

New experiments and observations touching cold, or an experimental history of cold, begun. To which are added an Examen of antiperistasis, and an Examen of Mr. Hobs's doctrine about cold / by the Honorable Robert Boyle ... ; Whereunto is annexed an account of freezing, brought in to the Royal Society, by the learned Dr. C. Merret. A Fellow of it.

Contributors

Boyle, Robert, 1627-1691.
Merret, Christopher, 1614-1695.
Hob, Mr.
Royal Society (Great Britain)

Publication/Creation

London : Printed for John Crook ..., 1665.

Persistent URL

<https://wellcomecollection.org/works/pmvdncag>

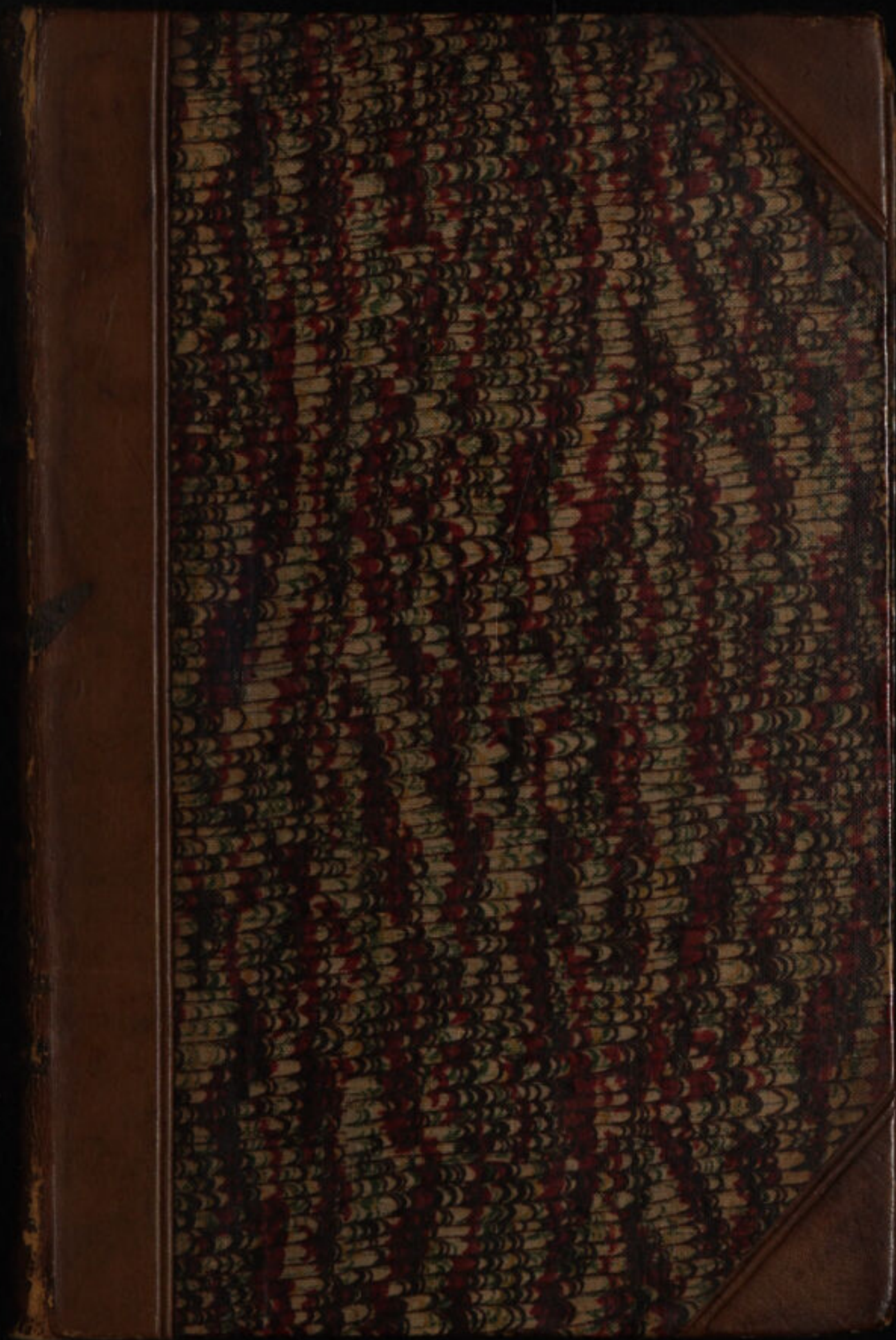
License and attribution

This work has been identified as being free of known restrictions under copyright law, including all related and neighbouring rights and is being made available under the Creative Commons, Public Domain Mark.

You can copy, modify, distribute and perform the work, even for commercial purposes, without asking permission.



Wellcome Collection
183 Euston Road
London NW1 2BE UK
T +44 (0)20 7611 8722
E library@wellcomecollection.org
<https://wellcomecollection.org>



BOYER'S
EXPERIMENTAL
HISTORY OF GOLD







2/1/79

15022/A

Nix. e

17

Fulton 70

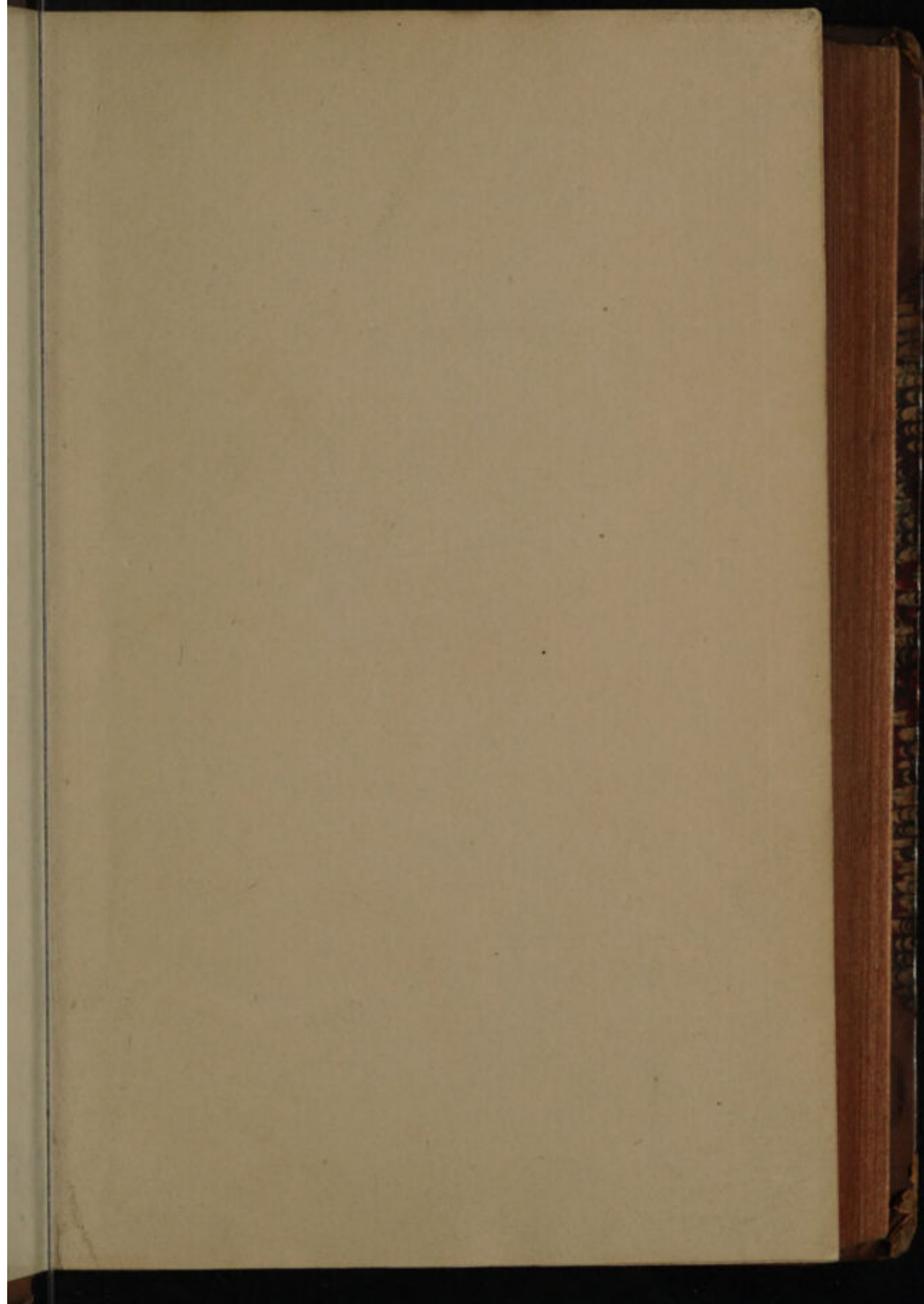
15B.20

28837

Payne
12/1/11

BOUND BY C. LEWIS.

2



NEW
EXPERIMENTS
- AND
OBSERVATIONS
TOUCHING
C O L D,
OR AN
EXPERIMENTAL HISTORY
OF
C O L D,
Begun.

To which are added
An *Examen* of *Antiperistasis*,
And
An *Examen* of Mr. *Hobs's* Doctrine
about C O L D.

By the Honorable *Robert Boyle*, Fellow of
the ROYAL SOCIETY.

Whereunto is annexed *An Account of Freezing*,
brought in to the Royal Society, by the learned
Dr. C. Merret, a Fellow of it.

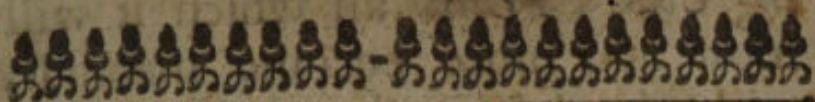
*Non fingendum, aut excogitandum, sed inveniendum, quid natura faciat,
aut ferat, Bacon.*

L O N D O N.
Printed for *John Crook*, at the Sign of the Ship
in *St. Pauls Church-yard*, MDCLXV.

2

28837





THE
PUBLISHER
TO THE
INGENIOUS READER.

I Am fully perswaded, you will much rejoyce to see that Exquisite searcher of Nature, the Illustrious *Robert Boyle*, come abroad again, as knowing he never does so, but when richly turnisht with very Instructive and Useful matter. He presents you here with a Treatise of *New Observations and Experiments, in order to an Experimental History of Cold*. This is the Body of the Book, but it comes accompanied with some *Preliminaries*, and an *Appendix*, whereof the former contains *New Thermometrical Experiments and Thoughts*, the latter an *Exercitation* about the *Doctrine of Antiperistasis*, followed with a short *Examen of Mr. Hobs's Doctrine, touching*

a 2 ing

2
ing Cold. From all which it will more and more become manifest, with what spirit and care this Excellent Person advanceth real Philosophy, with what exactness he pursueth his Engagement therein, and how great caution he useth, that nothing may slide into the Philosophical store, that may prove prejudicial to the Axioms and Theories hereafter perhaps to be deduc'd from thence.

Having thus shortly given you my sense of the substance of this Considerable Treatise, I am now to advertise you of one or two circumstances, necessary to be taken notice of in its perusal.

One is, that the Noble Author being at *Oxford*, when the Book was printed at *London*, he hopes the Reader will not impute to him the Errors of the Press, which yet he is perswaded will not be many, and out of which must be excepted a Blank or two, occasion'd by this, That the Authors Papers being near two years since given to be transcribed to one, whose skill in writing was much greater, than (as it afterwards appeared)

pear'd) his knowledge of what was, or was not good sense, or true English ; this person suddenly going for *Africk* before the Transcript had been examin'd, and not taking care to leave all the first Copy, the Author found, (besides several Blanks, that he filled up out of his Memory, or by repeating the Experiments, they belonged to) one or two, where he was not able to repair the Copists omissions : And besides unexpectedly met with very many Passages so miserably handled, that by putting him to the trouble of writing almost a New Book, when part of this was already in the Press, it much retarded the Publication of that, which now comes forth.

The other is, That, whereas in the Preface some passages are so penned, as to suppose the Book to be published early in the Winter, the Reader is to be advertis'd, That the former part of the Preface was sent a good while since to the Press, though the latter, however then written out, was hindred from accompanying it, by some hopes of the Authors to gain

by delay an opportunity (he missed of) to perfect an Experiment he was desirous to insert ; and that, when the Frost began, which was late in the season, the Coldness did within a while arrive at that degree, that by its operation upon the moisten'd paper, it long put a stop to the Proceedings of the Press. But the Author, that he might neither be quite defeated of his aim, nor disappoint the Curious of their Expectation, did in the first or second week of the Frost, which was about the end of the year 1664. present the *Royal Society* with divers Copies of the *History of Cold*, though the Book were not then quite printed off. And these Books being so near finish'd, that of 21. Sections, whereof the *History of Cold* consists, the Press had then reach'd to about the 19. and I had the 20. in my hands to supply it, when the weather should permit ; the Author hop'd, that by seasonably communicating so much of his intended Treatise to so many of the *Virtuosi*, that were the likeliest to make use of it, he had pretty well provided against
the

the Prejudice, that might otherwise accrue from the flownels of the Press, and therefore allow'd himself to subjoyn to the *History*, the discourse of *Antiperistasis*, and the *Examen* of Mr. *Hobs's* Do-

ctrine *, as belonging to the same subject :

** It was thought needless to insert Mr. Hobs's Scheme, touching this subject, because it only shews, that Wind is the cause of Cold.*

And finding the frosty weather to continue later, than was expected (which had he foreseen, before his *History* was printed off, it would have given him opportunity of Enlargements) he hopes the Publication may not be yet too late for diligent Readers, to make some use of the season for examining his Experiments, or trying some of the New ones, those may suggest. And therefore for the quicker dispatch of the Book, he purposely omits, and reserves for another occasion, besides the papers, that he hath not yet given me, some that I have already in my hands. And 'tis, I presume, for the same reason, that he forbears to publish, what he long since writ about the *Origine of Forms and Qualities*, in a small
Tract,

2
Tract, which he had thoughts of
sending forth in the company of the
ensuing *History*, as a Discourse fit to
be an Introduction as well to That,
as to his Historical writings about *Colours*
and some other Qualities.

This is all the Advertisement I had
to give you. And seeing it would be
altogether impertinent, for me to
take any pains, or to use any Art to
procure a Gust for a Book, compos-
ed by Mr. *Boyle*, I have no more to
say, but that the Author being so
Generous as to oblige Forrain Nati-
ons as well as his Own, has already
taken care of having it put into La-
tine, Farewel.

London, March 10.

1664
5

H. O.

THE



THE AUTHORS
P R E F A C E
INTRODUCTORY.

Cold is so barren a subject, and affords so few Experiments, that are either very delightful for their surprizing prettiness, or very considerable for their immediate use, that instead of admiring, that any of my friends should wonder at my having been induc'd to write of such a Theme, I freely confess, that I have been sometimes tempted to wonder at it my self; and therefore I think my self oblig'd to give my Readers an account of these three things, Why I thought fit to write of Cold at all? For what Reasons I have treated of it after the manner to be met with in the ensuing Book? And, Why I venture my unfinished Collections about it, abroad so soon?

I. To satisfy the first of these Queries, I have several things to say.

b

And

The Preface

2
And first, That the subject I have chosen is very noble, and important; for since Heat has so general an Interest in the Productions of Natures Phænomena, that (Motion excepted, of which it is a kind) there is scarce any thing in Nature, whose Efficacy is so great, and so diffus'd, it seems not likely, that its Antagonist, Cold, should be a despicable Quality. And certainly Cold, and Heat, especially when imploy'd by turns, are the two grand Instruments by which Nature performs so many of her Operations here below, that our great Verulam did not speak inconsiderately, when he call'd Heat the Right hand of Nature, and Cold her Left. And though in our temperate Climate the Effects of Cold seem not to be very remarkable, yet besides that, in more Northern Regions they are oftentimes stupendious, the Nature of that Quality must needs be very well worth our considering, if it were but for the Power it has to moderate and check the Operations of Heat, upon which account alone, if there were no other, it may be look'd upon as so considerable a Quality, that even lesser Discoveries about it, may both be acceptable and prove useful.

In

Introductory.

In the next place I shall represent, that notwithstanding Cold's being so important a subject, it has hitherto been almost totally neglected. For I remember not, that any of the Classick Authors, I am acquainted with, hath said any thing of it that is considerable. They do indeed generally treat of it, as one of the four first Qualities. But that which they are wont to say, amounts to little more, then that 'tis a Quality that does congregate both things of like, and things of unlike nature. The Unsatisfactoriness of which vulgar Definition, I had some years ago an occasion to manifest (in another Treatise.) Sceptical
Chymist. And having given us this inconsiderate Description of Cold, they commonly take leave of the subject, as if it deserved no further handling, then could be afforded it in a few Lines, wherein indeed they say too much about it, but not enough. And even among other Writers of Bodies of Natural Philosophy, or of the Doctrine of Meteors it self, the Reader will find, how little of true and pertinent has been contributed to the ensuing History of Cold. And though among the Vulgar, and the Writers that adopt their Traditions without examining them,

The Preface

2
I find some few particulars delivered, touching Cold; yet some of them are so untrue, and others so uncertain, that they have furnished me with little else, then the necessity of Questioning, or of disproving them: so that when I considered all these things, I could not but take notice, that very little has been hitherto said of Cold, by those Schoolmen, and other Writers, (that I have yet met with) who have professedly, (though but perfunctorily, and, as it were, incidentally) treated of it. But yet instead of thinking it a Discouragement, that so many Learned Men, to whom that Quality could not but be obvious, and to whom it was as familiar as to me, had in so many Ages said little or nothing of it to the purpose; I found this very thing an invitation to my attempt, that I might in some measure repair the Omissions of Mankind's Curiosity towards a subject so considerable, and so diffus'd, by trying what I could do toward founding the History of a Quality, which has been hitherto so neglected, as if all men judg'd it either unworthy of being cultivated, or incapable to be improved.

Another inducement to me was, that
having

Introductory.

having six or seven years ago written some Tracts (though I have not since had opportunity to publish them) in order to the History of Heat and Flame ; it seem'd the more proper for me to treat of the contrary quality, Cold, since according to the known rule, confronted Opposites give themselves a mutual Illustration. And another inducement of almost the same Nature, was afforded me by remembering, that whereas Cold in its higher degrees, is wont to be communicated to us by the Air, (whencesoever the Air has it,) and I have on several occasions been oblig'd to treat of divers Properties of the Air, as of its weight and spring (in my Physico-Mechanical Treatise) of the several strengths of that spring, in proportion to the degrees of the airs Condensation, the Experiments of which, reduc'd into Tables, were first publish'd (and for ought I yet know made) by us, (in the defence of that Book against Franciscus Linus *) and of divers other Qualities of the air in several passages of our other writings, which 'twere now superfluous to take notice of ; all this made it appear convenient enough, that among other Attributes of the Air, which we

* Chapter
the fifth of
that Treatise.

2
 either have had, or expect to have occasion to treat of, so eminent and diffus'd a one as its Coldness, should not be left untouch'd by the same Pen.

But though neither any, nor all these inducements had been sufficient to ingage me to draw together, and recruit my Observations concerning Cold, there was another, that could not miss of prevailing, The Command of the Royal Society, impos'd on me in such a way, that I thought, it would less misbecome me to obey it unskilfully, then not at all. Especially since from so Illustrious a Company (where I have the Happiness not to be hated) I may in my endeavours to obey and serve them, hope to find my failings both pardoned, and made Occasions of discovering the Truths, I aim'd at.

II. After this Account of the Motives that induc'd me to resolve to draw together the Notes I had on several occasions set down, about the Phænomena of Cold, it may be now expected, that I render some reason, why I have thus digested them, and why I have not written the following Treatise in a more accurate way.

First then I readily acknowledge, that the Method is not exact. Nay, that it is less

Introductory.

less so then the Scheme of heads of Inquiry, that I drew up to give my self a general Prospect of the subject I was to handle. But when I had considered, how comprehensive a Theme I had pitch'd upon, and how much more comprehensive, future discoveries and hints might make it, I thought, it altogether unadvisable for me, that had no more time, nor no more opportunity then I had, when I began to compile the following History, to engage my self to a method, according to which I was not perhaps able to treat of any one of the principal parts of the designed History. And yet on the other side, being unwilling to huddle my Experiments confusedly together, I thought it an expedient, that might in great part decline both those Inconveniences, to draw up a company of comprehensive Titles, under which might commodiously be rang'd most of the Particulars I had observ'd, reserving those few, that were not so easily referable to any of those, to be thrown at last into a Section by themselves. And this I the rather did, because I would not, by a Confinement to a strict method, discourage others from continuing the History, by adding new Titles to those 21. I have

The Preface

2
treated of, as well as by inserting other Experiments or Observations in any of them.

That the Sections or Titles are very unequal, will not, I presume, be much blam'd by them, that consider, that my Design being to set down Matters of Fact, not write a complete and regular Treatise, the length of each Section was to be determined not by its Proportion, to that which went before it or followed after it, but by the number and condition of the Particulars that were to compose it. And I thought it much more pardonable, that any of the Sections should be disproportionately short, then lengthened either by untruths or by impertinencies.

Some of the accounts will probably to some Readers appear too prolix; and I could very easily, as well as willingly, have prevented that objection, if I had not more consulted the scope of the Book, then the ease or Reputation of the Writer. But my design being, not only to gratifie some Readers, but to assist others to prosecute the work I had begun, as the Experiments are most of them new, and many of them tri'd by methods hitherto unpractis'd; I conceiv'd my self oblig'd to set down somewhat

Introductory.

what circumstantially, not only the Events, but the Manner of my Trials, that I might at once, both the better satisfy the scrupulous, and be assistant to those that would examine or repeat such Experiments, and also gratify those, who are pleas'd to think, that a somewhat assiduous Conversation with Nature, may have given me some little faculty in devising Experiments, and the ways of making them, above those that have been conversant only with Books and Notions. And in some of the following Trials I was the more induc'd to set down all the principal circumstances, because, that being not to be made, but by the help of Glasses skilfully shap'd, and Hermetically seal'd, and other Instruments and Operations, that require more tools, and more of manual Dexterity, then every ingenious Man is Master of; 'tis very likely that most Readers will not be able, or perhaps willing, to reiterate such Trials, and therefore will be glad to find them so deliver'd, as that they may without too much danger acquiesce in them, as being made with Diligence, as well as Faithfulness. The latter of which Qualities will, I presume, be allow'd me, as well upon the account of
the

The Preface

2
the plain and simple way, wherein matters of fact are delivered in the following Book, as upon the score of the Testimonies, that even Adversaries, as well as others, have thought fit to give to the Historical part of my former Treatises. And (to intimate That on this occasion) this strict Fidelity to Truth I scruple not to own, though perhaps it may be attended with an inconvenience in point of Reputation, that writers of less Veracity are less exposed to. For I have found by Experience, that some Men, who probably would not mention the Experiments of most others, without vouching their Authors, for fear of losing their own credit, in case the thing related should not prove true; have, without taking the least notice of me, made use of such Experiments of mine, as I have strong motives to think they never made nor saw, only because they had been related by one, after whom they thought they might without a hazard of their credit deliver any Matter of Fact. And the liberty that some have allow'd themselves in adopting my Communications (such as they are) is notorious enough to have been publickly complain'd of more than once, by Persons that are meer
strangers

Introductory.

strangers to me. But though I had not the Probability, which the Notice, that begins to be taken of it, seems to give me, of having some Justice done me: yet Veracity is a Quality that does, I think, so well become a Christian and a writer of Natural History, that I had much rather undergo any Disadvantage, I may be subject to for it, then decline the Practice of it. But to return to the following History.

I confess the Prolixity of some passages of our History is increas'd by the transitions, excuses, and suspitions that are made use of in them; but I confess too, That if this way of writing be a fault, it was not always caus'd by inadvertency. For as to what is said to connect the parts of our History together, or excuse the not prosecuting of this or that particular Trial, the heedful Reader may oftentimes perceive, that they contain in them, though not perhaps conspicuously, either cautions, or advertisements, or hints, not impertinent to my main scope, and improvable by an attentive Peruser. And as for the suspitions and scruples to which now and then I may seem to have too long indulg'd, I had two or three inducements to invite me to what I did. For the men-
tion

The Preface

tion of conjectures, that every Reader was not so likely to light upon, might more conduce, then at first one would think, to the main design of my Book, which was to begin, and promote the natural History of Cold, since these suspitions about the causes and scruples about other things, relating to our Experiments, may probably produce, not only new reasonings and Inquiries, but new Trials to clear the difficulties and determine the doubts. Besides I thought it not amiss to take such occasions to make some Readers sensible, that to make indubitable inferences even from certain Experiments, is not near so easie a work, as many are pleas'd to imagine. And whereas I was not without inducements to think, that some Critical and Sagacious Readers, will not only excuse my having taken notice of so many scruples, but wish I had mov'd more on some occasions, and propos'd some in certain cases, where I have not mention'd any, I thought it might invite such Jealous Readers to think, that I foresaw divers little difficulties and scruples, that might be mov'd in several cases, where I have not expressly taken notice of them, either because I judg'd them easie enough to be answered.

Introductory.

answered without my help, or because the things themselves were not considerable enough to deserve a long or solicitous discourse to clear them, especially from a Writer, that being often tir'd himself in examining such Niceties, was affraid he should too much tire the generality of his Readers, if he should too frequently insist upon them.

If it be objected, that notwithstanding some things are set down prolixly, yet other Experiments, that might properly be referred to some of the Titles I handle, are wholly omitted; I answer, that this were indeed a fault in one, that should pretend to write full and accurate Discourses of the subjects propos'd in his Titles, but not in me, who do not at all pretend to say under each head all that may be pertinently referr'd to it, (for that may probably be a great deal more then is yet come to my Knowledge) but only those Particulars, that I myself have tri'd or observ'd, or at least have receiv'd upon credible Testimony. And perhaps some amends may be made for these Omissions, by my having frequently enough mention'd the Experiments, that, when I propos'd them, I had only design'd or attempted without perfecting

The Preface

perfecting them. For the Experience of many Ages has shewn us, that hitherto, not only men do not appear to have made any store of Trials concerning Cold, but seem not to have so much as design'd it. And therefore it seem'd not unreasonable to presume, that it would prove an Assistance to the Generality of Readers, if probable and practicable Experiments were propos'd to them. And since 'tis the improvement of the subject that I aim at, by whomsoever it may happen to be improv'd, I thought it but reasonable to use my endeavour, that those Experiments, which for want of opportunity I myself could not try, might be tri'd by others, who may be befriended by more favourable Circumstances. Nor is that great Ornament and Guide of Philosophical Historians of Nature, the Lord Verulam himself, asham'd to substitute, on I know not how many occasions, his *Fiat Experimentum*, that is a precept or a wish to have an Experiment made, instead of an Account of the Experiment made already. And yet in this mention of things, I could wish to have tri'd, I have been far more sparing then every Reader will take notice of. For I judg'd it not discreet to
mention

Introductory.

mention all the Experiments I had thought upon, or even already set down in several Catalogues, lest they should appear extravagant to those, that are unacquainted with the several Notions, and trials, and contrivances, which made them appear to me not irrational, and which yet 'twould have been tedious, and not worth while to have particularly mention'd.

But that in what we have newly (and a little before) had occasion to say of our ways of making Experiments, our meaning may not be misconstru'd, we must here Advertise the Reader, that though in many of the following Experiments, the contrivances will not perchance be dislik'd, yet in many others they are far enough from being such as might have been propos'd by one, that had wanted no Accommodations fit for such a work as ours. But I was reduc'd to make many of those Experiments in a Village, and whilest I was writing them, was obliged to make frequent Removes, by which means I seldom had the liberty to make my Trials after such a manner, as I could contrive them, if I could have Instruments and other assistances to my wish. For some-
times

The Preface

times I wanted conveniently shap'd Glasses, sometimes the Implements necessary to seal them up with, sometimes such ingredients as I needed to work on, oftentimes frosty Weather, for the freezing of Bodies to be expos'd to the open Air, and not seldom Ice and Snow for Artificial congelations; sometimes Weather-glasses, especially seal'd ones, two of which being unluckily broken after one another, kept me from being able to make divers considerable Experiments; sometimes tender Scales, and sometimes other Mechanical Instruments of several sorts, and more then sometimes (for it happened very frequently) I wanted time so to prosecute and finish the Experiments, as to satisfy myself about divers circumstances, which, though possibly few Readers will take notice to be wanting; I would gladly have observ'd, if I had not been hindered, not only by the haste I was often fain to make for fear of losing a frost, but the importunity both of other Avocations, and even of the distraction given me by the multitude of Experiments, which haste made me prosecute at once. And indeed, as in divers others of the Treatises, I have occasionally written, so particularly in a
great

Introductory.

great part of this History of Cold, my writing in places, where I wanted such Mechanical Accommodations, as I could have wished for, and devis'd, has reduced me oftentimes both to leave Experiments untri'd, that would have much illustrated my subject, or clear'd the difficulties of it, and contrive several of those I mention, not after the best manner that might be, but after the best manner, that was practicable by the accommodations I was then able to procure: so that it need not be wondered at, or blamed, if in some passages of these Papers, Experiments to the self same purpose are more accurately tri'd, or by more Expedient ways at one time then another. For as a Physician, if he come to practise in the Country, where Apothecaries Shops are but ill furnished, both as to the Number and as to the Quality of the Drugs, must accommodate his Practise to the scant Materia Medica, of which alone he has the command: So when I write of Experimental matters, in places where I cannot have Workmen, nor Instruments fit for my turn, I must be content to vary my Experiments accordingly, and sute them to the accommodations I am confined to, which, though

The Preface

it be an unwelcome Condition, is made the less so to me, by a Hope, that the Equitable Readers will think it to be all that a man is bound to do in such cases, to procure the best assistances he can, and manage those, he is able to procure, to the best Advantage.

And this I the rather take notice of on this occasion, that ingenious men might not be too much discouraged by imagining, that, because they live in the Country, or upon other scores cannot furnish themselves with the best Instruments and accommodations, nor enjoy the assistance of the skilfullest Artificers; they are either Unqualifi'd for the making of Experiments and Observations, or Superseded from it. For though in some cases, where the measures of things must be nicely determin'd, and principally in Observations, whereon either Theorems or Hypotheses about the Proportions of things are to be grounded; very good Instruments are exceeding useful, and sometimes necessary: yet there are thousands of particulars, whose knowledge may be instructive to those, that Are or would be Naturalists, where no such Nicety is requisite, and where the measuring

Introductory.

ring things by Ounces and Inches will serve the turn, without determining them to Lines, and to Grains. And even in cases, where Exact observations are (to some purposes) Requisite, those that are not so, may be oftentimes very useful, by affording Hints, by which others may be excited and assisted to make those more accurate Trials. And here let me take notice, that a Tool or Instrument is not therefore to be despised, if it be Proper enough to the Particular use to which 'tis appli'd, because some more Mechanical head or hand, may propose or make another, that is more Artificially contriv'd, or more Neat and Portable, or that will also perform Other things, then that we are speaking of. For there is a vast multitude of Physical Phænomena, wherein Mathematical exactness is not necessary, and Observations about these, may be well enough made by divers Other ways, then by the most Artificial, that Can be devis'd. As though a fine Watch may have these Advantages, That it is a neater thing, and more portable then an ordinary Clock, that it may be improv'd by the Addition of an Alarum, and that it may also perhaps shew the Day of the

C 2

Month,

The Preface

Month, the Age of the Moon, the Tides, and divers other things, of which the Clock shews not any; yet an Ordinary Clock may serve to measure an hour by, as well as this finer Engine; and so may a Sun-Dyal, and many other Instruments on divers occasions, though in other Cases, and other Regards, they be far less commodious, then either a Watch or Clock. Besides, that in many cases a skilful Naturalist will by a variety and collation of Experiments, make the same discoveries, and perform the same things, for which others are wont to be beholding to Instruments, and perhaps do many things without them, that have never been done with them. And since Necessity is proverbially allow'd to be the Mother of Inventions, even in Tradesmen, and Vulgar heads, why should we doubt, but that the rich and inventive Intellect of a Philosopher, may in cases of necessity turn it self, and contrive the things it can dispose of, into so many differing forms, that it will often make its own Sagacity and Industry supply the want of exact Tools and Instruments. And these Considerations that tend to keep ingenious Men from Dispondency, I therefore think fit to Inculcate,

Introductory.

culcate, because the Common-wealth of Learning would lose too many useful Observations and Experiments, and the History of Nature would make too slow a Progress, if it were presum'd, that none but Geometers and Mechanicians should imploy themselves about writing any part of that History.

But to return to those Trials of our own, that occasioned this (as I hope, Seasonable) Digression, I was about to add, That as the acknowledgement I was making, that some of the Trials were for want of Accommodations less Artificial than I could have design'd or wish'd them, touches not all, nor haply the greatest part of the following Experiments; so it need not derogate from the Readers reliance on those which it does concern. For though some of them might have been more Artificially performed to the manner, yet they could not have been more Faithfully registred, as to the Events. which though I dare promise my self, that most Readers will be induc'd to believe, upon the Considerations not long since intimated; Yet I think it requisite to give this intimation on this occasion, because, that though I have * elsewhere largely manifested to

* The two Essays of the Unsuccessfulness of Experiments.

The Preface

what contingencies divers Experiments are liable, yet I have found very few, whose events are so subject to be varied, by slight and not easily heeded circumstances, as several Experiments concerning Cold: Where oftentimes the degree of that Quality, or the time during which it continues appli'd, or the manner of Application, or the thickness, shape and bulk, &c. of the vessels that contained the matter expos'd to it, may have a far greater influence on the success, than those, that have not tri'd, can easily imagine. And it increases the difficulty, that these Experiments of ours being (very few excepted) the only that are yet made publick concerning Cold; we cannot so easily, as in other cases, free our selves from the doubts, that may be suggested by different events, by comparing together several Experiments of the same kind, though to obviate this inconvenience, as far as I may, I have divers times in cases, where the Experiments seem'd like to be thought strange, or to be distrusted, set down several Trials of the same thing, that they might mutually support and confirm one another.

Of those Contingent Experiments
about

about Cold, I was newly speaking of, the Reader may meet with an eminent Example in the 21. Title, where mention is made of the differing Effects of Air blown out of a pair Bellows upon a Weather-glass: and as for the suspicion I there conclude with, (though I yet doubt, whether it will reach All the Cases incident to that Experiment) I have since been confirm'd in it, by finding, that by purposely varying the temper of the Bellows themselves, I could divers times considerably vary the operations, which the Winds, blown out of them in their differing states, had upon the Liquor in the

Weather-glass *

Of this I expect to have an opportunity of saying more, and therefore shall at present add but this one particular, which may sufficiently justify me for having said, That Weather-glasses &

* Another remarkable instance of the variable success of the Experiments of Cold, I have met with in an Experiment of the Learned Dr. Merrets, about the congealing of oyl of Vitriol. For though I expos'd that Liquor in small vessels of differing sizes and shapes, and even in slender glass-Pipes, seal'd but at one end, yet neither the Cold of the Air in frosty nights, that were extraordinarily sharp, nor which is more, our frigorifick mixture of Ice and Salt, would make the Experiment succeed, notwithstanding that we tri'd it with several parcels of Oyl of Vitriol. And yet, that the Learned Doctor by the help of the Air alone (for he uses not our frigorifick mixture) did bring that Liquor, either to a true congelation, or a coagulated substance, that look'd just like Ice; both some eminent Virtuosi, and I my self, who had the Curiosity to examine it, can bear him witness.

our Sensories may give very differing Informations about the Temperature of the Air turn'd into Wind, by being blown out of the same pair of Bellows. For having taken two Hermetically seal'd Weather-glasses furnished with highly rectified Spirit of Wine, and purposely made for my Experiments, by a person eminently dexterous in making such Instruments, and having likewise provided a large pair of Bellows, I found, that by blowing 20. blasts at a time on the Ball of one of them, though the Pipe were not only slender, but of an unusual length, amounting to about 30. Inches, yet the Liquor did not sensibly subside any more then rise. And in the other Weather-glass, whose Pipe was less long, but whose Ball was purposely made far greater to be the fitter for short and nice Experiments, we found more then once, and (that as well in the cold Air, as in a close Room) that the wind that was blown in divers blasts out of the Bellows, against the lower part of the Instrument, did not only make the Spirit of Wine subside, but did make it manifestly, though but very little, ascend. And 'tis not necessary, for the making good of what I taught, that such Trials should always
 Succeed

Introductory.

succeed just as these did, since it may suffice to prove what I pretended, that a good seal'd weather-glass did divers times discover the Wind to be rather warm, then cold, when upon Trial (then purposely made) it felt not only manifestly, but considerably cold, both to a By-standers Hand, and to my own Hand and Face, though my hand, that was blown upon, were immediately before more then ordinarily cold.

And I shall here add, That judging it fit to make further Trial, with an unseal'd weather-glass, I made one, that was in some regards preferable to those mentioned in the second Preliminary Discourse, by making the Bubble large, and the Cylindrical Pipe so proportion'd to it, that instead of a Drop of water, a Pillar about an Inch long of that Liquor was kept suspended, and play'd as well conspicuously as nimbly up and down in the Pipe. And having fastned this Instrument in an erected Posture, with the Spherical part uppermost, to the inside of a Window, by blowing upon the Ball with the Bellows above mentioned, which had lain some hours not very far from the Chimney-corner, (but without seeming to be

The Preface

be sensibly warm'd by the neighbourhood of the fire) a very few blasts made the suspended water hastily subside, (and thereby witness the Expansion, and so the warmth of the included Air) and upon my ceasing to blow, the same water would reascend in the Pipe, and that, though I stood near it to watch it, (which shows, that the former Depression was not caused by the approach of my warm Body) and this I did more then once, both alone and before witness, notwithstanding that the Air blown at the same time out of the same Bellows upon our hand and face seem'd cool enough. But fearing to insist any longer on this matter in a Preface, I think it now unseasonable to add, That as some contingent Experiments in subsequent Trials may Fail oftner, so other may perchance Succeed oftner then is expected: As I have sometimes observed in the figures, that appear in the Ice made of some Liquors, that abound with Volatile, Urinous, or with certain other Salts. But to say a word in general of Experiments, whose success is not always uniform: As a Magnetick Needle, though it do not always precisely respect the Poles, but both declines sometimes Eastward, and sometimes

Introductory.

times Westward, and varies that Declination uncertainly as to us, does nevertheless so far respect the North, as in spite of its Variations to be an Excellent guide to Navigators: So there are contingent Experiments, whose Events, though they sometimes vary, are seldom very exorbitant, but for the most part are regular enough to afford Philosophers very useful Informations and Directions.

If it be demanded, why in the 15, 18, and 19. Sections I have inserted so many Quotations out of several Authors, and how that agrees with what I have said not far from the beginning of this Preface of the uncultivatedness of the subject I have adventured on? I answer, That what I have done crosses not what I have said. For my complaint was, That there has been very little, especially of any moment, delivered concerning Cold by Classick Authors, and that even other learned Writers, who have had occasion to say something purposely of Cold, have handled it exceeding fejunely; but this hinders not, but that if a Man will take the Pains to seek out, and enquire of Travellers, and has the curiosity and opportunity to consult Voyages and Navigations,

The Preface

tions, he may among a multitude of other things, that have nothing to do with Cold, meet with some few that concern that subject: And yet the Authors that deliver such particulars, can no more properly be said to have written professedly of Cold, then of Botanicks, or Zoology, or Meteors, or Civil Philosophy, because in the same Journal they mention a great frost, or a great snow, as chancing to happen on such a day, with as little particular design as they mention a Storm, or a Whale, or a Bear, or the manners of an Indian people. This Consideration being premis'd, 'twill not be difficult to return an Answer to the former part of the Question lately propos'd. For the unfrequency of my Quotations in most of the Sections of the following History, will, I presume, sufficiently perswade the Reader, that I would not needlessly imploy so many of them in the three Sections, that are nam'd in the Question. But the Writers of Physicks being, for ought I know, silent as to the particulars I have transcrib'd out of other Writers, and the Observations being such as I could not myself make in this Temperate Climate; I must either make
use

Introductory.

use of other mens Testimonies, or leave some of the Remarkablest Phænomena of Cold unmention'd. And they that shall try how much pains it will cost them, to range among Books, which many of them contain little but melancholly Accounts of storms and distresses, and Ice, and Bears, and Foxes, to cull out here and there a passage fit to make a part of such a Collection as they may here meet with, will possibly rather thank, then blame me for having, to gratifie my Readers, given my self so laborious and unpleasant an entertainment. And I was the rather content to enlarge a little on the foremention'd occasions, not only because I was unwilling to be engag'd more then once in so troublesome an Imployment, but (and that chiefly) because most of the particulars, I have collected out of Navigators, are afforded me by the Voyages of our own Country-men, who having written only in English, an Account of what their Relations contain of most material concerning Cold, will probably be welcome, as well as new to the curious of other Countries, who cannot understand their Books, divers of which having been
long

The Preface

long out of Print, are so hard to be procur'd, even here in England, that I doubt not but these Extracts of them will be acceptable, even to divers of the Virtuosi of our own Nation, especially since I have been careful to alledge most of the Testimonies in the writers own words, though they are not always the best, wherein the things he delivers might be express'd. And this course I the rather took, that I might do what I think very useful to be done by all writers of Natural History, who would do well to distinguish more carefully, then hitherto many have done betwixt the matters of fact, they deliver as upon their own knowledge, and those which they have but upon trust from others. I know it would be more acceptable to most Readers, if I were less punctual and scrupulous in my Quotations; it being by many accounted a more Gentile and Masterly way of writing, to cite others but seldom, and then to name only the Authors, or mention what they say in the words of Him, that Cites, not Theirs, that are Cited. And there are some Writers of such known Diligence and Veracity, as to be safely trusted, and some Cases wherein

Introductory.

wherein I do not dislike, but comply with this Custom (after having first consulted my Author to be Master of his true and genuine sense,) but in matters Historical, and whereon Philosophical and Important Truths are to be built, I should think my self beholden to a Writer, for setting them down in such a way, as that I may satisfy my self, that the Testimony is faithfully reported. In order to which it will be sometimes very useful to be enabled to repair to the Original witness, and, if need be, survey there the context of the alledged passage. For I must here advertise the Reader, that in matters of any moment, 'tis not from every Writer, that I dare trust the Quotations he makes of the passages of other Authors, in his Own words, not Theirs: For upon comparing very many Quotations, I have found, that oftentimes there is no such thing, as is pretended to be really met with in the place referred to; and even when neither the Book, nor Chapter, nor Page are misquoted, I have too frequently found, that the Alledgers of Testimonies, do either through Inadvertency misapprehend, or misrecite the sense of the

The Preface

the Author they quote, or out of Design make him speak, that which may comply with their purpose, whether it were his own sense or no: and by their Indefinite citations make it too troublesome and difficult a work, for the Reader to find out, whether they have imposed upon him or not. But this only by the by, to return therefore to the passages we were speaking of, in the 15, 18, and 19. Sections, I shall now add, that having in the beginning of the XIX. Title of the ensuing History it self rendered an Account of my not scrupling to insert some strange Relations concerning Cold, it will not be requisite to mention here, what the Reader will meet with there. And I scarce doubt but he will excuse such passages, if he consider, That as I could not omit them without leaving out some of the eminentest Phænomena of Cold, so being unable to examine them here in England, all I could do, was, to report them faithfully, and mention only such as were either affirmed by Eye witnesses (as the most, I have inserted, are) or, at least recommended by credible Testimony, whereof we shall say more by and by; To which
sort

Introductory.

sort of Narratives, I know not whether I may refer That, (which yet for its strangeness may deserve a transient mention,) came awhile since to my ears, of an English man, who related to an eminent Virtuolo of our acquaintance, That a Dutch Master of a Ship, returning from the Northern Countries, very solemnly affirm'd, being therein seconded by one of his Country-men, and offered to produce his Journal for proof, That in-deavouring to sail Northwards as far as he could, he came within less then a degree of the Pole itself, and found the Sea open, and the cold very tolerable. But to return to what we were saying, before this odd Relation diverted us, I did not only decline the mention of divers things, with which I fear many Writers would have adorned a History of Cold, but even of those that I my self have inserted, I would have left out divers, were it not, that many of the Relations, that may appear so wonderful, seem not to me to be repugnant to the nature of things, but only suppose a far greater degree of Cold, then we have in these parts, and yet the familiar effects of the Cold we have here,

The Preface

would perhaps be looked on as incredible, by one that were born and bred in the Kingdom of Congo, where Odoardus Lopes, who lived long there, informs us, that Ice, that is water made solid, is so unknown a rarity, that it would there be valued as much as so much Gold. And a Learned Physician, that lived in Jamaica, being asked how far he found the Temperature of that Country to be like that of Congo, answered me, That in that Island he observed not all the Winter long, either Frost or Snow: And yet here it will not be unseasonable to say a word or two of the three Principal Authors, from whom most of our strange relations we are considering are transcribed.

The first is Gerat de Veer, who writ the Voyage of the Hollanders to Nova Zembla, a Book so eminent in its kind, that it may seem a wasting of time to set down a Character of it; and therefore I shall only advertise the Reader of two things, the one, That the Dutch did indeed make three consecutive Voyages to Nova Zembla, but that the
third

Introductory.

third being that in which they wintered, there most of the particulars are to be understood of that. The other thing is, that having lost the Translation that was made of those Voyages out of Dutch into English (published in a Book by themselves) without being able to procure another, I was obliged to have the citations transcribed, as I found them extant in that faithful Collection of Voyages compiled by Purchas, who seems by the Style to have (as to the Book we are speaking of) only plai'd the part of an Interpreter. And here 'twill be seasonable to add, that whereas that excellent Collection consists of several distinct Tomes or Volumes, the many Quotations to be met with in the Margent of our History under the name of Purchas, are to be understood, (unless the contrary be intimated) to belong to the third Part of his Pilgrim, where the Dutch and other Voyages into Northern Countries are to be found.

The next Book I intended to mention, is Olaus Magnus's History of the Northern Nations. And though this Author

The Preface

is of very suspected Credit, and delivers some things upon hear-say, which, they are kinder to him then I, that are pleas'd to beleive; for which reason I do but very sparingly make use of his History, yet considering, that he was Archbishop of Upsale in Sweden, and appears to have more Learning, then many that never read his Books, imagine; I thought I might Now and Then, make use of his Testimony, in matters wherein he either professes himself to speak upon his own knowledge, or delivers but such things as being consistent with the Laws of Nature appear Improbable, only, because of the Intense Cold that they suppose; which I the rather say, because he himself somewhere speaking of the Cold, that by the Laws of Nature reigns in the North, subjoyns this Passage; Sub quo quia natus, & versatus sum etiam circa elevationem graduum Poli Arctici 86. arbitror me posse hoc, & multis sequentibus Capitulis, nonnihil Cæteris vaga opinione scribentibus clarius demonstrare, quam vehemens & horrendum sit illic frigus.

Lib. I. Titulo de
frig. Asperitate,
pag. 9.

And

Introductory.

And, though perchance few Readers will perceive it, I have been so Severe in rejecting not only Relations, but even Authors otherwise not Obscure, that, how much soever I foresaw my scrupulousness might impoverish my History, yet there are some whole Treatises about Cold Countries, whence I have shunn'd to borrow any one Authority, because I perceiv'd the Authors had not observed the things they recount themselves, and were too easie in believing others.

The third Writer I meant to take notice of, is Captain James, a Person from whose Journal I have borrowed more observations, then from those of any other Sea-man, not only because his Book supplied me with them, and because it is somewhat scarce, and not to be met with in Purchas's Tomes, (having been written some years after they were finished) but because this Gentleman was much commended to me, both by some Friends of mine, who were well acquainted with him, and by the Esteem that

d 3 competent

The Preface

competent Judges appear to have made of him. For having been, not only employed by the Inquisitive Merchants of Bristol, to discover a Northwest passage into the South Sea, but designed for so difficult a work by so judicious a Prince, as the late King, and having at his return published his Voyages by his Majesties command; as by these circumstances, though not by these only, this Gentlemans Relations may well be represented to us, as likely to deserve our consideration and Credit: So by his breeding in the University, and his acquaintance with the Mathematicks, he was enabled to make far better use then an ordinary Sea-man would have done, of the opportunity he had to observe the Phænomena of Cold, by being forced to winter, in a place where he endured little (if at all) less extremity of Cold, then that of Nova Zembla.

I presume 'twill easily be taken notice of, That in the following History I have declined the Asserting of any particular Hypothesis, concerning the Adequate cause

Introductory.

cause of Cold. Not but that I may have long had Conjectures about that matter, as well as other men, but I was willing to reserve to my self an Intire Liberty of declaring what Opinion I most inclined to, till the Historical part being finished, I may have the better opportunity to Survey and Compare the Phænomena; and the leisure, (which I cannot promise my self in haste,) of calmly considering what Theory may best agree with them: especially since I freely acknowledge, That I found the framing of an Universal and unexceptionable Hypothesis of Cold, to be a work of greater difficulty, then every Body would imagine; especially to me, to whom some Experiments purposely made have suggested a puzzling Difficulty, which 'tis like that Philosophers have not yet thought of. And whatever Applause is wont in this Age to attend a forwardness to assert Hypotheses, yet though Fame were less to be sought then Truth; this will not much move me, whilest I observe, That Hypotheses hastily pitch'd upon, do seldom keep their Reputation long; and divers of
d 4 them

The Preface

them that are highly Applauded at the first, come after a while to be Forsaken, even by those that devised them.

As for the Title of the following Book, I call the Experiments new, because indeed, if I mistake not, nine parts of ten (not to say nineteen of twenty) are so. But though a 150. or 200. Experiments of that kind, besides Collections from Travellers, and Books that do not professedly treat of Cold, may, I presume, allow me to have begun the natural History of Cold; yet in the very Title Page I think fit to intimate, that I look upon what I have done but as a Beginning. For though some very noted Virtuosi have been pleased to seem surprized, to see what so barren and uncultivated a subject has been brought to afford this Treatise; yet I look upon these as things, that do rather Promise than Present a Harvest, and but as some early Sheaves of that Crop, which mens future Industry will reap from a subject, that is indeed Barren, but not Unimprovable. For I see not why it should not hold in the
History

Introductory.

History of Cold, as well as in many other attempts; That the greatest Difficulties are wont to be met with at the Beginning, and those being once surmounted, the Progress becomes far more Easie. And as the Magnetick Needle, though it point directly but at the North and South, does yet discover to the Seaman the East and West, and all the other points of the Compass: So there are divers Experiments, which though they do primarily and Directly teach us but a Notion or two, may yet assist us to discover with ease many other Truths, to which they seem'd not at first sight to afford us a Direction. So that what is here already done, such as it is, partly by Hinting various Inquiries about Cold, and partly by Suggesting ways not formerly practised of making further Experiments, may possibly make it more easie for others to Add to these a number, far exceeding that, which they will here meet with, then it would have been without such assistances, (which I had not) to contribute to the History of Cold, even such a stock as I have begun

The Preface

begun it with. And this I the rather incline to think, because I find, that when once a Man is in the right way of making Inquiries into such subjects, Experiments and Notions will reciprocally direct to one another, and suggest so many things to him, that if I were now to begin this work again, and had Cold, and fitly shap'd Glasses, and Instruments, with other Accommodations at command, there are divers parts, on which my Inlargements would not perchance be much Inferiour to what is Already extant there, if they did not much Exceed it. But besides That, I have other work enough, and that of a quite other Nature upon my hands; the Truth is, that I am plainly Tired with writing on this subject, having never handled any part of Natural Philosophy, that was so Troublesome, and full of Hardships, as this has proved; especially because, that not only the Experiments being New, and many of them subject to miscarriages, required to be almost constantly Watched, but being unable to produce or intend Cold as we can do Heat, nor command
the

Introductory.

the Experiments that concern Congelation, with as little difficulty, as we can do those, that belong to divers other subjects; I was fain to wait for, and make Use of a Fit of frosty weather (which has very long been a rarity) as solicitously as Pilots watch for, and improve a Wind.

