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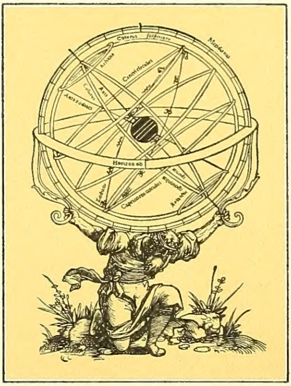

James Frobisher
November 30. 1826
London.

EXPERIMENTS

UPON

VEGETABLES,

DISCOVERING

Their great Power of purifying the Common Air in the Sun-ſhine,

AND OF

Injuring it in the Shade and at Night.

TO WHICH IS JOINED,

A new Method of examining the accurate Degree of Salubrity of the Atmoſphere.

By JOHN INGEN-HOUSZ,
Counſellor of the Court and Body Phyſician to their IMPERIAL and ROYAL MAJESTIES,
F. R. S. &c. &c.

LONDON:
Printed for P. ELMSLY, in the Strand; and H. PAYNE, in Pall Mall. 1779.

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TO

Sir JOHN PRINGLE, Bart.

Phyſician to his Britannic Majeſty, late Preſident of the Royal Society, Member of the Royal Academy of Paris, &c. &c.

SIR,

A GRATEFUL remembrance of paſt ſervices is as juſt a tribute due to thoſe from whom they are received as the acquitting of a debt contracted in any other manner. If it is not in the power of a man to make a return ſuitable to the benefits received, he is, however, in duty bound to ſhew, by the beſt method in his power, a thankful heart to his benefactor.

Ingratitude was by the ancient Greeks held as a crime of the blackeſt dye, as tending directly to deſtroy the motives of mutual benevolence, and to diſſolve the ties of friendſhip, that ſource of human happineſs, without which life itſelf is ſcarcely worth enjoying.

The ungrateful, conſcious of his miſbehaviour, and looking upon his benefactor as upon a judge who has pronounced a juſt and ſevere ſentence upon him, endeavours to find reaſon for breaking off with him; while his benefactor, looking upon the ungrateful as upon a monſter

unworthy

unworthy of his regard, is induced to ſhut, for the future, his heart againſt others.

No man upon earth can have ſtronger reaſons for a due ſenſe of gratitude than I acknowledge to you. You beſtowed many civilities upon me, who had never been in the way of doing you any ſervice whatever. You granted me your friendſhip almoſt as ſoon as I was acquainted with you. You encouraged my eagerneſs for improving myſelf in medical knowledge, by communicating to me what you had learned by a laborious life; by that experience which an aſſiduous and moſt attentive zeal, beſtowed in the care of the great military hoſpitals in the time of war, and a moſt ſucceſsful private practice, had afforded you, and of which your celebrated work upon *The Diſeaſes of the Army* will be an honourable and everlaſting teſtimony, as well as a real benefit, to the lateſt poſterity.

You always gave me, with the greateſt ſincerity, your advice in what manner, and in whoſe company, I could moſt improve myſelf in the various branches of medicine and natural knowledge which I took a delight to cultivate.

It was you, SIR, who, among thoſe many reſpectable and learned men, whoſe conſtant friendſhip towards me has made an indelible and grateful impreſſion on my mind,

con-

contributed principally to that particular happineſs I enjoyed during ſo many years in this iſland; that felicity which a free and independent man finds in the purſuit of knowledge and wiſdom in the ſociety and friendly intercourſe of thoſe who have diſtinguiſhed themſelves by their learning.

But, SIR, among the many obligations which I owe you, there is one of ſuch importance, that the very thought of it ſtrikes me with reverence and with the deepeſt ſenſe of gratitude for you. You did me a ſervice which I cannot forbear to mention; though I know that your modeſty would hardly permit me to expreſs the true ſituation of my mind in that reſpect.

Permit me, SIR, to leave behind me ſome public teſtimony of my reſpectful gratitude to you, as the only, though ſmall, return I can make you; the only way by which I can publicly ſhew, that the unſolicited favours ſo generouſly beſtowed upon a foreigner, who could not claim the leaſt merit with you, have made ſo ſtrong an impreſſion on my mind as no time is able to weaken. You have recommended me, SIR, without my ſoliciting any favour from you, to thoſe Auguſt Sovereigns who are ſtill the ſupport of the illuſtrious HOUSE OF AUSTRIA;

thoſe

thofe powerful Monarchs whofe graciousnefs, benevolence, and magnanimity, equal the fupreme grandeur of their ftation. Thefe Auguft Sovereigns, after having fuffered fo many repeated loffes by that dreadful difeafe the Small-pox, refolved at laft to check that terrible havock in their illuftrious Family, and ordered their Ambaffador to fend to their Court a phyfician from this ifland, capable of fulfilling the important truft of faving, by means of inoculation, the remainder of the Royal Offspring, which had as yet efcaped the infection. Being confulted on the choice of a proper perfon, you propofed me without hefitation, and thus opened

opened to me a wide door to emoluments and honours.

After having been ſo publicly and ſo honourably called from a diſtant country to the moſt generous and powerful Monarchs; and after having contributed to the tranquillity and happineſs of ſo many illuſtrious Princes, who, being educated under the maternal care of the moſt virtuous PRINCESS, are become highly important to mankind, and have filled the world with a well-founded confidence to ſee its happineſs promoted by their means; whatever advantage or reputation I have acquired from ſuch a flattering appointment, I derive it all from your friendſhip.

My earneſt deſire of not quitting this country without leaving you ſome public teſtimony of my real ſentiments towards you, excited me to hurry this work to the preſs without having time enough to finiſh it as I deſired. If it had been in my power to have ſpent the enſuing winter in this country, I might poſſibly have made it more worthy your patronage, and of appearing in the world under your auſpices. I preſent it to you imperfect as it is; and beg of you to look upon it only as a public mark of my reſpect and gratitude, which I ſhall retain in full

force

force to the end of my life, and with which I have the honour of fubfcribing myfelf,

S I R,

Your very much obliged

and faithful friend and fervant,

J. INGEN-HOUSZ.

London,
October 12, 1779.

PREFACE.

THE common air, that element in which we live, that invisible fluid which ſurrounds the whole earth, has never been ſo much the object of contemplation as it has in our days: it never engaged ſo much the attention of the learned as it has of late years. This fluid, diffuſed every where, *the breath of life*, deſerves ſo much the more the attention and inveſtigation of philoſophers, as it is the only ſubſtance without which we can ſcarce ſubſiſt alive a ſingle moment, and whoſe good or bad qualities have the greateſt influence upon our

our constitution. The most active poisons which are known do not so quickly destroy the life of an animal as the want of air, or the breathing of it when it is rendered highly noxious. It will appear in this work, that those very plants, which, influenced by the light of the sun, repair the injury done to this fluid by the breathing of animals, and by many other causes, may, in different circumstances, poison so much this very element, as to render it absolutely unfit for respiration, and, instead of keeping up life, to extinguish it in a moment. Therefore this universally-diffused element deserves not only the pursuit of philosophers, but claims more immediately the attention of those whose profession it is to preserve health and to cure diseases. I have

bestowed

beſtowed ſome labour upon this ſubject, both as a philoſopher and as a phyſician.

When I firſt found in the works of that excellent philoſopher and inventive genius, the reverend Dr. Prieſtley, his important diſcovery, that plants wonderfully thrive in putrid air; and that the vegetation of a plant could correct air fouled by the burning of a candle, and reſtore it again to its former purity and fitneſs for ſupporting flame, and for the reſpiration of animals; I was ſtruck with admiration: and I could not read afterwards, but with a kind of extaſy, the application which Sir John Pringle made of this diſcovery in his elaborate diſcourſe, delivered at the Royal Society in November 1773, when he conferred, as preſident of that

learned

learned Body, the annual prize medal upon Dr. Prieſtley, decreed to him as an honourable teſtimony of their approbation of the ſucceſsful labours beſtowed by him upon the doctrine of air. "From theſe diſco-
"veries," ſays he, "we are aſſured,
"that no vegetable grows in vain,
"but that, from the oak of the foreſt
"to the graſs in the field, every in-
"dividual plant is ſerviceable to
"mankind; if not always diſtin-
"guiſhed by ſome private virtue,
"yet making a part of the whole,
"which cleanſes and purifies our
"atmoſphere. In this the fragrant
"roſe and deadly night-ſhade co-
"operate: nor is the herbage, nor
"the woods that flouriſh in the
"moſt remote and unpeopled re-
"gions, unprofitable to us, nor we
"to them; conſidering how con-
"ſtantly

"ſtantly the winds convey to them "our vitiated air for our relief, and "for their nouriſhment. And if "ever theſe ſalutary gales riſe to "ſtorms and hurricanes, let us ſtill "trace and revere the ways of a be-"neficent Being, who not fortui-"touſly, but with deſign, not in "wrath, but in mercy, thus ſhakes "the waters and the air together, to "bury in the deep thoſe putrid and "peſtilential effluvia which the ve-"getables upon the face of the "earth had been inſufficient to con-"ſume."

Since I read that elegant diſcourſe, I have wiſhed that ſome induſtrious philoſopher would beſtow his labour in tracing Nature in its operations, and in diſcovering the manner in which the vegetable kingdom is ſubſervient to the animal, in correcting the mafs of air contaminated by their refpiration, and perhaps too by their perfpiration. The following pages will fhew, whether the pains I took, in the courfe of this fummer, to inveftigate this important fubject, have been attended with fome degree of fuccefs. I am far from thinking that I have difcovered the whole of this falutary operation of the vegetable kingdom; but I cannot but flatter myfelf, that I have at leaft proceeded a ftep farther than others, and opened a new path for penetrating deeper into this myfterious labyrinth.

Among the various ufeful difcoveries with which Dr. Prieftley has already enriched, and ftill continues to enrich, natural knowledge, none, in my opinion, are of more import-

ance

ance than thofe he made upon the various kinds of airs.

The difcovery of that wonderful aerial fluid, which in purity and fitnefs for refpiration fo far exceeds the beft atmofpheric air, that an animal may protract its life in it five times longer than in the beft common air, excites fo much the greater admiration, as he found it firft in fuch bodies which by their nature muft have been fufpected to conceal rather within their fubftance deleterious qualities, fuch as calcined mercury and red precipitate. He has given to this air the very proper appellation of *dephlogifticated air*, or air deprived of that inflammable principle which is the chief ingredient that renders our atmofpheric air more or lefs impure, and thus more or lefs fit for refpiration.

 His

His discovery of that peculiar quality which nitrous air possesses, of destroying or being destroyed by common air in proportion to its purity, is one of those inventions whose utility will be more and more conspicuous, when it shall have undergone all the improvements of which it is susceptible. Let it be mentioned to his honour, that his candour and modesty have made him under-rate the value of this useful production of his inquiries, when he says, in his last work, intitled, *Experiments and Observations relating to various Branches of Natural Philosophy, with a Continuation of the Observations of Air*, p. 269, "When I first discovered the pro-
" perty of nitrous air as a test of
" the wholesomeness of common
" air, I flattered myself that it might
" be

" be of confiderable practical ufe;
" and, particularly, that the air of
" diftant places and countries might
" be brought and examined together
" with great eafe and fatisfaction:
" but I own, that hitherto I have
" rather been difappointed in my
" expectation from it." And he
concludes thus: " I have frequently
" taken the open air in the moft
" expofed places in this country, at
" *different times of the year*, and in
" different ftates of the *weather*,
" &c.; but never found the differ-
" ence fo great as the inaccuracy,
" arifing from the method of
" making the trial, might eafily
" amount to or exceed."

Since I faw the manner of putting different airs to the nitrous teft, which Abbé Fontana now makes ufe of, and which I have in my inquiries for the moſt part imitated, I cannot but think more favourably of the importance of this diſcovery than the author himſelf does. I even think with the Abbé, that, by uſing convenient and accurate inſtruments, and by obſerving to the greateſt nicety all the manoeuvres of the operation conſtantly in the ſame way, we may with as much preciſion judge of the degree of purity of common air, as we now are able to judge of its degree of heat and cold by a good thermometer.

Indeed, by this method, even all the changes which the conſtitution of the atmoſphere undergoes daily, in the ſame place, are obſerved with ſo much accuracy, that, by making ten obſervations with the ſame air, the difference will ſcarcely amount

to

to $\frac{1}{500}$dth of the two airs employed in the experiment.

The difcovery of Dr. Prieftley, that plants thrive better in foul air than in common and in dephlogifticated air, and that plants have a power of correcting bad air, has thrown a new and important light upon the arrangement of this world. It fhews, even to a demonftration, that the vegetable kingdom is fubfervient to the animal; and, *vice verfâ*, that the air, fpoiled and rendered noxious to animals by their breathing in it, ferves to plants as a kind of nourifhment. But in what manner this faculty of the plants is excited remained ftill unknown.

There was even fome doubt left in the mind of many philofophers, whether the facts related by Dr. Prieftley were not owing to fome particular accident, as they had by no means been uniform; nay, had even been often contradictory, as he himſelf candidly owns (ſee vol. I. p. 91, &c. of Dr. Prieſtley's work on the ſubject of air, and his laſt work, p. 296.); and as Mr. Sheele had conſtantly obſerved a contrary effect from beans.

Dr. Prieſtley acknowledges, p. 299. that, by repeating (1778) again his experiments, *they proved to be unfavourable to his former hypotheſis.* "For," ſays he, "whe-"ther I made the experiments with "air injured by reſpiration, the "burning of candles, or any other "phlogiſtic proceſs, it did not grow "better but worſe; and the longer "the plants continued in the air, "the more phlogiſticated it was." He proceeds thus farther: "I have "had

" had ſeveral inſtances of the air
" being undoubtedly meliorated by
" this proceſs, eſpecially by the
" ſhoots of ſtrawberries, and ſome
" other plants, which I could, by
" bending, introduce into the jars
" or phials of air ſupported near
" them in the garden, when the
" roots continued in the earth.—
" I had other inſtances, no leſs un-
" queſtionable, of common air not
" only receiving no injury, but even
" conſiderable advantage, from the
" proceſs, having been rendered in
" ſome meaſure dephlogiſticated by
" it, ſo as to be much more dimi-
" niſhed by nitrous air than before,
" a thing which I was far from ex-
" pecting.—In moſt of the caſes in
" which the plants failed to me-
" liorate the air, they were either
" manifeſtly ſickly, or at leaſt did

" not grow and thrive, as they did " moſt remarkably in my firſt ex- " periments at Leeds, the reaſon of " which I cannot diſcover.—In " thoſe inſtances in which the plants " grew the beſt, they were, how- " ever, but ſickly, as appeared by the " leaves ſoon turning yellow, and " falling off when the leaſt motion " was given to them. In ſome " caſes, however, as in thoſe men- " tioned in vol. I. p. 91, I ſaw no " particular reaſon why the air " ſhould not have been meliorated.

" Upon the whole, I ſtill think " it *probable*, that the vegetation " of healthy plants, growing in ſi- " tuations natural to them, has a " ſalutary effect on the air in which " they grow. For one clear in- " ſtance of the melioration of air in " theſe circumſtances ſhould weigh

" againſt

"against a hundred cafes, in which "the air is made worfe by it."

Soon after, p. 305, he relates feveral inftances in which a plant had, in the fpace of feven, eight, ten, or more days, effectually mended the foul air in which it was made to grow. P. 309, he relates a fact, in which a fprig of *winter favory*, kept growing in a jar from the 16th of June to the 20th, had improved the air evidently, which improvement he found by three repeated trials to be in the proportion of 1.275 to 1.375. He relates another inftance, in which air was fo much improved by a fprig of parfley growing in it, from the 16th of June to the 1ft of July, that one meafure of it with one of nitrous air occupied only one meafure.

After

After all, he concludes with the following words, p. 310: " When " theſe obſervations are well conſi- " dered, I think it will hardly be " doubted but that there is ſome- " thing in the proceſs of vegetation, " or at leaſt ſomething uſually *at-* " *tending* it, that tends to meliorate " the air, in which it is carried on, " whatever be the *proximate cauſe* " of this effect, whether it be the " plants imbibing the phlogiſtic " matter, as part of their nouriſh- " ment, or whether the phlogiſton " unites with the vapour that is " continually exhaled from them ; " though of the two opinions I " ſhould incline to the former."

Mr. *Sheele* is ſo far from thinking that air is meliorated by plants, that he even maintains, that vegetation has the ſame effect on air that

that refpiration has. He allows, however, that plants do not grow fo well in dephlogifticated as in common air.

At the end of Section XXXIII, in which he treats *of the fpontaneous emiffion of dephlogifticated air from water in certain circumftances*, he fpeaks thus: "It will probably be "imagined, that the refult of the "experiments recited in this Sec-"tion throws fome uncertainty on "the refult of thofe recited in this "volume, from which I have con-"cluded, that air is meliorated by "the vegetation of plants, efpecially "as the water, by which they were "confined, was expofed to the open "air, and the fun, in a garden. "To this I can only fay, that I was "not then aware of the effect of "thefe circumftances, and that I

"have

" have represented the naked *facts* " as I observed them; and, having " no great attachment to any par- " ticular *hypothesis*, I am very will- " ing that my reader should draw " his own conclusions for himself." Dr. Priestley, having observed that bubbles of air seemed to issue spontaneously from the stalks and roots of several plants kept in the water, suspected immediately, that perhaps this air, if found better than common air, had been percolated through the plant, and purified by leaving its phlogiston in the plant as its nourishment. With this view he plunged many phials containing sprigs of mint in water, laying them in such a manner, as that any air, which might be discharged from the roots, would be retained in the phials, the bottoms being

a little

a little elevated. In this poſition the ſprigs of mint grew very well, and in ſome of the phials he obſerved a quantity of air to be collected, though very ſlowly; but he was much diſappointed, that ſome of the moſt vigorous plants produced no air at all. At length, however, from about ten plants he collected, in the courſe of a week, about half an ounce-meaſure of air, which he found ſo pure, that one meaſure of it and one of nitrous air occupied the ſpace of only one meaſure.

This remarkable fact contributed not a little to confirm his faith in the hypotheſis of the purification of the atmoſphere by vegetation; but he did not enjoy this ſatisfaction long; for, as he found that other plants of the ſame ſpecies produced no ſuch effect, and that, what he thought more extraordinary, the phials, in which the above mentioned plants had grown, the inſide of which were covered with a green kind of matter, continued to yield air as well when the plants were out of them as they had done before; he was convinced, that the plants had not, as he had imagined, contributed any thing to the production of this pure air. See Dr. Prieſtley's laſt work, p. 337 and 338.

Thus far this matter was carried on when I took it up in June laſt. I muſt acknowledge, that, from what is above related from Dr. *Prieſtley's* works, I had little doubt but there was ſome quality in plants proper for correcting bad air, and improving ordinary air. My curioſity led me to inveſtigate in what manner

this

this operation is carried on, whether the plants mend air by abſorbing, as part of their nouriſhment, the phlogiſtic matter, and leaving thus the remainder of the air pure (to which opinion Dr. Prieſtley inclines the moſt); or whether perhaps the plants poſſeſs ſome particular virtue hitherto unknown, by which they change bad air into good air, and good into better, which I ſuſpected to be the caſe.

I was not long engaged in this enquiry before I ſaw a moſt important ſcene opened to my view: I obſerved, *that plants not only have a faculty to correct bad air in ſix or ten days, by growing in it, as the experiments of Dr. Prieſtley indicate, but that they perform this important office in a compleat manner in a few hours; that this wonderful operation* *is by no means owing to the vegetation of the plant, but to the influence of the light of the ſun upon the plant. I found that plants have, moreover, a moſt ſurprizing faculty of elaborating the air which they contain, and undoubtedly abſorb continually from the common atmoſphere, into real and fine dephlogiſticated air; that they pour down continually, if I may ſo expreſs myſelf, a ſhower of this depurated air, which, diffuſing itſelf through the common maſs of the atmoſphere, contributes to render it more fit for animal life; that this operation is far from being carried on conſtantly, but begins only after the ſun has for ſome time made his appearance above the horizon, and has, by his influence, prepared the plants to begin anew their beneficial operation upon the air, and thus upon the animal creation,*

which

which was ſtopt during the darkneſs of the night; that this operation of the plants is more or leſs briſk in proportion to the clearneſs of the day, and the expoſition of the plants more or leſs adapted to receive the direct influence of that great luminary; that plants ſhaded by high buildings, or growing under a dark ſhade of other plants, do not perform this office, but, on the contrary, throw out an air hurtful to animals, and even contaminate the air which ſurrounds them; that this operation of plants diminiſhes towards the cloſe of the day, and ceaſes entirely at ſun-ſet, except in a few plants, which continue this duty ſomewhat longer than others; that this office is not performed by the whole plant, but only by the leaves and the green ſtalks that ſupport them; that acrid, ill-ſcented,

 and

and even the moſt poiſonous plants perform this office in common with the mildeſt and the moſt ſalutary; that the moſt part of leaves pour out the greateſt quantity of this dephlogiſticated air from their under ſurface, principally thoſe of lofty trees; that young leaves, not yet come to their full perfection, yield dephlogiſticated air leſs in quantity, and of an inferior quality, than what is produced by full-grown and old leaves; that ſome plants elaborate dephlogiſticated air better than others; that ſome of the aquatic plants ſeem to excell in this operation; that all plants contaminate the ſurrounding air by night, and even in the day-time in ſhaded places; that, however, ſome of thoſe which are inferior to none in yielding beneficial air in the ſun-ſhine, ſurpaſs others in the

power

power of infecting the circumambient air in the dark, even to ſuch a degree, that in a few hours they render a great body of good air ſo noxious, that an animal placed in it loſes its life in a few ſeconds; that all flowers render the ſurrounding air highly noxious, equally by night and by day; that the roots removed from the ground do the ſame, ſome few, however, excepted; but that in general fruits have the ſame deleterious quality at all times, though principally in the dark, and many to ſuch an aſtoniſhing degree, that even ſome of thoſe fruits which are the moſt delicious, as, for inſtance, peaches, contaminate ſo much the common air as would endanger us to loſe our lives, if we were ſhut up in a room in which a great deal of ſuch fruits are ſtored up; that the ſun by

 itſelf

itſelf has no power to mend air without the concurrence of plants, but on the contrary is apt to contaminate it.

Theſe are ſome of the ſecret operations of plants I diſcovered in my retirement, of which I will endeavour to give ſome account in the following pages; ſubmitting, however, to the judgement of the candid reader the conſequences, which I thought might fairly be deduced from the facts I am to relate.

I muſt not omit to acquaint the reader, that, in purſuing the experiments related in this work, he will find that he labours in vain, if he does not make uſe of pump-water freſhly drawn; for if this water has been expoſed to the open air during ſome time, it will have parted with a great deal of its own air, and will therefore be apt to abſorb the air from the leaves. It may alſo happen, that every pump-water may not be found equally as good as that which I met with in my country dwelling, though as yet I have no poſitive reaſon to think ſo; but I have ſome grounds to believe, that water drawn from an open well is far inferior in goodneſs to that which is forced up by a pump, as the former is too much expoſed to the open air.

By caſting an eye upon the experiments related in this work, it will be eaſily underſtood, why, in every experiment of this kind, ſome difference in the reſult will commonly be obſerved; for the peculiar degree of goodneſs of the dephlogiſticated air obtained from the leaves depends upon too many circumſtances to be conſtantly of the ſame quality. Some more or leſs light of the ſun thrown upon the jar will make ſome difference; the leaves being more or leſs crowded together, will make a remarkable difference, as a great number of them may be ſhaded from the ſun by others.

As I made the greateſt part of my experiments according to the preſent method of proceeding of my reſpectable friend the Abbé Fontana, it would have been difficult to imitate the experiments related in this work, and even to underſtand the manner in which they were made, if he had not given me leave to anticipate the publication of his own ingenious contrivance, and of his preſent me-

thod

thod of putting the different ſpecies of air to the teſt. This kindneſs of that gentleman deſerves my public thanks.

Inaccuracies in the manner of expreſſing myſelf will find ſome indulgence in a man born and educated in the Republic of the United Provinces, and who was not early in life acquainted with the Engliſh language.

The August Sovereigns, whom I have the honour to ſerve, condeſcending graciouſly to prolong my leave of abſence, and allowing me to ſpend the preſent ſummer in this iſland, I thought it my duty to apply the time granted me by their goodneſs to an uſeful purpoſe, and to make all the advantage I could of that peculiar degree of health which

I have

I have always enjoyed in this climate.

On purpose to avoid every cause of obstructing my mind in the close pursuit of the object I had in view, and in tracing Nature in its operation on this subject, I disengaged myself from the noise of the metropolis, and retired to a small villa, where I was out of the way of being interrupted by any body in the contemplation of Nature.

This work is a part of the result of above 500 experiments, all which were made in less than three months, having begun them in June, and finished them in the beginning of September, working from morning till night. From these experiments some more consequences might have been drawn, if I had had more time to employ myself

in

in a work upon ſuch important matter. Whatever I have been able to deduce from my labours is done in a haſty manner, as my ſtay in this country was far too limited to allow me to compoſe my work in a regular and more ſatisfactory manner.

Though I was very far from foreſeeing all the diſcoveries which I made in the courſe of this ſummer, yet I was perſuaded that a good deal of the oeconomy of the vegetable kingdom might be diſcovered by a ſteady purſuit of experiments tending to trace the operations of Nature. I had this object in view ſome years ago; but, as I did not enjoy ſuch a favourable diſpoſition of mind and body as was neceſſary for a taſk, in which all poſſible ſteadineſs, perſeverance,

verance, and close attention were requisite, I deferred the undertaking till I should find myself more fit for it.

Detached experiments may indeed be very useful when a sufficient number is collected to draw some conclusions from them; but, without pursuing methodically the same object, discoveries are to be expected only by mere chance, and are even sometimes overlooked. I owe to the example of my worthy friend, the Abbé Fontana, the thorough persuasion, which I now entertain, that natural knowledge can make but a very slow progress in the hands of those who have not patience and assiduity enough in pursuing one and the same object, till they discover some things undiscovered before; or till they find

that

that the difficulty of the undertaking ſurpaſſes their abilities.

WHEN this book was entirely printed, and nothing but the latter end of the preface unfiniſhed, I was informed by my friend the Abbé Fontana, that he diſcovered a few days ago a new method of procuring to a ſick perſon the benefit of breathing any quantity of dephlogiſticated air at a cheap rate.

This very year a paper of mine was read before the Royal Society, and ordered for the preſs (containing a new theory of the effects of gunpowder, and the diſcovery of a new and powerful exploſive air), in which I ſay, that the rapid progreſs our modern philoſophers daily make on the different kinds of air, induce me to believe that we touch at the happy moment, at which a very eafy and little expenfive method of producing this beneficial fluid, in any quantity wanted, will be produced for the cure of many difeafes.

I have the great fatisfaction to inform the reader, that my prediction is fulfilled even before it hath reached the public, and that this important *defideratum* in medicine is difcovered.

Abbé Fontana found that an animal breathing-in either common or dephlogifticated air renders it unfit for refpiration by communicating to it a confiderable portion of fixed air, which is generated in our body, and thrown out by the lungs as excrementitious. This fixed air is eafily abforbed abſorbed by ſhaking it in common water, but infinitely more readily by the contact with quick-lime water.

He fills one of the large receivers of an air-pump, which are very wide at their upper extremity, half full of dephlogiſticated air extracted from nitre, ſo that it may contain about 500 cubic-inches of this air, which will ſerve for breathing during half an hour. The manner of drawing this air out of the receiver, is either by thruſting a bended glaſs tube under the receiver (when this is floating in water, in which it is ſupported by its peculiar bulky form), reaching into the air itſelf, and keeping the other extremity in the mouth; thus drawing this air in the lungs, and breathing it out by the ſame tube. This air returning from the

the lungs is infected by fixed air; which being immediately abſorbed by the contact with lime-water, the dephlogiſticated air is reſtored very near to its former purity. Inſtead of the bended tube juſt mentioned, the top of the receiver may be made as the neck of a bottle, and a tube may be fixed to it, having a cock to open and ſhut the paſſage as required.

