovery of the abforbent fyftem, notwithftanding we find Bartholin himfelf, fo anxious and careful in af-ferting the contrary doctrine.

SECT. III.

ORIGIN *of the* LACTEALS.

There could be but one opinion, as to the part whence the lacteals derived their origin; but the *manner* in which they proceeded, from their commencement, formed a fubject for difpute, which, by the way, points out, how much the authors of laft century were occupied with this part of phyfiology. Lower imagined, that the lacteals arofe obliquely from the inteftines. Having firft fed a dog, he collected air in that portion of the jejunum, where ileum commences, and having applied ligatures above and below, he found no air in the lacteals with the chyle, after repeated preffure with the hand, before he untied the ligatures. He repeated this experiment, he fays, but made ufe of a coloured fluid inftead of air; fpiritus vini atramento tinctus; the refult, however, was the fame: hence, he concludes, (L) 'from this we have reafon to think, that the lacteals do not open directly and immediately into the inteftines, but run obliquely among their coats, before they penetrate the inteftinal cavity; in the fame manner, perhaps, as the ductus communis choledochus enters the duodenum, or the ureters the urinary bladder.*

This is the opinion of Lower. Whether it has been confirmed or refuted by modern experiments, I cannot pretend to fay; but it appears, that fuch an oblique commencement is contrary to the defign of Nature, by throwing obftacles in the way of a ready abforption. The numerous mouths which the lacteals prefent in the villi of the inteftines, fhews, how careful Nature is, to promote the grand and neceffary office of thefe veffels, *viz.* the abforption of chyle: and the beautiful defcription, which the accurate Cruikfhank has given, of the numerous mouths which he faw, deferves the attentive perufal of the phyfiologift.†

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* Vide the Experiments of John Hunter, as related in his Brother's Med. Comment. P. 38. The reader will, probably, be furprifed at experiments fo fimilar.

† Vide Chap. xi. of the Anatomy of the Abforbing Veffels.

Other writers, previous to this ingenious anatomift, have given defcriptions of the origin of the lacteals, as if they faw their mouths; but as they do not pofitively affert it, we cannot declare for them. (M) 'They (the lacteals) are inferted, fays Bartholin, into all the inteftines, into which they open with their mouths, furrounded with a cruft of mucous matter; and leaft the chyle, once received, fhould return into the inteftines, they are befet with valves.' Diemerbroeck, too, in his anatomy, gives a fimilar, but more accurate defcription, of the manner in which the lacteals take their origin. (N) They arife, fays he, from the inteftines, between the coats of which they open into their interior cavity, by numerous very fmall terminations, their mouths being concealed under a kind of fpongy mucus.' This fpongy mucus, was probably the ampullula of Liberhuhn, and the villi fo often mentioned by anatomifts.

It is ufelefs to give quotations from more authors, on this part of our fubject; they have arranged their ideas in almoft fimilar terms, which convey to us, that the doctrine generally adopted, was that which we have juſt now mentioned. They form deſcriptions, however, very analogous to thoſe we find given in modern books, on this ſubject, which oblige us to agree, if they did not actually ſee the mouths of the lacteals, that their ideas are very accurate.*

* Vide Hewſon, Cruikſhank, Monro, &c.

SECT. IV.

Structure *of* Lacteals *and* Lymphatics.

The abſorbents are, every where, deſcribed by the firſt diſcoverers, as tranſparent, pellucid veſſels, carrying a limpid fluid; hence they have been called by the different appellations of vaſa cryſtallina, rorifera, ſeroſa, &c. This pellucid appearance aroſe, it was imagined, from their delicate ſtructure; and from this circumſtance, they are mentioned as poſſeſſing but one coat. (O) 'The lacteals, ſays Diemerbroeck, are ſlender, pellucid veſſels, having but one coat; and being diſperſed, in infinite numbers, over the meſentery, are deſtined to carry the chyle.' This error, however, was ſoon corrected, by the ingenious Nuck, who ſeparated the thoracic duct into two coats. He gives the following deſcription of a lymphatic. (P) 'The membrane compoſing a lymphatic veſſel, when examined by the naked eye, appears pellucid

pellucid and flender, and the external fenfes can difcover no peculiar ftructure, on account of the minutenefs of the parts. But examined by the microfcope, it prefents a texture, which exhibits an infinite number of globules, great and fmall, in contact with each other; fome of which feemed to form fmall, but irregular ducts, united by various anaftomofes, like the mefhes of a net; but whether thefe lines were veffels or not, I cannot for certain determine.'*

The moft important part, in the ftructure of the abforbents, are the valves, which are more numerous in thefe veffels, than in any other of the human body. Thefe, too, notwithftanding their minutenefs, did not efcape the inveftigation of the anatomifts, who were acquainted with the abforbents. (Q) 'They are knotty, fays Bartholin, and appear externally unequally fwelled, efpecially if the flow of the lymph be ftopt by ligature; this happens, on account of the valves, which are not only affixed at the entrance into the axillary vein, but are alfo *difpofed, every where*

* Were thefe not the vafa vaforum, which Nuck faw? confounding them probably with the fibres of the lymphatic.

where double, and in pairs, through all their branches; are very ſlender in their ſtructure, and more numerous than in the veins, ſo much ſo, that ſome anatomiſts have demonſtrated above two thouſand in the lymphatics; their uſe is, to prevent the retrograde motion of the lymph.' So minute a deſcription, could have been the reſult only of laborious attention; and ſo indefatigable was their induſtry, that they attempted even to enumerate them. It is, notwithſtanding, aſſerted by Mr. Cruikſhank, that the diſcoverers of the lymphatics were not acquainted with their valves. ' One is ſurpriſed to find, he ſays, that one of the firſt diſcoverers of lymphatics was almoſt *entirely ignorant* of the valves; and that, after the veſſels themſelves had been deſcribed, both by Bartholin and Rudbeck, the dilucidatio valvularum ſhould have been left for Ruyſch.'* It is very certain, that

* P. 63. Mr. Cruikſhank here quotes a paſſage, from ſome work of Bartholin, in which he ſays, valvulæ, ob ſubtilem contextum, cultro anatomico ſeparari non poſſunt. This is almoſt directly contradictory to the one we have ſelected, and in which he deſcribes the valves ſo well. We could

that Ruyſch was the firſt who publiſhed, ſeparately, upon the valves; but ſurely he was not, on that account, the firſt who knew any thing of them: on the contrary, it appears, that Bartholin was as well acquainted with their ſtructure and uſe, as Ruyſch himſelf. This laſt anatomiſt, too, cenſured Rudbeck, for being ignorant of the valves; but, according to Mr. Cruikſhank himſelf, he is very diſtinct and clear upon the ſubject. Gliſſon, alſo, who wrote long before Ruſych, but after Rudbeck, and to whom Jolyffe firſt mentioned his diſcovery of the lymphatics, thus ſpeaks of the courſe of the lymph: (R) 'It clearly appears, that this fluid flows from the liver, and by no means towards it; this is alſo evidently proved, by the ſtructure of the valves, which are very numerous in theſe veſſels; they yield a ready paſſage to the fluid, coming from the liver, but altogether prevent its return.'

Any one who ſees a turgid abſorbent, muſt obſerve conſiderable inequalities in its ſurface.†

So

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could not find the paſſage advanced by Mr. Cruikſhank, as he has given no references to the authors he has quoted.

† It is not neceſſary to open the veſſels, in order to

So ftriking and unufual a ftructure, muft have arrefted the attention, and have induced the enquirer to examine into this curious appearance; and a fimple diffection would foon explain the caufe. Accordingly, we find the valves mentioned, and accurately defcribed, by the firft difcoverers, and the braided, or *nodulated* appearance, is delineated, in the engravings of Bartholin, Willis, Gliffon, Reverhorft, and innumerable others.

(A) Exortus lymphaticorum vaforum eft ab omnibus partibus et vifceribus, ex quibus lympha feparatur, hepate nempe veficula fellis, fterno, mediaftino, pericardio, corde, pulmonibus, inteftinis, ventriculo, liene, utero teftibus, &c. Ufus vaforum lymphaticorum omnium eft, liquorem limpidum feu lympham vehere; origo lymphæ

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difcover the valves; they are fufficiently marked on the outfide, and give the veffels the appearance of being notched or jointed, in different places. The lacteals are, generally, ftrongly marked in this way, which makes them, at times, refemble a ftring of beads. Anatom. of Abforb. p. 65.

phæ eſt a partibus ſingulis ſuperfluum ſerum remittentibus, vel ipſis arteriis, lymphâ abundantibus. Anat. Renov. P. 723-4.

(B) Cæterum quamvis vaſa lymphatica humorem ſuum ab arteriis accipere aſſerimus, non tamen lymphaticorum cum arteriis immediatam ponimus anaſtomoſin, niſi mediantibus cribris, vel aliis corporis partibus, in quæ ubi ſanguis effuſus, partium nutritioni et humorum ſecretioni proſpexerit, vaſa lymphatica lympham, venæ ſanguinem revehunt. P. 99-100*

(C) Etenim iſta lymphatica, æque ac reliqua totius corporis, non tam a membranis provenire, ſed vel ab interſtitiis, ubi arteriæ terminantur, &c. P. 68†.

(D) Parens meus venerandus, qui primus et nomen et æternitatem dedit vaſis a ſe inventis, lymphatica ab omnibus partibus, tam glanduloſis quam

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* Caſpari Bartholini Thom. F. Diaphragmatis Structura Nova. Pariſiis, 1676.

† Johannis Van Horne, Opuſcula Anatomico-Chirurgica, cum Annotationibus, &c. ad aucta ſtudio D. Johannis Gulielmi Pauli. Lipſiæ, 1707.

quam extremis artubus lympham, ad ſanguinem referre credidit. Struct. Nov. Diaphrag. P. 89.

(E) Vaſa lymphatica, quæ admodum dextrè in teſticulis detexit noſtri ſæculi curioſitas, non ſolum provenire nobis videntur, ſed etiam ab internâ teſticulorum ſubſtantiâ. P. 35*.

(F) Vaſa hæc lymphatica, tam interna quam externa, longe etiam et a reliquis lymphaticis diverſam vehunt lympham, utpote particulis bilioſis magis volatilibus gravidam, ideoque meritò lympham bilioſam dicendam. P. 17†.

(G) Quod etiam nobis confirmat Kirkringius, qui, quia in vaſe lymphatico, per ſummam fellis cyſtidem reptante, lympham ſubflavo colore tinctam conſpexit, et fellis ſaporem guſtu percepit, concludit lympham hanc amariorem extra arteriarum vaſa delatam fuiſſe; quia ſi vaſorum lymphaticorum cum arteriis eſſet anaſtomoſis, non amara reperitur ſed dulcis, qualis in arteriis deprehenditur. Struct. Nov. P. 100.