III. It remains now, that I give some account, why I suffer so Unfinished a piece, as I acknowledge this to be, to come forth at this time. And I confess, that if I had not preferred the gratifying the Curious, before the advantages of my own Reputation, I should have kept this Book in my hands some winters longer, that It might come forth, both more rich, and less unpolished. But how great a power my Friends have with me in such cases, the Reader may easily guess by the Preamble he will find prefixed to the first Title of the ensuing History. For by the Date of that, he will see, how early my Papers about Cold were to have been com-

The Preface

communicated; nor was I any thing near so much befriended, as I expected, by those interposing Accidents, that have for above a year and a half, kept those Papers lying by me. For the then next, and now last Winter proved so strangely Mild, as to be altogether Unfavourable to such a work as I had design'd. Wherefore finding, that Delays had done me no more service, and press'd by the solicitations of divers Virtuosi from several parts, I resolved, that I would suspend till another opportunity, the drawing together of what I had Observed or Collected, touching the Regions of the Air, and some of the chief Hypotheses, that are controverted about Cold, with what other loose Papers, touching that Quality, I not could so readily dispatch to the Press; and would not withhold from the Curious what assistance my Collections could afford them, to make use of this Winter to prosecute Experiments of Cold. And remembering how favourable an entertainment my former Endeavours to gratifie Ingenious Men, had found among them, I took a Course, wherein

Introductory.

wherein I was more likely to obtain Thanks then Praises, and chose rather to adventure on the Equity and Favour of the Reader, for the Pardon of those faults and Imperfections, that are imputable to Hast, then to deny him the opportunity of this Cold season, wherein to Examine the Truth, and Supply the Deficiencies of what I had delivered. And this I the rather did, both because I was desirous to Quit this subject, for another from which it had diverted me, and for which I have more Value and Kindness; and because, that as a tender Constitution of Body kept me, whilst I was writing the following History, from adventuring upon some Trials, that might (probably) have enrich'd it; so the Continuance of the same disadvantages, together with other inopportune Distempers superadded to them, do not permit me to Know, whether, and how far I shall be able to Prosecute the work I have begun: and do oftentimes reduce me to be more concern'd to Shun the Effects of Cold, then Observe the Phænomena of It. And indeed, whether those prove true Prophets or no, that assure me I shall

The Preface, &c.

shall lose no reputation by this History (as
incompleat as it comes forth,) I think, if
Ingenious Men knew, how much Trouble
and Exercise of my patience it has cost me,
they would, peradventure, vouchsafe me
some of their thanks, if not for what I
have done, yet for what I have suffer'd
for their sakes, (and would scarce have
undergone upon any Inferior account
whatsoever;) it being, though a less
Noble, yet no less Troublesome an Employ-
ment, to Dig in Mines of Copper, then
in those of Gold: and Men being often-
times oblig'd to Suffer as much Wet and
Cold, and Dive as deep, to fetch up
Sponges, as to fetch up Pearls.

Errata.



Errata.

PAge 5. line 17. read, that in not nice, for, that even in nice, p. 46. l. 8. r. effected, p. 48. l. 16. dele and, together with (), p. 82. l. 28. r. 28. chapter, p. 178. l. 7. dele which, p. 266. l. 22. r. it did rise four inches, p. 292. l. 6. r. that stood on the ice, p. 302. l. 9. r. three, for, thee, p. 380. l. 10. r. cemented by intercepted and then frozen water, instead of congealed by cold water, p. 488. l. 11. r. 52. degr. 52. min.

In the Appendix of Dr. Merret, pag. 35. lin. 36. read, upon these mixtures, not in.



Unable to display this page

(Eggs, Apples, and other) Bodies by
Cold. p. 184.

Title VII.

Experiments touching the Expansion of
Water and Aqueous Liquors by Freezing.
p. 222.

Title VIII.

Experiments touching the Contraction
of Liquors by Cold. p. 237.

Title IX.

Experiments in Consort, touching the
Bubbles, from which the Levity of Ice is
supposed to proceed. p. 245.

Title X.

Experiments about the measure of the
Expansion and the Contraction of Liquors
by Cold. p. 279.

Title XI.

Experiments touching the Expansive
force of Freezing Water. p. 296.

Title XII.

Experiments touching a new way of
estimating the Expansive force of Conge-
lation, and of highly compressing Air
without Engines. p. 382.

Title XIII.

Experiments and Observations touch-
ing the Sphere of Activity of Cold. p. 328.

Title

Title XIV.

Experiments touching differing Medium's, through which Cold may be diffused. P. 345.

Title XV.

Experiments and Observations touching Ice. P. 364.

Title XVI.

Experiments and Observations touching the duration of Ice and Snow, and the destroying of them by the Air, and several Liquors. P. 396.

Title XVII.

Considerations and Experiments touching the Primum Frigidum. P. 412.

Title XVIII.

Experiments and Observations touching the Coldness and Temperature of the Air. P. 464.

Title XIX.

Of the strange Effects of Cold. P. 520.

Title XX.

Experiments touching the weight of Bodies Frozen and unfrozen. P. 550.

Title XXI.

Promiscuous Experiments and Observations concerning Cold. P. 575.

Title XIV.

Of the manner of touching a burning Me-
dicine, and the manner of applying it to the
Body. P. 347.

Title XV.

Of the manner of touching a burning Me-
dicine, and the manner of applying it to the
Body. P. 347.

Title XVI.

Of the manner of touching a burning Me-
dicine, and the manner of applying it to the
Body. P. 347.

Title XVII.

Of the manner of touching a burning Me-
dicine, and the manner of applying it to the
Body. P. 347.

Title XVIII.

Of the manner of touching a burning Me-
dicine, and the manner of applying it to the
Body. P. 347.

Title XIX.

Of the manner of touching a burning Me-
dicine, and the manner of applying it to the
Body. P. 347.

Title XX.

Of the manner of touching a burning Me-
dicine, and the manner of applying it to the
Body. P. 347.

Title XXI.

Of the manner of touching a burning Me-
dicine, and the manner of applying it to the
Body. P. 347.

Title XXII.

Of the manner of touching a burning Me-
dicine, and the manner of applying it to the
Body. P. 347.



New
THERMOMETRICAL EXPERIMENTS
And
THOUGHTS.

The I. Discourse,
Proposing the I. Paradox,
Viz.

*That not only our Senses, but
common Weather-glasses, may
mis-inform us about Cold.*

IT may to most men ap-
pear a work of needless
Curiosity, or superflu-
ous diligence, to examine
sollicitously, by what
Criterion or way of estimate the
Coldness of Bodies, and the degrees
of it are to be judg'd, Since Coldness
B being

being a Tactile Quality, it seems impertinent to seek for any other judges of It then the Organs of that sense, whose proper object it is. And accordingly, those great Philosophers, *Democritus, Epicurus, Aristotle,* (and till of late) all others both Ancient and Modern seem to have contented themselves in the matter with the Reports of their Sensories.

But this notwithstanding, since we can scarce employ too much care and diligence in the Examining of those Touchstones, which we are to Examine many other things by, perhaps it will be neither unseasonable nor useless to premise something touching this Subject.

For though it be true, that Cold in its primary and most Obvious Notion be a thing relative to our Organs of Feeling, yet since it has also notable Operations on divers other Bodies besides ours ; And since some of them seem more sensible of its changes, and others are less uncertainly affected by them, it would be Expedient to take in the Effects of Cold upon other Bodies, in the Estimates
we

we make of the degrees of it.

And to make this appear the more reasonable, I shall not scruple to propose the following *Paradox*, namely, That our Sensories either alone, or assisted by Common Weather-glasses are not too confidently to be relied on in the judging of the degrees of Cold.

To make this *Paradox* Plausible (which is almost as much as I here pretend to) I shall represent in the first place, that the account, upon which we are wont to Judge a Body to be Cold, seems to be, that we feel its particles less vehemently agitated then those of our Fingers or other parts of the Organ of Touching. And consequently, if the temper of that Organ be chang'd, the Object will appear more or less Cold to us, though it self continue of one and the same Temper.

This may be exemplified by what has been observ'd by those that frequent Baths, where the milder degrees of heat, that are us'd to prepare those that come in for the higher, seem very great to them that coming

out of the cold Air dispose themselves to go into the Hot Baths, but are thought cold and chilling to the same persons when they return thither out of much warmer places; which need not be wondred at, since those, that come out of the cold Air, find that of the moderately warm Room more agitated, then the cold Ambient would suffer the External Parts of their Bodies to be, whereas the same warm Air, having yet a less agitation then that in which the hotter parts of the Bath had put the sensitive parts of the Bathers Bodies, must seem cold and chilling to Them.

But 'tis not only in such cases as this, wherein Men can scarce avoid taking notice of a manifest change in themselves, that these mistaken Reports of our senses may have place. For oft-times we are impos'd upon by more secret changes in the disposition of our Sensories, when there needs something of attention and of Reasoning, if not of Philosophy to make us aware of them. For being apt to take it for granted that our Temper is the same, when there is no very manifest

nifest cause why it should be chang'd, we often impute that to Objects, whereof the Cause is in our selves; and if this change in our selves be wrought by unsuspected Agents, or by insensible degrees, we do not easily take notice of it. Thus though in Summer divers Cellars, that are not deep, are perhaps no colder then the External Air was, (when it was judg'd but Temperate,) in Winter or the Spring; yet it will seem very Cold to us that bring into it Bodies heated by the Summer Sun, and accustomed to a warmer Air; nay cold does so much depend upon the degree of Agitation in the parts of the Object in reference to the Sentient, that even when we may think the Sensory unalter'd, it may judge an Object to have a degree of Coldness which indeed it hath not; as I remember, that to satisfy some Friends, that 'tis not every Wind which feels cold to us, that is really more Cold, then the still Air, I have sometimes shewen, that even in nice Weather-glasses Air blown out of a pair of Bellows does not appear to have ac-

quired any Coldness by being turn'd into Wind, though if it were blown against the hands or face, it would produce a new and manifest sense of Cold; of which the reason seems to be, That though the Organ in general seems not to be alter'd, yet the Wind by reason of its Motion, is able not only to drive away the parts of the Air contiguous to the hand, or face, and the warm steams of the Body which temper'd its Coldness; but to pierce deeper then the calm Air is wont to do into the pores of the skin, where by comparison to the more inward and hotter parts of the Sensory it must needs appear less agitated and consequently colder.

Besides, that sometimes we may meet with certain Steams in the Air that have in reference to the Blood and Spirits of humane Bodies (though not perhaps to divers other Liquors) a certain hidden power of chilling, as *Opium*, ev'n in outward applications (for in such ways I have known a great Chirurgeon much use it and highly extol it) strikes a Coldness into the Body by the subtile Effluvium's that

that insinuate themselves at the pores of the Skin ; and perhaps too, that Coldness is ascrib'd to External Bodies, which is produc'd in us by some Frigorifick Vapour, or other distemper ; which being too slight to be taken notice of as a disease, may yet be of Kin to those Agents, that produce what Physicians call horrors and Rigors at the beginning of Fevers, and some other distempers ; or produce that strange and universal Coldness of the external parts, which is frequently enough observ'd among other Symptoms in Hysterical Women. Moreover, Bodies may often appear colder to us then to a Weather-glass, because our Sensories are more affected by the density and Penetrancy of the parts. This may seem somewhat strange, but being sutable enough to some of my Conjectures about Cold, I have often made Tryals with very nice Weather-glasses, that have assur'd me, that (at least oft-times) when water seems to be cold enough to our touch, it appears not to be colder to the Weather-glass then the Ambient Air.

These Trials I have sometimes made with seal'd Weather-glasses, but the most with another sort of Weather-glasses (whole structure and use are by and by to be mentioned) which though they seldom prove durable, nor of any great use in any other then such nice and short Experiments, yet they discover slighter changes of the Temper of the Air then would be *notable* (not to say *sensible*) in ordinary Thermometers. But of multitudes of Trials that I sometimes made with these Glasses, I can at present find among my loose Papers but a very few ; and though I remember, that in one or two (made about the same time with some of those that follow) I observ'd Things that make me now wish I had had Opportunity to make those further Trials of Them, which some of their *Phænomena* seem to direct the making of : yet I shall annex these that follow as I find them entred, because they are not perhaps destitute of hints improvable by further prosecution. *June 26.* between two and four in the afternoon (the Weather

ther moderate for the season) I took a thin white glass-Egge blown at a Lamp about the bigness of a Walnut, with a stem coming out of it about the bigness of a large Pigeons Quill four or five Inches long, and open at the Top ; this slender pipe being dipp'd in water, admitted into its Cavity a little Cylinder of Water, of half an Inch long or somewhat more, which (the Glass being erected) subsided by its own weight, or the Temper of the Air in the Egge (in reference to the outward Air) till it fell to the lower part of the Pipe, where it comes out of the Egge, and thereabout it would rest. Now if taking this Glass by the Top betwixt my Thumb and forefinger, I deprest the Egge under the surface of a Bason of fair water (cold enough to the touch) the little Aqueous Cylinder, that parted betwixt the Air in the Egge, and the external, would, instead of being made to subside by the Eggs immersion into the Cold water, presently rise up from the lower part of the Pipe, till it reach'd about the middle of it, though the Glass
were,

were, in this and the following Trials, held erected; and as soon as it was taken out of the Water into the Air, the water would again subside, whether I held the Glass, or let it rest upon the Boards, or a Linen Carpet, that cover'd the Table, on which the Trials were made. And this I did several times as well with as without witness. I tried also that if, instead of water, I made use of Quicksilver, though not big enough to cover the Egge much above half way, and in the rest proceeded as above; the cold Quicksilver would presently make the Aqueous Cylinder hastily ascend near three Inches, sometimes almost, and sometimes quite to the Top of the slender pipe, whence the water would again quickly subside, when the Glass was taken out into the free Air, or set to rest upright as before.

Besides, having set the vessel of Quicksilver and the Balon of water very near one another, I did at least upon three or four several Trials find, as I expected, that when by immersing the Egge in water, the
pendulous

pendulous Cylinder was rais'd so high, that it did no longer sensibly ascend, by nimbly taking the Egge out of the water and depressing it in the Quicksilver, it would rise far higher: and I also tri'd, that nimbly removing the Egge out of the Quicksilver into the water, the pendulous Cylinder would subside, after plunging the Egge under water, though not so fast, nor near so low as it would do, in case the Glass were remov'd from the Quicksilver into the Air. Upon another Trial made much about this time, though not the self same day; the pendulous water in the same Glass, (the day being for the most part windy and rainy) did subside upon the immersion of the Glass into water, not only a while before noon, but an hour or two after dinner, and at distant hours afterwards, though the Descent of the Pendulous water was neither so quick, nor so considerable as it had been formerly in the Mornings.

June 27. In the morning a small Cylinder of Water pendulous in the
above

above mentioned Glass, upon the immersion of the Egge in a Balon of water, would immediately and very considerably subside, whereas the same glass, being immersed in the Vessel of Quicksilver formerly mentioned, would presently ascend. Both parts of this Experiment we several times tried, and the Reason was suspected to be, that the Quicksilver had stay'd all night in my Chamber, which was somewhat warm, whereas the water was brought up that morning, and to the touch seem'd colder then the Quicksilver, and a while after dinner, the same water having been still kept in the room, we divers times found, that as well That, as the Quicksilver, did immediately upon immersion impel up the pendulous water in the slender pipe. Another time in frosty weather (and about the beginning of *January*) we did with such a glass (as has been already several times mention'd) take some drops of water out of a vessel, wherein that Liquor had for a good while been kept; that it might be reduc'd as near as we could to the Temperature

perature of the Ambient Air; then suffering the suspended water to continue a convenient while in the long and slender stem of the Weather-glass, that the internal Air might be reduc'd to the temper of the external, we took up the Glass by the open end; and immersing the obtuse part of It into a shallow Vessel containing some of the above mentioned Water, we found the suspended drop suddenly impell'd upwards about half an Inch or more, and the Ball of the Thermometer being taken out of the Water into the Air, the pendulous drop did again (though far more slowly then it ascended) subside. This was repeated three or four times with some intervals between (and that in a Room where there was no Chimney) and still with the like success, save that in the two last Trials we took the Weather-glass out of the shallow water, and plunging it into a deep vessel of the same water (that stood very near the other) we found (for further confirmation of the Experiment) that the pendulous water was upon these
 new

new immersions, impell'd up, near (if not full) as high again, as when we had immers'd it only in the shallow vessel: and taking it out of this deep Glass, we found the Cold of the external Air to reduce It to its former humble station. Thus far the notes, I have yet been able to recover: and though, as I said, I dare not build very much upon them, yet by small seal'd Weather-glasses I find enough to invite me to suspect, that of the degrees of heat and cold in the Air we may receive differing informations, when we imploy only our Organs of Touching, and when we make use of fit Instruments.

I shall add on this occasion, that not only water it self, but moist vapours abounding in the Air, may make Us think it colder then the Weather-glass discovers it to be. For though it be generally taken for granted, that the Thermometer does only more exactly measure or determine the Effects, which cold hath both upon it and upon our Sensories, yet I have long suspected that there is somewhat else in the case. And I have

have observ'd, that sometimes the weather seem'd more or less cold to me, then that which preceded, when the contrary appear'd in the Weather-glass; and that, when upon consideration of the whole matter, that difference did not appear to depend upon those circumstances of Exercise or Rest, or the Temperature of the Air I came out of, or any of those other things, to which a considerate man, that goes upon no better then the common opinions about Weather-glasses, would be apt to impute to that *Phenomenon*. And I was the less dispos'd to think my self mistaken, because having purposely enquir'd of others in the same house, who were not told, what Information the Weather-glass gave, they agreed with me in the sense I had of the Temperature of the Weather. And having since, as occasion serv'd, communicated my Observations and suspitions to divers Ingenious Men, I have been by their recenter Observations confirm'd, that what I have taken notice of, was not the Effect of any *Idιοσυγγεγισια*.
 From

From which, and other particulars, that we may have elsewhere opportunity to mention, we may plausibly enough infer, that it were not amiss, not only to take notice, when we have opportunity, of the sense, that is Express'd of the degrees of Cold by Birds and other animals, whose diet is more simple and regular then ours, and whose perceptions are commonly more delicate and less diverted; but, especially, to examine the coldness of the Air and other Bodies as well by Experiments and instruments, as by the touch. And on this Occasion I must not pretermitt that memorable Account, that is given us by *Martinius* in that Noble piece of Geography which he calls *Atlas Chinenfis*, where speaking of the Air of that populous Countrey he has this singular passage: *Ad Cæli* (says he) *solique temperiem quod attinet, majus in hac provinciâ frigus est, quàm illius poscat poli altitudo: vix enim illa excedit gradum secundum supra quadragesimum; & tamen per integros quatuor sæpe menses flumina omnia adeò durè concreverunt gelu, ut currus equosque*

ac

Unable to display this page

Unable to display this page

lighter. And though this be a thing not taken Notice of by those that have treated of Weather-glasses, yet after what we have elsewhere manifested concerning the weight and spring of the Air, and what we have probably conjectur'd concerning the varying height of the *Mercurial Cylinder* in the *Torrecellian Experiment*; I see not why It should not much call in Question the Informations we receive from common Weather-glasses in those cases, where the height or weight of the *Atmospherical pillar*, that presses upon the Water in the Weather-glass, is considerably longer or shorter, lighter or heavier than is usual.

See the
18. of our
New Phy-
sico-Me-
chanical
Experi-
ments.

For besides the reason of the thing, we have Experience on our side. I might mention on this Occasion an Experiment I thought on, and also attempted last winter to show ev'n upon a Ballance the varying gravity of the *Atmosphere* in one and the same place, by hanging a small Metalline weight at one End of a pair of Scales so strangely exact, that they would turn with far less than the 500.

part of a grain; and counterpoising it at the other end with a Hermetically seal'd Glass Bubble, which being blown as large and as thin as could possibly be procur'd of so small a weight, might by its great disproportion in Bulk to the Metalline Body lose more of its weight than That would upon the *Ambient Airs* growing more heavy. But the particular Account of this Attempt belonging to another place, the trial ought not to be more than hinted here, especially since it may suffice for our present purpose to alledge that having found (as we have already in other papers noted) that in a Weather-glass, where the Water is not fenc'd from the External Air, the weight of the Atmosphere may make it alter considerably between the Top and Bottom even of a Church or Steeple, though it appear'd by more certain Thermoscopes, that 'twas not the differing Temperature of the Air as to Cold and Heat, but the differing gravity of the Atmosphere, which being shorter and lighter at the Top press'd less forcibly upon the subjacent

In the defence against Linus Cap. 4

cent Water and the included Air, as is more fully made out in the Treatise above related to. And having by the intervention of a Learned Acquaintance desir'd to have some Experiments made of the Effect of the Air upon Weather-glasses in deep Pits or Mines, where the Atmospherical Cylinder is longer and heavier, I receiv'd Information that an Ingenious Physician, who had the Opportunity of trying what I desir'd, had found, that in the Bottom of one of those very deep Pits, the water in a common Weather-glass rose near three Inches higher then at the top, in a shank or pipe of about thirty Inches long. And this notwithstanding the warmth, that is usual in such deep places, which seems not any thing near so plausibly referable to any other cause, as to the increas'd gravity of the Atmospherical Pillar incumbent on the Water, that Pillar being heavier at the Bottom then at the Mouth of the Pit, by the weight of an aerial Pillar equal in length to the pits perpendicular height or depth.

Dr. H. F.

But these are not the only Cases wherein the differing gravities of the Atmosphære may, as well as Heat and Cold, have an interest in the rising and falling of the Liquor in Common Weather-glasses. For though you should not remove them out of one place, and though consequently it may seem that the *Atmosphærical Pillar*, that presses upon the water, must be still of the same length, yet (not to urge, that That may alter, unknown to us) if retaining its length it retain not its gravity, we may be easily impos'd upon, and take that *Ascension* or *Subsidence* of the Liquor for the Effect of a higher or remiss degree of Cold, which may either totally or at least in part (and in what part, we are left to guess) be the Effect of the increas'd or lessened weight of the *Atmosphærical Pillar*, happening either by the copious dispersion of Vapours and other heavy Steams through the Air, or upon other Occasions not necessary to be here discours'd of, or by the *Præcipitation* of such vapours by rain or into dew, or else by the Removal
of

of the Occasions of the Augmented Gravity or Pressure of the Air. For we have often observ'd great Variations to happen in the height of the Mercurial Cylinder in the Torricellian Experiment upon great rains and fogs, and other sudden and considerable mutations of the Incumbent Air. But since I my self thought fit, notwithstanding the plausible ratiocination, that led me to this Conjecture, to examine it by Experience; I can scarce doubt but that others may have the like Curiosity that I had. And therefore, because it may seem a paradox, it will not be amiss, of many to annex three or four Trials I made to examine the propos'd doctrine, especially ours having been the first observations of this kind, that, for ought we know, have been made by any. And indeed others could scarce have well made such, though they had lighted on the same thoughts, for want of such seal'd Weather-glasses to make them with. To omit then those that I made with a seal'd Weather-glass, and an ordinary one (in which the

water remains suspended beneath the included Air) I shall briefly relate, that in a Room unfurnished with a Chimney, I kept two Weather-glasses, which for more exactness sake, I caus'd to be made of a length far greater then ordinary ; so that the divisions of the one were half inches, and those of the other not much less, and yet were Numerous. The one of these which was furnished with good spirits of Wine, was seal'd, the other not, but this last I caus'd to be so made of the shape represented by the Scheme, that the Air being shut up in the lower part of the Instrument (not as in common Weather-glasses at the Top) the Liquor might as well in this as in the seal'd Weather-glass rise with heat and fall with Cold. In these Thermoscopes (where the Ascension and relapse of the Liquors were, by reason of the length of the Pipes, far more conspicuous then in Vulgar Weather-glasses) I observ'd with pleasure, that the Hermetical Thermoscope (if I may for distinction sake so call It, by reason of its being Hermetically seal'd)

seal'd) did regularly enough descend
 in cold weather, and ascend in warm:
 But the other, which was not seal'd,
 but had a little hole left open at the
 Top of the Pipe, though, when the
 Atmosphære continued of the same
 weight, it would like the other rise
 with Heat and fall with Cold, yet
 when the Atmosphæres gravity was
 alter'd, they would not uniformly
 move together, but when (as we
 gather'd from other observations)
 the Atmosphære grew heavier, the
 Liquor in the Pipe did not ascend, as
 high as it would have done, if the
 Atmosphære had continued in its
 former degree of gravitation. And
 on the contrary, when the incumbent
 Air came to be lighter, the Liquor
 would rise in the open Weather-glass
 in a proportion greater then the
 single increase of heat would have
 exacted; so that by comparing the
 two Weather-glasses together, I did
 usually foretel, whether the Mercu-
 ry in the Torricellian Tube (which
 I keep purposely by me in a frame)
 were risen or fallen, and consequent-
 ly whether the external Air were
 heavier

heavier or lighter then before. As on the other side, by looking on the height of the Mercurial Cylinder, I could easily tell before hand, whether the Liquor in the open Weather-glass were higher or lower then that in the Hermetical; the rising or falling of the Mercurial Cylinder one quarter of an Inch (the Temperature of the Air continuing as to heat and cold) usually signifying a great disparity betwixt the Ascension or the falling of the Liquors in the two Instruments.

Among the several notes, I find among my loose papers, and in a Diary I kept for a while of these observations, I shall content my self to transcribe the following two, because, though divers others were made by my Amanuensis, whose care is not to be distrusted, yet by reason of my absence I could not take notice of them my self. The first of these *Memorandums* runs thus:

Last night I took notice, that there was but one or two Divisions difference betwixt the two Thermometers, but upon such a change of
Wea-

Weather, that happened this day, as made me imagine, that the Atmosphere would be lighter then before, consulting the *Barometer* (if to avoid Circumlocutions I may so call the whole Instrument wherein a Mercurial Cylinder of 29. or 30. Inches is kept suspended after the manner of the Torricellian Experiment.) I found the Quicksilver lower then it had been a great while, and thereupon concluding, there would be a notable disparity, between the seal'd and open Weather-glass, I hastned to them, and found that the latter being much alleviated from the weight of the Incumbent Air, was no less then 17. Divisions higher then the others, and comparing the height the two Instruments were this day at, with an observation I my self made about a week ago, when the Quicksilver was much higher then now it is ; I found, that although this afternoon the seal'd Glass being at 41, the other was at 58 ; yet Then, when the seal'd Weather-glass, was five divisions higher, namely, at 46, the unseal'd Weather-
there

ther-glass was but at 27. So that betwixt that time and this, the Liquor in the seal'd Weather-glass, has descended five Divisions, but that in the open Weather-glass has ascended 31.

Thus far the first of the above mentioned Notes; the second is as follows.

The Mercurial Cylinder being higher, then it has been a good while, and yet the Weather warm and Sunshiny, when the Liquor in the seal'd Glass stood very near the 50th division, that in the unseal'd was fallen down as low as the 32.

So that it is very possible, that the unheeded change in the weight of the external Air may have a greater power to compress the included Air in an unseal'd Weather-glass, then a not inconsiderable degree of warmth may have to dilate it, and consequently in an ordinary Weather-glass, where the Air is included at the Top, it may often fall out, that contrary to what men suppose must needs happen, the pendulous Water may rise in warmer weather, and fall in colder.

And

And ev'n since the writing of the immediately foregoing part of this page, within a few days that interven'd, I have my self made observations, that do yet more clearly manifest this truth, as may appear by the following notes. The first of which speaks thus.

Memorandum, that Yesterday night the Quicksilver being at 29 Inches, the Liquors in the seal'd and unseal'd Weather-glassses, were near about the same Division, the former being at 40, and the other being but half a Division short of that Number. But this night the Quicksilver being risen about $\frac{1}{4}$ of an Inch; the Liquor in the seal'd is ascended to 45, and the other descended beneath 35 about half a Division, so that there is now 10 Divisions between them.

This is the first Note, to which the following night enabled me to add this other.

The Quicksilver being risen almost $\frac{3}{4}$ of an Inch above the station it rested at the night before last night, the Hermetical Weather-glass being as it was then above the 40 Division; the
Liquor

Liquor in the other, which was open, in two days and nights is fallen to the 17, and consequently is subsided about 23 Divisions, whilst the other is about the same height at which it was at the beginning of that time.

Two or three days after, being returned to the place wherein I had made this last observation, and from which some urgent Occasions had for that time exacted my absence; I found the Disparity, betwixt the two Thermometers that is express'd in the following Memorial.

This day the Quicksilver being risen to 30 Inches, when the Liquor in the seal'd Weather-glass was at about 41 Divisions, that in the other was depress'd a pretty deal below the Ninth Division, so that the difference between the two Thermometers was increas'd since the last Observation from 23 to near 33 Divisions, all which the Liquor in the open Weather-glass had sunk down, whilst that in the seal'd continued almost at a stand. And the day after this Memorial, I had occasion to register

register another, which being the last, I shall here think requisite to take notice of in this Discourse, I shall subjoyn it with that, which immediately preceded in order of Time.

This day the Quicksilver continuing at the same height, at which I observ'd it yesterday, but the Weather being grown much colder, the Liquor appears in both the Glasses to have uniformly enough subsided; that in the seal'd Weather-glass, being about the 33, and the other being sunk quite below the lowest mark of all, which was more then I apprehended it would have done, when there was no frost, especially since by my Diary it appears, that one of the last times I observ'd the Hermetical Weather-glass to stand at near about the same height, namely, the 34; the Liquor in the other Glas was no lower then the 41: nor probably would there be now so great a difference, if the Atmosphere had not been this day very heavy; whereas, when this freshly recited observation was made, I find
by

by the Diary, the Quicksilver to have ascended but to 29 Inches, and a pretty deal less then a half.

Since that time, being forced by several Avocations to be often absent from the place where my Thermoscopes were kept, I was not careful to prosecute such Observations, those already let down (not to mention those that are not here transcrib'd) being judg'd abundantly sufficient to evince the Paradox propos'd to be prov'd by them: Only, to manifest that after I desisted from registering my Observations, the *Phænomena* may probably have been as remarkable as before; I shall add, That one of the last times I chanc'd to take notice of the Difference to be gather'd by comparing the two Weather-glasses, I found (the weather happening to be warmer then ordinary) the difference between them to exceed any that I remembred my self to have then observ'd, amounting to forty four, if not to forty five Divisions.

And

And ev'n since the writing of the Last Line, we have had opportunity to observe a *Phænomenon*, which if it had occur'd to us in the place where we might have compar'd the Barascope with the Exact Weather-glasses hitherto mention'd, (and whereby we had been invited to rely upon it) would perhaps appear more Considerable then any of the Observations yet recorded. For not very many hours ago, finding in the Morning the Quicksilver to be risen in a good Barascope of mine (though another from that, all this while referred to, and elsewhere kept) above $\frac{3}{4}$ of an Inch higher then the place it rested at the Night foregoing, and a somewhat Nice Weather-glass (where the included Air is kept in the lower part of the Instrument, which is shaped like that already describ'd in this Discourse) being consulted to shew what Effect so great and sudden a change of the Atmosphæres gravity would have upon it ; I saw the tinted Liquor in the shank depress'd a full Inch or more beneath the Surface of the Ambient Liquor in the Viol,

D

which

which strange depression of the Liquor in a pipe above 20 Inches long, and where the alterations of the Air as to Heat and Cold are not wont to produce any thing near so great an Effect, I could not but take much notice of. Since the season of the year makes it no way likely that the night, though Cold, could have had so powerful an Operation on it, especially since an Amanuensis that watch'd it much longer then I, affirms that he saw the Liquor driven down quite to the very Bottom of the pipe, and a Bubble of the outward Air to make its passage through the water, and to joyn with the Air contain'd in the cavity of the Viol.

The



The II. Discourse,

Containing some

New Observations about the Deficiencies of Weather-glasses, together with some Considerations touching the New or Hermetical Thermometers.

AND since I had occasion to speak of the Deficiencies of Weather-glasses, and the mistakes whereunto men are liable in the Judgement they make of Cold and Heat upon Their Informations, it will not perhaps appear impertinent to add three or four Considerations more to excite men to the greater Wariness and Industry, both in the making and using Weather-glasses, and in their Judging by them.

I. And first, I consider, that we are very much to seek for a Standard

or certain Measure of Cold, as we have settled Standards for weight, and magnitude, and time, so that when a man mentions an Aker, or an Ounce, or an Hour, they that hear him, know what he means, and can easily exhibit the same measure: but as for the degrees of Cold (as we have elsewhere noted concerning those of Heat) we have as yet no certain and practicable way of determining them; for, though, if I use a Weather glass long, 'tis easie for me to find, when the Weather is colder, or when warmer, then it was at the time when the Weather-glass was first finished, yet that is a way of estimating, whereby I may in some degrees satisfy my self, but cannot so well instruct others, since I have no certain way to know determinately, so as to be able to communicate my knowledge to a remote Correspondent, what degree of Coldness or Heat there was in the Air, when I first finished my Thermoscope; For besides that, we want distinct Names for the several gradual differences of Coldness, we have already
declar'd,

declar'd, that our sense of feeling cannot safely be relied upon to measure them ; and as for the Weather-glass, that is a thing, which in this case is suppos'd to be no fit Standard to tell us what was precisely the temper of the Air, when it self was first finished, since that does but inform us of the recessions from it, or else that the Air continues in the Temper it was in at the making of the Instrument, but does not determine for us that Temper, and enable us, to express it ; as indeed it is so mutable a thing, ev'n in the same place, and oft-times in the same day, if not the same hour, that it seems little else then a Moral impossibility, to settle such an universal & procurable Standard of Cold, as we have of several other things. And indeed there is scarce any Quality, for whose differences we have fewer distinct Names, having scarce any for the many degrees of Coldness that may be conceiv'd to be intermediate, betwixt Lukewarmness and the Freezing degree of Cold, and even these are undefin'd enough ; for that, which to some mens senses

will feel Lukewarm, by others will be judg'd Hot, and by others perhaps cold; nor is even the glaciating degree of Coldness well determin'd, since not only differing Liquors, as oyl, wine, and water, will manifestly freez much more easily one then another, but even Liquors of the same denomination; and of waters themselves some are more easily turn'd into Ice then others, and I see no great cause to doubt but that there may be sufficiently differing degrees of Cold, whereof the mildest may suffice for the congelation of some waters. I must not forget to add, that the same person, that has made many observations with a Weather-glass, is so confin'd by that numerical Instrument, that if by the spilling of the Liquor, or the cracking of the Glass, or the casual intrusion of some Bubbles of Air, or by any of divers other Accidents that may happen, the Instrument should be spoil'd, he would, though he should imploy again the same Instrument, be reduc'd to seek out a new Standard, wherewith to measure the varying tempe-

temperature of the Air. And though it be not difficult to include in the Cavity of a Weather-glass some other fluid Body instead of Air, yet it will be very difficult, if not impossible, to include a Body, fit to represent and show the Alterations of the Ambient Air, without being also liable to receive impressions from it at the time of its being first shut up.

Yet I will not here omit that I have sometimes consider'd whether the essential oyl of Aniseeds (which is that that is distill'd by the intervention of water in a Limbeck) might not, during a good part of the year, be of some use to us, in making and judging of Weather-glasses. For this Liquor, as we elsewhere also note, having the peculiarity of loosing its fluidity during almost all the Winter, and a good part of the Spring, and Autumn too, when the Weather or the time of the day is colder; this Liquor, I say, being such, in way of setting a Standard for Weather-glasses; namely, by observing the coldness, which is requisite to make distill'd water begin to freeze: But though the accurateness of this way may be as well as the other justly Question'd, and cannot often be put in practice, even in Winter it self, nor without trouble; yet it may also be advantageously made use of, when the cold happens to be great enough to freeze water.

An Ingenious man has proposed another case

case you very gently thaw it, and then putting into it, the Ball of a Weather-glass furnish'd with spirit of Wine that will burn all away, you suffer the oyl to re-congeal leisurely of itself, you may by observing the station of the spirit of Wine in the Thermoscope, when the Oyl begins manifestly to curdle about it, be in some measure assisted, to make another Weather-glass like it. For if you put such rectified spirit of Wine into a Glass, the Cavity of whose Spherical, and that of its Cylindrical part, are as near, as may be, equal to the correspondent Cavities in the former Glass, you may by some heedful Trials, made with thaw'd and re-congeal'd oyl of Aniseeds, bring the second Weather-glass to be somewhat like the first; and if you know the Quantity of your spirit of Wine, you may easily enough make an estimate, by the place it reaches to in the Neck of the Instrument, whose capacity you also know, whether it expands or contracts it self to the 40, the 30, or the 20 part, &c. of the bulk it was of, when the Weather-

Weather-glass was made. By the help of the same Oyl you may make some kind of estimate, though a more uncertain one, of the difference of two Weather-glasses of unequal bigness: And though I know how much may be alledg'd to show the uncertainty of this way of making a Standard for Weather-glasses; yet as what I have formerly represented, may manifest me to be far enough from looking on it as an exact Standard of Cold; so perhaps the way propos'd may not be altogether useless in the making and comparing Weather-glasses, since in such cases, where we are not to expect to hit the mark it self, it is of some advantage to be able to shoot less wide of it then otherwise we should.

II. But not to insist any further on a difficulty, which is so hardly evitable as that, which occurs about settling a perfect Standard of Cold, there *are* unaccuratenesses in the measuring of Cold by Weather-glasses, which *may* be avoided, but are not; For, Men are not wont to take care, that the Stems be even and Cylindrical

cal enough, but are wont to make use of such, as are much wider at the upper part near the bubble, then otherwhere; nor do they observe, as they might, a proportion betwixt the Diameter of the Bore of the Cylinder, and that of the Cavity of the Spherical Bubble, and divers other circumstances are commonly neglected, which if well order'd would make much towards the Certainty and instructiveness of the Informations, afforded us by Weather-glasses. To which may be added, that even in those, where some part of the Liquor is expos'd to the external Air, there may be made Contrivances much more convenient, in order, at least, to some particular purposes, then that of the Vulgar Weather-glass, some of which we have imploy'd, and others have been either skilfully devis'd, or also happily attempted by some eminently ingenious Members of the Royal Society. And though that, which we have already describ'd in another Treatise, be very simple, yet it is much more commodious for several of the following

Dr. Wren.
Dr. Goddard.
Mr. Hook.

lowing Experiments of Cold, then
 that, which is commonly in use.
 For in this, where the included Air is
 as it were pendulous at the Top of
 the Glass, 'tis very troublesome and
 difficult so to apply Cold Bodies, and
 especially Liquid ones to it, as there-
 with to measure their Temper,
 whereas the Thermometers, I speak
 of, being made by the insertion of a
 Cylindrical pipe of Glass (open at
 both ends) into a Viol or Bottle, and
 by exactly stopping with sealing wax,
 or very close Cement the Mouth of
 the Viol, that the included Air may
 have no communication with the Ex-
 ternal, but by the newly mentioned
 Pipe : In this kind of Instrument, I
 say, by chusing a Viol as large, as
 you please, and fitting it with a Cy-
 linder, slender enough, the propor-
 tion between the part of the Viol pos-
 sessed by included Air, and the Cavity
 of the Cylinder, in which the Liquor
 is to play up and down, may be easi-
 ly made so great, as to make the Li-
 quor in this Instrument, with the
 same degree of Heat or Cold, rise
 or fall four or five, or more times as
 much

much as the pendulous Liquor is wont to do in an Ordinary Weather-glass, where the cavity that lodges the Air, is wont to be much too small, considering the Bigness of the pipe, whereinto the Air must, when 'tis rarifi'd, expand it self. But 'tis not the greater sensibility (if I may so speak) of this very kind of Weather-glasses, nor their not needing frames, that makes me take notice of them in this place (where I purposely pass by contrivances that I know to be more curious) but this other Quality, which makes them fit for divers of the following Experiments, wherein we shall have occasion to mention them, namely, that with little or no trouble and inconvenience we may imploy Liquors or other Bodies to refrigerate the included Air, by immersing the Viol, if need be (by a weight) into the Liquor to be examin'd, and letting it stand there as long as we please. And so we may also measure the Coldness of Earth, Snow, powder'd Ice, and other consistent Bodies, which may be heap'd about the Viol, or in which it may be buri'd:

Unable to display this page

be reckon'd, may render this *Hypothesis* very suspicious; besides all this, (I say) I am not inclin'd to grant (what Philosophers have hitherto suppos'd) that the Condensation of the *Air*, and the ascension of the *Water* is *only*, or so much as *principally*, affected by the proper Virtue of the Cold, but by the pressure of the *Ambient Air*, as we shall ere long more fully declare: And if this be made out, then the computation, we are considering, will be found to be very fallacious, for we have elsewhere shown, *That the strengths requir'd to compress Air, are in reciprocal proportion, or thereabouts, to the spaces comprehending the same portion of Air*; so that if a Cylinder (for instance) of four Inches of *Air*, be just able to resist a strength or pressure equivalent to 10. pound weight, when it comes to be compress'd into two Inches; in this case, I say, an equal force super-added to the former, (which makes that a double force, or equivalent to 20 pound weight,) will drive up that already compress'd *Air* into half the space; that is, into one Inch or there-

Defence
against
Linus
Cap. the
5th.

thereabouts; whence it follows, that in estimating the condensation of the *Air* in a *Weather-glass*, we must not only consider, how much space it is made to desert, but also, what proportion that deserted space bears to the whole space it formerly possessed, and to what degree of density it was reduc'd, before the application of the then force; and we must remember, that the resistance of the included *Air* is not to be look'd upon, as that of a weight, which may remain always the same, but that of a spring forcibly bent, and which is increas'd more and more, as it is crowded into less and less Room. But these Nicer speculations it would here be somewhat improper to pursue.

IV. Wherefore I shall proceed to what may seem a Paradox, that even the particular Nature of the Liquors, imploy'd in *Weather-glasses*, is not altogether to be neglected, till we have a better and more determinate Theory of the causes of Cold, then I fear we have: For, though usually it matters not much, what Liquor you imploy, yet 'tis not impossible, that

that in some cases men may slip into mistakes about them, for it will not follow, that if of two Liquors, the one be much the more obnoxious to the higher degree of Cold, that of Glaciation, the other must be less easily susceptible of the lower degrees of Cold; since those, that make seal'd Weather-glasses, some with water, and some with spirit of wine, have confessed to me, that they find these (last nam'd) much more apt to receive notable impressions from faint degrees of Cold, then those that are furnished but with water, and (which yet is easily turn'd into Ice) by the cold of our Climate, which will by no means produce the like effect upon pure spirit of Wine.

Besides we cannot always safely conclude (as Philosophers and Chymists generally do) that the more subtile and spirituous Liquors must be the least capable of being congealed (that is, made to lose its fluidity, as oyl and some other substances are wont to be reduc'd to do by the Action of Cold) for the Chymical Oyl of Aniseeds distill'd by a Limbeck is
so

so hot and strong a Liquor, that a few drops of it conveniently dissolv'd will make a whole Cup of Beer taste as strong, and perhaps heat the Body as much as so much Wine, and yet this hot and subtle Liquor I have found upon Trial, purposely made, to be more easily congealable (in the sense freshly explain'd) by cold, then even common water; and to continue so several days, after a Thaw had resolv'd the common Ice into fluid water again. And I know some distill'd Liquors, whose component particles are so piercing and so vehemently agitated, that the tongue cannot suffer them, and they are not perhaps inferior to most Chymical Oyls, nor to *Aqua fortis* it self, and yet these may be congeal'd by far less degrees of Cold, then such, as would yet prove ineffectual to freeze either the generality of Chymical Oyls, or the generality of saline spirits.

And indeed till we attain to some more determinate Theory of Cold, and come to know more touching its causes, then we yet do, I see not,

E

why

why it should be absurd to suspect,
 that though there be some kind of
 Bodies, which seem fitted to pro-
 duce Cold indiscriminately in the
 Bodies they invade or touch, yet if
 the refrigeration of a Body be but the
 lessening of the wonted or former
 agitation of its parts (from what
 cause soever that remissness proceeds)
 it seems not impossible, but that be-
 sides those Bodies or Corpuscles,
 that may be look'd upon as the Ca-
 tholick Efficient of Cold, there
 may be particular Agents, which in
 reference to this or that particular
 Body may be call'd frigorisick,
 though they would not so much re-
 frigerate another Body, which per-
 haps would be more easily affected,
 then the former, by other efficient of
 Cold. For we may observe, that
 Quicksilver may be congeal'd by the
 Steams of Lead, which have not been
 taken notice of to have any such Effect
 upon any other fluid Body, and yet
 Quicksilver is not to be depriv'd of
 its fluidity by such a degree of Cold,
 as would freez not only water but
 wine. And by what we have for-
 merly

merly related upon the credit of that great Traveller, the Jesuit *Martini-
us*, it seems, that water it self may in some Regions be so dispos'd by the constitution of the Soyl, that 'tis susceptible of strange impressions of Cold in proportion to the Effect, which that degree of Cold produces there in humane Bodies. Besides, *Opium* also, of which three or four grains have too oft destroyed the heat of the whole mass of Blood in a mans Body, though that be a very hot, subtile, and spirituous Liquor, does not sensibly refrigerate water, as far as I could observe with a good seal'd Weather-glass, which I put sometimes in a glass of ordinary water, and sometimes into a glass of water of the same Temper, and (as we guess'd) of the same Quantity, wherein *Opium*, enough to kill very many men, was put in thin slices, and suffered to dissolve; which seems to argue, that as differing Liquors have each their peculiar Texture, so there may be certain Bodies, whose minute particles by their peculiar seize, shape and motion, may be qualified

to hinder, or at least lessen the agitation of the particles of the appropriated Liquor, into whose pores they insinuate themselves; And thereby, according to the lately mention'd supposition, they may refrigerate that particular Liquor without having the like Effect on other Liquors, whose Textures are differing. And I might countenance this by adding, that as fiery and agitated a spirit as that of wine, when well defleem'd, is justly thought to be; yet I know more liquors than one, that being mingled with it, will in a trice deprive it of its Fluidity; and the like change I have sometimes made in some other liquors also. But I must not insist on such matters, having mention'd them but only to awaken mens curiosity and circumspection, and not to build much upon them, which will be easily credited, if it be remembred, that a little above I myself sufficiently intimated, that this Conjecture supposes something about the Theory of Cold, which is not yet sufficiently clear'd. Only, because the former Experiments show, That
the

the various Agitation of the minute parts of a Liquor, whereon its Fluidity depends, may be hindred or suppressed by the intervention of adventitious Corpulcles : But do not *clearly* show, That the Liquor by being depriv'd of that Kind of Agitation does actually acquire a Coldness : I might subjoyn thus much, that by the Addition of a certain substance (which for just reasons I must forbear to describe) that would scarce sensibly refrigerate common Water ; I can make a certain (and for ought I know, one only) Liquor, that is wont to the touch to be much of the Temper of Water, to conceive a considerable degree of Coldness : This, I say, (as strange as it may seem) I might here subjoyn to countenance the Conjectures, I have been delivering, and afford some new Corrolaries ; but for the Reason newly intimated I forbear, and the rather because I think it high time to return thither, whence the Considerations, I have offer'd about Weather-glasses, have made me digress.

I was going then to take notice, upon the Occasion offer'd by what I related of the Influence of the Atmosphæres gravity upon common Weather-glasses, of the difference between them and thole that are Hermetically seal'd. And indeed, *these* are in some things so much more convenient then the others, that (if I be not mistaken) it has already prov'd somewhat serviceable to the Inquisitive, that I have directed the making of the first of them, that have been blown in *England*; At the Beginning indeed I had difficulty to bring men to believe, there would be a rarefaction and condensation of a liquor Hermetically seal'd up, because of the School Doctrine touching the impossibility of a *vacuum*, and especially, because I had never seen any Experiment of this kind, nor met with any that had, but after some Trials, which my Conjectures led me to make successfully enough, that in Hermetically seal'd Glasses, both Air and Water might be alternately rarifi'd and condens'd; I found my work much facilitated by the sight of

a small seal'd Weather-glass, newly brought by an Ingenious Traveller from *Florence*, where it seems some of the Eminent *Virtuosi*, that enobled that fair City, had got the start of us in reducing seal'd Glasses into a convenient shape for Thermoscopes. But since that, the Invention has in *England* by a dexterous hand, that uses to make them for me, been improv'd, and the Glasses we now use are more conveniently shap'd, and more Exact then the Pattern, I caus'd the first to be made by. But the filling of these long ones that we now use, is a work of more niceness and difficulty, then they that have not tri'd will be apt to imagine, and therefore may elsewhere deserve either from our Pen, or his, that is most vers'd in making them, a more particular account of the way of Performing it : The advantages of these Weather-glasses being at no hand inconsiderable. For, the weight or pressure of the Atmosphere (which, as we have noted, may work very much upon others,) their being seal'd defends them from : And by

this Advantage they may be us'd in the highest and in the deepest places, with as much certainty as any where else. Next, whereas in other Thermometers the Liquor is very subject to be spilt, in case they be removed from place to place, and which is worse, though they be not remov'd, is subject to be prey'd upon and wasted by the Air, whereby informations of such Weather-glasses are rendered in Tract of time somewhat uncertain: In seal'd Weather-glasses, there is no danger, that Liquor should either spill or evaporate. And upon the same Account, *these* have this Advantage, that you may safely let them down into the Sea, and immerse them in any Liquor, you please, without excepting the most corrosive to examine their Coldness: Not to mention, that instead of the courser Liquors used in common Weather-glasses, which are some of them not unapt to freeze, and others unapt enough to comply with the slighter alterations of the Air, and instead of the colourless Liquor, whether water or no (I know
not,

not) us'd in the *Florentine* Weather-glass I saw, We imploy highly rectifi'd spirit of Wine, whose being brought to a lovely red with Cochinele, open'd by the most subtile volatile spirit of Urine, by which means the included Liquor is not only very conspicuous and secur'd from freezing, but so susceptible of even the slighter impressions of external Bodies (which would work but faintly on water) that 'tis pleasant to see, how many Inches a mild degree of heat will make the Tincture ascend in the very slender Cylindrical stem of one of these useful Instruments; of which we have spoken the more particularly in this place, because we shall have frequent occasions to mention them in the following Papers; and no body as yet, that we know, has written any Account of them.