We conſume, by each inſpiration, about 30 cubic-inches of air; and thus, allowing 15 inſpirations for a minute, we conſume each minute 450 cubic-inches of air. The Abbé Fontana found, that the dephlogiſticated air being, after each reſpiration, purified again by the lime-water, will remain good about thirty times as long as it would when breathed in the ordinary way; and that

that thus the quantity of dephlogiſticated air neceſſary for one minute will now ſerve for breathing during half an hour, and thus the expences will be thirty times leſs.

We conſume, in the ſpace of 24 hours, about 21600 cubic-inches of air. One pound of nitre yields by heat about 12000 cubic-inches of dephlogiſticated air, and thus it yields more air than any patient could conſume by breathing this beneficial air the whole day (for we muſt allow at leaſt 12 hours in the 24 for ſleeping and neceſſary occupations), becauſe this quantity will ſerve thirty times longer by the method explained, than in the ordinary way. It follows by this, that the expences required for breathing a whole day dephlogiſticated

cated air cannot amount to one ſhilling.

I have only juſt time enough to announce this happy diſcovery to the publick; whoſe great utility will, I truſt, ſoon be found in the curing of inflammatory and putrid diſeaſes, &c. in which too great a quantity of inflammable principle is let looſe in our blood.

I HAVE alſo diſcovered, ſince my book was printed, that, in reading Dr. Prieſtley's laſt work, I had overlooked a remarkable paſſage, p. 270, in which he hints at what I found to be the caſe with inflammable air having ſtood a long while with plants. I think it my duty to relate his own words: "I am "ſatisfied, however, from my own "obſervations, that air may be "very offenſive to the noſtrils, pro-"bably hurtful to the lungs, and "perhaps alſo in conſequence of "the preſence of phlogiſtic matter "in it, without the phlogiſton be-"ing ſo far *incorporated with it*, "as to be diſcovered by the mix-"ture of nitrous air."

ERRATA, neceſſary to be corrected.

Page 35, l. 20, before the words *we may find* add *if we could trace the final cauſes of all the operations of nature,*

Page 43, l. 7, for *of which quantity gathered* read *which quantity was gathered*

Page 67, l. 5, for *leaving* read *living*

Page 97, l. 11, after *nitrous* add *and common*

Page 120, l. 5, read *which time may diſcover how to ſeparate, and thus*

Page 142, l. 10, *and this atmoſphere* read *and the air*

Page 187, l. 2, read *by Abbé Fontana's method, becauſe only one meaſure of dephlogiſticated air was employed.*

Page 290, l. 6, read *of bubbles when it was heated in the ſun, was ſo much recovered, that freſh leaves*

Explication of ſome Technical Terms.

AS this volume may poſſibly fall into the hands of ſome who have not yet read the works of Dr. Prieſtley, and are entirely ſtrangers to the new doctrine of air; I think it will be uſeful to explain the meaning of the new invented names given to different kinds of air mentioned in this book.

Nitrous air is that kind of *permanent elaſtic fluid* which is extracted by nitrous acid or *aqua fortis* from the moſt part of metals, principally from mercury, braſs, copper, &c. This air is by a bended glaſs tube conducted under an an inverted jar full of water. The nitrous air, thus difengaged, rifes through the water, and fettles at the inverted bottom of the jar. Mercury yields the ftrongeft nitrous air, and always of the fame quality; but heat muft be applied for its extrication, if a greater quantity is in a fhort time required. I ufe for convenience fake copper, from which nitrous air is extracted in abundance in a fhort time, without applying heat. The nitrous acid muft be diluted with water for the purpofe.

Inflammable air is that air which rifes up in bubbles from ftagnant waters, whofe bottoms are marfhy, when the ground is ftirred up with a ftick. It is alfo extracted from iron, zinc, and fome other metals, by diluted vitriolic or marine acid.

This

This air has in common with almoſt all other inflammable ſubſtances, that it is not ſuſceptible of actual inflammation, without it be in contact with common or reſpirable air. This air has the following qualities by which it may be known: it is not abſorbed by ſhaking it in water; it is not diminiſhed by the addition of nitrous air; it is inſtantly and abſolutely mortal to animals breathing in it; it burns by the approach of the flame of a candle, where it is in contact with common air; but the whole of it inflames at once, and explodes with a loud report, when it is intimately mixed with common, and principally with dephlogiſticated air.

Phlogiſticated air is, properly, air impregnated with phlogiſton, or with the inflammable principle. It has received this name becauſe common air, expoſed to the calcination of metals, becomes phlogiſticated air; which alteration ſeems to depend on the phlogiſton of the metal being communicated to it, for the metal itſelf has loſt it in the calcination; and becauſe the burning of a candle, and many other phlogiſtic proceſſes, impart to common air this quality. The air returning from our lungs is ſaid to be phlogiſticated more or leſs, becauſe it is found to poſſeſs the ſame qualities as the air expoſed to the calcination of metals. This kind of air is known by the following properties: it is not abſorbed by water; it is not, or not much, diminiſhed by nitrous air; it is poiſonous for an animal who breathes in it; it is not inflammable either by itſelf

ſelf or by the addition of reſpirable air, but extinguiſhes flame.

Dephlogiſticated air is that pure, etherial, permanent, and elaſtic fluid which the celebrated Dr. Prieſtley found out the firſt, and gave this very well adapted name to it. It is reſpirable air, deſtitute of the phlogiſtic or inflammable principle with which the beſt atmoſpheric air is found always to be more or leſs contaminated. It is in reality common air of the higheſt purity, ſuch as never exiſts in the common atmoſphere. It is even ſo far ſuperior in purity to common air, that an animal ſhut up in a veſſel, filled with this air, protracts its life four or five times, nay even in ſome caſes ſeven times longer, than if it was ſhut up in a veſſel filled with the beſt atmoſpheric or common air.

Some

Some of its qualities, by which it may be known, are the following: it is not abſorbable by the contact of water; the flame of a candle plunged in it becomes larger, and of the moſt admirable brilliancy, ſo as to dazzle the eyes; red-hot charcoal plunged in it becomes ſhining and ſparkling; it is much more diminiſhed by nitrous air than common air; it explodes, with an uncommon loud report, when mixed with a certain proportion of inflammable air, or when a few drops of good vitriolic æther are poured in a veſſel containing this air, as I diſcovered.

Fixed air is that kind of aerial fluid which iſſues in abundance from fermenting ſubſtances, and which, in ſome places, riſes out of the ground by itſelf, as in the fa-

mous

mous *Grotta del Cane* near Naples. It is this air with which ſome mineral waters are impregnated, and to which they owe their pungent taſte and their virtue, as, for inſtance, Seltzer waters. It is that air which ariſes in abundance from calcarious ſtones, by the addition of vitriolic acid. This air may be known by the following properties: it extinguiſhes flame; it is abſorbed by water, and communicates to it the ſame pungent, acidulous taſte as Seltzer water has, ſo as not to be diſtinguiſhed from it either by the taſte or by its virtues; it precipitates quick lime from water; it immediately cryſtallizes *oleum tartari per deliquium*, when put in a veſſel lined with this oil; it is mortal to animals breathing in it.

Eudio-

Eudiometer, is a new word; it ſignifies an inſtrument by which we may judge of the degree of ſalubrity of the common air. The invention of ſuch an inſtrument belongs to Dr. Prieſtley. It conſiſts chiefly of a glaſs tube, divided in equal parts; for inſtance, in two large diviſions; each of which is divided into ten others, and each of theſe ten ſub-divided again into ten parts: and a glaſs meaſure, containing exactly one of the great diviſions of the tube. One meaſure of common air and one of nitrous air, put together in a ſeparate glaſs veſſel, and left by themſelves till the diminution of the bulk of the two airs is compleated, and afterwards let up in the glaſs tube, indicates at once the exact diminution of the two joint meaſures. The degree of

6 goodneſs

goodneſs of the common air is found to be in proportion to the diminution of the bulk of the two airs. Mr. Magellan, F. R. S. has publiſhed a work upon an ingenious contrivance of his own of this kind, which is to be ſold by Mr. Parker in Fleet-Street, with the proper directions how to uſe it. What conſiderable improvements we owe in this regard to Abbé Fontana, will appear in the introduction to the ſecond part of this work.

ADVERTISEMENT.

AS the author intends to publiſh a French tranſlation of this work, he thinks it his duty to give public notice of his intentions, that no one may give himſelf any unneceſſary pains about it.

CONTENTS.

ON

ON THE NATURE OF PLANTS.

SECTION I.

Some general remarks on the nature of the leaves of plants, and their uſe.

IT ſeems to be more than probable, that the leaves, with which the moſt part of plants are furniſhed during the ſummer in temperate climates, and perpetually in hot countries, are deſtined to more than one purpoſe. Such a great apparatus, which nature diſplays as ſoon as the ſun begins to afford a certain degree of warmth upon the ſurface of the earth, can ſcarcely be conſidered as ſolely deſtined either to ornament, to nouriſhment of the plant, to its growth, to ripen its fruit, or for any other peculiar and ſingle uſe. It ſeems probable, that they are uſeful to the growth of the tree; for, by depriving the tree of all its leaves, it is in danger of decay. By taking a conſiderable part of the leaves from a fruit tree, the fruit is leſs perfect; and by taking them all away, the fruit decays and falls before its maturity. It is alſo probable, that the tree receives ſome advantage from the leaves abſorbing, by their means, moiſture from the air, from rain, and from dew; for it has been found a conſiderable advantage to the growth of a tree, to water the ſtem and the leaves now and then. But I leave the diſcuſſion of thoſe articles to others, who have made theſe conſiderations an object of their purſuits. The late Mr. Baker has publiſhed to the world his microſcopical obſervations on the ſubject. Mr. Bonnet, of Geneva, has publiſhed a very elaborate work upon the ſame, intitled, *Recherches ſur l'uſage des Fueilles dans les Plantes, et ſur quelques autres Sujets relatif à l'Hiſtoire de la Vegetation, par* Charles Bonnet, *à* Gottingen *et* Leiden, 1754. This work contains a great deal of intereſting inquiries upon the nature, properties, and utilities of thoſe wonderful organs; all which have been treated with the greateſt attention, and have thrown much light upon this ſubject.

This celebrated author has taken a great deal of notice of thoſe air bubbles which cover the leaves when plunged under water. He ſays, p. 26, that the leaves draw theſe bubbles from the water. He is the more perſuaded that this is the caſe, becauſe he found theſe bubbles did not appear when the water had been boiled ſome time, and appeared more when the water is impregnated with air, by blowing in it. He had alſo obſerved, that they did not appear after ſunſet. Page 31, he explains his opinion farther upon this head: he ſays, that theſe air bubbles are produced by common air adhering to the external ſurface of the leaves, which ſwells up into bubbles by the heat of the ſun; and that the cold of the night is the reaſon why theſe air bubbles do not make their appearance at that time. As he found that dry leaves put under water gather ſuch bubbles alſo upon their ſurface, he concludes, p. 33, that the appearance of theſe bubbles cannot be owing to any vital action in the leaves.

I took ſome pains to diſcloſe the cauſe of theſe bubbles, which, I think, are of more importance than Mr. Bonnet at that time imagined them to be; and found the fact to be generally this:

The moſt part of leaves gather theſe bubbles upon their ſurface, when plunged in any water in the ſun-ſhine or by day-time in the open air; but infinitely more in fresh pump water than in any other. In clear river water they appear later, less in number and in size; less so in rain water, and the least of all in boiled water, in stagnating, and in distilled water.

They are not produced by the warmth of the sun rarifying the air adhering to the leaves; for many kinds of leaves produce them almost as soon as plunged under water, though the water be very cold, and the leaves warm from the sun-shine be plunged in it.

They do not appear after sun-set, at least not in any considerable number; but those that already exist do not shrink in or disappear by the cold of the night.

As soon as the sun begins to diffuse its warmth over the surface of the earth in the spring, and to promote that general tendency to corruption which all dead bodies of the animal and vegetable kingdom, and many other ſubſtances, are ſo liable to, the trees diſplay in a few days the moſt wonderful ſcene that can be imagined. Contracted as they were in that ſtate of ſtupor and inactivity in which they remain during the winter, expoſing to the air no other ſurface than that of their trunk and branches, as if they wanted to have as little to do as poſſible with the external air, they all at once increaſe, perhaps more than a thouſand times, their ſurface by diſplaying thoſe kind of numberleſs fans which we call leaves. Some of them produce their leaves a long while before any flowers appear upon them; others a good while after the flowers are formed, and the fructification is already in an advanced ſtate; and keep their leaves in the beſt condition, and even puſh out continually new ones, long after the whole fructification is finiſhed; which ſeems to indicate, that the chief uſe of theſe fans is not to aſſiſt the fructification and propagation of their ſpecies. Theſe fans, when compleated, ſeem to compoſe or arrange themſelves in ſuch a manner as to expoſe their upper and varniſhed ſurface to the direct influence of the ſun, and to hide as much as they can their under ſurface from the direct influence of this luminary. It ſeems as if they required rather the light of the ſun than the influence of its heat, as their poliſhed ſurfaces muſt reflect ſome of the rays of the ſun, and thus moderate the degree of heat.

It

It will, perhaps, appear probable, that one of the great laboratories of nature for cleanſing and purifying the air of our atmoſphere is placed in the ſubſtance of the leaves, and put in action by the influence of the light; and that the air thus purified, but in this ſtate grown uſeleſs or noxious to the plant, is thrown out for the greateſt part by the excretory ducts, placed chiefly, at leaſt in far the moſt part of plants, on the under ſide of the leaf.

Is there not ſome probability that the under part of the leaves may have been chiefly deſtined for this purpoſe; becauſe in this way the dephlogiſticated air, guſhing continually out of this ſurface, is inclined to fall rather downwards, as a beneficial ſhower for the uſe of the animals who all breathe in a region of the

the air inferior to the leaves of trees? Does not this conjecture get ſome weight, if we conſider that dephlogiſticated air is in reality ſpecifically heavier than common air, and thus tends rather to fall downwards?

If we add to theſe reflexions another of no leſs importance, *viz.* that moſt ſorts of foul air are ſpecifically lighter than common air, we ſhall be inclined to believe that the difference of the ſpecifical gravity of that beneficial air of which I treat, and that which is become hurtful to our conſtitution by corruption, breathing, and other cauſes, indicates one of thoſe ſpecial bleſſings deſigned by the hand of God: for by this arrangement we get ſoon rid, in a great meaſure, of that air which is become hurtful to us, as it riſes ſoon up out of our reach; whereas the

the dephlogifticated air, being heavier than common air, is rather inclined to fettle on the furface of the earth among the animal creation.

But, as animals fpoil equally as much air in the winter as in the fummer by the act of refpiration, it might feem fomewhat furprizing, that this great laboratory ceafes intirely by the decay of the leaves. Is this defect fupplied by fome other means equally powerful? Though we are very far from being able to trace all the active caufes which contribute their fhare in keeping up the wholefomenefs of our atmofphere, yet we have already traced fome of them, and therefore muft not defpair of difcovering fome more. The fhaking of foul air in water will in great meafure correct it. Water itfelf has a power

of

of yielding dephlogiſticated air, as Dr. Prieſtley diſcovered. Plants have a power to correct bad air, and to improve good air. Winds will blow away the noxious particles of the air, and bring on air corrected by the waters of the ſeas, lakes, rivers, and foreſts. All theſe cauſes exiſt equally in the winter as in the ſummer, or at leaſt nearly ſo. The influence of the vegetable creation alone ceaſes in the winter: but the loſs of this influence is, perhaps, more than amply counterbalanced by the diminution of the general promoting cauſe of corruption, *viz.* heat. Every body knows, that warm weather haſtens in a great degree putrefaction. In the ſummer time numberleſs inſects are produced, which did not exiſt in the winter: theſe inſects infect the air by the corruption of

their

their bodies. That immenſe quantity of animal ſubſtances, and many others, which undergo a putrefaction by the warmth of the weather, ſeems to require an additional power or agent to counter-act it; and this office is deſtined to the leaves. In froſty weather no animal ſubſtance is ſubject to putrefaction, which cannot go on without a proper degree of heat. The perſpiration of animals is leſs offenſive in the winter than in the ſummer, and of conſequence muſt corrupt the atmoſphere leſs. It ſeems therefore probable, that, if we are deprived of one way by which air is corrected in the winter, we have alſo at that time leſs cauſes which tend to contaminate our element.

SECTION II.

On the manner in which the dephlogisticated air is obtained from the leaves of plants.

AS the leaves of plants yield dephlogisticated air only in the clear day-light, or in the sun-shine, and begin their operation only after they have been in a certain manner prepared, by the influence of the same light, for beginning it; they are to be put in a very transparent glass vessel, or jar, filled with fresh pump water (which seems the most adapted to promote this operation of the leaves, or at least not to obstruct it); which, being inverted in a tub full of the same water, is to be immediately exposed to the open air, or rather

rather to the ſun-ſhine: thus the leaves continuing to live, continue alſo to perform the office they performed out of the water, as far as the water does not obſtruct it. The water prevents only new atmoſpheric air being abſorbed by the leaves, but does not prevent that air, which already exiſted in the leaves, from oozing out. This air, prepared in the leaves by the influence of the light of the ſun, appears ſoon upon the ſurface of the leaves in different forms, moſt generally in the form of round bubbles, which, increaſing gradually in ſize, and detaching themſelves from the leaves, riſe up and ſettle at the inverted bottom of the jar: they are ſucceeded by new bubbles, till the leaves, not being in the way of ſupplying themſelves with new atmo-

ſpheric

ſpheric air, become exhauſted. This aiı, gathered in this manner, is really dephlogiſticated air, of a more or leſs good quality, according to the nature of the plant from which the leaves are taken, and the clearneſs of the day-light to which they were expoſed.

It is not very rare to ſee theſe bubbles ſo quickly ſucceeding one another, that they riſe from the ſame ſpot almoſt in a continual ſtream: I ſaw this more than once, principally in the *nymphæa alba.*

SECTION III.

The manner in which the dephlogisticated air oozes out of the leaves is different in different plants.

IT is ſomewhat amuſing to obſerve the conſtant manner in which the dephlogiſticated air makes its appearance upon the ſame kind of leaves, and the ſurprizing difference which takes place in the leaves of different plants. Some leaves, for inſtance, form always ſmall round bubbles, as is the caſe with the moſt part of leaves; others form irregular flat bliſters, as do the leaves of the honeyſuckle, *caprifolium.* Some, and indeed the greateſt part, produce round bubbles on both ſurfaces; others yield on one ſurface round bubbles, on the other irregular bliſters; for inſtance, leaves of oak, which give flat bliſters on the under ſide, and round bubbles on the upper ſide. Some form only thoſe irregular bliſters at the upper ſide, as the leaves of ſpurge, *cataputia* or *euphorbia*.

Some leaves form neither bubbles nor bliſters on either ſide, and yet yield a great deal of dephlogiſticated air; for inſtance, leaves of *naſturtium Indicum:* theſe leaves ſeem to have a quality repulſive to water, which only forms a kind of cover over the ſurface of the leaves, without coming into immediate contact. The air oozing out of the leaves floats under this cover, and riſes to the higheſt part of the leaves, where it forms a kind of bags, which at laſt detach themſelves from the edge, and riſe to the top of the jar. The

leaves

leaves themſelves, after ſtanding a day and a half in the water, are not wetted by it, but come out quite dry.

Some leaves have that peculiar quality of being wetted by the water only on one ſide; as, for inſtance, leaves of raſberry ſhrubs, which do not receive the wet on their ſhaggy under ſurface.

Strawberry leaves repel the water from both ſurfaces, form bliſters at the under ſurface, and chiefly round bubbles at the upper ſurface.

Some leaves begin very early in the morning to yield dephlogiſticated air, and ceaſe late in the evening; for inſtance, potatoe and *malva* leaves.

Some begin this operation very late in the morning, and ceaſe very early in the evening; for inſtance, leaves of *laurocerafus.*

Some leaves yield the air bubbles immediately, as leaves of potatoe plants; ſome in a few ſeconds, as *malva*; ſome in a few minutes, as walnut trees; ſome much later, as leaves of *laurocerafus.*

Some yield the air bubbles firſt on the under ſide, as almoſt all leaves of trees; ſome firſt at the upper ſide, as leaves of *laurocerafus*; ſome on both ſurfaces at the ſame time, as *malva.*

On ſome leaves the air bubbles grow almoſt all regularly one with another, as in leaves of a vine, walnut, lime tree; in ſome they are, from the very beginning, of a very irregular ſize, as in *malva*, parſley, &c.

These

Thefe few inftances fhew the various ways in which this beneficial air is oozing from the leaves, and which can only be owing to the different organization of the leaves in different plants.

I have difcovered a good deal more of fuch remarkable peculiarities among leaves of different plants; but thofe above mentioned will be fufficient to fhew, that each plant follows in this regard its own nature; and that, therefore, thefe different appearances feem to depend upon fome vital motion going on in the fubftance of the leaves.

SECTION IV.

The dephlogiſticated air oozing out of the leaves in the water is not air from the water itſelf.

THE reverend Dr. Prieſtley found, that water, chiefly pump water, ſtanding ſome days by itſelf, forms at the bottom and ſides of the veſſel a kind of green matter, ſeemingly vegetable, from which air bubbles riſe continually to the top of the jar, if expoſed to the ſun-ſhine: that this air is fine dephlogiſticated air, which ſhews that there is a faculty in water to produce by itſelf this beneficial fluid; and thus, that the maſs of the waters of the ſea, lakes, and rivers, have their ſhare in purifying the atmoſphere.

But

But as this dephlogifticated air is not produced immediately from the pump water, but only when this green matter is formed, it is clear, that the air obtained from the leaves, as foon as they are put in the water, is by no means air from the water, but air continuing to be produced by a fpecial operation carried on in a living leaf expofed to the day-light, and forming bubbles, becaufe the furrounding water prevents this air from being diffufed through the atmofphere.

It is true, that pump water, placed in the fun-fhine, will foon yield fome fmall air bubbles, fettling at the bottom of the jar, and every where at the fides; but this air is very far from being the fame as that contained in the air bubbles of the leaves.

I placed, in a warm ſun-ſhine, a great number of inverted jars, full of pump water, and collected carefully from them all the air yielded by theſe bubbles, which proved to be much worſe than the common air.

I boiled ſome pump water in a pot, in which I had placed a long cylindrical jar, quite full of the ſame water: a good deal of air was collected at the top of the inverted jar, which was by the heat diſengaged from the water. This air proved to be much worſe than common air, and entirely unfit for reſpiration.

Abbé Fontana has made, ſome years ago, a great many experiments, tending to inveſtigate the nature of air contained in different waters.

SECTION V.

The dephlogiſticated air oozing out of the leaves in the water is not exiſting in the ſubſtance of the leaves in this pure ſtate, but is only ſecreted out of the leaves when it has undergone a purification, or a kind of tranſmutation.

IF the dephlogiſticated air collected from the leaves in the ſun exiſted in them in its pure ſtate, it muſt appear as ſuch when ſqueezed out of the leaves under water; or, at leaſt, if the leaves are only ſhook gently under water, without hurting their organization, or when they are put in warm or in boiling water.

I ſqueezed a handful or two of potatoe leaves under water, and kept an an inverted jar full of water above it, to receive the air. A great deal of it was inſtantly obtained, which proved to be nearly as good as common air.

I ſqueezed, in the ſame way, ſome air out of leaves of ſage, *ſalvia*, which proved to be ſomewhat worſe than the former.

A potatoe plant was ſhook under water, ſo as not to hurt it: a good deal of air was immediately diſengaged, which, by the nitrous teſt, proved to be worſe than common air.

A plant of *lamium album* was treated in the ſame way, and in like manner a good deal of air was obtained, which was nearly of the ſame quality with the former.

Some leaves of an apple tree were put in a cylindrical jar full of pump water. The jar was then inverted

in

in a veſſel full of the ſame water, and placed upon the fire. As ſoon as the water grew warm, the leaves were covered with air bubbles, juſt as in the ſun. After the water had boiled a little while, it was put by to cool. A great deal of air was obtained, which proved to be ſo bad as to extinguiſh flame.

Some of the ſame leaves were put into a jar, inverted in a pot full of water, and only placed near the fire: a great deal of air was obtained, but as poiſonous as the former.

SECTION VI.

The production of the dephlogisticated air from the leaves is not owing to the warmth of the sun, but chiefly, if not only, to the light.

IF the sun caused this air to ooze out of the leaves by rarifying the air in heating the water, it would follow that, if a leaf, warmed in the middle of the sun-shine upon the tree, was immediately placed in water drawn directly from the pump, and thus being very cold, the air bubbles would not appear till, at least, some degree of warmth was communicated to the water; but quite the contrary happens. The leaves taken from trees or plants in the midst of a warm day, and plunged immediately into cold water, are remarkably quick in forming air bubbles, and yielding the beſt dephlogiſticated air.

If it was the warmth of the ſun, and not its light, that produced this operation, it would follow, that, by warming the water near the fire about as much as it would have been in the ſun, this very air would be produced; but this is far from being the caſe.

I placed ſome leaves in pump water, inverted the jar, and kept it as near the fire as was required to receive a moderate warmth, near as much as a ſimilar jar, filled with leaves of the ſame plant, and placed in the open air, at the ſame time received from the ſun. The reſult was, that the air obtained by the fire

fire was very bad, and that obtained in the ſun was dephlogiſticated air.

A jar full of walnut tree leaves was placed under the ſhade of other plants, and near a wall, ſo that no rays of the ſun could reach it. It ſtood there the whole day, ſo that the water in the jar had received there about the ſame degree of warmth as the ſurrounding air (the thermometer being then at 76°); the air obtained was worſe than common air, whereas the air obtained from other jars kept in the ſunſhine during ſuch a little time that the water had by no means received a degree of warmth approaching that of the atmoſphere, was fine dephlogiſticated air.

No dephlogiſticated air is obtained in a warm room, if the ſun does not ſhine upon the jar containing the leaves.

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SECTION VII.

Reflections.

IT might, perhaps, be objected, that the leaves of the plants are never in a natural ſtate when ſurrounded by pump water; and that thus there may, perhaps, remain ſome degree of doubt, whether the ſame operation of the leaves in their natural ſituation takes place.

I cannot conſider the plants kept thus under water to be in a ſituation ſo contrary to their nature as to derange their uſual operation. Water, even more than they want, is not hurtful to plants, if it is not applied too conſiderable a time. The water only cuts off the communication with the external air; and we know,

that

that plants may live a long while without this free communication. Befides, water plants, as *perficaria urens*, *becabunga*, and others, which I have employed in my experiments, are often found a long while quite covered by the water in which they grow.

By bending a living plant (the root remaining in its own earth) in an inverted jar full of water, you only furprize nature upon the fact in the middle of its operation, by fhutting at once all communication with the free air. In fuch a fituation no air can be abforbed by the leaves, or by any parts of the plant under water; but any air may freely come out of it.

Without covering the leaves or the plant entirely with water, it is impoffible to know what quantity of of air oozes out of the plant, and of what quality this air is; for any air iſſuing out of a plant incorporates immediately with the ſurrounding air, and makes a compound whoſe conſtituent parts are an intimate mixture of air from the plant and common air; and it would be as difficult to judge accurately how much dephlogiſticated air ſuch a plant has communicated to the ordinary air which was already in the jar, as it would be for a chymiſt to judge accurately what quantity of diſtilled water was mixed with a certain quantity of common water, if ſome of it was really added to it on purpoſe to puzzle him. It may, however, be aſcertained, in an inaccurate way, what quantity of this beneficial air a plant, placed in a jar full of common air, has communicated to it, by computing the degree of ſuperior goodneſs the air is found to poſſeſs.