(H) Verum

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* Regneri De Graaf Opera omnia. Lugd. Batav. 1677.

† Diſſertatio Anatomico-Medica de Motu Bilis Circulari ejuſque Morbis; Auct. Mauritio Van Reverhorſt, Medic. Cand. Lugd. Batav.

(H) Verum neque tu inficias iveris opinor, aliquid per cutem in vaſa capillaria tranſire poſſe, adeoque ſanguini commiſceri. Credo te (in praxi medica feliciter ac diu occupatum) fotuum uſum non raro imperaſſe: puta dum ſchirroſis tumidiſque abdominibus opem adferres, dubitaſne igitur, an aliquid eorum per cutim in vaſa ſanguifera penetret? P. 153.*

(I) Nempe crediderunt nonnulli, e partibus nutritis emergere iſta; alii glandulas omnes majores conglobatas primam omnium eorum ſcaturiginem ſalutarunt; alii hæc poſteriora negarunt, cum in partibus, pulmonibus ſcilicet, reperiuntur, ubi tamen nulla glanduloſa ſtructura notata fuerit. Veritatem dicunt fere omnes. Membranas, ut glanduloſas, omnibus muſculis, verbo omnibus partibus et viſceribus ſuper extenſas, tanquam *principium*, fontem, ac *originem*, declarabimus, cum illa æque ex conglomeratis ac conglobatis, hepate, pancreate, teſtibus, viſceribus omnibus, ex artubus, abdominis lumborum, et intercoſtalium muſculis, diaphragmate, pleura, pericardio, ac meningibus,

* De Reſpirationis Uſu primario Diatriba; Auct. Malachia Thruſton, M. D. Lugd. Batav. 1671.

ningibus, *verbo omnibus partibus egredi videamus.* P. 819*.

(K) Imo recentiorum docuit diligentia, vix ullam noſtri corporis partem, iiſdem carere; nam et in pulmonibus, mediaſtino, ventriculo, liene, teſtibus, utero, narium ſepto, palpebris, palato buccis, &c. reperiuntur. Opera Omnia. P. 73.

(L) Unde opinari licet, venas lacteas non directe et immediate in inteſtina hiare, ſed obliquè inter eorum tunicas ferri, antequam in cavitates inteſtinorum penetrent; non aliter, forſitan, ac ductus communis in duodenum, aut ureteres in veſicam terminantur. De Corde. P. 150.

(M) Inſeruntur in omnia inteſtina, in quæ oſculis ſuis hiant, mucoſæ materiæ cruſtâ oblinitis; et ne ad inteſtina verſus relabatur chylus, ſemel acceptus, donatæ ſunt valvulis. Anat. Renovat. P. 612.

(N) Exſurgunt ab inteſtinis, inter quorum tunicas plurimis tenuiſſimis radicum finibus verſus interiorem eorum capacitatem hiant, oſculis ſub ſpongioſo quodam muco latentibus. Anatome. P. 67.

(O) Sunt

* Diſſertatio Anatomica de Vaſorum Lymphaticorum Adminiſtratione; Auct. Joh. Zellero. M. D. Tubingæ, 1687.

(O) Sunt autem vaſa (lactea) tenuia et pellucida, ſimplici tunica donata, per meſenterium diſperſa, numero infinita, chylo vehendo dicata. Anatome, P. 67.

(P) Membrana vaſculum lymphaticum componens, externo oculo examinata, pellucida eſt et tenuis, nihilque peculiare (quoad ſtructuram) ob partium tenuitatem ſenſibus externis offert. Verum microſcopio examinata, texturam præbet infinitorum globulorum, majorum minorumque, contactu mutuo inter ſe cohærentium; quorum nonnulli ductulos aliquot, ſed irregulares, arearum in modum, componere videbantur, variis aneſtomoſibus inter ſe junctos; hi autem tractus, an vaſcula fuerint, necne, pro certo, affirmare non auſim. P. 41-2*.

(Q) Nodoſa quoque ſunt, et externe apparent inæqualiter tumentia, imprimis ſi, ligatis illis lympha ſiſtatur, idque propter valvulas, non tantum ingreſſui in axillarem affixas, ſed in omnibus ramis diſpoſitas, geminataſque ubique et oblique invicem ſuperpoſitas, textura tenerrimas et numeroſiores quam in

* Adenographia Curioſa et Uteri Fœminei Anatome Nova, Auctore Antonio Nuck, Med. Doct. Ludg. Batav. 1696.

venis, adeo ut ſupra bis mille in lymphaticis aliqui demonſtrent, quarum uſus eſt impedire ne aqua ad lymphatica regrediatur. Anat. Renov. P. 622-3.

(R) Manifeſte conſtabit, eundem ab hepate, non autem ipſum verſus tendere; idque etiam valvularum ſtructura, quæ numeroſæ in iis reperiuntur, dilucide evincit; illæ ſiquidem liquori ei hepate effluenti facile cedunt, regreſſuro autem ſecluduntur. P. 268 *.

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* Franciſci Gliſſonii, in inclyta Cantabrigiæ Academia Medicinæ Profeſſ. et Coll. Lond. Soc. Anatomia Hepatis. Ad Calcem Operis ſubjiciuntur nonnulla de Lymphæ-ductibus nuper repertis. Londini, 1654.

SECT.

SECT. V.

Identity *of* Lacteals *and* Lymphatics.

When Afellius difcovered the abforbents of the inteftines, he called them lacteals, from the fluids they contained; in like manner, Rudbeck and Bartholin gave the abforbents, in other parts of the body, the name of ferous and lymphatic veffels, for the fame reafon. It was, however, found, that the lacteals fometimes carried lymph, and, therefore, the term lymphatic was equally applicable to them. The appellation of lacteal, however, was retained for the fame reafon, that it ftil continues in ufe, *viz.* to diftinguifh between veffels which receive chyle from the inteftines, and thofe, in other parts of the body, which never contain that fluid. Notwithftanding that this diftinction of name remained, yet we find it mentioned, that the lacteals, occafionally, contained the fame fluid with the other obforbents of the fyftem, and

 therefore,

therefore, were called lymphatics. Mr. Cruikshank obferves, that 'the lymph is the fluid, fuppofed, by the firft difcoverers of the lymphatics, to be found *only* in them, and not in lacteals; and which occafioned their forming thefe veffels into two claffes'*. I confefs, that I do not find, in the Anatome Renovata of Bartholin, any expreffion, by which he unequivocally means, that the vafa lactea ever contained lymph; but he afferts, that the thoracic duct always contains lymph, except when it is diftended with chyle. (A) The receptacle receives the lymph from lymphatics, and through the fame thoracic lacteal ducts, delivers it to the heart. It is always filled with lymph, when no chyle is prefent†. And again, during the time of diftribution, its chief office is to carry chyle, but always at other times lymph. In a work, however, which we have fince met with, publifhed by Bartholin, he afferts, that he has feen the lacteals turgid with lymph. (B) I have more than once lately feen, in animals, opened two hours after they had taken food, the lacteals containing

* P. 95.

† Diemerbroeck alfo afferts the fame, P. 76.

taining no chyle, but all of them paffing from the inteftines, turgid with the cleareft lymph, and creeping along the glands, after the manner of the lacteals'.

Without having recourfe to any further quo tations from Bartholin, I find, in a work publifhed only a few years after the Anatome Renovata, as full and clear an account of the identity of lacteals and lymphatics, as can be expreffed in words. Speaking of the receptaculum chyli, the author* fays, (C) 'This fack is formed by the union of three branches of abforbents, *viz:* the hepatic, mefenteric, and iliac. Having mentioned the ramus iliacus, he adds; The fecond branch is the mefenteric, the greateft of the lacteal veffels, *all which are true lymphatics*, and are *only lacteals during the time of diftribution; that being paft, they again become turgid with lymph.* He fays, the lymphatics are found in all parts of the body. There is no part of the whole body, which does not contain

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* Zellerus. The Editor of Van Horne's Opufcula thus fpeaks of this author. Circa modum vafa lymphatica obfervandi et demonftrandi Zelleri differtatio meretur evolvi.

ſuch veſſels; ſo that they not only paſs out from the conglobate, but alſo from the conglomerate glands, from muſcular parts, from all the joints, from the diaphragm, the lungs, &c. but he adds, they are not every where equally numerous. Nuck, too, had long ago mentioned, the ſimilarity of ſtructure between lacteals and lymphatics; ſince therefore the ſtructure of the lacteals and lymphatic veſſels is the ſame, &c. In another place he ſays, (D) ‘ The meſentery has alſo its lymphatics, of the ſame ſtructure with the lacteals. I have ſometimes remarked, that the lacteals of the meſentery not only afforded a paſſage to the chyle, but alſo to the lymph; the diſtribution of chyle being finiſhed and received into the blood, the lacteals muſt give entrance to the lymph; for, beſides that, we have ſometimes ſeen lymph in the lacteals, it is not difficult to conceive, that the greater portion of the pancreatic juice, and alſo the lymph, ſecreted from the glands of the inteſtines, not finding any other way than theſe, by which they will therefore paſs through the thoracic duct into the blood. The ingenious Editor of Van Horne's works alſo maintains, that the lacteals sometimes carry lymph. (E) Moreover, the lacteal veins, *primi generis*, according to Ruysch,* have not so many valves as the lymphatics; and, indeed, the lacteals, *secundi generis*, *although both sets are true lymphatics*, carry a mere lymph, when chylification is completed.'

No comment can surely be required to these clear expressions. Why did Hunter and Monro suffer these truths to pass unnoticed? It appears then, from the testimony of Bartholin, Nuck, and others, that the lacteals and lymphatics were the same system of vessels, were similar in structure, and occasionally carried the same fluids.

(A) Lympham ex lymphaticis recipit receptaculum, et per eosdem ductus thoracicos lacteos amandat ad cor. Hoc lympha fere semper repletur quando chylus deest. P. 112. Chylum tempore distributionis, primario vehere et semper alias lympham. P. 621.

(B) Semel atque iterum nuper vidi in animalibus, duas post pastum horas apertis, nimirum

* Dilucid. Valvular. Cap. II. p. 34.

venas lacteas nullas chylo repletas, sed omnes ab intestinis prodeuntes, lympha limpidissima turgidas, eodem more glandulas mesenterii perreptare, quo lactea consuevere. P. 94. Struct. Diaphrag.

(C) Saccus ille formatur ex unione trium ramorum, hepatici, mesenterici, ac iliaci. Secundus ramus insignior est mesentericus ex omnibus lacteis vasis, quæ omnia vera lymphatica sunt, et lactea distributionis tempore solum apparent, ista vero elapso, iterum lympha turgent. Nulla est pars totius corporis, quæ non talibus gaudeat; ita ut non ex glandulis solum conglobatis egrediantur, sed etiam conglomeratis, ex musculosâ parte, omnibus artubus, diaphragmate, pulmonibus, &c. P. 812-13.