But though these Weather-glasses be much more to be relied on, than those that are commonly in use, yet we would have a Philosopher look upon both these and our Sensories, but as Instruments to be imployed by his Reason, when he makes his Estimates

mates of the Coldness of Bodies: And though perhaps it will signifie nothing in the Event, yet I see not, why it should misbecome a Naturalists Diligence and circumspection to try, whether ev'n such weather-glasses ought to be so far allow'd of, as to hinder men from looking after any other kind of ways of estimating Cold.

For, though the sealing of these Weather-glasses protect the included Liquor from the pressure of the Air, and keep it from evaporating, yet it will not follow from hence, that they must be exempt from all the other imperfections, which we formerly mention'd to be imputable to Weather-glasses.

I know not whether you will allow me to add on this occasion, that the tincted spirit of Wine (and the like may (for ought we know) be said of any such Liquor) being a particular mixture, in case it be allow'd possible, that the subtile steams of such Bodies (as we formerly noted to be frigorifick in respect to some Liquors) may insinuate themselves through the pores

pores of Glass; as 'tis granted, that the *Effluvia* of the Loadstone do readily per-meate It: in this Case, I say, though I willingly allow it not to be *likely*, yet it is not absolutely *impossible*, that some Steams, that wander through the Air, may be more or less Cold, or may more promote or hinder an agitation among the minute parts in reference to It, then in reference to other Liquors: as we formerly noted, that a grain or two of *Opium* will exceedingly allay the warmth and motion of the whole mass of Blood in a mans Body, thoughten times that Quantity will not sensibly refrigerate the tenth part of so much water. And that this may appear the less extravagant, I shall here add some mention of an odd *Phænomenon*, that, as it were, by some Fate has occur'd to me, since I began the Discourse I am now upon; for whilst I was yesterday writing It, I had occasion to Examine by such a Seal'd Weather-glass (as I have been speaking of) the Temper of a certain strange kind of mixture, that towards the close of this Treatise,

tise, I shall have Occasion to take special Notice of: and though to the touch it appear'd but Lukewarm, yet having put into it the Ball, and part of the stem of the seal'd Weather-glass, I found the Included Liquor slowly enough impell'd up so high, that at length, to my wonder, it rose eight or nine Inches in a Stem, which was not much above a foot long; but that which I relate, as the surprizing Circumstance, is, that when I had taken out the Thermoscope, and remov'd it again into a deep Glass full of Cold water, whence I had just before taken it out, to put it into the Anomalous mixture, I had a mind to examine; the Tincture in the Weather-glass did not (as it was wont, and as any one would have expected) begin to subside again towards its former station, but continued within about half an Inch or less of the very Top of the Instrument, though neither my own busie Eyes, nor those of a person very well Vers'd in making and using Thermoscopes, could perceive, that the expanded Tincture was any where

where discontinued by any Air or Bubbles, which at first we suspected might possibly (though it were very unlikely) have been generated by the Tepor of the mixture, But that which continued our wonder, if not increased it, was, that during four or five hours, that the Instrument continued in the Cold water, and during some hours also, that it was expos'd to the Air, the Tincture did not subside above half an Inch; and which is yet more strange, having left the Glass all night, in the window of a Room, where there was no Chimney, I found in the morning, that its descent was scarce sensibly greater, for it continued about eight Inches higher, then the mark it stood at, when I first put it into the Lukewarm mixture, and how long it will yet retain this strange expansion, is more then I can tell. But by this and what I may have occasion hereafter to relate concerning this mixture, it may appear somewhat the more reasonable to suspect, that even seal'd Weather glasses furnished with high rectifi'd spirit of Wine, may in some (though very

very rare) conjunctures of Circumstances, and from some peculiar Agents, either by their insinuating themselves through the Pores of the Glass, or on some other Account, receive impressions, that, as far as can easily be discern'd, are not purely the genuine and wonted Operations of Heat and Cold.

*Theatr.
Chymic.
volum. 6.*

The Chymist *Orthelius* tells us, that the Liquor distill'd from the Oar of *Magnesia* or *Bismute* (which seems to be the same Mineral, that we in English call Tin-glass) will swell in the Glass 'tis kept in, not only manifestly, but very considerably at the full Moon; and shrink at the new Moon; and if all my endeavours to procure that Oar had not prov'd fruitless, I should be able by my own Experience to disprove or confirm so admirable a *Phænomenon*; but being as yet unfurnish'd to make the Trial my self, lest it might appear a Vanity, so much as to mention (without rejecting it) a thing so very unlikely : I shall add, that since I find the Thing for the main, which was delivered by the Chymist, imploy'd

as an Argument by a famous Mathematician (the Jesuite *Casatus*) whose expressions are such, as if he himself had observ'd, that even in stoppt Glasses, the foremention'd Mineral spirit increased very sensibly in Bulk about the time of the full Moon, which wonder being admitted, may not only countenance what we were saying, but hint some other very strange things in Nature. This brings into my mind (what I have elsewhere mention'd) that a Tincture of Amber, I had made with high rectifi'd spirit of Wine, did for many Moneths in a well stoppt Glass discover it self to be affected with certain changes, which were thought to proceed from some secret mutations of the Air, that did sensibly

Vitrum optimè clausum ne quid exspirare posset, in loco ubi quiesceret statui, nec sine animi voluptate licebat in Pleniluniis manifesta inclusi liquoris incrementa observare, in Noviluniis vero Decrementa, &c. They are the words of *Paulus Casatus* in his *Terra Machinis mota*, Pag. 143. But since the writing of these Preliminary Discourses, the Author of them having consulted by the means of some Ingenious friends, the learned *Casatus*, finds, that He never made nor saw the Experiment himself, but relates it upon the authority of a certain Dutchman, whose name he adds not, and who therefore may probably be the same *Orthelius* that is mention'd by the Author of these Preliminary Discourses, who thinks it requisite to give the Reader this Advertisement, because *Casatus* himself did not, as he should have done, intimate that he deliver'd this but upon another's credit.

sensibly so work, as *I* had not observed it to do upon other Liquors, wherein the spirit of Wine abounded. And perhaps upon long and diligent observation, one might find a Disparity betwixt Weather-glasses kept in the same place, but furnished with differing Liquors, a Disparity, *I* say, that could not be so well ascrib'd to any thing as to the peculiar Nature of the Respective Liquors, which, though of divers kinds, may (to add that towards the facilitation of Trials) be made of a very conspicuous colour, by the self-same Metal, Copper, which not only gives the Known colour in *Aqua fortis*, but affords a fair solution in *Aqua Regis*, and it makes a Liquor of a most deep and lovely blew in spirit of Urine, or of *Sal Armoniack*, and the like; nay, *I* have found, that in good Chymical Oyl of Turpentine (for express'd oyls are too easily congeal'd) the bare filings of it will yield a sufficient Tincture. But because it is yet but a bare suspicion, that Seal'd Weather-glasses made of differing Liquors, but in other points alike may be other-

otherwise then uniformly affected by the Temperature of the External Air; I shall now add an observation already made, to show, that even the Seal'd Weather-glasses furnish'd with Spirit of Wine are not so perfectly secluded from all commerce with external Bodies, and liableness to their operations, but that they may be wrought upon otherwise then we think. For I have more then once observ'd, that even in seal'd Thermoscopes (made purposely at home for me, and with great care by the expertest maker of Them) after a good while, and when no such matter was expected, there have emerg'd Bubbles, which, whether they proceeded from some undiscernable Particles of Air, harbour'd in the Pores of the Water, which in process of time, by their Union came to make conspicuous Bubbles, or from some dispos'd particles of the spirit of Wine it self by successive alterations brought to a state of Elasticity, I now examine not; but only affirm, that sometimes I have had of these Bubbles great enough to possess

F

the

the space of many Inches in the Shank of a long seal'd Weather-glass, and I have been troubled with them in more Weather-glasses then one or two : which I therefore take Notice of, not only, because it serves to prove what I was saying, but because it is very fit, an Advertisement should be given of it to prevent mistakes. For when these Bubbles are small, and are generated or happen to stay at or about the Place, where the Sphærical and Cylindrical parts of the Glass meet, they may easily (as I have observ'd) lurk unheeded, and reaching from side to side, so divide the spirit of Wine in the Ball from That in the Stem, that the latter shall not be able to rise and fall according to the changes of the weather ; the Bubble notwithstanding its aerial nature, being more indispos'd to be mov'd up and down in the slender Stem of a small Weather-glass, then the spirit of Wine it self, as we have elsewhere shown, that when Air is not forc'd, a Bubble of it will not in several cases so readily pass through a very narrow passage, as would that grosser fluid, Water.

But

But all these difficulties (not to call them extravagances) which I have been mentioning about seal'd Weather-glasses, I represent not to show, that it is (at least as yet) worth while to suspect ours so far, as to imploy all the Diligence and Inventions, that were requisite to prevent or silence the suspicions of a Sceptick, or that might be thought upon, in case the matter did require or deserve such extraordinary Nicety, but only to give men a rise to consider, whether it would be amiss to take in (when Occasion presents it self) as many collateral Experiments and Observations as conveniently we can, to be made use of as well as our Sensories and Weather-glasses in the Dijudications of Cold. And perhaps an Attentive Enquiry purposely made, would discover to us several other Bodies, Natural or Factitious, which we might make some use of in estimating the degrees of Cold. For though (to give an instance) water be thought the Liquor, that is most susceptible of such an Intensity of Cold, as will destroy or suspend its

Fluidity, yet not here to repeat, what we formerly deliver'd of the easie congealableness of Oyl of Ani-seeds, we have (as we elsewhere note to another purpose) distill'd a substance from Benzoin, which becomes of a fluid, a consistent Body, and may be reduc'd to the state of fluidity again by very much lesser alterations of the Ambient Air, as to Heat and Cold, then would have produc'd Ice or Thaw'd it. I could also here take notice of, what I have sometimes observ'd in Amber-greese, dissolv'd in high rectifi'd spirit of Wine, or in other Sulphurous or Resinous concretions dissolv'd in the same Liquor; for now and then, though it seem'd a mere Liquor in warm Weather, it would in Cold weather let go part of what it swallow'd up, and afterwards redissolve it upon the return of warm weather; some of these concretions, as I have seen in Excellent Amber-greese, shooting into fine figur'd masses, others being more rudely congeal'd. And I might also add, what I have observ'd in Chymical Liquors, (not unskillfully prepar'd

par'd out of Urine, Harts-horn, &c.) which would sometimes seem to be totally clear Spirits, and at other times would suffer a greater or lesser proportion of Salt to ChrySTALLIZE at the Bottom, according to the Mutations of the Weather, in point of Cold and Heat. Such kind of instances (I say) I could mention, but I shall rather chuse to prosecute my Examples in that obviousest of Liquors, Water, and add, that even That may afford us other Testimonies of the increased or lessen'd cold of the Air, then that which it gives us in Common Weather-glasses. For in some parts of *France* the Watermen observe, that the Rivers will bear Boats heavier laden in Winter, then in Summer; and I have upon inquiry been credibly inform'd, that Seamen have observ'd their ships to draw less water upon the Coasts of frozen Regions (where yet the Sea is wont to be less brackish) then they do on our British Seas: which argues, that water is thicker and heavier in Winter then in Summer. Nay, I shall add, that not only in

*L' Hydro-
graphie du
P. Four-
nier, liv.
18. Cap.
12.*

differing Seasons of the Year, but even at several times of the same day *I* have often observed the Coldness of the Air to be (regularly enough) so much greater at one time of the day then at another, that a Glass bubble Hermetically seal'd and pois'd so as to be exactly of the same weight with its equal Bulk of Water, as that Liquor was constituted at one time of the Day, would about Noon, when the warmth, that the Summers Sun produc'd in the Air, had somewhat rarifi'd the water, and thereby made it bulk for bulk somewhat lighter then before, the Bubble would sink to the Bottom of the water, which (for the better marking the Experiment) *I* kept in a Glass-Tube; but when at night the coolness of the Air had recondens'd the water, and thereby made it heavier, it began by little and little to buoy up the Bubble, which usually by morning regain'd the Top of the Water; and at other times of the day it not unfrequently happen'd, that the Bubble continued swimming up and down betwixt the Top and the Bottom,

tom, without reaching either of them, sometimes staying so long in the same part of the Tube, that it much surpriz'd divers of the *Virtuosi* themselves, who thought the poising of a weight so nicely, not only a *very great* difficulty (as indeed it is) but an insuperable one. But of this Experiment I elsewhere say more; and because about other Weather-glasses I have said so much already, I think it may not be improper to Sum up my thoughts concerning the *Criteria* of Cold, by representing the following particulars.

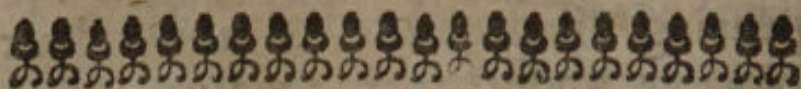
1. That by reason of the various and unheeded predispositions of our Bodies, the single and immediate informations of our senses are not always to be trusted.

2. That though *Common Weather-glasses* are useful Instruments, and the informations they give us are in most cases preferable to those of our sense of touching, in regard of their not being so subject to unheeded mutations: yet ev'n these Instruments being subject to be wrought upon by the differing weights of the Atmosphere,

Unable to display this page

the estimating of Cold, by their appearing disagreeable to the vulgar Notions about that Quality. For I doubt, our Theory of Cold is not only very imperfect, but, in great part illgrounded. And I should never have ventur'd at trying to make seal'd Weather-glasses, if I could have been withheld either by the grand Peripatetick Opinion, that (to shun a void) water must remain suspended in Glasses, where if it fall, the Air cannot succeed it; or the general opinion ev'n of Philosophers as well new as old, That Air must be far easier then any visible Liquor condens'd by Cold.

The



The III. Discourse,

Containing

The II. Paradox,

Viz.

Touching the Cause of the Condensation of Air, and Ascent of Water by Cold in Common Weather-glasses.

THough I thought here to end the Præliminary Discourse, as doubting it may be thought prolix enough already, yet for confirmation of what I was lately noting, about the incompleteness of the Theory of Cold (and because the evincement thereof may give rise to many Trials that may enrich the History of Cold) I will here subjoyn a Discourse formerly written on another Occasion. For though upon that Account I am
fain

fain to leave out the beginning of It, as not futed to the present Occafion, yet the main Body of the Difcourfe may be (I think not improperly) annex'd to what has been already laid about Weather-glafles, fince it examines the caufes of the principal *Phænomenon* of them, and will perhaps help to difcover the incompleteness of mens Notions about Cold, by fhewing that the true caufe, ev'n of the moft obvious *Phænomenon* of Common Weather-glafles (though almoft every man thinks he underftands It) has not yet been fufficiently inquir'd into.

The difcourfe then (that firft part of It as forreign to our present purpose) being omitted, is as follows.

—To profecute our Difquifition fatisfactorily, it will concern us to confider, upon what Account the water riles in Cold Weather and falls in Hot, in common Weather-glafles, whole Conftitution being fo well known, that we need not fpend time to fet it down, we may forthwith proceed to take notice, That concerning the reason, why in thefe
Weather-

Weather-glasses the water, or other Liquor in the shank or pipe, ascends with Cold, and descends with Heat: there are three opinions, that will deserve our Consideration.

The first is the common opinion of the Schools and Peripateticks, and indeed of the generality of learned Men of differing Sects, who teach, that the Cold of the External Air, contracting the Air included in the Weather-glass, and thereby reducing it into a narrower Room then formerly it posselt, the water must necessarily ascend to fill the place deserted by the retired Air, lest that space should become a *vacuum*, which Nature abhors.

But against this Explication we have several things to object.

For first, I am not satisfi'd, that any of the Schoolmen or Peripateticks (at least of those I have met with) have solidly evinc'd that Nature cannot be brought to admit a *vacuum*. Nor do I much expect to see that assertion well prov'd, by these, or by any other, that forbear to make use of the Argument of the
Cartesians

Cartesians drawn from the Nature of a Body, whose very essence they place in its having extension : which I say, because about this Argument I neither have yet published, nor do now intend to deliver my thoughts.

Next, it seems a way of Explicating, that little becomes a Naturalist, to attribute to the senseless and inanimate Body of water an Aim at the good of the Universe, strong enough to make it act, as if it were a free Agent contrary to the tendency of its own private Nature to prevent a *Vacuum*, that, as is presum'd, would be hurtful to the Universe.

But these Arguments we have elsewhere urg'd, and therefore need not insist longer on them here.

Thirdly, if you take a Bolthead, with a large Ball and long stem ; and do, with that and Quicksilver make the Torricellian Experiment, there will be an Instrument prepar'd like a Common Weather-glass, save that the stem is longer, and that the Liquor is *Mercury* instead of Water, and yet in this case we see not, that the *Mercury*, which remains pendulous

lous in the pipe at the height of about 30. Inches, offers to ascend into the cavity of the Bolthead, to fill up the space, whence the Air was expell'd by the *Mercury*, and which the Quicksilver also by its subsiding deserted. And the outward application of Cold Bodies to the forsaken part of the head will not perhaps Occasion the rising of the Quicksilver a $\frac{1}{4}$ of an Inch, if half so much, though the like degree of Cold would make the water ascend in a Vulgar Thermometer, though shorter, to the height of several Inches. But this Argument I also on another Occasion further display and vindicate.

Wherefore I shall add one more, taken from the Consideration of these seal'd Weather-glasses, that are describ'd in this present History of Cold. For, in these the Air does not shrink, but rather seems to be expanded, when the weather grows Colder. If it be said, that water being contracted by the Cold, the Air follows it to prevent a *Vacuum*: I answer, that those, that say this, should explain, *why*, whereas in Common
Weather-

Weather-glasses the water ascends to follow the Air, in these the Air must descend to follow the water: And *why*, since to avoid a *Vacuum* the one in common Weather-glasses, and the other in seal'd ones resists contraction, Nature does not rather make the Air in Common Thermometers, retain the extension, they conceive due to its nature, then put her self to the double Labour of suffering the Air to be preternaturally condens'd, and compelling the water to ascend contrary to its nature. But these Arguments *I* will not urge so much, as this other, that in our present case, the above propos'd Answer will by no means salve the difficulty. For if the water be really condens'd into less, and the Air expanded into more space then they respectively possess before; *I* see not, how a *Vacuum* or a worse Inconvenience will be avoided; for *I* demand, since Glass is granted to be impervious to Air and water (as indeed else Nature would not need to make water ascend contrary to its own tendency in a Common Weather-glass)
 what

what becomes of the Body, that was harbour'd in the space deserted by the water upon its Condensation? Which Question, those that do not say, any thing escaped away through the Glass, or that any thing was annihilated, will not easily answer. But this is not all, for I further demand, when the Air expands it self to follow the water, how by that expansion of the Air, a *Vacuum* both *coacervatum* (as the old *Epicureans* spoke) and *interspersum*, is avoided. For the aerial Corpuscles cannot advance into this space deserted by the water, without leaving either in whole or in part the spaces they fill'd before, so that by this remove an aerial Corpuscle only *changes place*, but does not adequately *fill any more place* then it did before. But if it be said, that the same Air without any substantial *Accession*, may adequately fill more space at one time then at another: If this, I say, be pretended, I shall not urge that it appears not, why it were not more easie for Nature in common Weather-glasses, as well as in seal'd ones, to rarifie the
Air,

Air, which they teach to be so very easily rarifi'd and condens'd, then to make the heavy Body of water to ascend. For I may very well reply, that I scarce know any Opinion in Natural Philosophy, that to me seems more unintelligible, and more worthy to be confidently rejected, then This harsh *Hypothesis* of Rarefaction. Of which I should think it injurious to so judicious a Philosopher, as my Lord *Brouncker*, to endeavour here to manifest the absurdity, though I had not in another place shewn it already.

*Defence
against
Linus
Cap. 3.*

The next Opinion, we are to consider touching the cause of the ascension of Water by cold in Weather-glasses, is that of Mr. *Hobs*, who, in the last Chapter of his Book *de Corpore*, Sect. the 12. having premis'd a delineation of a common Weather-glass, subjoyns this Explication :

In the sixth and seventh Articles of the 27. Chap. (where I consider the cause of Cold) I have shewn, that fluid Bodies are made colder by the pressure of the Air, that is to say, by a constant wind that presseth them.

G

For

For the same cause it is, that the superficies of the water is press'd at F, and having no place, to which it may retire from this pressure, besides the Cavity of the Cylinder between H and E, it is therefore necessarily forced thither by the Cold, and consequently it ascendeth more or less according as the Cold is more or less increas'd. And again, as the Heat is more intense, or the Cold more remiss, the same water will be depress'd more or less by its own gravity, that is to say, by the cause of gravity above explicated.

But however the Author of this Explication, to prepare us to receive it, tell us, that however the above mention'd *Phænomenon* be certainly known to be true by experience, the cause nevertheless has not yet been discover'd: yet I confess, I think, this newly recited assertion might as well have been plac'd after his explication, as just before it.

For first, whereas he remits us to the sixth and seventh Articles of the 27. Chapter (for the reference is misprinted) as containing the grounds

grounds of this Explication, I must profess my self far from being satisfi'd with the general Theory of Cold deliver'd in that Chapter, as being partly precarious, partly insufficient, and partly scarce intelligible, as I shall elsewhere have Occasion to shew : and as for what he particularly alledges in the sixth and seventh Articles of a constant wind, that presses fluid Bodies, and makes them Cold, besides that that is prooflessly affirm'd, we shall anon have Occasion to mention an Experiment, where water was not only much refrigerated, but turn'd into Ice, though it were seal'd up in Glass Vessels, and those suspended too in other Glasses, wherein some of them had Air about them, and some others were totally immers'd in unfreezing Liquors, so that the water that was seal'd up was sufficiently protected from being *rasked* by the wind, as Mr. *Hob's* concept of the Cause of freezing requires.

Secondly, I see no necessity, that the Cold should press up the superficies of the Water into the shank of

the Weather-glass, especially since 'tis manifest, that the Water will rise with Cold in a Weather-glass kept in a still place, and free from any sensible wind. Besides that, it should be prov'd, and not barely affirm'd, that an insensible Motion deserves the name of wind, and that such a one is the cause of the refrigeration of water, and it should be also shewn, how this wind comes to be able to raise the water, and that to the height of many Inches more in one part of the superficies then in another. Besides all this, I say, we find by Experience, that Water powred into a Bolthead, till it have fill'd the Ball, and reach'd a good way into the Stem, will upon a powerful refrigeration, short of freezing (which is the case of water in Weather-glasses, when the Air grows colder) manifestly shrink into a narrower room, instead of being impell'd up higher in the Pipe. And if in an ordinary Weather-glass, with a long shank, you apply a mixture of Ice or Snow, and Salt to the Bolthead, the water will readily ascend in the shank to the height

height of divers Inches, which how it will be explain'd by Mr. *Hob's Hypothesis*, I do not well see.

Thirdly, I wonder he should tell us, that the reason why the press'd water ascends into the shank of the Weather-glass, is, because it hath no other place into which it may retire from the pressure of the wind, since he, rejecting a *Vacuum*, and affirming the world to be every where perfectly full, should not, methinks, have so soon forgotten, that in the very Paragraph or Section immediately preceding this, himself had told us, that he *cannot imagine, how the same place can be always full, and nevertheless contain sometimes a greater, sometimes a less Quantity of matter; that is to say, that it can be fuller then full.* So that I see not, why the water should find more room to entertain it, in the Cylindrical cavity of the Weather-glass already adequately fill'd with Air, then elsewhere. And in the seal'd Weather-glasses, we have above been mentioning, and wherein the water descends with Cold, 'twill be very hard for Mr. *Hobs* to

*Sect. II. of
the same
30. Chap.*

Unable to display this page

proceeds not from the bare sinking of the water, but from its being thrust down by the pressure of the incumbent Air; since the waters own weight, would make the internal water fall but to a level with the surface of the external water, and not so much beneath it. And for further proof, you may, by keeping such a Weather-glass long enough in the hot Sun, bring the Air so far to expand itself, as to drive the water out of the shank, and break through the external water in divers conspicuous Bubbles, after whose eruption the remaining Air being again refrigerated by the removal of the Weather-glass into a cooler place, the loss of that part of the Air, that escap'd away in Bubbles, will make the water ascend higher in the shank, then in the like degree of Cold, it would formerly have been impell'd. And thus much may suffice to shew the unsatisfactoriness of Mr. *Hob's* conceipt.

The third and last opinion we shall mention, is, that of some ingenious modern Naturalists, who acknow-

ledging that the Air has a weight (which Mr. *Hobs* also does in effect admit, though he make not so good use of it as they) do by that explicate the ascension of water in Weather-glasses, teaching that the Cold of the Ambient Air making the included Air shrink into far less room then it possesse before, the water in the sub-jacent Vessel is, by the weight of the incumbent Air, which presses on it more forcibly in all the other parts of its surface, then it is press'd upon in that included in the shank, impell'd up into that part of the shank, which was newly deserted by the self-contracting Air.

But though this Account be preferable by far to those which we mention'd before it, and though it be not only ingenious, but, as far as it reaches, true, yet to me I confess it seems not sufficient, and therefore I would supply what is defective, by taking in the pressure, (and in some cases the spring) of the external Air, not only against the surface of water (for That the newly mention'd explication likewise does) but also against the internal

or included Air. For the recited *Hypothesis* gives indeed a rational account, why the water is impell'd into the place deserted by the Air; but then supposes, that the Air is made to contract it self by cold alone, when it makes room for the water that succeeds in its place, whereas I am apt to think, that both the effects may proceed, at least in great part, from the same cause, and that the pressure of the contiguous and neighbouring Air, does according to my Conjecture eminently concur to reduce the cool'd Air, shut up in the Weather-glass, into a narrower space. This it does in common Weather-glasses, because the Ambient Air retains the whole pressure, it has upon the Account of its weight, whereas the internal Air by its refrigeration, even when but equal to that of the External Air, looses part of the pressure, it had upon the account of its now weakned spring.

But this, as I newly intimated, is not the sole account, upon which the Air may in some sorts of Weather-glasses impel up the water, and contribute

tribute to the condensation of the Air incumbent on the water. For in some circumstances (one or two of which we shall produce by and by) it may so happen, that the rest of the Air, that bears upon the water to be rais'd, will not be so much refrigerated, as the included Air, that is to be condens'd, and consequently the other Air will have a stronger spring, then this last mention'd Air will retain, and therefore the former will have a greater pressure, then the latter will be able to resist.

We shall not now examine, *whether* the spring of the Air depend upon the springy structure of each aerial Corpuscle, as the spring of wool does upon the Texture of the particular hairs it consists of, *or* upon the agitation of some interfluent subtile matter, that in its passage through the aerial particles whirls each of them about, *or* upon both these causes together, *or* upon some other differing from either of them; but this seems probable enough, that as, when Air, being seal'd up in a Glass,

is

is afterwards well heated, though it acquire not any greater dimensions, as to sense, then it had before, yet it has its spring much increas'd by the Heat, as may appear, if the seal'd Tip be broken under water, by the eruption of Bubbles by the endeavour of the imprison'd Air to expand it self; so upon the refrigeration of the Air, so seal'd up, though the additional spring (if I may so speak) which the Heat gave it, will be lost upon the recess of that Heat, or as soon as the effect of that heat is destroy'd, yet there will remain in the included Air a considerable spring, and sufficient to make it as well fill (at least as to sense) the cavity of the seal'd Glass, as it did, when its spring was stronger. And proportionably we may conceive, that though Cold, at least such as we meet with in this climate of ours, do make the spring of an included parcel of Air weaker, then it was before the refrigeration of that Air, yet it may not make it so much weaker, but that the aerial Corpuscles may be kept so far extended as not at all (or, scarce sensibly)

to

to quit the room they possess before; in case there be not contiguous to them any other Body, which by its pressure indeavours to thrust them inwards, and so make them desert part of that space: which clause I therefore add, because, that if the case propos'd *do* happen, 'tis obvious to conceive, that the weakned spring of the Air cannot retain so much force to resist an external pressure, as it would have, if the Cold had not debilitated it, and consequently this cooled Air must yield and suffer it self to be condens'd, if it come to be expos'd to a pressure, to which it was but equal before its being weakned. And such in common Weather-glasses is the pressure, that is constantly upon the surface of the water without the Pipe, upon the account of the gravity of as much of the Air or Atmosphære, as comes to bear upon it.

Having thus explain'd our conjecture, we will now proceed to the Experiments we made to countenance it, as we find them entred in our loose notes.

In one of which I find what follows. We

We took a Viol capable of containing five or six ounces of water, and having fill'd it almost half full with that Liquor, we inverted into it a Glass-pipe of about 10. Inches long, and much bigger then a large Swans Quill, seal'd at one end, and at the other fill'd top full with water, so that the open Orifice being immers'd under the Vessell'd water (of the Viol) there remain'd no Air at the Top of the Pipe: Then, as much of the Orifice of the Viols neck, as was not fill'd by the pipe, being carefully clos'd with Cement, that no Air could get in or out, the Viol was plac'd in snow and salt, till the vessell'd water began to freez at the Top and Bottom: And according to our expectation we found, that notwithstanding this great degree of infrigeration of the Air in the Viol, the water in the Pipe did not at all descend, So that either the Air did not shrink by so great a Cold, or the water, whether to avoid a *vacuum*, or otherwise, did not remove out of the Pipe to possess the place deserted by the refrigerated Air.

After-

Afterwards we endeavoured to repeat the Experiment with the same Glasses, but having had occasion to be absent a little too long (though not very long) we found at our return the upper and seal'd part of the pipe beaten out, which we suppos'd to have been done by the intumescence of the water in the Viol upon its glaciation.

Wherefore we fastned into the same Viol another Pipe some Inches longer then the former, and drawn very slender at the seal'd end, that it might easily be broken there, and having set the viol to freez as before, without finding the water to descend in the Pipe, we did with a *forceps* break off the slender seal'd end, that the outward Air might come to press upon the suspended water, and, by it, upon the cool'd Air in the viol, whereupon, as we expected, the water was swiftly depress'd, by our estimate, eight or ten Inches, but not so low by a pretty deal, as the surface of the water in the viol.

After this, by rarifying the Air in the Viol, and by blowing into it through

through the pipe, the water was rais'd within about half an Inch of the Top of the Pipe, whose slender end being seal'd, the viol was again plac'd in snow and salt, but the spring of the Air at the Top, which was rarifi'd before, was by refrigeration so weakned, that it was unable sensibly to depress the water; wherefore breaking off the Apex, as before, the upper Air immediately drove it down divers Inches.

Our last Tryal therefore, was to leave in the same Pipe about 3 $\frac{1}{2}$ Inches of Air rarifi'd, as little as we could, and placing the viol in salt and snow, as before, we observ'd, that the Air in the Pipe did, upon the refrigeration of the Air in the viol, expand it self very little, though the water in the Viol were in part turned into Ice; but upon breaking off the slender seal'd end, the outward Air presently depress'd the water above two Inches beneath the last level, and by removing the Glass into a warmer room, we found, that the water ascended a pretty deal above an Inch higher, then the same
upper,

uppermost level, whereby we probably concluded our Weather-glass to be stanch.

Thus much I find together in one place among my promiscuos collections: but after this coming to have the conveniency of Glasses so shap'd as to be easily seal'd, I judg'd it fit to make use of some of them to keep ev'n the most suspicious from objecting, that I should also have made some Trials with Glasses, which being Hermetically seal'd, would be sure most accurately to hinder all immediate Intercourse betwixt the internal and external Air. And I remember, that once we took a Glass, like the Bolthead of a common Weather-glass, save that the small End was drawn very slender, for the more easie breaking of the Apex: And into this Glass a convenient Quantity of water was powr'd, and then the Glass being seal'd up at the sharp end and inverted, the water fell down to that end, and possesst its due space in the Pipe: Then the round end of the Glass, having a mixture of snow and salt appli'd, about

about it, though the internal air must needs have been thereby much refrigerated (as will be readily granted, and may be gather'd from divers of the Experiments mention'd in these papers) yet we observ'd not the water manifestly to rise. And though an attentive Eye should in such a Trial discern some sensible intumescence in the water; yet that may well enough proceed from some little expansion of the Aerial particles, which we have elsewhere shewn to be usually latent in Common water, upon the diminution of the pressure of the Air above the water, caused by weakning that air's spring by the Cold. But when we had, to complete the Experiment, broken the slender end of the Glass under water, the included air, becoming then contiguous to water, that had obtain'd immediate Intercourse with that water, whose surface was every where prest by a pillar of the External air that leaned upon it, the water was by the gravity of that outward air hastily impell'd into the Cavity of the Pipe (the spring of whose air

H—y

was,

was, as we said, weakned by the Cold) to the height, if I misremember not, of several Inches.

*See more
concerning
these Wea-
ther-glas-
ses in the
first of
these three
Discourses.*

Another sort of Trials I remember we made after the following manner. We took Glass Bubbles (blown with a Lamp) some of about the bigness of a Nutmeg, and some much greater; each of these Bubbles we furnished with a very slender stem (often no bigger then a Ravens Quill) which was usually divers, and sometimes many Inches long. Into this stem a drop or two of water being convey'd, might easily enough, by reason of the Lightness of so little Liquor, together with the slenderness of the Cavity (which permitted not the included air to penetrate the water at the sides, but rather impel up the intire Body of it) be kept suspended, and so betray very small changes, (and much smaller then to be taken notice of by common Weather-glasses) as to rarefaction and condensation in the air it lean'd upon. Now when in one of these Instruments, if watching when the pendulous water was somewhat near the
Top

Top of the stem, we nimbly applied to the Orifice of that stem the flame of a Candle, we could by that Heat almost in a moment seal it up, by reason of the thinness of the Glass, and the slenderness of the stem. And if then we plac'd the thus seal'd Glass in a mixture of snow and salt, how much soever the air within the cavity of the Ball must be, in all probability, refrigerated by this operation, yet it would scarce sensibly, and not at all considerably shrink, as we gather'd from the pendulous waters remaining in the same place, or its falling at most but inconsiderably lower. But if then, with a pair of Scissars or otherwise, we dexterously broke off the seal'd end of the stem, and thereby expos'd the internal refrigerated, to the pressure of the external air, the water immediately would be hastily thrust down, sometimes divers Inches below its former station, and sometimes quite into the cavity of the round end of the Glass. To which we shall add, that not only, when these Thermometers were seal'd, neither the usual degrees

of Cold, nor those of the Heat in the Ambient Air would at all considerably depress or raise the pendulous water, which if the Glass were not seal'd, would, as we formerly noted, shew it self wonderfully sensible of the mutations of the Air as to those two Qualities: But we sometimes purposely tri'd, that though upon the refrigeration of the formerly rarified air in the Glass, the pendulous water were descending fast enough, yet if ev'n then we nimbly seal'd up the open Orifice of the stem (which may easily be done in a trice) the descent of the water would be presently stopt, and it would stay either just in, or very near the same part of the shank, wherein it chanc'd to be, when by sealing of the Glass it came to be fenced from the pressure of the Atmosphere, and in that place it would continue till the seal'd end were broken off. For then in case the ambient air were as cool as it was, when the Glass was seal'd, the water would for the reason already given be further depressed, according as the weakened spring of the inward rarifi'd air

was

was more or less remote from an equality to the pressure of the ambient air.

Besides, for further Trial, we took a large Glass-egg with a long stem, which stem was purposely bent, that it represented a glass-Syphon, in whose shorter leg the glass was drawn very small, that it might be the more easily first seal'd, and then broken.

This done, we got in a convenient Quantity of water, which ascended to a pretty height in both the legs of the bent glass, after which the shorter leg being nimbly seal'd, after the manner hereafter to be mention'd, there remained a pretty Quantity of air above the water in that shorter leg, which was purposely left there, that it might by its spring impel up the water in the longer leg upon the refrigeration of the Air included in that longer leg. All this being done, the whole glass was so plac'd in a convenient frame, that the oval part of it was supported by the frame, beneath which the bended shank of the Weather-glass did hang so, that a

*See the figure a-
mong the
rest of the
Schemes.*

mixture of Ice and Salt might be conveniently laid upon this frame to surround and refrigerate the air included in the Egg, without much cooling the air in the Cylindrical part of the Glass. The account that I find of this Trial in one of my notes, is this,

In the greater bent Egg, that was seal'd up with water, in both legs, upon the application of Ice and Salt to the Ellipsis at a convenient time, the water in the longer leg ascended a little, but not by our guess above a barley Corns length, if near so much, and about four Inches of air (as I remember) that were left in the shorter leg, expanded it self (to sense) as much; but, as soon as I broke off the slender wire, wherein the shorter leg ended, the external air rushing in, made the water rise about two inches and a quarter in the longer leg, and then, there not being water enough, broke through it in many bubbles.

Thus far the note, to which I shall only add, that in this case the ascension of the water in the longer leg cannot

not be attributed to the weight of the air in the shorter leg, that being, I know not how much, too small to lift up so much water, but to the spring of that air. And also that we need not marvel, the Expansion of that air should be so small, since some of the Experiments, hereafter to be related, will shew us, that the refrigeration of the air in such Trials (as that newly recited) does not weaken the spring of it any thing near so considerably as one would expect. So that the air in the longer leg could yield but a very little to that in the shorter leg, especially since the smallness of this last nam'd portion of air made its spring to be more easily and considerably weakned by a small Expansion.

Thus far our Paradoxical Discourse, which contains divers particulars, that, being added to the considerations, whereunto we have (by way of Appendix) subjoyned It, might afford us several Reflections: But having dwelt too long on one subject already, we shall now conclude with This, upon the whole matter;

H 4

That

That there is somewhat or other
in the Businels of Weather-glasles,
which (I fear) we do not yet suffici-
ently *understand*, and which yet, I
hope, that by other Trials and
more heedful Observations we *shall*
discover.

The



The Paper that was prefixt
(by way of a short Prefatory
Address) to the ensuing
History of Cold, when be-
ing to be brought in, and
presented to the *Royal So-*
ciety, it was put into the
hands of (its most worthy
President) the Lord Vis-
count *Brounker*, was as fol-
loweth.

Little-Chelsey, Feb. 14. 1662. S.A.

My Lord,

THe time Your Lordship and the
Society appoint me for the
bringing in of my Papers, con-
cerning Cold, is so very short, that to
give You the fruits of my Obedience as
early as You are pleased to require them,

I must present them You very immature,
 and I should say very unfit for your Per-
 usal, if you were not as well qualified to
 supply Deficiencies and Imperfections as
 to discern them. For of all the Old Obser-
 vations, I made divers years ago in order
 to the History of Cold, I have not yet
 found enough to fill up one Sheet of Paper:
 And as for those, I made the last Frosty sea-
 son, besides that I was several times di-
 verted by Avocations distracting enough,
 the same sharpness of the weather, which
 gave me the Opportunity of making some
 Experiments, brought me an Indisposition,
 which by forbidding me to be often, and
 stay long in the cold Air, hindered me
 from making divers others; and (which
 is worst of all) whilest I was confin'd to a
 place where I wanted divers Glasses, and
 other Instruments I would have employ'd,
 the ways both by land and water, were so
 obstructed by the snow and ice, that I could
 not seasonably procure them from Lon-
 don, and was thereby reduc'd to leave
 several trials, I should have made, ei-
 ther unattempted, or unprosecuted. But
 lest You should think, that, what I in-
 tend only to excuse my unaccuracy, is
 meant to excuse my Pains, I shall without
 further

further Apology apply my self to do what the shortness of the time will allow me, which is little more then to transcribe into this Historical Collection, most of the Particulars, which Your Lordships Commands exact, though haste will make me do it in the very words, for the most part, that I find them, in a kind of Note-book, wherein I had thrown them for my own private use, which I the less scruple now to do, not only because the haste, that exacts from me this way of writing, may serve to excuse it in me, but that it may the better appear, how little I had design'd to wrest or byass them to any preconceiv'd Hypothesis.

THE



THE
EXPERIMENTAL HISTORY
OF
COLD
Begun.

Title I.

*Experiments touching Bodies
capable of Freezing others.*

TO go Methodically to work,
we should perhaps begin
with considering, what *sub-*
jects are capable, or not capable of
harbouring the Quality we are to
treat of; And to invite us to this, it
seems probable enough, that among
the Bodies, we are conversant with
here below, there is scarce any ex-
cept

cept Fire, that is not, at some time or other, susceptible of actual Cold, (at least as to sense :) And ev'n concerning Fire it self, till that difficulty be clearly determin'd, which we have elsewhere started ; namely, whether Fire be not, as Wind (at least like such as is made by Air blown out of a pair of Bellows) rather a *state of Matter*, or Matter consider'd whilst it is in such a kind of Motion, then a *distinct and particular species of natural Bodies* ? there may remain some Doubt, since we see, that Bodies, which may be either *in a Moment*, as Gunpowder, or (as far as sense can judge) *totally*, as high rectifi'd spirit of Wine, turn'd into fire, may yet *immediately before* their Accension, be actually Cold : And as to Gunpowder, *presently after* Accension, its scatter'd Parts caught in clos'd Vessels, will also appear cold to the Touch. But such things nevertheless we must not now insist on, *partly* because it requires the resolving of a somewhat difficult Question, which more properly belongs to the Considerations about Heat, where we have already

already handled it; *partly* because our Design in the following Collections, was not so much to gather and set down *Observations*, that were obvious to any that was furnish'd with a Mediocrity of Attention, as *Experiments* purposely made in order to the History of Cold; and *partly* too, because in this Collection, though we do, as occasion serves, take notice of some Experiments and *Phænomena*, that relate to Cold in General, or indefinitely; yet our chief work has been to find out, and deliver, the *Phænomena* of Congelation, or of that intense Degree of Cold, which either *does* freez the Bodies it works upon, or at least *were capable* of turning common water fitly expos'd to it, into Ice. And this may serve for a general Advertisement about the ensuing Papers; and consequently having premis'd it, we shall without any further Preamble proceed to the setting down such things, as we have tri'd and observ'd concerning those Matters: beginning with those that belong to the Title prefix'd to the first Part, or Section, of our History.

1. The

(III)

1. The Bodies that are cold enough to freez others, are in this climate of ours but very few, and among the most remarkable, is a Mixture of Snow and Salt, which though little known, and less us'd here in *England*, is in *Italy* and some other Regions much employ'd, especially to cool Drinks and Fruits, which men may easily do, by burying, in this mixture, Glasses, or other convenient vessels, fill'd either solely with Wine, or other Drinks, or else with water, that hath immersed in it the fruits to be refrigerated.

2. The Circumstances we are wont to observe in making and employing this mixture, we shall hereafter in due place deliver, and therefore here we shall only take notice, that we could not find upon some trials, that such Glasses filled with water, as would be frozen easily enough by this mixture of Snow and Salt, would be in like manner frozen, in case we employ'd Snow alone, without mingling any Salt with it. I deny not, that 'tis very possible, that in very cold Countries, as well Snow
as

as beaten Ice may freez water powdered into the Intervals of its Parts. But there is great odds betwixt water so intermingled with Ice or Snow, and only surrounded with it in a vessel where the water is, as it were, in one entire Body, and of a comparatively considerable thickness: And there is also a great Difference betwixt the degrees of coldness in the Air of Frigid Regions, and of *England*. And perhaps too there may be some Disparity betwixt the Degrees of Coldness of Ice and Snow in those Climates, and in ours. And we must have a care, that in case a Vial full of water buri'd all night should freez, we ascribe not the Effect to the bare Operation of the Snow, which may be (entirely, or in great Part) due to the coldness of the Air, which would perhaps have perform'd the Effect without the Snow.

3. But though Snow and Salt mixt together will freez water *better* than *Snow alone*, yet we must not think, that there is any such peculiar vertue in Sea-salt, to enable Snow to freez, but that there are divers other Salts,

Salts, each of which concurring with Snow, is capable of producing the like Effect. For we found upon trial, that we could freez water without the help of Sea salt, by substituting in its place, either *Nitre*, or *Alume*, or *Kitriol*, or *Sal Armonick*, or even *Sugar*; for either of those being mingled with a due proportion of Snow, would serve the turn, though they did not seem equally to advance the congealing power of the Snow; nor scarce any of them did do it so well as Sea salt. But of this elsewhere more.

4. When we had made the newly mentioned trials, some particular conjectures we have long had, about the nature of Salts, invited us to try, whether, notwithstanding the comminution and consequent change produced in Salts by Distillation, the Saline Corpulcles, that abound in the distill'd liquors of those concretes, as well as in their solutions, would not likewise, by being mixt with it, enable Snow to freez water, at least in small and slender Glasses? This we first went about to try with good
I
spirit

spirit of Salt, but we found, as we fear'd, that though it made a sufficiently quick dissolution of the Snow it wrought upon, yet its fluidity hindered it from being retain'd long enough by the Snow, to the bottom of which it would fall, before they had stay'd so long together, as was requisite to freeze so much as a little *Essence*-bottle full of common water.

5. Wherefore we bethought our selves of an expedient, whereby to try the operation, not only of those spirits, but of divers other bodies, which were unapt for a Due commixture of Snow after the way newly mention'd, or of which we had too little, or valued them too much, to be willing to spend quantities of them upon these trials. And this way (that remains to be mention'd) we somewhat the better lik'd, because the Experiments made according to it would also prove Experiments of the transmission of Cold through the extremely close body of Glass.

And even in this way of trying, we did at first meet with a discouragement, which least it should happen

pen to others, we shall here take notice of, namely, that having put a convenient quantity of Snow into a somewhat thick green glass Vial, though we copiously enough mixt with it a somewhat weak spirit of salt, (being loath to imploy the best we had) and having well stop't the vessel, did carefully shake together, and thereby agitate the mixture in it, yet the Glais appeared only bedew'd upon the outside, without having there any thing frozen. But suspecting, that the thickness of the Glass might be that, which hindred the operation of the included mixture, we put snow and a convenient proportion of the self same spirit of salt into a couple of thin Vials, one of which we clos'd exactly, and the other negligently, and having long shaken them, we found that what adhered to them on the outside, was (though but somewhat faintly and thinly) frozen.

6. And, as to this sort of Experiments we shall here observe once for all, that the Snow or Ice included, together with the Saline Ingredient

(whatever that were) was always thaw'd within the Glass, and that consequently, 'twas the condens'd vapor of the Air, or other liquor that adhered to the outside of the glass, which was turn'd into Ice, which is the Reason, why in mentioning these Experiments we often use the word *freez* in a transitive sense, to signify the operation of the frigorifick mixture upon other bodies.

7. This premised, let us proceed to relate, that we afterwards took Oyl of *Vitriol*, and mixing it with Snow in such an other vial as that last mentioned, we found its freezing power far greater then that of spirit of salt. And least it should be pretended, that in these Experiments, the cold was not transmitted through the sides of the glass, but that the Air within the vial, highly refrigerated by the mixture, Did upon the account of their free intercourse enable the Air contiguous to the outside of the vial to freez the Dew it met with sticking on it; we prosecuted the Experiments with the addition of this circumstance, that on several occasions

ons we seal'd up the vial, that contained the snow and the other frigorick body it was mixt with, and afterwards by the help of this mixture froze the externally adhering moisture.

8. Having then according to this way substituted spirit of Nitre for oyl of Vitriol, or spirit of Salt, we found, that it froze yet more powerfully then either of those two liquors, and continued to do so in those parts of the outsides of the glafs, that were adjacent to the included snow, till that snow was almost totally resolv'd into a liquor. This we tri'd both in a thin seal'd glafs, and in a pretty thick glafs stopp'd only with a Cork.

9. Afterwards we succesfully enough tri'd the Experiment with spirits less acid, as not only with spirit of Vinegre, but with spirit of Sugar, I mean the Red Emphyreumatical spirit forc'd over in a Retort, which mixt with snow, according to the manner of the Experiment, did at length freez the externally adhering moisture. But the filmes of ice were very thin, and very apt quickly to disappear.

10. Having thus made a number of trials with acid spirits, we thought fit to make some with Urinous spirits that abound in volatile salt, and accordingly having mixt spirit of Urine and Snow in an open vial, and agitated them, we found that the external moisture did discernably, though not very strongly, freez.

But with spirit of *Sal Armoniack* drawn from Quick Lime (according to the way I have delivered in another Treatise) the operation was quick and powerful enough.

11. Having tri'd to freez water with acid, and with volatile spirits apart, we thought it not amiss to try what they would do both together, and accordingly pouring upon snow both some spirit of Urine, and a little oyl of Vitriol, and shaking them into the snow in an open Vial, we found that the mixture did freez, though the glaciation, in this case produced, were very languid.

12. Having thus tri'd salts disingag'd from their grosser parts, or shattered into Corpuscles by distillation, we made some trial likewise with

with groſſer ſalts, as with *Sal Gem*, with a ſublimæ made with common Sublimate and *Sal Armoniack*, nay, and with both Loaf and Kitchin Sugar, with all which among the like bodies, that I can now Remember, the Experiment ſucceeded well enough: alſo a very ſtrong ſolution of Pot-aſhes, mixt with ſnow in a open ſingle Vial, did freez, but that very faintly. And both a very ſtrong ſolution of very pure ſalt of Tartar, and (at another time) a ſtrong ſolution of Pot-aſhes, being the one as well as the other, mixt and agitated with ſnow in a ſingle vial, produced filmes of ice (though thin ones) on the outſide of the glaſs.

13. After this, we thought fit to make a trial of another kind, of which I find this account among my Notes. We filled a ſingle vial with ſnow, and then powred into it a convenient proportion of a ſtrongly ſweet ſolution of *minium* in ſpirit of Vinegre, and having ſhak'd the mixture together, we found, that this ſweet Sugar of Lead, did as well as acid and alcalizate ſalts, excite the

cold of the snow so much, as to produce filmes of ice on the outside of the glass: but a parcel of the same solution, being for divers hours kept in snow and salt, was not thereby frozen.

In order to the discovery of some hints of the account, upon which the above mentioned mixtures were more intensely frigefactive then snow alone, we sealed up a single vial full of snow unmingled with any other ingredient, and found it to thaw much more slowly then any of those parcels of snow which we had mixt with salts or spirits.

In prosecution of this conjecture, we shall add, that for ought we could find by divers trials, no salt, that helps not the snow to dissolve faster then else it would, did inable it to produce ice, though usually it did produce dew on the outside of the vial, that contained the mixture; and accordingly, neither Chrystals of Tartar, nor Borax, both beaten to powder, nor, which is more (considering what we lately noted of the effects of another sort of Sublimate) would

would Sublimate inable the snow to freez; as well the powder of Sublimate, as that of Borax, and that of Tartar, lying for a great while in the snow undissolv'd.

14. Belonging to this matter, I find among my papers also this Note.

[Water of Quick Lime (made, by quenching store of unslak'd Lime in common water) twice tri'd would not make snow freez, perhaps because though the water were kept stopt, yet the liquor having been kept in the glass a twelve-moneth, and more; probably the spirits may have flown away, which I find by inquiring of one that Drinks much Lime-water, that it abounds with, when fresh, and grows destitute of a while after; and possibly also the badness of the Lime was the cause, why being mingled with snow it would not freez, though all the vials, that did not freez, did yet gather store of dew on the outsides (perhaps because of the snow, whose melting alone may suffice to produce that effect.)]