As plants yield in a few hours ſuch a conſiderable quantity of dephlogiſticated air, though their ſituation ſeems rather unfavourable for it when they are kept under water; may it not with ſome degree of probability be conjectured, that they yield much more of it when remaining in ther natural ſituation; for then, being continually ſupplied by new common air, their ſtock of dephlogiſticated air cannot be exhauſted. It is an unfavourable circumſtance, that air is not an object of our ſight; if it was, we ſhould perhaps ſee that plants have a kind of reſpiration as animals have; that leaves are the organs of it; that, perhaps, they have pores which abſorb air, and others which throw it out by way of excretion, as are the excretory ducts of animals; that the air ſecreted, being dephlogiſticated air, is thrown out as noxious to the plant (which article is clearly demonſtrated by Dr. Prieſtley and Mr. Sheele); that in the moſt part of plants, principally trees, the greateſt part of inhaling pores are placed upon the upper ſide of the leaf, and the excretory ducts principally on the under ſide.

If theſe conjectures were well grounded, it would throw a great deal of new light upon the arrangement of the different parts of the globe, and the harmony between all its parts would become more conſpicuous. We might find, that partial tempeſts and hurricanes, by ſhaking the air and the waters, produce ſome partial evils for the univerſal benefit of nature; that, by theſe powerful agitations, the ſeptic and noxious particles of the air are blown away, and rendered of no effect, by being thus diluted with the body of air, and partly buried in the waters. We might conceive a little more of the deep deſigns of the Supreme Wiſdom in the different arrangement of ſublunary beings. The ſtubborn atheiſt would, perhaps, find reaſon to humiliate himſelf before that Almighty Being, whoſe exiſtence he denies becauſe his limited ſenſes repreſent to him nothing but a confuſed chaos of miſeries and diſorders in this world.

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SECTION VIII.

Dry plants have very little or no power to affect air; but, when moiſtened, they infect air.

I FILLED a jar with dry hay, another with dry ſtraw, and left it inverted a good while; but could not find the air altered.

I put ſome leaves of a lime tree, dried for the purpoſe, in a jar full of freſh pump water; and placed another jar, filled with an equal number of freſh leaves of the ſame tree, near it in the ſun.

The dry leaves began much later than the others to yield round bubbles juſt in the ſame way, but which did not grow ſo quick, nor ſo large. However, in a few hours, a good deal of air was obtained, but of ſuch a bad quality as to extinguiſh a flame, whereas the freſh leaves had yielded dephlogiſticated air: which experiment ſeems to indicate, that the generation of the dephlogiſticated air is owing to the action of the living plant. The ſame reſult was alſo obtained from dry hay put into a jar full of pump water.

SECTION IX.

All plants poſſeſs a power of correcting, in a few hours, foul air unfit for reſpiration; but only in clear day light, or in the ſun ſhine.

THIS remarkable property of plants is indeed very great; for in a few

few hours, nay even ſometimes in an hour and an half, they purify ſo much a body of air quite unfit for reſpiration, as to be equal in goodneſs to atmoſpheric air. They will even do it when they are incloſed in a glaſs veſſel, without any water. One leaf of a vine, ſhut up in an ounce phial, full of air fouled by breathing ſo that a candle could not burn in it, reſtored this air to the goodneſs of common air in the ſpace of an hour and a half. But plants enjoy this privilege only in the day-time, and when they grow in unſhaded places.

This power of plants extends itſelf even to the worſt of all airs, in which an animal finds his deſtruction in a moment; ſuch as is pure inflammable and highly phlogiſticated air, which is little or ſcarcely at all diminiſhable by nitrous air. I obſerved ſome difference in various kinds of plants in this reſpect, and found that water plants ſeem to poſſeſs this quality in a greater degree than others. The willow tree and the *perſicaria urens* were found eminent in producing this effect: and may it not be providentially ordained it ſhould be ſo, as thoſe plants grow better in marſhy, low grounds, and even in ſtagnated waters, whoſe bottoms are generally muddy, and yield a great deal of inflammable air, which may be collected at the ſurface of the water by ſtirring up the ground, and may be kindled by throwing a burning paper upon the water, which is an amuſing experiment by night? Plants, however, want longer time to correct this kind of air, at leaſt that which is extracted

tracted from metals by vitriolic acid.

The property of plants is demonstrated in experiments 41, 51, 56, 57, 58, 59.

SECTION X.

All plants yield a more or less quantity of dephlogisticated air in the day-time, when growing in the open air, and free from dark shade.

THE quantity of dephlogisticated air, and even the quality of it, which the leaves of plants give, seems to be different in different plants: though, indeed, this may depend in a great measure upon some particular

ticular circumſtances, to which it is not eaſy to be ſufficiently attentive. It ſeems, however, to be a general rule, that the leaves of all plants, growing in a place where they are not much ſhaded by other plants, buildings, &c. yield, in a clear day, dephlogiſticated air; and that this air is yielded in greater abundance, and of a greater purity, when they grow in open places unincumbered by other plants higher than they are themſelves.

I got in general a large quantity of air of a very good quality from ſome water plants, as from the *perſicaria urens* and willow. The fir trees yielded alſo very fine air, and in abundance.

The *naſturtium Indicum* ſurpaſſed them all in general, in regard as well to the quantity as to the quality.

lity. One hundred leaves of this plant, which are very thin, yielded, in two hours time, as much dephlogiſticated air as would fill a cylindrical glaſs four inches and a half deep, and one inch and three quarters diameter; of which quantity gathered again afterwards from the ſame leaves, without taking them out of the water, ſee exp. 30—35. This quantity ſurpaſſes by far the bulk of the leaves themſelves, and ſhews to how amazing a quantity the air may amount yielded in a fair day by a lofty tree.

The leaves being more or leſs crouded together, being expoſed for a longer or ſhorter time, or ſooner or later in the day, will occaſion ſome difference in the quality and quantity of this air.

It

It ſeems that, in general, the fineſt air is obtained when the ſun has paſſed the meridian.

SECTION XI.

The faculty which plants poſſeſs of yielding dephlogiſticated air, of correcting foul air, and improving ordinary air, is not owing to the act of vegetation.

IF this wonderful faculty of plants depended upon their vegetation, they would exert it at all times, and in all places in which the vegetation goes on. A plant may vegetate, and even thrive very well, in the utmoſt darkneſs; and yet in ſuch a place

place it has no power to correct bad air, or to yield good; but, on the contrary, it ſpreads round about it deleterious exhalations, which render the beſt air even pernicious to the utmoſt degree.

It will not be difficult to underſtand now from what cauſe all thoſe different and contrary effects which Dr. Prieſtley has found in his experiments did really depend; and why Mr. Sheele had conſtantly found that the vegetation of beans always ſpoilt good air.

Theſe gentlemen expected the good effects from the vegetation of the plants, as ſuch. By making a plant grow night and day in ordinary air kept in a phial with the plant, the effect will depend upon the greater or leſs expoſure of the plant to the light. Beſides, by keeping

ing a plant a long while in pump water, the green matter, of which Dr. Prieftley found to iffue very fine dephlogifticated air, will be generated; and thus the air within the phial, being mixed with this good air, will not in reality indicate the effect of the plant upon this air, as Dr. Prieftley makes no fcruple to acknowledge in his late work, p. 338.

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SECTION XII.

The plants evaporate by night bad air, and foul the common air which ſurrounds them; yet this is far over-balanced by their beneficial operation in the day.

THE bad air which plants yield by night is ſo inconſiderable in comparifon of the quantity of dephlogiſticated air which they yield by the day-time, that it amounts to very little. By a rough calculation I found, that the poiſonous air yielded during the whole night by any plant could not amount to one-hundredth part of the dephlogiſticated air which the ſame plant yielded in two hours time in a fair day. But, from my experiments, one

one might naturally wonder that no remarkable miſchief happens from ſo many plants as a foreſt contains, if one plant, containing ſcarce a handful, may poiſon to ſuch a degree the quantity of two pints of common air in one night, as to render it abſolutely mortal for any animal who breathes in it.

I make no doubt but a great quantity of plants, kept in a cloſe and ſmall place during a night, or by day in the dark, may do ſome material miſchief, and even occaſion death, to any perſon who ſhould be imprudent enough to remain in ſuch a place. The undoubted facts of people being found dead in their beds, when they had ſlept in a ſmall room with a great deal of flowers in it, muſt inſpire us with a caution againſt keeping too many flowers in

cloſe

close rooms. My experiments go much further, and will, I hope, in future, make people aware of danger if they store up a great deal of fruit in a close room in which they sleep.

I think that nobody before me even suspected the least danger of keeping beans, peaches, or such like fruits, in their rooms; and yet a sufficient number of them would easily poison an unwary person sleeping in a close room, in which an abundance of these fruits is stored.

The gardeners by opening a hot-house early in the morning, which has been shut close during the night, or at any time in the day if the sun has not shined a good deal upon it, are very well aware of a particular oppression they feel by entering it. I remember to have felt it more than once, without even fuſpecting the cauſe of it. Dr. Prieſtley obſerved this remarkable offenſiveneſs of the hot-houſes with a more philoſophical attention; he tried the air within them, and found it worſe than common air.

By all this it ſeems evident, that it would not be ſafe to ſleep in a cloſe hot-houſe; that it would not be prudent to keep too many green branches, fruit, or flowers, in any room by night, particularly in that of a ſick perſon.

The beſt phyſicians have, indeed, often recommended to put green branches of lime-trees and others in the rooms of their patients, without ever ſuſpecting any other effect but benefit from them. I think ſtill, that ſome benefit may ariſe from putting, in a clear day, freſh green branches

branches in the room of a ſick perſon, by cooling the air, which is owing to the evaporation of moiſture; but I ſhould now apprehend rather ſome miſchief from them in a room whoſe doors and windows are ſhut, and which is not well lighted. At any rate, I ſhould no more allow them to be kept in the night-time in the room of any of my own patients.

Is it not ſomewhat probable, that among thoſe people who are found dead in their bed without any previous illneſs, ſome may owe their untimely end to ſome ſuch concealed cauſe, which nobody ever ſuſpected to be in any way dangerous?

But the miſchief which trees in reality do by night-time to the ſurrounding air, cannot do any obſerveable harm to animals: for thofe mifchievous exhalations being, very providentially, fpecifically lighter than common air, rife at the fame time up; and thus the lower region, in which we breathe, is freed from them almoft as foon as they are produced; whereas the dephlogifticated air iffuing out of the plants in great abundance in the day-time is fpecifically heavier than common air, and is therefore inclined to remain longer among us, and to afford us all the benefit for which the Supreme Wifdom has providentially deftined it.

SECTION XIII.

All roots, few excepted, when left out of the ground, yield by day and by night foul air, and infect the surrounding air.

THE experiments I made upon this ſubject convinced me that roots have this deleterious power, and ſome even to ſo great a degree, that it would not be ſafe to remain in a ſmall cloſe room in which a great quantity of roots of plants are kept. The roots of ſome aquatic plants are remarkably apt to foul the air in which they are placed, ſuch as roots of ruſhes, though ever ſo well cleaned from mud and dirt, and the roots of *perſicaria urens*. But I found the roots of *becabunga* almoſt inoffenſive in this reſpect, which I was the leſs ſurprized at, as their ſubſtance is but little different from the ſtalks. The roots of a muſtard plant gave in the ſun-ſhine a good deal of air, when kept under water; which air was worſe than common air, and extinguiſhed a candle. Theſe roots even corrupted good air in the middle of the ſun-ſhine.

A plant, with roots and all, placed in a jar full of water, did, however, yield dephlogiſticated air; ſo that the bad effect of the roots was overbalanced by the plant itſelf.

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SECTION XIV.

Flowers ooze out by day and night an unwholeſome air, and ſpoil, at any time and in every place, a conſiderable body of air in which they are placed.

DR. Prieſtley has obſerved, that a roſe, kept in a glaſs, had, in a ſhort time, ſo much infected the air as to be unfit for reſpiration, and concluded from this very juſtly, that flowers might be hurtful in a room. I have heard more than once of a perſon found dead in a room where a great deal of flowers were kept; and I make no doubt but ſome of theſe caſes are well founded.

I tried all the flowers I could find in my neighbourhood, but cou[ld] not diſcover one which did not yield poiſonous air, though in a ſmall quantity, by day and by night, and which had not the power of rendering quite unfit for reſpiration a very conſiderable body of common air. They even ſeem not to loſe in the leaſt their deleterious influence in the ſun; ſo that I cannot but think that it is unſafe to keep in a cloſe room a large quantity of any flowers, even ſuch as have the moſt delightful ſmell. I am, however, very far from thinking that there is any danger to apprehend from ſuch noſegays as are commonly kept in a room, either for ornament or perfume. The malignant influence which could be expected from ſuch a ſmall quantity of flowers is intirely diſſipated in the maſs of the ſurrounding air; but the exceſs

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muſt not go too far, if the room is cloſely ſhut and but ſmall. If a few flowers of the honey ſuckle (*caprifolium*), which poſſeſs the moſt agreeable ſmell, are able to foul in three hours time, in the middle of the day, a body of air equal to two pints (ſee experiments 65—70) we may judge what dangerous effect might be expected from a large quantity in a cloſe room. Thoſe flowers, like all others, after having rendered truly fatal a body of air, have loſt nothing of their flavour. The air itſelf, which they have poiſoned, is impregnated with the ſame fragrant ſmell as the flowers themſelves; ſo that a perſon, not aware of the concealed poiſon which flowers ſpread round about them, might be eaſily induced by the ſweetneſs of their

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ſcent to run the greateſt hazard of loſing his life, without the ſmalleſt apprehenſion of danger.

SECTION XV.

All fruits in general exhale a deleterious air by day and by night, in the light and in the dark, and poſſeſs a remarkable power of ſpreading a poiſonous quality through the ſurrounding air.

I WAS, indeed, not a little ſurprized to find this effect in even the moſt delicious fruit, ſuch as peaches, grapes, apples, and mulberries. By what I obſerved in my experiments I am apt to think, that the power of fruit,

fruit, at leaſt of ſome, ſurpaſſes the deleterious quality of flowers in the dark; but the influence of the ſun ſeems to check, in ſome degree, this hurtful quality in ſome fruit more than in roots and flowers, of which the moſt part preſerve, even in the brighteſt ſun-ſhine, their virulent effects upon the ambient air in its full force. I found, that one peach was able to render intirely poiſonous, in a few hours, a body of air at leaſt ſix times greater than the ſpace it occupied; and even that they could, in the middle of the ſun-ſhine, render ſuch a quantity of air ſo unwholeſome, that a candle could not burn, nor an animal breathe in it.

After I had obſerved, that all leaves of plants yield dephlogiſticated air by day-light; and conſidering, that in general all leaves are green,

green, and that that ſubſtance which Dr. Prieſtley diſcovered to yield ſo much dephlogiſticated air is of the ſame colour; I had ſome ſuſpicion, that green fruits, ſuch as beans, would alſo yield dephlogiſticated air. I placed, for this purpoſe, ſome French beans in a jar full of pump water, and expoſed it to a bright ſun-ſhine during four hours, and obtained a moderate quantity of air, oozing out of their ſubſtance by bubbles, in the ſame manner as out of leaves. This air was far from being dephlogiſticated air; for it was even worſe than common air, though it approached pretty near it in goodneſs.

I then wanted to ſee what effect theſe green fruits would have upon a body of air in the dark; and I was not a little ſurprized to find, that they had a very remarkable power to foul a great body of air, in which they were ſhut up, to ſuch a degree, that two dozen of ſmall French beans, placed in a jar holding two pints, had rendered in one night the air in the jar abſolutely poiſonous, ſo that a young chicken placed in it was killed in leſs than twenty ſeconds. I found even this deleterious influence of beans upon air to ſurpaſs the power of plants, which are known to be of a poiſonous quality. See experiments 75—91, and principally experiments 88 and 89.

Ripe mulberries, filling one-third of a jar, and placed in the ſun during four hours, had ſo much fouled the air within the jar that a candle would not burn in it.

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SECTION XVI.

The power of plants in correcting bad air is greater than their faculty of improving good air.

THE experiments already known of Dr. Prieftley, by which it appears that plants thrive wonderfully well in air fouled by breathing and burning of candles, gave me a great fufpicion, that the power of plants in correcting bad air might furpafs their faculty of improving good common air. In order to put my conjecture to the trial, I placed at eleven o'clock, in a warm fun-fhine, two jars of an equal fize, each containing an equal quantity of fprigs of pepper-mint, in pump water. In one of thefe jars was let up a certain quantity

tity of common air, whofe teft was at that time fuch, that one meafure of it with one of nitrous air occupied 1.06½. In the other jar was let up the fame quantity of air fouled by refpiration, of which one meafure with one meafure of nitrous air occupied 1.34.

The air of both jars was examined at two o'clock, when I found the common air fo much improved, that one meafure of it with one of nitrous air occupied 1.00. The foul air was fo much mended, that it was near as good as the atmofpheric air, for one meafure of it with one of nitrous air occupied now 1.08½.

I examined both airs again at four o'clock, when the common air was ftill more improved; for one meafure of it with one of nitrous

air

air occupied 0.95. The foul air now was not only become as good as refpirable air, or air of the atmofphere, but even furpaffed it in goodnefs, for one meafure of it with one of nitrous air occupied 1.05.

Now, as the fame plant brought the foul air from 1.34 to 1.05, and the common air from 1.06½ to 0.95, it appears clear, that the plant had corrected the foul air far more than it had improved the common air.

This experiment was repeated feveral times with nearly the fame refults.

As plants feem to delight in foul air, probably becaufe this air impregnated with phlogifton affords more proper nourifhment, *viz.* phlogifton to the plant; it muft of courfe happen, that a plant draws to it fo much the more phlogifton

as

as the air, in which it grows, contains more of this principle.

When a plant grows in the open air, it contaminates by night the ſurrounding air; but this air, being diluted with other air, does not appear in reality to be altered by any method yet found out: beſides, it is probable, that this air is riſen up as ſoon as it was become phlogiſticated by the plant, being ſpecifically lighter than common air. It ſeems therefore not improbable, that ſome plants, as for inſtance the *hyoſcyamus*, may contaminate in reality more air at night than they improve in the day; ſo that, if all the air ſpoiled by ſuch a plant was ſhut up with the plant a whole night and a day, the air would ſtill be found contaminated: but tho' this might be the caſe when the plant is ſhut up with the air, yet it could never be any real dis-advantage in the natural ſituation of things, becauſe this fouled air may be corrected in the atmoſphere by ſome manner or other unknown to us; and, if not, we are, at any rate, immediately out of its reach, as it riſes by its being become lighter. But if ſuch infectious plants are ſhut up in ſmall cloſe rooms, they certainly might do a material injury to our conſtitution, and even occaſion death.

It appears, by experiment 41, that a plant may really foul ſo much air at night as ſcarce to be able to correct in the day. But it is to be conſidered, that ſuch a plant, being maimed by its roots being taken off, and by being ſhut up in a narrow ſpace, muſt have loſt ſome of that vigour which plants naturally have when

when they remain undiſturbed upon their place. See alſo experiments 51, 56, 57, 58, 59, 60.

SECTION XVII.

On the effect of leaving plants kept in a room.

THO' I think, that the keeping of a few plants in rooms is very indifferent as to the health of the perſons who live in them; yet it is not ſo indifferent for us to know the effects which plants have in reality on the air of the room, that we may avoid danger from any exceſs.

The influence of plants on the air of a room in which they are kept is different in the night from what it is in the day. In the day plants are apt to contribute ſomewhat to purify the air of the room, if they are placed ſo as to receive all the light of the ſun poſſible: if they are placed ſo as not to receive the direct influence of the ſun, but to be free from any ſhade, they ſeem to have no influence at all, either in improving the air of the room or in fouling it. But when they are placed in a part of the room the moſt remote from the windows, ſo as to be much ſhaded, they are apt to render the air of the room more or leſs impure, in proportion to their ſize, and to the more or leſs degree of light of the place where they ſtand. At night

they

they abſolutely tend to foul the air, principally when they flower. I acknowledge readily, that a few flower-pots can do neither good nor harm. But I remember to have found ſeveral orange-trees in a room, by way of ornament, and, as I was told, to keep the air of the room wholeſome: I think now ſuch ornamental plants by no means indifferent, unleſs they were but ſmall and the room ample; at any rate I ſhould not ſuffer them to be kept in a room at night, where a ſick perſon is.

A plant ſhut up in a glaſs jar, and placed near the window in a room ſo as to receive the rays of the ſun, will make the air of the jar better than the air of the room: whereas a ſimilar plant, placed in the ſame room in a ſhaded place, will render the air of the jar worſe than the air of the room. If, after a few hours, you invert the experiment, by placing the plant which ſtood at the window in the ſhade, and that which ſtood in the ſhade near the window, the reverſe will take place, *viz.* the air of the jar, which was improved, will be found worſe than the air of the room; and the air of the jar, which had been contaminated, will be found corrected again. And this remarkable property of plants, in the way juſt mentioned, may be demonſtrated in a few hours. See experiment 45.

SECTION XVIII.

Leaves of plants die ſooner when the dephlogiſticated air, elaborated by them, is ſeparated from them.

WHEN the dephlogiſticated air, ſettled in the form of bubbles upon the leaves, is ſhook off, new bubbles ſucceed; and thus by ſhaking off ſeveral times theſe air bubbles a greater quantity of dephlogiſticated air is obtained. The ſecond crop of bubbles contains in general a finer kind of air than the firſt; the reaſon of which may be, that it is ſcarce poſſible to free the ſurface of the leaves entirely from all atmoſpheric air ſticking to them, particularly thoſe which have a rough ſhaggy ſurface ; as, for inſtance, the leaves of ſage, *ſalvia*.

Some of theſe leaves are ſo prolific in puſhing out theſe bubbles, that I have found them reproduced nine or ten times in leaves of a pear-tree. The leaves of a vine are alſo very ready to yield a good number of ſucceſſions of theſe bubbles. But I was curious to ſee whether the leaves decay ſooner or later when the air bubbles were left upon them, or when they were ſhook off now and then: I put a leaf of a vine in a jar full of pump-water, and left it expoſed to the open air without ever ſtirring it: the air bubbles grew to a very large ſize; and ſome of them quitted the leaf of themſelves and roſe up. This leaf remained as freſh as when it was put in the jar during a whole week; whereas another leaf of the ſame vine, placed near it in another jar, and whoſe air bubbles were ſhook off five or ſix times in a day, was withered in leſs than two days. This ſecond leaf had loſt the greateſt part of the rough ſurface, which covers, as a kind of ſcarf-ſkin, the under and unvarniſhed part of the leaf; at leaſt this ſcarf-ſkin became tranſparent, if it was not really deſtroyed; and this tranſparency was obſerved principally upon the very ſpots of the air bubbles. This experiment was repeated ſeveral times with the ſame ſucceſs.

It ſhould ſeem by this obſervation, that the loſs of this air, if it cannot be replaced by the abſorption of new air from the atmoſphere, makes the leaves decay ſooner; and thus the texture of the

the leaves, having no more air to elaborate, reſembles almoſt the organization of an animal, which loſes its life by becoming exhauſted through the loſſes ſuſtained by the increaſe of the various excretions which are carried on in its body, if theſe loſſes are not repaired by taking in new nouriſhment.

Vegetables ſeem to draw the moſt part of their juices from the earth, by their ſpreading roots; and their phlogiſtic matter chiefly from the atmoſphere, from which they abſorb the air as it exiſts. They elaborate this air in the ſubſtance of their leaves, ſeparating from it what is wanted for their own nouriſhment, *viz.* the phlogiſton, and throwing out the remainder, thus deprived of its inflammable principle, as an excrementitious fluid, and in this ſtate

ſtate hurtful to them, but rendered uſeful to the animals, who in their tour take from this air, by the act of reſpiration, what they want, and throw out the remainder as hurtful to them; but rendered again ſerviceable to the vegetables. This theory ſeems to be very reaſonable, and to have ſome foundation in nature. It throws a good deal of light upon the œconomy of nature, and the mutual influence which the vegetable kingdom has upon the animal, and the animal upon the vegetable. It has ſome analogy with other general operations of nature, which are well known.

A plant, which is a living being, deſtitute of motion, remaining upon the ſame ſpot on which it took its beginning, if not capable, as animals are, of going in ſearch of its food, muſt

muſt find within the narrow compaſs of the ſpace it occupies every thing which is wanted for itſelf, and to fulfil the office which has been dictated to it by the Author of nature. It is obliged to ſpread the numberleſs filaments of its root through the ſurrounding ground, as ſo many ſiphons to pump up the juice, which preſents itſelf to thoſe filaments; and theſe filaments are ſufficient to afford all that the greateſt part of trees want in the winter. But, being deſtined in the ſummer-time to more important offices, the tree ſpreads through the air thoſe numberleſs fans, diſpoſing them, in the moſt advantageous manner imaginable, to incumber each other as little as poſſible in pumping from the ſurrounding air all that they can abſorb

ſorb from it, and to preſent, if I may ſo ſpeak, this ſubſtance drawn from the common atmoſphere to the direct rays of the ſun, on purpoſe to receive the benefit which the influence of that great luminary can give it.

SECTION XIX.

On the power which vegetables have of abſorbing different kinds of air.

IF a plant is ſhut up in a certain quantity of air, and all light hindered from falling upon it, it abſorbs in general more air than it yields, and therefore the bulk of air is is found lefs. The quantity of air thus abforbed by plants may vary from numberlefs circumftances, as well as from the particular nature of the plant. I have no time to fearch in my notes for all the particularities I have obferved upon this fubject. I can fay in general from remembrance, that fome water-plants were very willing to abforb a good deal of air, principally when they were placed with roots and all in the air; and that they readily abforbed air fouled by breathing.

One of thofe plants had alfo abforbed a great deal of dephlogifticated air, fo that in one night it had abforbed half the quantity I had put with it, which amounted to 4 ounce meafures, if I rightly remember.

This

This abſorption alſo takes place in the day time; but as the plants at that time yield themſelves a great quantity of air, the abſorption is not ſo eaſily aſcertained.

SECTION XX.

On the beſt manner of judging whether the plants are ready to yield their dephlogiſticated air.

AS the light of the ſun, and not the warmth, is the chief cauſe, if not the only one, which makes the plants yield their dephlogiſticated air, it ſeems reaſonable to think, that in a bright ſun-ſhiny morning the

the plants will be earlier revived in their office than when the ſun is hid by thick clouds. I found this difference to be very remarkable, ſo that in a dark cloudy morning I found the plants to begin their daily operation an hour or two later than in a clearer day. I even found that all the plants in the ſame garden did not awake, if I may ſo expreſs myſelf, at the ſame time from their nocturnal ſtupor. Thoſe plants, whoſe expoſure was ſuch as favoured the rays of the ſun being caſt early upon them, were revived earlier than thoſe which were ſhaded by other plants, a wall, a houſe, &c. Nay, I even found that there was ſome difference in this reſpect between the leaves of the ſame tree; as I found thoſe which were the firſt influenced by the

the light of the ſun, the firſt ready to operate; when thoſe of the oppoſite ſide of the tree, ſhaded by the firſt, were ſtill in their ſtate of ſtupor.

A ready way to know this time exactly is to put a leaf or two, from the plant you are to examine in this reſpect, in a glaſs full of freſh pump-water, and to obſerve, whether the bubbles appear upon them about as quick as they uſe to do in the full day time. If they do, you may be ſure they are fit for the buſineſs.

But there is a readier way to know exactly this article of time, which I found by the water in the jar in which the green matter, diſcovered by Dr. Prieſtley, is already formed. The doctor found that this green matter yielded air bubbles only when placed in the ſun; which obſervation ſerved me as a good index, whether thoſe plants which have experienced nearly the ſame influence of the ſun as the green matter, were fit to yield dephlogiſticated air. The more briſk you ſee theſe bubbles riſe, the quicker your plants will give theirs.

But this manner of judging can only be of ſervice in the morning; for in the middle of the day all leaves of plants, even thoſe which were kept in a very dark place, revive ſo quickly, that they ſeem not to ſtand ſhort in an obſervable way with thoſe which were conſtantly in the open air.