(D) Mesenterium etiam suos habet lymphæductus, eandem fere fabricam ac vasa lactea componentes. Verum aliquoties notavi, vasa mesenterii lactea, non tantum chylo, sed et cum eo lymphæ transitum præbere, imo peractâ chylificatione, chyloque jam in sanguinem advecto, humori lymphatico tantum eadem patere; namque præterquam, quod lympham aliquoties in lacteis viderimus, etiam conceptu difficile non erit, si

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ſucci pancreatici portionem majorem, necnon in inteſtinorum glandulis lympham ſecretam, alias vias, præter has, nuſpiam invenire, per quas ductui thoracico, et ita ſanguini, accedere poſſet. P. 146-7.

(E) Venæ inſuper lacteæ primi generis, notante Ruyſchio, Dilucid. Valvular. Cap. II. P. 34, non tanta valvularum copia, quanta vaſa ſeroſa; imo et ſecundi generis lacteæ, quamvis utiuſque generis vaſcula revera ſint lymphatica, lympham videlicet meram alio, quam chylificationis tempore tranſportantia. P. 51. Note x.

SECT. VI.

FUNCTIONS *of the* ABSORBENT SYSTEM.

Having laid before the reader what was known in the laſt century, reſpecting the ſtructure, origin, &c. of the abſorbents, it remains, that we ſhould treat of their functions. A conſiderable part of this ſubject has been, unavoidably, anticipated, in the preceding ſections; but it is proper to bring the whole in one connected point of view, that it may be ſeen, how far our predeceſſors were acquainted with this part of phyſiology. The chyle is the fluid, which the lacteals moſt generally receive from the inteſtines, although they will, occaſionally, contain a variety of other ſubſtances. The firſt diſcoverers attributed to the lacteals, the abſorption of chyle; and, therefore, muſt alſo have allowed, that what was occaſionally mixed with chyle, would alſo be taken in. Bartholin, and others, mention, that they take up medicines,

 poiſons,

poiſons, &c. Of this, indeed, there was no doubt; but it has been imagined by the moderns, that the firſt diſcoverers did not know how the lymph, &c. got into the abſorbents of the body. It was the opinion of Bartholin, Willis, Gliſſon, Zellerus, &c. that the lymphatics were not continued from arteries, by continuity of canal. How then was it imagined, that the lymph was received into their mouths? The mode of abſorption, is a ſubject which ſtill divides the phyſiological world. No one doubts, that the abſorbents are intended to receive the various ſubſtances applied to their mouths; but it is ſtill a matter of diſpute, *how* the ſubſtance gets into the abſorbent; whether by the activity of the abſorbents themſelves, or by their action as capillary tubes, or by the preſſure of ſurrounding parts. Mr. Cruikſhank, himſelf, is, by no means, decided on this point. He is undetermined, whether to accede to the opinion of Aggiunti, of Boerhaave, or of John Hunter. We muſt not be ſupriſed, then, if the authors of the laſt century had their doubts upon the ſubject.

As many ingenious men flouriſhed about the period, when the diſcovery of the lymphatics was

much

much talked of, their great attention to medical purſuits, and to the laws of the animal œconomy, as their works fully teſtify, muſt have led them to the knowledge of the great importance of the abſorbent ſyſtem. They obſerved fluids continually pouring into different cavities; they knew, from the experiments of Bartholin, Lower, and others, that the red veins did not abſorb; they alſo knew, that the red veins did not throw out any fluid; they muſt, then, have concluded, that other veſſels exhaled, and that the lymphatics abſorbed. Accordingly we find, in the works of Gliſſon, Willis, and others, a full account of theſe important functions of the animal frame. Willis gives the following account of the lymphatics of the lungs. (A) 'Indeed a conſiderable number of veſſels of this kind are required in the lungs; for ſince the blood is here *chiefly heated*, and rapidly circulated, the veins do not eaſily receive all the blood from the arteries; moreover, the glands do not long retain *what is effuſed into them*; therefore there was a neceſſity for lymphatics, as ſo many rivulets, by which the continually ſuperabundant moiſture might be carried off. If it happens, that theſe

veſſels

veſſels are obſtructed or ruptured, an anaſarca pulmonum, or hydro-thorax will ariſe, as alſo coughs, and phthiſical affections, will be often produced*. Nuck alſo ſays, that dropſy may ariſe from rupture of lymphatic; and mentions a caſe, of what he ſays is a new diſeaſe, *viz:* a dropſy of the peritonæum.

Paulus, in his commentary on Van Horne's Microcoſmus, mentions this opinion of Nuck †; and Diemerbroeck held it as probable, that dropſy might ariſe from the ſame cauſe. (B) We muſt obſerve, with reſpect to the lymphatics of the abdomen, that if they be ruptured from any cauſe (for they are of a delicate ſtructure) then the ſerous lymph will be effuſed into the cavity of the abdomen, and aſcites will be at length, produced, by its accumulation; although it alſo ariſes from other cauſes.

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* This paſſage is very remarkable, becauſe Willis ſays, that the lymph is taken up from glands: and Mr. Cruikſhank, p. 175, from ſome overſight, gives a paſſage from this author, immediately before the one we have quoted, and another immediately after, but paſſes over the very pointed expreſſions mentioned above.

† Vide Nuck's Adenograph, p. 97-98-100. Van Horne's Microcoſ. p. 57, note h.

caufes. Hydatids, he thinks, are caufed, by the effufion of lymph, forming veficles. Lower doubted very much, whether the rupture of lymphatics can produce dropfy. He paft ligatures round the vena cava of a dog; the wound was then fewed up. The dog died in a few hours. When diffected, a great quantity of ferum was found in the abdomen. He alfo tied up the jugulars in another dog. All the parts above the ligature fwelled greatly, in a few hours; and in two days, the dog perifhed, as if fuffocated by angina *. (C) During this time, there was not only a profufion of tears, but alfo a great flow of faliva from the mouth, as if it had been brought on by mercury. After death, I feparated the fkin from the tumefied parts, and expected to have found them turgid with blood; but it was quite the contrary, for I could not difcover any trace or colour of blood; all the mufcles and glands were diftended with limpid ferum, and appeared, in fome meafure, pellucid.

How far thefe circumftances tend to inveftigate the caufe of afcites and anafarca, I leave to others

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* Vide Baglivi Differt. de Experim. Anatom. Pract. P. 107.

to determine; I ſhall only make this remark, ſays Lower, that aſcites does not always, if ever, ariſe from ruptured lymphatics: for I have diſſected many ſheep, killed by the dropſy of thorax and abdomen, in which I found the lymphatics more turgid and full, than I ever remember to have ſeen; ſo that if any one intends to give a complete hiſtory of the lymphatics, no ſubject can anſwer his intention or deſign better. This idea of rupture, as the cauſe of dropſy, is alſo mentioned by Bartholin, and has been ſince adopted by Whytt and Cullen. Willis, again, mentions the pulmonary lymphatics, and gives a plate of them, taken from the lungs of an ox *. The following paſſage, concerning their uſe, is ſo exactly ſimilar to Dr. Hunter's theory, that I ſhall be excuſed from tranſcribing the whole of it. (D) If I dare conjecture, concerning the uſe of theſe veſſels, it is very probable, that thoſe cavities, intercepting each of the lobules, receive vapors every where, copiouſly flowing from the heated blood (particularly as they can no where be effuſed or collected with greater convenience), that

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* Rudbeck, Bartholin, and Ruyſch, had ſeen the lymphatics of the lungs. Vide, Diemerbroeck, P. 87.

is to ſay, theſe *vapours tranſude from the terminations of the veſſels*, through their very delicate coats, and thence being propelled further, *are condenſed into water*, to be carried off from the lungs by theſe veſſels, fitted for that purpoſe: moreover, leaſt the lymph, thus formed and condenſed, from the vapours within theſe ducts, ſhould regurgitate into the lungs, they are beſet with numerous valves*.

The plates give the exact reticular appearance of the pulmonary abſorbents, as deſcribed by Mr. Cruikſhank †. In the works of Gliſſon, we find, completely, the modern doctrine of tranſudation, and abſorption. This will appear, by a tranſcript from his book. He firſt aſſerts, that the lymphatics take up a fluid, from parts into which they are inſerted. He then enquires, whence this fluid is derived. (E) Whence is this fluid derived, which thoſe

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* It is rather curious, that Mr. Cruikſhank quotes another paſſage from Willis, immediately after this clear account, without taking any notice of it.

† P. 84. During a lecture, which we heard in London, ſome time ago, on this ſubject, a plate of the lymphatics of the lungs was handed round; we were ſtruck with its exact ſimilarity to that of Willis's, and took the liberty of mentioning it after lecture.

thofe veffels carry off? It is poffible, that the arteries may claim to themfelves, fome part of this office. For it cannot otherwife happen, but that the blood paffing through them, fhould pour out a copious halitus upon thofe parts, into which it is fent; and the more fo, becaufe the fame halitus is, for fome time, confined, by the thick and denfe coat of the artery. This halitus, thus effufed, is retained, in confiderable quantity, by fibrous and membranous parts, and is again collected; and, after the manner of diftillation, is condenfed into a certain liquid, which forms fome portion of the fluid, carried off by the lymphatics. He thinks, the arteries alone do not furnifh this halitus, but that it is fupplied by other veffels. The whole of the fluid carried off by the lymphatics, is not derived folely from the halitus of the blood, nor from the arteries, but alfo from other veffels; thefe, he fays, are nerves. But notwithftanding that the nerves fupplied a fluid to be abforbed, yet he attributes the greater quantity of tranfudation to the arteries; by far the greateft part of this lymph is condenfed from the halitus of the blood, and carried off by the lymphatics. If Gliffon had not already exprefsly afferted, that a halitus is thrown out, condenfed, and taken up by the abforbents, we might collect, from thefe paffages, that he could not have imagined, the lymphatics to be a continuation of arteries; becaufe, he fays, it takes a great deal of pains to prove it, that the nerves throw out a fluid to be abforbed; which he certainly could not have faid, had he maintained, that the lymphatics were returned from arteries, by continuity of canal. The abforption of folids was, alfo, maintained by this author. He urges, when fpeaking of the lymph, that the abforbents are thofe veffels, which return into the fyftem, effufed fluids; that this effufion is not excrementitious; for fo true and œconomical is nature in her operations, that fhe never leaves her work incomplete. We muft conclude, that he meant the folids were taken up, from the following expreffions. The chyle is the daily fupport, by which the wafte of the blood is continually repaired; from its deficiency, the very folid parts are broken down into a fluid, and when neceffity requires, reftores the lofs of the blood. I could point out, were it neceffary, numerous other paffages in Gliffon, to the fame purpofe; and if any one will be

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at the pains of reading Chap. 45 of the Anatomia Hepatis, his trouble will be amply repaid.

