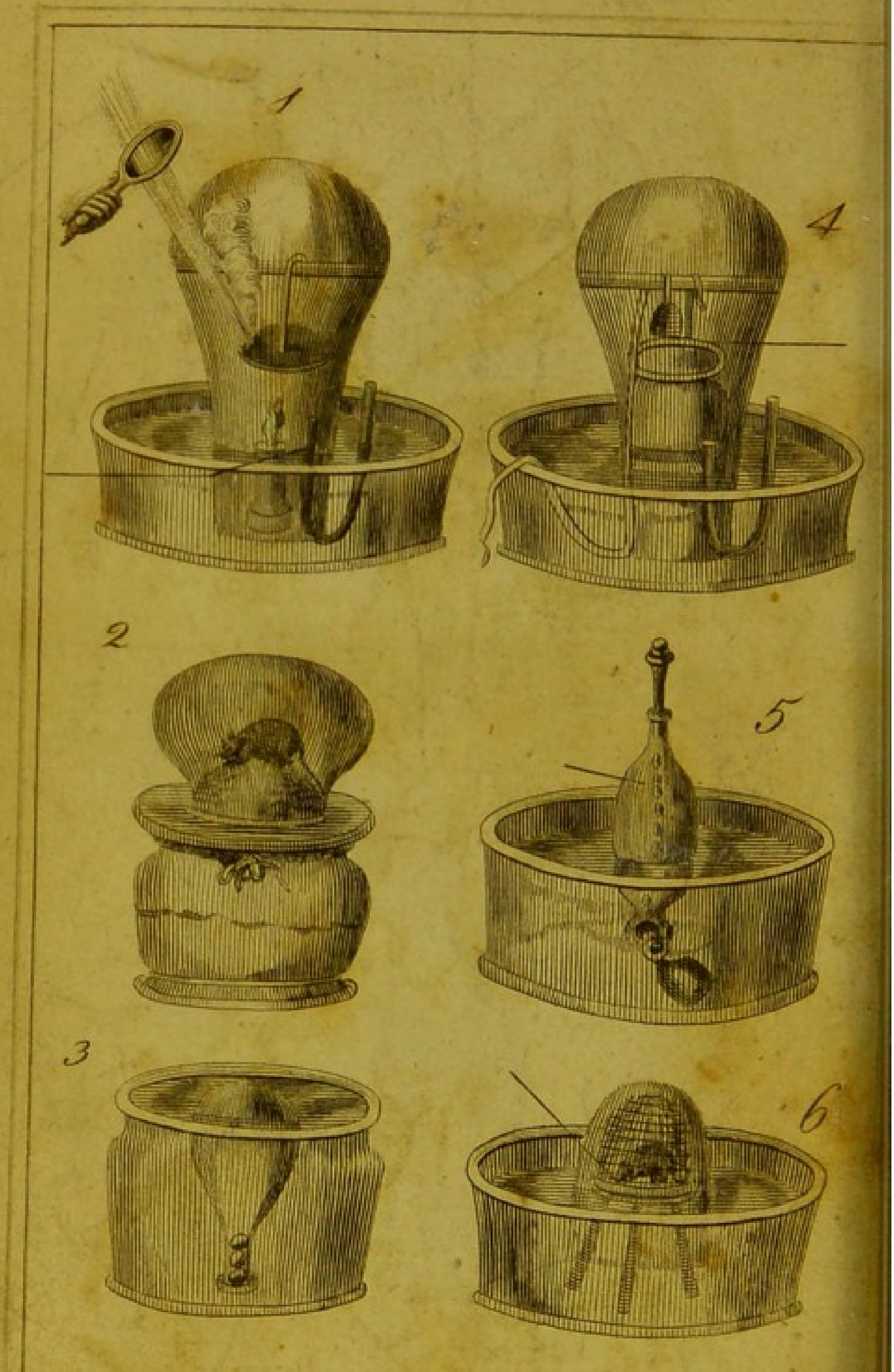
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CHEMICAL EXPERIMENTS

AND

OPINIONS.

EXTRACTED FROM

A WORK

PUBLISHED

IN THE LAST CENTURY.

(3)

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TO

EDMUND GOODWYN, M. D.

Dear Goodwyn,

I AM not ſo unreaſonable as to require you to remember every thing that paſſed in our correſpondence, or in our various converſations at Edinburgh and at London, upon a ſubject, which three or four years ago, and indeed both at an earlier and later period, much employed your thoughts. You may however poſſibly recollect your offer to beſtow upon a letter of mine the honour of annexing it to a certain treatiſe* on the leading vital function of ſuperior animals: neither can

* The Connexion of Life with Reſpiration. By Edmund Goodwyn, M. D. 1788.

you ſurely, any more than myſelf, have forgotten (for from whoſe memory are diſappointments ſo ſoon effaced?) how, though in compliance with my wiſhes, and to encourage my deſign, you ventured to tranſmit your corrected manuſcript to Oxford, accident totally deprived me of the advantage intended me by your kindneſs: for between my abſence, and the negligence of thoſe through whoſe hands it paſſed, I never received it, and, what would have been of much worſe conſequence, it had very nearly been loſt.

Having thus been defrauded of the benefit of ſuch an invitation to ſtate more fully and explicitly what I had dropped in the looſeneſs of converſation, and what you, I ſuſpect, did not more than half believe, I deferred the execution of my purpoſe to a diſtant and indefinite day; my thoughts were diverted into other channels, and a doubt aroſe, no ill-timed doubt perhaps, how far my elucidation of an obſcure but memora-

ble

ble paſſage in the Hiſtory of Science would be of ſuch importance as deſerve ſeparate publication, and ſo my intention became fainter and fainter, and was in a fair way to be laid aſide for ever.

Opportunity, however, the power that prompts ſo many actions of man, has unexpectedly revived it. Hitherto, though I had often tried to procure for myſelf the Collection of Mayow's treatiſes *, I had only ſeen it in our Public Library, where the Dæmon of Froſt, in league with ſuch jealous regulations as the mature experience of Göttingen and Edinburgh has ſhewn to be unneceſſary to the preſervation of books, deprives us of a great part of the benefit it might afford, if our ſtatutes a little leſs reſembled, in the moſt abominable quality incident to human in-

* Tractatus quinque Medico-phyſici. Studio Johannis Mayow, L L D. et Medici; necnon Coll. Omn. Anim. in Univ. *Oxon.* Socii. Oxon. 1674. Copies, I have been told, though of another edition, are more common abroad. I could never find the original edition of the ſecond treatiſe. See below, p. xxx.

ſtitutions,

ſtitutions, the laws of the Medes and Perſians. At laſt, however, I had the good fortune to meet with a copy in private hands, and to be permitted to carry it away with me, and to keep it as long as I ſhould deſire. And now, as I again turned over the leaves, and muſed at leiſure over their extraordinary contents, my former ſentiments, thoſe ſentiments by which I was firſt led to think of introducing to you modern philoſophers, an ELDER BROTHER ſcarce more reſpectable than unknown, were renewed with undiminiſhed force; and an additional circumſtance gave freſh ſtrength to my purpoſe.

For in the mean time I had not failed to talk to others as to you, and in perhaps ſtill ſtronger terms. I had even ventured publicly to beſtow upon an author, of whom hardly any body had heard, commendations thought ſtrongly to ſavour of extravagance and enthuſiaſm. They, however, had the effect of ſpreading a little the

knowledge

knowledge of his name, and in ſome meaſure of his writings; and ſhould theſe make upon readers in general the ſame impreſſion as they have lately made upon ſome, whoſe opinion may well paſs for a very favourable omen, then due honours will at laſt be paid to the genius of Mayow, and perhaps a ſide glance of approbation may occaſionally be caſt upon the deſign of reſcuing from oblivion his long-loſt memory. Should I aſk you, who of all your acquaintance is the perſon leaſt likely to be overtaken by ſurprize, you would, I think, name a certain Northern Profeſſor to whom you and I may have our obligations; and every one who has the good fortune to know him, muſt concur with you in opinion: Yet, at ſight of the annexed repreſentation of Mayow's pneumatic apparatus, this ſedate philoſopher lifted up his hands in compleat aſtoniſhment. I could mention other inſtances equally in point; but when the evidence, as in the preſent caſe, is full and direct as the teſtimony of the ſenſes, we ſeldom pay our own powers of perception ſo ill a compliment as to go to others in ſearch of confirmation. I cannot, therefore ſay that my own conviction has been greatly ſtrengthened by this coincidence. I own however that it is agreeable and flattering; and perhaps it has given my induſtry that ſpur which it ſo often needs.

But it is not with indolence alone that I have had to contend: it has alſo been neceſſary to prevail over prudence, and the victory has not been accompliſhed without a ſtruggle. For beſides the riſque encountered by all who expoſe themſelves to general cenſure, there are, I am apprehenſive, ſeveral circumſtances that aggravate the imprudence of the ſtep I am about to take.

Mayow himſelf certainly will not reward the flattery or praiſe which he can no longer hear. What pity, that he ſhould have left behind him no poſterity

poſterity whoſe bounty may fill the hand that draws aſide the veil which time and chance have thrown over the glory of their anceſtor! In this penury of natural patronage who would not have looked out for a Peer at leaſt, connected by a common college or ſome imaginary tie with the philoſophical hero whoſe memory he was endeavouring to revive? Inſtead of this, behold in the very ſpot conſecrated to dedication a plain and familiar letter to a perſon who has none of thoſe things to beſtow, after which we writers ſo hunger and ſo thirſt. If there be in all this a veſtige of prudence I would gladly know, what you call inconſiderate and thoughtleſs?

Still, I fear, there may be worſe behind. Mine is not by many the firſt attempt to refer modern advances in ſcience to an earlier date. Numbers before me have repeated that there is nothing new under the Sun, and I for one have generally heard the exclamation with inattention or contempt.---

Sometimes

Sometimes I have fancied I perceived nervelefs Ingenuity ftraining in vain to pile up mountains of paradox; fometimes it has been Oftentation difplaying a mouldy ftore of captious and quibbling erudition, and fometimes, I am afraid, Malice, in the fpirit of Macbeth's Weird Sifters, employing her fpells and incantations to blaft living reputation.

Here excufe me for a moment; for I am violently tempted to ftep afide; you, I truft, will admit the force of the temptation; it is to gather an anecdote; the bye-path that leads to it will immediately return into the main road. I once happened to meet with the perfon who for five and twenty years had been Voltaire's amanuenfis; I would tell you his name; but alas! I begin to find that names and numbers too eafily flide out of my memory. He fhewed me a book that had belonged to his employer: I thought it a great curiofity; not that I fhould value a book a rufh the more, had it merely belonged to a greater than Voltaire; but in this book might be ſeen ſomething of the method by which a writer of genius prepared himſelf for the exerciſe of his art. The margin was crouded with remarks all lively and keen (ſuch was the nature of the man) moſt of them pertinent.--- Slight as he may often have been found in his enquiries as he was ſometimes ſufficiently malicious in his diſpoſition, —in ſhort a moral and intellectual character chequered with light and ſhade— I ſaw not here a trace of that picture which we are ſo apt to ſummon before us; of the ſuperficial Frenchman miſunderſtanding in his haſte what ſtood diſtinctly in print before him, and in the fermentation of fancy ſubſtituting ſentiments quite foreign to thoſe of the author. On the contrary he read with his thoughts collected and his attention rivetted; nothing inconſiſtent or inconcluſive ſeemed to eſcape him; and he condenſed many obſervations into a line, which had there been ſpace, he might reaſonably have ſpread over a page.

Among

Among theſe I was much ſtruck with one at the time, and it now returns upon me with double force.

It ſo happened that thoſe phænomena of literature, on which I have juſt been beſtowing a few metaphors, the claſſi-cal or antiquarian bigots who have laid out their learning in efforts to adorn the hoary head of antiquity with the laurels of modern merit, came acroſs the author's imagination; and from among them one of the moſt liberal —I mean to the dead—was quoted as advancing that certain recent theo-ries were known to the antients. "Non" exclaims the ſarcaſtic commentator, "il ne le croit pas lui-même: il l' a ſeulement imprimè."

Whether this ſtricture be juſt or only ſhrewd, you, if you will take the trouble of comparing it with your ex-perience and obſervation, ſhall judge for yourſelf. I ſhould very ſolemnly proteſt againſt its application, if the reader's ſentence were to be regulated by by my proteſtations and not by the evidence which I ſhall produce. I can indeed conceive that it may be flattering enough to look down from the high watch-tower of wiſdom.—

——Sapientum templa ſerena
Deſpicere unde queas alios paſſimque videre
Errantes ——

And I might perhaps be miſchievous or frolicſome enough, but I own I want the courage to attempt to ſend, like April fools, in queſt of an Utopian wonder, ſo much gravity and decorum as is comprehended under the large and venerable title of the PUBLIC. Jokes like theſe could hardly be practiſed with ſafety, but upon inferiors or dependants or very familiar friends. Authors indeed there may have been on ſo ſafe a footing with the world; whoſe aſſertion would paſs for proof, and who might have ventured upon liberties, but it was firſt neceſſary to have eſtabliſhed their credit by much

and

and effectual ſervice — need I make the application to your correſpondent?