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SECTION XXI.

Conjectures why ſome waters, as diſtilled, boiled, and ſome other waters, do not promote, but impede the operation of the plants in yielding dephlogiſticated air.

AS I think I have proved clearly enough that the dephlogiſticated air yielded by plants is air elaborated by a kind of vital motion, carried on in the ſubſtance of the leaves, and kept up by the influence of the light of the ſun, it ſeems that no more is required to collect this air than to prevent its diffuſing itſelf through the common maſs of the atmoſphere. Water ſeems the moſt appropriated body for ſuch an intention, for it is not hurtful to plants.

Many of them even thrive the beſt in it. The beſt quality required therefore in the water uſed for this purpoſe ſeems to be, to poſſeſs of itſelf air enough, ſo as not to imbibe it readily from the plants; and not ſo much as to be overcharged with it; for if the water is too much deprived of its own air, it muſt be more diſpoſed to abſorbe it from bodies plunged into it. And if water ſhould be ſo much impregnated with any air, this air would readily ruſh into the ſubſtance of the leaves, and ſpoil by its bulk, or by its particular nature, the elaboration of the dephlogiſticated air; the more ſo, as water, when found ſaturated with air, is found to poſſeſs this air in the form of fixed air, which differs too much from the nature of dephlogiſticated air,

or

or atmofpheric air. Befides, water overcharged with air parts eafily with it, which of confequence will of itfelf fettle in the form of bubbles upon the leaves, and thus difturb their whole operation. We know that pump-water poffeffes of itfelf a great portion of air, which is generally thought to be for a part fixed air, to which it owes its agreeably pungent or brifk tafte, which makes it palatable above all other waters. We know with more certainty, that boiled and diftilled water are deprived of the greateft part of their air; and this is perhaps the reafon, why they are not fo palatable as common fpring or pump-water. Therefore it feems to be not quite improbable, that water which has been boiled or diftilled is very apt to abforbe itfelf the air which oozes out of the leaves, and that thus lefs air is gathered at the top of the bottle. This conjecture will perhaps find more ground from the following experiment. I placed fome leaves of a vine in water, which I had, for this experiment, impregnated with fixed air: they were fcarce under the furface of this water, but they were all covered with air bubbles; which feems to me to depend partly upon this water refufing to abforbe any air iffuing from the leaves, becaufe it was already overcharged with air itfelf. It is true that any other body, plunged in water impregnated with fixed air, will alfo become covered with air bubbles; but thefe bubbles do not appear fo foon, or increafe fo rapidly, as thofe of the living leaves. So that it feems, that the bubbles of the leaves increaſe faſter becauſe they are puſhed out of the leaves by a vital motion in the leaf. It is alſo true, that leaves thus placed in water impregnated with fixed air, do not yield that fine dephlogiſticated air which they yield when placed in common pump-water; which may be owing perhaps to the great abundance of fixed air penetrating the leaves, by being abſorbed, and oozing out as it were, in a kind of tumultuary way, together with the air already contained in the leaves. Thus the air iſſuing out of the leaves may not have undergone that degree of elaboration required to change it into dephlogiſticated air: for the leaſt circumſtance may diſturb nature in this work; the ſhade of a building, or of another plant, may change this wonderful operation, ſo as to produce quite the reverſe, and to obtain a poiſonous air inſtead of dephlogiſticated air: for the evaporation of bad air in the dark depends on the vital motion within the plant, which, being not influenced by the light of the ſun, produces a contrary effect. Thus a plant growing in an abſolute darkneſs is without green colour, and fruit without the influence of the light has no flavour.

SECTION XXII.

Some remarks on the green matter which ſettles at the bottom and ſides of the jars in which water is left ſtanding.

THIS green matter, which ſeems to be of the vegetable kind, was firſt found by the Rev. Dr. Prieſtley to yield very pure dephlogiſticated air: but it ceaſes at laſt to yield more air if the water of the jar is not renewed, which ought therefore to be done now and then.

It is wonderful that this matter ſeems to be never exhauſted of yielding dephlogiſticated air, though it has no free communication with the common atmoſphere, from which the moſt part of other plants ſeem to derive their ſtock of air. Does this vegetable matter imbibe the air from the water, and change it into dephlogiſticated air? This does not ſeem to me probable, for I could not obtain from water, even by boiling, ſo much air as the water in which this ſubſtance was produced yielded by itſelf. I ſhould rather incline to believe, that that wonderful power of nature, of changing one ſubſtance into another, and of promoting perpetually that tranſmutation of ſubſtances, which we may obſerve every where, is carried on in this green vegetable matter in a more ample and conſpicuous way. The water itſelf, or ſome ſubſtance in the water, is, as I think, changed into this vegetation, and undergoes, by the influence of the ſun

ſhining

ſhining upon it, in this very ſubſtance or kind of plants, ſuch a *metamorphoſis* as to become what we call now dephlogiſticated air. This real tranſmutation, though wonderful to the eye of a philoſopher, yet is no more extraordinary than the change of graſs and other vegetables into fat within the body of a graminivorous animal, and the production of oil from the watery juice of an olive tree. More examples are to be ſeen of ſuch wonderful tranſmutations of ſublunary beings in the article upon the mutability of air.

On purpoſe to obtain in a ſhort time a great deal of dephlogiſticated air from this green matter, I gathered a good deal of it from the ſides of a ſtone trough placed near a ſpring upon the high road, and

always

always kept full of water for the horſes. I put a good deal of this ſubſtance in a jar holding a gallon of pump-water, and inverted it in an earthen pan. In a week's time I found about 1½ pint of very fine dephlogiſticated air collected in the jar, which ſurpaſſed in purity the air obtained in another jar from the green matter generated by itſelf. See experiment 100.

SECTION XXIII.

In planting trees for rendering the air wholeſomer, it ſeems not to be quite indifferent what kind of trees are made uſe of.

AFTER what is already ſaid on the ſubject, there will be no doubt left, that vegetables have a remarkable ſhare in cleanſing and purifying our atmoſphere. But as it ſeems to follow from my experiments, that ſome trees yield by the day a purer dephlogiſticated air than others, and that ſome ſeem to be leſs diſpoſed to infect common air by night, it can ſcarce be conſidered as a matter entirely indifferent what kind of trees ought to be planted, if if the ſalubrity of the air was the chief object of ſuch a plantation. I made ſome experiments for this purpoſe, of which a few are placed in the ſecond part of this book. But I am far from thinking myſelf intitled to decide any thing upon this head; the more, becauſe all trees co-operate to the ſame end, and becauſe the œconomical advantage ariſing from the preference of one ſort of tree above another may be thought to over-balance the ſmall advantage to be derived from its ſuperiority in rendering the air purer. I muſt content myſelf with the diſcovery of the fact, and leave the reſt to others, who, by farther and more deciſive experiments, may have a better right to decide ſomething upon this head than I can as yet pretend to.

SECTION XXIV.

The largeſt and the more perfect leaves yield more and purer dephlogiſticated air, than thoſe which are not yet full grown.

NOTHING ſeems to me a more convincing proof that the elaboration of dephlogiſticated air is an effect of a kind of vital motion in the texture of the leaves, than that young leaves, not yet grown to their natural ſize, yield their air-bubbles ſlower and leſs in bulk, and that the air yielded by full-grown leaves ſurpaſſes in purity that which is obtained from leaves not yet come to perfection.

It is an amuſing ſight to obſerve in a jar full of pump-water the extremity of a branch of a vine, which contains leaves of different ages, from the matureſt to thoſe which only begin to unfold themſelves. The air-bubbles make firſt their appearance upon the old leaves, then upon thoſe that follow, and laſt of all on the new-born ones. The ſame proportion takes place alſo in the ſize of the bubbles; the largeſt or oldeſt leaves having always the largeſt bubbles, and therefore yielding far the greateſt quantity of dephlogiſticated air.

As it ſeems to be almoſt a conſtant rule, that the leaves which yield the greateſt quantity of air, yield alſo the pureſt; the ſame rule alſo takes place in the old and new leaves. The young leaves ſeem

ſeem not to have their organization compleated for the office to which they are deſtined, and therefore they are not yet able to elaborate ſo much nor ſo good dephlogiſticated air as the old ones. The experiment 122 and 123 ſeem to be deciſive in this reſpect.

SECTION XXV.

Though the diminution of the bulk of nitrous air is believed to be an unqueſtionable teſt of the goodneſs of any air, yet, it muſt be allowed, that in ſome kinds of airs this teſt may fail.

AFTER having tried a great variety of airs myſelf, and after having ſeen many more tried by Abbé Fontana, I no longer made the leaſt doubt, but the diſcovery of Dr. Prieſtley in judging of the exact degree of goodneſs of any air was without any exception. But, as I was reſolved to abſtain as much as poſſible from all analogical conclusions, without they were ſupported by direct experiments, I tried every air I could find, not only by the nitrous teſt, but alſo by the flame of a candle, without, however, harbouring any miſtruſt in the already adopted manner of examining the degree of goodneſs of them.

I had already been convinced, that inflammable air was made explosive in a few hours when expoſed in the ſun with any plant, though I ſometimes found it, by the nitrous teſt, ſo much corrected as to ap-

proach

proach near to the goodneſs of common air. This gave me ſome ſuſpicion, that this inflammable air might be ſuſceptible of a ſtill more remarkable correction or purification, at leaſt in appearance, without loſing its exploſive quality.

On purpoſe to diſcover the whole, I left ſome inflammable air upon *perſicaria*, and ſome upon wallnut leaves, during forty-eight hours, keeping the jars continually in the open air.

I tried firſt the air of the jar in which the wallnut leaves were, in the manner familiar to Dr. Prieſtley and in that of A. Fontana; and repeated each trial twice with the ſame reſult. I found the air by both theſe methods to exhibit all the appearance of air, ſuperior in quality to common air; as may be ſeen in experiments 110, 111. 113, 114, and 115; and yet I found this very air to explode with ſuch a loud report, even in a cylindrical jar, that my ſervant, who kept the glaſs in his hands, thought it was abſolutely broken. This event gave me no ſmall concern for a method of trying the goodneſs of airs, which I had already conſidered as infallible. However, I had ſtill ſome hopes left that I had committed ſome blunder in this experiment; and very luckily I had ſtill at hand the jar which contained the *perſicaria urens* with the inflammable air; but I was ſorry to find that my ſuſpicion was but too well grounded; for this air gave, by two different trials, the following reſult: one meaſure of it with one of nitrous air occupied 0.95; and with two meaſures

meaſures of nitrous air 1.92; by A. Fontana's method it gave 1 90, 1.96, 2.95; and thus it did appear by theſe trials to ſurpaſs far in goodneſs the common air; and yet it exploded at the flame of a candle with an uncommon loud report. See experiments 110 and 111.

There remained ſtill one experiment to be tried with this air, *viz.* to put a living animal in it. I was ſorry to have ſpent the moſt part of this air, ſo as not to have enough of it left for this trial. However, I was reſolved to puſh the experiment farther, and to let the inflammable air ſtand a longer while upon the plants, before it was to be employed for the different trials, and principally before an animal ſhould be put in it. Some entire plants of *perſicaria urens* were put in a gallon jar full of water, and as much ſtrong inflammable air was let up as to fill above one third of the jar. I left it in the garden during ſix days, when I found, to my ſurprize, that it was very far from being corrected; for one meaſure of it, with one of nitrous air, occupied 1.80; it gave the following reſult by Abbé Fontana's method, 2.58, 3.58: a chicken, near three weeks old, died in it in the ſpace of one minute.

This reſult, ſo different from the former, greatly puzzled me, and reſtored my hope that the nitrous teſt was without exception, and that I muſt have committed ſome error in the former experiment.

I was, however, far from giving up entirely my ſuſpicion of the failure of the nitrous teſt. I reſolved to repeat the experiment again, with all poſſible attention; I had ſtill half a pint left of the inflammable air, which had been during ſix days upon the *perſicaria urens* without being much mended. See exp. 112. I put a freſh plant of muſtard in a jar filled with water, and let up this inflammable air in the jar, ſo that the plant was in contact with the air. I placed it in the garden on a Saturday at twelve o'clock. I tried this air the next day between one and two in the afternoon, and found it by the nitrous teſt ſo much mended, that it appeared better than common air, and yet it exploded with a loud report by the approach of a candle. I replaced the jar again in the garden, and put the ſame air again to the nitrous teſt on the Monday following, when it appeared to be be far ſuperior to the atmoſpheric air, for one meaſure of it, with one of nitrous air, occupied 0.46; and yet it exploded as ſtrongly as before. I replaced it again in the garden during four hours more, when it appeared to be ſtill farther improved by the nitrous teſt, without loſing, however, in the leaſt, its exploſive nature. See experiment 115.

I had alſo on the ſame Saturday put ſome plants of *perſicaria urens* with their roots in a jar full of water, and let up two pints of ſtrong inflammable air. I found this air on Sunday, after the jar had been 24 hours in the garden, ſo much corrected, that it approached to the goodneſs of common air by the nitrous teſt, though it exploded with a loud report. I replaced the jar again in the garden, and again ex-

3 amined

amined the air on Monday between one and two o'clock, when it appeared, by the nitrous teſt, about as good as common air; and yet it had not loſt its exploſive quality. After this, I replaced the jar as before, in the garden, and put the ſame air again to the teſt between four and five in the afternoon of the ſame day, when it appeared to be better than common air, without having loſt its exploſive force. There remained now nothing more to be done, than to try the effect of this air upon a living animal. I placed a lively chicken, three weeks old, in a jar filled with this air: it grew ſick directly, and was in ſix minutes near dying, when I took it out quite motionleſs. It remained in the open air during ſeveral minutes in a dying condition, after which it gradually recovered.

I was

I was now thoroughly convinced, that the nitrous teſt failed entirely in ſhewing the degree of ſalubrity of this air; for it appeared by this method to be nearly dephlogiſticated air, and yet it was ſtill a true poiſonous air*.

I was indeed very ſorry to find this failure in a method ſo well adapted for the exploration of atmoſpheric air. But I am very far from thinking that this exception diminiſhes in any way the real value of the important diſcovery, that *Nitrous air diminiſhes reſpirable air in the proportion to its ſalubrity.* For this teſt holds good in atmoſpheric air, which is the chief object of our experiments.

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* Does this air owe its exploſive nature to the dephlogiſticated air oozing out of the plant? But this very air becomes alſo exploſive, though it ſtands with a plant

SECTION XXVI.

Air is one of the moſt changeable ſubſtances in nature, and appearing under very different forms and qualities from a variety of cauſes.

THE air of our atmoſphere is ſeldom during a whole day of the ſame quality. Its degree of wholeſomeneſs is perhaps not leſs ſubject to variations than its weight and its degree of heat and cold. The barometer indicates the firſt, and the thermometer the other. But thoſe two inſtruments ſeem to have no relation to the more or leſs purity of the atmoſphere, or the more or leſs fitneſs of the air for the uſe of reſpiration.

plant during the night, when the plants yield but a very ſmall quantity of bad air. So that it rather appears to be changed by the plant in a kind of ſimple exploſive air, or a true *fulminating air*, the only yet diſcovered, as far as I know.

The

The invention of an *Eudiometer*, or of an inſtrument or contrivance, by which the degree of purity of the common air, or its fitneſs for reſpiration, or rather its wholeſomeneſs, can be inveſtigated juſt as well as its weight, and its degree of heat and cold, is perhaps one of the moſt extraordinary inventions which ever was made.

We owe this important diſcovery to the Rev. Dr. Prieſtley. He found that nitrous air has the ſingular property of diminiſhing, or of being diminiſhed by, common air in proportion to its goodneſs; or that the bulk of the two airs joined together contracts itſelf in a ſo much the narrower ſpace, as the common air is better, purer, or more fit for reſpiration. It will ſoon appear to what a conſiderable degree of accuracy

racy the Abbé Fontana has brought this truly great diſcovery.

We have now in our hands the means of judging, not only of the degree of goodneſs of the common air upon the ſpot, but we may with as much eaſe alſo judge of the quality of the air of any country, by ſending the air of it in cloſe bottles. But as the air upon the ſame ſpot undergoes itſelf continual changes, we can but very ſeldom expect an accurate agreement of two experiments, unleſs made at the ſame time, or unleſs a quantity of the ſame air be ſhut up in a bottle ſufficient for different experiments.

Until accurate inſtruments fit for ſuch purpoſes are generally known, and employed with all the attention required, we ſhall not be able to judge of that degree of goodneſs which

which the air poſſeſſes for the moſt part of the year in a country, and thus to determine the advantages which would arrive to our conſtitution, in ſpending our lives in one country rather than in another, on purpoſe to preſerve a good ſtate of health, to cure particular diſeaſes which require a pure air, or to protract our exiſtence in this world in particular bodily diſpoſitions. We muſt as yet content ourſelves with the amuſement of the experiment.

The continual changes which I obſerved in the atmoſphere daily, by trying its conſtitution, convinced me of the too precipitate judgment of ſome philoſophers, who, though furniſhed with but indifferent inſtruments, have begun already to aſſert the degree of goodneſs of certain places, by one or two obſervations

vations

vations made in the time they paſſed through ſuch a place. But I muſt leave the diſcuſſion of this matter to my reſpectable friend Abbé Fontana, who, in my opinion, has caſt a great light upon this important ſubject; and intends ſoon to publiſh his obſervations on this head. I will add only ſome further reflections upon the changeableneſs of air, its Proteus-like and metaphorical nature.

Since the experiments of the Rev. Dr. *Hales*, we know that air enters the compoſition of bodies, and even ſerves as a kind of cement for the ſtronger coheſion of the conſtituent particles of a ſolid body. By this it ſeems that air may become itſelf a ſolid body, as it conſtitutes ſuch a conſiderable part of ſome particular bodies, ſuch as are, for inſtance,

inſtance, vegetables, calcarious ſtone, nitre, &c. That a fluid body may become a ſolid, is nothing extraordinary; we ſee that water becomes as ſolid as a ſtone, and remains ſo, in a place ſufficiently cold. There are perhaps in the world no ſubſtances which are by their nature *fluid:* for all ſubſtances yet found may be, by different operations, principally by a ſufficient degree of heat, rendered fluid; and all fluids may be changed into ſolid bodies by applying to them a ſufficient degree of cold. Mercury itſelf was rendered as malleable as any other metal, by Profeſſor *Brown* at St. Peterſburg, by a very great degree of cold.

Since that kind of air is known, which goes now under the name of fixed air, and which *Van Helmont*,

mont, called *Gas Sylveſtre*, it has been imagined by many, that different vegetables contain almoſt nothing but fixed air, becauſe they ſaw that as ſoon as they began to ferment they emitted really fixed air. But if from this we conclude that this very ſame vegetable contained this fixed air, as ſuch, concealed in its ſubſtance, and exiſting there, as it were in a concentrated or compreſſed ſtate, almoſt as common air is in a condenſing engine before the fermentation began, we may poſſibly make an erroneous concluſion; for it may be that this vegetable did not contain more fixed air as ſuch than inflammable air; but that a part of the ſubſtance of the vegetable has undergone ſuch a change by the action of the fermentation as to become what is now called fixed air, but what it was very far from being before the fermentation. That this may be the cafe I was induced to fufpect by the following experiments: I fqueezed the air out of different vegetables, keeping them under water, fuch as malva, potatoe-plant, hyofcyamus, apples, &c. I expected to find the moft part of this air fixed air: but I was much difappointed; for this air was not diminifhable by fhaking it in water. By examining it in another way, I found that the flame of a wax-taper would grow dim in it, and that it was only fomewhat inferior in quality to common air; for one meafure of this air drawn from an apple, with one of nitrous air, occupied 1.24; and that expreffed from the leaves of hyofcyamus occupied 1.25. The air expreffed from malva and potatoe-

potatoe-plants appeared to be ſome-what better. This air is undoubtedly the very air of the vegetable unaltered. I placed all thoſe vegetables ſeparately near the fire in water, and by examining the air diſengaged from them I found it to be of a much worſe quality than that which I obtained by ſqueezing; and by trying the air extracted from them by actual ebullition, I found it to be poiſonous, and to extinguiſh flame. The air from an apple obtained by boiling was ſo bad, that one meaſure of it with one of nitrous air occupied 1.71. Now theſe very plants, placed in the ſame water in the ſun-ſhine, yield very fine dephlogiſticated air, and by fermentation they yield fixed air. Is it therefore not probable, that the very air contained in the plant in its natural ſtate was really an air approaching in quality to common air ; and that the heat of the ebullition had changed this very air into phlogiſticated air, in the ſame way as the act of fermentation changes it into fixed air, the light of the ſun into fine dephlogiſticated air, the digeſtion in the ſtomach and the inteſtines of an animal (a great deal of the air contained in the inteſtines, and all that from which we eaſe ourſelves by the rectum, is pure inflammable air) and actual fire applied to it into inflammable air, and the obſcurity of the night into another kind of truly poiſonous air ? Could it be ſaid with any degree of probability, that one and the ſame vegetable contains theſe ſix kinds of air, ſo different in their nature, and even contrary to one another ?

Is

Is it not more reaſonable to ſay that vegetables contain an air, or by whatever name you will pleaſe to call it, which by undergoing different operations changes into different ſorts of air?

Whoever therefore ſays, that ſuch or ſuch ſubſtance contains ſuch or ſuch air, becauſe he extracts ſuch air from it by the action of fire, by fermentation, or by any other means, may ſpeak erroneouſly.

Nitrous acid, or ſpirit of nitre, yields nothing but nitrous air when it is poured upon mercury, copper, iron, &c.; but, when it is mixed with iron filings in a very diluted ſtate, it gives, by the aſſiſtance of a moderate degree of heat, a mixture of different airs, partly fixed, partly common air, and partly phlogiſticated air, (which experiment I ſaw at Abbé Fontana's). When this very acid is joined to ſome earthy ſubſtance, or to a vegetable alkaline ſalt (with which it conſtitutes nitre), it yields by the action of the fire nothing but pure dephlogiſticated air, in ſuch abundance, that the quantity of it is equal to about eight hundred times the bulk of the nitre, as Abbé Fontana found.

Such-like tranſmutations which air ſeems to undergo are every where obvious in nature. All bodies upon our earth, or almoſt all, undergo continually ſome alterations, and at laſt deviate entirely from what they were before. The plant which affords us the moſt wholeſome food is perhaps the next to another which draws out of the ſame ſpot of ground a poiſonous juice. The food by which a viper

lives

lives changes within his body into a fubftance which has nothing deleterious in itfelf, but in one place of its body a moft virulent poifon is elaborated from it. The fame juice which the root of a tree pumps from the earth is changed into various fruits, very different in tafte and qualities, if different fort of fruits are grafted upon it. An animal body becomes a manure for plants by corruption; it changes thus in the fubftance of a vegetable; this, being burnt, changes into afhes; which, by the action of the fame fire, and by the addition of fome fand and fome calx of lead, changes into fine tranfparent glafs. Thus what is now a part of our body may become in a fhort time a part of a pot or bottle.

The three mineral acids themſelves may poſſibly be but one, and the ſame acid modified by ſome particular addition, which time may diſcover, to ſeparate and thus to change marine acid into nitrous acid, &c. Some eminent chymiſts have already aſſerted this as their opinion. More or leſs phlogiſton in one acid than in another may make the one quite different in nature from the other. Common air impregnated with phlogiſton makes a poiſonous air; and common air, deprived of it, makes dephlogiſticated air; in the one an animal dies in a little time; in the other it lives four or five times longer than in common air. Vitriolic acid extracts from iron its phlogiſton, and allows it to impregnate the air diſengaged in the act of ſolution.

Nitrous

Nitrous acid difengages alfo the phlogifton from the iron, but does not allow it to pafs in the air difengaged from it, fo as to make it inflammable. It feems to keep this phlogifton to itfelf; for it is, after the folution, no more to be found in the diffolved iron, when precipitated in the form of ochre; but the fame fpirit of nitre, when diffolving iron in a very diluted ftate, leaves the moft part of the phlogifton with the metal, and rifes in the form of partly fixed air, common air, and what is called phlogifticated air, as was faid above; and by this method iron may be reduced to the moft impalpable powder, all obedient to the magnet, which is a method of making *Æthiops Martialis* of great importance for medical ufes, and was difcovered by an apothecary of

Paris.

Paris. Vitriolic acid extracts from calcareous earth, fixed air, and from some kinds of sparrs an air of a wonderful quality, corroding glass itself, which seems to be almost an incorruptible substance, and reducing it into dust by its contact only; and this air, so active upon glass, is by the first approach of water immediately reduced again into the form of the stone out of which it was extracted.

Considering all what is said before, I incline much to the opinion, that the various kinds of air extracted from the different bodies owe, for a great part, their specific nature to the transmutation which they undergo in the operation by which they are obtained; and that they cannot, at least not all, be said to exist in the body in a contracted

state

ſtate with more propriety than that glaſs exiſts actually in our body, becauſe, by the action of fire, our body may be changed in a conſtituent part of that ſubſtance; and that fat exiſts in graſs and other vegetables, becauſe in the organs of an animal feeding upon theſe herbs they are partly changed into fat. Thus, when we feed upon vegetables, we do not in reality take in fixed air, exiſting as ſuch in the ſubſtance of that food, and only let looſe or extricated in our bowels; but it is more probable, that ſuch food, undergoing in our ſtomach and inteſtines a kind of fermentation, yields really fixed air, not extricated, but generated by the act of fermentation.

As we have ſeen now, that common air is far from being an unalterable fluid, only to be changed by by the addition of ſomething, or by becoming deprived of ſomething extraneous to its own original ſimple nature; we can no more be ſurprized to find, that the conſtitution of the atmoſphere remains ſeldom a whole day the ſame, and that the degree of ſalubrity is continually changing. Indeed, in the courſe of three months, which I ſpent in my ſolitary retirement, I ſcarce found the degree of ſalubrity of the common air juſt the ſame during two days.

Thoſe who are not yet acquainted with the accurateneſs of Abbé Fontana's new *Eudiometer*, will be much inclined to believe, that the appearance of ſuch continual variations is more owing to the imperfection of the method of exploring the air, than to the real changes happening in in our element: and, indeed, I was much of that opinion, till Abbé Fontana convinced me of my error; for, by keeping a bottle full of air taken from the atmoſphere at the ſame time, the conſtitution of it is explored and accurately regiſtered; and examining ſome time afterwards this very air, cloſely ſhut up in a bottle, you find the reſult of the trial to correſpond exactly with the reſult of that which was made at the time when the air was taken from the atmoſphere, and by no means conform with the reſult of the trial inſtituted with the common air of the day, unleſs it ſhould happen that the conſtitution of the atmoſphere was juſt the ſame at both times. I take this to be a demonſtrative proof of the excellence of this method, as well as of the

the erroneous judgement which any body might form of the accurate degree of goodneſs of the air of any given place, by examining it once or twice with nitrous air, principally if the obſervator is not in poſſeſſion of an accurate inſtrument for making ſuch an obſervation, or if he has not obſerved to the greateſt nicety all the manoeuvres in the time of making the experiment.

It would be a difficult taſk to diſcover as yet the true cauſes of that continual fluctuation in the degree of ſalubrity of the air in the ſame place. But it ſeems to me not improbable, that this inconſtancy is to be attributed in general to the natural changeableneſs of the air itſelf, by which it undergoes continual alterations from a variety of cauſes, of which a great

number

number are perhaps not to be traced by human ſagacity; and, indeed, if the air of a vegetable is from the nature of common air, or air approaching it, changed into true poiſonous air, by applying only heat to it, as I have ſaid already, and that ſome more or leſs light to which a plant is expoſed changes its natural air into the moſt ſalubrious or the moſt poiſonous air, may it not be ſuſpected, with ſome degree of reaſon, that a great variety of cauſes, which have been till now overlooked, and which vary themſelves continually, may bring on a very material alteration in our atmoſphere, ſuch as, for inſtance, heat and cold, dryneſs and moiſture, light and obſcurity, which I have already demonſtrated to affect the operation of vegetables upon the air, winds blowing from different quarters, and conveying airs of different qualities, from diſtant countries, and many other operations of nature, unnoticed as yet?