During the latter period of the ſeventeenth century, medical men were very active, all over Europe, and eminently ſo in the learned Univerſities of Cambridge and Oxford. In their writings, we find the brightneſs of genius flowing through every page. The works of Willis, Mayow, Gliſſon, Bartholin, and innumerable others, contain experiments and obſervations, which will juſtly ſurpriſe a modern reader; and, for my part, I muſt confeſs, that in the theories of medicine, and their application to practice, I have derived conſiderable advantage, from the peruſal of their works: but, independent of theory, which the prolific brain of any may form, without a minute knowledge of anatomy, we find in them nice points determined, which require laborious anatomical inveſtigation. In the arts and ſciences, if we did not profit by the accumulated labours of our fore-fathers, our exertions would be vain, in as much as we ſhould not advance the progreſs of knowledge. Dr. Hunter ſays, he has often found his obſervations in old books, long after he had thought them peculiar to himſelf. His excellent remarks were, the reſult of tedious inveſtigation. If he had been, at firſt, acquainted with theſe obſervations, might they not have been a clue, by which he might have directed his uſeful labours? Much time would be ſaved, of that ſhort period allotted to man; and thus the various inveſtigations of many, would, as it were, be concentrated by the labours of an individual, and be productive of reſearches beneficial to ſociety*. It is ſurpriſing, that the uſeful labours of many, who have gone before us, ſhould have been ſo much neglected. Mr. Cruikſhank, through the whole of his work, takes no notice of the clear account Gliſſon gives of tranſudation and abſorption. He only quotes him once, when mentioning Jolyffe's right to the diſcovery of the lymphatics. 'As to Gliſſon's evidence I muſt obſerve, that Dr. Jolyffe conſidered

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* Though we allow him a great ſhare of ingenuity, ſagacity, and diligence, we find much to reprehend, and various occaſions to wiſh his knowledge more matured, by previous inveſtigations of the labours of others in the ſame departments.—Crit. Rev. for Auguſt, Art. Foot's Life of J. Hunter.

confidered the nerves as veffels, and therefore ufes the words *quartum genus vaforum*. At the fame time, he appears to me to have forgot Afellius's difcovery of the lacteals, or he would have faid, *quintum genus*, unlefs he imagined his veffels and Afellius's the fame; and then Gliffon would never have ferioufly informed us, that Jolyffe had difcovered a new fet of veffels, Afellius having informed us of them long before'*. Gliffon feems to have confidered the lacteals and lymphatics, as the fame fyftem of veffels; for he fays, (G) Hitherto, only four fets of veffels have been difcovered, *viz*. arteries, veins, nerves, and lymphatics. He could not have forgot the difcovery of Afellius; for he obferves, When I firft faw thefe veffels, the affertion of Afellius, that he had difcovered lacteals going to the liver, occurred to me †.

Upon confideration it appears, that both Jolyffe and Gliffon ranked the venæ lacteæ with red veins; and, therefore, the difcovery of Jolyffe

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* P. 35.

† We may conclude, from this, that Afellius faw the lymphatics.

would be a *quartum genus vaſorum.* We wiſh, that that diſtinguiſhed anatomiſt, Mr. Cruikſhank, had paid more reſpect to Gliſſon, and had given him that place in his publication, which his great merit and true theory certainly deſerved. No wonder that Dr. Hunter was mortified, when he read the works of Gliſſon, and was obliged to confeſs, that his theory was Gliſſon's, revived and confirmed. He might, alſo, have ſeen, that Willis believed in tranſudation. I muſt obſerve, however, that Willis, and others, did not ſeem to be aware, that tranſudation took place, only, through organic pores. They have no where poſitively aſſerted, *how* this tranſudation was performed; and as far as I know, this was left to be proved, by the ingenious author of the Treatiſe on the Abſorbent Syſtem. His excellent account, in the firſt part of his publication, forms a beautiful and conſiſtent theory. Let us now examine, what ideas were entertained, with reſpect to *how* abſorption was performed. Aſellius aſcribed the abſorption of chyle, to a traction in the heart; and this opinion was adopted by anatomiſts: but when the circulation was explained by Harvey, this doctrine, in a great meafure, fell to the ground; but even after that noble difcovery, fo ftrong was prejudice, that many adhered to the old opinion of a valida tractio in the heart. I have mentioned, that Lower imagined, from his experiments, that the lacteals arofe, obliquely, from the inteftines*. (H) From this circumftance, he thought, that the action of the inteftines compreffed the mouths of the lacteals, and prevented abforption; but as the periftaltic motion is not inceffant, he concluded, that the chyle was received into the lacteals, when the motion ceafed. It is evident, from this, he afcribed an activity to the mouths of thefe veffels. The chyle, having got into the lacteal, is propelled, by the vermicular motion of the inteftines into the receptaculum. The afcent to the chyle, would be very difficult, fays Lower, were the receptacle not placed between the tendons of the diaphragm; whence it happens, that as it is contracted during refpiration, its tendons being drawn clofe together, firmly comprefs, and agitate this lacteal fack, efpecially when filled with chyle, fo that they propel

* This was alfo the opinion of Drake.

the chyle into the ſubclavian veins. Other authors, if we are to judge from the terms they uſe, attributed an activity to the mouths of theſe veſſels; for we meet with *exſugo*, *abſorbeo*, and *exhaurio*.

An opinion, however, which prevailed very much, with reſpect to abſorption, was, that the contents of the abſorbents were received into them, and propelled by the action of ſurrounding parts; making the abſorbents altogether paſſive in their functions. This doctrine is mentioned by a variety of authors. Diemerbroeck ſays, that the chyle and lymph are impelled into the abſorbents, by the preſſure of adjacent parts, as muſcular motion. (I) The cauſe of the entrance of the chyle into the lacteals, and its motion through them, is two-fold, *viz.* the periſtaltic motion of the inteſtines, and the contraction of the abdominal muſcles, together with the action of the reſpiratory organs. When ſpeaking of the lymphatics, he obſerves, (K) The impulſive cauſe is the ſame, *viz.* motion and preſſure. The lymph, which comes from the joints, is impelled by the motion of the muſcles of thoſe parts, in like manner as

we

we ſee the ſalival liquor flows copiouſly into the mouth, by the motion of the jaw; but when we are at reſt, as in ſleep, there is little or none. By this preſſure of parts, not only the glands lying in them, but alſo the lymphatics, are preſſed upon, both by the muſcles, and the ſuperincumbent viſcera; and thus the contained fluid is ſqueezed out, and propelled forwards. I think the lymph moves ſlow and quick, at different times, according to the greater or leſs motion of thoſe parts, in which the conglobate glands and lymphatics are ſituated, as happens to the ſublingual ſalivary ducts, which proceed from conglomerate glands.

We have already mentioned, that Bartholin ſaid, the lymph was thrown out from the arteries. Diemerbroeck is of the ſame opinion. (L) Gliſſon of opinion, that the lymph is derived from the nerves; Bartholin maintains it comes from the arteries: the former idea is abſurd; the latter is the moſt probable, on account of the quantity of lymph, which cannot be ſecreted, in ſuch quantity, from any veſſels, as from the arteries. Diemerbroeck ſeems, here, to have forgot, that Gliſſon, although he held it probable, that a fluid

might

might be derived from the nerves, maintained, that by far the greater part of the lymph was secreted from the arteries. Arterial action, too, is expressly mentioned by some, as a cause of the motion of the lymph. Bartholin, speaking of the lymphatics which accompany blood-vessels, observes, (M) This I suspect, to accelerate the motion of the lymph, in like manner as I believe, the blood in a vein to be propelled, by the pulsation of the artery in the neighbourhood.

The doctrine, that the absorbents are perfectly passive in their functions, is fully explained by Collins, an ingenious anatomist, who flourished about the middle of the last century. Now somewhat may be said of the manner, how this milky liquor is transmitted through the mesentery, into the common receptacle; and this, I humbly conceit, may be accomplished by a double means. The first may be, the gentle contraction of the guts (made by carnous fibres) in their peristaltic motion, wherein the chyle, impregnated and diluted by more fluid, and excellent particles of the pancreatic juice, is impelled into the origins or roots of the milky vessels, whose pores hold an

analogy,

analogy, in ſhape and ſize, with the particles of the chyle, and thereby give a reception to this ſelect alimentary liquor. The ſecond way of tranſmitting chyle, through the meſentery into the common receptacle, is more powerful, and aſſiſtant to the former, and are the muſcles of the belly, which contract themſelves in expiration, and thereby compreſs the guts, and ſqueeſe the purer parts of the prepared aliment into the orifices of the lacteal veſſels; and the more groſs excrements, the reliques of concoction, having magnitudes and figures, different from the orifices or extremities of the milky veins, are ſecluded their cavities, and are protruded from one part of the guts to the other, as diſſerviceable to nutrition*.

As Collins thus allowed no action in the lacteal itſelf, it was neceſſary, that ſome other power ſhould propel the chyle, once received into it. He thus deſcribes it. The chyle being thus generated in the ſtomach, is thence tranſmitted into the inteſtines, and afterwards received into the origins of the lacteal, by the periſtaltic motion of

* P. 389, Vol. I.

of the guts, and the contraction of the abdominal muſcles, ſqueeſing the chyle into the milky veſſels, which import it through the meſentery, into the common receptacle (being lodged between the proceſſes of the diaphragm) compreſſed, when the medriff is moved, and brought towards a plain; whereupon the milky humour is impelled farther and farther upwards, through the thoracic ducts, till it landeth in the ſubclavian vein. This aſcent of the chyle is much aſſiſted by the mechaniſm of the ducts, as furniſhed with valves, which give way to the aſcent of the chyle upward, toward the ſubclavian vein, and give a check to its retrograde motion toward the common receptacle*. It is worthy of remark, that this author, through the whole of his work, never makes uſe of the word abſorption, which implies action in the veſſels themſelves; but when he wiſhes to expreſs this function, ſays, the fluid is impelled, received, tranſmitted, &c. into the origin of the lymphatic or lacteal. This anatomiſt has given the ſame theory of the motion of the lymph. The thinner ſerous excrements, are tranſmitted into the origin

of

* P. 682, Vol. II.

of the lymphæ-ducts, appertaining to the muscular parts, by whose local and voluntary motion, the natural progress of the lympha is much quickened. Again; the lympha of the adjoining lymphæ-ducts is promoted by muscular motion, from the circumference to the center to the common recaptacle*.