But when you recollect how ſtrangely diſcoveries have been ſometimes appropriated; how Solomon, Plato, and Father Paul have ſeverally been inveſted with the honour which Harvey derives from his inveſtigation of the circuit of the blood, you may be reaſonably deſirous to know on what principles I would have the merit of claimants decided. If by a looſe expreſſion, which he who uſed it, in all likelihood, threw out at random, and which impreſſed no idea upon the underſtanding of his contemporaries, then the ſooner you contrivers of difficult and deciſive experiments put a ſtop to your reſearches, the leſs you will loſe of your labour: In ſelf-defence, therefore, and leſt we be degraded into mere interpreters of myſticiſm and nonſenſe, it will be neceſſary to deal ſtrictly with theſe pretenſions of antiquity. I know indeed that the glance of genius has now and then anticipated the tardy

reſult

refult of inveftigation : and I acknowledge both the merit and utility of fuch happy conjectures. They are, as Haller well entitles them, "fcintillæ ad experimenta prælucentes", fparks fuch as kindle the light of experiment: yet how little do they detract from the praife due to thofe by whofe more perfect labour the truth is exactly defined and firmly eftablifhed ? neither will they eafily be made by chance; nor otherwife than by firft clearly afcertaining or at leaft conceiving extenfive principles, then rendering them familiar to the mind by reflection, and laftly purfuing the clue of analogy, where-ever it leads from them.

In the examination of fuch claims two things at leaft require attention. It fhould firft be enquired what has been expreffed, and next what has been proved. If the proofs be not cogent, then the honours due to difcovery may be juftly denied; and if deficient proof be accompanied by ambiguity of expreffion, may we not lay it down as highly probable that the paſſage bears ſome other meaning than that which coincides with the modern opinion? for to divine the ſecrets of Nature is by no means a characteriſtic of man; and very often we are acquainted with diſtant facts amounting to the proof of a propoſition, before we draw them together and make the inference.

Upon the whole I ſhould be aſhamed to ſtand up in ſupport of any one who ſhould ſhrink from the teſt of Pitcairne and Haller: "non eum verum inventorem eſſe cui vaga aliqua cogitatio elapſa eſt, in nullo fundata experimento, ſed eum omnino eam laudem mereri qui verum ex ſuis fontibus per ſua pericula ſuaſque meditationes eruerit et adeo firmis rationibus ſtabiliverit ut veri cupidos convincant"*.

If therefore Mayow has not clearly expreſſed his opinions, if he uſes ſuch ambiguous terms as require the aſſiſtance of modern diſcoveries to interpret

* Elem. Phyſiologiæ, I. p. 247.

them

them, if his experiments do not afford decisive evidence, if he has not deduced the consequences of his principles, then I am willing that the credit of one, who discovered far and that by the light of his own genius, should be withheld from him in as much as he has failed in any of these requisites. I can just conceive it possible that a single important discovery may be made without much sagacity: an undiscerning eye may perhaps by chance be so placed as to be aware of the manner in which nature performs some one of her hidden operations; but he who shall surprize her often, must be allowed to have the discerning eye, and to know where the proper point of view is to be found.

Then if we wish to make an estimate of merit, we must compare circumstances, and above every thing consider the imitative genius of man; for experience evinces that whatever we are capable of performing from the grossest mechanical application of the hand

to

to the moſt refined operation of the intellect loſes half its difficulty when we have once a pattern before us. It is not much that modern verſifyers are ſmoother than of old; it is not much that modern experiments are more concluſive, and modern reaſoning more juſtly drawn out from them, for there ſtand the living examples full in our view; and we have only the eaſier taſk of expreſſing them by a reſembling imitation. But let any one ſhew me where Mayow might find formulas ready to his hands by which he could regulate his facts and his reaſonings; a ſet of experiments bearing ſo directly on the point at which he aimed; or a chain of conſequences deduced with ſagacity equally enlightened, and ſo much patience of thought. Conſider only the quantity or maſs of truth which he ſurely detected "per ſua pericula ſuaſque meditationes" and then name among his predeceſſors or contemporaries,—I had almoſt raſhly added—or his ſucceſſors, a rival fitted to

contend

contend with him for the palm of philo-ſophy. But when you attend to its qua-lity, you will ſee that he anticipated the diſcoveries of ſucceeding ages, and was ſcarce ſubject to the laws by which other minds are regulated in their re-ſearches into nature; for experience ſhews, and it is agreeable to what we know of the human underſtanding, that our acquaintance with the mate-rial world proceeds in a regular train; and that before we can arrive at a given diſcovery, a certain ſum of prepara-tory knowledge muſt be accumulated. Thus you yourſelf, when ſpeaking of one of thoſe points which Mayow has moſt happily illuſtrated, obſerve that "the reſpiration of animals has long been a difficult point in Phyſiology, and no ſatisfactory ſolution could be given of it whilſt Chymiſtry continued in an uncultivated ſtate: the general facts were indeed attended to and conjec-tures propoſed to explain them*; but

* "Sir I. Newton imagined that the atmoſpheric air might communicate an acid vapour to the blood of the lungs, which

as long as conjecture *only* was uſed, doubt and uncertainty remained.

Within theſe few years the knowledge of Chymiſtry has been much improved, and experiments have been applied to reſpiration with conſiderable effect; new facts have been diſcovered, extraordinary phænomena explained, and conſiderable difficulties removed †."

The accidental appearance of a great genius may indeed haſten the pace of philoſophy, yet is its fate independent of individuals: Had Newton never exiſted, is it poſſible, think you, that aſtronomy ſhould have reſted where he took it up; would not rather twenty ſucceſſive Keplers have divided among them the labour of the Heavens?

Newton's diſcoveries concerning light, I cannot help fancying, ſtand in the ſame predicament with Mayow's on air:

was neceſſary to keep up the action of the heart. OPTICS, p. 351."

† Connexion of life, &c. Introduction, p. 8.

to me both exhibit themſelves as the greateſt deviations, preſented by the whole hiſtory of Science, from the ordinary and natural progreſs of knowledge; they would undoubtedly (we ſee that it has actually happened in the one caſe) they would undoubtedly have been ſometime made, but not till a century or two had improved the talent both of obſervation and of reflection; though our two illuſtrious countrymen, like the Progenitor of mankind when the Archangel had purged his ſight

"with euphraſy and rue"

were, by a peculiar privilege, admitted to the view of ſcenes reſerved for a diſtant poſterity.

The writings of Deſcartes, indeed, ſo much ſtudied in that age, might excite Newton to inveſtigate the nature of light. What determined Mayow to his particular reſearches? not, I think, the experiments of Boyle, ſo much as thoſe of Torricelli; after them the ſtudy of air languiſhed, and has only been fully revived of late.

Is not the general order of diſcovery influenced by at leaſt two great principles? If invention is ſharpened by neceſſity, will not, in the firſt place, thoſe qualities of objects be moſt likely to come ſooneſt forward to our notice in which we are moſt intereſted? but, ſecondly, the fulfilment of our wants and wiſhes muſt be regulated by our powers; hence we ſhall be apt to meet with the firſt ſucceſs in the eaſieſt enquiries, or, to expreſs myſelf with ſomewhat greater preciſion, in thoſe where effects are moſt apparent to the ſenſes. I ſhall not preſume to decide which of the two great branches of Natural Philoſophy contributes moſt to the uſe and ornament of life; but I think it natural that the mechanical department, where palpable maſſes of matter are brought into action, and motion is viſible in its progreſs, ſhould ſooner advance towards perfection than the chemical, where effects are produced by the inſenſible movements of imperceptible particles. It ſeems therefore to have its foundation in the order of things that the philosophers of the preceding age ſhould have been employed chiefly by Aſtronomy and Mechanics, and thoſe of the preſent by Chemiſtry. Yet if the rule be at all juſt, what a violent exception —ſuffer me to repeat it—have we in the inſtance of Mayow, who ſilently and unperceived in the obſcurity of the laſt century, diſcovered, if not the whole ſum and ſubſtance, yet certainly many of thoſe ſplendid truths which adorn the writings of Prieſtley, Scheele, Lavoiſier, Crawford, Goodwyn, and other philoſophers of this day?

He threw away with ſcorn the vague ideas annexed by the old chymiſts to the terms ſulphur, mercury, &c. He has clearly preſented the notion of phlogiſton, which rendered the name of Stahl ſo celebrated. He perceived the action of dephlogiſticated air in almoſt all the wide extent of its influence; he was acquainted with the compoſition of the atmoſphere, and contrived to make the mixture of nitrous and atmoſpherical air. He was well aware of the cauſe of the increaſe of weight in metallic calces, and diſtinctly aſſerted that certain baſes are rendered acid by the acceſſion of nitro-atmoſpherical particles, or what has ſince been denominated the acidifying principle. He diſcovered the method of producing factitious gas, and obſerved its permanent elaſticity; and what is ſtill more ſtrange, he invented the nice art of transferring it from veſſel to veſſel. The doctrine of reſpiration is all his own. He has carried on his inveſtigation of this function from the diminution of the air by the breathing of animals (as well as the burning of bodies) to the change it produces in the blood during its paſſage through the lungs and the uſe of the placenta. But the number and extent of his diſcoveries will beſt appear from the following analyſis. It would however be uncandid and fooliſh not to acknowledge that he too has paid the tribute of error, which, according to

Senac's

Senac's expreſſion, is ſtill due from human imperfection. His mind, as uſually happens from the impulſe of new diſcoveries, was tranſported too far in its ardour; and he has extended his principles beyond the bounds of truth. But let us remember that the philoſopher who opened to the view of modern times the operations of that very claſs of agents, which Mayow in vain pointed out to his contemporaries, once imagined that metals in their ſtate of calx might become heavier from acquiring fixed air. But does an erroneous conjecture take away from the general merit or the force of his proof where it compleatly aſcertains the properties of this elaſtic fluid? Though Mayow imputed thunder and lightning to his powerful principle, the experimental evidence of its real effects ſhould certainly not be thought to ſuffer from that miſtake. The cauſe of each miſtake ſcarce requires to be told. Nature, we now know, employs different elaſtic fluids to execute many of her purpoſes; but how, unleſs we ex-

pect

pect an individual to find out every thing at once, can we wonder, if upon the firft difcovery of one of the moft powerful of thefe agents, the fphere of its dominion fhould be too widely exended, while the claims of its rivals are yet unknown? But I will interfere no further with the reader's fpeculations upon the mixture of truth and error, which the pages of Mayow may contain. The following table of contents will render any obfervations of mine fuperfluous: it is a clofe tranflation from the original, except in one or two places, where, for the fake of perfpicuity, I have exchanged the notices of the index for the fuller expreffions of the text. It may be confidered as a faithful map of the author's opinions; and will ferve to fhew not only where but how he was mifled; I prefume at leaft, that by the help of this table, and the fubjoined analyfis, any one acquainted with the outlines of Chemiftry may perceive the progrefs by which his opi-

nions

nions ſucceſſively ramified from the trunk of his principles.

In this analyſis the chemical doctrines of the author have been my object. The Collection of Tracts is divided by himſelf into two parts, of which the latter contains two eſſays; one on muſcular motion, and one on the rickets: It ſeems as if the three eſſays of the former part were thoſe, to which he rather choſe to entruſt his reputation, or which he at leaſt regarded with moſt affection; ſince in his dedication he brings them forward to the notice of his patron, with the confidence of a man who feels how much he has enriched philoſophy. "Theſe exerciſes," ſays he, "will, I flatter myſelf, ſuggeſt reflections capable of filling up a few hours of leiſure uſefully and agreeably. In the eſſay on reſpiration, the whole machinery of the thorax is taken to pieces and the cauſe of many effects explained from anatomical experiments: and what I have written of nitre, pervades almoſt

the

the whole of nature, and explains many abſtruſe things, upon which of the innumerable crowd of writers ſcarce any one has ventured to touch: and though ſome other points have been treated alſo by other authors, yet have they been left in obſcurity." Perhaps, however, he thought the two laſt eſſays more confined by their ſubject to a particular profeſſion, and therefore leſs likely to engage the attention of Coventry. They do not fall within my deſign, but in order to ſatisfy the reader's curioſity, I have haſtily run over their contents.

In extracting and abridging thoſe parts of the three firſt treatiſes that relate to Chemiſtry, I have aimed at preſenting the author's opinions juſt as he conceived them. During the performance of this taſk it was ſome relief to interſperſe a few reflections, ſuch as aroſe without enquiry or pre-meditation; may the reader derive equal refreſhment from them! The *experiments*, by which the author's merit

muſt

muſt principally be decided, are related at length in his own terms. They are far from numerous; yet do they not afford full proof of the inferences; and what more can we deſire? Heie I think we have evidence of ſuperior ſagacity that cannot be controverted; for the talent of contriving an experimentum crucis, of making the powers of bodies exert themſelves in ſuch a manner as to exclude all hypotheſes but one, will be found very ſparingly diſtributed among the experimenters of every age.

He who diſcovers new things, neceſſarily brings upon himſelf the taſk of inventing new names: a taſk which the experience of our own times has abundantly ſhewn not to be eaſy: yet in the language of Mayow, who denominates ſubſtances from ſome of their properties, there is nothing obſcure, and perhaps, nothing more harſh than may be found in later writers, and is univerſally felt in new terms: He calls the

the great principle which he has purſued ſo far through nature, by a variety of titles: vital ſalt, becauſe this peculiar kind of air is neceſſary to reſpiration: igneous ſalt, becauſe it is neceſſary to the ſupport of fire; the term ſalt ſeems in his time to have been very indefinite; ſometimes it is denominated fire-air particles; and in this particular, the coincidence between the terms of two great Chemiſts of different ages, Scheele and Mayow, is remarkable; for it is to be obſerved, that in Scheele's original treatiſe on Air and Fire, there is no foundation for the vague poetical term *empyreal*, which has been introduced among ſo many other miſtakes in the Engliſh tranſlation. The author, bearing in mind one of its moſt remarkable qualities, and taking advantage of a language more flexible perhaps than any other to apply itſelf to new conceptions of the mind, compounded, as Mayow had done before him, the preciſe term, *feuer-luft*, fire-air: nitro-atmoſpherical, nitro-aerial ſpirit,

rit, or particles, is another name frequently used as synonymous with the preceding, and sufficiently expressive of a principle common both to nitre and the atmosphere.