Water itſelf, one of the ſimpleſt and the moſt unalterable ſubſtances known, ſeems to be changeable into dephlogiſticated air, or at leaſt to contain ſome things which may be transformed into this air by the influence of the day-light; for the green vegetable ſubſtance, which ſerves as a kind of laboratory, in which this ſalubrious air is produced, is formed from the water itſelf. Abbé Fontana made a great many experiments tending to examine the air extracted from different waters by heat. I was preſent at theſe experiments in the ſummer of 1777, being then at Paris. He extracted from water of the Seine, and of the aqueduct of Arcueil, an air better than common air, which was a step towards the discovery of still better air from simple water, by some other way not yet hit upon. These interesting experiments are printed in the *Journal de Physique de l'Abbé Rosier*, May 1779.

SECTION XXVII.

On the nature of the air oozing out of our skin.

AS our bodies perspire continually a watery liquid, either in an invisible way by what is called insensible perspiration, or by way of ſweat, ſo a quantity of air ſeems to iſſue continually from the pores of our ſkin. This is eaſily to be obſerved in a cold or warm bath, in which we may clearly ſee whole bubbles of this air riſing upon the ſkin, and at laſt riſe to the top of the water. By plunging the hand and arm even in cold water, we may immediately obſerve a large number of thoſe bubbles every where: and they are the more apparent when the ſkin is thoroughly dry before the part is plunged into the water; and much more ſo when it is plunged precipitately into it.

It is however to be obſerved, that all the air contained in thoſe bubbles, which appear upon the ſkin, when a part of our body thoroughly dry is on a ſudden plunged under the

the ſurface of the water, is not ſuch as really iſſues out of the pores; for, as our ſkin is always covered with ſome unctuous matter which ſeems to repel water, the ſudden immerſion does not allow the water to chace before it all the air ſticking, as it were, to the ſkin, but a good deal of it is left upon it, and forms partly theſe large bubbles. This ſeems to be the more probable, as particularly thoſe places to which theſe bubbles adhere are found quite dry, if obſerved attentively, when the part is withdrawn out of the water. But theſe very bubbles are in all probability alſo partly owing to air oozing out of the ſkin; for, if they were nothing but atmoſpheric air, they would not increaſe in ſize in cold water, but become ſmaller by condenſation : now they increaſe even to a very large ſize in the coldeſt water, and at laſt detach themſelves from the ſkin. A warm bath is not very proper to obtain the air oozing out of our ſkin. Water having been warmed has loſt a good deal of the air naturally contained in it, and thus is very apt to abſorb the air oozing out of the ſkin. The beſt water for this purpoſe is pump-water freſh drawn.

If we keep our arm, or any other part of the body, under water, and rub off all the air bubbles ſticking to the ſkin, we ſhall ſee in a little while a great many ſmall ones ſucceed. But the eaſieſt way to convince one's ſelf of the continual oozing out of air from our ſkin, is to rub the ſkin with the edge of an inverted glaſs full of water, and

long

long enough to keep a good part of it above the ſurface of the water in the time the brim of it is ſliding under water over the ſkin. In this way one may ſee an immenſe number of very minute bubbles riſe continually to the top of the water in the inverted glaſs, and gather in larger bubbles at the top. By this method I collected, in a little time, from my arms, a meaſure of this air, which ſeemed to be partly fixed air, as it was ſomewhat abſorbed by the water; at leaſt, I thought to find the maſs of it always leſs than it was before. This air put to the nitrous teſt was found far from being good reſpirable air; for one meaſure of it, with one of nitrous air occupied, 1.46.

I took a quantity of air in like manner from the arms of a healthy perſon, 19 years old, and found one meaſure of it and one of nitrous air to occupy 1.84; which convinced me that the air evaporating from the ſkin of young people is not purer than that emitted from the ſkin of people more advanced in years; and that if there ſhould be any advantage for old people to ſleep in the ſame bed with young ones, as ſome imagine, it cannot likely be owing to their perſpiring a better and wholeſomer air from their ſkin. It is a very erroneous opinion, and even tending to do material miſchief, that the air of a room, in which a great number of young people have been ſhut up, as in ſchools, is become very wholeſome for old people to breathe in. I have ſeen ſchool-maſters ſo ſtrongly prejudiced with this notion, that they even would not allow the windows of the ſchool to be opened, for fear that the young air, as they called it, of the ſchool-boys ſhould eſcape; thinking that breathing this infectious and truly noxious evaporation would prolong their own life.

As I found that the bubbles appearing upon the ſkin, when a part of our body is plunged under water, are ſo much the larger as the part is put the more precipitately in the fluid, I could ſcarce doubt but the air gathered from theſe large bubbles muſt be for a great part atmoſpheric air, which could not ſo quickly detach itſelf from the ſkin by the ſuddenneſs of the immerſion; and I expected, therefore, that this air would give by the nitrous teſt a better appearance than that gathered from the ſmall bubbles ſcraped from the ſkin by the edge of a glaſs, I gathered from another young and healthy perſon the air of the large bubbles found upon the ſkin, by plunging the arm ſuddenly under the water. And I found it approaching more to the nature of common air, though a candle could not have burned in it, nor an animal breathe in it without anxiety; for one meaſure of it with one of nitrous air occupied 1.40.

CON-

CONCLUSION.

I HOPE the indulgent reader will excuſe in me a ſmall degree of vanity, in flattering myſelf with having diſcovered a law of nature hitherto entirely unknown, and hid till now behind the ſcreen of that awful darkneſs which overcaſts our earth during the time it withdraws its ſurface from the direct influence of that all-reviving luminary, the ſun.

I flatter myſelf alſo to have put beyond all doubt, that the vegetables have a remarkable ſhare in keeping up the ſalubrity of our atmoſphere, by imbibing thoſe ſeptic, noxious, and phlogiſtic particles, which were communicated to it by the breathing of ſo many animals which inhabit the ſurface of the earth, and by many other cauſes; as well as by pouring down a moſt beneficial ſhower of purified or dephlogiſticated air, which, diffuſing itſelf through the maſs of common air, counteracts the general cauſes, tending to contaminate our atmoſphere, and to render it unfit for the uſe of reſpiration. I was lucky enough to diſcover that the *vegetation* itſelf of the plants has nothing to do with the cleanſing our atmoſphere; but that this great work is operated by the influence of the ſun's light, exciting and keeping up the vital and inteſtine motion of theſe numberleſs fans, which the moſt part of plants diſplay at once, juſt at the time when the general

tendency

tendency to corruption is increaſed by the increaſe of heat.

Though we are too much accuſtomed to look upon the moſt obvious operations of nature with a kind of unconcern and indifference, ſuch as, for inſtance, the vegetation of plants; yet we cannot look with ſo much indifference upon the final cauſes of thoſe every where obvious ſcenes when we diſcover them; for they do not ſo much affect the organs of our ſight and other external ſenſes, as they do our underſtanding, our reaſon, our judgment; by which only we are ſuperior to all other living animals. The conſideration of final cauſes gives us to underſtand that this great *univerſe* is not the offspring of chance, not coëval with the beginning of time, or of an eternal origin; but that it

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has been made by an Omnipotent Being, who, by giving it exiſtence, has, at the ſame time, endowed it with moſt wonderful qualities and powers, continually in action, and tending with an aſtoniſhing harmony to one general end, the preſervation of the whole.

An upright mind, averſe to that manner of living which induces many to wiſh, rather than really to believe, that this world is not ſuperintended by an intelligent Being, takes delight in finding out thoſe deep deſigns, which, by their obvious tendency to promote the preſervation of the whole, inſpire him with that awful reverence we owe to the Supreme Cauſe of every thing, and fill him with that conſoling expectation, that the only being upon earth capable of true reaſon, and

of

of tracing the exiſtence of a God in his wonderful works, and of contemplating him in adoration, may expect not to be entirely annihilated after his body is returned into duſt, out of which it took its origin.

But to come back from this digreſſion to the purpoſe, let us conſider how much the real facts drawn from nature itſelf are concordant with the theory deduced from my experiments. If the leaves of vegetables have really a conſiderable ſhare in cleanſing the atmoſphere, it muſt happen, that the time, when our common air is the pureſt, is the ſummer and the winter; for in the ſummer the plants are in their greateſt vigour; and in the middle of the winter the cauſes of general corruption are the moſt

moſt checked by the cold. Now this is juſt what happens. As ſoon as in the advanced autumn the leaves begin to wither and to fall, and to contribute even ſomewhat themſelves to contaminate the air by their corruption, the degree of purity of the atmoſphere is really leſs than it was during the time of the ſummer; and this atmoſphere does not return to its former good quality till the winter is ſet in, and till the remaining tendency to corruption is checked by the increaſe of cold. In the ſpring, when the ſun begins to promote ſomewhat the general tendency to corruption, without having yet influence enough upon the vegetables to make them diſplay their leaves, the common air begins to be leſs fit for reſpiration, till it returns again to its former

mer purity as ſoon as the leaves are produced. And this is what Abbé Fontana found to be a conſtant fact.

If I had more leiſure, I ſhould be inclined to expatiate in a wide and open field of reflections, which preſent themſelves to my mind, and to draw all the conſequences which ſeem to flow, as from a fountain-head, from the already mentioned obſervations.

Is it not probable, that thoſe who labour under conſumptive and aſthmatic complaints, and who find the greateſt relief, and ſometimes a perfect cure, by retiring to mild climates, where vegetation is lively, and begins ſooner in the ſpring, ſhould go to ſuch places where the conſtitution of the air is found by experience to be during the whole

year

year the beſt? But theſe places will not be known till ſome accurate method of examining the goodneſs of common air be in general uſe.

Is it not ſomewhat probable, that it is unſafe for the health of people to ſleep in rooms having windows towards a ſmall open place crouded with the branches of a large tree, ſo hidden from the influence of the ſun as to receive but ſeldom its rays? I remember to have heard people ſay, that it was unwholeſome to ſit under a wallnut-tree, and that they found themſelves affected by its ſhade. But I looked upon ſuch an apprehenſion as one of thoſe popular or vulgar errors which are propagated from father to ſon. I ſhould now be inclined to think, that an apprehenſion of ſome miſchief might not be entirely ill-grounded,

grounded, when ſuch a tree ſtands, as is often the caſe in a narrow yard confined by the ſurrounding buildings.

It is a general belief in the Weſt Indies, founded upon conſtant experience, that the mangeneel-tree *Hippomane Mancinella* (Linn. Spec. Plant. 1431) throws out very hurtful exhalations, ſo as to endanger thoſe people, who, ignorant of the nature of this tree, venture to lay down under it.

The plant called *Lobelia Longiflora*, growing alſo in the Weſt Indies, ſpreads ſuch deleterious exhalations from it, that a conſiderable oppreſſion is felt upon the breaſt in approaching, at ſeveral feet diſtance, this plant, placed in the corner of a hot-houſe or room. (See the deſcription of this plant in *Jacquini Hortus Botanicus Vindobonenſis)*.

The plant called *Dictamnus Albus*, or *Fraxinella*, which is by no means rare throughout almoſt all Europe, when in flower ſpreads round about inflammable air, which, by the approach of a candle by night, flaſhes as other inflammable air does. We know that an animal breathing in this kind of air loſes its life: ſo that if a man was to ſleep with his head in the middle of the branches of this plant, he might run a riſk of being killed by it.

May we not aſcribe the unwholeſomeneſs of the air of that immenſe plain in which *Rome* ſtands to the want of trees and other vegetables? That very plain was, in ancient times, reputed to be a very wholeſome country, when it was well cultivated and

and inhabited. And in our days, being not far from a real defert, it is fo notorious for being unwholefome, that the people of the country think it highly dangerous to pafs a fingle night in it, even in the middle of the fummer; whereas in Tufcany, which is peopled and cultivated to the utmoft, one may fleep the whole fummer in the open air without fearing more injury from it than from the air within the houfe. The Pontine Lake, *Lacus Pontinus*, in the dominions of the pope, in which formerly, when cultivated, were numbers of inhabitants, fupplying Rome with the beft productions of the earth, is at prefent a moft difmal defert, fpreading round about it unwholefome and deadly exhalations, fo that fcarce any living animal can breathe this air without ſoon loſing its health, and finding its deſtruction.

The want of proper cultivation contributes, perhaps, not a little towards rendering the immenſe plains of Hungary leſs wholeſome than they would otherwiſe be. The country round about Vienna is perhaps likewiſe in want of a ſufficient number of trees.

PART

PART THE SECOND.

Containing a ſeries of experiments made with leaves, flowers, fruits, ſtalks, and roots of different plants, on purpoſe to examine the nature of the air they yield of themſelves, and to trace their effects upon common air in different circumſtances.

SECTION I.

Introduction.

BEFORE I proceed to give an account of the various experiments I made during the courſe of

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this ſummer, 1779, I muſt firſt acquaint the reader, that the method which I generally purſued, in putting the different ſpecies of air to the nitrous teſt, was the ſame which the celebrated Abbé Fontana makes uſe of now, and of which he himſelf has not yet given an account to the publick. As I had no right either to claim the invention of his method, or to anticipate the publication of it without his leave, I have aſked his conſent on this head. He agreed to my requeſt very readily, gave me his notes to conſult, and even permitted me to get his inſtruments engraved; for which purpoſe he allowed me to make uſe of his own drawings.

As he had already ſhewn me his method of examining the different kinds of air in regard to their degree of of ſalubrity, or fitneſs for reſpiration, when I was with him at Paris in the beginning of the ſummer of 1777, and as I have, ſince he rejoined me in London, 1778, ſeen a very great number of the like experiments, I provided myſelf with the ſame inſtruments, on purpoſe to imitate his method of examining air, which I found ſo accurate, that, in ten experiments made one after the other with the ſame kind of air, the reſult differed ſeldom above $\frac{1}{500}$; that is to ſay, that the remaining bulk of the three meaſures of nitrous air, which he joins one after another to the two meaſures of atmoſpheric air, is ſo alike in the various experiments made with the ſame common air, that the difference will ſeldom amount to more than $\frac{1}{500}$ of the whole; which accuracy in exploring the degree of goodneſs of reſpirable air ſurpaſſes the exactneſs of judging of the degree of heat and cold by the thermometer of Reaumur.

The Abbé has, ſince I ſaw him at Paris, changed ſomewhat his inſtruments and method of uſing them, or rather corrected them a little; but they remain ſtill materially the ſame as they were before.

I muſt beg the reader to ſtop here, and to caſt his eyes upon the copper plate and the explication of the figures before he proceeds farther.

The new *Eudiometer*, or inſtrument for finding the accurate degree of ſalubrity, or fitneſs for reſpiration of a given air, conſiſts of different pieces; two of which are the

the principal and abſolutely neceſ-ſary. One is a glaſs cylindrical tube, or the *great meaſure*, 18—20 Paris inches long, of an equal bore throughout its whole extent, whoſe diameter muſt be of about $\frac{1}{2}$ inch, or not much leſs, though it may be larger. This glaſs tube has diviſions marked upon it, each of exactly three Paris inches. The inſide of this tube ought to be rubbed with fine emery, to take off the ſmooth ſurface of the glaſs; for, if the ſurface of the glaſs be not a little rough, the water will remain here and there in the form of drops adhering to the inſide of the tube, when air is let up into it; and thus ſo much of the ſpace deſtined for the air is occupied by thoſe drops, which renders the column of air longer than it would have been if the

the water had run down equally along the ſide of the tube; or at leaſt it renders the column of air uncertain in length: each diviſion of this glaſs tube is ſub-divided into 100 equal parts, which are not expreſſed upon the glaſs tube itſelf, but engraved for convenience upon a braſs ſlider or cylinder moving along the glaſs tube. This ſlider muſt be open on both ſides, to ſhew the inſide of the glaſs tube, that the height of the column of water in the tube may be ſeen.

The ſecond neceſſary inſtrument is the *little meaſure*, conſiſting of a glaſs tube of a ſimilar diameter with the great tube, and only three inches long. This ſmall tube muſt alſo be made rough on the inſide with fine emery. This little meaſure is fixed in a braſs ſocket, having a flat ſlider at the orifice of the tube, which, being puſhed in when the tube and the ſocket are full of air, cuts off exactly the column of air within the tube, and at once ſhuts out that quantity of air which is more than the three inches wanted. All that part of the column of air which is thus cut off by the flat ſlider, is let out by turning or inverting the whole meaſure under the ſurface of the water. Thus the quantity of air ſhut up in this little meaſure will be conſtantly the ſame, whatever change may afterwards happen to the expanſive force, or to the elaſticity of the air within this meaſure.

Abbé Fontana uſes this *Eudiometer* in the following manner: he firſt introduces two meaſures of the air

air to be examined, then one measure of nitrous air : at the moment the two airs come into contact with one another, he ſhakes the great tube in the water till both airs are thoroughly mixed together. This being done, the tube is put in the water-trough, in a poſition nearly vertical, to allow time for the water to run down along the inſide of the tube, and to leave the column of air free. He then ſlides the braſs ſcale upon the glaſs tube, till the o, or the mark where the ſub-diviſions begin, correſpond with that ſpot where the two columns of water and air meet. He obſerves with what ſub-diviſion of the ſcale the next mark upon the glaſs tube above the column of water coincides ; which number he writes down. He then lets up another meaſure

meaſure of nitrous air, ſhakes the tube in the moment the two airs come into contact, and, after ſome repoſe, he moves again the o of the braſs ſcale to the place where the columns of air and water meet, and writes down the degree of the ſcale which correſponds with the next mark of the glaſs tube above the water. After this he lets up a third meaſure of nitrous air, and, after ſhaking and repoſing as before, he marks alſo the degree of the ſcale correſponding with the next mark of the glaſs tube above the water, and thus finiſhes the whole operation, if the air examined is common air: for no more diminution of this air would happen if more nitrous air was added, as three meaſures of nitrous air are ſufficient to ſaturate fully two meaſures of any atmoſpheric air; he takes particular care to perform every experiment in the ſame manner, as well in the handling of the inſtruments, as in the exact time, even to a moment, of ſhaking the two airs together, of letting the tube ſtand by, before he examines the number upon the braſs ſcale, &c.

After the whole operation is finiſhed, he deducts the number of the ſub-diviſions of the whole column of air remaining in the tube from the number of all the ſub-diviſions or parts of both airs which were let up; and the reſult gives exactly the number of parts or ſub-diviſions which were deſtroyed: for inſtance, if, after the third meaſure of nitrous air being let up, the next mark of the tube correſponds

with

with the number 8 upon the ſcale, and if above this mark are remaining three entire diviſions of 100 partitions each of the column of air, the quantity of air exiſting in the tube amounts to 308 ſub-diviſions, which being ſubtracted from the 5 meaſures of both airs employed, or from the 500 ſub-diviſions of both airs, the remainder will be 192, which is the exact number of the parts or ſub-diviſions of the two airs deſtroyed.

If the air to be examined is dephlogiſticated air, he continues letting up one meaſure of nitrous air after another in the manner mentioned, till no more diminution takes place. Six, ſeven, and ſometimes eight meaſures of nitrous air are required to ſaturate two meaſures of dephlo-

dephlogisticated air if it be very pure.

What has been already said of this method of putting different airs to the nitrous test will be sufficient, I hope, to guide the reader in imitating it. But he will find in the result of every trial, made with the same species of air, so much difference, that he would mistrust the whole method if he did not observe every minute circumstance in the course of the whole experiment. It has cost the Abbé some years assiduous labour before he reduced this method to that degree of accuracy which it has now acquired in his hands.

Those who wish to perform this amusing experiment themselves will think it worth their while to look over the following necessary cautions

cautions to be obſerved, which I extracted from the manuſcript of the author.

He reduces the various ſources from which errors may ariſe in this manner of exploring air to twenty; which are, however, not all of equal importance, and may counterbalance in ſome meaſure one another, ſo that one error may correct another. But ſome of theſe are of ſuch importance, that by overlooking them it may happen that the beſt atmoſpheric air ſhall appear to be a true poiſonous one.

Thoſe errors may originate principally either from the great tube or meaſure, or from the little meaſure.

The errors which may originate from the ſmall meaſure are ſeven:

I. The firſt error may be committed by the hand, which, by touching this tube in the time of filling it with air, may expand this air by communicating its heat to it. The reſult of this error may amount to two ſub-diviſions.

II. The ſecond error may be committed alſo by the warmth of the hand in which this meaſure is kept, when it is raiſed till the flat ſlider is on a level with the ſurface of the water, in the moment it is puſhed in to cut the column of air within the meaſure from the air to be ſhut out. This error may alſo amount to two ſub-diviſions.

III. The third error may be committed by not keeping the meaſure, in the moment of ſhutting the ſlider, exactly at the height required; for, if the water within and without the

the meaſure be not on a level, the column of air within the meaſure may be more or leſs compreſſed. This error may amount to four ſub-diviſions.

IV. The fourth error may depend upon the inſide of this tube not being made rough by emery; for water ſettles in the form of drops on the ſmooth ſurface of glaſs. Theſe drops adhering here and there to the inſide of this tube render its capacity greater or ſmaller. This error may amount to at leaſt three ſub-diviſions.

V. The fifth error depending on this meaſure may be owing to the difference of time between the filling this tube with air and the ſhutting the flat ſlider: for after the air is let up into this tube the water runs down its ſide for ſome time; ſo that the longer the interval between filling it with air and puſhing in the ſlider is, the more the inſide of this tube is cleared from water, and thus the more air it will contain. This error may alſo amount to three ſub-diviſions.

VI. The ſixth error, which may be committed by the ſmall tube or meaſure, is indeed remedied in the meaſure in-uſe by our author by the flat ſlider; but it remains in the meaſure which is ſtill uſed by other philoſophers, which conſiſts only in a common phial not provided with ſuch a ſlider. The error reſulting from the want of this ſlider may amount to ten, and even more ſub-diviſions.

VII. The ſeventh error may depend upon the difference in the diameter of the ſmall meaſure compared

pared with the diameter of the large one; by which difference it may happen, that the dilation of the air within becomes greater or leſs by warmth, as the ſubſtance of the glaſs be thicker or thinner, and that the capacity of the tube itſelf varies for this reaſon. The difference of the reſult, however, can be but very ſmall from this cauſe.

Thus, by computing the number of ſub-diviſions to which theſe ſeven errors may amount, we find them to be 25. But, as all the five meaſures of airs are let up in the large tube one after another, theſe errors may, if they were all committed, amount to five times this number, or to 125 ſub-diviſions.

The miſtakes depending from the great tube, or meaſure, may alſo amount to ſeven heads.

I. The firſt ſource of error may depend on the inequality in the diameter of this tube, by which a difference of four ſub-diviſions may eaſily reſult in each partition.

II. The ſecond error may depend on the tube not being made rough on the inſide, from which a difference of ſix ſub-diviſions may happen.

III. The third may be owing to the degree of expanſion of the air communicated by the hand in the time this tube is examined to obſerve the length of the column of air. This difference may amount to four ſub-diviſions.

IV. The fourth may conſiſt in obſerving the height of the column of water within the tube, when the water within is not on a level with the water without; by which an error of three ſub-diviſions may be committed.

V. The fifth error may depend on the difference of time between the letting up each meaſure and examining the column. From this head a difference of 10 ſub-diviſions may enſue.

VI. The ſixth may be in determining inaccurately the length of the column of air in the tube, which may amount to five ſub-diviſions.

VII. The ſeventh error may depend on the tube being kept in a direction more or leſs perpendicular, which may amount to three ſub-diviſions. All thoſe errors reſulting from the great tube make together 35, and amount, in the three meaſures of nitrous air let up in one experiment, to the number of 105 ſub-diviſions.

Beſides the ſources of errors already mentioned, there may happen ſome others from accidental circumſtances, which may be principally three.

I. The degree of heat of the common air may change during the time you make your experiment, and may occaſion a greater or leſs extenſion of the column of air in the great tube.

II. The weight of the air, or its preſſure, may alſo change in the interval of inſtituting the experiment.

III. The difference of heat communicated to the tube by the body of the obſervator himſelf in the time he is near it to make the obſervation.

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Theſe three accidental errors, though ſmall in themſelves, yet may amount to ſix or more ſub-diviſions. So that all the errors already enumerated may amount, if they were all committed together, to 260 ſub-diviſions.

Beſides all the above-mentioned ſources of errors that may be committed, either by indifferent inſtruments, or by want of proper attention, there is one which had always vexed me in former times, and which ſeemed to me, as well as to many others, almoſt incorrigible. This is the inconſtancy in the quality of the nitrous air, which is found ſometimes much ſtronger or weaker than at other times, though the ſame method of producing it has been obſerved.

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Of all the metallic ſubſtances, mercury ſeems to be the beſt to obtain nitrous air of a conſtant quality; but heat muſt be applied, if a large quantity is required in a ſhort time. I made uſe, a long while ago, of pin-duſt, of which a ſmall quantity, put in diluted ſpirit of nitre, yields all on a ſudden a large quantity of nitrous air of an equal degree of ſtrength: but as the ſolution is very tumultuous, and a great deal of the pin-duſt, together with the nitrous acid, is apt to ruſh out of the phial, I found it at laſt better to uſe common copper. I coil ſtrong copper wire, neeled ſo as to be flexible, up in ſmall curls, and fill the phial with them. Thus the nitrous acid, diluted with five or ſix times its quantity of water being poured in it, finds a large

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and always about an equal ſurface of the metal expoſed to its action, and yields in a ſhort time a large quantity of nitrous air very conſtant in quality. Braſs ſeems to me to give nitrous air of a much more inconſtant quality. Inſtead of a glaſs bottle, I often uſe an elaſtic gum bottle, or *caoutchouc*, and, inſtead of a bended glaſs tube, I take one made of the ſame elaſtic gum. Such tube is eaſily made by coiling up pieces of caoutchouc bottles in the form of tubes, and ſticking them together by their extremities. This wonderful ſubſtance poſſeſſes a ſtrong power of attraction for itſelf, ſo that two pieces cut with a ſharp inſtrument will adhere ſtrongly together, if joined before the cut and ſmooth edges have been touched by the fingers, or before they are ſoiled

in

in any way. To the extremity of ſuch a tube I adapt a hollow glaſs ſtopper of a conical form, ſo as to fit almoſt all bottles. A braſs ring forced over the neck of the gum bottle preſſes its ſubſtance againſt the glaſs ſtopper, and prevents the nitrous air ruſhing out.

It is to be obſerved, that nitrous acid will at laſt deſtroy the elaſtic gum bottles by making its inſide brittle, eſpecially if the acid is very concentrated.

Though good nitrous air may be obtained by many ways, yet this air loſes gradually its ſtrength, and in a few days, if in contact with water, becomes very much weakened; ſo that it muſt be either made new almoſt every day, or we cannot be ſure of the reſult of the experiment.

The method of Abbé Fontana in putting the different ſpecies of air to the nitrous teſt, cut ſhort to the whole difficulty ariſing from the inconſtancy of ſtrength in the nitrous air. By over-ſaturating the air to be examined with nitrous air, it imports little what ſtrength nitrous air has, though even it had loſt almoſt its whole power of abſorbing common air.

In the method adopted by other philoſophers, by which always a certain proportion of nitrous air is added at once to a certain quantity of the air under examination, the reſult is very uncertain if the nitrous air be not exactly always of the ſame quality. But in the method of Abbé Fontana this article is of no conſequence at all.. The only difference ariſing from weak nitrous

nitrous air in this method is, that more meaſures of it are required before the ſaturation of the air to be examined is compleated.