The doctrine of Collins, then, implied no muscularity in the absorbents, and pointed out, that they were passive in their office, their contents being propelled by the action of surrounding parts. The opinion, that the chyle was received into the lacteals by muscular motion, found its advocates in Drake and Verheyen. The chyme, says the former, which, as soon as it is reduced to a consistence, loose enough to be obedient to the pressure and peristaltic motion of the intestines, is gradually worked out, through the pylorus, into the duodenum, along the sides of which, and the rest of the intestina tenuia, the vasa lactea are planted, into whose extreme minute orifices, the thinner and more fluid part of this mass is pressed, by the natural contraction of the guts†.

A

* P. 443, Vol. I. † P. 88.

A doctrine, precisely similar to this, has been adopted, of late, by some very ingenious men. Experiments and arguments have been brought, to refute the opinion of Cruikshank, and others, who ascribe muscularity to the absorbents. A very ingenious and eminent lecturer in London adheres to this opinion, denies altogether any necessity for muscularity in the absorbents, and maintains, that their contents are transmitted into them, and propelled by the motion of surrounding parts, as muscular and arterial action. I must confess, it is difficult to account for the rapid absorption of fluids, in many cases, without allowing some muscularity in these vessels; at the same time, the irritability and muscular power attributed to them by many, is much too great, and let me add, incompatible with the safety of life*. We cannot admit of the great irritability attributed by Mr. Cruikshank to the absorbents, although we think they possess some portion of muscular

* Mr. J. Hunter says, they devour parts, as caterpillars do leaves. A very late writer talks of their *pulling down*, and carrying away the materials, that are worn out, and unfit for the support of the machine.

mufcular power. If I am not miftaken, and if we are to judge from his expreffions, he gives them an irritability, greater than that of the heart. It has been imagined, that the veffels continue to abforb after death. This has been brought forward as one of the moft powerful arguments, in proof of their irritability. The mufcular flefh of the turtle, fays Mr. Cruikfhank, continues to contract and relax, from the ftimulus of air only, many hours after the animal's head is cut off. The abforbents appear to me, to have a fimilar power of action, and to be capable of abforbing, for fome time after the animal is dead. Malpighi had before faid, that one would be tempted to believe, that they abforbed after death, and I was determined to make the experiment. I tied up the trunks of the arteries and veins, belonging to a portion of the great inteftine in an afs, which had been dead a few minutes (the inteftines were ftill in the cavity of the abdomen and the parts not cold). I knew, that the trunks of the abforbents muft be inclofed in the ligature, though not one of them was then vifible. Two hours after, I returned, and found a number of abforbents, turgid

with

with a tranſparent fluid. I opened one of the largeſt with a lancet; the fluid iſſued in a ſtream, which it could not have done, unleſs the veſſels had continued to abſorb and to propel their fluids, with great force, after the death of the ſyſtem *. This circumſtance was before taken notice of by anatomiſts; but it was accounted for in a quite different manner, and upon principles more agreeable to truth. The ingenious Verheyen has the following obſervations. (N) The motion of the lymph does not ceaſe with life, like that of the blood, but continues a conſiderable time after death, which is proved, by ligatures being paſt round the lymphatic veſſels. This ſeems to ariſe from the following circumſtances: 1ſt. Becauſe the lymph is very fluid and ſmooth, and therefore flows eaſily. 2d. Becauſe the veſſels themſelves are lined with a coat, extremely poliſhed, which ſuffer compreſſion from the collapſed parts, and therefore the lymph is propelled forward, according to the natural motion. The proof, too, given, of their muſcularity, is very fallacious; becauſe motion was perceived

* P. 60-1.

perceived when oil of vitriol was applied, it has been concluded by Haller and Cruikſhank, that they are extremely irritable. Every one knows, that the cauſticity produces a chemical change; it corrugates the lacteal, hence the fluid is propelled*. I have applied ſulphuric acid to the inteſtines of mice, after death, and found, that not only motion took place, but that a hardneſs was induced. This is more evident in frogs; their inteſtines are beautifully tranſparent, white, and ſoft; by applying ſulphuric acid, they become opake and firm, very ſimilar to the tunica albuginea of the eye. We have ſeparated the coats of the ſtomach of a frog, in this way; for the acid corrugates the external coat, and the internal eaſily ſeparates: it corrugates, equally, the ſkin †, the lungs, the inteſtines, &c. Hence, we conclude, that it is not an unequivocal proof of the muſcularity of parts; a mechanical ſtimulus would be a better teſt ‡.

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* An experiment, ſimilar to this, was made by an anatomiſt of London, with the lacteals.

† The ſkin of a frog is perfectly membranous.

‡ Hanc arteriarum actionem ſatis demonſtrant ipſius arteriæ

From what has been ſaid, I think, no one can doubt, but that the great importance of the abſorbent ſyſtem was ſeen and admired by the medical world, during the laſt century. The many eminent men, who then flouriſhed, have expreſſed their ideas, on the ſubject, in ſuch clear and decided language, they have proved their opinions by experiments and arguments ſo complete, as cannot fail to bring conviction home to the moſt ſceptical mind. Their attention to the ſubject is evident, through all their works; and their accuracy is confirmed by modern labour and inveſtigation. The candid enquirer after truth, is ſurpriſed, how expreſſions, ſo clear, could be miſtaken; and the benevolent philoſopher hopes, that overſight, not wilful miſrepreſentation, has cauſed this want of candour. Highly as we reſpect the great abilities of Dr. Hunter, indebted as all are to him for his extenſive and uſeful purſuits, yet ſurely it is worſe than prejudice, to adhere blindly to the *ipſe dixit* of

riæ fabrica, manifeſte muſculoſa tunica inſtructæ et multa quoque experimenta in vivis animalibus inſtituta; ubi arteriæ, *admoto acri mechanico (nam veneno chemico parum fidendum eſt)* validiſſime ſe contraxerunt.—Conſpect. Medicinæ, P. 235, Vol I.

of ſuperior merit. How long, and how much, has not ſcience, and medicine in particular, ſuffered, from ſuch unphiloſophical bigotry.

'*Nullius addictus jurare in verba magiſtri*';— is a maxim, which, if not licentiouſly adopted, leads to calm enquiry, and would, in a great meaſure, have ſhortened the dark and tedious period of Galenic attachment. To acquieſce, implicitly, in the opinions of a man, becauſe he has gained applauſe and reputation, is to ſhut the door againſt inveſtigation and enquiry; and the doctrines of a teacher are only to be held in veneration, as they are found to be conſonant with reaſon and truth. It is not the authority, but the truth, which carries reſpectability along with it; and authority, if not guided by judgment, eventually obſtructs, inſtead of promoting the great end of ſcience; and the man, who propoſes a doctrine, tacitly confeſſes, that every one who reads it, has a right to form their own judgment. 'Non enim tam auctoritatis in diſputando, quam rationis momenta quærenda ſunt. Quinctiam obeſt plerumque iis, qui diſcere volunt, auctoritas eorum, qui ſe docere profitentur: deſinunt enim ſuum judicium adhibere; id habent ratum, quod ab eo quem probant, judicatum vident."* Such was the philoſophical creed of Cicero, and ſuch ought to be the opinion of every enlightened mind.

Sydenham, by adhering to no ſect, but by carefully attending to the operations of nature, has acquired that reputation in the literary world, which will be always attached to his name; and had the great Hippocrates confined himſelf to the doctrines of Pythagoras, thoſe noble aphoriſms, which have immortalized his name, would never have adorned and improved the ſcience of medicine.

If the claims of Dr. Hunter be founded in juſtice, the opinions we have advanced, will contribute to eſtabliſh that right; for if the paſſages we have ſelected ſhall induce medical men to examine the works themſelves, they will then ſee, whether the concluſions we have drawn be inconſiſtent with the premiſes; for it is a maxim, unerring in its application, that a calm and diſpaſſionate inveſtigation always promotes the knowledge of

* De Natura Deorum, P. 11 12.

of truth. Quotations are but feeble proofs, when compared to reading the works themfelves, where detached paffages are connected by clofe reafoning, and accompanied by an accurate detail of facts.

The numerous quotations which appear in this mifcellaneous work were unavoidable, from the nature of the fubject. Merely to have made affertions, without adducing proofs, would have been little regarded; and had I given a tranflation of the paffages, without the original, it might have been urged againft me, as my own interpretation. As it is, I fear, fome will regard it as dry reading, to wade through a mafs of quotations. Such, I may fafely affirm, are equally as carelefs upon whom the fate of fcience devolves, as from whom the merit of a difcovery is injurioufly wrefted.

(A) Profecto vafa hujus generis quam plurima in pulmonibus requiruntur; quippe cum fanguis hic maxime efflagret, et rapidè circuletur, venæ haud facile totum cruorem ab arteriis excipiunt; porro glandulæ quod in iis depofitum eft non diu continent, quare lymphæ-ductus, quafi alveis permultis, opus erat, quibus humor continuo super-abundans amandari possit. Si quando hos obstrui aut disrumpi contingat, pulmonum aut pectoris hydrops, quinimo tussis et affectus phthisici, sæpenumero oriuntur. P. 19.*

(B) Notandum, de vasis lymphaticis in inferiore ventre latentibus, quod si aliqua de causa rumpantur (sunt enim valde tenella) tunc serosam lympham in abdominis capacitatem effundi, ab ejusque accumulatione, tandem ascitem generari; quamvis aliis de causis fieri possit. P. 95.

(C) Toto hoc tempore, non solum lacrymæ copiosius fluebant, sed et plurima saliva ex ore, non aliter quam si ab assumpto mercurio fluxus ille concitatus fuisset. Post obitum, cutem ejus a partibus tumefactis separavi, atque expectavi ut partes tumefactæ sanguine extravasato turgerent; sed aliter omnino evenit, quippe nullum vestigium aut colorem fere sanguinis observare potui: sed musculi omnes et glandulæ sero limpido maxime distentæ et admodum pellucidæ apparebant. Quan-

tum

* Pharmaceutice Rationalis sive Diatriba de Medicamentorum Operationibus in Humano Corpore. Pars Secunda. Auct. Tho. Willis, M. D. in Univ. Oxon. Prof. Sedleiano.

tum hæc ad afcitis et anafarcæ caufas inveftigandas conducant, aliis judicandum relinquo; hoc folum advertere oportet, afcitem non ex ruptis lymphaticis femper, fi omnino unquam evenire; quippe plurimas oves, hydrope pectoris atque abdominis defunctas, diffecui, in quibus vafa lymphatica ita turgida et repleta ubique inveni, ut nufquam magis; adeo ut fi quis venarum lymphaticarum hiftoriam plene abfolvere in animo habeat, nulla corpora ad votum ejus et propofitum magis cefferint. P. 82-3.