Notwithstanding the length to which this letter has been protracted, you, I trust, and every reader, will yet have a little attention left for the particulars of Mayow's life, as far as they can be collected, and a few observations on the fate of his writings.

Wood furnishes little else besides a few dates, and these, however scanty they may appear, are almost, I am afraid, all the memorials that remain.

"John Mayow," he informs us, "descended from a genteel family of his name, living at Bree, in Cornwall, was born in the parish of St. Dunstan's in the West, in Fleet-street, London, admitted a scholar of Wadham College, the 27th of September, 1661, aged 16 years

years, chofe probationary-fellow of All Souls College foon after, upon the recommendation of Henry Coventry, Efq. one of the Secretaries of State, where, though he had a Legift's Place, and took the degrees of Civil Law, yet he ftudied phyfic, and became noted for his practice therein, efpecially in the fummer-time, in the city of Bath, but better known by thefe books, which fhew the pregnancy of his parts.

De Refpiratione Tract. 1. } 1668.
De Rachitide, Tr. 1. } 1669, *Oxon.*

Of both which tracts is a large account given in the Phil. Tr. No. 41, p. 803. *an.* 1668.

De Sale nitro et fpiritu nitro-aerio.

De Refpiratione fætus in Utero et ovo.

De motu mufculari et fpiritibus animalibus. — *Oxon*, 1674, in a large quarto.

Of

Of thefe three laft (which were printed again with the two firft) is a large account given in the Phil. Tr. No. 105, p. 101, &c. and all five were printed together at the Hague, 1681, 8vo. He paid his laft debt to nature in an apothecary's houfe, bearing the fign of the Anchor, in York-ftreet, near Covent-Garden, within the liberty of Weftminfter, (having been married a little before, not altogether to his content) in the month of September, 1679, and was buried in the Church of St. Paul, Covent-Garden.* "

This fhort narrative fhews, that on account either of his patron or his merit, fome favour was fhewn him at his reception into All Soul's College. For of the forty fellows of that fociety, fixteen are free from the obligation of entering into orders, and the offices of the college devolve oftener upon them in the inverfe proportion of the numbers.

* Wood's Athenæ Oxon. II. 637. 1722.

This privilege is conferred by the Warden, and thoſe, who enjoy it, are now called Juriſts, with which I ſuppoſe Wood's term *Legiſt* to be ſynonymous.

The compariſon of the dates ſupplied by the Oxford antiquary will ſhew, that the treatiſe on Reſpiration was publiſhed in his twenty-third year; the treatiſe itſelf ſhews, that he had already made thoſe diſcoveries which he afterwards related at greater length in the treatiſe that now ſtands firſt in the collection. Intellectual eminence, if I am not much miſtaken, is far more early attainable in thoſe things which depend upon books and the internal workings of the mind, than in the obſervation of nature. Is there any other inſtance of one, who had ſo ſoon diſcovered a ſyſtem of natural operations ſo extenſive and ſo remote from the common apprehenſion of his times? The Vice-Chancellor's permiſſion for printing the collection of his tracts is dated July 17, 1673, that is, in his 28th, or early in his 29th year!

I know not whether there may not be perceived in the "large account" of the three latter treatiſes, in the Tranſactions, ſomething of an attempt to exalt Boyle at Mayow's expence; an attempt not neceſſary to ſupport the reputation, and ſurely altogether repugnant to the mild and modeſt diſpoſition, of that moſt amiable of philoſophers.— Did any ſparks, left here by the meeting of philoſophers, which afterwards grew into the Royal Society, fire the genius of Mayow? He certainly never figured among that body; as he would have done on his removal to London, if he had not died ſo much too ſoon for Science.

It may be added from Jöcher*, that the Dutch not only reprinted, but tranſlated Mayow's works ſoon after their appearance; an honour which they did not obtain from any other people.

* Gelehrten- Lexicon. *Art.* Mayow.

The ſame Lexicographer on the authority, I believe, of Morhof's Polyhiſtor*, affirms, that in his own age, his philoſophy found very little approbation. So much does the fortune, both of truth itſelf, and of thoſe who ſpeak it, depend upon the diſpoſition of the times in which it is ſpoken. His name, therefore, as it never was echoed by popular applauſe, was ſoon forgotten among men and his memory obliterated. In the Biographia Britannica there is no article appropriated to him, and what perhaps is more extraordinary, I think he is never mentioned by the biographer of the other great philoſopher of the ſame period †. Mr. Lavoiſier, in the hiſtorical part of his Eſſays paſſes on

* I wiſhed to ſee the paſſage in which this circumſtance is related, but alas, Morhof's Miſcellany is without an index— that principle which preſerves ſo many old books from total decay, and the want of which in ſo many modern books—— but why ſhould I raiſe melancholy ideas in the minds of my contemporaries?

† Birch's life of Boyle prefixed to his works, and ſeparately printed in octavo.

from

from Paracelſus and Helmont, to Boyle and Hales (who quotes Mayow's experiments on reſpiration and combuſtion) probably without ſuſpecting the exiſtence of ſuch an author. From Haller, whom nothing totally eſcapes, he has obtained ſome regard, though that learned phyſiologiſt had a very inadequate idea of his merit. You recollect that in the catalogue annexed to the laſt volume of the Elements of Phyſiology, the author has marked thoſe books which contain any thing original with a ſingle aſteriſk, and thoſe which he entirely approved with two. In this catalogue, Mayow's treatiſe on reſpiration is honoured with a ſingle note of approbation, but the Collection of his works is left undiſtinguiſhed from the herd of books manufactured by the pen or the ſciſſars alone. In the body of that great work, he is occaſionally quoted, and once at leaſt with honour: thus, " aer imprimis elaterem ſuum amittit per ipſam reſpirationem animalis. Primus id obſervavit Johannes

 Mayow.

Mayow *." He is alſo mentioned as the firſt modern who ſuppoſed the office of the placenta to be the ſame as that of the lungs, for to Hippocrates this notion is likewiſe attributed. How far what is ſaid of Mayow, in the Engliſh tranſlation of Scheele's Eſſay on Air and Fire, has been effectual in directing the public curioſity towards his works, I can ſcarce tell, but I do not myſelf know any perſon who was led to ſeek them by that recommendation, if recommendation it may be called, that degrades an experimental inveſtigation to obſcure hints. "John Mayow," we are told, "in his Opera Omnia, &c. Hague, 1681, has already given ſome obſcure hints about that part of our common atmoſpherical air, which properly ſpeaking, is the moſt proper" *(properly ſpeaking, is it not the only proper?)* "for reſpiration, or in which the flame of a candle will burn longer than in the ſame bulk of our mixed common air." . . . . "Mayow pre-

* Elem. Phyſ. III. 206.

tends"

tends" *(he pretends no ſuch thing)* "that ſome ſalt-petre, or even aerial ſpirit of nitre, flies about in the air, which when breathed, enters the lungs, and yields in the human body the animal ſpirits and heat, which it communicates to the maſs of blood. By reſpiration the air loſes that elaſticity which it had obtained from that kind of nitre, which he ſuppoſes in common air.—This dephlogiſticated air, though very obſcurely, ſeems to be hinted at in the *Diſſ. de Sal-nitro* and alſo *de reſpiratione*.—The honour of the diſcovery of this kind of air, is ſo much the property of Dr. Prieſtley and Mr. Scheele, though neither knew of the other's experiments, that there is not the leaſt doubt about it: but is it leſs certain that Mayow proved his theory in a manner ſo little convincing by experiments, that he cannot have the leaſt claim to any diſcovery? It is only curious that a looſe hint, thrown out at random, in that age, ſhould at ſuch a diſtance of time, in a new, more circumſtantial, and ample manner be confirmed in a ſeries of experiments, by two philoſophers, who certainly knew nothing of Mayow *." Should it be aſked, if the author of this note had read Mayow, and ſhould it be replied, Yes; then let it again be aſked, if he underſtood him; will that queſtion alſo be anſwered in the affirmative? One late writer on Phyſiology, I ſee, has been fully aware of the nature and importance of Mayow's diſcoveries. "Magna jam pars memorabilium horum phœnomenorum," ſays he, ſpeaking of reſpiration, "quibus nuperis luſtris et phyſica de aeribus factitiis diſciplina et phyſiologia negotii reſpirationis tam egregié ditata et illuſtrata eſt, jam ante centum et quod excurrit annos innotuit acutiſſimi ingenii medico Joanni Mayow, cujus de *ſal-nitro et ſpiritu nitro aereo* (quo nempe nomine dephlogiſticatum aerem inſignivit) tractatum, *Oxon.* 1674, 8vo. editum, magna cum voluptate legi et relegi †."

* Obſ. on Air and Fire. Note at the end of the author's preface, p. xiii.

† Blumenbach Inſtit. Phyſiologicæ, p. 114. Goettingæ, 1787.

I have now quoted latin more than enough to ſeaſon any letter, and more, I ſuſpect, than will abide the teſts of claſſical purity: it is uſual, you know, with the philologiſts to collect together, and add to the works of the author whom they publiſh, (ſcarce, I imagine, for the purpoſe of diminiſhing the ſize or price of the book) almoſt every thing that others have ſaid concerning him: but, as I by no means wiſh to ſee this practice introduced into books of ſcience, I ſhall trouble you with no more teſtimonies. I have deduced, as well as I could, the hiſtory of Mayow's writings to our own time. Henceforward, I flatter myſelf, that he will ſhare the glory of Verulam and Newton, and be named with due reſpect by all, by thoſe eſpecially who have never looked into his works; and that when the enthuſiaſm of an Engliſhman ſalutes his country, as

Magna parens frugum!—
Magna virum!—

Mayow will be ranked among the nobleſt productions of the latter ſort.

Should any one now enquire, in the ſimplicity of his heart,---for if the queſtion were dictated by any other ſpirit, I ſhould bid the enquirer ſeek information for himſelf—whether the ſum of honour, due to the moderns, is to be found by ſubtracting the ſhare of Mayow, let him be aſſured, that the diſcoveries of our times ſet out from a different point, and proceeded in a different train, in perfect ignorance of him, and therefore without any aſſiſtance from him.

When I reſumed the purpoſe mentioned at the beginning of this letter, I deſtined the execution of it to be the employment of a few winter days, and intended to offer it as one of thoſe little preſents, to which the cuſtom of a late ſeaſon gives occaſion. Delay, however, I believe, and enlargement beyond the original con-

ception,

ception, are very incident to authors. From the delay, as I was not impatient for publication, I have felt no diſappointment; but I wiſh, too late, that I had confined my account within a narrower ſpace. Such as it is, it muſt paſs the fatal boundary that divides an author from the public; for the preſs is even now about to cloſe upon my laſt words its ponderous and iron jaws: And circumſtances no longer permit me to new-model this ſlender MONUMENT to NEGLECTED GENIUS. Whether it be worthy of HIM in honour of whom it is erected, or HE be worthy of the labour of the Architect, now remains to be determined chiefly by thoſe who as yet know little of either. —

By one, at leaſt, I am ſure the workmanſhip will be viewed with all the indulgence that can be deſired by

HIS FRIEND
THOMAS BEDDOES.

OXFORD,
FEBRUARY. 12, 1790.

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The heat of moiftened quicklime does not feem to arife from particles of fire fixed in it---but from the effervefcence of contrary falts---proof that quicklime contains an alkali---as alfo an acid---why quicklime does not produce heat when fpirit of wine or any fulphureous liquor is thrown upon it---origin of the contrary falts in quicklime---why they do not effervefce unlefs water be thrown upon it---why the contrary falts in lime water do not deftroy each other---of the meeting of contrary falts---when mixed they do not quite deftroy each other---fpirit of nitre is fhewn to be a volatile acid---how a certain vitriolated tartar may be made from nitre---acids combine with metals---alkali will unite with fulphur---proof that fulphur contains no acid---different falts fhould not

be

be uſed in the ſame preſcription---the contrary ſalts of quicklime cannot combine more intimately---in what caſe contrary ſalts combine without heat---why liquors are rendered turbid by precipitation taking place in them ---why quicklime added to a ley of aſhes makes it more acrimonious,

Chap. XV.

Of the Bath waters—by the way of the origin of ſprings.

Of the ingredients of the Bath waters—they contain neutral ſalts—but no nitre—nor ſulphur in ſolution—which they will not diſſolve in a boiling heat, nor are they impregnated with ſal ammoniac—whether vitriol be contained in them—they contain a certain metallic matter---which is converted into vitriol on the addition of an acid---whence the heat of warm baths or thermal waters is derived---not from ſubterraneous fires---but a fermentation in the bowels of the earth---of the origin of ſprings---they do not ariſe from the ſea, but from rain water---how an artificial fountain may be made---why ſalino-ſulphureous ore (pyrites) acquires heat, when expoſed to moiſt air---air reſides in the pores of water---fiſh imbibe air from water---of the

the air contained in the ſwimming bladder of fiſh---thermal heat depends upon air---why ſpring water is a little warm---why ſoap cannot be diſſolved in it.

SECOND TREATISE

On Reſpiration.