15. It may seem somewhat more
strange,

strange, that distilled oyl of Turpentine, which is so hot and fiery a liquor, should not enable snow to freez, but this agrees not ill with the conjecture lately mentioned, for it will hereafter appear, that in oyl of Turpentine Ice dissolves flower then in Divers other liquors, without excepting common water it self.

16. And yet notwithstanding the bad success of this trial, we were not Discouraged from making another with spirit of Wine; for, though according to the common opinion of Chymists and Physicians, it be a mere vegetable Sulphur, yet we, that have elsewhere ventured to ascribe some such operations to it as Chymists would have belong to Saline Liquors, did not scruple to seal up in a single vial almost filled with snow, a convenient quantity of pure spirit of Wine, (drawn off from quick Lime the better to dephlegm it) and of this mixture we found the operation more powerful then any of those we have formerly mentioned: for the freezing vertue of this did not only last long, both in the seal'd single vial, and

and in another that was open, but the inclosed mixture presently cruſted the outside of the glaſs (or of the neck, if it were made to fill that) with ice, which might be taken off in flakes of good breadth, or in pieces of good thickness. Nay, it preſently froze Urine into Figured ice, which might be taken off in ſcales.

17. This laſt circumſtance puts me in mind of another Experiment, whereby we tried by a vigorous mixture of Snow, and ſome choice ſpirit of Nitre, we had met with to freeze liquors of more difficult congelation than fair water.

We took then ſome ſnow, and mingled with it ſome of the newly mentioned ſpirit of Nitre in ſo luckily a proportion, that it froze very vigorously and very ſuddenly, inſomuch that once almoſt as ſoon as it was ſet to the ground, it froze the vial to the floor it was ſet on, and the outside of the glaſs, that contained this mixture, we wetted with ſpirit of Vinegre, which was frozen into pretty thick ice. But yet (not quite to forget that circumſtance) retaining the
salt

salt taste of spirit of Vinegre, and
 though this mixture would not dis-
 cernably freez spirit of Nitre on the
 outside, yet it transmitted cold
 enough to freez weak spirit of Salt,
 and to give Us the pleasure of seeing
 some Saline liquors presently turned
 into figur'd Ice, as not only the last
 mentioned spirit exhibited some little
 (as it were) Saline Iceikles crossing
 each other, and quickly vanishing,
 but (which was far prettier) having
 often observed, that *Sal Armoniack*
 being dissolved in water, and the so-
 lution being put very slowly to eva-
 porate in part, but not too much,
 away, the remaining liquor would in
 the cold shoot into parcels of salt very
 prettily figur'd, some of them re-
 sembling combs with teeth on both
 sides, and others resembling feathers;
 having observ'd this, I say, and be-
 ing desirous to try, whether the spi-
 rit of *Sal Armoniack*, distilled by the
 help of quick Lime, being put to
 congeal on the outside of a glafs,
 would not afford a Resemblingly fi-
 gured Ice; we found upon trial,
 both that the mixture was able to
 freez

freez that subtile spirit, and also, that it shot into Branches almost like those, exhibited by such salts undistilled. And it was not unpleasant to behold, how upon the inclining the glass so, that the freezing mixture rested a little, near any part of the spirit, this liquor would shoot into such branches as we have been speaking of, so nimbly, that the eye could plainly discern them, as it were, to grow, and hastily overspread the surface of the glass, but those Branches were wont quickly to vanish.

I had almost forgot to mention, that I tried the freezing with snow, and divers fermented Liquors undistilled instead of spirit of Wine, and though the Experiments succeeded not with small Beer, much less with water, yet there was a glaciation, though but slight, produc'd not only by the addition of Wine, but even by that of moderately strong Ale.

18. Having observed, that the Liquors and other bodies, that assisted the snow to freez, were generally such as hastned its dissolution, we thought

thought it not altogether unworthy the trial, to examine, what would be the Event of procuring a speedy dissolution of the snow, by substituting bodies actually warm, instead of potential hot ones: Of this sort of trials, I find among my Notes these two registred.

[1. Into a single vial almost filled with snow, there was poured a pretty quantity of well heated sand, that it might dissolve the snow in many places at once, without heating the ambient Air, or the outside of the glass; but though the solution of the snow seemed to succeed well enough upon the shaking of the vessel, yet the outside of the glass was only bedewed, not frozen.

2. Into another single vial almost filled with snow, we poured some water, which we judg'd of a convenient warmth, and we poured it in by a funnel, that had but a slender orifice beneath, that the warm water might fall into the middle of the snow, without Running to the sides, and taking a convenient time to shake the glass, we did by this way produce

duce a very considerable degree of cold, and much dew on the outside, but were not satisfied, that any of that dew was frozen, though the success would have invited us to have made further trials in greater glasses, if we had had any more snow at hand.]

Wherefore This Experiment is to be further and more artificially tri'd.

19. It is a common tradition, not only among the vulgar, but (I presume, upon their account) among learned men, that the oftentimes variously, and sometimes prettily enough figur'd hoar frost, which is wont to appear upon glass windows in mornings, preceded by frosty nights, are exsudations, as it were, that penetrating the glass-windows, are, upon their coming forth to the cold external Air, frozen thereby into variously figured ice: How groundless this conceipt is, may be easily discovered, if men had not so lazy a curiosity, as not to try (which they may do in a moment, and without trouble) whether the Ice be, according

according to the tradition on the outside of the window, and not contrary to it on the In-side, where indeed it is generated of the aqueous Corpuscles, that swimming up and down in the Air within the Room, are by the various motion that belongs to the parts of fluid bodies as such, brought to pass along the window, and there by the vehement cold of the neighbouring external Air, communicated through the glass, condens'd into dew, and frozen into Ice.

20. And because divers modern Naturalists have taught (I think erroneously) that glass is easily enough pervious, not only to Air, but to divers subtile liquors, lest the favourers of this Doctrine should object, that we have ill assigned the natural cause of the ice, appearing on the outside of the glass in the former Experiments, which according to them may rather proceed from the subtler (but yet visible) parts of the excessively cold mixture of the snow and saline bodies penetrating the pores of the glass, and settling on the outside of it: To obviate this objection, I say, and to confirm

confirm what we have taught in another Treatise about the wandring of store of aqueous vapours through the Air, we will add the following Experiments, purposely made to evince these truths.

21. At one time four ounces and a quarter, of a mixture of Ice and Salt, being inclosed in a vial, and thereby enabled to condense the vapours of the ambient Air, was by their accession increas'd 12. grains.

Another time a vial, wherein snow (weighing two ounces six drachms and an half) was suffered to condense the vapid Air, the dew, that partly adher'd to it, and partly fell from it, made the whole weigh four grains more then the vial did, when it was first put into the scale, in which scale we found some water flowing from the dew, which gave that increase of weight. And here let me add by the way, that the tip of This seal'd vial, being broken under water, suck'd in a considerable quantity of it, whether, because of some little rarefaction of the Air included in the sealing, or because of the in-

K frigidation

frigidation of that Air by the snow, or for both these Reasons, or any other, I shall not Now dispute.

22. But other Experiments to the same purpose we made, wherein the increase of weight was more considerable; and that the way, we used, may be the better understood, and the conclusion built upon it the more undiscuss'd, we will add a couple of trials, that we find among our notes concerning this matter.

[In a single vial we seal'd up as much snow and salt, as afterwards, when melted, we found to weigh between five and six ounces, after a while the salt beginning to melt the snow, the Dew on the outside began to congeal, and being rubb'd off, the hoar frost would quickly begin to come again. This vial for further trial being put into a pair of scales with a counterpoise, after a while, as the vapours, that wandred through the Air in the warm room, hapned to be detain'd more and more upon the outside of the glass, and to be there frozen, the scale, wherein the glass was, began to be deprest, and
to

to shrink lower and lower, after which, by adding a little to the counterpoise, we reduced them again to an *Equilibrium*: And yet after a while, the scale, that held the vial, subsided again more and more, till the Included snow was melted; so that to reduce the scales to their first *Equilibrium*, we were fain to add in all to the Counterpoise a weight, which we Estimated to be about eight or ten grains, (for we had then no great weights by us.) The vial being taken out, there appeared near half a small spoonful of Liquor in the scale it stood in, which proceeded from the thaw of the Ice, that was generated about it. But in that part of the scale, which was covered with the convex part of the bottom of the glass, there appeared no wet.

A like or smaller quantity of snow and spirit of Wine being seal'd up in a single vial, the outside quickly appeared cas'd with ice as high as the mixture reacht within, and this vial also being counterpois'd in a pair of scales, did by degrees depress the scale that held it, till it had sunk it

very low, and about seven grains did but reduce the scales to an *Equilibrium*, but the scales being somewhat rusty, we could not make the Trials with that Exactness we desired.]

23. But at other times, when the Experiment was more luckily, though not more carefully tri'd, with better scales, the increase of weight from the condens'd vapours of the Air, was somewhat more considerable; for I find in a short note,

[That at one time a mixture of spirit of Wine and Snow, weighing three ounces and three quarters, afforded of condens'd vapours about 18. grains.]

And at another time a mixture of Snow and *Sal Gem*, weighing three ounces and seventy grains, procured us an accession of water weighing about 20. grains.]



Title II.

Experiments and Observations touching Bodies Disposed to be Frozen.

1. **T**Were almost endless to try particularly, which bodies *are*, or *are not* capable of congelation, and the degree of cold would also in such Experiments be (as near as men can) determin'd; because many bodies will freez in one degree of cold, that will not in another; wherefore we are willing to leave these trials to those, that have more leisure and opportunity to prosecute them, and shall only set down *some*, and those, somewhat various, that we may not leave this part of the History of Cold quite unfurnish'd. And we must mention the fewer, because, being in the Countrey, we were not provided of divers of the bodies

which we should have expos'd.

2. In very cold snowy weather, we tri'd, that (besides common water) Urine, Beer, Ale, Milk, Vinegre, and French and Rhenish Wine (though these two Last but slowly) were turned into ice, either totally, or in part. But such instances will possibly be thought too obvious to be insisted on; therefore I shall add, That not only we froze a strong solution of *Gum Arabick*, and another of white Sugar in common water, but that We took Alum, Vitriol, Salt-Petre, and Sea salt, and made of each of them in a single vial as strong a solution as we could, we also made a strong solution of Verdegrease in fair water (which was thereby deeply coloured) all these we exposed to the cold Air. The solution of Alum, Nitre and Verdegrease froze without affording any notable *Phænomena*, either in the figuration of the Ice, or otherwise: Of the solution of Vitriol there remain'd at the bottom of the glals, a pretty quantity unfrozen, and of a clear substance, whose colour was very high of the Vitriol, whereas

whereas the upper part of the same solution differed very little in colour from common Ice.

3. But because it seems not so strange, that these gross sorts of Saline bodies should be turned into Ice, we thought fit to try, whether or no also divers salts, freed from the grosser parts of their concretes by the fire, were not likewise capable of congelation. We exposed therefore spirit of Vinegre in one small glass, and spirit of Urine in another, to an intense cold, and found, that not only the former, but the latter also froze.

4. We took likewise some of the fiery lixivate salt of Pot-ashes, and a single vial, in which we put, to two ounces of water, a drachm of the Alcaly, and exposing it to a very sharp Air, we did, when we came to see the success of the trial, find Ice lying on the top in little sticks (something crossing one another) almost like the Crystals of rock'd Petre, and besides these that lay levell'd, there were others that shot downwards in very great numbers.

5. We also found that Oyl of Tar-

tar *per deliquium*, or at least a strong solution of the fixt salt of Tartar, though it seemed much to resist the cold, yet it was once by snow and salt brought to Congelation,

Appendix to the II. Title.

Since I wrote the present Book concerning Cold (excepting some of the Appendices) having once had the Opportunity of an Hours Discourse with an Ingenious Man, that not only liv'd some years in *Muscovy*, but was, and is still Physician to the great Monarch of that Empire, and having likewise at other times conversed with Navigators, and some other credible persons, that had travelled either to *Greenland*, *Terra Nova*, or other gelid Climates, I propos'd them divers Questions, by their Answers to which, I learned some particulars, which together with others, that I have met with in Voyages and other Books, I think it not amiss

amiss to annex by way of Appendices to the foregoing, and some of the following Sections, or Titles.

About the freezing of common express'd Oyls, I know not well what to determine ; For that they may by a very intense Cold be depriv'd of their Fluidity, and be made capable of being cut into portions, that will retain the figure given them, my own Trials invite me to believe ; but whether such oyls will be turned into *true* (by which I mean) hard and brittle Ice, is a Question scarce to be determin'd by any Experiments we can make here in *England*, where we could not reduce oyl Olive into Ice : And for the Relations of those that have liv'd in colder Countries, I find them to disagree : For when I asked the lately mention'd Doctor the Question, how far he had known oyl Congeal'd in *Muscovy* ? He answered me, That it did there freez much harder then in our Climate, but would not, that *he* had observed, be turn'd into true & perfect Ice. On the other hand I find the Testimony of that Ingenious Navigator Captain
T. James,

T. James, who relating the effects of cold he met with in the Island where he and his men were forc'd to winter, does in one place reckon Oyl among the Liquors, such as Vinegre, and Sack, that ev'n in their house was firmly frozen, and more expressely elsewhere. *All our Sack* (says he) *Vinegre, Oyl and every thing else that was liquid, was now frozen as hard as a piece of wood, and we must cut it with a Hatchet.* And *Olaus Magnus* speaking of the fights, wont to be made upon the Ice in the Nothern Regions, *Glacialis Congressus* (says he) *sit in Laneis Calcibus, non pellibus, aut Coriis unctis: Vis enim frigoris, quodcunque sit unctuosum convertit in Lubricitatem glaciale.*

Olai Magni Gent. Sept. Hist. Lib. 11. Cap. 24.

There being a great Similitude in point of Inflammability, and disposition to mix with many subtle Oleous Bodies, betwixt spirit of Wine and Oyl, and as great an affinity in divers other regards, betwixt that spirit and both aqueous and saline Liquors, with which it will readily mix; I had a great Curiosity to know, what kind of change would be produc'd in vinous spirits, in case they were exposed

fed to a cold great enough to work a visible change in their Texture; I therefore solicitously inquir'd of the *Russian* Emperors lately mention'd Physician, whether or no he had observ'd in *Muscovy* any manifest change produc'd by cold in Hot Waters, and Spirit of Wine? To which he returned me this answer; That common Aniseed-water, and the like weak spirits would be turn'd into an imperfect kind of Ice, and that ev'n the very strong spirits, though they would not be turn'd into Ice, would be turn'd into a kind of substance like Oyl.

Title



Title III.

Experiments touching Bodies Indispos'd to be Frozen.

1. **W**E found many liquors, whose subtle parts being by Distillation brought over, and united into very spirituous liquors, and so either totally, or in great measure freed from those phlegmatick or aqueous parts, that dispose Bodies to congelation, could not be brought to freeze, either by the cold of the external Air, to which in frosty nights we exposed them, or by such an Application of snow and salt, as served to freez other Bodies.

2. Of this sort were, among acid *menstruum's*, *Aqua fortis*, spirit of Nitre, of Salt; also oyl of Turpentine, and almost all, (I add the word *almost*, because the Essential oyl of Aniseeds, and the Empireumatical oyl

oyl of common oyl will lose their fluidity in a less degree of Cold, then that of our mildest frosts,) I say *almost all* the Chymical oyls we had by us, as likewise spirit of Wine, and other strong spirits of fermented Liquors, and even Sack it self, if it were good, would very hardly be brought to afford us any Ice at all : But among the many liquors, that would not freez, there were a few, whose trials afforded us some circumstances not altogether unworthy their being mention'd.

As 1. I being desirous to satisfie some friends, that 'twas the brisk spirit of the Grapes, whether resulting from, or extricated and exalted by fermentation, that kept (all) the rest of the Sack from freezing : I took a parcel of that liquor, that would afford us no Ice at all, and by the help of a lighted candle, or some other actually flaming body, kindled it, and letting the inflammable part burn away, the remaining part of the Liquor (which was by vast odds the greatest part) was easily brought to freez.

Next

Next when the formerly mention-
ed trial was made with water and
Pot-ashes, we likewise, in another
glals, exposed a solution, wherein
the proportion of salt of Pot-ashes,
in reference to the water was four
times greater, there being in this $3ij$
of the salt to $3i$ only of water, and
this solution, though the glals were
covered with hoar frost and Ice on
the outside, froze not at all within.
And likewise, when another time we
made a very strong solution of salt of
Tartar, that was very pure and fiery,
it did not freez, though a considera-
bly strong solution of salt of Pot-ashes,
that was exposed with it, did. So
that these Experiments about the gla-
ciation of Lixivate Liquors must be
repeated to be reduc'd to a certain-
ty.

3. That the common express'd
oyls of Vegetables will, after their
manner, freez, that is, lose their
fluidity, and become, as it were,
curdl'd in very cold weather, is a mat-
ter of common observation; but I
had a mind to try, whether or no
Train oyl, that is made of the fat of
Animals,

Animals, (commonly that of
 Whales) though not by distillation,
 properly so called, yet by the help
 of fire, would not be more capable
 of resisting the violence of the cold,
 and accordingly I found, that Train
 oyl, exposed to the Air in a conve-
 nient vial, continued fluid; notwith-
 standing a more then ordinary sharp-
 ness of weather, and this I tried two
 or three several times, but at length
 one night proved so very cold, that
 the next morning I found the oyl un-
 fluid; which differing Events seem
 a little to Countenance, but more to
 disavour the Report of *Olaus Mag-*
nus, who writes, That whereas in
 Northern Regions 'tis usual for strong
 places to lose in winter the protecti-
 on afforded them in Summer, by
 their Ditches, though never so wide
 and deep, because the frost makes
 them easily passable to the Enemy:
 This inconveniency is wont to be pre-
 vented by pouring into the Ditches,
 the Ice, if there be need, being first
 broken, great store of this Train
 oyl, which swimming upon the sur-
 face of the water, and being incon-
 gealable

Olaus Magnus in Historia Gentium Septentrionalium, lib. 11. cap. 20. &

21.

gealable by the cold, protects the subja-cent water from the freezing violence of the cold, and keeps the moats unpassable. But because our Author mentions this as a known and vulgar Practice in those Icy Regions, it may perhaps deserve a little Enquiry, whether the Whale Oyl, used by the *Swedes*, *Laplanders*, *Muscovites*, and other Inhabitants of those parts be not differing, either as to the Fishes, 'tis made of, or as to the way of making it, or as to the way of keeping it from such Train Oyl as we Employed, unless perhaps it do already appear by the Relation of writers belonging to those Countries, or of Travellers, that have been in them, that *Olaus Magnus* has in that particular, as I fear, he has in some others, misinformed his Readers.

4. We took notice, that a strong solution of *common Sugar* was easily enough turned into Ice; but on a strong solution of *Sugar of Lead* we could not with salt and snow work the like change, and this, though the trial were not negligently made; which I therefore think not unworthy

thy to be mention'd, because that the two only Ingredients of this Sugar were *Lead*, which is esteemed a very cold Body, and *Spirit of Vinegre*, from which, as I noted above, we did by the like degree of cold to that we here employed, obtain Ice : And though in this *metalline Sugar* we may well suppose the Saline parts of the Spirit of Vinegre to be much more concentrated or united, then they were in the spirit; yet the *solution* must abound with aqueous parts : and this Sugar seeming but a kind of Vitriol of Lead, 'tis worth our Notice, that its solution would not freez, as well as that of common Vitriol, though in this latter concrete the metal be corroded by a spirit, which, as far as can be judged by the Liquors afforded in distillation, is very much sharper and stronger then spirit of Vinegre.

5. We likewise tried to freez *Quicksilver*, and for that purpose provided a bubble, that being blown with a Lamp, was but thin, and so flat, that the sides almost touched, and it held but a little *Mercury*, and

L

that

that by the figure of the Glass, being reduced to a large surface, with but very little depth or thickness, it was far more exposed, then if it had been in a ordinary round Bubble, to the action of the cold; but we could not at all freez this extravagant liquor, though we tried it more then once, and though the last time, we exposed it in the same vessel to the same degree of Cold, wherewith we made one of the following Experiments, that required a very Intense degree of that Quality. And in another thin glass-Bubble we long exposed Quick-silver to an extraordinary sharp air, but though the cold had some operation upon it, not here necessary to be mention'd; yet we could not find, that it did at all bring it to freez: wherefore I could wish that trial were made in *Muscovy*, *Greenland*, *Charles-Island*, or some other of the most Icy Regions, where the Effects of cold (which here are upon Quick-silver but languid) are the most considerable, and sometimes stupendious.

6. It is very remarkable, that
though

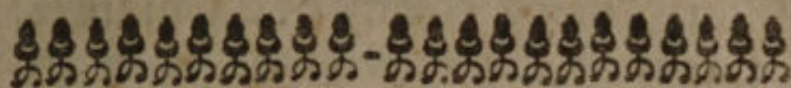
though not only the solutions of other gross salts, but, as we have seen, divers more saline and spirituous liquors, were brought by snow and salt to Congelation; yet a brine made very strong of Common salt, could not be brought to freez at all, though we kept it exposed with the other saline solutions, that *did* freez, during a whole night, that was exceeding sharp. Which Experiment I also tried many years since, to draw thence an Argument in favour of the *Cartesian Hypothesis* about cold, which I shall not now consider; but rather add, that being desirous to try, with what proportions of Sea salt and water, the congelation of them might be effected, I found, I could freez some Sea water, that had been brought up in a Barrel to that Monarch of the *Virtuosi*, the King, for the making of trials with it; and that having in a single vial exposed to the Air in a very bitter night, a solution consisting of twenty parts of water, and one of salt, which is double the proportion of salt to be commonly found in our Sea-water, the next day we found a

good part of the Liquor frozen, the Ice swimming at the top in figures almost like Broom, spreading from the surface of the water downwards. And to add, That upon the by, we suffered the Ice of salt-water to thaw, to try, whether it would yield fresh water, but it seemed not devoid of some Brackishness, which whether or no it proceeded from some parts of the contiguous brine, that adhered to the Ice, I leave to further and exacter observations, since I am credibly informed, that in *Amsterdam* there are divers, that use the thaw'd Ice of the Sea-water to brew their Beer with, instead of common fresh water.

Cap. 6.
Pag. 42.

3. And since I made that Experiment, I find in the industrious *Bartholinus's* newly publish'd Book, *De Nivis Usu*, a Confirmation of the probability of the Report I just now mention'd, his words being these, *De Glacie ex marinâ aquâ certum est, si resolvatur, salsum saporem deposuisse, quod etiam non ita pridem expertus est Cl. Jacobus Finckius Academiæ nostræ senior, & Physices Professor, benè meritis, in glaciei frustis è portu nostro allatis.*

Title



Title III.

Experiments and Observations touching the Degrees of Cold in several Bodies.

I. **A**FTER having treated of the Bodies that are the most capable of producing Cold, and of those that are most dispos'd, or indispos'd to receive it, it would be Methodical to take notice of the *Degrees of Cold*, to be met with in differing Bodies: But though a work of this nature might somewhat conduce to the Discovery of Cold in general, yet it is so laborious a Task, and, to be well perform'd, requires so much more of Leisure, and Conveniency, than I am Master of, that I must resign it to those that are better furnish'd with them; which I the freelier do, because the Experiments, which at this

Time make the principal part of our History, being chiefly of the highest Degrees of Cold, we may seem to have done something of what more properly concerns our present Design, by having made the Experiments, anon to be subjoyn'd within this present Section or Title. And yet thus much we elsewhere do towards the framing of a Table of the Degrees of Cold, that we do on other occasions set down those hitherto unpractis'd ways that we have employ'd, to estimate the greater or lesser Coldness of Bodies, by several kinds of Weather-glasses, differing from the common ones, and far more fit then they, for such a Purpose. For by Hermetically seal'd Thermoscopes furnish'd with high rectifi'd spirit of Wine, we can estimate the differing degrees of Coldness in Liquors, of which we shall presently mention an Example. And by using such Weather-glasses, as have their Air included not at the top, but at the bottom of the Instrument, we can within some reasonable Latitude, measure the Coldness both of intire
solid

solid Bodies, or minuter Bodies, as Salts, &c. by beating them *alike*, and very small, and placing the Instruments at equal Depths in the powder of each of them. And besides that the shape of these Thermoscopes does, as we have elsewhere shewn, make them proper for these uses, for which the vulgar ones, where the included Air is at the top of the Instrument, are not fit: besides this, I say, 'tis easie in these we make use of, to make the Pipe so slender in proportion to the Cavity of the Vial, whereinto 'tis inserted, that very much minuter Differences of Cold will be *manifest* in these, then are wont to be *sensible* in common Weather-glasses. And besides these two See the Preliminary Discourses. sorts we have elsewhere propos'd, and describ'd a third and new kind of Thermometer, wherein a drop of liquor being suspended in a very slender Pipe of Glass, betwixt the outward and the inward Air, makes it far more fit for those Experiments, wherein we either despair, or care not, to *measure* the Difference of Cold betwixt two Bodies, but are

only desirous to try, *whether* or no they differ in Coldness, and in case they do, which of them has most : For these Weather-glasses, are so exceeding sensible even of the minute Differences of Heat and Cold, as manifestly to discover Disparities, which other Thermoscopes are not nice enough to give us any Notice of. Only this Advertisement we must add about them, that when we use them to examine the Coldness, not of liquid, but of consistent Bodies, we alter a little the figure of the wide end of the Glass; and instead of making it a round bubble, as we have elsewhere describ'd, we make it with a flat or flattish bottom, that the whole Instrument might thereon, as on a Basis, stand of it self upright, and so, being still taken up by the open and slender end, for fear of rarifying the included Air, (which Caution is here given once for all) may be transferr'd with a pendulous drop in the Pipe, and plac'd sometimes on one, and sometimes on another of the solid Bodies to be examined by it. For if the Body, 'tis removed

moved to, be more or less cold then that it rested on before, that coldness communicated through the Glass to the Air, by which the pendulous drop is supported, that Airs Expansion or Contraction will manifestly appear by the rising or the falling of the drop. And thus we have taken pleasure to remove it from one kind of wood to another, from woods to metals, and from metals to stones, &c. But the Expedients, that may be propos'd to improve these little Instruments to the purposes we have been treating of, and the Cautions, that may be added to prevent mens drawing mistaking Inferences from the Informations they seem to give them, will take up more time, then we are willing to spend upon an occasion, that will not perhaps be thought to deserve it, nor much to require any others, then those we shall by and by subjoyn. And therefore I shall proceed to the Experiment promis'd at the beginning of this Title or Section.

2. To make so much as a tolerable Estimate of the Difference betwixt
such

such great Degrees as are not any of them too weak to congeal water, is a thing, which, as we have not yet known to be attempted, so it seem'd not easie to be perform'd. For, *Freezing* having been commonly reputed the ultimate Effect or Production of Cold, men have not been solicitous to look beyond it. And though the Disparity we find betwixt several Fits of weather, all of them frosty, seem to be too manifest and frequent to be probably ascrib'd to nothing, but the differing Dispositions of our Bodies, yet how to estimate that Difference, it is not so obvious. For though we should have recourse to common Weather-glasses, yet they might easily deceive us, since not only by estimating by them, the coldest day of one Winter, with the coldest day of another, but in judging of the Coldness of any two days in the same fit of frosty weather, there intervenes time enough to make it doubtful, whether the vari'd Gravitation of the Atmosphere, produce not the change observ'd in the Weather-glass. Besides that, admitting
vulgar

vulgar Thermometers could not, as they easily may, misinform us, they are employ'd only to give us an Account of those degrees of Cold, which Nature of her own accord produces in the Air; but not to *discover*, whether or no Nature assisted by Art, may not produce greater: And, 'twill easily be granted, that they are yet less made use of to help us to an *Estimate* of this Disparity. And though some guesses may be made by the operations of Cold upon Liquors expos'd to it, yet some, as water, and very aqueous Liquors, will freez too soon, and others, as Vinous spirits, will not at all, (that we have found) here in *England*. And though French-Wine will sometimes be brought to begin to freez, yet that happens but very seldom, and in many Winters not at all, and leaves too great an Interval, betwixt the degrees necessary to congeal Wine, and sufficient to congeal Water, not to mention the uncertainty proceeding from the differing strengths of the Wines.

3. Upon these and other considerations

rations we thought it requisite to make use of an Expedient, whose Nature and use will be easily gathered out of the following Experiments: And though by a mischance, that broke my Weather-glass, I have been hindred from measuring exactly in what Proportion to the whole bulk the spirit of Wine was contracted, by the surplusage of Cold, that was more then necessary to make water freez, yet I doubt not but something of use to our present Theme, may be thence collected, and especially the main thing design'd will manifestly appear, which is the Intensity of Cold produc'd by Art, beyond that which Nature needs to employ upon the glaciating of water.

[4. A small seal'd Weather-glass furnished with spirit of Wine, the ball being about the bigness of a large Nutmeg, and the Cylindrical stem being very slender, and about ten Inches long, the Ball and part of the stem being immers'd in a vessel of water, half buri'd in snow and salt, when the water began to freez at the top, the bottom and the sides (but before

before the Ice had reach'd the Ball, for fear it should break it) the tinctured liquor was found subsided to $5\frac{2}{3}$ Divisions, being half Inches, and being taken out thence, and Ice and Salt being immediately appli'd to the Ball, the Liquor fell lower to about $1\frac{1}{2}$ Division.]

And that it may not be doubted, but that the water, though in part congeal'd, remain'd warm in comparison of the spirit of Wine, though uncongeal'd, that had been refrigerated by the snow and salt, we will add this other Experiment, which we find in another of our Notes thus set down.

[5. The seal'd Weather-glass being kept in the water till it began to freez, 4. Jan. 15. descended to $5\frac{1}{2}$: Being immediately remov'd into the same snow and salt, that made the water begin to freez, it descended at the beginning very fast, and afterwards more slowly, till it came to the very bottom of the stem, where it expands it self into the Ball; then being remov'd into the same glass of water, whence it was taken, and which was well stor'd with

with loose Pieces of Ice, it did nevertheless hastily ascend at the beginning, and was soon after impell'd to the former Height of five Divisions and an half, or thereabouts.]

6. But perhaps some amends may be made for the disaster of the Weather-glass, by adding, that I found by another Trial, that the Condensation of Liquors by such Colds, as we are wont to have, or can easily produce here, is nothing near so great as one would imagine. And though for want of a Glass-ball, furnish'd with a neck slender enough, I could not make the Experiment so much to my satisfaction, as perhaps else I might have done; yet the goodness of the scales, I made use of, and some greater care, then possibly every Experimenter would have imploy'd, may make the following Observation Luciferous.

7. We took then (on a cold, but not frosty day) oyl of Turpentine, as a Liquor, whose being free from phlegm or water, we would easily be more certain of, then if we had imploy'd spirit of Wine; and this
oyl

oyl it self we rectifi'd in a gentle
 heat, to make it the more pure and
 subtle. Then we took a small round
 vessel of clear glass furnish'd with a
 conveniently long stem or pipe, and
 having first weighed the glass alone
 in a pair of very good scales, we
 found it to weigh ʒi , ʒi , $56\frac{1}{2}$ gr.
 then putting in oyl of Turpentine,
 till it fill'd the round part of the
 Glass, and ascended a little way into
 the stem, we carefully mark'd with
 a Diamond on the outside of the
 Glass, how high it reach'd, and then
 weigh'd the Glass and the Oyl toge-
 ther, which weigh'd ʒij , ʒvii , and
 $34\frac{1}{2}$ gr. then we put in by degrees a
 quarter of a Drachm, and with a Dia-
 mond carefully mark'd, how high
 it reach'd in the pipe, and so we con-
 tinued putting in several Quantities
 of oyl, still carefully weighing each
 parcel in the scale, and marking its
 height on the outside of the Glass
 (which we did in order to a certain
 design, and found it a work tedious
 and troublesome enough) till the Li-
 quor and the Glass together weighed
 ʒiiij , ʒj , $4\frac{1}{2}$ grains; then we put fair
 water

* See the
latter part
of the next
Title.

water into an open-mouth'd Glass,
in which we also plac'd the little Bolt-
head with oyl of Turpentine, and by
such a circumposition of salt and
snow, as is * hereafter to be often
mention'd, we made the water,
which was contain'd in the wide
mouth'd Glasses, and by which the
Sphærical part of the Bolt-head, con-
taining the Oyl, was surrounded,
we made this water, I say, begin to
freez, and when we perceiv'd a little
Ice to be produc'd in it, we carefully
mark'd with a Diamond to what part
of the stem the oyl of Turpentine was
subsided, and then transferring the
Bolt-head into a mixture of snow and
salt, where we kept it for an hour or
two, till we could perceive it to fall
no lower, and marking with a Dia-
mond this station also of the Liquor,
we afterwards remov'd the Glass in-
to a warmer Air, till the Oyl by ex-
panding it self had regain'd the high-
est mark, whence it had begun to
sink. Then into a very little Glass,
carefully counterpois'd in a pair of
exacter scales then the former, we
gently poured out of the Oyl, till
what

what remain'd rested against that mark on the outside of the stem, to which it fell, when the water began to freez : and this we found to amount to somewhat above $9\frac{1}{2}$ grains, so that for conveniency of reckoning, we may safely enough take the Intire number of 10. grains. - After this we poured out of the remaining oyl into the same little Glals, till, what rested in the Pipe, was even with that mark, to which the snow and salt had made it fall ; and this parcel of oyl hapned to be almost precisely of the same weight with the other ; so that in this Trial (for perhaps in others, which it were therefore worth while to make, the degree of Cold may much vary the Events) the Artificial way of freezing, we imploy'd, made the oyl subside as much after it had been refrigerated and condens'd by a cold capable of freezing water, as that degree of Cold had been able to condense it at first. And lastly, having deducted the weight of the Glals from the weight of the whole Oyl and Glals, to obtain the weight of the oyl alone ; and having divided

M

ded the weight of the whole Oyl, *first*, by that of the former parcel, we have mentioned to be ten grains, and *then* by the superadded weight of the second parcel, we took out, (both which parcels together we estimated at twenty grains,) we found that rectifi'd oyl of Turpentine of a moderate temper, being expos'd to such a degree of Cold, as would freez common water, did by by shrinking lose but *about* a ninty fourth part of its Bulk, and being reduc'd to as great a degree of Cold as we could bring it to by snow and salt, ev'n then it lost but *about* a forty seventh part of its Bulk; I say *about*, because I thought it needless, as well as tedious to mind fractions and little odd numbers, especially since, as we formerly intimated, it was scarce possible to arrive at a great exactness in such a Neck, as that of our Bolt-head, though it were proportionable enough to the Ball, and chosen among several, that were purposely procur'd for the trying of Experiments.

8. There are some other Trials
about

about the Degrees of Cold, which for want of Ice and other Accommodations we could not make, as we would have done, *often*; nor shall scarce be able to do it, till more friendly Circumstances afford us an opportunity: And yet because our Trials, though not prosecuted as far as we thought, may possibly prove not unwelcome, we will subjoyn something about two of the chiefest of them.

9. The one was design'd to measure in what proportion water of a moderate degree of Coldness, would be made to *shrink* by the composition of snow and salt before it begin by Congelation to *expand* it self: of this, what we shall here take notice, is only, That by a Trial purposely made with common water, in a round Glass furnish'd with a long stem, we found the water in that stem to subside so very little, that, whether or no it were insensible, it was inconsiderable. But probably a greater Quantity of water, and a slenderer stem, would have made the shrinking of the Liquor more No-

table; and upon that Account 'tis, that I here mention It.

10. The other Thing was, to measure by the differing weight and Density of the same portion of water, what change was produc'd in it, betwixt the hottest time of Summer, and first a glaciating Degree of Cold, and then the highest we could produce by Art. And in order to this, we weigh'd with a pair of exact scales, a glass bubble heavier then water, in that liquor, when it seemed to be at a moderate Temper, as to Coldness, and by the Diminution, which we found of the glasses weight in the water, we easily collected, according to the Rules of the Hydrostaticks, the weight of as much water, as is equal in bulk to the glass Bubble, and thereby the Proportion betwixt the glass and an equal bulk of such water, as we first weighed it in; then by the application of snow and salt, we made that water begin to freez, and weighing in it again the same bubble, 'twas easie to collect by the Decrement of its weight in this refrigerated water, what Proportion an equal Bulk of the liquor

liquor did *then* bear to the Glafs; and by comparing these two differing Proportions together, we were assisted to make an Estimate, how much the water was made more heavy, and dense by the Action of a freezing degree of Cold; Afterwards taking our time in Summer, we thought fit in the same parcel of water (that had been purposely reserved in a glafs) to weigh the same bubble, that by the difference of its weight in the water, when made much lighter by the heat of the ambient Air, we might obtain the Information we desir'd : to which we shall add, That we also recommended to some *Virtuosi*, that were likely to have the opportunity of gratifying Us, that such an Experiment might be procured to be made in the midst of Summer in some part of *Italy*, by the help of the there not unfrequent Conveniency of a Conservatory of snow, wherein the water might be reduc'd to freeze before the end of the same hour, at whose beginning the there warmer Air had given it its greatest Expansion, and so the Difference

rence betwixt the Density of the same parcel of water might be the more conspicuous. But as I have not received any Account of my Desires from abroad, so coming now at home to review the Memorial, I caused to be written of the newly mention'd Observation, I find, that through the Negligence or Mistake of an *Amanuensis*, there must needs be a manifest oversight committed in the setting down the Numbers, which my Memory does not now enable me to repair. And the season being now improper to repeat the Experiment, as well as the numerical parcel of water I had kept, and I imployed both times, being thrown away, I think it may be sufficient, if not too much, to have thus particularly intimated the way we took, without adding the Cautions, wherewith we proceeded, nor what Trials we made to the same purpose with high rectifi'd spirit of Wine, since unlucky accidents frustrated our Attempts.

II. Whether the making of these kind of Trials, with the waters of the particular Rivers or Seas, men
are

are to sail on, may afford any useful estimate, if, and how much, Ships and other Vessels, may on those waters be safely loaden more in Winter then in Summer, may be an Enquiry, of which I shall not in this place take any further Notice, then to intimate thus much, That the difference betwixt water highly refrigerated, and that which is but of an usual degree of coldness, is not so great as some Learned Moderns seem to have thought. For on a Day, which (though made cold by snow intermingled with the rain that then fell) was not a frost, we took common water, and weighed in it a glass Bubble, whose weight in the Air was 150. grains, and this Bubble weigh'd in that water, lost so much of its former weight, as to weigh about $28\frac{5}{8}$ grains : and then by snow and salt, reducing that water to such a degree of Coldness, that it began to be turned into Ice about the inside of a small open glass that contain'd it, we found the same Bubble not to weigh at all above one eighth part of a grain less then it did before : So that,

(108)
if we may judge of the shrinking and
condensation of the water by the In-
crement of weight, it shrunk but
about a 230. part of its former Bulk,
and this according to a pair of scales,
that would turn with about the 32.
part of a grain: which may keep us
from wondring at what we lately de-
livered concerning the very inconfi-
derable subsidence of the water, we
exposed to snow and salt in a small
Bolthead. And it may also make
that the more probable, which we
not long since related about the oyl of
Turpentine not losing much above
a 100. part of its Bulk, by being
expos'd to such a degree of cold, as
made water begin to freez. Whe-
ther we may from this, and from the
formerly recited Experiment, of the
great subsidence of spirit of Wine in
a seal'd Weather-glass, safely con-
clude, these subtile distill'd Liquors
to be much more sensible then water
of Cold, as well as of Heat, further
Trials will best resolve; and these I
have not now so much opportunity,
as I could wish, to pursue.

13. But they that have a mind to
prosecute

prosecute Experiments of this kind, and others, that relate to the *Degrees of Cold*, may perchance be somewhat assisted even by these Relations, and especially by those Passages that mention the use of the seal'd Weather-glass, furnish'd with spirit of Wine, and of those wherein a drop of liquor is kept pendulous. For the former of these being not subject to the Alterations of the Atmospheres Gravitation, nor (as may be probably suppos'd, by reason of the strength of the high rectifi'd spirit of Wine) to be frozen, by sending the same Weather-glass (which may be made portable enough, as I have tried by transporting one of them in a Case that might be easily carri'd even in a Pocket) from one Countrey to another, one may make far better Discoveries of the differing Degrees of Coldness in differing Regions, and know (somewhat near) how much the Air even of *Muscovy*, or *Normay*, or *Greenland* it self, is colder then that of *England*, or any other Countrey, whence the Weather-glass shall be sent :

sent: The Instrument being accompanied with a memorial of the Degree, it stood at, when expos'd to such a Cold, as made water begin to freez.

13. The other Thermometer, where a drop of liquor is kept pendulous, may not only be imploy'd in such cases, where the Pipe and Bubble can be erected upon the Horizon, but by reason, that the outward Air will indifferently impel the Bubble laterally or upwards, upon the Refrigeration of the inward, and that the bubble will not barely by its weight drop out of the inverted Instrument, because of the resistance of the subjacent outward Air; for these causes, I say, such a Thermoscope may, as we have tri'd, be also us'd, where the Pipe shall be held Horizontal, or inclin'd, or even Perpendicularly downwards, so that the flat Part of the Bubble may be appli'd to discover the Coldness, either of the Wall, or of the Ceiling of a room, or other Bodies however situated. And if the Pipe be made
long

long and even, (as sometimes we im-
 ploy one above a foot long) not only
 sensible, but great Effects of very
 little Disparities in the Coldness of
 Bodies, to which the Instrument is
 appli'd, may with pleasure be obser-
 ved. And the same drop of liquor
 may be long enough preserv'd useful
 in the Pipe. But this Advertisement
 I shall give, that as sensible as this
 Instrument appears to be of the nicer
 Differences of Coldness, as of Heat,
 yet they that shall have the Curiosity
 to examine with it, as I have done,
 the Temperature, I say not, of more
 resembling Bodies, but of Liquors,
 that may be thought to have their
 parts so differinglly agitated, as com-
 mon Water, high rectifi'd Spirit of
 Wine, and even rectifi'd oyl of Tur-
 pentine, (I add not *Dephlegm'd oyl of*
Vitriol, becaule of some odd *Phæno-*
mena not here to be insisted on) will
 perhaps find the Event so little, in
 many cases, answer the Expectation
 he would have had of uniformly
 finding great Disparities in their
 actual Coldness, if he had not met
 with

with this Advertisement, that he will not much wonder, that a Person, who wants not other Employments for his Time, was willing to decline so tedious and nice a Task.

Title



Title V.

*Experiments touching the Ten-
dency of Cold Upwards or
Downwards.*

1. **T**Hough, after the consideration of the sphere of Activity of Cold, it would be the most proper place to take some Notice of the *Direction* of its *Activity*, yet because one of the Experiments, that belong to This head, is of great use to facilitate the trial of many of those, that follow, throughout this whole Collection; we will no longer delay to say something of this matter, namely, in what Line, or, if you please, towards what part the frigefactive vertue of cold Bodies does operate the furthest and the most strongly.

2. 'Tis a Known Doctrine among Philosophers, that the Diffusion of Heat tends chiefly upwards, as the flame

flame of a Candle will burn many things held over it at a greater Distance, then it would considerably warm them at, in case they were held beneath its level, or even by its sides: and 'tis true, that in all cases vulgarly taken notice of, the observation, for reasons elsewhere discoursed of, holds well enough; and therefore it may be worth enquiry, whether in Cold, which is generally looked upon as the contrary Quality to Heat, the diffusion (from cold bodies) be made more strongly downwards, then either upwards or towards the sides.

About this matter, I can as yet find among my Notes but the two following Experiments, and those not both together.

[A very thin bubble was blown at a Lamp, and purposely made flat at the bottom, that it might be the more exposed to the cold, and it was suspended by a string within a pretty deal less then an inch of a mixture of beaten Ice and Salt, where-with we had half fill'd a conveniently large wide-mouth'd glass, but we could

could not find, that a cold, Capable of freezing, did strike so high upwards, for the water in the bubble remained altogether unfrozen; which agrees very well with what we have observed, that a mixture of ice and salt did not congeal the vapours, that wandered through the Air, above half a barley corns breadth higher, then the mixture in the Glass reached.]

3. [A mixture of snow and salt being put into a vial with a long neck, the round part of it was by a weight kept under water, out of which being taken after a while, the outside of the glass beneath the surface of the water was cased with solid Ice, *N B.* especially about the bottom of the vial, of greater hardness and thickness then one could easily imagine.]

4. Thus far the notes, from which nevertheless I will not *positively* conclude, though they seem to perswade it, that the tendency of the cold produced by Bodies qualified to freeze others, is greater *downwards* then *upwards*: For, the satisfactory determination of that matter may, for ought I know, require Trials more artificial

artificial and nice, then those we have been reciting. And I could wish, that I could find the last of them to have been carefully repeated and registred, because it seems somewhat strange, that the Ice should be much thicker at the bottom of the vial, then elsewhere, in regard, that when we have, as we very frequently have, put mixtures of snow and salt into vials, and left them in the open Air, we generally observ'd, that the outside of the Glafs was cas'd with Ice, or covered with hoar frost, directly over against that part of the inside of the Glafs, wherein the frigorifick mixture was. So that part of the snow and salt resolving one another, and falling down in the form of a liquor to the bottom, the unmelted part of the mixture would float upon this liquor, and the external Ice would appear over against the floating mixture, by which it was generated: So that as the mixture grew thinner and thinner, so would the Zone or girdle, if I may so call it, of external Ice, grow narrower and narrower, till at length, when the
snow

(1775)
snow was quite melted away, the external Ice would quickly also vanish. But from this observation (which we frequently made) That as in such vials the Ice did not appear (as I just now related) above half a corns breadth higher then the mixture in the glass; so I remember not to have observed it much lower beneath the mixture; from those things, I say, it may be probably conjectured, that even the coldest Bodies (at least unless their Bulk alter the case) do not diffuse their freezing vertue, either upwards or downwards to any considerable distance.

5. These trials, as I was intimating, may suggest some difficulties about the last of the two Experiments, transcribed out of my notes. But as 'tis evident these observations were made in the open Air, by the freezing of its roving vapours, and the mentioned Experiment was made under water, so how much this difference of *mediums* may alter the case, as to the way of the Diffusion of cold, I dare not, till further trial, boldly determine, especially since

N

one

one Circumstance, to be under the next Title mentioned, about the freezing of Eggs, may pass for an additional Experiment as to our present Enquiry: For the Cases obtain'd by frozen Eggs suspended under water, which seem to argue, that the Diffusion of their cold was made every way, since they were quite enclosed in the Ice, they had produced.

6. Though the Experiment of freezing water by the Intervention of salt and snow be not a new one for substance, yet I hold it not amiss, to make a further mention of it on this occasion. Because that what I am to deliver about it, is a Particular not taken notice of (that I know of) by others; the premising of which, will, according to what we lately intimated, much facilitate the trial of many of the Experiments to be set down in the following part of these papers, and will indeed appear to be of no small moment in our whole Attempt of Framing an *History of Cold*. For it has long seemed to me one of the chief things, that has hindered men from making any considerable progress

progress in this matter, that whereas glais-vessels are generally much the most proper to freez liquors in, because their transparency allows us to see what changes the Cold makes in the liquors exposed to it; the way of freezing with salt and snow, as it has been hitherto used, does almost as little, as the common way of barely exposing vessels to the cold Air in frosty weather, prevent the unseasonable breaking of the glasses. For in both these ways, the water or other liquor, usually beginning to freez at the top, and it being the Nature of Glaciation, as we shall see anon, to distend the water and Aqueous liquors it hardens, it is usually and naturally consequent, that when the upper crust of Ice is grown thick, and by reason of the Expansion of the frozen liquor bears hard with its edges against the sides of the glass, contiguous to it, the included Liquor, (that is by degrees successively turned into Ice) requiring more Room than before, and forcibly endeavouring to Expand it self every way, finds it less difficult to burst the

glass, then lift up the Ice ; and consequently does the former, and thereby spoils the Experiment, before it be come to perfection, or have let us see what Nature would have done, if she had not been thus hindred in her work.

7. The consideration of this invited me to alter the common way of freezing, and order the matter so, that whensoever I pleased, the exposed liquor should not begin to freeze at the top or sides, but at the bottom, which I concluded it very easie to do, by mingling the salt with that part only of the snow, which was to lye beneath and about the bottom of the glass I placed in it. For by this means the snow, that was contiguous to the sides, was able but to cool the water, and dispose it to Glaciation, whereas the mingled snow and salt, on which the bottom of the glass rested, did actually turn the neighbouring Liquor into Ice, and lift up the incumbent liquor toward the higher and empty parts of the glass: And this liquor also I could afterwards freeze at pleasure, without danger of breaking

breaking the vessel, only by so applying salt and snow to the sides of the glass, that they never reached, except perhaps at the very conclusion of the Experiment, so high by a reasonable distance, as the upper surface of the liquor in the glass; so that the superior parts of that liquor were always kept fluid, and capable of being easily impell'd higher and higher by the Expansion of the freezing parts of the subjacent liquor.

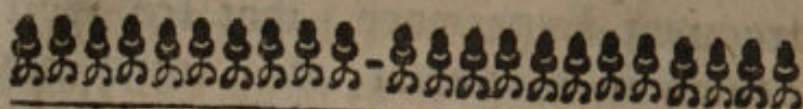
8. The Speculative inference, that may be drawn from this Experiment, of making water begin to freeze at the bottom, not the top, will be more properly taken notice of in another place; In the mean time I shall only intimate by the way, that there is no great necessity of any nice proportion of salt to snow, nor of any exquisite mixture of them: a third or fourth part or thereabouts of Sea salt, in reference to the snow, will not do amiss; nor do I usually put salt to all the snow at once, unless in some case, wherein I have a mind to freeze a liquor quickly, and make a speedy resolution of the snow and salt in order

In the Discourse touching the primum frigidum.

thereunto; to which I shall only add, that by the way above mentioned, I do upon particular occasions make the exposed liquor freeze, not at the bottom or the top, but next to what side of the Glass I please, according to the Exigency of the Experiment. But though it may suffice to have *hinted* the Speculative Inference, that may be drawn from this way of freezing Liquors, it will be expedient to give *explicitly* this practical Advertisement, concerning it, that whereas it seems to have been taken for granted, that snow is necessary in this Artifice, and we ourselves were for some time led away with the rest, by that supposition; yet that is but a presumption, and ought to be removed as one very prejudicial to those that with us design the prosecuting Experiments, in order to the *History of Cold*. For snow is but seldom to be found on the ground in comparison of Ice, and being but a Congeries of many small Icesicles with much Air intercepted among them, it is not (*cæteris paribus*) near so durable as the more intire Body of solid

solid Ice; and yet we have found by frequent Experience, that Ice well beaten in a Mortar, will serve our turn for Artificial Glaciations, as well (if not in some respects better) as snow, and therefore in this History of Cold we indifferently prescribe *Snow and Salt*, or *Salt and Ice* as the Ingredients of our Glaciating Mixtures.

N^o 4 Title



Title VI.

Experiments and Observations touching the Preservation and Destruction of (Eggs, Apples, and other) Bo- dies by Cold.