(D) Circa horum ufus ut conjectari aufim, verifimile eft cavitates iftas, quofque lobulos intercipientes, vapores a fanguine accenfo quaquaverfus copiofe emanantes, (cum nullabi melius detrudi aut feponi poffint) excipere, qui fc. e vaforum finibus in cavitates hafce, per tenuiffimos ipforum parietes, tranfudant, indeque mox ulterius propulfi, in aquam, per hæc vafa appropriata e pulmone deportandam, condenfantur: infuper ne lymphæ intra ductus iftos, a vaporibus factæ ac ita condenfatæ, in pulmones (magnam ipfis noxam illaturæ) regurgitent, denfiffimi valvularum obiçes impediunt. P. 32.

 (E) Quid

(E) Quid humorem illum apportet, quem vasa isthæc auferunt? Possunt quidem arteriæ indirecte et veluti per accidens, atque aliquatenus muneris hujus partem aliquam sibi vindicare. Fieri enim nequit, quin sanguis per illas exiliens (utpote calore insigni effervescens) copiosos halitus diffundat in partes illas, in quas immittitur: eoque magis, quod iidem halitus jam antea a crassa densaque arteriæ tunicâ aliquandiu repressi fuerint. Halitus isti, sic dispersi, a partibus fibrosis ac membranosis, magnam partem retinentur et recolliguntur denuo, atque ad distillationis modum, in liquorem quendam condensantur, qui humoris a lymphæ-ductibus avecti partem aliquam constituit. Materiam omnem per lymphæ-ductus delatam, non solum a sanguinis halitibus proficisci; neque ab arteriis duntaxat, sed et ab aliis quoque vasis suppeditari. P. 402-3. Partem hujus lymphæ longè maximam a sanguinis halitibus, roris instar coactis, et per lymphæ-ductus refluis, constitui. P. 448.

(F) Chylus est victus quotidianus, quo sanguinis dispendia reparantur denuo, in illius autem defectu, ipsæ solidæ partes resolvuntur et in liquorem abeunt atque ubi necessitas ingruit, sanguinis jacturam reficiunt. P. 410.

(G) Hactenus

(G) Hactenus quidem quatuor duntaxat vaſorum communium genera innotuerunt, arteriæ ſcil. venæ, nervi, et lymphæ-ductus, P. 403. Cum primum vaſa hæc conſpicatus eſſem, ſubiit animum Aſellii aſſertio, quâ ait, exploratum ſibi eſſe venas lacteas in hepar diſtribui. P. 268.

(H) Probabile eſt, tunc temporis chylum in venas iſtas aſſumi, quando pars aliqua inteſtina a motu et corrugatione conquieſcit, et proinde orificia lactearum laxiora et patentiora fiunt, P. 150. Et quandoquidem chylus per vaſa thoracica, præſertim in erecto corporis ſitu, ut in homine, difficilius aſcendit, et proinde ob motus tarditatem ad coagulandum proclivior eſt; ideo inter tendines diaphragmatis, ubi ſpinæ alligantur, magnum hoc receptaculum conſtituitur; quo fit ut quoties diaphragma in omni inſpiratione contrahitur, tendines iſti valde attracti, ſacculum hunc lacteum modo chylo turgidus fuerit, plurimum comprimant et exagitent; adeoque chylum in illo contentum, per ductus chyliferos, in venam ſubclaviam propellant, et continuo vectigali maſſam ſanguinis continuo pereuntem inſtaurent. P. 152-3.

(I) Cauſa quod chylus lactea vaſa ingreditur, per eaque moveatur, duplex eſt; una debilior, ſcilicet undoſa contractio quæ a fibris ipſorum inteſtinorum peragitur, quam valde conſpicuum exhibent cuniculi et feles vivi ſecti. Altera fortior muſculorum abdominis una cum reſpiratione ſurſum deorſumque motorum impulſio. P. 69.

(K) Cauſa enim impellens eadem eſt, ſcilicet, motus et preſſio. Quæ ex artubus progreditur, per motum muſculorum iſtarum partium promovetur, eodem modo ſicut videmus per motum maxillæ etiam ſalivalem liquorem copioſum in os effluere, per quietem vero et in ſomno, paucum vel nullum. Hac enim partium preſſione, ſimul etiam in iis latentes glandulæ, ut et vaſa lymphatica, cum a muſculis, tum a depreſſis incumbentibus viſceribus premuntur, ex iiſque contentus liquor exprimitur et propellitur. Ego exiſtimo lympham modo citius, modo tardius moveri, pro majore minoreve motu ac preſſione illarum partium, in quibus ſubſiſtunt glandulæ conglobatæ et vaſa lymphatica, ſicut illud ipſum contingit in vaſis ſalivalibus ſublingualibus, quæ ex glandulis conglomeratis procedunt. P. 94-5.

(L) Gliſſonius e nervis, Bartholinus ex arteriis, eam (lympham) prodire exiſtimat. Prius abſurdum eſt. Poſterius, propter lymphæ copiam (quæ a nullis vasis copiosior secerni potest, quam ex arteriis) maxime verosimile videtur. P. 94.

(M) Quod motum lymphæ accelerare posse suspicor, non secus ac sanguinem in vena per arteriæ illi junctæ pulsus propelli credidi. Diaphragm. Struct. P. 101.

(N) Transitus lymphæ non, uti sanguinis, cum vita cessat; sed etiam notabili post mortem tempore persevèrat, quod probatur, per ligaturum vasis lymphaticis injectam. Rationem puto, tum quod lympha tenuis sit et multum fluida, adeoque ad motum valde apta; tum quod ipsa vasa constent tunica admodum tenui, quæ proinde cessante post mortem calore naturali, a partibus collapsis facile comprimitur, et quidem magis in partibus, quæ posita ligatura exteriora respiciunt; sic ut lympha alioquin in viis hæsura, promoveatur continuo versus interiora, conformiter ad propulsionem naturalem. P. 197-8.

APPENDIX

CONTAINING A

CORRESPONDENCE

WITH THE

AUTHOR,

RELATIVE TO

SEVERAL SUBJECTS IN THE WORK.

Fig. 1.

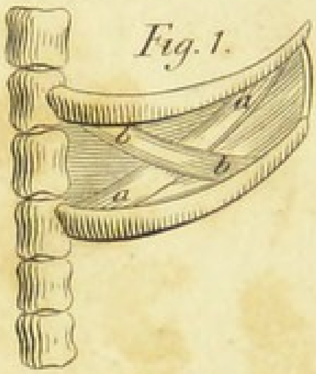


Fig. 2.

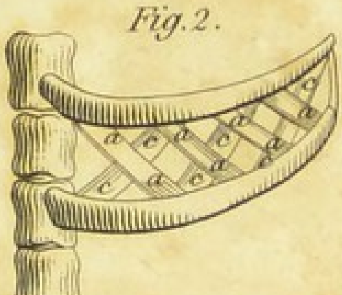


Fig. 4.

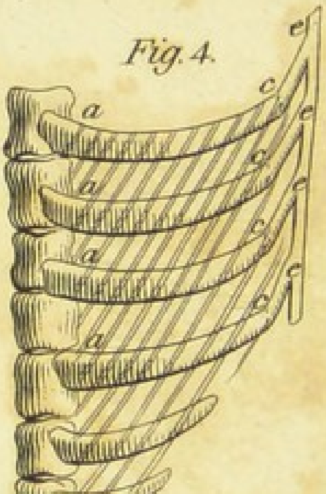


Fig. 3.

Fig.1.

Fig.4.

Fig.2.

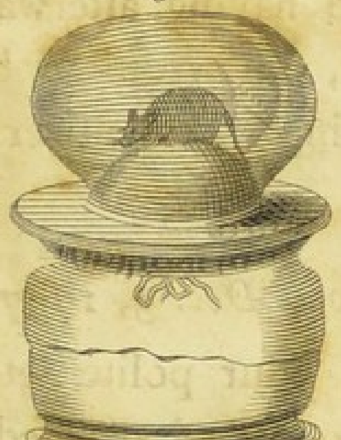
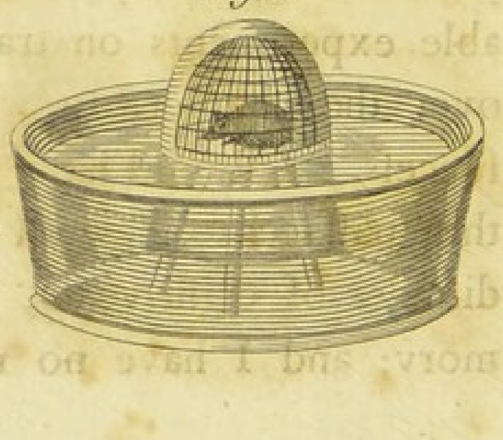


Fig. 5.

Fig.3.

Fig.6.



APPENDIX, No. I.

LETTER *from* Dr. BEDDOES, *concerning an Experiment made by* MAYOW.

In Sect. VI. P. 100, is related a curious experiment, on transfusion, mentioned by Mayow. Having read the works of this author, without detecting the experiment, and having afterwards seen it quoted by Dr. Beddoes, we wrote to him on the subject, and the following is the answer we received.

Dear Sir, *Clifton, Dec.* 5, 1797.

I am much obliged, by your polite letter. I was, at the same time, a good deal alarmed at your saying, that you could not find the remarkable experiments on transfusion. I have looked over the chapter, to which I refer, but without success. I should have felt sure that I had shewn the passage to several individuals; but at the distance of eight years, I will not trust my memory; and I have no means of ascertaining the truth.

truth. If I have any papers, by help of which I could clear up the miſtake, they are not now at my diſpoſal, having been packed up, when I quitted Oxford, and left at one hundred miles diſtance from Clifton.

I have been long ſenſible, that my abſtract of Mayow was drawn up with more ardour than judgment. I ſhould have referred, for example, to his tracts, page by page. But I had no conception, that I could attribute to him an experiment, which he never made, and one ſo remarkable in its plan and reſult. Perhaps your reſearches may enable you to explain the myſtery. If there be not in Mayow any paſſage, which I could have miſtranſlated, have you met with no author, whom I might have had upon the table, and in the moment of diſtraction, have quoted in his ſtead? Have you Dr. Schereris' Analyſis? It is in German; I know not the title, but it is, I dare ſay, very accurate. I ſhould be glad, if I did commit ſo enormous a blunder, to give you and the Public all the ſatisfaction in my power, by accounting for it; but, at preſent, I am utterly unable. If you chooſe to inſert the whole, or any part of the above in your publication, it is much at your ſervice. Should any thing more ſatisfactory occur, I will trouble you with a few lines. I am glad you have ſucceeded with the

foſſil

foſſil alkali, in irritation of the bladder:* it has pretty faithfully ſerved me in ſuch caſes.