HOW the air makes its way into the lungs--- not for fear of a vacuum--- nor becauſe the air is protruded by the breaſt, as it dilates— the inflation of the lungs depends on the preſſure of the atmoſphere—how this may ariſe from the elaſticity of the air—cauſe of the elaſticity of the air---the inflation of the lungs illuſtrated by examples--- the lungs cannot dilate themſelves of their own accord ---why the lungs protrude out of the cavity of the thorax when it is perforated---how the lungs are inflated when the thorax is wounded---how wounds of the thorax ought to be cloſed---in what way the breaſt is dilated---the external and internal intercoſtals ſerve to dilate it---why theſe muſcles are inſerted into the ribs in an oblique direction---why the thorax is always contracted in the dead-- the

the ribs are connected with the ſpine, by a double articulation---thoſe articulations contribute to the dilation of the thorax---why the ribs are joined to the ſternum by intervening cartilage---the diaphragm helps to dilate the thorax---of the various ways in which reſpiration is injured---of the diſorder of horſes, called broken wind---of orthopnaea---of uterine ſuffocations---of other aſthmatic paroxyſms---why a whiſtling noiſe ſometimes accompanies reſpiration : of ſobbing—of the night-mare --- how expiration is performed---the abdominal muſcles contribute to it---how laughter is performed ---of the uſe of reſpiration---inſpired air does not contribute to cool the heart---nor is its only uſe to afford the blood a paſſage through the lungs---nor to break down the blood—the vital particles of the air ſeem to be of a nitrous nature---of the uſe of the inſpired breath---proof that there is no ferment in the heart---in what life conſiſts---inſpired air contributes to animal motion---in what way---why the ſtopping of the breath occaſions death---why animals in violent motion breath more deeply---inſects cannot do without a ſupply of air.

TREATISE III.

Of the reſpiration of the fœtus in the uterus and the Egg.

How the fœtus lives in the uterus without air ---how crying and ſucking are performed in the uterus---the nutritive liquor of the ute-rus ſupplies the place of reſpiration---the umbilical arteries are formed for the purpoſe of reſpiration---they are not ſolely deſigned for the nouriſhment of the ſecun-dines, nor for the digeſtion of the food--- nor for carrying back the crude parts of the nutritive liquor from the embryo---nor are they formed for carrying on the circula-tion of the blood---the author's opinion concerning their uſe---the nutritious liquor of the uterus is full of nitro-atmoſpherical particles, as alſo the ſeminal liquors of the egg---how the blood of the embryo is im-pregnated with aerial ſpirit in the umbilical veſſels---the neceſſity of the umbilical arte-ries is proved---of the reſpiration of the chicken in the egg---it is performed by the help of the umbilical veſſels---in what man-ner the heat excited in the egg ſeems to con-tribute to perform the office of reſpiration--- why a new-born foetus, while yet involved in in the membranes, can live without refpiration---of the cavity fituated at the end of all eggs---the air contained in it is not breathed by the chicken---it is highly elaftic---by incubation the liquors of the egg are reduced into a lefs fpace---in how many ways the condenfation of things can be brought about ---how the liquors of the egg are condenfed ---of the ufe of the air contained in the egg.

ANALYSIS

OF

MAYOW'S

CHEMICAL OPINIONS.

ANALYSIS

OF

MAYOW'S

CHEMICAL OPINIONS.

AS I am about to draw the firſt line of this ſketch, my hand is ſuſpended by the intruſion of a wiſh, that has often before mixed with my thoughts. It may, perhaps, not be a very inviting taſk, but methinks, in the ſuperfluity of literary men, one at leaſt might be ſpared from other ſervices, to explore the dark volumes of antient Chemiſtry. Having made the experiment upon Glauber, over whoſe pages a ray is ſhed, from an intelligible relation of ſeveral facts, I was at firſt entertained by by the whimſical effect of his mixture of deep lamentations over the evil days on which he had fallen and of his oſtentatious piety with the myſteries of Alchemy. But abſurdity uttered in one unvaried tone, ſoon loſes its power of amuſement, and the great ſecret, half-promiſed in every page, but ſtill expected in vain, at laſt wearies all patience of the reader. Should any one, however, be able, ſo far to ſubdue his diſguſt, as to define, by careful enquiry, what degree of knowledge had been acquired from ſyntheſis and analyſis concerning the conſtitution of bodies, before the middle of laſt century, I am ready to believe, that he would find more credit due to Mayow than I dare venture to aſcribe to him. He has frequently occaſion to ſpeak of the more common chemical operations; and I cannot help fancying, that an eye like his would diſcern appearances that had hitherto eſcaped obſervation. Had any one, for inſtance, ſo clearly ſtated the following facts, as they relate to the compoſition and deflagration of nitre? It conſiſts of a very fiery acid, combined either with an alkali or a volatile ſalt, — the fixed and volatile alkalis were not yet placed together in a diſtinct claſs.

In

In the diſtillation of nitre, the acid paſſes over into the receiver, and a ſubſtance, exceedingly like alkali, is left behind : again, if ſulphur (inflammable matter) be repeatedly thrown upon nitre in fuſion, the reſiduum is improperly called *fixed nitre*, becauſe it really is only one of the conſtituent parts of nitre ; ſo in deflagrating equal quantities of nitre and tartar, we have an alkaline reſiduum equal in weight to the whole of the tartar, which is improperly called *ſalt of tartar*, becauſe part is ſupplied by the nitre ; and tartar, beſides a fixed alkaline ſalt, contains a ſpirit* and a fœtid oil, which are diſſipated during the deflagration.—

By adding nitrous acid to any alkali, or to a volatile ſalt (which will ſerve inſtead of alkali) nitre will be formed —

Nitre may be found in almoſt any ſoil, impregnated by the atmoſphere, but moſt abundantly in ſuch as are full of ſulphur (inflammable matter) and fixed or volatile alkali. Such ſoils are generally ſuppoſed to

* By this term, I think, he means an acid, for in ch. v. he ſays, all ſpirits belong either to the inflammable or ſaline claſs, and in this to the order of *acids*.

attract nitre from the air; nor can it be doubted, that the air contributes greatly to its production, ſince earth lixiviated and then expoſed to the atmoſphere, will be found, after ſome time, to be again impregnated with nitre.

But it is only the more volatile and finer part of nitre that is furniſhed by the atmoſphere: nor does the nitre contained, as is commonly believed, in the air, differ only in volatility from the common kind; for the alkaline ingredient muſt be derived from the earth; ſince, on account of its fixedneſs, we cannot ſuppoſe it to reſide in ſo thin a a medium as the air. *Earth* indeed ſeems to be ſulphur and alkali in cloſe combination, for if theſe two bodies be fuſed together in a gentle heat, a dark-coloured maſs very like earth will be formed; the only difference, perhaps, is, that in earth, theſe principles are not yet mature and more firmly united.

A looſe analogy! I confeſs, and a lame concluſion after ſo promiſing a beginning! but in their wider views of aeriform ſubſtances, modern chemiſts have yet only conjecturally decompounded the fixed alkalis.---

Vegetable

Vegetable alkali at leaſt appears from the Natural Hiſtory of nitre, to be formed between organic bodies and the atmoſphere. Is the alkali, as the acid, of this ſalt, formed though by the other ingredient of the air, uniting with ſome naſcent elaſtic fluid, diſcharged by decaying animals and vegetables? Yet phlogiſticated air is ſupplied, according to Mr. Berthollet, by the former of theſe. It is not eaſy, at preſent, to imagine what can be the uſe of ſo large an ocean of this gas: ſome, though a ſmall portion, may poſſibly be converted into nitrous acid by combining with the contiguous particles of vital air; for, to Mr. Cavendiſh's experiment, I can add, that nitrous acid has been procured by making vital air from manganeſe paſs along with atmoſpheric through an heated tube, though the experiment has not yet been ſufficiently varied, or repeated, to be publiſhed at large and with names.

But let us now ſee what ſatisfaction the next chapter, which treats of the nitrous acid, will afford.

Mayow was once in doubt whether this acid might not exiſt, full-formed, in the atmotſphere, for ſome ſalts, he obſerves, as the vitriols, calcined till their acid be quite expelled, will again contract acidity on expoſure to the air, and become in ſome meaſure nitrous; and iron filings are corroded by moiſt air as by an acid. Hence, if we conclude, that an acid of the nitrous kind reſides in the atmoſphere, we may ſuppoſe, that it forms nitre by combining with the alkaline ſalts, with which it may happen to come in contact.

But upon reflection, he thought the acid too denſe to float in ſo rare a medium; and moreover, the nitro-atmoſpherical ſalt, of whatever nature it really may be, is certainly the pabulum of fire, and alſo, in reſpiration, is received into the blood of animals; now it can ſcarce be nitrous acid in ſubſtance, becauſe this acid deſtroys, inſtead of ſupporting, both fire and life.

But although nitrous acid do not exiſt in ſubſtance in the air, yet, as nitre is, in part, derived from thence, and as its alkaline part is entirely ſupplied by the earth, its acid muſt be drawn in part, at leaſt, from the air; and to perceive more clearly, what ingre-

ingredient of nitrous acid really comes from this ſource, attention muſt be paid to the following circumſtances.

Mr. Boyle's experiments ſhew, beyond a doubt, that the air contributes to the ſupport of flame; but this effect is not produced by the whole, but only by the more active part of this fluid, ſince after the extinction of a candle in a cloſe veſſel, there always remains a large quantity of air; and no inflammable ſubſtance can be ſet on fire in an exhauſted receiver.

Nitre mixed with ſulphur will burn rapidly in vacuo, and under water, which proves that the fire-particles of air, or thoſe neceſſary to ſupport flame, are contained in nitre, and conſtitute its active and igneous part. Take gunpowder, and reduce it to a maſs of firm conſiſtence, by means of a little water; with this maſs fill a tube, cloſed at one end, and ram it tight; next ſet it on fire at the open end; then invert the tube, and plunge it into water; and the gunpowder will totally burn away: when mixed up in the ſame manner, it will alſo burn in vacuo, where all other fire goes out for want of its aerial pabulum—a deciſive proof that particles of fire-air, ſuch as are neceſſary to ſupport flame, are contained in nitre.

I ſhould be glad to know whether this experiment and this concluſion are ranked by the tranſlator of Scheele, among the obſcure hints: but in place of a commentary or a panegyric, I ſhall only tranſcribe an elegant experiment lately contrived by Mr Lavoiſier.

" I took a braſs tube, about eight lines in diameter, and long enough to contain about two ounces of the following mixture.

" I uſed very pure nitre and charcoal well-calcined: both were weighed, when very dry. I pounded them in a glaſs mortar, adding a little water, leſt the motion of the peſtle ſhould throw out the charcoal. I then loaded the tube with this mixture, preſſing it gently down with a wooden cylinder which juſt fitted the tube. I next ſet fire to it, and immediately plunged it under water or mercury. The detonation continues very well under water, provided care can be taken to hold the tube upright, to prevent the water from getting in, and

coming

coming in contact with the nitre and charcoal *."

Mr. Lavoisier's purpose was, to catch the air in inverted vessels as it was extricated.

Nitre, therefore, infers the author, does not, as Willis supposes, burn by virtue of any sulphur it may contain, but of its fire-air: and to produce combustion or flame, the concurrence both of particles of fire and sulphur (infl. matter) is necessary; hence no inflammable substance will burn in vacuo, unless nitre be mixed with it; neither will nitre deflagrate without inflammable matter, which shews, that it contains no such matter. The reason why the deflagration of nitre is so different from the burning of any inflammable substance is, because in nitre, the particles of fire-air are very closely condensed, and break forth in great numbers at once; hence, the impetuosity of the flame in this case, the form of which depends chiefly on the particles of fire-air.

Boyle's experiments on the burning of gunpowder in vacuo, he observes, do not

* Mem. de Mathematique, &c. XI. 686. 1786. 4to.

prove

prove, that the accefs of air is neceffary to this effect. He thinks the gunpowder goes out before it is all burnt away, for want of an uninterrupted chain of particles of fire-air, which the atmofphere, when preffing upon the grains of gunpowder, fupplies.

Since then, nitre is in part derived from the atmofphere, and fince it is found to contain the igneous particles of that body, thefe very particles are the principle which it derives from thence; and fince the atmofpherical ingredient of nitre exifts in the acid, the igneo-aerial particles of nitre, contained in the acid, are the ingredient fupplied by the atmofphere.

What reader will not regret, that he who could reafon thus fagacioufly, did not give roundnefs and perfection to his inveftigation, by procuring his nitro-atmofpherical fpirit in a feparate ftate from nitre?

He goes on to impute the extreme activity and caufticity of fpirit of nitre to thefe particles; and obferves, that nothing approaches nearer to the nature of fire than the orange-coloured acid; the red appearance obferved in the veffels in which it is diftilled, feems to be owing to theſe particles thrown into motion, to which fire or heat is always owing.

But why, it may be aſked, is not nitrous acid combuſtible? he anſwers, becauſe it contains moiſture.

Here, by abandoning his principles, he falls into error: otherwiſe he might have been led to the ſtriking experiment of mixing nitrous acid with oil.

The next chapter is employed on a ſubject of reſearch, ſtill more profound: it undertakes to aſcertain more preciſely how the atmoſpherical ingredient of nitre produces inflammation.