The reaſon of this will appear obvious, if we conſider that it is only the true nitrous air which is capable of diminiſhing reſpirable airs, and that it performs this diminution in the proportion to its ſtrength, ſo that weak nitrous air will always diminiſh common air in the proportion of its own good or bad quality. Now I will ſuppoſe that the nitrous air, to be added to the two meaſures of common air, is become ſo weak, either by ſtanding, or by the admiſſion of any other air, as to poſſeſs only half the ſtrength of good nitrous air. The conſequence will be, that as much again cf it will be required to ſaturate

turate the two meaſures of common air; and thus, after the ſaturation of the two meaſures of common air is compleated, there will remain in the great meaſure, or tube, a column of air ſo much the longer as the nitrous air employed was the weaker. I will illuſtrate it with an example: let us ſuppoſe, that after the three meaſures of ſtrong nitrous air are let up, and the ſaturation of the two meaſures of the air under examination be compleated, the remaining column of air be found equivalent to three meaſures, and eight ſub-diviſions, or to 308 ſub-diviſions; this number, ſubtracted from the 500 parts or ſub-diviſions of both airs employed, will give a reſult of 192, which is exactly the quantity of both airs deſtroyed. Let us now again

again ſuppoſe, that the nitrous air employed was ſo weak, that, inſtead of three meaſures, ſix were required before the ſaturation was fully compleated, and that thus the remaining column of air in the great tube occupies 608, inſtead of 308, ſubdiviſions; we ſhall find that the reſult will be juſt the ſame; that is to ſay, that, by ſubtracting the 608 parts remaining from the 800 parts of both airs employed in the experiment, there will be found exactly 192 ſub-diviſions deſtroyed; and that thus in both caſes the accurate ſalubrity of the air is aſcertained. If ſuch bad nitrous air was only at hand as was juſt now ſuppoſed, it follows, that a longer tube ought to be employed.

This obſervation, which I owe entirely to Abbé Fontana, is in my opinion

opinion of the utmoſt conſequence, and throws a great deal of light upon the nature of nitrous air, and upon its wonderful property of deſtroying reſpirable air; and it illuſtrates his ingenious theory of this quality, which, I hope, the author will ſoon publiſh, but which I have no right either to claim or to anticipate.

In conſequence of this obſervation we need not be ſo anxious about the goodneſs of the nitrous acid, nor about the ſtrength of the nitrous air.

We have now already examined eighteen different heads, from which errors or differences in this important experiment may ariſe, which may render the whole reſult of the teſt fallacious and entirely uncertain.

 There

There are two other great ſources of errors that may be committed in the examination of any reſpirable air by the nitrous teſt, which Abbé Fontana has alſo pointed out and corrected.

The firſt of theſe two reſides in the act of mixing the two airs. If the nitrous air is let up into the large tube, in which was already the air to be examined, and the tube put by for a while without ſhaking it, to give time to the two airs to incorporate with one another; or if the two airs are firſt put in a ſeparate veſſel before they are let up into the large tube; there will ſcarcely ever be two experiments correſponding with one another; the difference will be ſo great, and the reſult ſo uncertain, that it may amount in one experiment to an error of fifty

fifty ſubdiviſions, which, being added to the above-mentioned 256, will make their number in all 306 ſubdiviſions; the difference of time between the moment of the mixture of both airs and the examination will make alſo a conſiderable difference; even a few ſeconds may make a ſenſible difference, if the examination be intended to be finiſhed in a little while; but if the two airs after their mixture are left to ſtand by themſelves a long while, as for inſtance 24 hours, the whole reſult will be uncertain, particularly if the degree of goodneſs of dephlogiſticated air is to be examined.

The remaining ſource of error conſiſts in letting up at once all the quantity of nitrous air to be employed in the experiment. The uncertainty of the reſult ariſing from this head is ſo much the greater when dephlogiſticated air is under examination.

I have now pointed out twenty ſources of errors, which may be committed in the examination of the ſalubrity of different airs, which indeed it would be impoſſible to commit all in one and the ſame trials, but of which ſome are continually committed in the methods hitherto in uſe, and which render the accuracy of the teſt as uncertain as the experiment itſelf is: however, I readily acknowledge, that even an accurate philoſopher, provided with the ſame inſtruments which the Abbé uſes, will find it difficult in the beginning to make two experiments with ſuch nicety as not to obtain a difference of ſome few ſubdiviſions; but he will by a

little

little practice ſoon be convinced, that by this method the degree of ſalubrity of any air may be aſcertained with as much exactneſs as the degree of heat and cold by the thermometer of Reaumur; for the difference of the reſult will ſcarce exceed $\frac{1}{500}$ of the two airs employed, and it would be difficult to judge upon the thermometer of Reaumur of $\frac{1}{6}$ of a degree of his ſcale.

Though I have for the moſt part made all my experiments according to the preſent method of Mr. Fontana, as I was not in poſſeſſion of a tube long enough to put the various airs to the teſt, according to the original method of the Rev. Dr. Prieſtley, with as much accuracy as I could have wiſhed, yet I made a great many trials in a way not eſſentially different from his, by letting up one meaſure of common air with one of nitrous air, and ſhaking the tube immediately when the two airs came in contact with one another. In examining the dephlogiſticated airs, I joined two meaſures of nitrous airs to one of dephlogiſticated air, ſhaking them in the ſame way as in the former caſe; but, as theſe two meaſures of nitrous air did not always compleat fully the ſaturation of this kind of air, I added another meaſure of nitrous air to it, and ſo on till no farther diminution took place.

I have been careful to expreſs the two different teſts in my experiments chiefly in the firſt ſection, ſo that the reader, who is already initiated in the method of Dr. Prieſtley, may find how far the reſult of my experiments will be concordant with the refult of his own.

When I had an air worfe than common air to put to the teft, I found Dr. Prieftley's method extremely convenient, by adding one meafure of nitrous air to one of the air under examination; but I always took care to fhake the two airs together at the moment of their mutual contact, on purpofe to abridge the experiment.

The principal thing to be attended to in putting air to the nitrous teft, either by the original method of the Rev. Dr. Prieftley, or by any other, is to perform with the greateft nicety every circumftance conftantly in the fame manner in the various experiments; for, if you allow the two airs to ftand longer one time than another, if you begin to ſhake them both together ſooner or later, if you continue ſhaking them for a longer or ſhorter time in one experiment than in another, if you caſt up the account by obſerving the degree of the meaſure, or of the braſs ſcale, ſooner or later, &c. you will find in every experiment very conſiderable variations.

SECTION II.

Experiments ſhewing in general the degree of goodneſs or purity of the dephlogiſticated air which the leaves of plants yield in the ſun-ſhine.

Exp. I. TWO handfuls of *graſs*, the roots being cut off, were put in an inverted jar holding a gallon, filled with pump-water, in the ſun between eleven and two o'clock; a great quantity of dephlogiſticated air was ſettled at the inverted bottom of the jar, in which the flame of a wax-taper became very brilliant. By the teſt of nitrous air, according to the preſent method of Abbé Fontana, the reſult was as follows: two meaſures of it being let up in the glaſs

Quantity of the two airs deſtroyed.

Quantity of the two airs deſtroyed.

glaſs tube, and one meaſure of nitrous air joined to it, the mark ſtood after ſhaking and repoſing at 1.92; a ſecond meaſure being added, it ſtood at 1.79; after a third meaſure a $1.68\frac{1}{2}$; after a fourth meaſure at $1.87\frac{1}{2}$; after a fifth meaſure at
415 2.85.

By trying it in the other way familiar to Dr. *Prieſtley*, the reſult was as follows: one meaſure of it, with two meaſures of nitrous air, occupied 0.88.

By puſhing this manner of trial farther, as I do, the reſult was this: by adding to the two former meaſures of nitrous air a third one, the mark was 1.83; by adding a
319 fourth, it marked 2.81. Thus it appears that by this laſt method the quantity of both airs deſtroyed amounts

amounts to 96 ſub-diviſions leſs than by *Abbé Fontana's* method.

Quantity of the two airs deſtroyed.

2. Two handfuls of leaves of a *willow* tree were put in the ſame way in the ſun-ſhine, between eleven and two o'clock; the dephlogiſticated air obtained gave, by the nitrous teſt of Abbé Fontana, the following reſult:

1.96; $1.83\frac{1}{2}$; 1.71; 1.64; 2.55. 445

By the other teſt it gave the following reſult: one meaſure of this air with two of nitrous air occupied 0.85; with three 1.75; with four 2.72. 328

3. Two handfuls of *Lamium Album* were expoſed in the ſame way to the ſun-ſhine, from ten in the morning till two in the afternoon. A great quantity of dephlogiſticated air was obtained of a fine quality;

Quantity of the two airs destroyed.

quality; it gave, by Abbé Fontana's test, 1.90; 1.73½; 1.53½; 1.39;
467 2.33.

One measure of it with two of nitrous air occupied 0.98; with
340 three 1.60; with four 2.60.

4. Two handfuls of leaves of a *vine* were exposed in the same way in the sun-shine, between eleven and one; a good deal of dephlogisticated air was obtained, which gave, by the test,
415 1.92; 1.79; 1.61½; 1.87; 2.85.

One measure of it with two of nitrous air occupied 0.85; with
319 three 1.83; with four 2.81.

5. One handful of *Becabunga*, the roots being cut off, was exposed in the same way to the open sun-shine, between twelve and four in the afternoon; a great quantity of dephlogisticated air was obtained of

a remarkable fine quality, in which the flame of a wax-taper burned with the most beautiful brilliancy. It gave, by Abbé Fontana's test, 1.87½; 1.73; 1.54½; 1.37; 2.01; 3.00. 500

Quantity of the two airs destroyed.

One measure of it with two of nitrous air occupied 0.94; with three 1.37; with four 2.33. 367

6. A plant of a moderate size of common *thistle*, just before it flowered, was put in the same manner in the sun-shine, from eleven till two o'clock; much dephlogisticated air was obtained of a pure quality; it gave, by Abbé Fontana's test, 1.81; 1.51; 1.36; 1.60; 2.60. 440

One measure of it with two of nitrous air occupied 0.65; with three 1.67; with four 2.79. 321

7. Two handfuls of leaves of French *Beans* were in the same way exposed

Quantity of the two airs deſtroyed.

expoſed to the ſun during ſix hours; a great quantity of very fine dephlogiſticated air was obtained, in which a flame became very brilliant; it gave, by Abbé Fontana's teſt, 2.02;
504 1.92; 1.89½; 1 85; 2.01; 2.96.

One meaſure of it with two of nitrous air occupied 0.90; with three
345 1.55.

8. Two little plants of *Teucrium Marum* were expoſed in the ſame manner, during four hours, to the ſun-ſhine; they yielded a great quantity of dephlogiſticated air of a fine quality; it gave, by Abbé Fontana's teſt, 1.81; 1.59; 1.37; 1.34;
466 2.34.

One meaſure of it with two of nitrous air occupied 0.60; with three
344 1.59; with four 2.56.

9. Some leaves of *Tobacco* were in the ſame manner expoſed to the ſun

ſun during four hours; a great deal of dephlogiſticated air was obtained, which gave, by Abbé Fontana's teſt, 2.07; 2.06; 2.05; 2.41; 3.39. 361

Quantity of the two airs deſtroyed.

10. Some leaves of *Cyſtus Ladanifera*, a very fragrant plant, were expoſed to the ſun in the ſame way during four hours; a great quantity of fine dephlogiſticated air was obtained; which gave, by Abbé Fontana's teſt, 1.89; 1.72; 1.56; 1.92; 2.90. 400

11. Leaves of *Juniperus Virginiana*, treated in the ſame way, yielded a large quantity of fine dephlogiſticated air, whoſe teſt was 1.91; 1.75; 1.60; 1.79; 2.79. 421

12. Leaves of *Laurus Camphorata*, the camphire-tree, treated in the ſame way, yielded a good quantity of very fine dephlogiſticated air; which

Quantity of the two airs deſtroyed.

which gave, by Abbé Fontana's teſt, 2.01; 1.90; 1.78; 1.73; 1.76; 2.56; 3.52.
548

13. Some branches of *Cedar of Lebanon* were expoſed in the ſame manner, from nine in the morning till two in the afternoon, in the ſunſhine; a great quantity of dephlogiſticated air was obtained; whoſe teſt, by Abbé Fontana's method, was 1.95; 1.77; 1.64; 1.51; 2.25;
477 3.23.

14. Some branches of *Artemiſia Pontica* were treated in the ſame way; a great quantity of very fine dephlogiſticated air was obtained; whoſe teſt, by Abbé Fontana's method, was 2.00; 1.95; 1.85; 1.79;
454 2.46; 3.46.

One meaſure of it with two of nitrous air occupied 0.92; with three
337 1.63.

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SECTION II.

Experiments, ſhewing the difference in the purity of the dephlogiſticated air, which the leaves of the ſame plant give at different times.

15. Leaves of an *apple-tree* in an inverted jar full of water were expoſed to the ſun-ſhine from ten in the morning till five in the afternoon; the teſt of the air was, 1.80; 1.58; 1.39; 1.86; 2.79. 421

Quantity of the two airs deſtroyed.

16. Leaves of the ſame tree expoſed during the ſame time in the ſun-ſhine in a green bottle, 1.82; 1.62; 1.71; 2.64; 3.64. 336

17. The ſame leaves, which were employed the day before in experiment 15, were expoſed again with freſh pump-water in the ſun-

 ſhine;

Quantity of the two airs destroyed.

ſhine; the air obtained was remarkably fine, 1.85; 1.69; 1.54;
556 1.38; 1.58; 2.49; 3.44.

18. Leaves of the ſame tree expoſed to the open air in a very dark and cloudy day, from five in the afternoon till ſix, had yielded but a ſmall quantity of air, which was worſe than common air, 1.84;
164 2.36; 3.36.

19. Leaves of the ſame tree expoſed in the ſun from nine in the morning till twelve, 1.89; 1.71;
417 1.52; 1.60; 2.60.

20. Air from the leaves of a *willow-tree* treated in the ſame way as in experiment 1, expoſed in the ſun between twelve and two, 2.00;
457 2.12; 2.19; 2.41; 3.43.

21. Air from the ſame tree, gathered in a dark, rainy, and windy day,

day, proved to be of an inferior quality, 1.89; 1.71; 1.55; 2.26; 3.26. Quantity of the two airs destroyed. 374

22. Air from the ſame leaves gathered between nine and three in a fine warm ſun-ſhine, 1.90; 1.72; 1.53; 2.22; 3.22. 378

N. B. I think the reaſon of the inferior quality of this air to be, that the leaves were too much crouded in the jar, and that thus too many were deprived from light, being ſhaded by the others.

23. Air of the ſame leaves collected in a fine ſun-ſhine between twelve and 5 o'clock, 1.90; 1.71; 1.49; 1.53; 2.52. 467

24. Air of the ſame leaves gathered in a fine ſun-ſhine between two and five, 1.92½; 1.80; 1.62; 1.60; 2.40; 3.35. 465

Quantity of the two airs destroyed.

25. Air of the ſame, gathered in a warm ſun-ſhine between three and five, 1.94; 1.79; 1.63;
401 2.02; 2.99.

SECTION III.

Experiments tending to inveſtigate at what time of the day plants yield the beſt dephlogiſticated air.

26. THREE jars, each containing a gallon, were filled with pump-water, and two handfuls of *willow* leaves put in each; they were all expoſed at eleven o' clock near one another in a fair ſun-ſhine.

The

Quantity of the two airs deſtroyed.

The air of the firſt jar was examined at half an hour paſt two in the afternoon; its teſt was 2.03; 2.05½; 2.03½; 2.02; 2.54; 3.50. 450

The air of the ſecond jar was examined between four and five o' clock; its teſt was 2.06; 2.07½; 2.06; 2.02; 2.08; 3.03. 497

The air of the third jar was examined between ſix and ſeven; its teſt was 2.12½; 2.16½; 2.14; 2.12; 2.56; 3.50. 550

27. Three jars, of a gallon each, full of pump-water, were expoſed at ten in the morning in the ſun, the weather being agreeably warm; each jar contained two handfuls of the leaves of an *Elm-tree*.

The air of the firſt jar was put to the teſt at two in the afternoon; the reſult was 1.90; 1.81; 1.76; 2.66; 3.66. 337

Quantity of the two airs deſtroyed.

The air of the ſecond jar was put to the teſt at four; the reſult was
377 1.91; 1.77; 1.65; 2.19; 3.23.

The air of the third jar was examined between ſix and ſeven; its teſt gave 1.97; 1.93; 1.85; 2.16;
388 3.12.

28. Two jars, of a gallon each, full of pump-water, were expoſed in a very agreeable warm day in the ſun at ten in the morning; each contained two handfuls of *willow* leaves.

The air of the firſt jar was put to the teſt at three in the afternoon; the reſult was 1.10; 2.09;
515 2.08; 2.02; 2.06; 2.97; 3.85.

The air of the ſecond jar was examined at five o'clock; it gave by the teſt 2.09; 2.11; 2.07;
535 2.08; 2.29½; 2.78; 3.65.

29. Three

29. Three jars as above were expofed to warm fun-fhine between eleven and twelve; in each were put two handfuls of leaves of an *Elm-tree*.

Quantity of the two airs deftroyed.

At three in the afternoon the air of the firft jar was put to the teft; it gave 1.91½; 1.93; 1.81; 2.10; 3.10. 390

The fecond jar was examined at five o'clock, the air gave 1.88; 1.67; 1.67; 2.65; 3.65. 335

The third jar was examined at fix; its air gave 1.97; 1.88; 1.84; 2.57; 3.54. 346

N. B. On the day this experiment was made it was a very warm fun-fhine from eight in the morning till two in the afternoon, when the weather began to be more and more dark; we had at three a thunder ftorm; and after the fky was always

 cloudy,

Quantity of the two airs destroyed. cloudy, though it continued to be warm weather.

SECTION IV.

Experiments tending to inveſtigate what quantity of dephlogiſticated air a certain number of leaves yield.

30. ONE hundred leaves of the *Naſturtium Indicum* were put in a jar holding a gallon, filled with pump-water; it was expoſed to the ſun in the ordinary way, as in Exp. 1, between ten and twelve o' clock, when ſo much very fine dephlogiſticated air was ſettled at the inverted bottom of the jar, that it filled

filled a cylindrical jar $4\frac{1}{2}$ inches long, and $1\frac{3}{4}$ inch diameter; which air gave by the nitrous teſt 1.94; 1.82; 1.67; 1.57; $2.45\frac{1}{2}$; 3.44. 456

Quantity of the two airs deſtroyed.

31. All the air of theſe hundred leaves being taken away, they were again expoſed to the ſun till ſeven o' clock in the evening, when about half the above-mentioned quantity of dephlogiſticated air was again found in the jar, which proved to be ſtill better than the former; its teſt was 1.99; 1.87; $1.73\frac{1}{2}$; 1.65; $1.93\frac{1}{2}$; 2.85; 3.79. 520

32. After ſeparating again this air from the leaves, I replaced the jar in the open air upon the ſame place, and left it till next morning at eleven o' clock, when I collected from the ſame leaves nearly the ſame quantity as the laſt time of very fine dephlogiſticated air ſettled

at

Quantity of the two airs deſtroyed.

at the top of the jar; it gave by the nitrous teſt $1.91\frac{1}{2}$; 1.75; 1.58;
511 1.44; 2.20; 3.15; 3.89.

33. Three leaves of a ſavoy-cabbage, of a moderate ſize, were put in a ſimilar jar with pump-water, in a fair ſun-ſhine, between twelve and two o'clock, when two ounce-meaſures of dephlogiſticated air were diſengaged: which gave by the nitrous teſt 1.94; $1.78\frac{1}{2}$; 1.63;
364 2.38; 3.36.

34. A handful of the leaves of *Naſturtium Indicum* yielded, in a few hours on another day in a fine ſun-ſhine, one ounce-meaſure of dephlogiſticated air, of the following goodneſs, 1.93; 1.76; $1.56\frac{1}{2}$;
504 1.39; 1.99; 2.96.

35. Seven and twenty ſingle leaves of a walnut-tree were in the ſame manner expoſed to the ſun-ſhine

ſhine in a fair warm day, from eleven till five o'clock, when they had yielded about one ounce-meaſure of good dephlogiſticated air.

SECTION V.

Experiments tending to inveſtigate the quality of air yielded by plants in the night, and by day in dark or ſhaded places.

36. TWO handfuls of graſs, the roots being cut off, were put in an inverted jar of a gallon, full of pump-water, and placed in a room during the night, the jar being covered ſo that no light could come at it before I examined it. In the morning

morning a ſmall quantity of air was ſettled at the inverted bottom of the jar, in which a candle was extinguiſhed directly.

37. At nine o'clock in the evening, Auguſt 8, when no leaves would yield any more air bubbles, except potatoe-leaves (which always begin the firſt to yield air in the morning, and ceaſe to yield them at night the laſt of all), I filled different jars with pump-water, and put in each a good deal of leaves of ſome or other plant or tree, viz. leaves of *oak*, *lime-tree*, *willow*, *yew-tree* or *taxis*, *apple-tree*, *ſage* or *ſalvia*, *artichoke*, *perſicaria urens* or *water pepper*, *potatoe* leaves: they were all kept in a room till next morning, when I examined the air which they had yielded.

Early

Early in the morning I found they all had yielded but an inconſiderable quantity of air.

The air of the oak leaves and walnuts was the worſt of all, it was not diminiſhed by nitrous air; that of the willow and the ſage was near as poiſonous; that of the lime-tree was not quite ſo bad; that of the artichoke was ſomewhat better; that of the potatoe-leaves was the leaſt poiſonous of all; however, in none of them a candle would burn even for a moment. That of the *water-pepper* was ſo poiſonous, that it extinguiſhed the flame, though diluted with five times as much common air; the apple-tree leaves had yielded ſo little air that I could not put it to any teſt.

38. Two handfuls of leaves of *French beans* were put in a jar full of

of water, which was kept inverted the whole night in a room; next morning a ſmall quantity of air was obtained, which proved to be of a very poiſonous quality; it extinguiſhed directly a flame. One meaſure of it with one of nitrous air occupied 1.94; ſo that an animal abſolutely could not have lived in it during one ſingle minute.

SECTION VI.

Experiments tending to inveſtigate to what degree plants may affect common air in the night, and by day time in ſhaded places.

39. SOME plants of graſs, with roots and all, were put in a jar of a gallon; the jar was inverted upon a diſh, and ſome water put in the diſh to keep the plants alive, and to cut off the communication with the external air; in the morning the air in the jar was altered for the worſe, the flame of a wax-taper grew dim in it. One meaſure of it with one of nitrous air occupied 1.24.

40. Two handfuls of leaves of *French beans* were put in a jar of a gallon; it was kept inverted upon a diſh, and ſome water poured upon it; next morning I found the air ſo much fouled that a candle could not burn in it. One meaſure of it with one of nitrous air occupied 1.39.

41. After having taken out ſome of the air for trial, I placed the jar with the remaining air and leaves in the ſun from nine till eleven o'clock, when I found the air ſo much mended, that a candle could burn in it, and that one meaſure of it with one of nitrous air occupied 1.12.

After this I replaced it again in the ſun till five in the afternoon, when I found the air ſo much mended as to be equal in goodneſs to common air.

42. A little branch of *Cedar of Lebanon* was placed over-night in a room under a cylindrical jar of about two ounce-meaſures; next morning the air was much fouled by it. One meaſure of it with one of nitrous air occupied 1.45.

43. Three jars inverted were placed upon diſhes in the manner as in Exp. 39; under one was placed a potatoe-plant; under the ſecond a plant of common *night-ſhade*; under the third a plant of *hyoſcyamus*.

Next morning I found the air of all three very much fouled, but that of the *hyoſcyamus* the moſt of all. In none of them would a candle burn. One meaſure of the air of the potatoe-plant with one of nitrous air occupied 1.59; one meaſure of that of the *night-ſhade* occupied with one of nitrous air 1.77; and that of the *hyoſcyamus* 1.83.

44. I placed a very lively chicken, eight days old, under a jar containing a quart full of the air fouled by the *hyoſcyamus* plant in the preceding experiment; it directly grew ſick, and was ready to expire in leſs than half a minute. I took it immediately out of the jar, and put it directly in another jar full of dephlogiſticated air drawn from the leaves of plants. The creature remained for ſome minutes quite motionleſs, ſhewing no ſigns of life but by drawing languidly its breath; it recovered gradually in this air in ſix or ſeven minutes ſo much that it began to ſtand upon its legs and to jelp now and then with a very weak voice. I then took it out of the dephlogiſticated air and put it upon

upon my hand in the open air; as ſoon as it came out of the laſt jar it ſeemed to grow worſe, and could ſtand no more: however it recovered gradually again.

45. A ſprig of *pepper-mint* was placed under an inverted jar upon a diſh, with ſome water upon it, to keep the plant alive, and to ſhut off the communication with the air in the jar, and the air without. It was placed in a warm day in a room againſt the window towards the ſun, from eleven till one o' clock. Another ſprig of the ſame plant, and of the ſame ſize, was put under a ſimilar jar, and placed upon the floor far from the windows in a room very well lighted, but in which the ſun did not ſhine at that time. By examining both airs I found the air of the jar which was

placed towards the ſun rather better than the common air was at that time. But the air of the other jar was leſs good than the common air; for one meaſure of it with one of nitrous air occupied 1.13; whereas one meaſure of the common air with one of nitrous air occupied $1.06\frac{1}{2}$.

46. Two handfuls of leaves of a *walnut-tree* were put into a gallon jar, filled with pump water, and placed about four feet from the wall of the houſe towards the north, and under the ſhade of raſsberry-buſhes, ſo that no rays of the ſun could reach it. After ſtanding there during ſeven hours in an agreeable warm day, ſcarce $\frac{1}{10}$ of air was obtained of what theſe leaves uſed to give in the ſun-ſhine; and this air was of ſuch a bad quality that a candle

candle could not burn in it: whereas two handfuls of walnut-leaves placed in the fun-fhine had yielded in the fame time a great quantity of very pure dephlogifticated air, whofe teft was 1.69; 1.82; 1.69; 1.54; 2.35; 3.34.

Quantity of the two airs deftroyed.

466

47. At the fame time two handfuls of *oak* leaves were in the fame manner placed under the fame bufhes; the air of thefe leaves was remarkably foul, for it was very little diminifhed by nitrous air.

48. The fame was done with *willow* leaves; the air obtained was alfo of a bad quality, but better than that of oak or walnut-leaves.

49. The fame was alfo done with leaves of *elm-tree*; the air obtained was very foul, as one meafure of it with one of nitrous air occupied 1.90.

50. I placed over-night in a room about an equal quantity of branches of a lime-tree, walnut-tree, a vine, an oak, and a willow, each in a different jar holding a gallon, without water, putting only ſome water in the plate upon which the jars were inverted, to keep the branches alive; the jars were left without being covered, and examined between eight and nine in the morning; the air of all was tried by putting one meaſure of it with one of nitrous air immediately one after another in the glaſs tube.

The air of the lime-tree occupied - - - -	1.24
the air of the walnut-tree	1.25
——————— vine	1.30
——————— oak	1.26
——————— willow	1.23

51. After

51. After having taken out as much air as was wanted for the trial, the jars were placed in the garden in a fine ſun-ſhine, and the air of them was again examined between ten and eleven.

The air of the lime-tree occupied	- - - -	1.08
the air of the walnut-tree		$1.07\frac{1}{2}$
——————— vine		$1.05\frac{1}{2}$
——————— oak		$1.12\frac{1}{2}$
——————— willow		1.07

After which they were again expoſed to the ſun till three in the afternoon, and their air examined.

The air of the lime-tree occupied	- - - -	1.06
the air of the walnut		1.05
——————— vine		$1.05\frac{1}{2}$
——————— oak		$1.12\frac{1}{2}$
——————— willow		1.07

So that the ſame air which the plants had fouled in the night was again reſtored to its former purity, and even by ſome of theſe plants rendered better than common air, principally by the walnut-leaves, lime-tree, and vine, the teſt of the common air being at the time that one meaſure of it with one of nitrous air occupied 1.07.

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SECTION VII.

Experiments ſhewing that the damage done to common air by the night is very inconſiderable compared to the benefit it receives in the day.