1. **I**T is a Tradition common enough, though not here in *England*, yet among those that have given us Accounts of very cold countries, that if Eggs or Apples, being frozen, be thawed near the fire, they will be thereby spoiled, but if they be immersed in cold water, the internal cold will be drawn out, as they suppose, by the external, and the frozen Bodies will be harmlessly, though not so quickly, thawed. This Tradition I thought fit to examine, *not only* because it may be doubted, whether it will succeed in

our

our more Temperate Climate, and because I love not to rely upon Traditions, when I have the opportunity to examine them (especially if no one Credible Author affirms them upon his particular knowledge,) *but also* because I thought the Experiment, if true, might be so varied and made use of, as to become *luciferous* enough, and afford us divers *Phænomena* of cold, not so easie to be produced by the more known ways of experimenting. And accordingly having exposed some of these Bodies to a cold that was judged sharp enough, we afterwards put them in water, but found not the event answer our expectations, no Ice appearing to be generated; nevertheless we were not hereby so discouraged, as not to repeat the Experiment (which we judged to be not unlikely) with more sollicitousness and advantage then before; and having thereby brought it to succeed, we afterwards made several trials of it with several distinct aims, but cannot now find any Entry of divers of them. But those I have hitherto met with
among

among my Notes, I shall subjoyn, as having in them some Particulars, that may afford useful hints to an Enquirer into the *History and Nature of Cold*. And I shall set down together, and that in this place (though it would not otherwise be the most proper) those I have met with, because some Circumstances of one or other of them may be of use to us on several occasions in the present Treatise.

2. [An Egg weighing twelve drachms and one grain wrapt in a wax'd paper (to keep it from the liquor of the thawing snow) and frozen with snow and salt, wanted four grains of that weight : put into a dish of fair water there crufted as much Ice about the outside as made the Egg and Ice fifteen drachms and nine grains ; the ice being taken off from the shell, and the shell very well dried, the Egg was found to weigh twelve drachms and twelve grains ; the Egg being broken, was found almost quite thawed ; the Egg frozen swam in water, being thawed it sunk.]

3. [We

3. [We took two Eggs strongly frozen, and in a room where there was a good fire, we put one of them into a deep wooden-dish full of very cold water, and set the other by it, upon a table about two yards from the fire, that they might be in Air of the same temper as to heat and cold; then perceiving the Egg, that lay under water, to have obtained a thick crust of Ice, we took it out, and having first freed it from the Ice, broke it, and found that some part of the white was not yet freed from a pretty store of little parcels of Ice, but the rest of the white (which was much the greater part) and the Yelk seemed to be much what of the same consistence, as if the Egg had not formerly been frozen, whereas the other Egg, that lay by upon the dry table, had not only its whole white frozen into a consistent Body, but the Yelk it self, though we saw no distinct particles of Ice in it, was grown so hard, that it cut just like the Yelk of an Egg over boiled, and being cut quite through, shewed us certain concentrical circles of somewhat differing Colours,

Colours, with a speck much whiter then any of them in the middle of the Yelk; which last circumstances, whether they were accidental or no, further observation must determine.]

Note, that though we have not found above once, that frozen Eggs would swim, yet when we had broken such Eggs, the frozen white would swim, but not the yelk.

4. We afterwards repeated the Experiment of laying two frozen Eggs near together in the place above mentioned, the one under water, and the other out of it, till *that* put in water had got a thick Icy crust, and by breaking of them both, presently after one another, were confirmed in the Perswasion, that frozen Eggs will thaw by great odds (*ceteris paribus*) faster when immersed in water, then when surrounded only with Air.

15. [We likewise took a frozen Egg, and from a fix'd place suspended it so by a slender packthread, that it hung quite under water without yet touching the vessel, that the water was in. This we did partly upon another

another Design, and partly to observe, whether or no the Ice would in this case be considerably thicker or thinner against the lower parts of the Egg, as we formerly mention'd our selves to have observed it to be very manifestly at the lower parts of a glass, which having Ice and Salt in it, was immersed under water; but when we took out the Egg, after we saw that its Icy case had covered the packthread it was hung by, we found the case upon breaking it, of a thickness uniform enough to keep us from concluding any thing from this trial; since, though there were a pretty deal of Ice generated at so small a distance from the case of the Egg, that it seemed to owe its Production to the same cause; yet, which was somewhat odd, we did not find, that this Ice stuck to that which did immediately embrace the Egg, though we had some faint suspicion, that the Rudiments of it might have been very early parted from the Egg, by some little shaking of the table occasioned by peoples passing to and fro in the room.]

6.[We

6. [We took some Pippins, and exposing them to freez all night, and putting them the next morning into a Bason of very cold water (though in a warm room) they were not long there without being inclosed with cases of Ice of a considerable thickness ; Where note, 1. That that part of a floating Apple, that was immersed under water, had a very much thicker coat then the other part which remained above it. 2. That the extant part seemed likewise to be harder then the immersed. 3. That one of these Pippins being purposely left out of the Bason, but layed by it, seemed upon cutting to be harder and more frozen then those Apples which had been put into the water, which scarce seemed to be at all harder then ordinary Pippins, that had never been set to freez, at least as to those parts of the Apples that were near the rinde, and consequently near the Ice. 4. That neither frozen Pippins nor frozen Eggs, notwithstanding their great power of turning part of the contiguous water into Ice, did appear to Us to detain or congeal any

any of the roving vapors of the Air, as Ice or Snow included with Salt in glasses is, (as we have formerly observed) accustomed very remarkably to do.]

7. [We took Eggs, and froze them with ice and salt, till the shells of them were made to crack, then we took them out, and put one of them in Milk, two of them in a wide Drinking Glass full of Beer, and two more in a large Glass, wherein we covered them with Sack, that was poured in till it reached much higher in the Glass than the Eggs. But none of these trials produc'd, as we could perceive one grain of ice.] And being desirous to see, whether the Acid salt of Vinegre, or the Cold in a well frozen Egg, would have the chief Operation, if those two Bodies were put together: I found upon Trial, that the Saline parts of the Vinegre began to dissolve the Egg-shell, as appeared by the much altered Colour of it, but the Cold of the ice in the Eggs was not able to freeze any part of the water or phlegm of the Vinegre.

8. We

8. We had also thoughts of trying whether or no pieces of Iron of several shapes and bignesses, being for divers days and nights exposed to the freezing Air, and afterwards immersed in water, would produce any ice, as frozen Eggs and Apples do. For the Brittleness of the Laths of Stone-Bows in sharp frosts, together with other observations elsewhere mentioned, seem to argue, that (to use a popular phrase) the Frost does also get into these Bodies. And I have been assured by one, whom the Trials, I had made with Eggs and Apples, invited me to consult, that a great Cheese, he immersed in water in a Cold Countrey, was presently covered over with ice. But though, as I said, I had thoughts of making the above mentioned Trials, yet for want of a frost sufficiently durable, I was not able to effect what I design'd. But thus much I tri'd, That though I kept good Lumps of Iron, and as I remember of other Metalls, besides pieces of Glasse, and a stone or two of a convenient size, in snow and salt, I know not how much longer, then
would

would have suffic'd to make Eggs or Apples, or such kind of things fit to produce store of ice in water, upon their being thaw'd therein; yet we could not find, that upon the immersing the several newly nam'd Mineral Bodies, there was the least ice produced in the cold water, where we kept them covered. I must not nevertheless omit to make some mention of that which lately seem'd to happen at the door of our own Laboratory (respecting the North East) where some Glasses, newly brought from the shop, and not imployed, lying in a Basket, as they poured water into one of them to rince it, part of it was presently turned into ice, whilst one of my Domesticks held it in his hand, who coming presently to show it me, I suspected the ice might have come from, or rather with the water that was poured into the Glass, but upon enquiring was assured of the Contrary.

9. But here I must not omit another trial relating to the former Experiments, which may seem somewhat odd, if its Event prove con-

O

stantly

stantly the same, as when we tried it. For after these and divers other Experiments made, with frozen Eggs and Apples, we thought it might be worth the examining, whether or no Ice and the Liquors of these Concretes would produce the like effects, as Frozen Eggs and Apples; and because 'tis usually an easier way, then that which is more common of bringing Bodies, whose degree of cold is more languid, to freeze water, to include them with ice or snow in a single vial, and so put them upon acting only upon the minute, and easily congealable vapors that wander in the Air: we took that Course in the trials we are mentioning, whose success is thus briefly set down in one of our notes.

[10. Ice and Juice of Pippins well shaken together in a single vial, produced abundance of dew, but we could not satisfy our selves, that it produced any Ice.]

[11. Also Ice and the white of an Egg moderately beaten into a Liquor, were tried, with just the like success: But these trials having scarce
been

been made above once, and at most but twice, are to be repeated.]

12. As for what is said, That Eggs and Apples thaw'd in the water, are better preserv'd then thaw'd by the fires side, we tri'd it in Pippins (for in Eggs the Experiment is not so easily and quickly made) and as far as we could discern, found it true, and somewhat wondred to see, how soon, and how much putrefaction was induc'd into those loosely contexted Bodies by an overhasty thawing.

13. If we may believe the Relations of Navigators, and others of good Credit (of one or two of whom I had the opportunity to make Inquiry) there may be good use made of what happens in the different ways of thawing Eggs and Apples, by applying the Observation to other Bodies, and even to Men, that happen to be dangerously nipp'd by excessive Cold. For it is a known Observation among those, that have inhabited or visited the Northern Climates, that if those, whole hands or feet, or faces happen to be frozen, approach

them too near or hastily to the fire, they are in danger of losing, or at least much prejudicing the overhastily thaw'd parts. (*Upon divers of us (says Captain James, speaking of his companions) had the cold rais'd Blisters as big as Walnuts. This we imagined to come, by reason that they came too hastily to the fire.*) And therefore they that are more careful to be safely then quickly deliver'd from the painful cold, are wont, before they come near the fire, whether it be open or in Stoves, either well to wash their hands, or other frozen parts, in very cold water, or else to rub them well with Snow it self. And this brings into my mind, that I sometimes indeavoured to find by trial, what Beef long exposed to freez, and differingly thaw'd, would teach me by way of confirmation of this Tradition; but being then oblig'd to unseasonable Removes from the place where I made my Trials, they did not for that Reason afford me the satisfaction I desir'd; but meeting with an intelligent person, that had been an housekeeper in *Muscovy*, and enquiring

quiring of him whether he had observed any thing about this matter, he told me, that having once had two very large Cheeses frozen, he thaw'd one of them in water, and the other in a Stove, but found, that thawing in water was much the better way of the two ; and I was well pleased to be answered by him, that the Cheese, thaw'd in water, did soon acquire therein a Crust of ice.

14. But more memorable is that Relation, which I remember I have read in the experienc'd Chirurgion *Fabritius Hildanus's* Treatise of *Gangrenes*, where he relates from credible Testimony, how the whole Body of a Man was successfully thaw'd, and which is more strange, cas'd all over with ice, by being handled as our Eggs and Apples were. His own words, because the Narrative may prove of some use, I shall subjoyn, and they are these ;

Narravit mihi vir quidam nobilis & fide dignus, se, cum eas Regiones peragraret, incidisse aliquando in Viatorem secundum Viam frigore rigidum, ac pene mortuum, quem plauastro suo impositum, cum dedux-

*Guilielmus
Fabritius
Hildanus
de Gangr.
& Spha-
celo Cap.
10.*

isset in Diversorium, hospes illico demersit in frigidam, quo facto undiquaque ita erupit Gelu, ut ipsius Corpus glacie, seu ferreo Thorace contextum conspiceretur. Tum quoque propinatum illi aiebat Cyathum ampliozem Hydromelitis, quo illi seu potu ordinario utuntur, addito pulvere Cinamomi, Caryophyllorum & Macis, unde sudor in lecto provocatus est; atque ita agrum ad se rediisse amissis duntaxat manuum & pedum extremis Articulis. Hinc intelligimus hanc Methodum sanandi congelatos veram ac tutam esse, ac eam etiam probat Summus Philosophus qui regiones illas frequentavit, &c.

15. The Experiment deliver'd at the Beginning of this Title, (of speedily producing ice on the outside of frozen Eggs and Apples, by immersing them in Cold water) I take to be one of the two or three most illustrious, I have hitherto met with about congelation; and as likely as any to assist us to investigate the causes of it. But though the *Phænomena* seem very favourable to their *Hypothesis*, that suppose congelation to be effected by the ingress of frigorifick Atoms into the water or other Bodies

to be congealed ; yet (for some reasons) I shall not here offer to draw any speculative inference from the Experiment, contenting my self to have here, and at the beginning of this Section hinted *in transitu* the hopefulness of its proving Luciferous.

16. But I remember that the Title of this Section promises something concerning the preservation and destruction of other inanimate Bodies, as well as Eggs and Apples, by Cold ; but as that intimated promise makes the last part of the Title, so what I have to deliver on this subject must not be expected to be other then the last part of this Section. And indeed to be able to add much to that little, which is generally known about this subject, I should either have liv'd in colder Climates then ours, or have had, which I had not, the opportunity of making Experiments, that require length of time. And therefore I shall only propose a general Consideration about this matter, and subjoyn a few of the chief Observations I have met with in

Navigators or others about it. *That* then, which I would premise in general, is only this, That whether Bodies be frozen by the ingress of frigorifick Atoms, which by their intruding in swarms, can scarce avoid discomposing the Texture of the Body, or whether it be made by the recess of some matter, that did before Congelation, more strongly agitate its parts; which way soever, I say, freezing is effected, 'tis manifest, that the Nature of a frozen Body is, at least for the time, much alter'd, and therefore we thought fit to place it among our general Articles of Inquiry about Cold, what the effects of it may be as to the Conservation or Destruction of the Textures of Bodies. But as for the duly prosecuting this inquiry, we do, as we lately intimated, want the time and conveniency, we judge needful for such a work, the matter seeming to require, that it be watchfully and considerately manag'd, and that both the Nature of particular Bodies, and the differing degrees of Cold, and the differing times wherein the Condition of the

the expos'd Body is estimated, be taken into Consideration. For we find, that a moderate degree of Cold preserves many Bodies, and that glaciation destroys, or at least prejudices most others (probably by decomposing or vitiating their Texture) when they come to be thaw'd, though whilst the Frost is in them, it keeps almost all Bodies from disclosing any putrefaction.

17. This being the general Consideration I intended to propose, it remains that I add out of credible Writers, or other Relators, some Observations to illustrate and confirm the chief particulars comprehended in it.

And first, that a moderate degree of cold conduces much to the preservation of the greatest part of inanimate Bodies, is a thing vulgarly taken notice of and acknowledg'd. And I do not readily remember any instances that manifest, that any degree of Cold, though more than moderate, provided it fall short of freezing the Bodies expos'd to it, does spoil them. *Regii Mutinenses* (says the

Barthol. de
usu Nivis
pag. 80.

the industrious Bartholinus) *nivem hoc
fine arte compactam servant in Cellis
Nivariis, in quibus fervente æstate vidi
carnes mactatorum Animalium à putre-
dine diu se conservasse.* The next thing
I shall mention to our present pur-
pose, is a memorable passage in
Captain James's Voyage, which
shows, that so great a Degree of
Cold, as may be suppos'd to have
reign'd in his ship, that was frozen
up all the Winter in one of the Cold-
est Regions of the World, was not
great enough to spoil the meat and
drink, that had layen all that time un-
der water, because it seems by the
story, that they were not actually
frozen; the words of his Journal are
these. *By the Ninth of May we were*
Pag. 74. *come to, and got up our five Barrels of
Beef and Pork, and had four Buts of
Beer, and one of Cyder, which God had
preserved for us: it had layen under wa-
ter all the Winter; yet we could not per-
ceive, that it was any thing the worse;
which is the more remarkable, be-
cause of what we shall note by and
by, both out of other Books, and
even out of this, about what became
of*

of a stronger Liquor then Beer, once brought to Glaciation : And it seems our Navigator found Cold, it extremely intended, so destructive a thing, that he thought fit to take notice in his Journal, That even a Cable having layen under the ice all the Winter, was not in June found a jot the worse. Pag. 79.

18. And it seems by a passage in *Simlerus's* account of the *Alpes*, that even Intire Bodies may be very long preserved by snow, and, as far as I can guess by the story, without glaciation. Refert (says *Bartholinus*, speaking of him) in *Rhetis apud Riva* *Barthol. de figura nivis pag. 79*
waldios, nivium è monte ruentium moles sylvam & proceras Abietes dejecisse; accidisse etiam Helvetio milite per Alpes iter faciente ut 60. homines & plures eadem nivis conglobatione opprimerentur. Hoc igitur Nivium tumulo sepulti, ad Tempus Æstatis delitescunt, quo solutâ nonnihil Nive Deciduâ, Corpora mortua inviolata patent, si ab amicis, vel transeuntibus querantur. Vidimus ipsi triste hoc spectaculum, &c.

19. Secondly, I could alledge many instances to show, that many, if not

not most inanimate Bodies, (I say *inanimate*, because of the *Gangrenes* and *Sphacelations* that often rob living men of frozen Toes, Noles, and sometimes other parts) if they be actually frozen, will not disclose any putrefaction, whilst they continue in that state. Nor is this much to be wondred at, since whether we will suppose, that in Glaciation the moist and fluid parts are wedg'd in by intruding swarms of frigorifick Atomes, or that those restless particles, that were wont to keep the Body fluid or soft, are called forth of it, be the cause of glaciation ; which soever of these two ways we pitch upon, we must in frozen Bodies conceive an unwonted rest to be produced of those movable particles, whose internal commotions, and disorderly coalitions and Avolutions, are either the Causes, or the necessary Concomitants of Corruption.

20. On this Occasion I remember, that meeting with a knowing Man, whose affairs stopp'd him during the Winter upon the Coasts of *Sweden* and *Denmark*, being desirous to learn
of

of him, how long they could in those colder Climates preserve in Winter Dead Bodies unburied, and yet uncorrupted, he told me, he had opportunity to observe, that though the frost lasted, as it usually did in that season, three or four moneths together, or longer, the Bodies might without any Embalming, or other Artificial way of preservation, be kept untainted by the bare coldness of the Air. Of Bodies lasting long unputrified in ice, Navigators and others have afforded us several instances, but we will mention two, because they contain something more remarkable then the rest. The one is thus delivered by *Bartholinus*. *Nec-*

tandum, Corpora occisorum hyeme eodem Barthol. de usu Nivis pag. 83.
positu, eademque figurâ permanere rigi-
dâ, quâ ante eadem depræhensa sunt.

Visum id extra urbem nostram, quum II. Feb. 1659. oppugnantes hostes repellerentur, magnæque strage occumberent: alii enim rigidi iratum vultum ostendebant, alii oculos elatos, alii ore diducto ringentes, alii Brachiis extensis gladium minari, alii alio situ prostrati jacebant. Imo ex mari gelato, primo vere resoluta, eques

Capt.
James's
Trav. pag.
76.

equus equo suo insidens integer emerfit, nescio quid manibus tenens. The other instance is afforded us by Captain James's Journal, and is by him thus delivered. In the Evening (of the 18. of May) the Master of our ship, after Burial returned aboard ship, and looking about her, discovered some part of our Gunner under the Gun-room ports. This man we had committed to Sea at a good distance from the ship, and in deep water near six moneths before. The 19. in the morning I sent Men to dig him out, he was fast in the Ice, his head downwards, and his heel upwards, for he had but one Leg; and the Plaister was as yet at his wound: in the afternoon they digged him clear out, after all which time he was as free from noisomness, as when we first committed him to Sea. This alteration had the Ice and water and time only wrought on him, that his flesh would slip up and down upon his Bones like a Glove on a mans hand. But there is one pertinent particular more, which if it be strictly true, is so very remarkable, that I cannot on this occasion forbear to annex it, which is, That according to the relation of the Mer-

Merchants of *Copenhagen*, that return
thither from *Spitzberg*, a place in
Greenland, the extreme Cold will there
suffer nothing to putrifie and corrupt, in-
somuch that Buried Bodies are preserved

*Barthol. de
usu Nivis
Cap. 12.*

30. years intire and inviolated by any
rottenness.

21. Thirdly, though whilest Bo-
dies continue frozen, the cold (as
may be supposed) by arresting the
insensible particles, from whose tu-
multuary motions, and disorderly
Avolutions Corruption is wont to
proceed, may keep the ill operati-
ons of Cold upon the violated Tex-
tures of Bodies from appearing; yet
when once that impediment is remo-
ved, divers bodies make haste to dis-
cover, that their Texture was dis-
compos'd, if not quite vitiated by the
excessive cold. I might alledge on
this occasion, that I have shown di-
vers ingenious Men by an Experi-
ment I have taught in another * *Trea-*
tise, that the change produc'd in the
Textures of some Bodies by glaciati-
on, may be made manifest even to
the sight. For by freezing an Oxes
Eye, the Crystalline humour,
which

* *Of the
usefulness
of Experi-
mental
Philosophy.*

which in its natural state is transparent enough, to deserve its Name of Crystalline, though not fluid enough to deserve the Name of humour, lost with its former Texture all its Diaphaneity, and being cut in two with a sharp knife, appeared quite throughout very white. But for confirmation of this I shall rather add, that I remember, that the person formerly mention'd, that had made trial of the two Cheeses, confess'd to me, That, though that which had been thaw'd in Cold water, was very much the less spoil'd, yet they were both of them manifestly impair'd (and the other of them was so in its very consistence) by the Frost, though the Bulk of the Cheeses was very considerable, and though they were both of them, of a more then ordinarily good and durable sort.

22. The next thing I shall alledge to this purpose, is the Observation of the *Hollanders*, even by such a degree of cold as they met with in *Nova Zembla*, before the middle of *October*, at which time their strong Beer, by
being

being partly frozen, had its Texture so vitiated, that the reunion of its unfrozen to its thaw'd parts could not restore it to any thing near such a spirituous Liquor, as it was before. *We were forc'd* (says *Gerad de Veer*, that wrote the story) *to melt the Beer,* Purch. Lib. 3. cap. 5. Sect. 2. pag. 493. *for there was scarce any unfrozen Beer in the Barrel, but in that thick Yeast that was unfrozen, lay the strength of the Beer, so that it was too strong to drink alone; and that which was frozen tasted like water, and being melted, we mix'd one with the other, and so drank it, but it had neither strength nor taste. And in the next Moneths Journal he tells us, that their best Beer was for the most part wholly without any strength, so that it had no savour at all. But a more remarkable instance to our present purpose, is afforded us by our Countrey-man Captain James, because it manifests the Cold to have the same effect upon a much stronger and more spirituous Liquor. I ever doubted* (says he in his Journal) *that we should be weakest in Spring, and therefore had I reserved a Tun of Alegant Wine unto this time. Of this by putting seven parts of water to one*

P of

of Wine, we made some weak Beverage, which (by reason that the Wine by being frozen, had lost his virtue) was little better then water.

23. And I remember that a learned Man, whom I ask'd some questions concerning this matter, told me, that in a Northern Countrey, less colder then *Muscovy*, he had observed, that Beef having been very long frozen, when it came afterwards to be eaten, was almost insipid, and being boil'd afforded a Broth little better then common water.

24. If I had not wanted opportunity, I should here subjoyn an Account of some Trials, for which I made provision, as thinking them not absolutely unworthy the making, though extravagant enough not to be likely to succeed. For I had a mind to try, not only whether some plants, and other Medicinal things, whose specifick virtues I was acquainted with, would lose their peculiar Qualities by being thoroughly congeal'd, and (several ways) thaw'd; and whether thaw'd Harts-horn, of which the Quantity of Salt and Saline spirit

spirit of such a determinate strength should beforehand be tri'd by distillation, would, after having been long congeal'd, yield by the same way of distillation the same Quantity of those actual substances, as if the Harts-horn had not been frozen at all. But I had also thoughts to try, whether the Electrical faculty of Amber, (both the Natural, and that factitious imitation of it I elsewhere teach) and whether the attractive or directive Virtue of Loadstones, especially very weak ones, would be either impair'd, or any ways alter'd by being very long exposed to the intensest degrees of Cold within my power of producing. But to have nam'd such extravagancies, is that, which I think enough, and others I fear may think too much.

25. Yet some few things I shall subjoyn on this occasion, because it will add somewhat not impertinent to the Design of this Treatise (which is to deliver the *Phænomena* of Cold) as well as countenance what I have been proposing ; and those things are, That I can by very credible Testimo-

ny make it appear, that an intense Cold may have a greater operation upon the Texture even of solid and durable Bodies, then we in this temperate Climate are commonly aware of. I shall not urge, that even here in *England* 'tis generally believ'd, that Mens Bones are more apt to break upon falls in Frosty, then in other Weather, because that may possibly be imputed to the hardness of the frozen Ground. Nor, that I remember when I was wont to make use of Stone-Bows, I found it a common observation, that in Frosty Weather the Laths, though of Steel, would, by the Cold, be made so Brittle, that unless extraordinary care were had of them, or some Expedients were us'd about them, they would be apt to break. Nor yet, that an Ingenious Overseer of great Buildings has informed me, that those that deal in Timber and other Wood, find it much more easie to be cleft in hard Frosts, then in Ordinary Weather. These and the like instances I do, as I was intimating, forbear to urge, because these effects of Cold

Cold are much inferior to those that have been met with in more intemperate Regions.

26. And to begin with its Operation upon what we were last treating of, *wood*. Of *Charleton-Island* Captain *James* has this passage about the Timber, they imploy'd upon their work, *The Boys* (says he) *with Cuttle*. Pag. 67. *axes must cut Boughs for the Carpenter; for every piece of Timber, that he did work, must first be thaw'd in the fire.* And a little before, he tells us, that even when they found a *standing Tree*, *They must make a fire to it to thaw it, otherwise it could not be cut.*

27. And I remember, that two several persons, both of them Scholars, and strangers to one another, that had occasion to travel as far as *Mosco*, assur'd me, that they Divers times observ'd in extreme frosts, that the Timber-work (whether the Boards or the Beams) of some Houses, which, according to the Custom of that Countrey, were made of wood, and perhaps not well seasoned, would, by the operation of the Cold, be made to crack in divers

places, with a Noise, which was surprizing enough to them, especially in the Night.

28. I remember also, that a Physician, who liv'd for some years in one of the Coldest Plantations of the *West Indies*, related to me, that he had observ'd the Bricks, he had imploy'd about Building, to be very apt to be spoil'd by the long and vehement frosts of the Winters there; where he likewise said, that 'twas a usual thing for the Houses builded of Brick, to decay in fewer years by far, then here in *England*, which he said was generally, and, as he thought, truly imputed to the excessive Cold, which made the Bricks apt to crumble, and moulder away. But though I dare not lay much weight on this Observation, unless I knew, whether the Bricks were sufficiently burn'd, and free from pibbles, calcinable by the heat that burn'd the Bricks: yet we must not deny, that extreme Colds may be able to shatter or dissolve the Texture of as close and solid Bodies as Bricks, especially if the Aqueous Moisture be not sufficiently

ciently driven away, if we will admit, what I remember I have mention'd in another Treatise, out of a very Learned and credible Author, of the power, that a freezing Degree of Cold has had to break even solid Marble. And much less shall we doubt the possibility of what the Physician related, if we will not reject the Testimony of the Learned *Olaus Wormius*, according to which, Instruments made even of so hard a Metal as Brass, are not priviledg'd from the Destructive Operations of some Degrees of Cold. For, *Ex ære facta opera* (says he in his *Curious Museum*) *Lib. 1.*
vi frigoris quandoque rumpuntur, quod Sect. 3:
tamen pauci credunt, id tamen expertus Cap. 5.
est Eratostenes, & Nostras Johannis pag. 122.
Munckius in difficillimo suo Itinere, quo per fretum Christianum transitum in mare Australe invenire moliebatur. To which, perhaps most Writers, would, if they met with it, add this passage out of the Dutch-mens Voyage to *Nova Zembla*. The 20. (of October) it was calm Sunshiny weather, and then again we saw the Sea open, at which time we went on Board, to fetch the

rest of our Beer out of the ship, where we found some of the Barrels frozen in pieces, and the Iron Hoops that were upon the Fosam Barrels, were also frozen in pieces. But though this Testimony seems to prove, that extreme Cold may break even Iron itself, and though possibly such an Affirmation might in the general not be erroneous, yet I shall forbear to draw that inference from this passage, because I suspect, that since the Irons, that were broken, were Hoops, and since it seems probable by the story, that there were Barrels not Hoop'd with Iron, broken also by the same Frost; the breaking of the Hoops may have been the effect, not of the violence of the Cold, as acting immediately upon the Iron, but of the Liquor in the vessels, which being by the Cold that froze it, turn'd into ice, was so forcibly expanded, as to burst, what ever oppos'd its dilation, according to what we shall have occasion in its due place more fully to deliver.

An Appendix to the VI. Title.

INquiring of the formerly mention'd Physician to the *Russian* Emperor, what experience teaches about some of the matters treated of in this (sixth) Title, in those cold Climates, where the effects of freezing are more notable : He told me, that the tradition (mention'd above touching the safest way of thawing) is in *Muscovy* generally receiv'd, and that 'tis usual for Men, that have their Cheeks and Noses frozen, to rub them well with snow, and escape unharmed ; whereas if they go immediately into their Stoves, they often lose the Tops of their Noses, and introduce into their Cheeks a kind of paralytick Distemper, or benumbedness, that they cannot get rid of in many Moneths.

And having also inquir'd of the same Ingenious person, whether Wine frozen, and then permitted to
thaw,

thaw, till the unfrozen Liquor had quite resolved the ice, was not thereby spoil'd by having its Texture vitiated, he answered, that in very strong Claret-wine he found the Colour scarce at all destroy'd, nor the Liquor otherwise much impair'd; but that in weaker Claret-wine the Colour was spoil'd, and the Liquor was otherwise much the worse. But note, that in the French-wine there remain'd a third part or more unfrozen, so that it seems not to have been expos'd to near so extreme a cold, as that of the *Hollanders*, or of *Captain James*; and that Physician likewise told me, that of some very strong Beer, that he had in great part frozen, the ice had some Taste of the Hops, but was dispirited like phlegm.

Having inquir'd how long dead Bodies would keep, he told me, that if they were thoroughly frozen, they would be preserv'd incorrupted till the thaw, though that perhaps might not happen within four or five Moneths after the Death of the Man. He added, that he had the Venison of Elkes sent him unsalted, and yet untainted

untainted, out of *Siberia* (which is some hundreds of leagues distant from *Mosco*) and that Beef and other flesh well frozen, would keep unpurified for a very long time ; and when I ask'd whether the freezing did not impair it, he answered, that with long keeping it congeal'd, it will grow very dry and be impair'd in Taste, and will not make so good Broth as meat that was never frozen. And he further told me, that in case frozen meat were leisurely thaw'd, it would be far the less impair'd, and might be well roasted, but if before it was thaw'd, it were laid down to the Fire, it would not ever be well roasted, and would eat very scurvily ; and though a shoulder of Mutton, for instance, were kept very many hours turning before the fire, yet it would continue raw in the middle.

Having inquir'd about the rubbing Bodies with Snow to unfreez them, he told me (agreeable to what I noted him to have said above) that he had seen several persons, that had been frozen, & that when a man is told, that he is frozen, and having ask'd where-
abouts

abouts (for the party himself usually knows it not) is inform'd, that it is in this, or that place, which is commonly the Nose or the upper part of the Cheek, or perhaps the Tip of the Ear, he usually rubs the part very well with Snow, and lets it thaw by degrees, else, if without that preparation he should go immediately into the Stove, he would be in danger to lose his Nose, or other frozen part. The Doctor added, that they use to rub the frozen Meat and Fish with Snow, and that he once examin'd a Man, who in his youth had been frozen all over, and inform'd the Doctor, that having had occasion in a journey to quit his Sled for a while, and do some Exercise, that had almost made him sweat, being careless of himself when he return'd to the Sled again, he was frozen all over, and had so died, had not the Company by Accident taken Notice of him, and by rubbing him over with Snow, and by the use of the like means recover'd him again; but he told the Doctor, that by this whole Accident he was put to no pain, save that

that when he came to himself again, he felt such a pricking all his Body over, as men are wont to find in an Arm or Leg benumm'd by having been long lean'd upon.

When I ask'd whether the sharpness of the Cold, did not work upon the stones, he answer'd, That as to Flints he could not tell, but as to other stones, and such as are oftentimes us'd for Building, the violence of the Cold made them frequently moulder into Dust. And to satisfy my Curiosity about the Effect of Cold upon Wood, he told me, that he had very often in the night, especially when their keen frosts were unaccompani'd with Snow, heard the Trees cleave and crack with very great, and sometimes frightful noises, and that the outside of the Fir Trees, that were laid upon one another in their Buildings, and was expos'd to the Air, would do the like, and that he had often seen the gaping Clefts sometimes wide enough to put in his fingers, which would remain in the Trees, and in the Fir-wood, till the thaw, after which they would pretty well close of themselves.



Title VII.

Experiments touching the Expansion of Water, and Aqueous Liquors by Freezing.

1. **T**hat water and other Liquors are condensed by Cold, and so much the more condensed, by how much the greater the degree of Cold is that condenses them, has been for many ages generally taught by the Schools, and taken for granted among men, till of late some more speculative than the rest, have called it in question upon the account of the levity of Ice, since which I have met with two modern writers, that have incidentally endeavoured to prove, *that Ice is water, not condensed, but rarified by the intumescence of water exposed to freezing in vessels fitly shap'd.*

These

These Attempts of these learned Men putting me in mind of what I had tried to this purpose, when I was scarce more then a Boy, invited me to consider, that by the usual ways of Glaciation, such as these ingenious Men employ'd, the Experiment is wont to meet with a Disaster, by the breaking of the Glasses, which not only makes the Event liable to some objections of theirs, that befriend the common Opinion, but (which is more considerable) hinders them from judging what this Expansion of water, that is made by freezing may amount to : wherefore we will now set down what we have done to ascertain (and yet limit) the Experiment, as also to advance it further.

2. Whereas then these two *Nicholaus Zucchini, & Melchior Corna-* learned Men, we have been mentioning, do so expose the water to freez, that it is turn'd into Ice at the top as soon as elsewhere ; the inconveniences of which way we have already noted, we, by freezing the water, as we have formerly taught, from the bottom upwards, can easily preserve our Glasses entire, and yet turn the whole

whole contained water into Ice ; so that if according to this way You so place a Bolthead or a Glass:egg, in whose Cavity the water ascends to the height of an inch, or thereabouts, within the stem or shank, in a mixture of Ice, or snow and salt, as that the water is first turned into ice at the bottom and sides, and not till the very last at the top, you shall manifestly see, that the ice will reach a good way higher in the neck, then the fluid water did, and that upon a gentle thaw of the ice, the water, it returns to, will rest at the same height in the stem, to which it reached, before it was exposed to be frozen.

3. We have likewise used other ways unspoken of by the lately mentioned writers, to evince, that water is expanded by being frozen ; as first, that we took a strong earthen vessel of a Cylindrical form, and filling it with water to a certain height, we exposed it unstopped, both to the open Air in frosty nights, and to the operation of snow and salt, and found, that the ice did manifestly reach higher then the water did, before

fore it was congealed.

Besides, if a hollow Pipe or Cylinder made of some compact matter, be stopped at one end with wax, or some things else, which it may be more easie to drive out, then to burst the Cylinder, and if at the other end it be filled with water, and that orifice also be stopped after the same manner, this Pipe suspended in a sufficiently cold Air, will have the included water frozen, and by that change, if the Experiment have been rightly made, the water will upon congelation take up so much more room then it did before, that the above mentioned stoppels, or at least one of them will be thrust out, and there will be produced a rod of Ice a good deal longer then the pipe, at each of whose ends (or at least at one of them) a Cylindrical piece of Ice of a pretty length may be broken off, without meddling with the Pipe, or the ice that fills it.

Divers other ways of proving the same Truth might be here alledged, but that, though these were not, as they are, sufficient, the matter would yet

Q

be

be abundantly confirm'd by divers of the Experiments, that will here and there come in more opportunely in the following part of this Treatise.

4. But here it will not be altogether impertinent or unseasonable, to take notice, that not only those School Philosophers, who have considered the breaking of well stoppt Glasses in frosty weather, (an accident but too frequent in Apothecaries Shops, and Laboratories) but divers modern *Virtuosi*, are wont to ascribe the *Phænomenon* to this, that the Cold of the external Air, contracting the Air and Liquor within, the Ambient Air must break the sides of the Glals to fill that space, which being deserted upon the condensation of the included Air, the liquor would otherwise leave a *vacuum* abhorr'd by nature; and even those few Moderns, that are loath to ascribe this *Phænomenon* to Natures abhorrency of a *vacuum*, either not being acquainted with the weight of the Air, know not, what probable account to give of it, or if they acknowledge that weight, are wont to ascribe

ascribe it to that, and to the great contraction of the internal Air, made by the Cold of the External.

5. But as for the Peripateticks, the above mentioned Experiments sufficiently evince, that in many cases, 'tis not the shrinking, but the Expansion of the liquors contained in the stopt vessels, that occasions their bursting, and therefore in these cases, we need not, nor cannot fly to I know not what *fuga vacui* for an account of the *Phænomenon*; and whereas as it may be objected, that even glasses not half full of distill'd waters, if they be exactly stopt, are often broken by the frost in Apothecaries shops: I answer, That neither in this case do I see any need of having any recourse, either to the *fuga vacui*, or to the weight of the external Air, for even here the Expansion of the freezing liquor may serve the turn, for in such inartificial glaciations the liquor begins to freeze at the top, and the ice there generated, fastning it self (as on other occasions we declare) very strongly to the sides of the Glass, contiguous to its edg, as

the liquor freezes deeper and deeper, this crust of Ice increases in thickness and strength, so that the water is included, as in a vessel Hermetically sealed betwixt this Ice at the upper part, and the sides and bottom of the Glass every where else, and consequently, the remaining water being uncapable of Congelation without Expansion, when the ice is grown strong enough at the top to make it easier for the expansive endeavour of the freezing water to crack the sides or bottom of the Glass, then to force up that thick cake of Ice, the vessel will be broken, how much soever there be of it empty above the surface of the Ice. And this Conjecture may be confirmed by these two Particulars, the one, That when water is frozen in a broad vessel, which is too strong to be broken or stretch'd by the frost, the surface of the ice contiguous to the Air will be convex or protuberant, because that though the glaciation began at the top, the thickness and Compactness of the vessel makes it easier for the expansive endeavour to thrust up that cake of ice in those parts

parts of it, that are the remoter from the sides, whereunto they are strongly fastned, then to break so solid a vessel.

6. The other Particular is afforded us by that Experiment of ours (mention'd in the Vth. Title foregoing) wherein if a vessel half full of water be made to freez, not first at the top, but at the bottom, that liquor may be turned into ice without danger to the glass. But we will now add an Experiment, on whose occasion we have set down these Considerations. For being inclined to think, that the spring of the Air, shut up in a vessel stopped, will preserve it expanded, or at least keep it from considerably shrinking, notwithstanding a very great degree of Cold, in case the vessel be strong and close enough to fence it from the pressure of the external Air, we conjectured that the bare weight of the outward Air added to the Refrigeration of the included Air, would not be sufficient to break much weaker glasses, then those we have been speaking of. And therefore partly to satisfy some in-

genious Men, that this Conjecture made me dissent from, and partly to show the Peripateticks, and those that adhere to them in the question under consideration, that either the Cold alone cannot always, as they teach us, contract the Air, or that if it do, the breaking of well stopp'd glasses in frosty weather is much fitter to evince, that there may be a *vacuum*, then that there can be none, we made the following Experiment.

7. We took three glals-bubbles of differing shapes and sizes, which we caused to be blown with a Lamp, that, to make the Experiment very favourable for our Adversaries, we might have them much thinner, and consequently, weaker then those glasses that are wont to be made use of to keep liquors in, and which notwithstanding are wont to be broken, though they be not full by the frost.

These Bubbles, when the Air was at a convenient temper within, were (as easily they might be) nimbly seal'd up with care, to avoid the heating of the Air in them, and being afterwards expos'd sometimes to the Air it self

self in very frosty weather, and sometimes to that greater Cold, which is produced by the placing them in a mixture of snow and salt, we could not nevertheless find, that any one of the three was at all broken or cracked, so that in case the included Air were condensed into a lesser room, the space it deserted may be concluded empty, or else it will hardly appear, what necessity there can be, that Nature should break, as the Peripateticks pretend, very much stronger glasses in Apothecaries shops, to prevent a *vacuum*.

8. Having shown, that water it self, acquires a considerable Expansion by Cold, we will next shew, that Aqueous Bodies, or those that abound with waterish parts, do divers, if not all of them, the like.

We took Eggs, and exposing them to a sufficient Degree of Cold, we observ'd, that when the contain'd liquors were turn'd into Ice, they burst the shells asunder, so that divers gaping Cracks were to be seen in them, as long as they continu'd frozen.

9. Milk, Urine, Rhenish-wine, and good spirit of Wine, being set to freez in distinct glass Eggs, neither of the three former liquors was observ'd to subside before it began to rise. The Event in sum was, that the Urine was much longer, then either of the two other liquors, before it began to swell, but rose to a far greater height, then they, afterwards. The Wine did not leave the mark above an inch beneath. The Milk ascended about two inches, and the Urine by guess six or seven.

10. A strong solution of *Dantzick* Vitriol, being put into a Cylindrical Pipe, seal'd at one end so, that the liquor fill'd the Pipe to the height of about six or eight inches, being frozen with snow and salt, the congeal'd liquor grew very opacous, and look'd as if it had been turn'd or shot into Vitriol, save a little that remain'd fluid, and transparent near the bottom. And this Ice as appeared, rose considerably higher then the liquor did before Congelation.

It were perhaps worth trying, whether or no even several Bodies of

a stable consistence, and durable Texture, might not be found to receive some, though less manifest Dilatation by excessive Cold. And methinks those, who attribute Glaciation to the plentiful Ingress of frigorifick Atoms into Bodies, should by their *Hypothesis* have been invited to make some Trials of this kind, since we see that the invisible Moisture of the Air against rainy weather, does seem manifestly enough to alter the Dimensions of doors, window-shuts, and other such works made of wood not well season'd. And even without supposing the truth of the *Epicurean Hypothesis*, if we consider, that in Bread, though we are sure, that much more water was added to the Meal, or Flower, than was exhal'd in the Oven, yet there appears not the least drop of water distinct in the Concrete, and that Harts-horn, Sponges, and many other Bodies, that seem very dry, will afford by distillation good store of phlegm or water, and more then can probably be ascrib'd to any transmuting Operation of the Fire: If, I say, we consider

der these and the like things, it may seem worth while to try (which I want the conveniency to do) by accurate measures, whether the invifible and interspers'd water, its comminution notwithstanding, will not upon freezing swell the Body that harbours it. And I would the more gladly have been satisfi'd in this, because I hop'd it might help me to unriddle a strange *Phænomenon*, afforded us by the Narrative of the Dutchmens Voyage to *Nova Zembla*, wherein they relate, *That the Cold was so great, that their Clock was frozen, and would not go, though they hung more weight upon it then before*: So that they were fain to measure their Time by hour-glasses. For though this odd Effect might be suspected to proceed from some little Ificles sticking to some of the Wheels, or the Line, in regard they not far off tell us, that the steams of their Bodies, and other things within their close house, did

*It froze so
fore with-
in the
house, that*

so fasten themselves to the walls, to the Roof, and even to their Cabins, the Walls & the Roof thereof were frozen two fingers thick with Ice, and also in our Cabins, where we lay all those three days, while we could not go out. Gerat de Veer in his third Voyage.

as to line them with Ice, of no less
 then two fingers thick; yet besides,
 that it cannot be probably suppos'd,
 that they, who had so great need of
 their Clock, during the tedious ab-
 sence of the Sun for many weeks toge-
 ther, should not all the Winter long
 be aware of this. Besides this, I say,
 I find that in Captain James's winter-^{Page 64.}
 ing at Charleton, his Clock and
 Watch were so frozen too, *That they*
could not go, notwithstanding they were
still kept by the fire side in a Chest, wrapt
in clothes. So that in case it appear,
 that according to what we formerly
 noted out of *Wormius*, the frost can
 get into Metals, it can also distend
 them, and other stable Bodies: We
 might conceive, that the stopping of
 the Clocks might proceed from the
 stiffness, or the swelling of the line,
 to which the weight was fastned, or
 a swelling even of some of the wheels,
 or other Metalline parts of the Clock,
 that may spoil the necessary congrui-
 ty between the Teeth, &c. as I have
 tri'd, that some parts of an Iron In-
 strument, I caus'd to be made, would
 by no means fit one within another,
 when

when expanded by much Heat, (and though Cold be the cause of the expansion, the Effect may be the same) though at other times they would. And if we knew whether Springs lose any thing of their Elasticity by the violence of the Cold, we might thence also be assisted to guess, whether the frosts Operation upon the Spring of Captain James's Watch (for he mentions that, as distinct from his Clock) might contribute any thing to the forcing it to stand still. But these are bare Conjectures, from which I will therefore pass on to the following Section.



Title VIII.

Experiments touching the Contraction of Liquors by Cold.

1. **B**UT notwithstanding all the former Experiments, we must not conclude universally, that all liquors are dispos'd to be expanded by Cold, neither by a moderate degree, nor even by so intense a degree of it as suffices to freez or congeal the liquors expos'd to it; this we have tri'd, not only in spirit of Wine, *Aqua fortis*, Oyl of Turpentine, and divers other liquors, that we could not bring to freez, but also in oyl congeal'd by the Vehemence of Cold, so that as to the change of Dimensions produc'd in Liquors by Cold, there must be a great difference allowed betwixt water and aqueous liquors on the one side, and oyl and divers other liquors, that are some of them of an
oleaginous,

oleaginous, and some of a very spirituous, or a very highly corrosive nature, on the other side. Nor have we yet made trials enough to reduce this matter to a certainty. For though we could not bring some strong Saline spirits, nor the most of Chymical oyls to freez, yet in some our Attempts succeeded not ill. But I remember not, that in any liquor we could by Cold produce any sensible expansion, but rather a manifest Condensation, unless we could bring it actually to freez.

2. The trials we made of the Efficacy of Cold to condense liquors, were many, but it may, for the present, suffice to set down two or three differing ones, that occur to us in our Collections.

To the entry of the Experiment, lately recited, of the expansion of Milk, Urine, and the Rhenish Wine, there are subjoyned these words.

[But the Egg that held the spirit of Wine, though it were much smaller then we usually employ, and fitted with a proportionably slender stem, and though it were kept divers hours

hours partly in Ice, and Salt, and partly in Snow and Salt, yet it froze not at all, but subsided by degrees below the first mark to the quantity of $\frac{3}{4}$ of an inch in the stem; and though it afterwards seemed to rise a little, yet it never swelled up again to the said first mark.]

3. [We took a round Bolt-head of about in Diameter, and poured in *Mercury* till it reached a pretty way into the neck, which was purposely drawn more slender then ordinary, and having, without approaching it to the fire, freed it from some of the larger bubbles of Air, that appeared at the sides, we put it into a mixture of Ice and Salt, where the Cold so wrought upon it, that watching it attentively, we could discern not only its having moved, but its motion, downwards, which it continued (though not visibly in the progress, as at the first) till it was subsided in the neck two inches or better, which was far more then could be attributed to the contraction of any sensible Aerial Particles, though they had lost not only the 30. part of their
Dimensi-

Dimensions, as we have sometimes observed, of the Air, but had been contracted to a point; and we observed too, that the Quicksilver once thus intrigidated, though not frozen, retained some of the acquired Cold, for many hours after, as appeared by its keeping below the mark of its first height, though we had kept it all night in a warm room.]

4. [We took a small Egg with a proportionably slender stem, into which we poured common oyl, till it rose a pretty way (but not much) above the oval part of the glass, then having put a mark upon the station of the liquor, we placed the vessel in snow and salt, and observed it not to swell as other liquors, but to subside, with Cold, till being quite frozen or congeal'd, it appeared to be shrunk about an inch or more beneath the mark, then being thaw'd, it swelled again to the mark.]

5. The Experiment was repeated the second time, with not much worse success, but we found, that if the glass were removed out of the snow into some place near the fire, the

the hot Air would not only thaw it, but so rarifie it, as to make it ascend above the mark. A third time we seal'd up the same oyl in the same glass, and repeated the Experiment with like success to that, we had the second time, and that the frozen oyl was really condensed, we found, because it would sink in oyl of the same kind cold, but unfrozen; and this, notwithstanding divers bubbles, which we observed usually to be made about each lump of congeal'd oyl, that we cast in, upon its beginning to sink in the fluid oyl. This we tri'd, both with oyl well congeal'd (or if another word please better, Incrassated or Curled) by snow and salt, and with oyl less congeal'd, frozen by the bare cold of the Ambient Air; but this latter seemed to fight to sink more slowly then the other, as being less congealed and ponderous, yet would not lumps of the mass of oyl sink or continue immersed. I say not in common water, but in Sack or Claret-wine, and if thrust down into either of these liquors, they nimbly enough emerged.

R

6. Whether

6. Whether or no Chymical oyls, though, like expressed oyls, they shrink with a moderate degree of Cold, would by congelation be, like them, contracted, or like Aqueous liquors expanded, we could not satisfy our selves by Experiment, because we were unable to advance Cold to a degree capable of bringing such oyls to congelation, only we had thoughts to make a trial with oyl of Aniseeds, distilled with water in a Limbeck, in regard, that though it be a very subtile liquor, and as Chymists call it, an *Essential* oyl, and though in the Summer time, and at some other seasons (if the weather be warm) it will remain fluid, yet in the Winter, when the Air is cold, it will, if it be well drawn, and genuine, easily enough lose its fluidity, and therefore we thought it might do well to pour some of it in moderate weather, into a conveniently shap'd glass, and then to freez it externally by the application of Ice and Salt, that we might observe, whether upon congelation it would shrink or be expanded. And accordingly, though

we

we were not provided with any Quantity of this oyl, yet in weather that was not sharp, we did by the help of some Ice which we procur'd, when the season made it a Rarity, surround a glass pipe fill'd with fluid oyl of Aniseeds, and found, though the Pipe were but short, yet the inclosed substance, when it had lost its fluidity, had considerably lost of the height which it reached to before.

7. And because the *Empyreumatical* oyls, that are driven out of Retorts by somewhat violent fires, seem'd to be of a nature differing enough from those *Essential* oyls (as Artists call them) which are drawn in Limbecks by the help of water, as well as fire: And because we observ'd, that some of the firmer oyls may be us'd in Physick in much larger Doses, then 'tis thought safe to give the latter in: Conjecturing from hence, that probably *Empyreumatical* oyls may be less hot, and so less indispos'd to Congelation, we thought fit to make trial (no body else in probability having done it) whether the Cold in our Climate

R 2 could

Unable to display this page



Title IX.