He rejects the hypotheſis of his time, and which has maintained its ground in this country ever ſince, that heat is produced by the ſmall particles of *any* body thrown into motion; and embraces a middle opinion; that heat indeed conſiſts in the motion of minute particles, but they muſt be of a particular ſubſtance, of fire-air. To ſhew that heat is not produced by ſulphureous matter in motion, he obſerves, that nitre kept in

fuſion

fufion in a crucible over the fire, through the bottom and fides of which heat muft be continually paffing, does not deflagrate, yet this effect will immediately be produced by the addition of any combuftible matter: therefore heat cannot confift of any fuch matter, fince it has not the fame qualities: again, if a piece of polifhed metal be held in the flame of a candle, it will be penetrated by heat, but the fulphureous particles will be depofited on the furface in the form of foot, which fhews, that they are too grofs to enter into the fubftance of the metal.

Mayow imputes heat to the motion of nitro-atmofpherical particles; the moderns, as it is produced in common combuftion, to their condenfation; he had no fufpicion that the aeriform ftate might depend on the interpofition of this fubtile matter, between groffer particles, and therefore could not fuppofe, that it was precipitated or thrown out, as they approached each other and returned to a liquid or a folid ftate.

Which train of reafoning upon this difficult fubject is the more logical? There undoubtedly are many cafes in which the temperature is lowered as a body dilates, and where an an apparent influx of heat produces no effect but expansion; and again, where an increase of temperature accompanies contraction of bulk. Then what can be more plausible than the idea conveyed by capacity for heat, and the analogy of water alternately imbibed by a spunge, and squeezed out of it, by which we gain at least some conception of a mechanism placed so far beyond the sphere of the senses? It has always been a great recommendation of false opinions when they were accompanied by some analogy easily grasped by the imagination.

I am very doubtful how far the specious theory of our philosophers will, this time, prove to be the system of Nature: does not the very familiar experiment of the deflagration of gunpowder present appearances directly contrary to its principles?—not to enumerate the other well-known instances of mixture, where much heat is generated, and at the same time, an abundant extrication of aeriform matter is observed—here we have great expansion and violent heat. Will it be said, that both the heat, that goes to constitute elastic fluids, and that becomes sensible, flows out from the mixed materials in consequence of a diminution of their capacity; and

and was already contained in them ? Can this be proved in the cafe of nitrous acid and oil, black wadd and oil, lamp-black and oil, iron filings and fulphur ? And when we confider the violent concuffion which attends the electric fhock, does not its power to produce elaftic fluids feem more favourable to the hypothefis of motion?

Mayow's inference was drawn from a very different combination of facts. He faw heat produced by the action of vital air indifferently both in an elaftic and in a folid ftate, fuch as it exifts in nitre: and fince there was nothing which could direct his attention towards expanfion and contraction, he naturally fell upon the quality of motion, as the only circumftance common to his fire-air particles in either cafe.

As far as our ordinary experience extends, dephlogifticated air has fo much to do with heat and inflammation, that I do not wonder that he fhould fuppofe heat to be produced by its motion, or that Scheele fhould imagine this fluid to be one of its conftituent parts, or that both fhould affign it the fame name, fire-air.

In many inſtances, however, we know, that the temperature is raiſed, when vital air, either in that or a ſolid form, is not concerned; and ignition or luminous heat, even ſo ſtrong as to fuſe earthy and metallic ſubſtances, is, in all probability, often excited in nature, without the concurrence of fire-air. Does not the heat of ſubterraneous fires depend on the violent efforts made by ſteam and elaſtic fluids, to expand on the one hand, and the ſtrong compreſſion under which they labour, on the other?— If thoſe fires were ſupported by a ſupply of vital air, would not the iron of lava, and eſpecially baſaltes (unerupted lava*) be more calcined? And is there any real inflammation, any conſumption of vital air, till the ignited matter comes in contact with the atmoſphere?

In the concluſion of this chapter, the author applies his doctrine to a fact of great importance in chemiſtry, and till lately of great obſcurity. He imagines that the celeſtial fires are kept up without any ſupply of ſulphureous matter, ſimply by the motion

* See Dr. Hutton's theory of the earth. Veltheim's Etwas uber die Baſalt. Hamilton's letters on the coaſt of Antrim.

of

of particles of fire-air, and that the rays of the Sun in paſſing through the atmoſphere impreſs motion upon theſe particles: Hence he thinks antimony, calcined in the focus of a burning glaſs, is fixed juſt in the ſame manner as when nitrous acid is frequently poured upon it and abſtracted: for it is by the nitro-atmoſpherical particles that it is fixed and rendered diaphoretic; in the flame of nitre alſo, in which the nitro-atmoſpherical particles are ſo cloſely crouded together, antimony equally acquires a diaphoretic quality; nor is it to be omitted, that antimony, calcined in the rays of the Sun, increaſes conſiderably in weight; and he thinks it can ſcarce be conceived whence it ſhould acquire this augmentation, unleſs from nitro-atmoſpherical particles, which it fixes during calcination.

The next chapter holds out another branch of the pneumatic theory, which many a modern chemiſt will be juſtly ſurpriſed to find in ſo old a writer. Before he fixes upon the ingredient, which ſpirit of nitre derives from the earth, he finds it neceſſary to lay down a general theory of acids. For this purpoſe he combats a received opinion, and denies that vitriolic ſpirit is contained in ſul-

phur

phur before deflagration. This combuftible fubftance confifts of two ingredients, a faline or metallic, and a purely fulphureous or phlogiftic part. According to the theory, which I am not ready to adopt, lately built by an ingenious and well-informed author*, upon Dr. Prieftley's new experiments on inflammable and vital air, fulphur, and other acidifiable bafes will confift of phlogifton and fomething elfe. This founds fomething like Mayow's hypothefis.

The faline part, which he imagines, from the attraction of fulphur for alkalis, rather to be of an alkaline nature, is that which is changed into an acid, by the converfion of its particles into fharp fword-like atoms: and the flame of fulphur is fo flow and faint, becaufe the igneous particles are checked in their motion by the interpofition of a third kind of matter between them and the phlogiftic particles. In the diftillation of vitriol he thinks the fire fupplies nitro-atmofpherical particles, which combine and effervefce with the metallic fulphur of the colcothar, and, by attrition, change its faline particles into vitriolic acid: moreover, the acids obtained from honey, fugar, and woods, have the fame origin; their acidifying principle is derived

* Dict. of Chemiftry, 1789. p. 122.

derived from nitro-atmoſpherical particles, in the motion of which fire conſiſts. That the acid does not pre-exiſt in theſe bodies appears probable, becauſe powder or decoction of Guaiacum, for inſtance, will efferveſce with ſpirit of vitriol, but not with alkali. And if vitriol, calcined till the acid is totally expelled, be expoſed to a moiſt air, it will be anew impregnated with acid, by the ſlow combination of the nitro-atmoſpherical ſpirit with the metallic ſulphur of the colcothar; and it is impoſſible to conceive in what other way the ſpirit of vitriol is regenerated in the colcothar, for it certainly does not exiſt in it immediately after diſtillation, nor can it be ſuppoſed to be derived totally from the air.

The ſame reaſoning is applicable to the production of vitriol from pyrites or marcaſites, expoſed to air and moiſture, for the nitro-atmoſpherical ſpirit, efferveſcing with the metallic ſulphur of the pyrites, converts the more fixed part into an acid liquor, which as ſoon as it is generated attacks the metallic particles and forms vitriol with them.

The *mechanical* part of this theory is only an awkward dreſs which Truth borrowed from the faſhion of the times.

The

The ruſt of iron which is an imperfect kind of vitriol (has he not here antici-pated the French nomenclators in their idea of an *oxide*) is produced by the nitro-atmoſ-pherical particles attacking the metallic ſul-phur of the iron: and the effect is the ſame as if the iron were moiſtened with ſome acid.

Nor is it in ſolids only that the power of this agent to generate acidity is manifeſted. Wine, ale, &c. are turned ſour in the ſame manner; and he imagines that all fermenta-tion conſiſts in the effervefcence of nitro-atmoſpherical ſpirit, either already contained in the liquor or coming from without, with ſalino-ſulphureous particles. He adds an obſervation well calculated to confirm him in this opinion of the converſion of alka-line baſes into acids, by the action of vital air, although we now know, that it depends on a different principle. If, ſays he, brim-ſtone be diſſolved in lime water or alkali, the ſolution, though at firſt alkaline, in time will acquire ſo much acidity as not to be able to hold the ſulphur in ſolution. I need not add, that the precipitation here depends on the acceſſion of an acid, fixed air.

Theſe principles, combined with his hypotheſis of the conſtitution of mould, afford a ready explanation of the origin of nitre, for the nitro-atmoſpherical ſpirit has only to act upon the ſulphur or phlogiſton of mould, and it will ſharpen the atoms of the other ingredient (the immature alkali) into an acid, as in the other caſes already recited. Nitre then conſiſts of three principles; the atmoſpherical ſpirit, "the ſaline vehicle (or baſe) which this ſpirit carves for itſelf out of the earthy materials, and in which it reſides, as in a proper ſubject—this ſaline vehicle, and the igneous ſpirit incorporated with it, conſtitute the acid ſpirit, which ſoon after its formation combines with the fixed ſalts of *earth* when brought to maturity, and forms common nitre."

The difference of acids he imagines to depend upon the difference of the ſalts, upon which the nitro-atmoſpherical ſpirit acts (or the baſis), and the degree of force with which this ſpirit has acted in ſharpening and attenuating their particles; yet there is ſtill a great reſemblance between all acids, for, in all, particles of fire-air reſide, as in a proper ſubject.

In

In the chapter on fermentation, he lays down his ideas on the elementary principles of things. He profeſſes himſelf unable to comprehend what the Chemiſts mean by their term, *ſpirit* : if ſpirit, diſtilled from fermented liquors, it ought to be referred to the head of ſulphur or phlogiſton; and corroſive and ſaline ſpirit belongs to the claſs of ſalts. Under this appellation, as well as that of *Mercury*, if to be uſed at all, nitro-atmoſpherical particles ought to be underſtood. Next to this ſulphur (phlogiſton) is the moſt fermentative principle, and indeed, there ſeem to exiſt in nature but theſe two active ſubſtances. Sulphur is found in a great variety of ſtates.

He thinks theſe two principles are very hoſtile to each other, and that almoſt all fermentation, under which term he comprehends nearly all chemical action, depends upon their colliſion. This idea pervades the whole chapter; but my own feelings upon reading it induce me to refer the reader to the table of contents. A minute analyſis would afford neither pleaſure nor information. It ought, however, not to be paſſed over, that he diſtinctly mentions the acidity of the fumes of burning charcoal, but ſuppoſes them to conſiſt of the nitrous acid.

In treating of the cauſe of rigidity, he imagines it to be owing to nitro-atmoſpherical particles fixed among the ſmall parts of bodies like ſo many wedges or ſpiculæ. He explains the ſparks produced from the colliſion of flint and ſteel, by ſuppoſing fire-air particles, contained in the metal, to be thrown by the ſhock into a violent agitation: and affirms, that theſe bodies will give ſparks in vacuo: his receiver muſt have been very imperfectly exhauſted. The long digreſſion concerning the cauſe of elaſticity ſavours ſo ſtrongly of the corpuſcular philoſophy, that I was glad to recollect, that it belonged not to my purpoſe.

The next chapter treats of the elaſticity of the air: and here the reader will be pleaſed to ſee this excurſive mind returning to experiment and induction, from his too bold attempts to reduce all nature under ſubjection to his principles.

The riſing of the ſkin into the cupping-glaſs is aſcribed to the formation of a partial vacuum, from the conſumption of the nitro-atmoſpherical

atmoſpherical particles by the flame: ſo that the air becomes not only unfit for maintaining combuſtion, but is alſo in part deprived of its elaſticity, as appears further from the following experiments.

Let a lighted candle be ſo placed in water that the wick ſhall riſe about ſix inches above the ſurface: then let a tall glaſs veſſel be inverted over it, as in *fig.* 1. That the water may ſtand at the ſame level within and without the inverted veſſel, one leg of a ſyphon ſhould be introduced into it, before it is plunged in the water: the other leg remaining on the outſide, and both riſing above the ſurface of the water: the uſe of the ſyphon is to let out the air as it is compreſſed by the immerſion of the glaſs veſſel; otherwiſe the water would ſtand lower in the inſide: the ſyphon is to be taken out as ſoon as the air ceaſes to iſſue out through it (which will almoſt inſtantly be the caſe) leſt the air ſhould flow in again. Having made theſe preparations, you will ſoon ſee the water gradually aſcend into the cavity of the inverted veſſel, while the candle is ſtill burning.

He will not ſay that the riſing of the water is not in part owing to the included air being leſs agitated and rarefied at the inſtant the candle is going out, but it is in part alſo owing to the flame having conſumed the nitro-atmoſpherical particles, ſo that the air is now leſs capable of reſiſting the preſſure of the atmoſphere, as appears moreover from the following experiment.

Let any ſubſtance, that takes fire readily, be ſuſpended, as in *fig.* 1. in a capacious glaſs veſſel inverted. He uſes a piece of camphor, under which a little linen, calcined to blackneſs and dipped in ſulphur, is laid. The mouth of the veſſel is to be immerſed about ten fingers breadth in water: the water within and without is to be brought to the ſame level as before; then, for the ſake of a more diſtinct view, let ſome water be taken out of the external veſſel, or rather let the inverted glaſs cucurbit be removed, by introducing under it a ſmall veſſel, capable of containing its mouth, and then lifting it into a ſhallower veſſel than the firſt, containing ſome water: and then let the whole ſtand till the air, expanded by the heat of the hand, ſhall have returned to its former dimenſions; now let the height of the water within be marked by ſticking bits of paper with paſte, made of barley flour boiled in

water,

water, to the ſides of the cucurbit; which is next to be brought into the light of the ſun, ſo that the combuſtible matter may be fired by a burning glaſs: when this is done, you will perceive the water within to deſcend, on account of the motion of the igneous particles, and the rarefaction of the air. On the extinction of the flame, the veſſels are to be removed out of the light of the ſun, that the rarefied air may cool and return to its former ſtate, when you will find the water within above its former level; and upon calculation, he finds the air to be reduced 1-30th in bulk.