52. TWO handfuls of *Elm-tree* leaves were put in a jar full of water, as in the former experiment, and expoſed to the open air from the 14th of Auguſt to the 16th, when the air diſengaged from them was examined, and found to be good dephlogiſticated air; its teſt was 1.95; 1.85; 1.77; 2.40; 3.50. 350

Quantity of the two airs deſtroyed.

53. Some graſs, kept in a green bottle from the evening till next day at eleven o' clock, had yielded ſome air, whoſe teſt was 1.80; 1.64; 1.54; 2.31; 3.26. 374

54. A

Quantity of the two airs deſtroyed.

54. A handful of *Perſicaria urens,* water pepper, was kept in a green bottle, and expoſed to the open air from the evening till next day at ten o' clock, when the air collected from it was found of the following
226 teſt, 1.90; 1.86; 2.78; 3.74.

55. Two handfuls of leaves of *Lauro-ceraſus* were put in a jar full of water, and expoſed to the open air during 24 hours; a good quantity of dephlogiſticated air was obtained, whoſe teſt was 1.78; 1.61;
306 2.02: 2.97; 3.94.

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SECTION VIII.

Experiments ſhewing that plants have a remarkable power to correct bad air in the day.

56. A ſprig of *pepper-mint* put in a jar full of air fouled by breathing (ſo as to extinguiſh a candle), and expoſed to the ſun, had corrected this air in three hours ſo far that a candle could burn in it. Quantity of the two airs deſtroyed.

57. A ſprig of a *nettle* was put in a jar full of air fouled by breathing ſo as to extinguiſh a candle; it was placed in a room during the whole night; next morning the air was found as bad as before. The jar was put at nine in the morning in the ſun-ſhine; in the ſpace of two

Quantity of the two airs destroyed.

two hours the air was ſo much corrected, that it was found to be nearly as good as common air.

58. A ſprig of *Perſicaria urens* was put in a phial full of air fouled by breathing, ſo as not to allow a candle to burn in it; it was expoſed to the ſun during an hour and an half, in which time the air was ſo much corrected that a candle could burn in it. The ſame effect was obtained from a ſprig of a vine, and that of a camomile plant, and from ſome ruſhes.

59. A *muſtard* plant was put in a jar; the ſtem was cut off on a level with the orifice of the jar; the jar was then inverted in an earthen pan containing ſome water to keep the plant alive, and placed over-night in a room; next morning the air of the jar was found much fouled,

ſo

ſo as to extinguiſh the flame of a wax-taper; its teſt was, by Abbé Fontana's method, 1.98; 2.87; 3.83. 117

Quantity of the two airs deſtroyed.

60. It was then expoſed to the ſun, and examined again after a quarter of an hour, and found already ſomewhat corrected; for its teſt was then 1.97; 2.84; 3.79. 121

The jar was again put in the open air, when, after ſtanding 1½ hour in the ſun, the air was found to be remarkably corrected; for now its teſt was 2.01; 2.25; 3.24. 176

The jar was again replaced in the ſun; when it had been expoſed during three complete hours, the air was ſo much improved as to be better than the common air at the time; for its teſt was 1.95; 2.21½; 2.20 180

The

Quantity of the two airs destroyed. The test of the common air was at that time, by Abbé Fontana's
$173\frac{1}{2}$ method, 1.96; 2.25; $3.26\frac{1}{2}$.

See also Experiments 40 and 41.

SECTION IX.

Experiments shewing that acrid, stinking, and poisonous plants yield by day-time as good dephlogisticated air as any others.

61 A plant of *Hyoscyamus* was exposed in the ordinary way in a jar full of water in the sun-shine, from twelve till five o'clock; a great deal of fine dephlogisticated air was obtained, in which the flame of a wax-taper was very brilliant. One

meaſure of it with two of nitrous air occupied 0.93; with three 1.70.

Quantity of the two airs deſtroyed.

62. Leaves of *Lauro-ceraſus* (one of the moſt active poiſons that exiſts in the vegetable kingdom, when in a concentrated ſtate, killing an animal almoſt in an inſtant, and equally poiſonous when taken in the ſtomach, as when applied to a wound, which laſt effect was lately diſcovered by Abbé Fontana) treated in the ordinary way yield a good deal of dephlogiſticated air. Two handfuls of them expoſed in water to the ſun, between eleven in the morning and five in the afternoon, yielded a good deal of dephlogiſticated air, whoſe teſt was 1.87:
1.67; 1.50; 2.04; 3.04. 394

63. Two handfuls of common *night-ſhade*, a ſuſpected plant, ex-

poſed

Quantity of the two airs deſtroyed.

poſed in the ſun between two and five o'clock, had yielded a great deal of dephlogiſticated air, whoſe teſt was 1.92$\frac{1}{2}$; 1.79; 1.65; 1.52;
495 2.08$\frac{1}{2}$; 3.05.

64. I got in the ſame way good dephlogiſticated air from *tobacco* leaves, (ſee Exp. 9); from *Atriplex Vulvaria*, a plant of a very particular offenſive ſmell; *Cicuta Viroſa* or *water hemlock*, one of the moſt dangerous poiſons; and *Sabina*.

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SECTION X.

Experiments ſhewing that all flowers in general yield a very poiſonous air, though in a very ſmall quantity, and are apt to ſpoil a great quantity of good air by day and by night.

65. TWO handfuls of flowers of *Marigold* or *Calendula* were expoſed in pump-water to the open air during 48 hours; a ſmall quantity of air was obtained, which extinguiſhed flame directly, and was ſcarcely diminiſhed at all by nitrous air.

66. Two handfuls of *Camomile* flowers were put in a quart jar filled with pump-water, and invert-

ed: after two days ſtanding in the garden, a ſmall quantity of air was obtained, in which a flame was immediately extinguiſhed.

67. Forty-five flowers of *Marigold* were put in a quart jar without water, and kept the whole night in a room, the jar being inverted; next morning I found the air ſo much fouled by them that a candle would not burn in it. One meaſure of it with one of nitrous air occupied 1.43.

68. I placed the ſame flowers with the remaining air in the ſunſhine from nine till twelve, when I found the air ſtill more infected. One meaſure of it with one of nitrous air occupied 1.54.

69. A few flowers of *Honey-ſuckles, Caprifolium*, were placed under a jar of about a pint; when they had

had ſtood about three hours in the room, the air of the jar was ſo much infected that a candle could not burn in it.

The ſame effect was obtained with a ſimilar quantity of the ſame flowers expoſed during three hours in the ſun-ſhine.

70. A ſimilar quantity of theſe ſweet-ſcented flowers were kept in a pint jar over-night; when they had ſo much fouled the air that an animal muſt have died in it, one meaſure of it and one of nitrous air occupied 1.68.

All kinds of flowers had nearly the ſame effect. All of them fouled the air more or leſs, either in a room or in the open air, as well by day as in the night, equally in ſun-ſhine as in a ſhaded place.

SECTION XI.

Experiments ſhewing that roots of plants when kept out of the ground yield, in general, bad air, and ſpoil common air at all times, ſome few excepted.

Quantity of the two airs deſtroyed.

71. THREE handfuls of roots of muſtard plants, waſhed clean, were put in a jar full of water in the ordinary way, and expoſed to the ſun during ſix hours; when ſome air was obtained, which extinguiſhed a candle directly.

72. Two handfuls of roots of common *ruſhes*, well cleaned from dirt, were in the ſame manner expoſed to the ſun during ſeven hours; when a ſmall quantity of air was

6 obtained,

obtained, in which a candle could not burn. *Quantity of the two airs destroyed.*

73. One handful of roots of muſtard plants, cleanly waſhed, was put in a quart jar full of water, and three ounce-meaſures of common air let up: after ſix hours ſtanding in the ſun, the air was found changed for the worſe, for its teſt was, by Abbé Fontana's method,
1.95; 2.34; 3.37. 162

74. A handful of roots of *Becabunga* was expoſed to the ſun in a quart jar filled with water during ſix hours; a moderate quantity of air was obtained, which, by the nitrous teſt, proved to be as good as common air.

All other roots which I tried yielded bad air, and ſpoiled ordinary air at all times.

SECTION XII.

Experiments ſhewing that all fruits in general yield bad air, and infect ordinary air at all times, but principally in a dark place, and in the night.

75. Six *peaches* of a ſmall ſize were put under an inverted quart jar placed upon a diſh, in a room not very light, between two in the afternoon and ſeven in the evening; when I found the air in the jar ſo much ſpoiled, that a candle could not burn, nor an animal live in it. One meaſure of it with one of nitrous air occupied 1.86.

76. Two of theſe *peaches*, being put under the ſame quart jar during

two

two hours, had ſo much altered the air that a wax-taper could ſcarcely burn in it a moment, but was ready to go out.

77. Six *peaches* of a ſmall ſize were placed under an inverted quart jar in the ſun between nine and eleven; when, by examining the air, I found it to extinguiſh a candle. One meaſure of it with one of nitrous air occupied 1.55.

78. One *lemon* placed under a jar, containing three-quarters of a pint, infected the air ſo much in a few hours, that a candle burned dim in it.

79. One handful of *filberts* were placed under a jar of two pints during the night; I found the air in the jar ſo much fouled as to extinguiſh a candle.

80. Six ſmall *Bergamot* pears were put over-night under a jar of two pints; the air was altered ſomewhat for the worſe; a flame grew dim in it. One meaſure of this air with one of nitrous air occupied 1.25.

81. Three apples nearly ripe were placed under a two-pint jar over-night; the air was found much infected by them; it extinguiſhed a flame. One meaſure of it with one of nitrous air occupied 1.48.

82. The remainder of the air in this jar was kept with the apples, and expoſed to the ſun during ſeven hours, when the air was become ſtill worſe. One meaſure of it with one of nitrous air occupied 1.72. A flame was directly extinguiſhed in it.

83. Four lemons were placed under a quart jar in the ſun during ſeven hours, when the air was changed for the worſe. One meaſure of it with one of nitrous air occupied 1.18.

84. A jar holding a gallon was one-third filled with ripe *mulberries*, and expoſed to the ſun, being inverted upon a diſh; in the ſpace of four hours the air in the jar was ſo much infected as to extinguiſh a flame directly. One meaſure of it with one of nitrous air occupied 1.63.

85. *Plumbs* and *blackberries*, ripe and unripe, ſpoiled alſo common air in the ſun and in the dark.

86. Six apples, as ſoon as taken from the tree, were directly put in a gallon jar full of pump-water, and expoſed to the ſun, the jar being inverted

inverted upon a diſh; the apples became covered with a great number of ſmall air-bubbles. After they had been thus expoſed to the ſun from ten in the morning till four in the afternoon, a moderate quantity of air was obtained, which proved to be very bad; a flame was directly extinguiſhed in it. One meaſure of it with one of nitrous air occupied 1.69.

87. Two dozen of young and ſmall *French beans* were put in a quart jar full of water, and expoſed in the ſun from ten till two o' clock: they were covered all over with a great many ſmall air-bubbles; the quantity of air collected was but ſmall, and in quality ſomewhat worſe than common air. One meaſure of it with one of nitrous air occupied 1.14; whereas one meaſure

ſure of common air with one of nitrous air occupied $1.08\frac{1}{2}$.

88. Two dozen of young and ſmall French beans were put under an inverted quart jar over-night in a room without water; they ſtood till eleven in the morning and were not covered, ſo that they had been a long while expoſed to all the light of the room. The air in the jar was found ſo remarkably poiſoned, that it even ſurpaſſed in foulneſs the air infected by a plant of *Hyoſcyamus* (ſee Exp. 43); for one meaſure of it with one of nitrous air occupied 1.95.

89. I was willing to ſee the effect of ſuch offenſive air upon a living animal. I placed a very lively chicken eight or nine days old in this air; in the very inſtant it entered the jar it ſhewed ſigns of the

utmoſt

utmoſt anxiety, fell down motionleſs, and died in leſs than half a minute. When I ſaw it dying, I took it out with all the expedition poſſible, in order to recover it in another jar full of dephlogiſticated air, which I had kept ready for the purpoſe; but, notwithſtanding it had not been 20 ſeconds in this foul air, it was quite deprived of life.

Comparing the ſuddenneſs of deſtroying the life of an animal with this air, with that of inflammable air drawn from metals by vitriolic or marine acid, I found that the air fouled by theſe beans was as deſtructive to animal life as the inflammable air itſelf.

90. I placed ſix of theſe beans over-night in a gallon jar inverted upon a plate, on purpoſe to ſee whether

whether ſuch a ſmall number of them could affect obſervably ſuch a great body of air. I was aſtoniſhed to find they had ſo much affected the air, that it was rendered quite unwholeſome for breathing; it extinguiſhed a flame; and one meaſure of it with one of nitrous air occupied 1.34.

91. Three ſmall unripe walnuts were put under a jar of about three ounce-meaſures, from twelve till two o'clock, in a room by day, when the air in the jar was ſo much ſpoiled as to extinguiſh flame. Its teſt was, that one meaſure of it with one of nitrous air occupied 1.54.

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SECTION XIII.

Experiments ſhewing that no part of plants improve ordinary air, or yield dephlogiſticated air, but the leaves and the green ſtalks.

Quantity of the two airs deſtroyed.

92. THE former experiments with flowers, roots, and fruits, are already above related. There remain only the green ſtalks or branches, not yet covered with the rough ſkin or bark, and the wood itſelf, to be examined.

I put ſome green ſtalks of a willow-tree, the leaves being ſtripped off, in a gallon jar filled with pump-water; the jar was expoſed, inverted, as ordinary, upon a wall in a warm ſun-ſhine during four hours.

They became moſt beautifully co- vered with an infinite number of round air-bubbles. A great deal of dephlogiſticated air was obtained, which gave, by the nitrous teſt, 1.96; 1.87; 1.83½; 2.68; 3.64. 336

Quantity of the two airs deſtroyed.

93. Some branches of a mulberry-tree, covered with grey bark, were put in a gallon jar full of pump-water, and expoſed to the ſun. A moderate quantity of air was obtained, which, being put to the nitrous teſt, proved to be about the ſame quality with common air; its teſt was 2.01; 2.10; 3.10. 190

SECTION XIV.

Experiments ſhewing what kind of water obſtructs leaſt the natural operation of leaves yielding dephlogiſticated air.

94. AN equal number of willow leaves were put in four different jars, each holding a gallon; one jar was filled with ſtagnating water taken out of a pond rather unclean; the ſecond jar was filled with rain-water collected the day before; the third with river-water; and the fourth with water taken freſh from the pump. They were all placed, at eleven o'clock, upon a wall in the ſun-ſhine; and the air yielded by the

the leaves was taken out of the jars at three in the afternoon.

Quantity of the two airs destroyed.

The refult was, that the leaves put in pond-water had yielded the leaft quantity of air, and that of no better quality than common air. Thofe in the rain-water had yielded more air, and of a better quality. Thofe in the river-water had yielded ftill more and better. The pump-water had yielded the moft and beft of all.

To be able to judge the better of the exact degree of purity of thofe airs, I put them all to the nitrous teft; the refult was as follows:

Air from the leaves in ftagnating-water 2.04; 2.20; 3.22. 178

Air from the leaves in rain-water 1.94; 1.96½; 2.69; 3.69. 231

Quantity of the two airs deſtroyed.

Air from the leaves in river-water
256 2.05; 2.04; 2.47; 3.44.

Air from the leaves in pump-water 1.96; 1.85; 1.72; 1.64;
456 2.47; 3.44.

95. I put a handful of leaves of a willow in a jar full of newly diſtilled water, and expoſed it to the ſun during four hours; the leaves gathered ſome bubbles upon the under ſide, but very few upon the upper ſide; and very little air was obtained, ſcarcely enough to put it to the teſt; and of this air about $\frac{1}{5}$ was common air, which had ſlipt in by inverting the jar. It was very far from being dephlogiſticated air; it was even worſe than common air.

96. I obtained ſome water which was diſtilled ſome months ago, and put ſome leaves of a vine in it. A

ſmall

ſmall number of bubbles ſettled upon the under ſide of the leaves, but very few upon the upper ſurface. The jar was placed in the open air during about five hours, the weather being cloudy. A ſmall quantity of air was obtained, which was worſe than common air.

97. I impregnated ſome water drawn out of a well with fixed air by Dr. Noot's contrivance, or by the glaſs apparatus ſold at Mr. Parker's. I put ſome leaves of a vine in a jar full of this water ; as ſoon as they were under water, they were all covered moſt beautifully with bubbles. After ſtanding about five hours in the garden in a cloudy day, ſome air was obtained, which proved alſo, by the nitrous teſt, to be worſe than common air, the greateſt part of it being abſorbed

by the water before it was put to the teſt.

98. I impregnated a jar full of water with fixed air by means of ſalt of tartar and ſpirit of vitriol, according to the method of Dr. Hulme. I put ſome leaves of a vine in this water, which I found covered with air-bubbles as ſoon as they were plunged under the water, firſt at the under ſurface, and ſoon after at the upper ſurface alſo. After ſtanding about four hours in a warm ſun-ſhine, I found a very large quantity of air collected at the inverted bottom of the jar, which I found by far the greateſt part to be fixed air, as it was abſorbed in the water by ſhaking. I put to the nitrous teſt that part of it which remained unabſorbed, and

and found it inferior in quality to common air.

It might be found reaſonable to think, that thoſe numerous air-bubbles, which appear upon the leaves as ſoon as put under the ſurface of the water impregnated with fixed air, are owing to the fixed air ſettling in the form of theſe bubbles upon the ſurface of the leaves.

The ſudden appearance and increaſe of theſe bubbles depend greatly upon the fixed air ſettling on the ſurface of the leaves; for any other body gets alſo bubbles in ſuch water; but the vital motion of the leaves acts its part in this ſcene; for theſe bubbles appear firſt on the ſame ſurface of the leaves on which they appear in common water. It appeared, by a variety of experiments I made on this head, that water much impregnated with fixed air difturbs the natural operation of the leaves in yielding dephlogifticated air, and that the air thus obtained was chiefly the fixed air from the water, and fome little quantity of air, which is fometimes better than common air, but for the moft part much worfe.

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SECTION XV.

Experiments ſhewing to what degree of purity dephlogiſticated air may be elaborated by vegetables.

IT has appeared in the courſe of ſeveral hundred experiments which I made in my retirement, that leaves of plants in general yield the fineſt air when they are not much crouded together, ſo that the moſt part of them receive the direct influence of the ſun principally in the afternoon between mid-day and ſix o' clock in the middle of the ſummer.

99. I obtained from ſeveral plants ſuch a pure dephlogiſticated air, that the flame of a wax-taper not

Quantity of the two airs deſtroyed.

only burned in it with ſuch a degree of brightneſs that it dazzled my eyes, but it excited a crackling hiſſing which accompanies the flame when plunged in pure dephlogiſticated air. Among the plants which yielded the pureſt airs were ſome aquatic plants and the turpentine-trees, from which I always got air of an eminent degree of purity, ſo that ſometimes ſix meaſures of nitrous air were required before the ſaturation of the two meaſures of the dephlogiſticated air could be obtained, and that above $\frac{500}{800}$ of the bulk of the two airs were deſtroyed.

100. In September I got from young leaves of a vine ſuch pure air that its teſt gave the following reſult: 1.97; $1.87\frac{1}{2}$; 1.78; 1.68;
470 2.33; 3.30.

101. And

101. And the ſame day, from full-grown leaves of a vine ſtill purer, it gave the following reſult: 1.95; 1.85; 1.72; 1.60; 1.61; 2.53.

Quantity of the two airs deſtroyed.

547

102. The air obtained from the green matter ſurpaſſed in purity the dephlogiſticated air obtained from leaves; this purity was ſo great, that this dephlogiſticated air required eight meaſures of nitrous air to ſaturate two meaſures of it, and that $\frac{645}{1000}$ of the bulk of the two airs were deſtroyed. The dephlogiſticated air, which I obtained from the green matter collected from a ſtone-trough kept full of water near a ſpring upon the high-road, was ſo great, that $\frac{652}{1000}$ of the bulk of the two airs were deſtroyed before the complete ſaturation was obtained.

As

Quantity of the two airs deſtroyed.

As this green matter is probably of the vegetable kind, I make no doubt but as good dephlogiſticated air might be obtained from leaves of plants by ſome way or other which I have not yet been lucky enough to hit upon.

However pure this dephlogiſticated air may be, that which may be extracted from certain ſubſtances which do not belong to the vegetable kingdom is ſtill ſuperior to it, as is the air obtained from nitre and red precipitate.

To give an exact account of the nature of theſe airs, I will place here the reſult of the firſt mentioned dephlogiſticated air, drawn from the green matter, produced by itſelf in the jar. The method of trying it was that of Abbé Fontana, 2.05; 2.01; 1.93; $1.81\frac{1}{2}$; $1.72\frac{1}{2}$; $1.70\frac{1}{2}$;
645 $2.62\frac{1}{2}$; 3.55.

The

The reſult of the teſt of the de-phlogiſticated air, obtained from the green matter gathered from the ſtone-trough on the high-road, 2.08; 1.07; 2.01; 1.92; 1.89; 1.78; 2.54; 3.48. 652

Quantity of the two airs deſtroyed.

How near the purity of this air approaches to that of the dephlogiſticated air, extracted by fire from red precipitate, may be ſeen in the following teſt of it: 1.63; 1.28; 93; 59; 27; 58; 1.02½; 2.50. 750

So that the two meaſures of this dephlogiſticated air had been reduced to about $\frac{1}{7}$, and that $\frac{750}{1000}$ of both airs had been deſtroyed to complete the ſaturation.

SEC-

SECTION XVI.

Experiments ſhewing the effect of plants upon inflammable air.

103. TWO ounce-meaſures of inflammable air (which was ſo ſtrong as not to be diminiſhed at all by nitrous air) were let up in a quart jar containing one handful of pepper-mint ſprigs; it ſtood over-night within the houſe: next day I found the bulk of the air ſomewhat increaſed, but ſtill ſo bad as not to be diminiſhed at all by nitrous air.

104. The ſame evening I put two ounce-meaſures of inflammable air in a ſimilar jar with one handful of walnut-leaves; next day

I found

I found the bulk of air increaſed to about $\frac{1}{10}$. One meaſure of it with one of nitrous air occupied 1.90.

105. Two ounce-meaſures of inflammable air was alſo let up in a ſimilar jar with one handful of *Perſicaria urens* or water-pepper; next day I found the bulk of the air diminiſhed about $\frac{1}{25}$. One meaſure of it with one of nitrous air occupied 1.97.

N. B. All theſe three jars ſtood in the houſe from the evening till between twelve and one next day, ſo that the light of the day may have extricated ſome air from the pepper-mint and the walnut-leaves.

As neither of theſe plants could be ſaid to have really corrected this poiſonous air, I was curious to ſee what

what effect they would have upon the same air in the sun.

106. For this purpose I let up again two ounce-measures of the same inflammable air in the jar containing the walnut-leaves Exp. 104; and placed it in the sun between two and five o'clock; when I found the bulk of the air increased to $\frac{1}{4}$, but very little corrected, for one measure of it with one of nitrous air occupied 1.89. See, in Exp. 107, the reason why these leaves failed to correct this air.

107. I also let up two ounce-measures of the same inflammable air in the jar containing the *Persicaria urens* Exp. 105; and placed it in the sun between two and six; when I found the bulk of the air increased $\frac{1}{12}$, and so much mended, that one measure

of

of it with one of nitrous air occupied 1.33.

108. I had alſo let up, in the ſame jar in which the pepper-mint had been the whole night, two meaſures of inflammable air, and kept it in the ſun about three hours; but, having forgot to copy the reſult of this experiment in my notes, I repeated it next day by itſelf. I let up two ounce-meaſures of inflammable air in a quart jar, in which I had put four ſprigs of pepper-mint, ſo that the whole made up about one handful; I placed it in a fine ſun-ſhine from one till half paſt four; when I examined the air, I found it increaſed about $\frac{1}{10}$, and very much mended; for one meaſure of it with one of nitrous air oocupied 1.21; ſo that it approached very much to the nature

of respirable air ; it exploded however with a loud report.

109. As, in experiment 106, the two measures of inflammable air let up in the jar containing the walnut-leaves were scarce corrected at all in the day time, whereas the other plants had corrected this air in a very great measure ; I suspected that the walnut-leaves had suffered from the inflammable air in the night time, and that, perhaps, they had lost their natural power of correcting this kind of air, or that some mistake had been committed ; I thought it therefore adviseable to repeat the experiment another day, which I did. Having let up in a jar filled with pump-water, in which a handful of walnut-leaves were, two measures of inflammable air, and left the

the jar in the ſun-ſhine from twelve till five o'clock, I found the air much corrected, for one meaſure of it with one of nitrous air occupied 1.30. The air was very exploſive.

I was now ſatisfied that all plants poſſeſs the power of correcting inflammable air; but I wanted to ſee whether plants could reduce inflammable air to the purity of common air, by letting the inflammable air remain during two or more days with the plant.

110. A meaſure of inflammable air was let up in a jar containing a handful of *Perſicaria urens*, and another meaſure in a jar containing a handful of leaves of a walnut-tree. They ſtood 48 hours in the open air, when I examined them.

Quantity of the two airs deſtroyed.

The inflammable air put with the walnut-leaves ſeemed to be corrected ſo much as to appear, by the nitrous teſt, better than the common air was at the time; for one meaſure of it with one of nitrous air occupied 1.03; whereas one meaſure of common air with one of nitrous air occupied 1.05. This inflammable air gave the following reſult, by Abbé Fontana's
184 teſt, 1.91; 2.16½; 3.16.

Having filled a cylindrical jar with this air, I found it explode with an uncommon loud report, which ſurprized me not a little, and gave me ſome apprehenſion that the nitrous teſt might fail in ſome inſtances. The inflammable air let up in the jar with the *Perſicaria urens* gave the following teſt: one meaſure of it with one of

nitrous

nitrous air occupied 0.95; and with two meaſures of nitrous air 1.92. By Abbé Fontana's method it gave 1.90; 1.96; 2.95.

Quantity of the two airs deſtroyed. 205

Thus this air ſeemed to ſurpaſs far the goodneſs of common air.

111. I then tried it by the flame of a candle, and found it to explode with a very loud report. As I thought the reſult of theſe trials very extraordinary, and to afford a remarkable exception in the application of nitrous air to the teſt of any air, I repeated each of theſe experiments twice, and obtained conſtantly the ſame reſult.

112. I was reſolved, however, to repeat again the experiment: Some plants of *Perſicaria urens* were put in a gallon jar, and a good quantity of pure air was let up in the jar. It was kept in the open air from Sunday till Friday follow-

ing, when it was examined, and found to be so poisonous that a chicken, three weeks old, died in it in less than a minute. It proved also very bad by the nitrous test; for one measure of it with one of nitrous air occupied 1.80; and the result of Abbé Fontana's method was 2.58; 3.58.

This result, being quite different from the result in Experiments 108, 109, 110, and 111, restored my hope that some blunder had been committed in the experiments just mentioned; I resolved therefore, if possible, to discover this mystery.

113. Two pints of strong inflammable air (which could not be diminished by nitrous air) were let up in a gallon jar containing some plants, with roots and all, of *Persicaria urens*, which was placed in

the garden. After it had ftood 24 hours, the air was examined, and found much mended; for one meafure of it with one of nitrous air occupied 1.23; it exploded with a loud report. It was again replaced in the garden, and examined after it had ftood 48 hours, when it was found, by the nitrous teft, at one o' clock in the afternoon, near as good as common air; for one meafure of it with one of nitrous air occupied $1.11\frac{1}{2}$. It gave, by Abbé Fontana's teft, 2.04; $2.33\frac{1}{2}$; 3.32. And yet it ftill exploded as 168
before.

Quantity of the two airs deftroyed.

After this trial it was again placed in the open air, and re-examined the fame day between four and five in the afternoon, when the nitrous teft indicated it to be better than common air; for one meafure of it with one of nitrous air occupied 1.06$\frac{1}{2}$; whereas one meaſure of common air and one of nitrous air occupied at that time 1.08.