*Experiments in Consort, Touch-
ing the Bubbles from which
the Levity of Ice is suppo-
sed to proceed.*

1. **S**INCE the first thing that made the
Moderns suspect, that water is
expanded by freezing, is the floating
of Ice upon water, it will not be
amiss for confirmation of that Argu-
ment, to take some notice of the levi-
ty of Ice in respect of water; This is
best observed in great Quantities of
Ice, for whereas in small fragments
or plates, the Ice, though it sink not
to the bottom of the water, will of-
tentimes sink so low in it, as scarce
to leave any part evidently extant
above the surface of the water, in
vast quantities of Ice, that extancy
is sometimes so conspicuous, that

Navigators in their Voyages to *Island*, *Greenland*, and other frozen Regions, complain of meeting with lumps, or rather floating rocks of Ice, as high as their main Masts. And if we should meet with Cales, wherein we might safely suppose the Ice to be as solid as entire pieces of Ice are wont to be with us, and not to be made up of icy fragments cemented together, with the interception of considerable Cavities filled with Air, it would not be difficult for any that understands *Hydrostaticks* to give a pretty near guess at the height of the Extant part, by the help of what we lately observ'd of the Measures of water's Expansion, and by the knowledge of the immersed part; which, supposing that the Ice were of a prismatical figure, and floated in an erected posture, would in fresh water amount to about eight or nine times the length of the part of the *Prisme* superior to the surface of the water.

2. But because perhaps the great disparity in the degrees of Cold, whereby water is in this, and in those
gelid

gelid Climates turn'd into Ice, may breed a difference in the expansion of the frozen water, and because some other circumstances may be needful to be taken into consideration, about the height of floating Ice above water, and these will be more properly taken notice of under the following Title, I shall only upon this head (*of the Levity of Ice*) subjoyn the ensuing transcript of one of our notes concerning That subject.

[We found, that pieces of Ice, clear and free, for ought the Eye could take notice of, from bubbles, would not be made to sink in spirit of Wine once distilled from Brandy, and it floated likewise in strong spirit of Wine drawn from quick Lime; but if the spirit of Wine were well warmed, such Ice, as I mentioned, would sink in it, though as it grew cold the same Ice would slowly ascend, and sometimes remain for a while, as if it were suspended without sensibly rising or falling. But all this while the Ice, thawed apace in the water whereinto it was dissolved, did manifestly seem to run down like

a stream through the lighter body of the Spirit of Wine, the Diversity of the Refractions making this easie to be taken notice of ; yet common water, though heated as hot as I could indure to hold the glass in my hand, would not let the fragments of the same parcel of Ice sink into it : but in oyl of Turpentine, and in thrice Rectifi'd Spirit of Wine, the Ice would sink like a stone.]

3. That the levity of Ice in respect of water proceeds from the bubbles that are produc'd in it, and make the water, when congeal'd, take up more room then when fluid, has scarce been doubted by any, that has consider'd the Texture of Ice, as well as taken notice of its levity. But if this be the true and only reason, we may conjecture, that there must be great store of bubbles in Ice, extremely minute, and undiscern'd by the naked Eye. For though in very many parcels of Ice, the bubbles are as well conspicuous as numerous, in-
somuch that they render the Ice whitish and opacous, yet we have observed, that other pieces would swim,
which

which yet were of an almost crystalline clearness. And therefore we thought fit to look upon some clear pieces of Ice in a Microscope, and we shall subjoyn the Event, because that when we beheld some of this ice in one of our Microscopes, which has been counted by several of the curious, as good a Magnifier, as perhaps any is in the world, we could not discover such store of bubbles, as it seemed there should appear upon the supposition, that the adequate cause of the levity and expansion of frozen water is but the interspersion of such bubbles.

The Observations I have been mentioning, I find thus set down among my Notes.

[A piece of Ice, that to the Eye look'd clear like crystal, being put into the great Microscope, appear'd even there free from bubbles, and yet the same piece of Ice being presently remov'd, and cast into common water, would swim at the top, and if it were forcibly duck'd, would swiftly enough emerge.

Another piece of Ice, that to the
naked

naked Eye was not so clear as the former, appear'd in the same Microscope to have store of bubbles, some of them appearing there no bigger then a small pins head, and some of them being yet lesser, and scarcely visible in the Microscope it self.]

And here, because it seems a considerable doubt, and well worth the examining, whether or no water, when frozen into Ice, grows heavier or lighter, not in reference to such water as it was generated of (since it is evident, that upon that it will float) but more absolutely speaking, we judg'd it not amiss to examine this matter by an Experiment, but we could not discover any difference between the weight of the same parcel of water fluid and frozen, as will appear by the ninth *Paragraph* of the Experiment to be a little beneath recited.

But since that, whether or no we allow any other cause, together with the bubbles, to the levity of Ice, it seems a thing not to be doubted, that its expansion and lightness is mainly, if not only, due to the interspersion of bubbles,

bubbles, the generation of them seems to be one of the considerablest *Phænomena* of Cold, and the Investigating by what cause those cavities are produced, and in case they be perfectly full, what substance 'tis that fills them, is none of the meanest enquiries, that should exercise the industry of a searcher into the Nature of Cold.

4. Mr. *Hobs*, and some others seem to think, that the expansion of water by congelation, is caus'd by the Intrusion of Air, which constitutes those numerous bubbles wont to be observ'd in Ice; we might here demand, why in case that upon freezing there must be a considerable accession of Air from without, when oyl is frozen, it is, notwithstanding the ingress of this Air, not expanded, but condens'd; but because these conjecturers do not allow glafs to be pervious to common Air, we shall at present press them with this Experiment, which we have divers times made.

We took a glafs Egg with a long stem, and filling it almost with water, we seal'd it *Hermetically* up to
exclude

exclude the pretence that some adventitious Air might get in, and insinuate it self into the water, and yet such an Egg being exposed to congelation, the frozen water would be manifestly expanded, and swell'd by numerous bubbles, which oftentimes gave it a whitish opacity.

To which we may add, that new metalline vessels being fill'd with water, and carefully stopp'd, the liquor would nevertheless, when exposed to the Cold, be thereby expanded, and turned into Ice furnished with bubbles.

5. If it be objected, that in the Experiment of the *Hermetically* seal'd glass, the produced bubbles might come from the Air, which being seal'd up together with the water, might by the expansion of that water be brought to mingle with it: I answer, that this is very improbable. For 1. if the bubbles must cause the expansion of the water, how shall the water be at first expanded to reduce the Air to a Division into bubbles. Next, 'tis evident by the Experiments we shall ere long relate, that the

the Air as to the Body of it, retains its station above the water, and preserves it self together in one parcel, since it suffers a compression, that oftentimes makes it break the glaſs that imprisons it, which it would not need to do, in case it dispers'd it self into the Body of the water; for then there would appear no cause, why the Air and water should after congelation require more room then they did before. 3. In this Experiment we usually begin to produce Ice and bubbles in the water, contiguous to the bottom of the vessel (that part being by the snow and salt first refrigerated) in which case there appears no reason, why the Air, which is a *thousand times* lighter then the water, should against its nature dive to the bottom of the water, and if it were disposed to dive, why should we not see it break through the water in bubbles, as is usual in other cases, where Air penetrates water. 4. In metalline vessels, and in Glasses quite filled with water, before they are stopped, there is no pretence of the diving of the Air from the top, there

there having been none left there.
 5. and lastly, If all the bubbles of Ice were made *by*, and filled with true Air descending from the upper parts of the vessels, and only dispersed through the water, then, upon the thawing of this Ice, the Air would emerge, and we might recover as much of real Air as would fill the space acquired by the water upon the account of its being turned into Ice, which is contrary to our Experience. And this Argument may also be urged against any that should pretend, (for I expect not to see him prove it) that though Air, as numerous experiments evince, cannot get out of a seal'd glass, yet it may, in such a case as this, get into it. But we find upon trials, that the Cavities of these bubbles are not any thing near filled with Air, if they have in them any more Air at all, then that little which is wont, as we have elsewhere shewn, to lurk in the particles of water, and other liquors. And the making good of this leads us to the second Enquiry, we were proposing about these bubbles, namely, whether or
 no

no their cavities be fill'd, and fill'd with Air.

6. The full resolution of this whole Difficulty would be no easie Matter, nor well to be dispatched with so much brevity as my occasions exact. For it would require satisfactory Answers, to more then one or two Questions, since, for ought I know, it may lead us to the debate of those two grand Queries, whether or no Nature admit a *Vacuum*, and whether a great part of the Universe consist of a certain Ethereal matter, subtile enough to pass through the pores, not only of liquors, but of compact bodies, and even of glass it self: we should also be obliged to enquire, whether or no Air, I mean true and permanent Air, can be generated anew, as well out of common water, as many other liquors, and whether it may be generated by Cold it self, and perhaps we should be oblig'd to inquire into the *Modus* of this production, and engage our selves in divers other difficulties, whose full Prosecution, besides that they would as much exceed our present

sent leisure, as Abilities, seems more properly to belong to the more general part of Physicks, where such kind of general Questions are fittest to be handled.

Wherefore we will now only consider this Particular Question, whether or no the Cavities of the Bubbles wont to abound in Ice, be *filled* with common Air; and even this question, though it seem but one, comprizes two: for to resolve it, we must determine, whether there be any *true Air* contained in those Cavities, and whether in case there be, they be adequately filled with that Air, (by *true Air* I mean such an invisible fluid, as does permanently retain a spring like the common Air.)

7. The former of these two Questions, I must confess my self not yet resolved about, my Experiments having not hitherto succeeded uniformly enough to satisfy so jealous an observer. But yet I shall annex our trials, *not only* because the thing has not been, that we know of, so much as attempted by others, and our ways of Experimenting, if they be duly prosecuted,

cuted, seem as promising and hopeful (if the Question be reducible to any certain Decision) as perhaps will be easily lighted on ; but because *also* we have, if we mistake not, resolved the second Question, by shewing that there is but a small part of true Air contained in the Bubbles of Ice, whatever Ingenious men, that rely upon probable Conjectures without consulting Experience, have been pleas'd to believe to the contrary.

That the bubbles observed in Ice cannot all be filled with the Aerial particles lurking in the water, seems evident enough by the expansion of the water, and the Quantity of space taken up by those bubbles, which how the interspers'd, and formerly latent Air can adequately *fill*, unless the same parcel of Matter could truly *fill* much more space at one time than at another (which I take to be physically impossible) I do not yet apprehend.

But two ways of trial there are, which we imployed to shew, that the Icy bubbles are nothing near *filled with true Air*, whether Men will have

S

that

that pre-existent in the water, or stolen in from without, or generated anew; the former of the two ways of trials probably arguing, that these bubbles proceed not *only* (for that they may proceed *partly* we do not at all deny) from the Air pre-existent in the water, and the latter concluding more generally, that but a small part of the icy bubbles are filled with genuine Air.

8. And 1. we were invited to conjecture, both, that sometimes, or in some cases, the Air latitant in the water might contribute to generate icy bubbles, though it was unable adequately to fill them; and again, that sometimes or in other cases such bubbles would be almost as numerously generated, notwithstanding the recess of far the greatest part of that latitant Air, by the three following Experiments taken *verbatim* out of our Collections.

I. We took fair water, and having kept it in the exhausted Receiver of our Pneumatical Engine for a good while, till we perceived it not to send up any more bubbles, we presently

sently transferred it into snow and salt, where it was long enough before it began to freez, and then we observed, that the water did not swell near so much as common water is wont to do, and the ice seemed to have few or no bubbles worth taking notice of: but when I afterwards placed it between my Eye and the vigorous flame of a Candle, I could perceive, that it was not quite destitute of bubbles, though they were extremely small, in comparison of those, that would probably have appeared in ordinary water.

Thus far the first Experiment; the second follows, which was made at another time.

II. The water that had been freed from the bubbles in the Receiver, though it afforded an ice, that seem'd to have smaller bubbles, yet this ice being thaw'd, part of the water was gently poured into a pipe of glass, wherein being frozen, it swell'd considerably enough above its first level, and besides burst the glass, being also very opacous by reason of the bubbles.

The third Experiment was more industriously prosecuted, as may appear by this ample Narrative of it, transcribed out of our Collections.

III. We took a small Egg with a pretty long neck, and pouring in water till it reach'd an inch within the stem, conveyed it into a long slender Cylindrical Receiver, provided on purpose to make trials with such tall glasses, the Air being by degrees drawn out of the bubbles appeared from time to time greater and greater, and when the Receiver was well exhausted, the water seemed to boil a longer time than one would have expected, and sometimes the bubbles ascended so fast and great, that we were in doubt, whether the water did not boil over the top of the Pipe: the exhausted Receiver was permitted to be so for a good while, till the water had discharged it self in bubbles of its Air, and then the glass Egg was removed into a vessel furnished with ice and salt, and there left ten or twelve hours, that all the water, save that in the neck, might be thoroughly frozen, and then we found
it

it to have risen a great way above its first height, and removing it into an Air temper'd like that wherein the first part of the Experiment was made, & having left it there in a quiet place for ten or twelve hours to thaw leisurely (lest too warm an Air, or too much stirring the glass might be an occasion of generating new bubbles,) in the exterior part of the ice near the glass, we saw pretty store of bubbles, but when that was thaw'd, the rest of the ice appeared of a peculiar and unusual texture, having no determinate bubbles, that I could easily distinguish, but seeming almost like a piece of frosted glass, where the Parts, that made the Asperity, were exceeding thick set, but this ice swam in the water, wherein to the rest had been dissolved before it was all thawed: when there yet remained a lump about the bigness of a small Walnut, we reconveyed it into the Receiver, to try whether upon the exuction of the Air, the ice would be presently melted, but the alteration produced, was so small, if any, that we durst not ground any

S 3 thing

thing upon it. The Receiver being exhausted, there did at length appear some bubbles in the water, but they were not numerous, and a hundred of them seem'd not to amount to one of those larger ones, the same water had yielded us the first time it was put in: in the ice also some small bubbles disclosed themselves, which we did not perceive there before, wherefore we took out the Egg, and found (the ice being now thaw'd) that the water was subsided to the mark we had made, before it was expos'd to congelation, if not some very little way beneath it: Then we went about to find the Proportion wherein this dispirited water was expanded by glaciation, but in pursuing this there happened a mischance to the glass, which kept the Experiment from being so accurate as we designed. And therefore, though it seemed to us, that it amounted to about the twelfth part, which is less then that of the undispirited water, yet we designed the repetition of the Experiment. Only in this we could not be mistaken, that the expansion was considerable, since the

the water rose three inches and a half in the stem, though the whole water in the Egg and stem too, weighed but two ounces and a half. If the vessel had not been unluckily cracked, we should have frozen the water once more, and then sealing up the glass Hermetically, and suffering the ice leisurely to thaw, should have inverted it, and broken it under water, and have proceeded with it as we had done with some other glasses in the formerly mentioned Experiments.

9. [A little glass Cylinder open only at one end, of a convenient length, was thrust into a deep and wide mouth'd-glass about half filled with a mixture of Ice and salt: but the Cylinder was neither so quite filled, that the water should run over, nor yet far short of being so; that, (for all the opacous mixture of Ice and Salt) we might guess at the freezing of that part of the water, that we could not see by the changes appearing in the other. Then conveying all into a Receiver, that we had in readiness beforehand, we quickly pumped out the Air, upon

wh^{ch} there came both from the upper & lower parts of the water, great store of Bubbles to the top, where most of them brake into the Receiver, having found upon trials purposely made, that the Engine had continued stanch all the while, and perceiving by the intumescence of the superior parts of the water, that the other were frozen, we let in the external Air, and having removed the Receiver, and taken out the mixture before the Ice was half melted, we found the water, as high as the mixture reached, to be turn'd into ice, which besides some large and conspicuous bubbles had small ones enough to render it opacous; and upon the account of this expansion it was, that the water did in the free Air continue a good deal higher then the mark, it was but level with, when the Cylinder was exposed to freez.]

10. The other way we employ'd to examine what was contained in icy bubbles, and which seemed clearly enough to manifest, that they are very far from being filled with true and springy Air, is intimated in the
last

last clause of the foregoing narrative, but will be best understood by the annexed Experiments transcribed just as I find them registred in my Collections : and though they be prolix, and contain some few Particulars, that make not directly for the purpose I alledge them for, yet I think not fit to dismember or to epitomize them, or otherwise to alter any thing in them, *partly*, that the inference I make from them, may be the less mistrusted, *partly*, because the way of Experimenting being altogether new, will be best apprehended by the subjoyned Examples, and *partly* too, because those Particulars that relate not directly to the occasion of our mentioning these trials, may be useful to illustrate or confirm something that is already delivered, or is hereafter to be delivered in the present *History of Cold*.

II. [We took this day a glass of ^{Feb. 4.} the form of an Egg, but of double ^{1661.} the capacity, out of whose obtuse end rose up a long Cylindrical neck, capable to receive the end of my little finger, and no more, this being fill'd with

with common water, till the liquor reached a pretty way within the pipe, and the surface of the water being carefully marked on the outside, was placed in a vessel, wherein ice very grossly beaten, was mingled with a convenient Proportion of salt (according to our way of Glaciation) the Mixture not reaching up to the mark by above an inch. The Experiment afforded us these Particulars.

I. A heedful Eye did not perceive the water sensibly to subside before it began to freez.

II. The water began to swell, and some parts of it next the side or bottom of the glass, to freez within a quarter of an hour.

III. The ascent of the water in the pipe increased so fast, that within an hour, from the time the glass was put in, it did rise 4. inches and $\frac{2}{5}$ above the mark, & afterwards the swelling continued so, that we took it out, though a good part of the water remain'd unfrozen, it had reach'd five inches and somewhat more then a half above the first Mark.

IV. The ice and salt being purposely

posely kept always beneath the surface of the water, the lower parts of the water were frozen, and never the upper surface.

V. During all this great Elevation of the water, there appeared no bubbles worth taking notice of in the unfrozen parts of the liquor, but the ice was very full of them, divers of which toward the latter end of the Experiment were very large Bubbles (but not all of them round) some being about the bigness of hail shot, some small like Mustard-seed, and others again not much inferior to little pease.

VI. Having taken out the glass, when the water was at the highest mark, we did upon a certain design, pour in as much sallet Oyl as swam about two inches above it, and then the glass was nimble at the flame of a Lamp seal'd up, during which time the included water subsided a little, but the glass being again put into the ice and salt, the Cold quickly restored the water to its former height, and there remained about an inch and a half of the seal'd glass unpossessed by

by the two contain'd liquors.

VII. Then with a good pair of scales we weigh'd the glass-Egg first in the Air, and then in the water (the better to discern, whether any shrinking of the glass interven'd in the case,) where it hung freely, and was left hanging in its *Equilibrium* with its opposite weight.

VIII. Whilest it thus hung, upon the thawing of the ice many bubbles, great and small ascended (the great ones with a wrigling motion) and vanish'd at the top.

IX. As the ice thaw'd, the water and oyl descended, till the whole ice was return'd to water, at which time we observ'd these two remarkable things, the one, That the *Equilibrium* remain'd the same; the other, (which was more considerable) that the water was subsided again as low as the first mark, with which it was level before it began to swell, without falling beneath it, notwithstanding the recess of such a multitude of Bubbles, divers of which were very large.

X. The glass being inverted, the seal'd

seal'd end, which was drawn slender, was gently broken under water, of which some, being impell'd in, did sensibly reduce the Air at the opposite end into a narrower room; and, as one of the spectators observ'd, into a much narrower, which is consonant enough to reason.

XI. The glass being again inverted, and held till it was settled, we found, that the water drawn in together with the water it found there, and the oyl, possess'd the same places, (as appeared by the marks in the Cavity of the Receiver,) that they did, when it was seal'd up.

XII. And lastly, having thrown out the oyl, and employing, where need was, a little water of the same kind we had made use of all this while, we found the glass fill'd to the highest mark, to weigh 4374. grains, when it was fill'd but to the lowest mark, 4152. grains, and when quite empty'd 1032. So that the water contain'd betwixt the highest and lowest mark, and rais'd by the Glaciation, was about a fifteenth part of the water set to freez, and proba-

probably would have amounted to much more, if the water had been all frozen.]

Decemb.
11. 1662.

12. [A large glass Egg being taken with a proportionably big stem, we poured water into it, till it reached about an inch above the bottom of the stem, and fastning a mark there, we exposed it all night to freez in snow and salt, which was so placed, as not to reach so high as the bottom of the stem; the next day about ten of the clock we found the water risen in the stem about 15. inches above the mark, the whole Cylinder of water being fluid by reason of the snows not reaching to it. (Then upon a design to be elsewhere mentioned, we seal'd up the glass by a very slender pipe, that had been before purposely drawn out to a pretty distance from the body of the Cylinder, that the glass might be seal'd, in a trice before the flame of a Candle could sensibly rarifie the Air, and after a while we broke off the Apex of this slender pipe in prosecution of our former Design.) Then suffering the water to swell freely, within seven
or

or eight hours it reach'd the very top of the glass, a drop or two running over at the slender Orifice thereof, so that in all, the water ascended about 19. inches above the first mark: then we tried by the flame of a candle to seal the glass, but by reason of the Rarefaction of some of the water, by the Heat, into vapours, by which some of the other water was, from time to time, spurted against the flame of the Candle, we found it troublesome enough to seal it up, the vessel being removed into a warm place, till next morning, and all the ice in the belly of it (for the water in the stem continued fluid) being thawed, the water subsided, not only to its first mark, but a little beneath it, by reason of that which was thrown out, upon occasion of the sealing of the glass: but when we came to invert this, after the manner above mention'd, into a vessel of water, to see how much of the space deserted by the thaw'd Ice, was fill'd with Air, and how much was fill'd with a subtiler substance, or empty, just then a mischance frustrated our Expectation.]

Decemb.

13. [An Egg about the same bigness with the former, was placed to freez in beaten ice and salt, and in less then a quarter of an hour, it was risen near an inch above the Mark, where the surface of the water was at the first, and the water in the ball and the joyning of the neck was frozen into *Lamine*. After an hour and a quarter, those *Lamine*, that before appeared in the beginning of the neck, now disappear'd, but the ball seem'd frozen into a white ice, and the water in the neck was risen above the first mark four inches and a half. There now appear'd abundance of small bubbles, continually ascending through the neck (which so continu'd all the time after, till it was quite thaw'd) and the white ice appear'd full of bubbles. The Experiment being further pursu'd, the water ascended higher and higher, till it had reach'd about eight inches above the first mark : Then the top of the pipe, being with a Lamp drawn out, into a very slender Cylinder (for the conveniency of sealing up) the glass was again put into the ice, that the Air heated

heated by the Lamp might cool, upon which the water continued swelling, till it began to run over at the orifice of the slender pipe, which being held by in the flame of a candle, was in a trice seal'd up, so that the whole glass now appear'd full of water, bating an inconsiderable Quantity of rarifi'd Air, (not amounting to the bigness of half a small Pea) that remain'd contiguous to the seal'd part; the Egg being brought into a warm room, was kept there all night, and a good part of the next morning, before the ice was quite thaw'd, which when it was, the water was found subsided to the first mark, and which being done, the glass was inverted, and the seal'd end immers'd a good way under water, where being broken, the external Air impell'd the water in the Basson into the Cavity of the pipe, inso-much, that when we took it out, which we did, as soon as we thought no more water was impell'd up, re-inverting the glass, we found, that the admitted water reach'd seven inches above the first mark, and left

an inch and a half of the stem, before it began to be wire-drawn, besides as much of the slender part of the stem, as by guess amounted to a quarter of an inch or more, so that it seem'd, that the Bubbles, which made the water swell, and appear'd in the Ice, amounted to an inch and three quarters of Air, which consequently seem'd to be for the most part generated by this operation, and to seven inches either of a *vacuum*, or some subtle substance, which by its having no spring to resist the Pressure of the outward Air, appear'd not to be Air: We could not exactly measure the Quantity of water we had in all, and the proportion of it betwixt the marks, because having left the glass in the window, to try whether time or Cold would make the admitted water shrink (which we did not find it to do) the weather was so sharp, that beginning (as we concluded) to freez the water in the stem, the increasing ice burst out the belly of the glass into many pieces.]

Another time.

14. [A seal'd glass being broken
under

under water, there was impell'd into the Cylinder ten inches and a little above a half. And the mark, it should have risen to, was eleven inches and a quarter above the first and lowest mark.]

Another time.

15. [In the same Bolthead, wherein the greatest condensation of the Air was tri'd, the water was by the Cold made to swell very near a foot above the mark it rested at, when it began to freez; then the glass being nip'd up, the contain'd water was removed, and suffered leisurely to thaw, and upon the Dissolution of the ice, the water fell back to the former mark: lastly, the glass being inverted, the Apex was broken off under water, and the water in the stem was by the outward Air, pressing upon the water in the Basen, with some *Impetus* and noise driven up into the Cavity of the glass; and, the glass being seasonably and warily remov'd from the Basen, we found there had been impell'd up of the water in the Basen, a little more then eleven inches, so that there seem'd to be near

Decemb,

$\frac{7}{8}$ of an inch of Air generated or separated by the former operation.]

Another time.

*Decemb.
the 17.*

16. [In the same glass we made the water to swell about ten inches, and inverting the stem, and breaking the Neb under water, we found about ten inches of water to have been impell'd into the stem; so that in this there seem'd no generation of Air.]

17. To all these Experiments we shall subjoyn, in two words, that as in water, so in some aqueous liquors we found, that the icy Bubbles were not fill'd with Air (though we did not think fit to take the pains to measure their respective Expansions by being congeal'd:) For in that elsewhere mention'd Experiment, where we expos'd Milk, Urine, and Rhenish-wine to freez, when all those liquors were risen above their former marks, as is there related, our Notes inform us, that the Experiment was thus prosecuted.

18. [Being seal'd up (the foregoing words mention'd the above-named expanded liquors) and suffer'd to thaw, the several liquors subsided

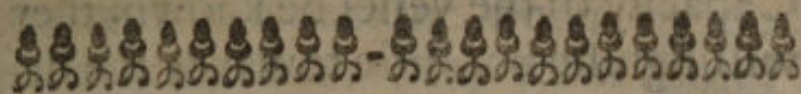
to their first marks or thereabouts, and the glasses being inverted and broken under water, we were by an accident hindred from observing what we desir'd in that which had the Wine, though when it was taken out of the freezing pot, it had ice, but not much, swimming in it. But into the glass that had the Milk, the water was manifestly impell'd by the outward Air, and so it was into the glass that had the Urine, which being remov'd from the Bason, and re-inverted, appear'd to have as much new liquor in its stem, as amounted by gues to five or six inches.]

19. To which Experiment we may add, that another time a seal'd glass of partly frozen Claret-wine being broken under water, the water was impell'd up between half an inch, and an inch above the mark, beyond which it would not have ascended, if the bubbles had been full of true and permanent Air.

20. If it be said, that though I have delivered too many Particulars about so empty and slight a Theme as Bubbles, I have this to answer, that

possibly all these Experiments have rather shew'd us, what it is not that fills them, then what it is, so that more then all these Experiments appearing requisite to clear up the Difficulties about them, I shall not think I have altogether mis-spent my time, especially if so many past Experiments, both new, and not altogether impertinent, by their not having taught us enough about so despicable a subject as a Bubble, shall, as they justly may teach us Humility.

Title



Title X.

Experiments about the Measure of the Expansion and the Con- traction of Liquors by Cold.

1. **T**O the Experiments (menti-
on'd in the Seventh and
Ninth Titles) which shew, that wa-
ter has an Expansion, it will be pro-
per to subjoyn some of those, where-
by we endeavour'd to measure that
Expansion. And here we shall not
content our selves to say, that where-
as the Authors, we had formerly oc-
casion to point at, take notice of their
having rais'd water in a Bolthead
half an inch or an inch by freezing,
we have made it ascend a foot and a
half and more ; This, I say, we shall
pass by, because that though by such
Experiments we have very clearly
and undeniably manifested the Ex-

panfion of the water, yet unleſs the Capacity of the veſſel be known, they will ſignifie but little towards the determining the *Quantity* of that Expanſion, which yet is the thing we are now enquiring after, wherefore we ſhall add, that we employ'd two differing ways to *meaſure* this Expanſion.

2. The one was, by putting in, by weight, ſuch a number of ounces of water, into a Bolthead, till the water was riſen a pretty way in the long ſtem, wherewith it was filled, then marking on the outside, to what height every freſhly added ounce of water reach'd in the ſtem, we afterwards poured out a convenient Quantity of the liquor (yet leaving enough to fill the whole cavity of the ſpherical or obtuſe end of the veſſel, and of the lower part of the ſtem) then leiſurely freezing this remaining water from the bottom upwards, we obſerved, that when it was frozen, the ice that was made of 82. parts of water, filled, as one of our Notes inform us, the ſpace of 91. and (if I miſtake not the Character) an eight, ſo that by
this

this troublesome way of Examination, we found that the water by the Expansion, it received from Cold, was made to possess about a ninth part more space then it did before congelation.

3. [In another of our notes, we find as follows, 55. parts of water extended themselves by freezing into sixty and a half, about six of those parts remaining unfrozen, so that in this Experiment the waters Expansion was not much (though somewhat) differing from what it was in that last mention'd,]

4. The other way we made use of to measure the Dimensions, that water gains by freezing, was, to take a Cylindrical pipe of glass seal'd at one end, and left open at the other, at which we fill'd it with water to a certain height, that we took notice of by a mark appli'd to the outside, and then keeping it in an erected posture, and freezing it from the bottom upwards, we found, that it had acquir'd by a tenth part or thereabouts, greater Dimensions in the form of ice, then it possessed in the form of water.

But

But the nature of the particular parcel of liquor exposed to the Cold (for it is not necessary that all waters should be equally dispos'd to be expanded by freezing) and some other circumstances, not now to be discoursed of, may well beget some little variety in the success of this sort of trials. For in one that we made carefully, we found the Expansion somewhat greater, then that last mentioned, as may appear by the following Note, which compar'd with what was lately delivered, of the trials we made by weight of the water's Expansion, may invite us to think, that we cannot much err by estimating in general, that the room that Ice takes up more then water, amounts to about a ninth part of the space possessed by the same water, before it was turned into Ice. The note we were speaking of, is this.

5. [In a more then ordinarily even Cylindrical glass, we exposed some water to freez, to measure its Intumescence, and found that it expanded its self to about an eighth part, or at least a ninth upon glaciation;

tion; this we tri'd twice, and thought that the Intumescence might have been more considerable, but that in a Cylinder the freezing did not seem to succeed so well.]

But here we must resolve a difficulty, which though ordinary Readers may take no notice of, yet may breed a scruple in the minds of those that are acquainted with *Hydrostaticks*. For to such Readers this Account of ours may seem to be contrary to the Experience of Navigators into cold Climates, who tell us (as we shall have occasion to take notice in due place) of vast pieces of Ice, as high, not only as the Poops of their Ships, but as the Masts of them ; and yet the Depth of these stupendious pieces of Ice, seems not at all Answerable to what it may be suppos'd to be, in case we compare together the Estimate above deliver'd of the Expansion of water, and that grand *Hydrostatical Theorem* demonstrated by *Archimedes* and *Stevinus*, That floating Bodies will so far, and but so far, sink in the Liquor that supports them, till the immersed part of the Body be equal to a Bulk of water, weighing

Unable to display this page

that he met with and observ'd in that Sea, which though but somewhat above 40. fathome under water, were extant 20. fathome, that is (near half as much) above water, whereas it seems, that according to our above mention'd Computation of the Expansion of water, the part under the water ought to be eight or nine times as deep, as that above the water is high.

7. To clear this difficulty, I shall represent these three particulars.

First, that in our Computation the Ice that sinks so deep, is suppos'd to float in fresh water, whereas in the Observations of the above nam'd Navigators, those vast pieces of Ice floated on the Sea-water, which by reason of its saltness, being heavier then fresh-water, Ice will not sink so deep into that, as into this. And that salt may hugely increase the weight of the water, wherein it is dissolv'd, may be clearly gather'd from the ponderousness of common Brine, and from the practise of several sorts of Tradesmen, who to examine the strength of their *Lixiviums*,
and

and other Saline Liquors are wont to try, whether they will keep an Egg floating, which we know common water will not do. And I have also by the Resolution of some Metalline Bodies in fit *Menstruums* made Liquors, that are yet much more ponderous, then is sufficient for the support of Eggs.

But yet we must be so candid, as to take notice of what some Modern Geographers deliver with probability enough, namely, That nearer the poles the Seas are not wont to be so salt, as in the temperate and the Torrid Zones, and those Northern being not so salt as our Seas, there is the less to be allow'd for the difference in gravity (and consequently in the power to keep Ice from sinking) betwixt those Seas and ours.

8. But secondly, this lesser saltiness of the water in the Northern Seas, may, as to our case be recompenc'd by the greater coldness of it. For though, as we have formerly observed, the Condensation of fresh water, effected here by a degree of Cold capable to make it begin to freez, is
not

not so great as most men would imagine; yet besides that, I have often taken pleasure to make the same Body to sink or ascend in the same water, by a much less variation of Cold than that we have been mentioning; it is to be consider'd, that the degree of Cold, to which water was brought in the Experiment deliver'd in the fourth Section, to which we are now looking back, was but such a degree as would make fresh water begin to freez; whereas the salt Sea-water, being indispos'd to congelation, may by so vehement a Cold as reigns in the Winter season in those gelid Climates, be far more intensely réfrigé- rated, and thereby more condens'd than common water is here, by such a measure of Cold, as may begin to freez small portions of it. But though, what we have hitherto represented, may well be look'd upon as not inconsiderable to the purpose for which it has been alledg'd, yet the main thing, that is to remove the scruple suggest- ed by the height of Icy hills above the water, is,

9. Thirdly, that such Hills of Ice
are

are not to be look'd upon as intire and solid ones, but as vast piles or lumps, and masses of Ice, casually and rudely heap'd up and cemented by the excessive Cold, freezing them together by the intervention of the water that washes them, which piles of many pieces of Ice are not made without great Cavities intercepted, and fill'd only with Air, between the more solid Cakes or Lumps; so that the weight of these stupendious pieces of Ice, is not to be estimated by the bigness they appear of at a distance from the Eye, but considering how much Air there is intercepted between the Icy Bodies, of which they are compiled, there may be a hollow structure of Ice reaching high into the Air, and yet the whole Aggregate or Icy pile, will press the subjacent water on which it leans, no more then would as much water, as were equal in Bulk only to the immers'd parts; as we see in Barges loaden with Boards, which though pil'd up to a great height above the water, make not the vessel to sink more then a Lading that would make

a far less show, and oftentimes be all contain'd within the Cavity of the vessel, provided it be more ponderous *in specie*. But to enter into any further Consideration of these *Hydrostatical* matters, would be improper in this place, especially since we have * elsewhere treated of them. And ^{* In our Hydrostatical Paradoxes.} that these floating Hills and Islands of Ice, are not intire and solid pieces of it, we shall elsewhere have occasion to shew out of Navigators, and even in the Observation, we have mentioned out of *Janus Munck*, the Learned Relator of it *Bartholinus*, takes notice, that those vast pieces of Ice (we have been mentioning) that reach'd 20. fathome above water, ^{Ex nive copiosa glaciata compacta,} were compiled of store of Snow frozen together.

10. These Considerations may serve to render some Account of those stupendiously tall pieces of ice, whose extant part bears so great a proportion to the immersed part, when the whole mass does really float. But I confess I doubt, that not only in the Examples we have alledg'd, but in other eminent ones of

U moun-

mountains of ice, if I may so call them, there may be a mistake, and that the height of them above the water, would be far less, and the depth under water far greater, if the ice had water enough to swim freely. For Sea-men by reason of the difficulty, are not wont to measure the height of those pieces that float at liberty in the Sea. And as for those that are on ground, as their heights lye far more convenient to be measured, so the measurers not knowing how long they may have been on ground, for ought I know, much of that admir'd height, may be attributed to the snows, that from time to time fall very plentifully in those frozen Regions, and are compacted together, either by the Sun, whose Beams sometimes begin to thaw it, and sometimes by the water of the waves that beat against the Ice, and being congeal'd with the snow, does as it were cement the parts of it together, and sometimes by both of these causes. So in the instance alledg'd out of Captain *James*, of pieces of ice that were twice as high as his Top-mast-

mast-head ; it is said also, that they
 were on ground in 40. fathome.
 And in the other Example mention'd
 out of *Bartholinus*, though there be
 40. fathome attributed to the immer-
 sed part of the ice, yet that measure
 is not exclusive of a greater, for it is
 said, that the ice reach'd downwards
 above 40. fathome ; and how much
 downwards, and whether as far as
 the ground, we are left at liberty to
 guess. And in that stupendious piece
 of Ice recorded in the *Nova Zembla*
 voyage, to have been in all 52. fa-
 thome, that is, 300. and twelve foot
 deep, though it be granted what they
 affirm, that it was 16. fathome above
 the water, which is almost a third
 part of the whole depth ; yet I ob-
 serve, that of this Icy mountain it is
 said, that *it lay fast on the ground*. So
 that as on the one side it seems pro-
 bable, that the upper part of Islands
 of ice may be increas'd by snow ; and
 as I remember, that in that famously
 inquisitive Navigator Mr. *Hudsons*
 voyage for the discovery of the
 North-west passage, 'tis rela-
 ted, that his company was

* Mr. Hud-
sons Voy-
age for the
discovery
of the
North-west
passage,
written
partly by
Mr. Aba-
cuck
Pricket.

* so well acquainted with the Ice, that when Night, or foggy or foul weather took them, they would seek out the Broadest Islands of Ice, and there come to Anchor, and run and sport, and fill water that stood the Ice in ponds very fresh and good. So on the other side we know not, how much lower the Dutch-mens Ice and Captain James's would have reach'd into the Sea, in case the ground they rested on, had not hindered them. For though one might probably think, that these are the greatest depths that any Hills of Ice have been observ'd to attain, that mention'd by the *Hollanders* reaching 36. fathome beneath the water, and that mention'd by Captain James, no less then 40. fathome: yet I find in Mr. *Hudsons* Voyage, that the English in the Bay, that bears his Name, met with more then one or two Islands of Ice, of a far greater depth under water. For among other things, the Relator has this memorable passage; *In this Bay, where we were thus troubled with Ice, we saw many of those mountains of Ice a ground, in six or seven score fathome water. And if the Sea had been deep enough,*

enough, even these stupendious moles of Ice would probably have sunk much lower, and so have lessened the heights of the mountains,

II. I know that delivering the measure of the Expansion of water alone, I have not said all that may be said about the Expansion of Liquors: But because, as it has not yet appeared to me, that any Liquor is expanded by Cold, unless by actual freezing; I doubted, whether Aqueous Liquors, as Wine, Milk, Urine, &c. were otherwise expanded by congelation, then upon the Account of the water or phlegmatick (and, in a strict sense, *congealable*,) part contain'd in them; and whether it were worth while, for a man in haste, to examine, their particular Expansions. Notwithstanding which, I would not discourage any from trying, whether or no by the differing Dilatations of Aqueous Liquors, some of them of the same, and some of them of differing kinds, we may be assisted to make any estimate of the differing proportions they contain, of phlegm, and of more
 U 3 spiri-

spirituous or useful Ingredients.

12. After what has been hitherto delivered concerning the *Expansion* of Liquors by Cold, it may be expected we should say something of the measure of their *Contraction* by the same Quality. But as for water, which is the principal Liquor, whose Dimensions are to be consider'd, I have formerly declar'd, that I could seldom or never find its contraction (in the Winter season when I tried it) to be at all considerable. And I shall now add, that having for greater certainty, procur'd the Experiment to be made by another also, in a Bolthead, the Account I received of it, was, that he could scarce discern the water in the stem to fall beneath its station, (mark'd at the upper part of the pipe,) when the water in the Ball was so far infrigidated as to begin to freez. Though I will not deny, that in warmer Climates, as *Italy*, or *Spain*, the contraction of the water a little before glaciation begins, may be somewhat considerable, especially if the Experiment be made in Summer, or in case (either there

(29)
there or here) the water expos'd to
freez be put into a vessel very advan-
tageously shap'd, or brought out of
some warm Chamber or other place,
where the heat of the Air, that sur-
rounded it, had rarifi'd it. But to
examine the measures of Contracti-
on in the several Liquors, and with
the nice Observations, that such a
work, to be accurately prosecuted,
would require, would have taken up
much more of my time then I was
willing to imploy about a work
which I look'd not on as important
enough to deserve it. And therefore
I shall here add nothing to what I
have said under the Title of the *De-
grees of Cold*, touching the contraction
of spirit of Wine, and of oyl of Tur-
pentine, by the differing degrees of
that Quality. And as for the con-
densation of Air, the vastest fluid we
deal with, I did indeed think fit to
measure how much Cold condenses
it. But the account of that Experi-
ment will be more opportunely deli-
ver'd in * one of the following Dis-
courses.

* In the
Sect. about
the Tem-
perature
of the
Air.



Title XI.

Experiments touching the Ex- pansive Force of Freezing Water.

1. **H**AVING shewn that there is an Expansion made of water, and Aqueous Bodies, by Congelation, let us now examine how strong this Expansion is, and the rather because no body has yet, that we know of, made any particular trials on purpose to make discoveries in this matter, so that although some unhappy Accidents have kept our Experiments from being as accurate as we designed, (and as, God assisting, we may hereafter make them) yet at least we shall shew this Expansion to be more forcible, then has hitherto been commonly taken notice of, and assist men to make a somewhat less uncertain Estimate of the force of it, then

then they seem to have yet endeavour-
red to enable themselves to make.

2. And 1. we shall mention some
Experiments, that do in general
shew, that the Expansion of freezing
water is considerably strong.

We took a new Pewter-bottle, ca-
pable to contain, as we guess'd, a-
bout half a pint of water, and having
fill'd it top full with that Liquor, we
scru'd on the stopple, and exposed
it during a very frosty night, to the
cold Air, and the next morning the
water appeared to have burst the
Bottle, though its matter were me-
talline, and though purposely for this
trial we had chosen it quite new, the
crack appeared to be in the very sub-
stance of the Pewter. This Experi-
ment we repeated; and 'twas one of
those bottles fill'd with Ice that had
crack'd it, which a *Noble Virtuoso*
would needs make me (who should
else have scrupled to amuse, with
such a Trifle, so great a Monarch,
and so great a *Virtuoso*) bring to his
Majesty, to satisfy him, by the wide-
ness of the crack, and the Protube-
rance of the Ice, that shewed it self in
it,

it, that the water had been really expanded by Congelation.

3. We also tried, whether or no a much smaller Quantity of water, would not, if frozen, have the like Effect, and accordingly, filling with about an ounce of water a scrūd Pewter box (such as many use to keep Treacle & Salves in) quite new, and of a considerable thickness, we found, that upon the freezing of the included water, the vessel was very much burst.

Afterwards filling a Quart Bottle (if I mistake not the capacity) with a congealable liquor, and tying down the Cork very hard with strong Pack-thread, we found that the frost made the liquor force out the stopple in spite of all the care we had taken to keep it down.

But afterwards we so well fastned a Cork to the neck of a quart bottle of Glass, that it was easier for the congealing liquor to break the vessel, then to thrust out the stopple, and having for a great many hours expos'd this to an exceeding sharp Air, we found at length the bottle burst, although

though it were so thick and strong, that we were invited to measure the breadth of the sides, and found that the thinnest place, where it was broken by the Ice, was $\frac{3}{8}$ of an inch, and the thickest $\frac{1}{2}$ that is twice as much; we also by the help of the frost broke an earthen bottle of strong Flanders metal, of which the thinnest part that was broken, was equal by measure, to the thinnest part of the other.

4. But the above mention'd Instances serving only to declare in general, that the Expansion of water by Cold is very forcible, I thought fit to attempt the reducing of the Matter somewhat nearer an Estimate less remote from being determinate, and because the water expos'd to congelation, may be probably supposed to be Homogeneous, we judg'd, that the quantity of it, may very much vary its degree of Force, and because some may suspect, that the Figure also may not be inconsiderable in this matter, we thought fit to make our Trials in a Brass vessel, whose Cavity was Cylindrical, and which to make

make it stronger, had an orifice but at one of its ends: and whose thickness was such, that we had reason to expect, that whilst the top remained covered, but with a reasonable weight, the included water would find it more easie to lift up that weight, then break the sides. To this Cylinder we fitted a cover of the same mettall that was flat, and went a little way into the Cavity, leaning also upon the edges of the sides for the more closer stopping of the orifice; the cavity of this Cylinder was in length about five inches, and in breadth about an inch and three quarters. This Cylinder being fill'd top full with water, and the cover being carefully put on, was fastned into an Iron frame, that held it erected, and allowed us to place an iron weight, amounting to 56. pound, or half a hundred of common English weight, which circumstance I mention (because the common hundred that our Carriers, &c. use, exceeds five score by twelve.) But this vessel being exposed in a frosty night, to the cold Air, the contain'd water did not the
 next

next morning appear to be frozen, and the trial was another time that way repeated with no better success, as if either the thickness or clearness of the mettall had broken the violence of the external Airs frigefactive Power, or the weight that oppressed the Cover had hindred that Expansion of the water, which is wont to accompany its Glaciation.

Wherefore we thought it requisite to apply to the outside of the vessel a mixture of salt with ice or snow, as that which we had observed to introduce a higher degree of Cold then the Air alone, even in very frosty nights; and though this way it self, the glaciation proceeded very slowly, and sometimes scarce at all, yet at length we found, that the water was by this means brought so far to freez, that on the morrow the ice had on one side swelled above the top of the Cylinder, and by lifting the cover on that side, had thrown down the incumbent weight; but in this trial the cover having been uniformly, or every where lifted up above the upper orifice of the Cylinder, we repeated

peated the Experiment divers times; as we could get opportunity, sometimes with success, and sometimes without it; and of one of the chief of our Experiments of this sort, we find the following account among our Collections.

5. [The hollow brass weight, being about one inch and three quarters in Diameter, and the brass cover put on, was loaded with a weight of 56. pound upon the cover, and expos'd to an excessively sharp night, the next morning the cover and the weight were found visibly lifted up, though not above (that we could discern) a small Barley-corns breadth, but the thickness of the brass cover was not here estimated, which was much less than half an inch, which according to former observations, one might expect to see the ice ascend. But that which we took particular notice of, was, that the inclosed Cylinder of ice, being by a gentle thaw of the superficial parts taken out, appear'd so full of bubbles, as to be thereby made opacous: Also when in the morning the Cylinder was brought into my Chamber,

Chamber, before the fire was made, the 56. pound weight being newly taken off, at a little hole, that seemed to be between the edge of the Brass and Ice, there came out a great many drops of water, dilated into numerous bubbles, and reduced into a kind of froth, as if upon the removal of the oppressing weight the bubbles of the water had got liberty to expand themselves, but this lasted but a very little.]

6. After this, the difficulty we have often met with in the placing of great weights conveniently upon the cover of a Cylinder, and the Expectation we had to find the Quantity of the water, we made use of, capable upon its Congelation, to lift up a much greater weight, invited us to make trial of its Expansive force, by somewhat a differing way, which was, to fit a wooden plug to the Cavity of the Cylinder (after we had suffered it to soak a convenient time in water, that, swelling as much as it would before, it might be made to swell no more by the water, which would lye contiguous to it in the vessel)

sel) and then to drive it forcibly in, till by considerable weights appended to the extant part of the plug, when the Cylinder was inverted, we could not draw it out; the success of one of these Trials is thus set down in our Collections.

7. [A Plug was driven into the Cavity of a Brass Cylinder, first filled with water, the Plug being also well soaked, then the Cylinder being inverted, the Plug took up half a hundred and a quarter of a hundred weight, and would possibly have taken up much more, and being exposed to a very sharp night, the freezing water thrust out the plug about a barley-corns breadth, quite round above the upper edge of the Cylinder, and it freezing all that day and the next night, it was again exposed, the plug not being yet taken out, and then the plug was beaten out a little more, namely (in all) near a quarter of an inch.]

8. Thus we see, that the expansive endeavour of the water forced a resistance, at least equal to that which would have been made by a weight of

of 74. pound, and probably, as the note intimates, would have appear'd able to do more, if we had had convenient weights and Instruments, wherewith to have measur'd the strength of the waters endeavour outwards, which some subsequent Trials, made us think very considerable, though not finding their Events set down in our notes, we think it fit at present to leave them unmentioned.

But one thing there is in these trials, that I think not unworthy a Philosophers notice, and his considering, namely, that this endeavour of the water to expand it self, is thus vigorous, though the uttermost term to which it would expand it self, in case it were not at all resisted, would be but to about a ninth, or at most an eight part of the space it possess before it began to freez; whereas Air ^{* New} may by Heat (which ^{Exp. Physico-mech.} yet we have ^{Exper. 6.} elsewhere shewn, will not reduce it to any thing near its utmost expansion) be brought to possess (though not to fill) according to the diligent ^{† See the forecited place.} Mersenus's observation, seventy times, the Dimensions it had before Rarefaction,

X

on,

on, and consequently the Air expanded by Heat, does by its endeavours, tend to acquire above 60. times the space that the water does, when expanded by so high a degree of Cold, as is capable to turn it all into Ice: not to mention that the expansion to which the Air tends upon the Account of its own spring, is, (as we shew in another * place) many times greater then that to which *Mersennus* could bring it upon the bare Account of Heat.

* The Appendix to the Physico-mechanical Experiments.

9. There remains yet one way, whereby we hop'd, though not to measure the Expansive force of freezing water, yet to manifest it to be prodigiously great, or in case we fail'd of this aim, to produce at least some other *Phænomena* relating to Cold, that would not be inconsiderable. And though our endeavours succeeded not, yet because a happier opportunity may bring them to be one way or other succesful, we shall annex, That we caus'd to be made, an Iron Ball of between two and three inches in Diameter, which Ball was solid, save that in the midst there was

was a small Cavity left to place a little water in, together with a female screw, as they call it, reaching from the outward surface of that internal cavity; and to this was applied a strong Iron screw, so fitted to the internal cavity of the other screw, as to fill it with as much exactness as could be obtained. And this screw was made to go so hard, that it requir'd to be screw'd in by the help of a Vice, that it might not be forc'd out, without breaking the Iron it self. Our design in imploying this Instrument was, that having well fill'd the internal cavity with water, and forc'd in the screw as far as it could be made to go, the Instrument thus charg'd with water, might be expos'd to the highest degree of Cold we could produce. For having thus ordered the matter, we thought we might expect, either that the water how much soever we heightned and lengthned the Cold, would not freez at all, being hindred from the Expansion belonging to Ice in comparison of water; or, if it did freez, that one of these two things would hap-

pen, *either* that the expansive force of that little water, would by forcing such an Iron Instrument, manifest its strength to be stupendious, *or* by not breaking it, present us with ice without Bubbles, or at least not rarer and lighter, then the water it was made of; but for want of a sufficient Cold our designs succeeded not, so as to satisfy us, though we more then once attempted it. For the great thickness of the Iron being consider'd, we were not sure that the waters not freezing, might not proceed rather from the thickness and compactness of the metal, then from its resistance to the expansion of water. And therefore we must suspend the inferences, this Experiment may afford us, till we have opportunity to make trial of it, with a Cold not only very intense, but durable enough, the want of which last circumstance keeps us from daring to build any thing on our Experiment.