After the fumes had diſappeared, and the glaſs had become as clear as at firſt, he tried to inflame another piece of camphor in the ſame air, but in vain; " a clear proof that the combuſtion had deprived the air of its fire-air particles, ſo as to have rendered it altogether unfit to ſupport flame."

Leſt any one ſhould attribute the failure of this ſecond attempt to an opacity of the glaſs produced from the fumes, he fixes a piece of paper by paſting its edges to the inſide of the bell, and withdraws it after the firſt experiment, and then tranſmits the rays through the clear ſpot.

In further confirmation of his hypothefis he adduces the following experiment. He ties a moift piece of bladder tight over the mouth of a veffel; and then applies an inverted glafs cucurbit, including a moufe, clofe to the bladder, as is fhewn in *fig*. 2. This difpofition being made, the edge of the cucurbit will be feen after fome time to adhere very firmly to the bladder, and the bladder itfelf be drawn up into the body of the cucurbit, juft as in cupping; and this will take place while the animal is yet breathing: and by laying hold of the cucurbit, the veffel below, unlefs it be very heavy, may be lifted up; fo that in the operation of cupping, a fmall animal might ferve inftead of fire.

The next experiment is added for the fake of afcertaining how much the air is diminifhed when deprived of its "vital particles," by the refpiration of animals. Let a fmall animal be confined within an inverted glafs veffel, or rather let it be fufpended under proper confinement, in a glafs cucurbit, as the jar is in *fig*. 4. The water within the inverted cucurbit is to be brought as before to the level of that in the veffel which fupports it; then, for the fake of feeing diftinctly the height of the water within, let ſome water be taken out of the outer veſſel; and let marks be made by paſting bits of paper on the ſides of the cucurbit: In a ſhort time you will ſee the water gradually ariſing into its cavity; notwithſtanding the heat and exhalations from the animal would ſeem likely to produce a contrary effect.

The diminution of bulk in the air he aſcertains by meaſuring the ſpace it occupied before and after the ſuffocation of the animal, which may be done by filling the veſſel up to the marks on the ſides. He finds, from the mean of many experiments, that the diminution is about 1-14th*: and adds, that the animal ought always to be placed near the ſurface of the water.

Hence he infers, that in reſpiration, animals derive from the air certain "vital and elaſtic particles." If the lungs were only to be inflated for the purpoſe of ſhaking and breaking down the blood, why ſhould an animal ſo ſoon expire, while ſo much air remains, and while it is forced by very nearly the whole preſſure of the atmoſphere into the lungs?

* He knew not that fixed air is exhaled, and that lime-water will abſorb it.

There is no reafon to deny that the air enters into the blood, becaufe we cannot perceive the ducts. Yet in lungs boiled and diffected, the microfcope difcovers innumerable pores, but he will not affirm, that thefe are the ducts in queftion.

But he thinks it clear, that the air is exhaufted of the fame particles by refpiration and combuftion: for if a burning candle and an animal be inclofed together in a veffel, as in the former experiments, the candle will be foon extinguifhed, and the animal will not long furvive it. He finds that an animal will not live much above half the time that it would have lived without the candle. Neither can it be fuppofed, that the animal is ftifled with fumes, becaufe if fpirit of wine be ufed, the animal will ftill die fooner: it is becaufe the air is in part deprived of nitro-atmofpherical particles by the combuftion; fo that both the light is extinguifhed, and the animal expires, for want of thofe particles.

He fuppofes that flame can be fupported only by a large and continual fupply of this matter, whereas a fmaller quantity inhaled for at intervals is ſufficient for animals, and that for this reaſon they ſurvive the flame. He made an unſucceſsful attempt to inflame combuſtible matter in air, in which an animal had expired.

What obſcurity there may be in the manner in which this author has expreſſed his opinions, or deficiency of proof in his experiments, my repreſentation is, I hope, faithful enough to enable the reader to determine. The following paſſage of a writer, hardly leſs diſtinguiſhed by the attention he has paid to the progreſs of Natural Philoſophy, than by the additions he has made to this department of it, will ſhew how little Mayow's *theory* was noticed. " That Candles will only burn a certain time in a given quantity of air, is a fact, not better known than it is that animals can live only a certain time in it; but the *cauſe* of the death of the animal is not better known than that of the extinction of flame, in the ſame circumſtances *."

The pages that follow theſe *obſcure hints* betray the reign of the corpuſcular philoſo-

* Prieſtley's exp. on air. I. 70.

phy. Enquiring how the elaſticity of the air is impaired by theſe proceſſes, he ſtates, and alas! rejects alſo this ſimple explanation, that one of the ingredients having paſſed into the blood, there is a diminution of ſubſtance; had he well conceived the idea of tranſition from an aeriform to a ſolid or liquid ſtate, he would have ſaved himſelf much unneceſſary ſpeculation: but the ſulphureous particles impinge violently againſt the nitro-atmoſpherical, on which the tenſion of the air depends, and ſtrike them out of the air as ſparks from ſteel; thus the particles of air are changed from rigid to flexible, and ſo oppoſe leſs reſiſtance to the preſſure of the atmoſphere.

But thoſe who ſhall have courage to read on, will find a paſſage in which elegant experiments and nice obſervations are ſo cloſely crouded, that juſtice cannot be done to it otherwiſe than by tranſlation. Having obſerved, that there is a ſtream of air conſtantly ſetting towards flame, and that not only on account of the rarefaction of the contiguous air, but alſo becauſe that which has been deprived of the nitro-atmoſpherical, its moſt ſolid, part, has become lighter, and therefore is forced upwards; he proceeds, by ſaying,

"that

" that if an animal of ſmall ſize, as a mouſe or bird, be placed as before, in a cloſe veſſel, but near the top, it will die much ſooner, and the water will riſe much leſs than if it had been placed in a lower ſituation. This will appear ſtill more clearly, if of two birds, or two mice, one be ſet at the top and the other at the bottom of the veſſel; for the animal at the lower ſtation will live ſome time longer. We are not to ſuppoſe that any vapours exhaled from the latter fill the top of the veſſel and exclude the air; every thing of this kind is condenſed on the ſides; otherwiſe (if it were elaſtic) the water would be depreſſed: but the air being rendered lighter from the loſs of nitro-atmoſpherical particles, aſcends to the top, and there reſiſts the preſſure of the atmoſphere, though it is unfit to ſupport life; while the air at the bottom is yet ſo little tainted, that it can be breathed. It deſerves to be remarked, that when one of theſe animals begins to feel the want of air, it raiſes its mouth upwards in ſearch of ſomething to breathe: as its diſtreſs increaſes, it turns its head downwards, and finding a little refreſhment, protrudes its mouth as low down as poſſible, and retains it in that ſituation."

Here

Here the author expreſſes his admiration of that proviſion of nature, by which air rendered unfit for reſpiration is carried upwards out of the way of animals. To diſtinguiſh and propoſe fit objects of enquiry is no equivocal mark of a mind at once active and enlightened. The author, conſidering the great waſte of fire-air, which he here calls the Elixir of life, juſtly conceives that there muſt be ſome contrivance in the conſtitution of things to repleniſh the atmoſphere with it. Had he but ſtopped at the problem! but—Thou, mean while, whom neither wind nor water mills, neither lions nor flocks of ſheep, could appal, undaunted knight of La Mancha, look down well-pleaſed on his enterprize: it is the enterprize of a kindred ſpirit—he is determined to ſolve it. Therefore, the ſun muſt be an immenſe chaos of nitro-atmoſpherical particles agitated by a rapid and inceſſant motion: which becomes fainter as we recede further from that luminary, and by the time we have deſcended ſo far below the moon as the middle region of the air, is entirely loſt; and here theſe particles conſtitute the element of cold: Now for the application; the vitiated air aſcends to the region of the element of heat, or of the rapid movement of the nitro-atmoſphe-

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rical particles, with which it is there anew impregnated; and now being heavier deſcends; but is turned off towards the poles by the vapours continually aſcending from the hot regions about the equator. Hence the north wind, loaded as it is with theſe particles in a quieſcent ſtate, comes to us ſo cold. And in this manner is maintained a circulation of air, indiſpenſably neceſſary to life. The ſky appears blue, becauſe we behold the region where the motion is much ſlackened; thus, burning ſulphur, and all fainter flames, are of a blue colour, from the ſame ſlow motion of thoſe particles.

The eighth chapter is partly a ſupplement to the treatiſe on reſpiration, and partly a recapitulation. It ſeems, therefore, not improper to join them together in this abſtract.

In the mean time I paſs on to a part which, as well as the beginning of the ſeventh chapter, diſplays the happieſt conjunction and co-operation of the dextrous hand, the obſervant eye, and ſagacious mind. To be ſenſible of the merit of the following contrivances, we have only to recollect how difficult it muſt have appeared, before a liv-

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ing 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, whether air can be generated anew, as appeared probable when he conſidered the various ways in which it is deſtroyed. On this ſubject he relates an experiment reſembling, as he takes care to mention, one of Mr. Boyle's.

Put a mixture of equal quantities of ſpirit of nitre and water into a large glaſs veſſel; then plunge a phial into the mixture, and fill it compleatly; afterwards introduce two or three globules of iron into the mouth of this phial, and invert it on the bottom of the larger veſſel, as in *fig.* 3, taking care leſt the globules fall out of the phial, which may be managed by ſtopping the orifice with the finger or ſome other obſtacle, till it be brought to reſt upon the bottom of the veſſel; the acid will ſoon act upon the iron,

* Hon. H. Cavendiſh's exp. on factitious air. Ph. Tr. LVI. page 142 and the plate.

and

and raiſe a great effervefcence; and the exhalations *(halitus)* raiſed by the commotion will aſcend in the form of bubbles to the top of the phial, and there conſtitute a gas *(aura)*, which will gradually depreſs the water, as it is generated. As ſoon as the phial is full, raiſe it, ſo that the iron globules may fall out, and remove them from the liquor, taking care at the ſame time not to lift the mouth of the phial above its level. On ſtanding, this gas will, by degrees, be condenſed, and the liquor will riſe into its place; about 1-4th will retain the ſtate of gas, and no cold, however intenſe, will condenſe it: if the iron be introduced again, freſh gas will be produced, of which part will never be condenſed into a liquid.

Here the diminution of the nitrous air depends upon its abſorption by the nitrous acid, and in a ſmall degree on the diſperſion of the heat excited by the effervefcence, to which he imputes it: a little phlogiſticated air will alſo be generated at the ſame time, and this will not be abſorbed. He adds, that if the effervefcence be very ſlow, or if the extrication of air be continued a day or two (in which caſes the acid will be ſaturated with gas) or if vitriolic acid be uſed inſtead of nitrous (when inflammable air will be produced) the gas will ſuffer little or no condenſation. Whether this gas be common air or not, is not, he thinks, immediately evident, but the following experiment proves it to have equal elaſticity. Take a glaſs tube, hermetically ſealed at one end, of the bore of a quill and about four fingers breadth long: let fall a drop of water into it, and mark upon a ſtrip of paper fixed to the outſide, how high the drop ſtands, then add another drop, marking its height, and ſo on. Then put the open end of the tube into the neck of a bottle, and cloſe the juncture with a compact cement, as in *fig.* 5. Fill this bottle, tube and all, full of water, and then invert it in a veſſel of water. In order to transfer the gas, introduce a ſmall veſſel (as a ſaucer) below the mouth of the phial containing the gas, and remove it into the veſſel containing the bottle with the tube. Then bring the mouth of the phial full of gas under the mouth of the bottle full of water, as is repreſented in *fig.* 5, and incline the phial till the gas aſcends into the bottle, which may be quite filled with gas in this manner, though a very little will ſuffice for the experiment.

Now

Now the bottle is to be removed into a receiver, which is to be exhausted by Mr. Boyle's air-pump: for this purpose, let a small vessel, which however must be large enough to hold the mouth of the inverted bottle, be brought under it, and then let it be removed, full of water and with the bottle resting upon it, under the receiver. When part of the air is exhausted, the gas will expand and make its way out of the bottle: as soon as the air is extracted as much as possible, let it be again admitted, and the water in the small vessel will be forced up into the bottle, and fill it almost entirely, for the gas will occupy but a small part of the tube; and yet this small quantity was before sufficient to fill the bottle and resist the pressure of that portion of air which could not be extracted as well as of the water.

By measuring the capacity of the whole bottle by dropping water into it, and comparing it with the capacity of the portion of the tube, which the remaining gas filled, we may find how much it had dilated (provided, however, none escape out of the bottle) for as much as the former exceeds the latter, so great had been the expansion.