114. This reſult convinced me entirely that the nitrous teſt really fails in this kind of air; for though it gave all the appearance of good air, yet it exploded with a loud report; and a chicken placed in it grew immediately ſick, and was ready to expire in ſix minutes, when I took it out quite motionleſs.

115. The remainder of the ſame inflammable air, which had ſtood during ſix days with the *Perſicaria urens* in Exp. 112, without being much changed, was let up in a jar containing a plant of muſtard. After ſtanding 24 hours in the garden I put it to the teſt, when I found it ſo much mended, that one meaſure of

of it with one of nitrous air occupied 1.02; one measure of it with two of nitrous air occupied 2.00. The result of Abbé Fontana's test was 1.96; 2.13$\frac{1}{2}$; 3.12$\frac{1}{2}$. 187$\frac{1}{2}$

Quantity of the two airs destroyed.

So that it already surpassed in appearance the best common air; it exploded however with a loud report. I placed the jar again in the garden, and examined the air after it had stood during 48 hours, when I found it to all appearance still more improved; for one measure of it with one of nitrous air occupied 0.96; and with two measures of nitrous air 1.80. The result of Abbé Fontana's test was 1.97; 1.93; 2.72$\frac{1}{2}$; 3.66. 235

It still exploded with great violence.

I placed the jar again in the garden during four hours longer in

Quantity of the two airs destroyed.

a fair sun-shine, when I found the air still better by the nitrous test; for now one measure of it with one of nitrous air occupied 0.94; and it gave by Abbé Fontana's me-
260 thod 1.96; 1.87$\frac{1}{2}$; 2.44; 3.40.

It had not, however, lost its explosive force.

116. Some of the pure inflammable air was put in a jar with an inverted plant of *Persicaria*, so that the root was in contact with the air; it stood during six days in the garden, when I found only $\frac{1}{5}$ of the air remaining, and this was no longer explosive nor inflammable, but a flame only grew dim in it. So that roots of water-plants have a remarkable power of absorbing inflammable air, as I found by several other experiments.

117. I gathered ſome inflammable air from ſtagnating water, by ſtirring up its muddy bottom. This air was ſo bad, that one meaſure of it with one of nitrous air occupied 1.98. One meaſure of this air was let up in a jar containing a ſprig of pepper-mint, the root being cut off. It ſtood from ten in the morning till four in the afternoon in the ſunſhine; I found it ſo much corrected, that one meaſure of it with one of nitrous air occupied 1.60. It burnt as well as before.

118. An equal meaſure of the ſame inflammable air from the ſtagnating water was let up in a jar containing a ſmall plant of *Perſicaria urens*, with root and all. After ſtanding the ſame time as the former, the air was examined, and found more corrected than the other,

for

for one meaſure of it with one of nitrous air occupied 1.48; but it was as inflammable as before.

It ſeems to me probable, from the above-mentioned experiments, that plants have a power of correcting even the worſt of all airs, inflammable air; but that they require ſome days to perform this tranſmutation, and that one and the ſame plant does not live long enough in full vigour to finiſh the bunefs, if it is ſhut up in a narrow ſpace with a certain quantity of this air; and that this air, after having been in a great meaſure mended by a plant, returns again to its former poiſonous quality, if it remains with the plant after the vital operation of the plant ceaſes, which I apprehend was the cauſe of the difference

of

of the event in Experiments 110, 111, 112, 113, 114, 115.

Quantity of the two airs deſtroyed.

It appears alſo that plants have a power of changing inflammable air into a kind of air which is not to be known by the ordinary nitrous teſt, and which is the only air I know that explodes without the addition of any other air; ſo that it ſeems to be by itſelf a true *fulminating air*; for this inflammable air, after the *perſicaria* plants were changed four times during 16 days, gave at laſt the following reſult, 1.81; 1.56; 1.37; 2.27; 3.25. 375

One meaſure of it with one of nitrous air occupied 0.84; with two meaſures of nitrous air 0.98; with three 2.00; and yet it had not loſt its exploſive quality, though by this diminution of its bulk with nitrous air it indicated to be far better than common air, nay even to be dephlogiſticated air.

I make no doubt but the plants had communicated to this air the quality of being diminiſhed by nitrous air, by mixing with it the dephlogiſticated air they yield of themſelves; which is alſo the opinion of Abbé Fontana, to whom I communicated the experiment. But I cannot but think that the plants, by their vital powers, had changed this pure inflammable air into *fulminating* or exploſive air, as this quality is given to it in one night, or in a dark place in a few hours; though plants yield no dephlogiſticated air in the night or in dark places, but ſcarcely any air at all, and whatever air they yield is phlogiſticated air, unfit for ſupporting flame. It even ſeems to me not

improbable,

improbable, that living plants not only improve good air, or correct bad air, by communicating their dephlogifticated air to it, but alfo by a peculiar faculty they poffefs of purifying the circumambient air, which they may do by taking to themfelves the inflammable particles, or by fome other faculty they poffefs. Air fouled by breathing is thus rendered quite pure again in a few hours by a plant growing in it, as is already fhewn above.

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SECTION XVII.

Experiments towards inveſtigating what plants or trees infect the ſurrounding air the leaſt by night.

119. I PLACED in four different gallon jars an equal quantity, as near as I could, of leaves upon their ſtalks of the following trees, *lime-tree*, *oak*, *laurocerafus*, *walnut*. I placed all thoſe jars over-night in a room, each inverted upon a diſh; in each jar was as much water as would preſerve the leaves alive by keeping the ſtalks wet. Next morning I found the air of all the jars contaminated: that of the walnut-leaves was become unfit for breathing, and extinguiſhed flame;

flame; that of the *laurocerasus* was next in foulness to the walnut; then followed the lime-tree; the oak had spoiled the air the least of all.

One measure of the air in which the walnut-leaves had been, with one measure of nitrous air, occupied 1.53; that of the *laurocerasus* 1.26; that of the lime-tree 1.16; and that of the oak 1.10.

120. I have observed that the branches of a vine generally infect the air much less by night than most part of other trees. Cabbage among the culinary plants was, of all I tried, the least disposed to contaminate air.

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SECTION XVIII.

Experiments ſhewing that the pureſt dephlogiſticated air, and the greateſt quantity, is yielded by full-grown leaves.

121. I PLACED in a jar full of pump-water the extremity of a branch of a vine containing leaves of different ages, from the full-grown to thoſe which begin only to unfold themſelves. The bubbles appeared the firſt in the old leaves; and they broke out gradually upon the next in age; ſo that they appeared the lateſt upon the new-formed leaves. The ſame proportion takes place alſo in the ſize of the bubbles, as well as in the

the quantity of the dephlogiſticated air obtained from them.

Quantity of the two airs deſtroyed.

122. I placed in a gallon jar filled with water ſome old or full-grown leaves of a vine, and expoſed it to the ſun from nine in the morning till two in the afternoon, when a great quantity of very pure dephlogiſticated air was obtained, whoſe teſt was 1.95; 1.85; 1.72; 1.60; 1.61; 2.53. 547

123. I placed in another jar of the ſame ſize a ſimilar quantity of young leaves of the ſame vine, and expoſed them to the ſun during the ſame time. I obtained a good quantity of fine dephlogiſticated air, but leſs, and of an inferior quality, than that obtained from the old leaves. Its teſt gave 1.97; 1.87½; 1.78; 1.68; 2.33; 3.30. 470

SECTION XIX.

Experiments ſhewing that the ſun by itſelf, without the aſſiſtance of plants, does not improve air, but renders it rather worſe.

124. TWO jars, half-full of air taken from the atmoſphere at the ſame time, and half-full of pump-water, were left by themſelves during four hours, the one expoſed to a bright ſun-ſhine, the other placed within the houſe, only two ſteps from a door opening in the garden.

The air kept in the houſe gave, in ſix different trials, conſtantly the appearance of being better than that of the jar placed in the ſun. One meaſure of the air kept within doors with one of nitrous air occupied 1.06½; whereas that exposed to the sun occupied 1.08½.

I must however acknowledge, that this experiment ought to be repeated more than once, to put the fact out of any doubt. I made it the very last day of my stay in the country, and thus had no time to repeat it.

SECTION XX.

Experiments tending to investigate the most accurate and expeditious way of putting common air to the test, on purpose to judge of the salubrity of any country.

I HAVE already said enough, in the introduction to the second part of this work, of the accuracy with which this difficult and important investigation may be made by employing the instruments of Abbé Fontana; but as there is much more attention and dexterity required to judge with the greatest nicety of the degree of salubrity of the atmospheric air than of any other, as the other airs are of much less importance

importance to mankind; I reſerved this article for the laſt, not diſcontinuing to purſue my experimental enquiries till the book was already nearly printed off. Beſides, it was but in the middle of September that I got the braſs tube, expreſſed in Fig. I. in which the glaſs tube or great meaſure is ſuſpended; ſo that the column of water within and without the glaſs tube be at a perfect level, which is neceſſary to obtain a reſult conſtantly the ſame with the ſame air. This braſs tube is a valuable addition to the apparatus.

I had before that time made experiments every day with the atmoſpheric air, placing the glaſs tube in a cylindrical jar filled with water, and lifting up the glaſs tube till the extremity of the column of water

water within the tube was on a level with the brim of the jar; always taking hold of the glafs tube by means of a piece of linen folded five or fix times, and thoroughly imbibed with water, to prevent the warmth of my hand communicating itfelf to the glafs tube.

Though I ftill think that Abbé Fontana's method of examining atmofpheric air is the moft accurate; yet, as I had it more in view to trace nature in the operation of vegetables than to examine the degree of falubrity of the common air, I endeavoured to abridge this trial as much as poffible, on purpofe to fave time. For this reafon, I got at laft in the habit of making this trial in the time of a minute or two, and found a furprizing accuracy in the refult. This mode is in fome

degree

degree compofed of the methods ufed by the two moft eminent philofophers in this branch of natural knowledge, the Rev. Dr. Prieftley, and Abbé Fontana. It is this: I let up in the little meafure as much common air as will fill it; after which, I take hold of its brafs flider, and keep it under water exactly 15 feconds, when I lift it up till the brafs flider be on a level with the water of the trough, and fhut the flider, to cut off the column of air within the meafure; I then invert the meafure under water, to let out all the air which was remaining under the flider. I let up immediately this meafure of air in the large tube, and fill the little meafure in the fame manner with nitrous air newly made from red copper, in the manner explained p.

 171;

171; which being alſo let up in the large tube, I begin to ſhake forcibly this tube in the water-trough exactly 30 ſeconds (beginning the motion preciſely at the moment the two airs come into contact), and place it directly afterwards in the braſs tube, and let it ſtand thus in the middle of the trough for the ſpace of one minute, pouring continually water upon it, to bring the temperature of the glaſs tube to that of the water; for, holding it in the hand while ſhaking, it receives ſome degree of heat from the hand, and of courſe the column of air within is rarified. I then ſlide the glaſs tube up or down within the braſs tube, which is filled with water, till the two columns of water come to a level with each other, and with the 0 of the braſs

meaſure,

meaſure, as is expreſſed in the plate by BB in Fig. I. Then I obſerve with what number of the ſcale the firſt diviſion of the glaſs tube above the column of water coincides, which ſhews me at once how many ſub-diviſions are remaining from the two meaſures of airs, or from the 200 ſubdiviſions let up in the tube, and thus indicates the degree of goodneſs of common air, or indeed of any air approaching in goodneſs to common air, or being of an inferior quality. But this method will not do in examining dephlogiſticated air, as this air requires more nitrous air to bring it to a full ſaturation. By this ſimple and eaſy method the whole operation is performed in three or four minutes; and its accuracy is ſuch, that frequently in ten trials, made with

with the ſame common and nitrous air, the difference of the reſult does not amount to $\frac{1}{200}$ of the bulk of both airs.

The different degrees of ſalubrity will be found in general to lie between 103 and 109; at leaſt, I found it almoſt always to be within theſe two extremes: that is to ſay, that of the bulk of the two airs the remaining column will be found to occupy between 103 and 109 ſubdiviſions. The magnifier applied to the braſs tube (D, Fig. I.) aſſiſts greatly the accuracy of the obſervation.

This ſimple method conſtantly ſhewed me all the variations in the conſtitution of the atmoſphere, in regard to its fitneſs for reſpiration, which I could diſcover by any other method.

A glaſs

A glaſs tube longer than that which I had at hand would bear a larger ſcale, and thus indicate with ſtill more accuracy the goodneſs of the air: but the two meaſures of air let up muſt not fill more than one half of the glaſs tube, for otherwiſe it could not be ſhook in the water without danger of ſome bubbles of air coming out of it, or ruſhing in it, by the force of ſhaking it up and down.

In the works of the Rev. Dr. Prieſtley, one meaſure of common air is ſaid to occupy ſometimes 120, and even more ſubdiviſions, which is owing to his peculiar method. He firſt joins the two airs together in a ſeparate jar, and allows them to ſtand a certain time to incorporate one with the other; after which, he lets them up in his large tube

tube exactly divided, and fees at once, without any fhaking, how much of the two airs is deftroyed. If this method is purfued accurately, and if the fame interval of time is obferved between joining the two airs and letting them up, the refult will, however, be found different in different experiments, as Dr. Prieftley makes no fcruple to allow.

I made a great many experiments to find out the reafon of this difference; but this tafk I leave to Abbé Fontana, who commenced his enquiries on this fubject prior to me. I will only relate one of my own, which will fhew the reader what refult he may expect from his experiments, though performed in the moft regular manner.

I filled a jar with common air, and put one meaſure of it, with one of nitrous air freſhly made, in five veſſels, each of a different diameter, to incorporate with each other without moving them: after an hour's time, I let up the airs of the different jars into the large tube or meaſure, when I found that the column of air occupied ſo much the greater ſpace as the veſſel in which they ſtood was of a leſs diameter; but none of theſe airs were diminiſhed near ſo much as they were when ſhook immediately together in the way above-mentioned. It is very remarkable, that I could ſcarcely reduce any of theſe airs afterwards to a leſs bulk, though I ſhook them very forcibly in the large tube after I had examined them.

125. The common air uſed for this experiment proved to be of ſuch a degree of goodneſs, that in ſix different trials, made one after another in the expeditious manner explained, the two meaſures occupied $1.06\frac{1}{2}$ exactly; whereas the ſame common air with the ſame nitrous air, after ſtanding an hour in the five different veſſels, gave the very different reſults expreſſed in p. 287.

By repeating the ſame experiments at different times, the ſame reſult was obtained as to the difference of the remaining bulk of airs kept ſtanding in veſſels of different diameters; but there was commonly a difference of ſome ſubdiviſions even in the experiments made with the ſame glaſſes.

The bulk of the two airs kept in the veſſel of the largeſt diameter occupied in the glaſs tube without ſhaking	1.10$\frac{1}{2}$,	And after being ſhook when firſt examined in the glaſs tube, 1.10$\frac{1}{2}$
That in the glaſs of the next in diameter	1.23$\frac{1}{2}$,	1.22
That in the glaſs of the next diameter	1.28$\frac{1}{2}$,	1.28
That in the following	1.35,	1.35
That in the glaſs of the ſmalleſt diameter of all	1.44,	1.43

POST-

POSTSCRIPT.

AS I went on with my experiments during the whole time this book was printing, I continued to difcover more and more the fecret operations of nature in regard to cleanfing our atmofphere. I have carefully regiftered in my notes the refult of the experiments, which I may poffibly communicate to the publick in a fecond volume, together with fome more deductions which I may draw from my remarks.

Though I am obliged abruptly to ftop my further refearches, I cannot difmifs the reader without acquainting him, that, as foon as the warm weather began to ceafe, and the autumnal colds to fet in (the thermometer of Fahrenheit being under 50 in the ſhade, which had been in the time of the other experiments in general between 70 and 83), the leaves, fruits, and roots had loſt a good deal of their miſchievous influence upon the circumambient air in the night, and by day in ſhaded places, though they had loſt nothing of their ſalubrious power in yielding by day dephlogiſticated air; but that the flowers ſeemed to have loſt very little or nothing of their malignant effluvia by which they contaminate the ſurrounding air; and that water ſtanding by itſelf, or with plants in it, loſes by the ſun-ſhine, or rather by the warmth communicated to it in the ſun, the faculty of promoting, or rather of not obſtructing, the plants yielding dephlogiſticated air; but that it recovers almoſt to an equal degree its former faculty, by the coldneſs of the night. Water, in which I found ice in the morning, and which the day before obſtructed the leaves in yielding a tolerable quantity of bubbles, was ſo much recovered, when it was heated by the ſun, that freſh leaves put in it yielded air-bubbles very briſkly, when the thermometer plunged in it was at 37.

From what has been ſaid in the nineteenth Section, as well as from other experiments, I am more and more induced to believe that our atmoſpheric air is a ſubſtance of a very changeable nature, and that it is, in common with a great many other ſubſtances, equally liable to become worſe, or of undergoing a kind of corruption by the increaſe of heat; and that this tendency to corruption is checked by the vital operation of the plants in the fummer, and by the cold in the winter. By this obfervation, we may perhaps be induced to believe, that thofe countries which are very hot in the fummer, and are little or not cultivated, as is a great part of Hungary and the country round about Rome, are not only expofed to have their air contaminated by the breathing of animals in it, and by the corruption of many other fubftances, but alfo by the corruption which the air itfelf is liable to undergo during the heat of the feafon; and which mifchief can chiefly be remedied by making a fufficient quantity of vegetables grow in them, principally trees. Draining the marfhes, and preventing inundations by keeping the rivers within their bounds by dykes, and by cutting canals to let out the waters, will greatly affift the operation of vegetables, which would be infufficient to cleanfe the atmofphere of low countries, without this great caufe of corruption, owing to marfhes, being removed.

EXPLA-

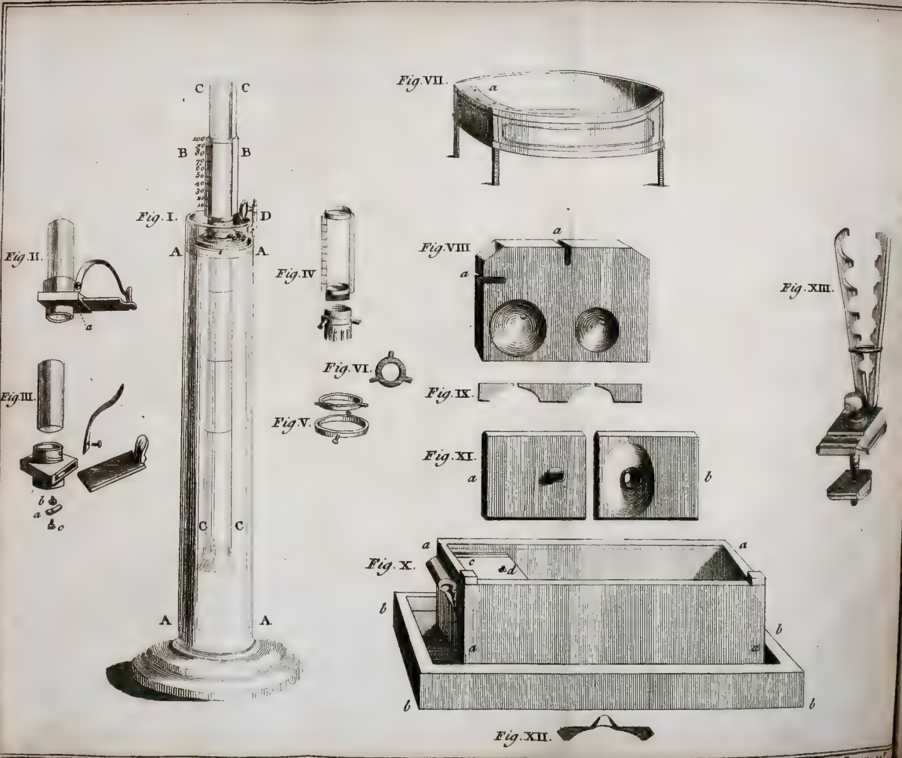
Fig. I.
C C
B B
100 90 80 70 60 50 40 30 20 10
D
A A
C C
A A
Fig. II.
a
Fig. III.
b
a
c
Fig. IV
Fig. VI.
Fig. V.
Fig. VII.
a
Fig. VIII
a
a
Fig. IX.
Fig. XI.
a
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Fig. X.
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Fig. XII.
Fig. XIII.

T. Bowen sc.

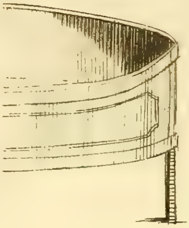


Fig. II.

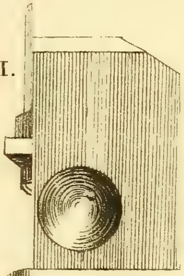
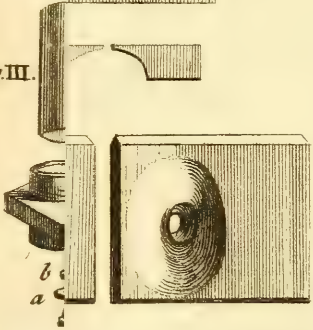


Fig. III.



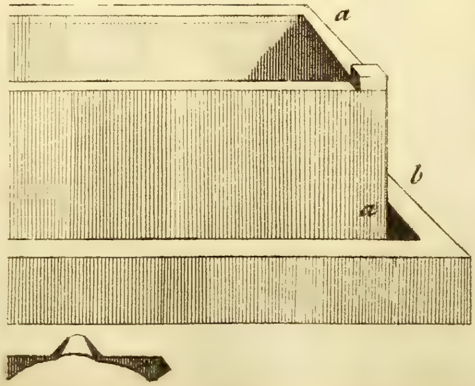


Fig. XIII.

T. Bowen sc.t

Explanation of the Figures.

Fig. I. THE great meaſure, or great glaſs tube, in its ſituation in the experiment of examining air, with the braſs ſcale upon it, and the magnifier ſtuck to the braſs tube, on purpoſe to adapt accurately the firſt mark of the ſcale to the beginning of the column of air. The glaſs tube is ſuſpended upon the two braſs rings or gingle (in the way the common ſea-compaſſes are ſuſpended) to keep it always in a perpendicular ſituation. AAAA is the braſs tube full of water, in which the glaſs tube fixed to the braſs meaſure is ſuſpended. This braſs tube is repreſented tranſparent on purpoſe to ſee in what manner the

the glaſs tube is ſuſpended in it, B B, the braſs ſcale of three Paris inches divided into an hundred parts, C C C C, the glaſs tube or great meaſure, whoſe lower and open extremity is ſecured by a braſs ferrule. D, the magnifier for the more accurate obſervation.

Fig. II. The ſmall tube or meaſure fixed in its braſs ſocket. *a*, is an elaſtic piece of braſs, having a pin paſſing through a hole in the under part of the ſocket, which pin is puſhed upwards by the elaſticity of the piece *a*, and enters in a cavity on the under ſide of the ſlider, made on purpoſe to ſtop it, and to prevent the bended ſteel ſpring from forcing the ſlider entirely out of the ſocket. N.B. I think that this machine may do very well

without

without the bended ſteel ſpring, and therefore I did not put it to that which I made uſe of.

Fig. III. The ſmall tube or meaſure, with the braſs ſocket, ſlider, and ſprings, all taken aſunder to ſee their ſhape. The ſcrew *c* under the elaſtic piece *a*, is to be fixed to the under part of the ſhoulder of the braſs ſocket (at *a*, in Fig. II.), to fix the elaſtic piece *a* to it. The pin *b* ſupported by the piece *a*, and paſſing through a hole in the under part of the ſhoulder of the ſocket, enters a cavity made in the under ſide of the braſs ſlider, when this ſlider is drawn out as in Fig. II. and thus ſtops its coming out entirely.

Fig. IV. The braſs ſcale with its under-piece to be ſcrewed to it, and ſerving to embrace cloſely, by its ſpring, the glaſs tube, ſo as to be ſuſpended by it upon the braſs gingle, expreſſed in Fig. V. The inſide of this under-piece ought to be lined with a piece of ſponge, on purpoſe to preſs ſoftly againſt the glaſs, and to prevent its being ſcratched by ſliding the glaſs tube up and down againſt the braſs.

Fig. V. The gingle, or the two braſs rings, ſuch as are uſed in common ſea-compaſſes, whoſe moveable axes act contrary to one another, to give the body ſuſpended upon them every poſſible motion, and thus to keep it in a perpendicular line.

N. B.

N. B. I keep my tube ſuſpended ſimply upon a braſs ring, ſoldered a little way within the braſs tube, which does very well.

Fig. VI. That part of the under-piece of the braſs ſcale by which it is ſupported upon the gingle.

Fig. VII. The wooden-trough full of water, in which the whole apparatus is uſed. This trough ought to be (in the inſide) 2 feet long, 13 inches deep, and 17 inches wide. The board *a*, upon which the jars, &c. are placed, ought to be fixed at the diſtance of 3¾ inches from the brim; the length of the board ought to be about 9 inches, the thickneſs of it two inches. The trough ought to be kept full of of water except about two inches from the brim.

Fig. VIII. The board (expreſſed by *a*, in Fig. VII.) by itſelf, and inverted. It has two funnels hollowed out on its under-part, which is in this figure repreſented uppermoſt: the orifices of the funnels are repreſented by the two round holes, one of which muſt be larger than the other. *a a* repreſent two oblong inciſions to receive the extremity of the bended tubes, through which the various kinds of air are let up into the inverted jars placed upon the board.

Fig. IX. A cut of the two funnels hollowed out in the board, repreſented by Fig. VIII.

The

The reſt of the figures are only intended for thoſe who ſhould like to engage farther in this entertaining branch of natural philoſophy, and to produce thoſe kinds of air which are liable to be altered or abſorbed by water.

Fig. X. A wooden trough to be filled with mercury, for ſuch experiments with air as cannot be done in water. Many kinds of airs are abſorbed themſelves by water, as is fixed air, and all thoſe aërial fluids which ſhould rather be claſſed among vapours, as alkaline air, acid air, &c. of which an account may be ſeen in the works of Dr. Prieſtley, and which will ſoon be treated in a more ample manner by Abbé Fontana.

Air

Air extracted from ſpars cannot be examined, nor even obtained, but by making uſe of mercury inſtead of water: for this ſingular air, which corrodes glaſs, is immediately reduced into ſtone by the firſt contact with water.

This trough conſiſts of two different ſtrong wooden boxes. *a a* is the box containing the mercury; it is in the inſide 11½ Paris inches long, 4 inches and 2 lines deep, and 4 inches and 2 lines wide. The board *c* is placed at one inch and 2 lines diſtance from the brim, and is 7 lines thick. The orifice *d* of the funnel hollowed out of the under-ſide of this board is two lines above the ſurface of the board. This box is placed within another larger box equally ſtrong, *b b b b*, which

which ſerves to receive the mercury ſpilt by moving the veſſels in the other box.

Fig. XI. The board (repreſented by *c* in Fig. X.) of the box containing mercury, repreſented in *a* as it is fixed in the box; and inverted in *b*, on purpoſe to ſee the funnel hollowed out in the underſide of it.

Fig. XII. A ſection of the board of the box (repreſented by Fig. X. *c)*, on purpoſe to ſhew the form of the funnel, and the manner of fixing this board in the box, by letting its ſloping edges in a groove cut out in the ſubſtance of the box, ſo that the mercury cannot puſh it up, but that it may be taken out at pleaſure.

Fig. XIII.

Fig. XIII. A kind of *forceps* or tongs, to receive the necks of different veſſels, in which air is to be extricated by heat. It is fixed by the ſcrew to the brim of the water-trough, or to the box of mercury; and the neck of the glaſs veſſel is ſqueezed between the two branches by means of the moveable ring, by which they may be more or leſs ſqueezed together, according to the ſize of the neck of the glaſs. This inſtrument is very uſeful for different operations, which, without its help, would require an aſſiſtant to hold the glaſs to keep it from falling.

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*** p. refers to the number of the Pages; and exp. to that of the Experiments.

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