10. And here we may take notice, that it may be an inquiry, more worthy a Philosopher, then easie for him, whence this prodigious force,
we

we have observ'd in water, expanded by glaciation, should proceed. For if Cold be but, as the *Cartesians* would have, a privation of Heat, though by the recess of that *Ethereal* substance, which agitated the little Eel-like particles of the water, and thereby made them compose a fluid body, it may easily enough be conceiv'd, that they should remain rigid in the Postures wherein the *Ethereal* substance quitted them, and thereby compose an unfluid Body like Ice: yet how these little Eels should by that recess acquire as strong an endeavour outwards, as if they were so many little Springs, and expand themselves too with so stupendious a force, is that which does not so readily appear. And on the other side in the *Epicurean* way of explicating Cold, though the *Phænomenon* seems somewhat less difficult; yet it is not at all easie to be salv'd: For though, granting the Ingress of swarms of Cold Corpuscles, the Body of water may be suppos'd to be thereby much swell'd and expanded, yet besides that these Corpuscles

stealing insensibly into the Liquors they insinuate themselves into, without any shew of boisterousness or violence, 'tis not so easie to conceive how they should display so strange a force against the sides of those strong vessels that they break, when they may as freely permeate or enter them: besides this, I say, we observe that in Oyl, which requires a far greater degree of Cold to be congeal'd to a good degree of hardness, the swarms of frigorifick Atoms that invade it, are so far from making it take up more room then before, that they reduce it into less, as may appear by those former Experiments which manifested, that Cold does not expand, either oyl or uncongealable Liquors, but condense them.

11. After what I have thus largely delivered, concerning the expansive endeavour of freezing water, I hope I may be allow'd to leave to others (if they shall think it worth the labour) the prosecution of the like Experiments upon Wine, Milk, Urine, and other Liquors abounding with Aqueous parts, concerning which

we

we shall only in general remind those that may have forgotten it, That by some of our Experiments it appears, that such Aqueous Liquors are expanded by congelation, and, that their endeavour outwards is considerably forcible, seems more then likely from what we formerly noted out of the Dutch Voyage to *Nova Zembla*, where 'tis related, that by the extreme Cold, both some of their other Barrels, and some of those that were hooped with Iron, were, as they speak, *frozen in pieces*, that is, according to our Conjecture, burst together, with the Hoops, whether of Wood or Iron, by the expansive force of the imprison'd Liquors brought to freez.

12. To which I shall add, that when I asked an Ingenious person, whether in *Russia*, where he liv'd a good while, Beer and Wine did not, when brought to congelation, break the vessels they were frozen in; He Answered, *That he had not observed wooden vessels to have been broken by them, (perhaps because of their yielding) but glass and stone Bottles often.*



Title XII.

*Experiments touching a New
way of estimating the Expan-
sive force of Congelation,
and of highly compressing
Air without Engines.*

I. **T**Here is yet another way, that I bethought my self of, at once to measure the force wherewith freezing water expands it self, and to reduce the Air to a greater degree of condensation, then I have as yet found it brought to by any unquestionable way of compressing it: But whereas by this method to determine exactly the Expansive force of the water, it were requisite not only to know the quantity of the water, and that of the Air exposed to the Cold, but to make the Experiment in vessels conveniently shap'd to measure the
Dilutation

Dilatation of the one, and the compression of the other; our Experiments being made in a place where we were not provided of such glassess, we were not able to make our trials so instructive and satisfactory, as else we might have done; nevertheless we shall not scruple to subjoyn those of them, that we find noted down among our Collections, allowing our selves to hope, that will not be unacceptable or appear impertinent, not only upon the account of their novelty, but for two other reasons.

2. The first, because though they do not accurately define the Expansive force of freezing water, yet they manifest, that it is wonderfully great, better perhaps then any Experiment that has been hitherto practised (not to say, thought of) as may appear by comparing what we have delivered in another Treatise, of the great force requisite to compress Air considerably, with the great compression of Air that has already been this way effected.

3. The second, because this new way affords us one of condensing the
Air

Air much farther then hitherto it has, by any method I have heard of, been unquestionably reduced, I say, *unquestionably*, because though the diligent *Mersennus*, and others, seem to have conceived himself, to have reduced it in the wind-Gun into a very narrow room, yet besides that, by our Expedient, we have compressed it beyond what these Ingenious *Men* pretend to: Besides this, I say, I have long much questioned, whether the way of compressing Air in a wind-Gun, which both they and we have imploy'd, may safely be relied on; for the oyl or some other analogous thing, that is wont this way to be imploy'd, and the overlooking of several circumstances, that are more necessary to be taken into diligent consideration, then wont to be so, may easily enough occasion no small mistake in assigning so great a degree to the compression of the Air; but our Exceptions against this way of measuring it, may be more opportunely discours'd of in another place. And therefore we will now proceed to take notice, that of the two known ways

ways of compressing Air, the clearest and most satisfactory, seems to be that which is performed in the *wind Fountain*, as 'tis commonly called, where yet I have seldom, if ever, seen the Air, (that I remember) by all the violence men could use to syringe in water, crowded into so little as the third part of the capacity of the vessel. And an ingenious Artificer, that makes store of these Fountains, being consulted by me, about the further compressing of Air in them, he deterr'd me from venturing to try it, by affirming to me, that both he and another skilful Person of my Acquaintance, had like to have been spoiled by such attempts; for endeavouring to urge the Air beyond a moderate degree of compression, it not only burst some Fountains made of Glass, but when the Attempt was made in a large, but thick vessel, made of strong and compact Flanders Earth (the same with that of Jugs and stone Bottles) the vessel was by the over-bent spring of the Air burst with a horrid noise, and the pieces thrown off with that violence, that
if

if they had hit him, or his Friend that assisted him in the Experiment, they might have maimed him, if not killed him out right, so that the greatest unquestionable Compression of the Air seems to have been that, recorded in the Fifth Chapter of our *Defence* against the learned *Linus*, where nevertheless, we could reduce the Air by the weight of a Cylinder of *Mercury* of about 100. inches, (which consequently might near countervale a Cylinder of six score foot of water) but into a little less than a fourth part of its usual extent; but how much further the Air may be compressed by our new purposed way, it is now time to shew by the ensuing notes, of which we have not omitted any that we could find, both that some scruples, which might else arise about the way we imployed, may be prevented, or satisfied, and that the way, we imployed in practising this method, might by some variety of Examples be the better understood.

Decemb.
the 13.

4. [We took a large glass Egg, with a Cylindrical stem about the bigness

bigness of my middle finger, and
 pouring in water, till it reach'd
 about a fingers breadth higher then
 the bottom of the stem, we let it to
 freez in snow and salt, for some
 hours, with the stop of the stem
 (which was drawn out into a very
 slender pipe almost at right angles
 with the stem) open, and there left
 it for some hours, and the water was
 risen betwixt six and a half, and seven
 inches. This we did in order to ano-
 ther Experiment, but then easily and
 nimbly sealing up the slender pipe
 above mentioned, that the Air in the
 stem might not be heated, we let it
 continue in the snow, sometimes ad-
 ding fresh for about 24. hours to ob-
 serve, to what degree the water, by
 expanding it self, would compress
 the imprison'd Air. The length of
 the Cylinder of Air to be condens'd
 at the time of the sealing, was (ac-
 counting by Estimation for the slen-
 der pipe newly taken notice of) al-
 most $9\frac{7}{8}$ inches. This space we ob-
 served the ascending water as the ice
 increas'd below, to invade by de-
 grees: (for we watch'd it, and mea-
 sur'd

fur'd it from time to time) so much, till at length the water reach'd to 8. inches and $\frac{7}{8}$ almost, above the station (which we had carefully mark'd with a Diamond) in which we found it, when the glass was seal'd up, leaving but about an inch of Air at the top, so that of the whole space before possess'd by the Air, the water had intruded into near nine parts of ten ; then being partly apprehensive the glass would hold no longer, but have its upper part blown off, as it happened to us a little before with another vessel, and partly being desirous to try that which follows, we leisurely inverted the glass, that the Air might get up to the ice, for all the water in the stem had been purposely kept unfrozen, and having provided a Jar to receive the water that should be thrown out, we broke the slender pipe which we had seal'd up, and immediately as we expected, the compressed Air with violence and noise, blew out of the stem into the Jar about ten inches of water, which was somewhat more (between half an inch and a whole inch, by reason

son of the *Impetus* of the self expanding Air) then the space possess'd by the Air, before it began to be compress'd. And besides this, such a strange multitude of Bubbles, that were formerly repress'd, did now get liberty to ascend from the lower parts of the glass to the top of the remaining water, that it somewhat emulated that which happens to bottled Beer; upon the taking out of the Cork. *N.B.* when the Air was compressed beyond seven inches, we observ'd divers times, that the inside of the glass possess'd by the Air, and nearest to the water, was round about, to a pretty height, full of very little drops like a small dew, but when we came to break the glass, we took noe such notice, whether the rising water had lick'd them up, or their concurrence made them run down into it, or for some other reason, we determine not.]

Another.

5. [We took a single vial filled ^{Decemb.} with water, about half an inch above _{13.} the lower part of the neck, and leaving about two inches of Air in the remain

remaining part of the neck, which was drawn out into a slender pipe, like that of the glass last mentioned, we seal'd it up, the Air being first well cool'd, and exposing it to freez, we observ'd a while after, that it had by guess condens'd the Air into lesser room. A while after, being in another Chamber, we heard a considerable noise, and imagining what it was, we went directly to the glass, whose upper part consisting of about an inch of the neck, besides the slender pipe, we found had been blown off from the table upon the ground, the body and part of the neck remaining in the snow; but this glass was of a mettall that uses to be more brittle then white glass.]

Another.

6. [A round white glass, almost fill'd with water, was seal'd up with care to avoid heating the included Air, which amounted to a Cylinder of about two inches and $\frac{7}{8}$; after a while the water swell'd and compressed the Air almost two inches, that is full two thirds: and then (as we conjectur'd, because the snow reaching

ing

ing too high, froze it in the neck) we found the glass crack'd in many places of the Ball, and the top thrown off at some little distance from it.]

Another.

7. [A large single vial seal'd, in whole neck the Air was not condens'd to half its former room, just as we were going to break it under water, to observe the sally of the compress'd Air, suddenly blew off with a good noise, and threw from the table almost the whole neck of the Vial in one intire piece, which is near four inches long, and at the *Bas* above an inch broad.]

8. [A glass about the bigness of a Turkey Egg, and of an oval form, with a Neck almost Cylindrical, but somewhat wider at the lower then the upper part, was fill'd with water, till there was left in the neck four inches and a half, whereof the last quarter of an inch, and a little more, was much narrower then the rest, being drawn into a conical shape, that it might be easily seal'd at the Apex ; along this Cylinder, from the surface of the water, to the top of the glass,

Y

was

was pasted a list of Paper, divided into inches and quarters, and then the glass being carefully and expeditiously seal'd up by the flame of a candle, we observ'd, that by holding the glass a while in a warm hand, and a room where there was a good fire, the water was swell'd up near a quarter of an inch, but placing the glass amongst solid pieces of ice mixt with salt, the water quickly began to subside upon the Infrigidation, and a while after beginning to freez, it began to swell, and by degrees compress'd the Air, till it had crowded it into less then a 17. part, by what seem'd indisputable, for by estimate, it seem'd to some to be crowded into less then a 20. part, if not a much lesser part of the room it formerly possess'd, which difference of Estimates, notwithstanding the divided Paper, proceeded from the change of the figure of the upper end of the glass, from the Cylindrical, and to shew that there was no leak at the place where the glass was seal'd; besides, that by prying diligently, we could discern none; besides this,

I say, when the pressure of the thus crowded Air grew too strong for the resistance of the glass, it burst with a noise, that made us come to it from several places of the house; the vessel broke not in the Cylindrical part (as I may so speak) but in the oval, the whole pipe with the seal'd end remaining entire, the ice appear'd full enough of Bubbles, which made it white and opacous, and the water that had ascended into the neck, upon the breaking, was all driven out of it.]

Thus far our Collections, but because we had in another glass, where the operation was sooner dispatch'd, an opportunity of watching & observing somewhat more exactly, we will add,

9. That the last, and possibly the best Experiment we had of compressing Air by freezing, was made in a short and strong glass-Egg, whose ball was very great in proportion to the stem, that the expanding of the water might have the more forcible operation; This vessel being exactly seal'd, and having a divided list of

paper pasted along the stem, was set to freez with snow (or ice) and salt, and the contain'd water did quickly begin to crowd the Air into a lesser room, and for a good while ascend- ed very fast, till at length it having thrust the Air into so small a part of the Cavity of the pipe, that we vehemently suspected there might be some unheeded flaw or crack of the glass, at which the Air had stollen out, we drew near the vessel, and attentively prying all about it, to try if we could discover any ground of our suspition, we found (as far as the divided list, and other circumstances could inform us) that the Air (supposing none of it to have got away) was reduc'd by our Estimate into the 19. part of the space it possess'd before. And this our curiosity prov'd not unseasonable, for whilest we were narrowly survey- ing the glass, to spy out some flaw in it, we were quickly satisfied there had been none, by a huge crack made upon the Eruption of the included Air, whole spring being by so great a compression made too strong for the glass to resist, it did with a great noise

noise break the ball of the glass into many pieces, throwing the unfrozen part of the water upon me, and also throwing off the stem of the Egg, which yet I had the good fortune to recover intire, and which I yet keep by me as a rarity.

10. Thus far we then proceeded in compressing the Air, which being done in vessels Hermetically seal'd, where no Air can get in or out, seems to me a more unexceptionable way, then those that have hitherto been thought of. But further, we could not then prosecute it for want both of convenient glasses, and of ice or snow, of which if we were provided, and particularly of strong glasses, we should little doubt of reducing the Air to a yet more considerable degree of compression.

11. We may add on this occasion, that we look'd upon the same way as somewhat less unpromising then others, that have been hitherto us'd to try the compression of water; for though hitherto neither the Experiments of Ingenious Men, nor those made by our selves have fully satisfi'd

us, that water admits any more compression, then it may suffer upon the account of the little parcels of Air, that is wont to be dispersed among it, yet the unsuccessfulness may perhaps (for I propose it but as a mere conjecture) be imputed to the porousness of the vessels, wherein by the ways already practis'd, the Experiment must be made, whereas in this new way of ours, not only the force wherewith the compress'd Air presses upon the water, grows at length to be exceeding great, and is appli'd not with a sudden *Impetus*, as when a Pewter vessel is knock'd with a Hammer, but by slow and regular degrees of increase, but the water is kept in a vessel impervious to its subtilest parts, so that it may indeed crack the glass, but cannot get out at the pores, as water compress'd is wont to do at those of metalline vessels. The prosecution of this Experiment to bring it to any thing of Accuracy, we omitted, partly through forgetfulness and Avocations, and sometimes for want of conveniency to try it. But by the first of
the

the lately mention'd Experiments, about the condensation of Air, it seems by the strong multitude of Bubbles, which upon the breaking of the glass appear'd in the water that had been compress'd betwixt the Air and the Ice, that those two Bodies had very violently compress'd it: and this we are the more apt to believe, because that another time, when we had seal'd up some Air, and water in a glass-Egg, and permitted the water to swell by the operation of the Cold, but till it had reduc'd the Air, included with it, to about three quarters of the space it posses'd before, even then (I say) to try whether the subjacent water were not also compress'd by the Air it urg'd, we broke off the seal'd Apex of the glass, and perceiv'd, as we expected, the water to ascend, and that to the height of a quarter of an inch, as we found by measure. But such trials having not been, as we just now acknowledg'd, duly prosecuted, we shall at present content our selves to have nam'd this way of attempting the compression of water, without grounding any Inferences upon it.



Title XIII.

Experiments and Observations touching the sphere of Acti- vity of Cold.

1. **T**He sphere of Activity of Cold, or to speak plainer, the space, to whose extremities every way the action of a Cold body is able to reach, is a thing very well worth the enquiring after, but more difficult to find, than at first one would imagine: For to be able to assign the determinate limits, within which, and not beyond them, a cold Body can operate, several things are to be taken into consideration; as first, what the degree of Cold is, that belongs to the assigned Body: For it seems rational to conceive, that if a cold Body as such, have a diffusive virtue, those that have greater degrees of Cold,

Cold, as Ice and Snow, will be able to diffuse it to a greater distance, as we see that a coal of Fire will cast a sensible heat much further then a piece of wood, that is heated without being kindled. Secondly, the *Medium* through which the Diffusion is made, may help to enlarge the Bounds, or straiten the Limits of it, as that *medium* is more or less dispos'd to receive or to transmit the Action of the cold Agent. Thirdly, Not only the Consistence, and Texture of the *Medium*, but its Motion, or Rest may be considered in this case. For in frosty and snowy weather, men observe the winds that come from frozen lands, to blow more cold, then winds from the same Quarter would do, in case there were no Ice nor Snow in their Passage. Fourthly, There may be made very differing Estimates of the Diffusion of Cold, according to the Instrument that is imploy'd to receive, and acquaint us with the Action of Cold. For a liquor or other Body may not appear cold to him, that examines it with a Weather-glass, whilst he shall feel it

it cold with his hand; and, as we elsewhere also note, to that sensory it self, as 'tis variously dispos'd, the same object will seem more or less cold; so much may the Predisposition of the Organ impose upon the unskilful or unwary. Fifthly, The very bulk of a cold Body may very much enlarge or lessen its sphere of Activity, as we may have occasion to shew ere long. And besides there may be divers other things, that may render it very difficult to ascertain any thing in this matter. And therefore I shall reserve them for other opportunities, and observe now in general, that in such small parcels of Ice it self, as in our Experiments we are wont to deal with, we have found the sphere of Activity of Cold exceeding narrow, not only in comparison of that of heat in fire, but in comparison of the Atmosphere, if I may so call it, of many odorous Bodies, as Musk, Civet, Spices, Roses, Wormwood, *Assa dulcis*, *Assa foetida*, *Castoreum*, *Camphire*, and the like; nay, and even in comparison of the sphere of Activity of the more vigorous

rous Loadstones, insomuch that we have doubted, whether the sense could discern a cold Body, otherwise then by immediate Contact ?

2. And to examine this, having taken a piece of Ice, we did not find upon trials, that I partly made my self, and partly caus'd in my presence to be made by others, that if a mans Eyes were close shut, he could certainly discern the Approach of a moderately siz'd piece of Ice, though held never so near his fingers ends. Nay, which is more considerable, having had the curiosity to make the Trial, with one of those very sensible Thermoscopes I have formerly mention'd (wherein a pendulous drop of liquor plays up and down in a slender pipe) I found, that by holding it very near to little Masses of snow (somewhat compacted too) the movable drop, did not betray any manifest operation of so cold a neighbouring Body ; but if the glass were made to touch the snow, the effect would then be notable, by the hasty descent of the pendulous drop, or its motion towards the obtuse part of the Instru-

Instrument, in case that were not perpendicularly, but laterally appli'd to the snowy Lumps. But this languidness of operation, may perhaps proceed in great part from the smallness of the Pieces of Ice that were imploy'd: For hearing of a Merchant, that had made divers Observations about Cold in *Greenland*, I desir'd, by the mediation of a very learned Friend, to be inform'd, whether or no in the night they could perceive those vast heaps, or rather mountains of ice, that are wont to float up and down in that Sea, by any new and manifest accession of Cold, and was inform'd by way of Answer to that Question, that being at Sea, they could know the approach of Ice, as well by the increase of Cold, as by the glaring light which the Air seem'd to receive from the neighbouring Ice.

3. But that which makes me suspect, that there may in this account be some mistake, is, that I have not yet met with any like observation in any of the voyages into gelid Climates, that I have had occasion to peruse,

peruse, though in some of them the Navigators frequently mention their having met with vast rands (as some call them) and Islands of mountainous ice in the night. And 'tis, as I remember, the complaint of one or two, if not more of them, that the Ship lay close by such vast pieces of ice, without their being aware of it, by reason of the fogs. By which it seems that there was no sensible Cold diffused to any considerable distance, whereby they might be advertised of the unwelcome neighbourhood even of so much ice : But possibly the approach of far smaller masses of ice, would have been sensible to them in such a Climate as ours, where the organs would not have been indisposed to feel, by a long accustomance, of any thing near so intense a degree of Cold, as that which then reigned in those Northern Seas.

4. Whilest we were considering the Difference, betwixt the operations of even the Coldest Bodies at the very nearest Distance, and upon immediate Contact, we thought it an Experiment not altogether unworthy
to

to be tri'd, whether, though ice and snow alone, that is, unassisted by salts, would not in some of our formerly mention'd Experiments freez water, through the thickness even of a thinglass, they may not yet do it when the water is immediately contiguous to them. And I remember, that we took a conveniently shap'd Glass, and having frozen the contain'd water for some hours, from the bottom upwards, till the ice was grown to be of a considerable thickness, we mark'd, what part of the glass was possess'd by the unfrozen water, and then removing the vessel to a little Distance from the snow, and salt, it stood in before, we let it rest there, to try whether the ice would freez any part of the contiguous and incumbent water; but some intervening accidents hindred us from being able to derive any great satisfaction one way or other from our trial.

*Voyage de
Moscovic
& de Per-
se, Liv.V.*

5. Wherefore we shall add by way of Compensation, that the diligent *Olearius* relates, that at *Ispahan*, the Capital City of *Persia*, though it be seated

seated in a very hot Climate, and though it seldom freez there above a finger thick, and the ice melt presently at Sun-rising, yet the Inhabitants have Conservatories, which they furnish with solid pieces of ice of a good thickness, only by pouring at night great store of water at convenient intervals of time, upon a shelving floor of Free-stone or Marble, whereon, as the water runs over it, the most dispos'd of its parts, are in their passage arrested, and frozen by the contiguous ice, which by this means (says my learned Author) may be brought in two or three successive nights, to a very considerable thickness.

6. We several times gave order to have this Experiment tried in *England*, but partly through the negligence of those we imploy'd, and partly upon the score of intervening circumstances, our expectation was but ill answered. And in this case I mention *intervening circumstances*, because having caus'd a servant to pump in the night, upon a not very thin plate of ice, that was laid shelving

ving upon a Board, and another flat piece of Ice being about the same time laid under a place, where water derived from a neighbouring spring, is wont continually to drop, he brought me word, that not only in this last nam'd place, the ice melted away, but that under the pump, instead of increasing in thickness by the waters running over it, it was thereby rather dissolv'd. At which somewhat wondring, I went in the morning myself to the pump, and causing a good flake of ice to be in a convenient posture plac'd under it, I observed the water as it came out of the pump, and was falling on the ice, to smoak, as if the depth of the Well had made the water, though very Cold to the touch, somewhat warm in comparison of the ice, and thereby fitter to resolve then to increase it; (which inconvenience may be prevented by suffering the water of deep Springs and Wells, to stand to cool in the Air, before it be put to the Ice,) and this, though the neighbouring Air were, as I found by manifest proofs, so cold, that I was not tempted to
impute

impute the unsuccessfulness of the Experiment, rather to its want of a sufficient coldness, then the water's. So that till I have an opportunity of making a further Trial, I cannot say more to the *Persian* way of augmenting ice. But to proceed, our having met with but an unsatisfactory Account of this Experiment, which we were the more troubled at, because this seem'd a promising way of trying that, which otherwise is not so easily reduc'd to Experiment; for the Temperature of the Air, must be seriously consider'd in assigning the Cause of divers trials, that may be made for the resolving of the same Question. For to omit other Examples, here in *England* we find, that water poured on snow, is wont to hasten the Dissolution of it, and not to be congeal'd by it; whereas having inquir'd of an Ingenious Person, that liv'd a good while among the *Russians*, he inform'd me, that it was their usual way to turn water and snow into ice, by pouring a convenient Proportion of that liquor into a great quantity of snow, and having

also inquir'd, whether ice had not the like operation, he told me, that 'twas usual, and he had seen it practis'd in *Muscovy*, to cement Ice to Buildings, and other things, and also to case over Bodies, as it were, with Ice, by gradually throwing water upon them. But I doubt, whether that Effect be to be ascrib'd barely to the Contiguity of the Ice, because I learn'd of him, that this way of increasing ice is practis'd in very frosty weather, when water thinly spread upon almost any other Body, would be frozen by the vehement sharpness of the Air.

7. The Glaciations, that nature unguided by Art, is wont to make, beginning at those parts of Bodies, at which they are expos'd to the Air, it usually happens, that they freeze from the upper towards the lower parts. But how far in Earth and Water (the most considerable Bodies, that are subject to be frozen) the frost will pierce downwards, though for some hints, it would afford, worth the knowing, is not easie to be defin'd, because the deepness of the frost

frost may be much varied by the degree of Coldness in the Air, by which the Glaciation seems to be produc'd, as also by the greater or lesser Duration of the frost, by the looser or closer texture of the Earth, by the nature of the Juices wherewith the Earth is imbued, and by the constitution of the subjacent, and more internal parts of the Earth, some of which send up either actually warm, or potentially hot and resolving steams, such as those that make corrosive liquors in the bowels of the Earth; so that the frost will not seiz upon, or at least cannot continue over Mines; and I have seen good large scopes of land, where vast quantities of good Lime-stone lay near the surface of the Earth, on which I have been assur'd by the Inhabitants, that the snow will not lye. There are divers other things, that may vary the depth to which the frost can penetrate into the ground, (I say, into the ground, because in most cases it will pierce deeper into the water.) But yet that we may not leave this part of the *History of Cold* altogether uncontribu-

ted to, we will add some of our Notes, whereby it will appear, that in our Climate the frost pierces far less into the ground, then many are pleas'd to think.

8. The notes I find about this matter are these that follow, which I transcrib'd unaltered, because 'twere tedious, and not worth while to add the way we imploy'd, and the cautions we us'd in making the observations, but we shall rather intimate, that the following trials were made in a Village about two miles from a great City.

[I. Jan. 22. After four nights of frost, that was taken notice of for very hard, we went into an Orchard, where the ground was level, and not covered with grass, and found by digging, that the frost had scarce pierc'd into the ground three inches and a half. And in a Garden nearer the house, we found not the Earth to be frozen more then two inches beneath its surface.

II. Nine or ten nights successive frost froze the grassless ground in the Garden, about six inches and a half,

or

or better in depth, and the grassless ground in the Orchard, where a wall shelter'd it from the south Sun, to the depth of about eight inches and a half, or better.]

[February the 9. we digg'd in an Orchard near a wall, that respects the North, and found the frost to have pierced the ground about a foot and two inches, at least above a foot: This is the eight day since it was about eight inches and a half.]

[A slender pipe of glass, about 18. inches long, and seal'd at one end, was thrust over night into a hole, purposely made with a Spit, straight down into the ground, the surface of the water being in the same level with that of the Earth, the next morning the Tube being taken out, the water appear'd frozen in the whole Capacity of the Cylinder, but a little more then three inches. But from this stick of ice, there reach'd downwards a part of a Cylinder of ice of about six inches in length, the rest of the water remain'd unfrozen, though it were an exceeding sharp night, preceded by a Constitution of

the Air, that had been very lasting, and very bitter. The Earth in the Garden, where this Trial was made, we guels'd to be frozen eight or ten inches deep, as it was in another place about the same house. But if this Tube had not been in the ground, the ambient Air would have frozen it quite through.]

9. Another Note much of the same import, we find in another place of our Collections.

Finding that by reason of the mildness of our Climate, I was scarce to hope for any much deeper Congelation of the Earth or Water, I appli'd my self to inquire of an Ingenious Man, that had been at *Musco*, whether he had observed any thing there to my present purpose, as also to find in Captain *James's* Voyage, whether that inquisitive Navigator had taken notice of any thing, that might inform me, how far the Cold was able to freeze the Earth or Water in the Island of *Charleton*, where that Quality may probably be supposed to have had as large a Sphere of Activity, as in almost any part of the

habis

habitable world : And by my Inquiries I found, that even in frozen Regions themselves, a congealing degree of Cold pierces nothing near so deep into the Earth and Sea, as one would imagine : For the Traveller, I spoke with, told me, that in a Garden in *Musco*, where he took notice of the thing I inquir'd about, he found not the ground to be frozen much above two foot deep. And in Captain *James's* Journal, the most that I find (and that too, where he gives an Account of the prodigiously tall ice they had in *January*) concerning the piercing of the frost into the ground, is this, that *The ground at Pag. 63.*
ten foot deep was frozen. Whence by the way we may gather how much sharper Cold may be presum'd to have reigned in that Island, then even in *Russia*. And as for the freezing of the water, He does in another place occasionally give us this memorable Account of it, where He relates the manner of the breaking up the Ice in the frozen Sea, that surrounds the Island we have been speaking of. *It is first to be noted (says he) that it doth Pag. 86.*

not freez (naturally) above six foot, the rest is by accident, such is that Ice, that you may see here six fathome thick. This we had manifest proof of by our digging the Ice out of the Ship, and by digging to our Anchors before the Ice broke up. The rest of that account not concerning our present purpose, I forbear to annex, only taking notice, that notwithstanding our lately mention'd Experiment of freezing water in a glais Tube thrust into the Ground, yet it seems, that at least where Captain James winter'd, the water was not much above half so thick frozen as the Earth. But we have already noted the indisposition of salt-water to congelation, and whether fresh water would not have been deeper frozen may be justly doubted.



Title XIV.

Experiments touching the differing Mediums through which Cold may be diffus'd.

1. **I**N examining whether Cold might be diffus'd through all *Mediums* indefinitely, notwithstanding their Compactness or the Closeness of their Texture, we must have a Care not to make our Trials with *Mediums* of too great thickness, lest we mistakingly impute that to the Nature of the *Medium* which is indeed caus'd by the distance which the *Medium* puts betwixt the Agent and the Patient. For the mixtures of Ice and Snow, wherewith we made our Experiments, will operate but at a very small distance, though the *Medium* resist no more then the common Air, as may appear by some of the Experi-

Experiments recorded in this Treatise.

This premis'd, we may proceed to relate, that having plac'd a copious mixture of ice and salt in Pipkins glaz'd within, and in white Balons glaz'd both within and without, we observ'd, that the outside of both those sorts of vessels was crufted over with ice: though, however the bak'd Earth had not been compact, nor the vitrifi'd fufaces of a very close Texture; the very thicknefs of the vessels was fo great, that it feem'd it would fcarce have been able to freez at a greater diftance.

2. By the Experiments formerly mention'd of freezing water in Pewter bottles, it appears, that Cold is able to operate through fuch mettalline vessels.

3. And this may be fomewhat confirm'd by one of the prettieft Experiments, that is to be perform'd by the help of Cold, namely, the making Icy Cups to drink in. The way we us'd was this; We caus'd to be made a Cup of *Lattin* (by which I mean Iron reduc'd into thin plates, and

and tinn'd over on both sides) of the shape and bigness I intended to have the Cup of ; then I caus'd to be made of the same matter another Cup of the same shape with the former, but every way less, so that it would go into the greater, and leave a competent interval for water, betwixt its convex surface, and the concave of the other. This innermost Cup was furnished with a rim or lip, by which it lean'd upon the greater, and by whose help its sides and bottom were easily plac'd at a just and even distance from the sides and bottom of the other ; but the Distance between the two bottoms is made greater, then that between the sides, that the icy Cup might stand the firmer, and last the longer. The interval between the two parts of this Mould being fill'd with water, and the Cavity of the internal Cup being fill'd with a mixture of ice and salt, (partly to freez the contiguous water, and thereby cooperate to the quicker making of the Cup, and partly by its weight to keep the water from buoying up so light a Cup,)
the

the external part was surrounded with ice and salt, whose Cold so powerfully penetrated to the internal metalline Mould, that the water was quickly frozen, and (the Parts of the Mould being disjoyn'd) appeared turn'd into an icy Cup of the bigness and figure design'd. And these Cups being easily to be made, and of various shapes (and that in the midst of Summer, if snow or ice be at hand) are very pleasant trifles, especially in hot weather, when they impart a very refreshing coolness to the drink poured into them, and though they last not long, especially if they be imploy'd to drink Wine, and such like spirituous Drinks in, yet whilst some are melting, others may be provided, and so the loss may be easily repair'd ; all the difficulty we met with, was to disjoyn the parts of the Mould which are wont to stick very fast to the ice they include. And we tri'd to obviate this, sometimes by annointing the inside of the Mould with some unctuous and not offensive matter, to hinder the Adhesion of the ice, and sometimes by applying

plying some convenient heat both to the convex part of the external, and the concave part of the internal piece of the Mould, which last mention'd way is quick and sure, but lessens the durableness of the Cup.

(We were lately inform'd, that this way of making Cups of Ice, is set down in *Berkley's Argenis*, and 'tis like enough, that ingenious Man may have learn'd it amongst some of the *Virtuosi* of *Italy* he convers'd with: But if we that learn'd it from none of them, had not been taught it by Experience, we should scarce have ventur'd to try it upon the Credit of a Romance; that sort of Composures being wont to be fabulous enough to pass but for Poems in Prose.)

4. The learned and industrious Mathematician *Erasmus Bartholinus*, mentions in his newly publish'd Discourse *de Figura Nivis*, an Experiment, by which he tells us, that some Masters of Nature's secrets, do easily, even in the midst of heat, reduce water into Air. For they put a little snow or ice into a Funnel, and thereby so refrigerate and condense the
ambient

ambient Air, that there will dew trickle down the sides of the Funnel: By which means it has been said, that some Ingenious Men have hop'd to make an artificial Fountain in the midst of Summer. But I here mention this Experiment *rather*, because 'tis not unlikely to please thole to whom 'tis new, and because having purposely tri'd it in large and thick funnels of glafs, it may be pertinent-ly enough deliver'd in this place, (where we are treating of the Trans- mission or Propagation of Cold, through close and thick *Mediums*,) *then* because we expect to make of it that use, especially that Oecono- mical use, that has been lately intima- ted. For first, 'twill be very hard to prove, that 'tis the very Air it self, and not rather the vapours swimming in it, that are by this means transmuted into water. And secondly, 'tis true indeed, that a mixture of snow and salt will condense vapours on the outside of a Funnel, but either they, that hop'd to make this use of the Ex- periment, have little Experience of it, and write conjecturally, or else they

they have made it with a success very differing from ours. For though, we imploy'd a large Funnel, and suspended it by a string (artificially enough tī'd about it) in the free Air: And though the mixture of ice and salt we put in, were sufficiently infrigidating (as will appear by and by) and far more so, then ice or snow alone would have been, yet that mixture being not able to condense the vaporous Parts of the Air into dew, much, if at all, longer then the mutual Dissolution of the salt and snow lasted, the liquor that was this way obtain'd, and dropp'd down at the bottom of the Funnel (whose internal Perforation ought to be carefully stopp'd, least any of the resolved snow and salt should fall through, and spoil the other liquor) was indeed sweet like rain water, but so very little, as well, as so slowly generated, that it amounted not any thing near to that which the snow, imploy'd and spoil'd to make it, would have afforded. So that it may be question'd, whether some cooling liquors, which can as well as this mixture condense the

the vapid Air into water, and whose Texture is not destroy'd in this operation, as that of the snow is, might not be more hopefully employ'd to obtain water from the Air; to which I shall only add this one thing, That the mixture of snow and salt did turn the vapours, that fasten themselves to the outside of the glass, first into Ice, before they dropt down in the form of water; in almost all our Trials of this nature, as well in thick Funnel, as in other and thinner glasses.

5. That in *Hermetically* seal'd glasses, an included mixture of snow and salt will freez the vapours of the Air on the outside of the glass, divers of the Experiments of the present Treatise do manifestly evince, which argue, that even so extremely close a *Medium* as Glasses, is not able to hinder the Transmission of Cold. And this is not superfluously added, because in vessels not *Hermetically* seal'd, it may be pretended, that 'tis the internal Air that communicates its Coldness by some unheeded, but immediate intercourse, with the external. After

After this we thought it worth an Experiment, to try, whether, or how, Cold would be diffused through a *Medium*, that some would think a *Vacuum*, and which to others would seem much less disposed to assist the Diffusion of Cold, then common Air it self; to compass this, the Expedient we bethought our selves of, was, to suspend a slender glass full of water in one of the small Receivers belonging to our Pneumatical Engine, and when the Air was very carefully pump'd out, to bury the exhausted Receiver in a copious and ready prepar'd mixture of Ice and Salt, to see, whether notwithstanding the withdrawing of the *Medium*, the water suspended in a kind of *Vacuum*, as to Air or gross substances would yet be frozen by the Cold. That Event of our trials, which alone I find among my Notes, is registred in these terms.

6. [A small pipe seal'd at one end, was, at the other, fill'd almost with water, and was put into a Receiver, consisting of a somewhat long and slender Tube of Glass, seal'd at one
 A a end,

end, and inverted upon the Engine plate, then the Air was carefully exhausted, for the pump was ply'd a while after no Air appear'd to come forth in any bubble out of the Receiver, through the external water; nor did the water in the small pipe within, disclose any number of bubbles worth taking notice of: then by the help of an almost Cylindrical plate of Iron, beaten Ice and Salt, were heap'd against the outside of the Receiver, about the height, to which the water in the small pipe reach'd. And at length, though, as we all thought, much more slowly than such a Congelation would else have been perform'd, the water was for the most part frozen in odd kind of flakes from the top to the bottom, and the ice seem'd not to have any considerable number of Bubbles.]

7. There is one Experiment, I have made about the Transmission of Cold through indispos'd *Mediums*, which may not be unworthy to be here inserted. For I had once a mind to try, whether a cold Body could operate through a *Medium*, that was, as

to touch, actually hot, and had its heat continually renew'd by a fountain, as it were, of heat, that perpetually diffus'd through it, new supplies of warm Liquor, so that the cold Body could not here, as in other cases, first allay the heat of the *Medium*, and then lessen it more and more, till it had quite extinguish'd it. To compass this, I had soon after an opportunity of making some trials presented me : For being at the Mineral Springs at *Tunbridge*, to drink those wholesome waters for my healths sake, I soon accustomed my self to drink them in considerable Quantities very early in the morning, when they were exceeding Cold, and sometimes drinking them in bed, as well as sometimes at the Springs-head, I had the Curiosity to observe, whether in case I took them down very fast, they would not through the warm Muscles and outward Parts of the Abdomen, diffuse a sensible Coldness ; and upon more Trials than one, I found, that by laying my warm hands on the outside of my Belly, I there felt at least, as it seem-

ed to me, a manifest and considerable Degree of Coldness. And when I related this to some ingenious Persons, that were better acquainted with those Springs than I, they told me, that there was among those many that then resorted to those famous Springs, a Knight, whose Name I remember not, whose Disease being judg'd formidable, the Physicians enjoyned him to drink in a morning two or three times the Quantity, that afforded me the Observation I was relating, and that when this Knight had fill'd his Belly with so much water, he us'd mightily to complain of the Coldness it diffus'd through his Abdomen, insomuch that he was fain to ply those parts long with hot Napkins clapp'd to them, one after another, which yet, as he complain'd, were soon refrigerated by the excessive Cold that the water diffus'd to the outside of his Belly, which yet nevertheless was not, that I could learn, at all prejudic'd, no more than mine, by so sensible and piercing a Cold.

8. It may be doubted, whether in
case

case water be not fluid upon the account of a congenite motion in the Corpulcles it consists of, its fluidness may not proceed from the agitation of the ambient Air, either immediately contiguous to the surface, or communicating its agitation to the water, by propagation of its Impulse through the vessel that interposes betwixt them. To contribute to the clearing of this, and some other things, we devis'd the following Experiment. We provided a glass-bubble of about the bigness of a Walnut, and the form almost of a Pear, whose stem was purposely made crooked for the conveniency of suspension. This being fill'd with water (which is troublesome enough to be done, unless one have the knack) we hung it at one end of a thread, whose other end we past through a Cork, by a perforation purposely made: into which, we afterwards fastned the thread, by thrusting in a small peg to rivet it in. Then filling a glass not very broad, but yet furnished with a mouth wide enough to receive the bubble, with oyl of Turpentine,

Unable to display this page

when we had taken it out, it appeared cleft very deep (from the outside almost to the centre) according to a line drawn from the slenderest part of it, almost as if one should with a knife cut a Pear in two, from the stalk downwards, according to its whole length. And these two pieces were easily enough separable, and (to adde that circumstance) for trial sake we left them divided in the same liquor and vessel, with some thawing Ice and Salt about them, for 14. or 15. hours, without finding them any thing near so much wasted or resolved into water, as most would have expected.

Whilest the above mentioned Bubble was exposed to be frozen, we likewise placed by it in another vessel a Glass-Egg, whose Ball and a little part of its stem we had fill'd with some of the very same parcel of oyl of Turpentine, and placing about the sides of this Egg some ice and salt, we observed, as we expected, that the liquor was, after a little while, made by the Cold to subside about half an inch, so that 'tis worth some

Philosophers considering, why, if according to the lately mention'd Atomical doctrine, Cold be made by the introduction of swarms of real and extended, though Atomical Bodies, they should pervade the oyl, and contract it without freezing it, but freeze the water without contracting it, but expending it rather.

9. [A small bubble of the bigness of a very little Nutmeg, fill'd with water, and Hermetically seal'd up, was by a cork and a string suspended in spirit of Wine, so as to be surrounded therewith, and being exposed to the Air the same night, in the stopt glass, was the next morning found altogether frozen, though the spirit of Wine it self were not at all so : But another bubble, by the help of a string Cork, and piece of Lead, carefully suspended in a strong solution of Sea-salt, and exposed at the same time in a like vessel with the former, when they both came to be look'd upon, appear'd to be no more frozen then the brine it self, which was not so at all,]

10. [A glass Bubble of the bigness

ness of a small Nutmeg, fill'd with water, and Hermetically seal'd, being immersed by a weight of Lead fastned to it, beneath the surface of a very salt Brine, but yet not so as to reach the bottom of the liquor or glass, was exposed all night to freez, in weather that was extraordinarily cold, but neither the imprison'd water, nor the other appeared to be at all frozen. The like Experiment we repeated another frosty night, but without freezing either of the liquors. But to show the usefulness of repeating Experiments about Cold, if there be opportunity, and especially in such cases, where the degree or some other circumstance may much vary the event, we will add, that having exposed a Bubble like that newly mention'd, and immers'd in spirit of Wine, we found the next morning the water in the bubble turn'd into ice, and having likewise exposed such a bubble immers'd in very strong Brine, to be frozen by a mixture of ice and salt, within about two hours after, we found the bubble broken,

as

as we suppos'd, upon the Expansion of the water upon its growing Ice. And we also found the upper part of the bubble with the Ice sticking to it, and the other part of the glais was crack'd, with lines running from a point almost like the Pole and Meridian in a Globe, whence we concluded the glais to have been, as 'tis probable, burst asunder upon the Expansion of the fresh water into ice, and that the Reason why there remain'd but a comparatively little parcel of ice, was probably, that the salt water getting in at those crannies or chinks, dissolved as much of the new made ice, as in a little while it could easily reach.]

Besides,

11. [We fill'd a glais bubble with fair water, and having Hermetically seal'd it, we suspended it by a string fastned to the cork in the cavity of a wide mouth'd glais, well stop'd, so that the bubble was every way at a good distance from the sides, bottom, and top of the glais. This we did to try, whether a sufficient degree

gree of Cold at that distance,
would be freely transmitted through
the glass, without the intervention of
a visible liquor, and accordingly we
found the suspended Bubble crack'd
by the ice that fill'd it.]

Experiments and Observations

on the freezing of Water

in various Circumstances

By Mr. John Hadley

Delivered at the Publick Lectures of the Royal Society

in the Theatre of Experiments

in the City of London

By Mr. John Hadley

Delivered at the Publick Lectures of the Royal Society

in the Theatre of Experiments

in the City of London

By Mr. John Hadley

Delivered at the Publick Lectures of the Royal Society

in the Theatre of Experiments

in the City of London

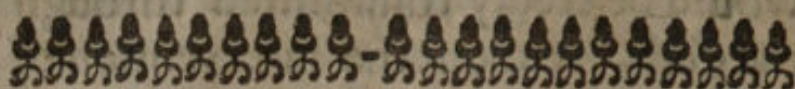
By Mr. John Hadley

Delivered at the Publick Lectures of the Royal Society

in the Theatre of Experiments

in the City of London

Title



Title XV.

Experiments and Observations touching Ice.

1. **A** Great part of our present History, being imploy'd about delivering the *Phænomena* of Congelation, it is not to be expected, that in this Section, where we treat of Ice as a distinct part of our Theme, we should deliver all those particulars, that have occur'd to us, wherein ice is concern'd. And therefore we shall restrain our selves to the mention of those, that belong to ice, considered, as it consists of intire and distinct Portions of congeled water. And though we shall deliver some few Experiments of our own, such as we had any opportunity to make, yet much the greater part of this Section will fitly enough be taken up by Collections

ons

ons out of Travellers, and Navigators, into those Colder Regions, that afford much considerabler, or at least much stranger Observations concerning ice, then are to be met with in so temperate a Climate as ours. And what we have to deliver in this Section, will naturally be divided into two parts, the one consisting of our own Experiments, and the other containing some Passages, that we have selected out of Voyages, or that have been afforded us by the Relations of credible Travellers. And of these two sorts of Observables, that which has been first mention'd shall be first treated of.

2. Some that have been in the *East Indies* inform us, that in some parts of those Countries, they were looked upon as great Liars, for affirming, that in *Europe* the fluid body of water, was often without any artifice or endeavour of Man, turned in a few hours into a solid and compact Body, such as Ice. And certainly, if custom did not take away the strangeness of it, it would to us also appear very wonderful, that so great a change

change of Texture should be so easily and inartificially produced. But how solid the Body of ice is, or rather how strong is the mutual adhesion of its parts, has not yet, that we know of, been attempted by Experiments to be reduced to some kind of Estimate; and indeed so many things must be taken into consideration, that it will be difficult to arrive at any more than a fair conjecture in this matter; especially, because (I think) it may justly be doubted, whether or no differing degrees of Cold may not vary the degree of compactness of the ice, and my doubt will not perhaps appear groundless, if I add, that having, to satisfy myself, inquired of an intelligent Person, that liv'd some years in *Russia*, he answered me, that he found the ice of those parts to be much harder then that of these.

3. We had in our thoughts divers ways to Estimate the cohesion of the parts of ice, whereof one was, to freeze water in a hollow metalline Cylinder, and taking out the ice, and keeping it in a Perpendicular posture

sture cast into a scale weigh'd before-hand, and carefully fastned to the bottom of the ice, more and more weight, till the mere weight broke the Cylinder, and this we had thoughts to try in Cylinders of differing Diameters and lengths, but wanted conveniencies to make the Experiments; (which if they were made (as some of our Trials were) in the open Air, and in places exposed to some gelid wind, it would the better secure the ice from being weakned or thaw'd during the Trials.)

4. We therefore attempted by another way, to investigate the strength of ice. For we took a plate of it, of an uniorm, and also of a considerable thickness, and with sides cut parallel, that it might serve for a kind of leaver, and plac'd it betwixt two wooden Bars, whose distance we knew, and then laying on it a great weight, the Centre of whose Pression, as near as we could estimate, was equally, or in determinate measures, distant from the wooden *fulcrums*: we endeavoured to try, how great

great a weight it would support ; but in the Village, where we made the trials, we could not get weights that were conveniently shap'd, and ponderous enough, to break it, and though we caused a Man to stand upon it, yet neither could his weight break it, till he chanced to add an impressed force with his foot, to the weight of his Body. So that being unable to determine, what that additional and impressed force might amount to, almost all that we could safely conclude, either from this Experiment, or some other ways of trial with scales, and other ways that we made use of (but for want of conveniencies unsuccessfully) was, that the force of ice to support weights, is much greater then men are wont to imagine, which seems somewhat the more strange, because it is not here in *England* so solid a Body, as by this one would guess : for not only glass would readily scratch it deep enough, but even with common Knives we would cut it, and that with great ease.

5. Yet one not inconsiderable Account

count I was able to give my self of the strength of ice, which I find in my Notes thus delivered.

[There was taken a piece of ice three inches long, and three broad, and somewhat less then a quarter of an inch thick; this was laid cross ways upon a frame, so that the two parts, on which the ice lean'd, were distant three inches, then there was taken an Iron, shap'd like the figure of (the common Arithmetical Cypher, that denotes *Seven*) 7, to whose hanging leg, if I may so call it, there was fastned at the end, which was under the middle of the ice, a scale, into which several weights were put, such as by some former Trials we guess'd to be almost as much as the ice would well suffer, after which the horizontal leg of the Iron was very gently laid upon the ice, as near as we could guess, in the middle of the distance, between the two sides of the frame, and consequently parallel to them both, then the weights not proving altogether sufficient to break the ice, we let them hang a while at it, and observ'd how

* The
breadth
was, I
know not
how, omit-
ted in the
note, but as
I remem-
ber, it was
about an
8. part of
an Inch.

the edge of the incumbent leg of Iron (which edge was * broad) did work it self downwards into the ice, so that by our guess, when the ice broke, as after a while it did, it had lost at one end of the Incision, if I may so call it, half its thickness, and at the other, about a third part of it.

The weights that broke it, amounted to 17.pounds *Haberdupois*, and 117. ounces *Troy*.

6. The Experiment was repeated with all the former circumstances, only the piece of Ice was two inches and a half broad, and a quarter of an inch thick, the distance of the frame was three inches, as before, the weights that broke it, were 17.pounds *Haberdupois*, and 48. ounces *Troy*. The horizontal arm of the iron had melted somewhat more then half through the ice when it broke, viz. more then $\frac{2}{3}$ of the thickness at one end, and somewhat less then half at the other.

7. We divers times intimated in some of the first Sections of our present History, that the addition of salt to Ice, did hasten the dissolution of it, which

which though it may be easily proved by some other *Phænomena* of our Experiments, yet it will not be amiss to mention here a couple of particular trials, by which we have more manifestly evinc'd it: And first, we divers times took a broad and flat plate of ice, less then a $\frac{1}{4}$ of an inch thick, and having placed it horizontally upon a joyn'd-stool, (a table, or any other flat piece of wood will do as well) we strewed here and there a convenient quantity of Bay-salt upon it, and though we observed, that, if the surfaces of the ice and stool, were not both of them flat, and congruous enough, the ice would be thaw'd indeed, but the other part of the Experiment would not well succeed; yet when we made the trial carefully, and watchfully, the plate of ice partly thaw'd by the salt, would be so firmly frozen to the stool it leaned on, that we were fain with an iron instrument, to knock it all to pieces, before we could sever it from the stool, into whose pores the ice newly generated by the Experiment, did pierce so deep, that notwithstanding

our knocking, many little parcels of ice would continue to stick close to the wood, whose pores they had invaded. But the circumstances which in this Experiment made the most to our purpose, are these two; The one, that having sometimes laid the salt but on few, and somewhat distant parts of the plate, the intermediate parts would many of them remain unfrozen to the stool, whilest those, where the salt had been laid, were frozen so hard to it. And the other circumstance is, that the grosser grains of salt, would so far dissolve the ice whereto they were contiguous, as (if I may so speak) to bury themselves therein, whilest the other parts of the ice, upon which, or near which, no salt had been laid, kept their surfaces smooth and intire. We tried likewise two or three times to freeze a plate of ice to a flat piece of wood, by making use of *Aqua fortis*, instead of common salt, but the Experiment succeeded not well, though once we brought the ice to stick to the wood manifestly, but not strongly.