He found that the gas expanded to above two hundred times its original bulk, and it would have reached four hundred times, but for the preſſure of the ſurrounding water. Common air does not expand more: care, he adds, muſt be taken to make the preſſure of the ſurrounding water at all times equal, and to exhauſt the receiver at the ſame degree. The air alſo, in which a light had gone out, or an animal had died, he finds to be equally expanſible with any other kind, a fact hardly conſiſtent with his reaſonings upon the mechaniſm of the air's elaſticity in ch. 7. His contrivance for taking this air out of the veſſel in which it had been left after combuſtion or reſpiration had ceaſed, is ſufficiently curious: he fills a phial, or other glaſs veſſel, not too large, with water, and then brings it inverted under the mouth of the former veſſel; in order to lift it above the level of the water, he has a ſtick placed acroſs the inſide, as in *fig.* 4, and acroſs this ſtick a cord is thrown, in ſuch a manner, that both ends ſhall come out under the edge of the veſſel; one of theſe ends he ties round the bottom of the inverted phial, which he wants to raiſe, and pulls the other till it is brought above the level of the water, in conſequence of which, the water it contains

tains falls out, and it is filled with air; and now, by laying hold of the other end of the cord, he can pull it down, take it out, and transfer the air into his meaſuring apparatus; *fig.* 5.

But though the gas produced in the manner already mentioned, is as elaſtic as common air, it does not follow, that it is the ſame, that is, endowed with vital or igneous particles; ſince that, in which a light or life have been extinguiſhed, has equal elaſticity, though deſtitute of this principle.--- The queſtion then is, whether it is capable of ſupporting life. He firſt collects a quantity, by repeatedly filling a ſmall phial, as above, by introducing globules of iron into the neck, &c. and then transferring the gas into a larger veſſel. The gas thus collected he uſes in an experiment, in the relation of which, I find great obſcurity, unleſs he uſed, as he ſurely did, his inflammable air only, and not his nitrous: otherwiſe the reſult muſt have been very different: he brings a mouſe, confined in a trap placed on a ſupport, under an inverted glaſs veſſel, as in *fig.* 6, and ſets the veſſel in water, contriving, as above, by means of his ſyphon, that the water ſhall ſtand at the ſame level within and without; the time the animal lives muſt be carefully noted: then another mouſe is to be placed in exactly the ſame ſituation, with the ſame quantity of freſh air, and now about twice or thrice as much gas is to be transferred into this veſſel as it contains of air: and the veſſel is to be raiſed till the water within, now depreſſed by the gas, is of the ſame level with the ſupport, on which the animal is placed, care being taken not to lift it above the external water. In this caſe, the animal will live little longer than in the former, when no gas was introduced; whereas, were the gas really air, and fit to maintain life, it ought to have lived at leaſt twice as long. The reaſon why it lives at all longer is, he believes, becauſe the air, on account of the quantity of gas mixed with it, is breathed in ſmaller quantities at once. Notwithſtanding this and other differences, mentioned in the laſt chapter, he ſtill imagines there may be a conſiderable reſemblance between common air and gas produced from iron, of which the particles are rigid, and corroſive liquors abounding in nitro-atmoſpherical ſpirit.

The following chapters of this treatiſe either lie beyond the bounds I have ſet to myſelf,

myſelf, or are ſuch that few readers need deſire much more information concerning them than the table of contents will ſupply. It may excite ſome ſurprize to ſee the opinions of Deſcartes brought forward in the 12th chapter with the ornament of nitro-atmoſpherical particles; ſince Newton, whoſe purity from ſuch crude ſpeculations and groundleſs conjectures as ſtain the pages I paſs over, deſerves to be admired almoſt as much as the greatneſs of his diſcoveries, had now appeared; and had exhibited, not only in his lectures at Cambridge, but in a periodical publication, which it would be ſtrange if Mayow had not yet ſeen, the outlines of his matchleſs work on light and colours*.

Yet amid theſe adopted doctrines of an age, when the facts obſerved were not numerous nor well arranged, and when the art of reaſoning upon them was very imperfect, there appears now and then ſomething not unworthy of his other diſcoveries. He obtains

* No. 80. of the Phil. Tranſ. begins with *a letter of Mr. Iſaac Newton, containing his new theory of light and colours*; this letter is dated February 6th, and was publiſhed February 19th, 1671-2. Mayow might have written this treatiſe ſooner, but ſtill he might have ſtruck out his errors. But Newton's diſcoveries did not univerſally meet with a kind reception, as indeed ſuch diſcoveries never will at firſt.

air

air from water, by making it boil in ſuch an apparatus as is ſeen in *fig.* 3. He ſuppoſes that the heat of the Bath waters proceeds from the fermentation of pyrites moiſtened with ſubterraneous water: becauſe there are almoſt whole mountains conſiſting of this mineral or aluminous ore, and becauſe thoſe hot ſprings contain a matter that ſeems to be ſupplied by the pyrites, of which copperas is made: that moſt ſprings are fed by rain water: that fiſhes derive air from the water in which they live, and that their gills ſerve them for lungs; that in an exhauſted receiver the diſtention of the air bladder cauſes the belly of a fiſh to turn upwards; that theſe animals conſume but little air: that ſoap is not ſoluble in ſpring water, becauſe the acid it contains unites with the alkali and ſeparates the oil; that oil of vitriol poured into ſpring water produces a gentle efferveſcence.

The treatiſe on Reſpiration, as it was the firſt, ſo it may be juſtly conſidered as the moſt perfect of Mayow's works. In this treatiſe, as he ſays, he does not pay any regard to the authority of writers, but to truth itſelf. He reſts all his poſitions upon evidence, if not always peculiar to himſelf, yet ſuch

ſuch as he had cloſely examined. Hence his diſcoveries are here intermixed with no wild Carteſian hypotheſis, and with very little error of any kind. He delivers the ſubſtance of his chemical doctrines on the nature of air, and beſides, at the age of two or three and twenty, ſhews a wonderful proficiency in the nice and ſlow-paced labour of diſſection. The former, and indeed the larger part of this treatiſe explains the mechaniſm of reſpiration. In the exordium I read the dictates of a mind habitually ardent, and now unuſually animated, as it contemplates the importance of the ſubject, the magnitude of difficulties found inſuperable by others and the enlargement of human knowledge juſt accompliſhed by its exertions.

Having rejected the the fuga vacui, and the other hypotheſes enumerated in the table of contents, he attributes the inflation of the lungs to the preſſure of the atmoſphere. He ſuppoſes a bladder to be placed within a pair of bellows, with the neck quite cloſe to the pipe, ſo that all the air that enters at the nozzle ſhall paſs into the body of the bladder; and offers this as an elucidation of the manner in which the lungs are inflated.— They might alſo be inflated by the air's elaſticity,

ticity, as if an animal ſhould be incloſed in a phial cloſely ſtopped, and ſo this preſſure be intercepted, the tendency of the air to expand would fill its lungs: the elaſticity of the air, however, is regulated by the ſame preſſure: for the air, eſpecially that near the earth, makes a conſtant effort to expand, juſt as a fleece of wool will reſtore itſelf upon removing any weight that may preſs it: here he quotes the experiment of placing a bladder flaccid and almoſt empty of air, under the receiver of an air-pump, and extracting the air, in conſequence of which, the bladder ſwells out to its full dimenſions.

The lungs being without muſcles, cannot dilate themſelves; if, indeed, they had muſcles, their action would be to produce contraction.

As the inflation of the lungs depends upon the air entering the dilated thorax, he enquires how this dilation is effected: and he attributes it firſt to the elevation of the ribs. He aſſerts ſtrongly, and proves the conſpiring action of both ſets of intercoſtal muſcles, and rejects the ſuppoſition of the internal ſet ſerving to contract the thorax; a poſition generally ſuppoſed to have

been

been firſt eſtabliſhed by the celebrated controverſy between Hamberger and Haller*, in 1748, and the following years.

He delineates and deſcribes the double articulation (for he ſays it was generally ſuppoſed to be ſingle) of the ribs with the ſpine, and the obliquity of the cartilages that connect the ribs and the ſternum: and obſerves, that by theſe contrivances, the ribs cannot be elevated without at the ſame time being thrown outwards, and enlarging the capacity of the thorax: to which effect the depreſſion of the diaphragm contributes in the ſecond place.

After explaining ſome diſeaſes and ſome affections of reſpiration, he goes on to ſhew that the abdominal muſcles act in expiration together with the aſcent of the diaphragm.

The uſe † of reſpiration is neither to cool the heart, nor break down the blood. He

* l. c. p. 83.

† Boyle (Phyſico mech. exp. 1661) ſeems more than half willing to believe that the air, in reſpiration, ſerves only to carry off the excrementitious exhalations of the blood; but cautiouſly concludes, that there appears reaſon to ſuſpect, that there is ſome uſe of the air which we do not yet thoroughly underſtand, that makes it ſo neceſſary to the life of animals.

rejects

rejects alſo the opinion moſt received in his time, and perhaps alſo in ours, that this function ſerves to tranſmit the blood from the right to the left cavities of the heart.—This queſtion has but very lately been finally ſettled *, yet Mayow approached very near to the truth. He ſays, "it is certain that the blood may paſs through the lungs, tho' they do not move: for blood, or any liquor, thrown with a ſyringe into the pulmonary artery of a dead animal, will paſs readily into the left ventricle of the heart; and any one who ſtops his breath for a time, will feel a pulſation in the arteries of the wriſt, which could not happen if the blood did not in the mean time paſs to the left ventricle." Here, for farther information, he refers to a paſſage below, where he ſays, "I know not whether in ſuppreſſed reſpiration the blood, for want of nitro-atmoſpherical particles, becomes ſo thick as to be unfit for motion, and to ſtagnate in the left ventricle; for the blood, though not yet impregnated with air, is thrown with force enough out of the right ventricle, from which the left does not dif-

* Goodwyn's Connexion of life, &c. p. 37—47. Mayow expreſſes himſelf very ſtrongly and preciſely below, "Imo ſanguis etiam per ipſos pulmones ſine reſpirationis ope tranſire poteſt." p. 312.

fer,

fer, except in being ſtronger to propel the blood, though that be now ſtill of a thicker conſiſtence." Here he ſeems to have been ſtruck by the objection ſtated by Dr. Goodwyn, if the black blood be a ſufficient ſtimulus to the right cavities of the heart, why not to the left alſo? The fact however is manifeſt, whatever may be the cauſe, which it would certainly be deſirable to aſcertain.

He then lays down the opinion unavoidably anticipated in the account of the former treatiſe; that the office of the lungs is to ſeparate from the air and convey to the blood one of its conſtituent parts.

In the following treatiſe he mentions a moſt curious fact, to this purpoſe, and one which ſhews how exactly he made obſervations. In experiments of transfuſion, a dog, upon receiving the arterial blood, though he was before breathing deeply and panting, will appear ſcarce to fetch his breath at all; he adds, that on expiration ſomething noxious is thrown out.

At this early age he had formed the peculiar ſyſtem that pervades all his works: his mind indeed diſcovers perpetual reſtleſſneſs,

and

and an habitual tendency to advance; for having conveyed the vital particles into the blood, here was now a very inviting refting place; but he could not be content without proceeding to inveftigate what part they afterwards perform in the animal œconomy; a queftion which has never been refolved, and fcarce propofed by phyfiologifts, except in as far as the contraction of the left cavities of the heart is occafioned by them, to which office who will fuppofe that their operation is folely confined? he fuppofes that they are neceffary to all mufcular motion, and therefore to that of the heart: but he will explain—it is his ruling paffion—the mechanifm by which they accomplifh this end: it is by exciting an effervefcence with the falino-fulphureous particles, and fo caufing the mufcles to fwell: this is the fum of the doctrine of the fourth treatife.

An animal will live for fome time without breathing, becaufe the blood in the pulmonary veffels, being impregnated with air, is fufficient to keep up the motion of the heart; when that fails, life ceafes—life effentially confifts in the diftribution of the animal fpirits, which are only the vital air of the blood. In violent motion and hard exer-

cife,

cife, more frequent refpiration is neceffary to procure a fupply of vital air for the frequent effervefcences of mufcular contraction: in violent action the heart muft beat oftener, which it cannot do without an additional fupply of that fubftance: "So that the chief ufe of refpiration is to keep up the motion of the mufcles, and chiefly of the heart." A frog, which lives for a time without refpiration, will alfo live after the heart is cut out; but the animals that require a continual fupply of animal *fpirits*, and confequently, an inceffant action of the heart, require alfo to breathe.

He adds, that when the motion of the heart has ceafed from interrupted refpiration, if air be blown into the vena cava, it will be again reftored, "fo that air feems to be the principle, without which the heart cannot move, nor does it fignify whether it be introduced into the mafs of blood by the lungs, or in any other way."

He concludes with faying, that infects, though they move when cut in two, yet can neither move nor live in vacuo as they have neither blood, heart, nor lungs, (they are now, however, well known to have organs

of

of refpiration of a different form) and that the air is only neceffary to them for the purpofe of motion: and in beafts of burthen, that ufe their mufcles for a whole day together, we fee whence the great fupply of *explofive* matter is derived. Thus does a man of genius bring remote facts into the fervice of his hypothefis.

I return to the eighth chapter of the firft effay, which treats of the nitro-atmofpherical fpirit, as it is infpired by animals. He fufpected for a time, that the vital particles were fhaken out of the air by the peculiar ftructure of the lungs; but, upon more mature reflection, the aerial particles appeared to be introduced into the blood, and there to be at once deprived of nitro-atmofpherical particles and elafticity, which he thinks the following experiment renders probable. Let a ftick, equal in length to the diameter of a glafs cucurbit, where it is wideft, be placed crofswife within it, fo that both ends may reft on the fides, as in *fig.* 4. Over this ftick let a glazed earthen ware veffel, capable of containing about four ounces and a half, full of fpirit of nitre, be hung, by means of an iron hook; and let a bunch of pieces of iron, tied to a cord, be thrown

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across the ſtick, directly over the veſſel: this cord muſt be long enough for the end of it to come out under the edge of the cucurbit; now the mouth of the cucurbit is to be immerſed about five fingers breadth in a veſſel of water, and the water within and without brought to the ſame level by a ſyphon, then take a little water out of the containing veſſel, till that within ſtand about three fingers breadth higher; let every thing remain thus, till the air, rarefied by the heat of the hands, ſhall have returned to its former ſtate; then mark the height of the water with bits of paper.