I am,

Dear Sir,

Your's, &c.

To Dr. Yeats,
Bedford.

THOMAS BEDDOES.

* This was a caſe of very long ſtanding, and had reſiſted every remedy which had been uſed. So great was the pain, &c. that it was imagined a ſtone was preſent, and the dreadful operation of lithotomy was thought of. When I firſt ſaw her, I ſaid it was nothing more than a diſeaſe of the mucous membrane of the bladder, conſtituting the dyſuria mucoſa of Cullen, and the catarrhus veſicæ of Lieutaud. Hoffman relates a ſimilar caſe (Conſultationes Med. Vol. II. p. 560) which he calls rarus veſicæ affectus. The ſoda pills gave complete relief. I attended the caſe with my ingenious friends, Meſſrs. Chapman, Surgeons, Ampthill. It is but juſtice to obſerve, that theſe gentlemen are ever conſpicuous, for an aſſiduous attention to their patients; and I have frequently derived conſiderable pleaſure from their acute obſervations.

APPENDIX, No. II.

LETTER *from the* AUTHOR *to Dr.* BEDDOES.

Dear Sir, *Bedford, Dec.* 11, 1797.

Your very polite letter of the 5th, I have had the pleaſure to receive. I have not been able to detect, in Mayow, the experiment on transfuſion you mention; and I am at a loſs to know, in what author you found it. The paſſage I allude to, is mentioned by you (in a note) in your Obſervations on Calculus, Sea Scurvy, &c. p. 104, and it is alſo quoted from you, by Dr. Thornton, in Med. Extracts, Vol. I. p. 126. I find one paſſage in Mayow, reſpecting transfuſion; but it is of another kind; it is as follows: 'Enimvero ſi opii diſſolutio, aut aqua frigida, per venam jugularem injiciantur, cordis motus ſtatim crebrior erit, uti *ſæpe* expertus ſum.' P. 303. The edition I have, is the collection of his tracts, publiſhed by himſelf at Oxford, 1674. If he was anxious enough to perform *repeated* experiments of this kind, it ſeems very probable, that his indefatigable zeal would have prompted him to transfuſe blood, from

from one animal into another; particularly as Lower had juſt diſcovered the method, and which was much noiſed abroad: and indeed in the year 1666, two years before Mayow gave to the world any of his works, the experiments of Lower, on transfuſion, were publiſhed in the Phil. Tranſ. (de Corde, P. 129.) So remarkable and curious a diſcovery, we ſhould imagine, would have arreſted the attention of Mayow; and if he does not mention it, it muſt appear rather extraordinary, particularly as it ſeems ſo much connected with his ſubject. I cannot find, in any author that I have conſulted, an obſervation, like the one in the quotation alluded to. I therefore diſtruſt my own accuracy, and the paſſage may ſtill be in Mayow, although it has hitherto eſcaped my examination. I have read, with ſome attention, almoſt the whole of Mayow's tracts, except the laſt, de Rachitide: it is not likely that the paſſage ſhould be there.

Dr. Scherer's Analyſis is in my poſſeſſion; it is a M.S. tranſlation from the German original, which I was favored with by Dr. Thornton. It is accurate; but does not give a ſufficiently full account of the beautiful ſyſtem publiſhed by Mayow. In the work of mine, about to make its appearance, I trace the hiſtory of air, as far back as the time of Hippocrates; and I remark a curious coincidence, between ſome of the doctrines of the Pythagorean

thagorean School (refpecting air and water) and the modern difcoveries on that fubject. It is but juftice, however, to obferve, that my attention was directed to this circumftance, by a letter which I received from Dr. Gregory, of Edinburgh.* Mayow I make my refting place; and examine the progrefs of opinion, from his time to the prefent. The opinions of this great man, certainly, met with no encouragement in his own life time; for I do not find his name mentioned, by any one author, till after his death, when his doctrine became the prevailing fafhion of the times; and although greatly mifunderftood by many, fpread rapidly on the Continent, as I can prove, from a work which has come into my hands, (Schelhamerus de Nitro, and alfo from Morhof). The adoption of Mayow's theory of refpiration, by the great Baglivi at Rome, contributed much to extend his reputation in Italy.

You, Sir, permit me to fay it, are the man who firft held up the works of Mayow to the philofophic world. To you is fcience indebted, for refcuing from oblivion, the beautiful experiments of a great genius, who certainly fhines, with all the fplendor of a modern difcoverer; and if the name of Mayow is mentioned, with refpect, in literary circles, it is Dr. Beddoes who has reftored

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* Vide Introduction to Part Firft.

ſtored him to diſtinction. You have promiſed to favor me with any thing that may occur, more ſatisfactory, on this ſubject, I ſhall thank you for the communication; and

I remain, &c.

To Dr. Beddoes, G. D. YEATS.
Clifton, near Briſtol.

Having reperuſed, with accuracy, Mayow's tracts, we diſcovered the paſſage, which had hitherto eſcaped examination; and which confirms the obſervations made in the above letter, *viz.* that it would have been a very extraordinary circumſtance, if a fact, ſo curious, and ſo much connected with Mayow's ideas of reſpiration, had not attracted his attention. We immediately communicated our ideas to Dr. Beddoes, from whom we received the following anſwer.

APPENDIX, No. III.

LETTER *from Dr.* BEDDOES *to the* AUTHOR.

Dear Sir, *Clifton, March* 13, 1798.

I cannot but be very thankful for your obliging letters, and rejoice, that your diligence has discovered the missing passage in Mayow. At the same time, with the best will possible, I cannot return your politeness in kind; for I have sent my copy of Mayow to a person of more leisure, to hunt for the account of the phænomenon in question; and, therefore, cannot have the satisfaction of referring as you direct.

What you say about the air in ovo, excites my curiosity; but if I had Mayow, I could not have time to study him now; for besides professional occupations, I have embarrassed myself with a course of chemical lectures.

If you can do it, without disturbing the order of your matter, I wish you would introduce, from me, my own censure, on the manner in which I have abridged Mayow. It is too rhetorical a great deal; and besides giving this proof of bad taste, I have omitted constant reference to the pages of the original,

original, which I hold to be requiſite, in a work, of which exactneſs is the principal merit.

I am,

Dear Sir,

With much regard, &c.

To Dr. Yeats, THOMAS BEDDOES.
Bedford.

APPENDIX, No. IV.

LETTER *from the* AUTHOR *to Dr.* INGEN-HOUSZ, *relative to ſome curious Obſervations of* MAYOW, *mentioned in Sect. IX, Part Firſt, of this Work.*

Dear Sir, *Bedford, Jan.* 21, 1798.

When I had the pleaſure of meeting you in London, I mentioned to you, that Mayow, of the laſt century, entertained an opinion, ſimilar to your own, but which you firſt proved by experiment, *viz.* that the ſoil abſorbs oxygen from the atmoſphere. As I know you take delight in chemical inveſtigations, as ſoon as I arrived here, I ſat down to ſend you that paſſage from Mayow, which is very remarkable, for the expreſs manner in which he aſſerts this curious fact. This ingenious author, when ſpeaking on the neceſſity of a continual ſupply of fire-air to the reſpiration of animals, obſerves, ' Adeo enim ad vitam quamcumque ſal iſtoc aereum neceſſarium eſt, ut ne plantæ quidem in terrâ, ad quam aeri acceſſus præcluditur, vegetari poſſint, ſin terra iſta aeri expoſita, ſale hoc fœcundante denuo imprægnetur, ea demum plantis alendis iterum idonea evadet.

Plane

Plane ut vel ipſæ plantæ aliqualem reſpirationem aeriſque hauriendi neceſſitatem habere videantur.'

Does it not appear, from this paſſage, that the idea, that fallow was neceſſary for the abſorption of oxygen, preſented itſelf to Mayow? He has ſeveral other paſſages, to the ſame purpoſe. In my work, on the anticipation of modern diſcoveries, I ſhall take notice of this curious coincidence of thought, between you and Mayow. Do you not imagine, that oxygen, when abſorbed by the ſoil, enters into various combinations, for the food of plants? I know you have aſſerted this in your valuable publications; but you ſeem to attribute the moſt to carbonic acid.

You have mentioned ſome curious facts, in your ingenious letter to Sir J. Sinclair, with reſpect to the utility of alkaline ſalts in vegetation. Mayow has ſome remarkable ideas upon this ſubject. When you made the experiment with the acid, in Baron Dimſdale's garden, do you imagine any other effect to be produced by them, beſides diſengaging carbonic acid, by their union with calcareous earth?

Will not the oxygen, abſorbed in ſuch large quantities by the ſoil, aſſiſt in forming thoſe vaſt reſervoirs of water found in the bowels of the earth? Is it not partly intended for this purpoſe? I think it probable.

I remain, &c.

To Dr. Ingen-Housz. G. D. YEATS.

APPENDIX, No. V.

ANSWER *from Dr.* INGEN-HOUSZ, *containing an Account of his Discovery of the Food of Plants.*

Dear Sir, *February* 27, 1798.

I ſhewed your letter to Dr. G———, who was much pleaſed with the peruſal of it. I think the paſſage of Mayow very ſtriking, as are many others of that ſagacious man. You know, that Dr. Prieſtley and Mr. Scheele were of opinion, that plants thrive better in putrid, than in common air; and that vital air was hurtful to them. I diſcovered this to be a great miſtake; for I think I have proved, that the ſame air, which is ſalubrious to animals, is alſo ſalubrious to plants; and that plants will even live ſo much the longer in vital, than in common air, in proportion as the vital air is ſuperior, in purity, to the common air; and I point out a plain reaſon, why a plant, ſhut up in the dark, with a given quantity of common air, cannot be kept alive, but for a very limited time, though the carbonic acid be daily waſhed from the air; but that the ſame plant, ſhut up in the dark, with

with an equal quantity of vital air, (the carbonic acid being washed off) may be kept alive, as long as there is enough of that air remaining, to cover the plant. The death of the plant, in the common, and in the vital air, is produced by a very different cause. I do not know, whether you have seen the French edition of my work on vegetables, in two volumes, (there are three volumes of the German edition) where I explain this by facts, which you will find accurate.