Now let the pieces of iron be let down into the nitrous acid, by looſening the cord, when a violent efferveſcence, accompanied with heat, will ariſe, and the water within will be depreſſed by the generated gas.--- When the efferveſcence has continued about twenty minutes, or rather when the water has been depreſſed about three finger's breadth, the iron is to be raiſed out of the liquor: you will now ſoon ſee the water within aſcend gradually, and in an hour or two, it will ſtand far above the firſt mark; for having been at firſt depreſſed three fingers breadth below, it will be now as much above that mark ; ſo that about one-fourth of the ſpace occupied by the air will be filled with water : "*and indeed the water thus elevated will deſcend, in no long time, to its former level.*" I ſuſpect ſome miſtake here : the author ſays nothing more of this ſtrange new depreſſion of the water : how can any air be generated in theſe circumſtances ? Did one of the pieces of iron at any time come off the bunch, and remain behind in the acid ? I wiſh the paſſage were out of the book, or at leaſt, that ſome one would explain it to me.

By renewing the production of gas, after the air had loſt one-fourth of its elaſticity, he found that ſome, though but a ſmall diminution, would now take place, upon ſuffering the veſſels to ſtand for a time, This diminution muſt be owing partly to the contraction of the gas, when the heat occaſioned by the efferveſcence is over, and partly to its abſorption*. The auther diſtinctly imputes the diminution in the firſt caſe to the deſtruction of the air's elaſticity ; in the ſecond, to the condenſation of the gas, which the reader will recollect that he had ſeen in other experiments ‡, though in them it depended on

* See Prieſtley III. 103—110.

‡ p. 35. where the nitrous gas was abſorbed byt he acid, as here by water.

a cauſe a little different. He thinks the particles of air, being rigid and brittle, are broken by the fermenting particles, and that the nitro-atmoſpherical are ſtruck out, much as in combuſtion, which is only a very violent fermentation. Yet he does not infer, that the diminution determines the quantity of theſe particles.

This moſt extraordinary diſcovery paſſed, as far as I know, unnoticed, till at laſt the revolution of more than half a century * brought it under the cognizance of Dr. Hales, who only further obſerved, "beſides the reddiſh turbid fume," that freſh air admitted to nitrous gas will be diminiſhed, and that this gas will extinguiſh a candle †. It is odd, that he who quotes Mayow's experiments on combuſtion and reſpiration ſhould not have recollected this, which is ſo much more ſtriking, and more peculiar to him.

* The Vice-Chancellor's imprimatur for Mayow's Collection is dated 1673, Sir I. Newton's, as P.R.S. for Hales's 1ſt vol. in 1726-27.

† Stat. Eſſays II. 283—287. I add a minute obſervation. Dr. Prieſtley, by referring to this paſſage, will ſee that he has quoted Hales inaccurately. He ſays, that as Hales procured his nitrous air from the Walton pyrites, he never expected to ſee this remarkable appearance, "ſuppoſing it to be peculiar to that particular mineral" (I. 108). Yet H. ſays, he procured it alſo from ſteel filings and mercury with nitrous acid.

But a century paſſed away before Dr. Prieſt-ley paid full attention to it, and made the proper application of it.

Mayow found that the gas from the nitrous acid and alkali (fixed air) and from vitriolic acid and iron (inflammable air), would neither of them produce this diminution, when applied in the ſame way; whence he concludes, that no contraction can appear, unleſs the gas by itſelf (as he found nitrous gas, when confined by nitrous acid, would) will undergo condenſation; and he ſeems to ſuſpect, that theſe other elaſtic fluids deſtroy the air, though the effect be not viſible.

Now in the ſame manner the blood, being in a ſtate of fermentation, ſtrikes the nitro-atmoſpherical particles from the air: and the florid colour of that contained in the arteries is owing to the air, which being mixed with the blood, excites a fermentation in it.

That the blood in the arteries is in a ſtate of greater efferveſcence than in the veins, and contains more air, he offers this experiment to ſhew. If blood, kept ſome time, be put into an exhauſted receiver, it will give

give out a few bubbles, where it has acquired a florid colour; whereas blood just drawn from an artery will expand greatly and give innumerable bubbles: but is not this owing to the greater evaporation from the warmer liquid in vacuo?

If nitrous acid be mixed with spirit of hartshorn, loaded with its oil, it will produce a deep red colour*, which is attributed to the effervescence of the nitro-atmospherical particles of the acid with the salino-sulphureous particles of the alkaline liquor.

The heat of the blood† is owing to this effervescence, as that of pyrites exposed to air and water—In violent exertions we are hotter, because the respiration is increased, and not from attrition †, and if any one, sitting still, breathes more frequently and deeply, he will perceive a glow over his body. Some heat, however, is generated by the effervescence in muscular motion.

The blood becomes acid, by the attrition its saline particles undergo, while the nitro-

* Scheele observed the same blood red colour, in a strong solution of calculus in nitrous acid. ESSAYS, p. 200.

† How was this doctrine thrown away on Boerhaave and his disciples! even upon Haller, the best of them all.

atmofpherical effervefce with the fulphureous. From the blood the urine acquires acidity, for urine will act upon copper like fal ammoniac, and precipitate fulphur from a folution of liver of fulphur. It contains a falt like fal ammoniac.

I come with pleafure to the next fhort treatife: for it brings me within full view of the conclufion, perhaps too long delayed, and I may now borrow a little affiftance from that fullen reviewer in the Tranfactions, who could never once prevail on himfelf to fmile upon the fair features of this new-born offfpring of Science. "So much," fays he, "*if not too much*, for the firft treatife."— Our author, confidering with himfelf how a fœtus can live in the womb without the accefs of air, and finding the offices hitherto affigned to the umbilical arteries to be ill-grounded, fcruples not to affirm with the learned Everard *, that the faid arteries are formed chiefly, if not only, for the ufe of refpiration, declaring, that the blood of the Embryo, being conveyed through the umbilical arteries to the *placenta uteri*, carries to the

* He totally rejects Everard's premifes, though he acquiefces in the fame conclufion. Everard is not, I think, quoted by Haller, as he ought to be at p. 261—2, Vol. VIII. Elem. Phyf.

the fœtus not only the nutricious juice, but alſo with it a quantity of nitro-aerial particles, whereby the blood of the fœtus, by its circulation through the umbilical veſſels, is impregnated juſt as it is in the veſſels of the lungs; whence he would not have that placenta called any longer the liver, but the lungs of the womb. And this ſupplement for reſpiration he extends to the chicken in an egg, aſſerting, that the ſame doth, no otherwiſe than a child in the womb, breathe by the ſaid arteries, eſteeming, that the primogenial liquors of the egg, furniſhed with a pure aerial ſubſtance, being inceſſantly conveyed through the umbilical veſſels to the chick, perform to the ſame office not only of nutrition, but of reſpiration alſo.

To this he adds, that even that gentle warmth, excited in the egg by incubation, may alſo contribute ſomething there to ſupply the defect of reſpiration; foraſmuch as he ſuppoſes to have proved in his treatiſe of reſpiration in general, that the nitro-aerial particles, by the blood's fermentation ſtruck out of the parts of the air, ſerve animals for reſpiration." In anſwering a queſtion propoſed by Harvey, and at that time much agitated, why a fœtus will live in its membranes

branes for ſome hours; whereas, if it have breathed a few minutes, it will die immediately on the excluſion of the air, he obſerves, that in the action of the diaphragm and other muſcles of reſpiration, much more nitro-aerial ſpirit is ſpent than before, by theſe new exertions, and now this waſte will neither be ſupplied, as in the chicken in the egg, nor by reſpiration, and ſo the infant muſt die for the want of animal ſpirits.

In the fourth treatiſe of muſcular motion and the animal ſpirits, " our author thinks it probable, that the fibrillæ, tranſverſely inſerted into the greater fibres of the muſcles, perform the chief office in their contraction, by reaſon as well of their poſition as of their ſize and number. And as to the cauſe of this contraction in theſe fibrillæ, he thinks that beſides the animal ſpirits, there are alſo required to this motion, ſome of the ſalino-ſulphureous parts of the blood; and that thoſe animal ſpirits, that contribute to the animal motion, conſiſt of thoſe nitro-aerial parts, which he aſſerts to be tranſmitted into the blood by inſpiration. And both theſe parts he judges neceſſary to this muſcular motion, becauſe he underſtands not how that animal motion can be performed without different particles mixed together and brifkly moved. Having difpatched this hypothefis about the nature of animal fpirits, he endeavours to explain from thence the manner of all fermentations and concoctions performed in the vifcera of animals, and particularly in the Stomach, Pancreas, and Spleen, in the laft of which he takes occafion to fhew both how the fixed falts of animals are volatilized, and that from plants (which he faith are furnifhed with no volatile falt) if they be laid to putrify, a confiderable quantity of volatile falt may be extracted by diftillation *."

Thefe ideas concerning fecretion, and the efficient caufe of mufcular motion, however

* Here Oldenburg quotes Dr. D. Cox (certainly one of the beft chemifts of that time, though as certainly *impar congreffus Achilli*) and tacitly prefers him to Mayow, with what juftice a comparifon of the following paffage, with Mayow's account of the compofition of Cream of Tartar and Nitre, p. 2, 3. will enable the reader to determine : "Tartar, which is fenfibly acid, and from which a volatile falt cannot be feparated by any commonly known method, by bare calcination becomes a ftrong and perfect alkali.—Nitre, an undoubted acid, with a fmall proportion of mineral or vegetable fulphur, is *converted* into a genuine fiery alkali.—Nitre, which is made by the effufion of an acid fpirit upon an alkali, may be almoft totally diftilled into an acid fpirit, there appearing not the leaft footfteps of a volatile falt, *and fcarce any of the alkali*, out of which it was chiefly produced." Phil. Tranf. No. 107, page 152. October 26, 1674.

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abſurd they may at firſt appear, and however erroneous they may be, are, as I have already intimated, clear indications of a mind ſtill preſſing forward to pervade the receſſes of Nature. That the blood departs from the left or poſterior cavities of the heart, richly laden with nitro-atmoſpherical ſpirit and returns deſtitute to the right, ſeems ſufficiently eſtabliſhed: and that it is more impoveriſhed as it approaches nearer to the end of its circuit, is highly probable. From the frequency of reſpiration, it is evident, that much of this principle is received; and therefore much alſo is left behind for the conſumption and uſe of the animal œconomy: and ſurely it ſerves other purpoſes beſides keeping up the play of the left ventricle of the heart: If we conſider further, that the fœtus, which can live without reſpiration, muſt nevertheleſs borrow from the maternal blood, by means of the placenta, a ſupply of the ſame principle; that, as our author obſerves, the tranſmiſſion of the blood cannot be the ſole object of reſpiration, ſince this purpoſe might have been accompliſhed by continuing the organization of the fœtus*; and how much blood goes to the ſecretory organs and the brain (through which he ſuppoſes a current of his principle is conſtantly

flowing

lowing): from the whole we may ſafely infer, that theſe data will one day conſpire with future diſcoveries in chemiſtry, to unravel their myſterious operation; and the MAYOW of another age, by explaining, and perhaps alſo by teaching how to manage, their intricate and multifarious machinery, may ſee that art, which can reſt firmly upon no other foundation than a juſt theory of the functions of the body, riſing under his hands into a beautiful and ſolid ſtructure. Nor, however remote medicine may at preſent be from ſuch perfection, do I ſee any reaſon to doubt that by taking advantage of various and continual acceſſions as they accrue to ſcience, the ſame power will be acquired over living, as is at preſent exerciſed over ſome inanimate bodies, and that not only the cure and prevention of diſeaſes, but the art of protracting the faireſt ſeaſon of life, and rendering health more vigorous, will one day half realize half the dream of alchemy.

The *Rickets* are imputed to a want of *nervous influence*, which does not ariſe from a defect in the brain, but in the ſpinal marrow.

Such are the principal opinions of this author, a mingled maſs of brilliant diſco-

veries

veries and hypothefes now repulfive and dif-gufting. The former it is time to relinquifh to public admiration; nor will an intelligent reader, as he looks back and recollects the age of him who made them, omit to reflect what he might have done, if he had gone on thus for a fpace of time, equal to that by which Newton furvived him, with a maturer judgement and powers grown more ready from practice: neither fhould I envy that underftanding or temper which, after this exercife of the imagination, fhould refufe itfelf to regret, fuch as that of the poet:

> Heu, miferande — —, fiqua fata afpera
> rumpas,
> Tu *Marcellus* eris ———

From the wildeft of his errors I can draw no conclufion harfher or more inftructive, than that they difplay the rafh and adventurous fpirit of a young man: that by one illuftrious example, they fhew, what is indeed attefted by every other, that the moft perfect facility in contriving and performing delicate and convincing experiments, united with the utmoft velocity of thought, can never ftretch beyond certain bounds in tracing the order of Nature: that he who

ventures

entures further, muſt rely upon ot
nd or miſs his way, according as they
uide him ; and hence, that thoſe who p
eſs the happieſt talent of reaſoning, wi
dopt from their age ſome opinions, of
which many will afterwards wonder how
he ſlighteſt application of common ſenſe
ould fail to detect their falſhood and ab-
urdity.

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