As Mayow has anticipated so many things, discovered one hundred years after him, you may possibly also find, in the works of this great genius, what I have imagined to be my original discovery, about light and darkness. If you should find that doctrine, I hope you will inform me of it, as I should rejoice to have been, by accident, lucky enough to coincide with the opinions of so great a man. I do not want to repeat, what I here mention, as you must know it, *viz.* that plants correct injured air, and improve good air, by the assistance of the day or sun's light. Dr. Priestley found, before me, that there was such a power in plants; but confessed he was ignorant of the condition, necessary to make plants perform such an office, as he and Mr. Scheele observed, that they very frequently injured air. Dr. Priestley, also discovered, that a slimy, filmy matter, spontaneously deposited from well-water, had a power of producing

producing pure air, in the fun only; and Mr. Berthollet proved, that pure air is produced from oxygenated marine acid, by the fun; which air was alfo found to be generated, by means of the fun's light, from nitrous acid, by Mr. Scheele. But neither of thefe philofophers have uttered a word, that thefe bodies were of a vegetable nature, or have drawn any conclufion from thefe remarkable facts; neither have they given it, as their opinion, that as thefe bodies give out vital air, by means of the fun's light, that the effect of plants, in mending the air, was owing to the fame caufe; and, indeed, no one, in his found fenfes, can doubt, but that they would have mentioned it; if they had known, or even fufpected it, before 1779, when I publifhed my work on that fubject.

The importance of the difcovery, could fcarce fail of exciting an envy, in a mind fo laudably fond of philofophical merit, as that of Dr. Prieftley, and induce him to employ all poffible ingenuity, to make his readers believe, that he found out the myftery, by explaining the whole of it, without even mentioning my name. He was, however, not the only one, who laboured under the difficulty of finding out fome turn of expreffion, to draw the difcovery to their fide. Private letters were quoted by fome, private converfations by others, and many other far-fearched arguments. But to their mortification, not one of my competitors has ever ventured to ſay, openly, that they have publiſhed before me, that plants have ſuch power, by the aſſiſtance of the ſun's light. But you are, perhaps, ignorant of what ſtill puzzles ſome of my competitors, and puts them, even now, to a kind of torture; it is this, by finding, that a plant ſhut up with air had ameliorated it, it was impoſſible to conclude, from ſuch a fact alone, that it was the ſun's light, and not vegetation, as ſuch which had produced the effect, before examining another portion of the ſame air, after it had been expoſed to the influence of a ſimilar plant, in a ſhaded or dark place. Now the ſame teſt, by which it was diſcovered, that air expoſed to the ſun, became better, muſt have alſo pointed out, that the air, placed in the dark, was injured; and it was by this double examination I diſcovered both equally wonderful phænomena.

There is, indeed, more ſingularity in the effect of plants on air, in the dark, than in its effect on air in the day-light, as no one ever doubted, but that vegetation improves the wholeſomeneſs of countries. What other reaſon could be given, for the total ſilence of Dr. Prieſtley, on the effect of plants on air in the dark, but the unavowed averſion of quoting any ſuch examination made after my publication, as neither he, nor any one elſe, had even ſo much as ſuſpected, that ſuch a wonderful power exiſted in plants. Mr. Senebier, and ſe-

veral

veral others, after having diſputed with me the fact (though it required but a few hours to verify it) during more than twelve years, and even declaring the doctrine to be a calumny againſt nature, have ſince publicly declared, that ſome inadvertency had made them fall into an errror, and that they acknowledge Dr. Ingen-Houſz's diſcovery to be founded in the laws of nature. Dr. Prieſtley is the only one, who ſuppreſſes the whole nocturnal influence, by a profound, and as ſome think, an affected ſilence. The reaſon of this ſilence is obvious enough. I remember a learned profeſſor coming from Geneva (where Mr. Senebier had perſuaded him, that the doctrine of the nocturnal influence of plants on air was totally erroneous) addreſſed me in the following words, though, as you will eaſily believe, accompanied with a ſneer. 'If the nocturnal influence of plants on air can be demonſtrated as true, it would be one of the moſt wonderful diſcoveries ever made; a diſcovery diſcloſing a phænomenon produced ſince the creation, by one half of the organiſed world, and which was not ſo much as ſuſpected by any mortal before you; it would make an epoch in natural philoſophy. But, my dear friend, continued he, I am ſorry to tell you, that you muſt not flatter yourſelf with ſuch an honour, for you will ſoon ſee, in Mr. Senebier's book (then in the preſs) how much you are miſtaken.' As I am now in the country, without my books, I cannot point out to you, the places where I ſpeak of the air, as the nouriſhment of plants. My opinion has always been, and is ſtill, that air, abſorbed by plants, as well as water, ſalts, &c. undergo various chemical operations, and are variouſly modified, in their organs, as food is modified in the organs of animals. I repeat this doctrine, in my pamphlet on the food of plants, and the renovation of ſoils. Plants imbibe, by their leaves, atmoſpheric air, with all its ingredients; and this air is totally renewed, in leſs than half an hour, day and night. But I think, that the roots and the flowers imbibe a great deal of it, after it is changed into carbonic acid. I adduce facts to prove it; and as flowers prepare, inceſſantly, carbonic acid, to rear up the new offspring, I argue from it, that the principal part of the nouriſhment of plants, is air. I have thrown ſome more light on this my opinion, in a letter quoted in the pamphlet on the food of plants, addreſſed to Sir John Sinclair, Preſident of the Board of Agriculture;* but which makes no part of the pamphlet. There was, alſo, a further article on alkaline ſalts, as manure, in that letter, as alſo in a former. Theſe letters occaſioned a correſpondence

* Lord Somerville is, we believe, now Preſident.

refpondence between Sir John Sinclair and myfelf, who, through zeal for his fociety, engaged me to put in writing, fome further reflections on agriculture, which might occur to my mind, if they were but mere hints.

It would be too tedious, for you to read all my conceptions on the effect of acids, and other manures. My mind is fometimes full of them; but I neglect to put thefe fugitive thoughts to writing, as not worth attention. You feem to think, by one of your queftions, that, according to my opinion, nothing is to be expected further from acids, thrown upon the foil, but the production of carbonic acid, as you may, perhaps, confider the union of the acid with the calcareous earth, as a permanent body; but this may be an erroneous opinion, as gyps expofed to the chaotic mafs of vegetable foil and air, is eafily decompofed, and affifts the putrid fermentation, neceffary for promoting vegetation. I cannot anfwer your queries about oxygen, conceived by you as productive of vaft quantities of water, as I never confidered the queftion. I think your experiment with the frog, very deferving notice:* I hope you will continue thefe ufeful purfuits. You will

* A frog was made paralytic with mercury, in which the animal electricity did not produce its ufual effects.

will excuſe me, in engaging ſo much of your time, in conſideration of my great eſteem for you; with which I profeſs to be,

Your's,

To Dr. Yeats, J. INGEN-HOUSZ.
Bedford.

APPENDIX, No. VI.

LETTER *from Dr.* SHERER *relative to* MAYOW.

Sir, *London, March* 2, 1798.

Your kind and instructive letter, puts me under very great obligations, and much as I wished to accept your liberal offer, I am sorry to say, my time is too short, to allow me the satisfaction of your personal acquaintance, and must, therefore, content myself, with having obtained it by correspondence only. From all that I can collect, I conclude you to be a sincere friend to chemistry; and this circumstance alone, will probably be sufficient to cement, when I am at a greater distance, your valuable acquaintance. I have dedicated all my life to chemistry, and my enthusiasm for it always increases, when I have the opportunity of forming acquaintances, such as yours; and hope, we shall have, in future, means to communicate each others opinions upon chemical subjects; it being one of my first desires, to continue my connections, by correspondence with men of every country, where chemistry finds its admirers. I expect your publication, with the greatest anxiety. *Mayow has not yet met the attention he merits,*

*merits. Weigel, Mertſger, and Haller, have but very imperfectly made uſe of his writings.** The paſſage of his, concerning transfuſion, I really do not recollect.† I flatter myſelf, your publication of Mayow will be complete. I ſhall not think of publiſhing my own, till I have read and ſtudied yours. In reſpect to Munday you are very right, and I was tempted by my nameſake to expect more from him.‡ My preſent departure for Paris, this day, occaſions ſo much confuſion in my mind, that I beg you to excuſe this unſatisfactory letter. With the utmoſt regard, I have the honour to add, the aſſurance of the ſincerity with which

I am, &c. &c.

D. ALEXANDER NICOLAUS SCHERER.

To Dr. Yeats,
Bedford.

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* The reader will ſee, by conſulting Mr. Cavallo's Treatiſe on Air, p. 508, that he alſo had a very imperfect idea of Mayow's diſcoveries.

† The paſſage concerning which the author wrote to Dr. Beddoes.

‡ Vide Introduction to Part Firſt.

FINIS.

DIRECTIONS to the BINDER.

THE HEAD of MAYOW to face the Title.

The other PLATES to be inserted immediately before the APPENDIX.

CORRECTIONS.

P. 10, l. 10, *for* as, *read* ſo.—P. 12, l. 12, *for* form, *read* prove.—P. 14, l. 11, *for* liberty, *read* levity.—P. 29, l. 2, *for* attaching, *read* attacking.—P. 29, l. 12, *for* putrefaction, *read* putrefactive.—P. 47, l. 1, *for* become, *read* becomes.—P. 58, l. 11, *for* generaly, *read* generally.—P. 61, l. 13, *between* ſtring *and* to, *insert* tied.—P. 81, l. 14, *for* hypothiſis, *read* hypotheſes.—P. 87, l. 21, *for* effuſion, *read* affuſion:—P. 89, note ‡, *for* digeſtion, *read* indigeſtion.—P. 90, l. 11, *for* ultimately, *read* intimately.—P. 91, l. 6, *for* fluid, *read* florid.—P. 93, l. 18, *after* allows, *expunge the comma*.—P. 93, l. 22, *for* in, *read* to.—P. 98, l. 3, *expunge* a.—P. 97, note *, *after* P. *add* 113.—P. 114, l. 7, *for* had, *read* has.—P. 117, l. 8, *for* event, *read* want.—P. 117, l. 16, *for* mutual, *read* maternal.—P. 119, l. 11, *for* air, *read* blood.—P. 122, l. 12, *for* is, *read* are.—P. 157, l. 18, *for* varies, *read* raiſes.—P. 159, l. 15, *after* ſhut, *add* up.—P. 168, l. 3, *for* nerves, *read* animal ſpirits.—P. 215, in the note, *for* of, *read* and.—P. 225, in the note, *for* tinged, *read* turgid.—P. 227, l. 2, *for* the, *read* that.—P. 241, l. 19, *for* reſt, *read* zeſt.—P. 244, l. 21, *before* the, *add* was.—P. 258, l. 3, *before* account, *add* an.—P. 282, l. 22, *for* or, *read* and.—P. 321, in the note, *for* Waller *read* Haller.—P. 325, l. 15, *for* ſhews, *read* ſhew.—P. 322, l. 7, *for* braided, *read* beaded.—P. 355, l. 7, *for* flowing, *read* glowing.—P. 359, l. 16, *for* to, *read* of.—P. 361, l. 17, *before* of, *add* is.—P. 384, l. 18, *for* Schereris, *read* Scherer's.