8. To this we shall add, the following Experiment, which when we watchfully made it, succeeded well, and I find it among my notes set down in these terms.

[Solid fragments of ice having pretty store of salt thrown on them, upon the first falling of the salt among the ice, there was produced a little crackling noise, and for a good while after there manifestly ascended out of several parts of the mixture, conveniently held betwixt a candle and the eye, a steam or smoak, like that of warm meat, though the night were rainy and warm, and though the morning had not been frosty.]

The mention here made of the crackling noise made by the ice upon the addition of salt, (which seemed to proceed from the crackling of the brittle ice, produc'd by the operation of the salt upon it) brings into my mind an Experiment I had formerly made, whereof a greater noise of the same kind is a *Phænomenon* : though the Experiment were chiefly made for the Discovery of the texture of Ice : The event of the trial I find thus set down among my notes.

9. We

9. [We took some cakes of ice, each of the thickness between an $\frac{1}{8}$ and a $\frac{1}{4}$ part of an inch, but not so very compact ice, as to be free from store of bubbles; some good *Aqua fortis* dropp'd upon this, did quickly penetrate it with a noise, that seem'd to be the cracking of the ice, underneath which the sower liquor was very plainly to be tasted; Oyl of Vitriol did the same, but much more powerfully, and without seeming to crack the ice which it pass'd through; so that though but three or four drops were let fall upon the plate, it immediately shew'd it self in drops exceedingly corrosive on the other side of the ice. And the like success we had with a trial made with the same liquor upon three such plates of ice frozen one upon the top of another.]

10. Having proceeded as far as we were able towards the bringing the strength of ice to some kind of Estimate, by such Experiments as we had opportunity to make here, we thought it not amiss to seek what information we could get about this matter among the Descriptions that are

are given us of Cold Regions: But I have not yet found any thing to have been taken notice of to this purpose worth transcribing, except a passage in the Arch-Bishop of *Upsal*, wherein though the estimate of the force of Ice be, as we shall by and by show, made after a gross manner, yet since this it self is more then I have met with elsewhere, I think it worth subjoyning, as our Author delivers it in these terms: *Glacies* (says

*Olaus Ma.
Gent. Sep-
tentr. Hist.
Lib. 1.
Cap. 14.*

he) *primæ & mediæ hyemis adeò fortis & tenax est, ut spissitudine seu densitate duorum digitorum sufferat hominem Ambulantem, trium vero digitorum equestrem Armatum; unius palmæ & dimidiæ, turmas, vel exercitus militares; trium vel quatuor palmarum integram Legionem seu myriadem populorum, quemadmodum inferius de bellis Hyemalibus memorandum erit.*

But though this be sufficient to afford us an illustrious Testimony of the wonderful strong cohesion of the parts of ice, yet we mention'd it but as a popular way of estimate, which may better embolden Travellers, then satisfy Philosophers, in regard

that the Author determines only the thickness of the ice, and not the distance of that part of it, that supports the weight from the shore or brink, on which, as on a *Hypomochlion*, the remotest part of the ice does lean or rest. And if we consider the ice as a Lever, and the Brink or Brinks on which it is supported, as a single or double *fulcrum*, the distance of the weight may be of very great moment in reference to its pressure or gravitation on the ice, which may much more easily support the weight of divers men plac'd very near the prop, then that of one man plac'd at a great distance from it, as will be easily granted by those, that are not strangers to the Mechanicks, especially to the nature and properties of the several kinds of Levers. But not now to debate, whether in certain cases, the ice we speak of, may not receive some support from the subjacent water, nor whether some other circumstances may not sometimes be able to alter the case a little, our very considering the ice as a single or double Lever, though it may hinder us

us from measuring the determinate strength of ice upon *Olaus's* Observation, yet it will set forth the strength of it so much the more, since by his indefinite expressions he seems sufficiently to intimate, that when the ice has attain'd such a thickness, its resistance is equivalent to such a weight, without examining on what part of the ice it chances to be placed.

II. Thus far our Experiments concerning ice (with the Appendix subjoyned out of *Olaus* to the same purpose.) We will now proceed to some of the observations we have met with in Seamen's Journals, and elsewhere. I say to *some*, because to enumerate them all, would spend more time and labour than I can afford, and therefore I shall restrain my self to the mention of some few of the chiefest.

I. And in the first place for confirmation of what I deliver'd at the beginning of this Section, from the report of a Traveller into *Russia*, touching the hardness of ice in those gelid Climates, in comparison of our ice, which I have found it easie
to

Purchas.
Lib. 4.
Cap. 13.

to scrape with glasse, or to cut with a knife ; I shall subjoyn this passage of Captain *G. Weymouth*, in his Voyage for the Discovery of the Northwest passage. *As we were (says he) breaking off some of this Ice, which was very painful for us to do, for it was almost as hard as a rock, &c.*

Purchas.
lib. 4. cap.
13. pag.
813.

II. Next to shew, that it was not a superfluous wariness, that made me in a former Section doubt, that even the ice made of Sea-water might be altogether or almost insipid ; I will subjoyn, that I have since met with some Relations, that seem to justifie what is there deliver'd. And in one of our Englishmens Voyages into the Northern Seas, I find more then one instance to my present purpose, though I shall here set down but one, which is so full and expresse, that it needs no companions : Our Navigator speaking thus ; *About nine of the Clock in the forenoon, we came by a great Island of Ice, and by this Island we found some pieces of Ice broken off from the said Island, and being in great want of fresh water, we hoysed out our Boats of both ships, and loaded them twice with Ice, which*

which made us very good fresh water.

But all this notwithstanding, I yet retain some scruple, till those that have better opportunity to make a more satisfactory Experiment shall ease me of it. For though by these Narratives it seems more then probable, that the ice in the midst of the Sea consists but of the fresh Particles of water, that plentifully concur to compose the Sea water, yet besides that, in case the fresh water were taken, as some of that, I have found mentioned in Voyages, has confessedly been, from the top of the ice, it might possibly be no more then melted snow, which, as we elsewhere take notice, does in those extremely cold Regions easily freeze upon the ice it falls on, and oftentimes much increases the height of it: Besides this, I say, the Argument from the insipidness of the resolved ice, will conclude but upon supposition, that as that ice was found in the Sea, so it was also made of the Sea water; which though it may have been, yet I somewhat doubt, whether it were or no, since I find some Navigators of the
most

most conversant in the cold Climates to inform us, That most of those vast Quantities of ice that are to be met with about *Nova Zembla*, and the strait of *Weigats*, and that choke up some other passages, whereby men have attempted to pass into the south Sea, are compos'd of the accumulation of numerous pieces of ice (cemented together by cold water) that are brought down from the great River *Oby*, and others, so that it may

* Neither hereafter will I marvel, though the strait of *Weigats* be stopped up to the Northeast,

* very well be suppos'd, that these mountainous pieces of ice may be some of these, which, upon the shattering of ice in Bays and straits, partly by the heat of the Sun, and partly by the Tides, may be afterwards by the winds and currents dri-

with such huge mountains of Ice, since the Rivers *Oby* and *Jenesce*, and very many more, whose names are not yet known, pour out such a quantity thereof, that in a manner it is incredible: For it cometh to pass in the beginning of the Spring, that in places near unto the Sea, the Ice through the excessive thickness, and multitude thereof, doth carry down wood before it. And without doubt this is the cause, that about the shore of the Strait of *Weigats*, so great abundance of floating wood is every where seen: and whereas in that Strait near unto *Nova Zembla*, it is so extreme Cold; it is no marvel, if in regard of the narrowness of the strait, so huge heaps of Ice are gathered and frozen together, that in the end they grow to sixty, or at least to fifty fathomes thickness: Says the Description of the Countreys of *Siberia*, *Samojeda*, &c. extant in Purchas's third part of his Pilgrim. Lib. 3. Cap. 7.

ven

ven all up and down the Seas, to parts very distant from the shore, and some of these it may be, that our Countreymen met with, and obtain'd their fresh water from : Which I the rather incline to think, because that (as we shall have occasion to observe in another Section) the main Sea it self is seldom or never frozen. But my scope in all this, is, but to propose a scruple, not an opinion.

III. The next and principal thing concerning ice, is the bigness of it, which I find, by the Relations partly of some Acquaintances of my own, and partly of some Navigators into the North, to be sometimes not only prodigious, but now and then scarce credible. And therefore, as I shall mention but few instances, that I have selected out of the best Journals, and other writings I have met with, so I shall add a few more Testimonies to keep them by their mutual support, from being entertain'd with a Disbelief, which their strangeness would else tempt men to.

Of the vastness of single mountains of ice, the most stupendious Example,

ple, that for ought I know, is to be met with in any language but ours, is that, which I formerly took notice of out of the Dutch Voyage to *Nova Zembla*, which was ninety six foot high (that is above twenty foot higher, than on a certain occasion I found the Leads of *Westminster Abbey* to be.) But 'tis probable, that our Captain *James* met with as great, if not greater: For though in some places he mentions divers hills of Ice, that were aground in 40. fathom water, and consequently were as deep under water, as that newly taken notice of out of the *Hollanders*: And though he elsewhere mentions other pieces of no less depth, and twice as high as his top-Mast head, and this in *June*, yet elsewhere, and long after relating his return home, he has this passage; *We have sail'd through much mountainous Ice far higher then our Top-Mast head: But this day we sail'd by the highest that I ever yet saw, which was incredible indeed to be related.*

Page. 14.

Page. 106.

But the stupendiouslest piece (for heighth and depth) of single Ice, that perhaps has been ever observ'd and measur'd

measur'd by men, is that which our Famous English Seaman Mr. *W. Baffin* (whole name is to be met with in many modern Maps and Globes) mentions himself to have met with upon the coast of *Greenland*, whose whole Relation I shall therefore subjoyn, not only because of the stupendiousness of this piece of ice, but because he takes notice of an observation, which I knew not to have been made by any, and comes somewhat near the estimate, we formerly made, of the proportion betwixt the extant and immers'd parts of floating ice, only the following Estimate makes the extant part somewhat greater then we did, which may easily proceed from other mens having, as Mr. *Baffin* here does, grounded their computation upon what occur'd to them at Sea, or in salt water, where the ice must sink less, then in fresh water, such as my Estimate suppos'd. Our Navigators words then are these,

The 17. of May we sail'd by many great Purchas. Islands of Ice, some of which were above ^{lib. 4. cap.} *200. foot high above water, as I prov'd* ^{18. pag.} *by one shortly after, which I found to be* ^{837.}

240. foot high, and if the report of some men be true, which affirms, that there is but one seventh part of Ice above water, then the height of that piece of Ice which I observed was one hundred and forty fathoms, or one thousand six hundred and eighty foot from the top to the bottom. This proportion I know doth hold in much Ice, but whether it do so in all, I know not.

Thus far of the height and depth of single pieces of ice: as for the other Dimensions (the length and breadth) I remember not, that I have read of any, that had the Curiosity to measure the extent of any of them, excepting Captain James, whose Ship being once arrested, between some flat and extraordinary large pieces of ice, he and his men went out to walk upon them, and he took the pains to measure some of the pieces, which he says he found to be a 1000. of his paces long. And probably among so many mountains and Islands of ice, there would have been found some intire pieces, of a greater extent then even these, if men had had the curiosity to measure them.

Pag. 17.

Hitherto we have treated of the
bignels

bigness of single pieces of ice, we will now proceed to say something of the dimensions of the aggregates of many of them, among which having selected four or five as the principal, I remember my self to have yet met with, I presume it will be sufficient to subjoyn them only.

About ten of the clock we met with a mighty bank of ice, being by supposition seven or eight leagues, or twenty four miles long, (says that experienced English Pilot James Hall, in his Voyage of Denmark for the discovery of Greenland.)

Another of our English Navigators mentions, that even in *June all the Sea* (wherein he was endeavouring to sail) as far as he could see from the top of a high hill, was covered with ice, saving that within a quarter of a mile of the shore it was clear round about once in a Tide. By which last clause, it seems, that this vast extent of ice, was either one intire floating Island, or at least a vast bank or rand (as some Seamen term it) of ice.

But the strangest account of banks of ice, that I have yet met with in

any sober Author, is that which is mention'd by the learned French *Hydrographer, Fournier*, who relates, that in the year 1635. the French fleet sailing to *Canada*, met with several pieces of ice, as high as steeples, and particularly one, whether piece or bank of ice (for the French word *Glace* may signifie either) which they were troubled to coast along for above forty leagues. If this be the same story, (as one may suspect it to be, by the circumstances of the place, and fleet,) there is a great mistake in another place, where our Author speaks of the vastness of the ice: but if it be another story (as some differing circumstances argue) the French it seems met with ice far more stupendious, then even that already mentioned. For, (says our Author) in the Sea which washes *Canada*, there is often seen, even in the moneth of *August*, to pass by, Ices much bigger then Ships. In the year 1635. the French Fleet sailing there, coasted along, for three days and three nights, one that was above 80. leagues long, flat in some places like vast

*Hydrographie du P.
G. Fournier, liv. 9.
cap. 29.
compar'd
with the
22. Chap.
of the
same
Book.*

vast Champions, and high in others like frightful hills. The latter part of which passage may confirm what we formerly deliver'd in another Section, concerning the unequal compagination of Icy Islands.

To what has been said touching the extent, and other dimensions of floating, or at least loose pieces of ice, it will be fit to add something of the extent of ice, coherent to one or both of those shores, that bound the water, whose upper part is congeal'd. And in the first place, we shall out of many instances to our present purpose, that might be borrowed from the writings of *Olaus Magnus*, select this one memorable one that shall serve for all : *Neque minori bellandi impetu (says he) Sueci ac Gothi super aperta glacie, quam in ipsa solidissima terra confligunt; imo, ut prius dictum est, ubi antea aestivo tempore acerrima commissa sunt bella Navalia, eisdem in locis glacie concreta, aciebus militari modo instructis, Bombardis ordinatis, habentur horrendi conflictus. Adeo solida glacies est in equestribus turmis sufferendis, ampliter vel strictè collocatis.*

*Olai Mag.
lib. 3. cap.
2. pag. 334*

I pretermitt then, what he elsewhere relates of the Voyages and Wars made in Winter by the Northern Nations. They that have liv'd in those Countries, relate, as things most known and familiar (what has been confirmed to me by more then one unsuspected eye witness) the long Journeys that are commonly taken upon the Icy Bridges, or rather plains, by travellers, with all their Carriages to very distant places. And that which may bring credit to these strange relations, by shewing, that no less unlikely ones are sometimes true, is, what all *Europe* knows, that within these three years the whole *Swedish* Army, led on by their King, march'd over the Sea to the Island of *Zeeland*, where *Copenhagen* the Capital City of *Denmark* stands. * But it

* — Sæpe
aliàs &
his annis
fatalibus
tam pro-
fundè con-
gelavit
(marina

Aqua) ut non tantùm plaustra, sed integrum exercitum ad aliquot Milliaria Germanica secure vexerit, &c. Inquit T. Barthol. De nivis usu, pag. 43.

the

the Cold, been congeal'd to a prodigious breadth. *Insolitum est*, (saith Bartholinus) *quod refert Constantinus Manasses in Annalibus accidisse, Theophilus imperante, ut hyems sæva mare congeret in glaciem ad profunditatem sanè immensam, humidumque illud Elementum, Lapidis ad duritiem, fluxione prorsus ademptâ, redigeret.* And Michael Glycas relates, *That in the year 775. the Winter was so sharp in the East, that along the Coast, the Sea (he means the Mediterranean) was frozen for 50. leagues, and the Ice was compacted as into a rock, 30. Cubits deep; so strange a Quantity of snow, likewise falling, that it was rais'd to the height of 30. Cubits above the Ice, which likewise agrees very well with what we formerly noted, touching the possible increase of the height of some pieces of ice by the falling of the snow upon them.*

*Barthol. de
nivis usu,
cap. 6.*

*Glycas apud Four-
nier, liv. 9.
cap. 19.*

IV. It remains now, that we subjoyn a few promiscuous observations concerning ice, that are not so readily reducible to the three foregoing heads.

And we shall begin with what was taken notice of by the Dutch in their

Nova Zembla Voyage, where relating how they fastned their Ships to a great piece of ice, to shelter themselves from the stormy winds, There (add they) we went upon the ice, and wondred much thereat, it was such manner of Ice: for on the top it was full of earth, and there was found about forty eggs, and it was not like other ice, for it was of a perfect Azure colour, like to the skies, whereby there grew great contention of words amongst our men, some saying that it was ice, others that it was frozen land; for it lay unreasonable high above the water, it was at least eighteen fathom under the water, close to the ground, and ten fathom above the water.

In the Evening we were inclosed amongst great pieces (of Ice) as high as our Poop, and some of the sharp blew corners of them did reach quite under us.
Capt. Jam. pag. 6.

The like blew colour in rocky pieces of ice, I remember I have somewhere found, to have been taken notice of by a modern Navigator, or whether the words of *Virgil*, concerning the frigid Zone, *Cerulea glacie concreta, atque imbris atris*, belong to this subject, I leave others to consider, nor shall I stay to examine, whether this blewness, that has been observ'd in ice, be always an inherent or permanent colour, or else sometimes

times one of those that are styl'd Emphatical.

'Tis very considerable, if it be true, what is related by *Olaus Magnus*, concerning the degenerating (if I may so speak) of ice, from its wonted hardness in the Spring of the year. For in the same Chapter, where he gives us the lately transcribed account of the strength of Ice in those Northern Countries, after having interpos'd some other passages, he subjoyns these words; *Liquescente tamen glacie ad principium Aprilis, nullus ejus spissitudini, minus fortitudini, nisi in aurora, ambulando confidit, quia solis diurno aspectu tam fragilis redditur, ut quæ equestres armatos paulo ante portas verat, vix hominem nunc sufferre possit inermem.* *Olaus lib. I. cap. 14.*

This puts me in mind to add, that oftentimes in the writers of Journies and Voyages, we meet with mention of great noises made by the breaking of ice, and in this very Chapter our Archbishop taking notice of the clefts that sometimes happen in Champions of ice, adds, *That when the ice chances thus to open, especially if*

it be in the night, the noise of it may be heard a far off, like the loud and horrid noise of thunder, and of earthquakes. And on this occasion may be subjoyned a couple of passages extant in different places of the formerly mention'd James Hall's Voyages: The first is thus delivered; when we met with a huge and high Island of ice, we steering hard to board the same, and being shot a little too Northwards of it, there fell from the top thereof, some quantity of ice, which in the fall did make such a noise, as though it had been the report of five Cannons. But the next passage is more directly pertinent to our present subject, and is couch'd in these words; About twelve of the clock this night, it being still calm, we found our selves suddenly compassed round about with great Islands of ice, which made such a hideous noise, as was most wonderful, so that by no means we could double the same to the Westward, wherefore, &c.

Of these kind of *icy thunders* (as some travellers call them) there are divers instances to be met with, mention'd in the several Voyages of the *Hollanders*, & particularly in those

to *Nova Zembla*: But many of those noises seem to be made by the dashing of the great pieces of ice against one another: But if it happen, when the ice (as sometimes it is said to do) seems to cleave, as it were, of its own accord; to us that live in a temperate Climate, it may be a matter of some dispute, whence these loud ruptures of ice may proceed. For *Olaus Magnus*, in the Chapter above cited, does not improbably ascribe them to the warm exhalations, that in some places ascend out of the ground. And I remember, in favour of this opinion, that I once caused divers pieces of thick ice to be brought out of a cool place into a somewhat warm room, and listening, observ'd a noise to come from them, as if it had been produced by store of little cracks made in them, but somewhat or other prevented me from repeating the Experiment, and satisfying my self about the Conjecture. But having lately inquired of an intelligent *Polander*, that has travelled much upon these icy plains, he agreed with our Author, and others,

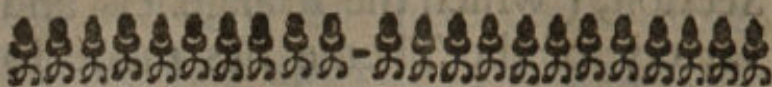
others, as to the frightful noise, that are produc'd by these cracks of ice, but affirm'd upon his own observation (for that I particularly inquired after) that these great clefts were often made, not by thawing heat, but by excessive cold, and that he had taken notice of them in extremely sharp weather. Indeed we sometimes observe, that in very bitter frosts the frozen ground will cleave, as we elsewhere have occasion to take notice. But whether that be not a different case from this, or whether the *Polonian* Gentleman were not mistaken, or whether both these mention'd accounts of the cleaving of ice, may on different conjunctures of circumstances take place, we leave to farther inquiry.

*Olaus
Magnus
lib. 11. &
Bleskenius
in Purch.
lib. 3. cap.
22.*

There is a tradition concerning ice, about the famous *Volcan-Hecla*, in *Island*, which, though verily believ'd among the superstitious vulgar of those parts, is spoken of so slightly by *Bleskenius*, who being upon that coast, had the curiosity to sail purposely thither, that I think it not worth while to take any farther notice of it. But
twere

'twere too tedious to set down in this Section, (which the strangeness and variety of the Theme has made so prolix already) the other things, that may be mentioned without impertinency concerning ice; and therefore we shall here desist from so laborious a task, as also omit the handling of snow and hail: For though they are reducible to ice, yet I shall at least suspend the treating of them, partly because *Bartholinus* and *Meteorologists* have sav'd much of my labour, and partly for the reason newly intimated, so that we shall conclude this Section as soon as we have taken notice, that there is yet somewhat relating to ice, which, being in itself considerable, and whereof hitherto no experimental account appears to have been given, what we our selves have tried about it, may challenge to be treated of apart.

Title



Title XVI.

*Experiments and Observations
touching the duration of Ice
and Snow, and the destroy-
ing of them by the Air
and several Liquors.*

1. **I**T may be an Experiment, as well instructive as new, to determine, what liquor dissolves ice sooner than others, and in what proportion of quickness the solutions in the several liquors are made. For Men have hitherto contented themselves to suspect in general, that there are other liquors potentially hot, wherein ice will sooner dissolve, than it will in water. But this opinion either being grounded upon no Experience at all, or taken up upon the sight of what happens to pieces of ice, which no care was taken to re-
duce

duce to the same bulk and figure, no more then to measure attentively how long one outlasted the other; we thought fit to try, if we could not bring this matter to Experiment, and make a determination in it, though not exactly true, yet *less* remote from exactness then had been yet, for ought I know, so much as attempted.

2. In order to this we procured some bullet moulds, and having first carefully stopped the little Crevise, that is wont to remain betwixt the two halves of the mould, with a good close Cement, we afterwards filled them with water, and carefully closed up the orifice of the hole, at which the water was poured in, and then setting the mould to freez in ice and salt, we found it difficult enough to keep the water (more or less of it) from running away through some unperceiv'd passage, before the cold could have time by congealing it to arrest it. But after a while, when we had thus made a bullet of ice, we found it a new and greater difficulty to get it whole out of the moulds, with-

Unable to display this page

mit a big mans little finger, this glass
 being stop't at one end, and kept
 open at the other, was filled to the
 height of about half a foot or more of
 fair water; and ice, and salt, being
 heaped up about it, that the cold
 might reach as far as the water did,
 it was quickly frozen. In the mean
 while, I had caused several wide
 mouth'd glasses to be brought into
 my Chamber (wherein, by reason
 of some indisposition, that hindered me
 from going abroad, I kept some fire)
 and having poured several liquors
 into these glasses, which had been
 placed all on a row, we suffered them
 to rest there a while, that the ambi-
 ent Air might have time to reduce
 them, as far as it could, to its tem-
 per, and consequently to the same
 temper as to heat and cold, and then
 with the warmth of ones hand, the
 included ice being loosened from the
 glass, as it was taken out, and a ru-
 ler divided into inches and eights,
 being laid alongst it, with a knife a
 little warmed, the ice was soon, and
 yet not carelessly, divided into several
small Cylinders of three quarters of
 an

an inch, a piece ; and these **Cylinders** thus reduced to as sensible an equality as we could, were nimbly and carefully put into the several liquors hereafter to be mentioned, and whilest we our selves watched very attentively, till each of these icy **Cylinders** was *quite*, and yet but *just* dissolved, we caused others to keep time by the help of a *Pendulum*, whose Vibrations were each a second minute (or 60. part of a Common Minute, whereof 60. go to make an hour) and it was easie for those we appointed, to watch the Vibrations of the *Pendulum*, notwithstanding the Quickness of its Motion, because it was fitted to a little Instrument purposely contrived for such nice observations, wherein a long Index moving upon a divided Dyal plate, did very manifestly point out the number of the *Diadromes* made by the *Pendulum*.

3. This Experiment was afterwards repeated twice with **Cylinders** of ice, each of them an inch long, and though the successes of these trials were various enough, yet we shall subjoyn both

both the last, (as being made with more advantage then the first) that the more light may be gathered from them, and that at least we may discover how difficult it is to make such Experiments in this matter, as that all the nice circumstances of them may safely be relied on.

I. *Trial.*

1. Oyl of Vitriol, where a Cylinder of Ice, of an inch long, being put into, lasted 5. minutes.

2. Spirit of Wine, (in which the ice sunk) lasted 12. minutes.

3. *Aqua fortis* lasted 12½ minutes.

4. Water lasted about 12. minutes.

5. Oyl of Turpentine lasted (not good) 44. minutes.

6. Air lasted 64. minutes.

II. *Trial.*

1. In Oyl of Vitriol, where an inch of Cylindrical ice lasted 3. minutes.

2. In Spirit of Wine, lasted 13. minutes.

3. In Water, lasted 26. minutes.

4. In Oyl of Turpentine, lasted 47. minutes.

5. In Sallet Oyl, lasted 52. minutes.

6. In the Air, lasted 152. minutes.

4. We likewise thought it worth trying, whether there would be any difference, and how much difference there would be in the Duration of pieces of ice of the same bulk and figure, some of them made of common water, and others of frozen Wine, Milk, Oyl, Urine, and other spirituous liquors; these several pieces being expoled to be thaw'd in the same Air, or other ambient liquor.

5. We also tried whether Motion would impart a heat to ice, by nimbly rubbing a strong piece of ice upon a plate of ice, and though this seemed to hasten the dissolution in that part of the icy plate, where the Attrition had been made, yet we were unwilling to determine the matter, till further and exacter trial have been made.

6. And this brings into my mind an Experiment, that has by some been thought very strange. The occasion I remember was, that I received the last Winter the honour of a visit from

from a Nobleman of great eminency and learning, who chancing to come in, while I was making some trials with ice, would needs know what I was doing with it, but the presence of a very fair Lady, in whom *Hymen* had made him happy, and of some other Company of that Sex, that he brought along with him, inviting me to give him the answer, that I thought would be most suited and acceptable to his Company, I merrily told him, that I was trying, how to heat a Cold liquor with ice, and to satisfy him, that was no impossibility, I held out an open mouth'd glass, full of a certain liquor (which for some just reasons I do not describe, but do plainly teach it in an opportuner place) and desired them to feel, whether it were not actually Cold, and when they were satisfied, it was so, I chose among the pieces of ice, that lay by me, that I judg'd by the eye to be fit for my purpose, (for every piece was not so, for a reason I elsewhere shew,) and throwing it into this liquor, it did not only in a trice vanish in it, but the Lady,

I was mentioning, seeing the liquor smoak, and advancing hastily to try, whether it were really warm, found it so hot, that she was quickly fain to let it alone, and had almost burnt her tender hand, with which she had, in spite of my dissuasion, taken hold of the glass, which Her Lord himself could scarce indure to hold in his. But this Experiment, which for the main I have repeated before competent witnesses, though it be not impertinent to the *History of Cold*, yet I shall not build much upon it, because, how strange soever many have been pleased to think it, I shall elsewhere shew, that I made use of a certain unperceivable flight, which, in my opinion, did as well, as the nature of the liquor and the texture of the ice, contribute to the suddenness and surprizingness of the Effect.

7. But to return to the duration of the effects of Cold, I think those much mistaken, who imagine, that the effects of Cold do continually depend upon the actual presence and influence of the manifest efficient, as the light of the Air depends upon the
the

the Sun, or Fire, or other luminous body, upon whose removal it immediately ceases. For when cold agents have actually brought a disposed subject to a state of congelation, though the manifest efficient cause cease from acting, or perhaps from being, the effect may yet continue. For in most cases, if a certain texture be once produced in a body, it is agreeable to the constancy of nature, that it persevere in that state, till it be forceably put out of it, by some agent capable to overpower it, and though we usually see ice and snow, as it were of their own accord to melt away, when the frosty constitution of the Air ceases; yet the cause of that may be not barely the cessation of frosty weather, but that those easily dissoluble bodies are exposed to the free Air, which being heated by the Sun beams, and perhaps by calorick expirations from the earth, is furnished with an actual cause, upon whose account it destroys the texture of the ice and snow; but even here above ground, if snow be well compacted into great masses, in which

by reason of the closeness of the little icicles, but little Air is allowed to get between them, I have seen such masses of snow last so long, not only in thawing, but in rainy weather, as to be wondered at, and if such snow (or ice) be kept in a place where it may be fenced from the Sun, and other external enemies, though the place, it is lodged in, be not any thing near cold enough to produce ice, yet it will, as some trial hath taught me, preserve ice and snow for a very long time.

Appendix to the XVI. Title.

AN eminent instance to confirm what is delivered at the close of the foregoing Section, is afforded us by the conservatories, wherein snow and ice are kept all the Summer long. Of these I have seen in *Italy*, and elsewhere; but supposing I had the command of some *Italian*, and other books, wherein I should meet with the



p 407

the dimensions, and other circumstances that belong to them, my finding my expectation disappointed by those books, makes me think it very well worth while to subjoyn somewhat about things, that may give us opportunity of making a multitude of Experiments about Cold. And therefore meeting the other day (by good chance) with my ingenious friend Mr. *J. Evelyn*, his inquisitive travels, and his insight into the more polite kinds of knowledge, and particularly Architecture, made me desire and expect of him that account of the *Italian* way of making conservatories of snow, that I had miss'd of, in several Authors; and having readily obtain'd my desire of him, I shall not injure so justly esteem'd a style as his, to deliver his description in any other words, than those ensuing ones, wherein I received it from him.

[The snow Pits in *Italy*, &c. are sunk in the most solitary and cool'd places, commonly at the foot of some mountain or elevated ground, which may best protect them from

the Meridional and Occidental Sun, 25. foot wide at the orifice, and about 50. in depth, is esteem'd a competent Proportion. And though this be excavated in a Conical form, yet it is made flat at the bottom or point. The sides of the Pit are so joyc'd, that boards may be nail'd upon them very closely joynted. (His Majesties at *Greenwich* newly made on the side of the Castle-hill, is, as I remember, steen'd with Brick, and hardly so wide at the mouth.) About a yard from the bottom is fix'd a strong Frame or Tressle, upon which lies a kind of wooden grate; the top or cover is double thatch'd, with Reed or Straw, upon a copped frame or roof, in one of the sides whereof is a narrow door-case, hip-ped on like the top of a Dormer, and thatch'd, and so it is complete.

*I have
seen also
the sides
lin'd with
reeds long-
ways, in-
stead of
boarding
or steening.*

To conserve Snow.

They lay clean Straw upon the grate or wattle, so as to keep the Snow from running through, whilst they beat it to a hard cake of an icy consistence, which is near one foot thick,

thick, upon this they make a layer of straw, and on that snow, beaten as before, and so continue a bed of straw, and a bed of snow, S. S. S. till the pit be full to the brim. Finally, they lay Straw or Reed (for I remember to have seen both) a competent thickness over all, and keep the door lock'd. This grate is contriv'd, that the snow melting by any accident in laying, or extraordinary season of weather, may drain away from the mass, and sink without stagnating upon it, which would accelerate the Dissolution, and therefore the very bottom is but slightly steen'd. Those who are most circumpect and curious, preserve a tall Circle of shady trees about the pit, which may rather shade, then drip upon it.]

Thus far this learned Gentlemans account of Conservatories of Snow. And on this occasion I might add what the *Dutch* in their *Nova Zembla* Voyage relate, namely, that the three and twentieth of June, though it were fair Sunshiny weather, yet the heat was not so strong as to melt the Snow, to afford them

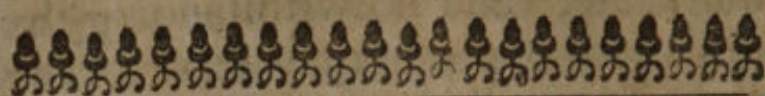
water

water to drink, and that in spite of their being reduc'd to put Snow into their mouths, to melt it down into their throats, they were compelled to indure great thirst. But because it was in so cold a Climate, that this duration of the Snow was observ'd, I shall rather take notice, that in the *Alps*, and other high mountains, even of warmer Climates, though the snow doth partly melt towards the end of Summer; yet in some places, where the reflection of the Sun beams is less considerable, the tops will even then remain covered with snow, as we among many others have in those Countries observed. And for further confirmation of the Doctrine deliver'd at the end of this 16. Title, I shall subjoyn a Passage, which having unexpectedly met with in an unlikely place of Captain *James's* Voyage, I think not fit to leave unmention'd here, not only because 'tis the sole artificial observation that I yet met with, concerning the lasting of ice, and so may recommend to us the Ingenuity of an Author, whose Testimony we somewhat frequently make use of, but because

cause the observation is in it self remarkable, and notwithstanding the difference of places may serve for the purpose we alledge it : Our Navigators words are these ; *I have in July, and in the beginning of August taken some of the Ice into the ship, and cut it square two foot, and put it into the Boat, where the Sun did shine on it with a very strong reflex about it. And notwithstanding the warmth of the Ship (for we kept a good fire) and our breathings, and motions it would not melt in eight or ten days.* And it is also considerable to our present purpose, what the same Author elsewhere has about the durability of the Congelation of the ground not yet thaw'd at the beginning of June. *For the ground (says he) was yet frozen, and thus much we found by experience in the burying of our men, in setting up the Kings Standard towards the latter end of June, and by our well at our coming away, in the beginning of July, at which time upon the land, for some other reasons, it was very hot weather.*

Pag. 101.

Pag. 65.



Title XVII.

*Considerations and Experiments
touching the Primum Fri-
gidum.*

1. **T**He dispute, which is the *Primum Frigidum*, is very well known among Naturalists; some contending for the *Earth*, others for the *Water*, others for the *Air*, and some of the Moderns for *Nitre*: But all seeming to agree, that there is some Body or other, that is of its own nature supremely Cold, and by participation of which, all other cold Bodies obtain that quality.

2. But for my part, I think, that, before men had so hotly disputed, which is the *Primum Frigidum*, they would have done well to enquire, whether there be any such thing or no (in the sense newly express'd.) For
though

though I make some scruple, resolutely to *contradict* such several Sects of Philosophers, as agree in taking It for granted, yet I think it may be not irrationally *Question'd*, and that upon two or three accounts,

3. For (first) it is disputable enough, as we shall hereafter see, whether cold be (as they speak) a positive quality, or a bare privation of heat, and till this question be determined, it will be somewhat improper to wrangle solicitously, which may be the *Primum Frigidum*. For if a Bodies being cold, signifie no more, then its not having its insensible parts so much agitated, as those of our Sensories, by which we are wont to judge of tactile qualities; there will be no cause to bring in a *Primum Frigidum*, upon whole account particular Bodies must be cold, since to make this or that Body so, it suffices that the Sun or the Fire, or some other agent, whatever it were, that agitated more vehemently its parts before, does now either cease to agitate them, or agitate them but very remissly: So that, till it be determined,

termin'd, whether cold be a positive quality, or but a privative; it will be needless to contend, what particular Body ought to be esteem'd the *primum frigidum* (in the sense above speci'd.)

4. Secondly, Though it be taken for granted, not only by the *Schools*, but by their Adversaries the *Chymists*, that heat and moisture, driness and gravity, and I know not how many other qualities, must have each of them a *πρῶτον δεκτικόν*, or a principal subject to reside in, upon whose account, and by participation of which, that Quality belongs to the other Bodies, wherein it is to be met with; though this be so, I say, yet we have * elsewhere fully enough manifested, that this fundamental Notion, upon which much of the Doctrine of Qualities, is both by *Aristotelians*, and vulgar *Chymists*, superstructed, is but an unwarrantable conceit, and therefore not sufficient for a wary Naturalist to build the Notion of a *primum frigidum* upon; there being indeed many qualities, as gravity, and figure, and motion, and

* In the
Sceptical
Chymist.

and colour, and sound, &c. of which no true and genuine *πρῶτον δεκτικόν* can (for ought I could ever yet discover) be assigned : and because heat and cold are look'd upon as Diametrically opposite Qualities, we may consider, that it will be very hard to show, that there is a *πρῶτον δεκτικόν* of heat ; since stones, and mettals, and plants, and animals, and (very few excepted) all consistent Bodies, we are conversant with, may by motion be brought to heat, which to attribute to the participation of some portion or other of the imaginary Element of fire, is not only precarious (being affirm'd by many, and prov'd by none) but erroneous, or at least needless, as we have more at large declar'd in other papers.

The Dialogues about heat and flame.

5. A third thing, that induces me to question, whether there be a *primum frigidum*, is, that among those Bodies, that the chiefest Sects of Philosophers, whether Ancient or Modern, have pitch'd upon, there is not any, that seems clearly to deserve the title of the *primum frigidum*. But to make this appear, we must distinctly

stinctly (though as briefly as our design will permit) consider those four several Bodies, which we have (at the Beginning of this Section) taken notice of, to stand in competition, in the Opinions of Philosophers, for the title of *primum frigidum*.

6. First, then *Plutarch* and others contend, that it is the Earth; but, to omit other Arguments, we see, that the Earth is frozen not by its own cold, but by its vicinity to the Air, as may be argued by this, *viz.* that the congealing cold even in the midst of Winter affects but the surface of the Earth, where it borders on the Air, and seldom pierces above a few feet, or, at most, yards, beneath that part wherein the Earth is exposed, and immediately contiguous, to the Air, as may appear by what we have formerly deliver'd concerning the small depth, to which frosts reach in the ground. And therefore if the Earth be protected from the Air (though by so cold a Body as water) it may be kept unfrozen all the Winter long, as may be gathered from that remarkable practise in the great Salt-

Salt-marshes of the French Islands of *Xaintonge*, where, as a diligent Writer of that Countrey, very well vers'd in the making of the French Salt, informs us, when once the season of Coagulating Salt by the heat of the Sun is quite past, the Owners are careful by opening certain Sluces to overflow all the Banks, and Dams, that make and divide the Salt-ponds, and serve for the Workmen to pass to and fro : for (says my Author in his own language) if they left those Marshes (or Salt-works) uncovered, the frost would make such havock amongst them, that it would be necessary to make them up again every year, but by means of the water, they are preserv'd (or kept in repair) from year to year : which practise I the rather mention, because the hint, it affords, as it is considerable to our present purpose, so it may on some occasions be applicable to practises useful to humane society.

*Monsieur
Bernard
de Palisse,
au Traité
du Sel
commun,*

7. Besides, the Earth being (according to those we reason with) the coldest, heaviest, and solideft of Elements, it is not so probable, as to ex-

E e

cuse

cuse them from the need of proving it, that those excessively cold Agents, that freez the Clouds into Snow and Hail, should be terrene Exhalations carried up to the middle Region of the Air, especially since it must be done by Agents, either hard to be guess'd at, or considerably hot. And 'tis not easie to give a reason, why, if Elementary Corpuscles steaming from the Earth, have such a congealing cold, where they are disunited, and but interspers'd among the particles of Air, the Mass of the Earth it self, whence those exhalations are suppos'd to proceed, should not be able also to congeal water, since the Terrestrial Corpuscles being more thick set, and united in a Clod of Earth, then in an equal portion of the Atmosphere, it seems, that where the frigorifick matter is more dense, the cold should be more vehement, as Philosophers observe, that heat is more intense in a glowing bar of Iron, then an equal portion of the flame of kindled Straw.

8. But (not to repeat what we formerly mention'd about Colds being

a Privation) there is another Argument against the Earths being the *primum frigidum*, and that is taken from the Subterranean fires, which breaking forth in many places of the Earth, as in *Ætna*, *Vesuvius*, *Hecla*, the *Pico of Tenariffe*, &c. seem to argue a Subterranean fire, upon whose existence not only many Chymists build great matters, but even divers Philosophers have adopted it, and the learned *Gassendus* himself seems so far to countenance it, as to imploy it as one Argument of the Earths being naturally neither hot nor cold. The mention of this Subterranean fire brings into my mind some things that I have met with amongst good, though not Classick, Authors, and amongst men that have been either diggers of (or conversant in) Mines, not improper to be here taken notice of. For though I do not now intend to declare my opinion about the Central fire, either of the *Chymists*, or *Cartesians*, and though the Examples newly mention'd, and such other seem to me but very inconsiderable, in reference to the whole Earth, yet

'tis observable to our present purpose, that there should be so much Subterranean heat or warmth, at least generally to be met with: For even where there appear no manifest signs of Subterranean fires, I have known those, that were wont to go to the Bottom of *deep* Mines, complain, that a very little Exercise would put them into a great sweat; and a learned and experienced French Doctor, that hath written in his own Language of Stones and Jewels, affirms, that in *such* Mines the Subterranean Vapors and Exhalations, are visibly so abundant, and likewise so hot, that the Mine-men are constrain'd (which a person I spoke with affirmed to me, touching himself) to work in their shirts, by reason of the great heat they there felt, and though I would have been glad to know, whether those deep places would have appear'd as hot, when judg'd of by a seal'd Weather-glass, as they did to the Mine-mens Sensories, because of some little doubt I harbour'd, whether much of that copious sweating, and seeming heat, might not proceed from the
thick-

thickness of the dampish Air, and its
 unfitness for Respiration; yet, be-
 cause a *Virtuoso*, that had a Lead-
 Mine of his own, in which he
 wrought himself for curiosity, an-
 swered me, that he was not wont to
 find any difficulty of breathing in the
 place, where he was so apt to sweat;
 and since I find not, that others have
 complain'd of having their respirati-
 on incommodated in such places, un-
 less by Accidental Damps, my scruple
 was much abated, and the rather,
 because the Author lately mention'd,
 expressly affirms, that the Sudorifick
 heat (if I may so speak) is to be
 found in the Bowels of the Earth, as
 well in Summer as in Winter, which
 prevents the ascribing of it to *Anti-*
peristasis. And in other places then
 Mines 'tis generally observ'd, that
 Wells and Springs freez not, if the
 place, whence the water is drawn, be
 very deep, but, as we have observ'd
 elsewhere, that it oft comes up
 smoaking, and, as it were, reaking,
 which argues, that at the least the
 Earth, wherein it was harbour'd, or
 through which it pass'd, was, if not
 warm,

De Claves
 au second
 Livre des
 pierres &
 pierreries,
 Cap. 2.

Ibid.

Monsieur
de Claves
livre II.
cap. 2.

warm, free from such a degree of Cold, as might be expected in the Earth, if it were the *primum frigidum*. Nor can it be reasonably pretended, that the Subterranean heat comes from the Beams of the Sun, since learned Men have observed, that those heat not the Earth above six or seven foot deep even in Southern Countries, and though we should allow them to pierce three times as far, yet that would not be considerable to the depth of the Mines above mentioned, and if the lower part of the Earth were of its own nature cold, and received the heat, it discloses only from the Sun and Stars; the deeper men dig, the lesser of heat and steams they would meet with, whereas the above cited French Minerallist affirms, that the lower they go, the more vapours, exhalations, and heat they find.

9. But because this learned man delivers this circumstance in a dogmatical, rather than an historical way, I will add somewhat out of a relation (whence I have
* else-

* elsewhere taken other particulars) made by a French Physician likewise, that had the curiosity to descend himself into the deep Mines of Hunga-

ry, some of which, that he went down into, may be collected by his Narrative, to have three or four hundred fathom, that is eighteen, or twenty four hundred foot of perpendicular depth. This Author then relates, that after he had descended about 180. or a hundred fathoms, he came into a very warm Region of the Earth, which lasted to the bottom of the Mine, and is so hot both Winter and Summer, that the Laborers are wont to work in it without their clothes, and he was scarce able to indure the heat of it, although the external Air were very hot: the weather being very fair, and the moneth July,

** In the Discourses about Antiperistasis, the following passages are taken, out of a small narrative, consisting of about two sheets of paper of Joh. Baptista Morinus, published in the year 1619. and titled, Relatio de locis Subterraneis, annexed to a discourse (too much built on Astrological and Aristotelian grounds) of the threefold Region, that he conceives to be as well in the Earth as in the Air.*

* Unde calor ille procederet petii à præfeto. Respondit, ex partibus inferioribus, inferius enim perpetuo calet. Quod responsum magis adhuc miratus, quasi vires ita sese haberet in fodinis omnibus. Respondit ita se habere in omnibus, saltem profundis, ut post profundum Terræ frigida tractum, in locum calidum descendatur. Et quod, ubicunque terra foditur post similem profunditatem, nullum amplius sentitur frigus, sed semper calor, quantumcunque profundè fodiatur.

* He adds, that he having demanded of the Overseer of the Mine, whence this heat came, he was answer'd, to that and several other questions, That it came from the lower

parts of the earth; that in all deep Mines, after one is past the Colder crust of the earth, one comes into a region, that is perpetually warm, and that where ever they dig the ground, after they are come to such a depth (which he elsewhere mentions to be about 80. or a hundred fathom) they feel no more any cold, but a perpetual heat, how deep soever they dig, (* yet without observing, that after they are once into that warm region, they find the heat sensibly increase, the nearer they approach to the centre of the earth,

* — Per-
cunctatus
sum an quo
magis ac-
ceditur ad
terræ cen-
trum, calor
ille major
percipere-

tur. Respondit, id nunquam fuisse animadvertum, nisi interdum fodiendo occurebant venæ calidorum Mineralium.
— Hæc Responsa non in unicâ fodinâ, & ab unico præfeto accep-
ti; sed, &c.

unless by accident they happen to dig through veins of hotter Minerals.) And these answers (subjoyns my Author) I received not in one Mine alone, or from a single overseer, but in all the Mines, and from all the Masters of them; so that if these were not mistaken, we may safely conclude, that as far as experience can inform us, the body of the earth in its lowermost parts, where 'tis presum'd to be coldest, is every where, and that considerably, hot. I said, *if these Mine-men were not mistaken*, because having been in the bottom of some Mines my self, though I find it acknowledged, that 'tis still warm in the bottom of deep ones, yet I confess, I somewhat suspect by what I have observ'd, that this degree of heat, which our French Physician found in the *Hungarian* Mines, might be rather in great part from the peculiar nature of those places, or of the Minerals generated there, then barely (as he and those that inform'd him suppose) from the greatness of their depth beneath the surface of the earth; for I know several mixtures, besides

besides those that are common, of bodies neither of them actually hot, which will produce a considerable degree of heat. And very credible eye witnesses affirm, that in some parts of *England*, they dig up good store of a kind of Mineral, which is thought to be of a Vitriolate nature, which by the bare addition of common water, will grow hot, almost to ignition. So that the *Hungarian* Mines being deep, and as appears by our Authors Narrative, being not destitute of water enough to make a Subterranean Spring in the Mine itself, besides what water may plentifully ascend in the forms of vapours, and moisten the Oar, it may be suspected, that either the water, or some appropriated Mineral spirit or juice (of which the bowels of the earth may contain divers, that we know nothing of) may produce together with the Mineral a warm steam, which for want of sufficient vent in those narrow, and close places, may heat them considerably, which conjecture may be countenanced by these three circumstances, that

that I took notice of in our Authors Narrative; *one*, That the smoak that copiously ascended out of the Mine by the perpendicular grove, was not barely hot, but consisted of stinking exhalations, which were so saline, and fretting, as oftentimes to corrode and spoil both the wooden ladders or stairs, and the iron instruments of the diggers. *The other*, that the overseers themselves of the Mines, told *Morinus* (as we lately saw) that they in some places met with veins of hot Minerals, which made it hotter, then the bare vicinity of those places

to the centre of the earth would have done. And lastly, * as our Author was descending into the

**Cum descendendo calorem illum magis ac magis augeri sentirem: hujus rationem petii à præfecto, quod in nullâ adhuc fodina similem caloris intensiorem percipissem. Respondit, Mineram Vitrioli paulo inferius existere, qua calorem multiplicaret.*

golden Mine at *Cremnitz*, he found in one place, the heat to increase as he descended more and more, (which seems not to agree with a passage we lately mention'd out of him) and to exceed any he had met with in any other Mine; and afterwards the overseer bringing him into a room, that

that abounded with smaragdine Vitriol, (the Mineral whence this heat proceeded) though the room were spacious, he found there, besides a sharp spirit very offensive to his throat, so troublesome a heat, that he was ready to faint away with sweating, and very much wondered how the diggers were able to work there. And elsewhere the Author himself notes, that such hot Mines of Vitriol, or Sulphur, may be found even in the first region of the earth, (as he calls that which is somewhat near the surface, and which he thinks fit to name the *cold region*) and within a large sphere of activity make it perpetually hot. But this, as I was intimating, I mention but as a suspicion, or a conjecture, and notwithstanding that the degree of heat may be much increased in these Mines, by the concurrence of accidental causes, in case the conjecture be admitted ; yet since the frequency of a sensible degree of heat in very deep places does very little favour their opinion, that will allow the earth to have no other heat, but what it receives

ceives from the Sun beams, or by the manifest fire of burning hills, as *Æt-na* and *Vesuvius*. And if it should be objected, that this Subterranean heat is adventitious to the Earth, which is supremely cold of its own nature; *Gassendus* might reply, that 'tis as likely, that the coldness of it near the superficies may be adventitious too, and that it appears at least as manifestly, that the one proceeds from the contiguous Air, as it does, that the other proceeds from some included fire; and if I misremember not, he hath this consideration, that 'tis somewhat strange, that Nature should have intended the Earth for its *summum frigidum*, and yet that a great part (and for ought we know the greatest) should be constantly kept warm, either by the Sun, as under the Torrid Zone, or by the Subterranean fires. But the objection mention'd against *Gassendus*, opposes but one of the Arguments we have alledg'd against the Earths being the *primum frigidum*, and would leave the others in their force, though it did more convincingly answer, that,
against

against which 'tis framed, then it seems to do.

10. And if the Patrons of the Earths coldness, to evade the Arguments I have alledged, should pretend, that when they affirm the Earth to be the *primum frigidum*, they mean not the Elementary Earth, but some Body that is mingled with it; I shall desire to know, which 'tis they mean of the many other Bodies, that make up the Terrestrial Globe, that we may examine what right it has to that Title; and in the mean time I shall conclude against them, that the Earth it self has none, since they grant a colder Body then it, and such a one as the earth must be beholding to, for the greatest degrees of coldness it chances to possess.

11. But though I presume, enough has been said to make it appear unlikely, that the Earth should be the *primum frigidum*, yet I must in this dissent from the learned *Gassendus*, that he thinks the Earth, not only not to be the *primum frigidum*, but not to be naturally cold any more then hot. For the insensible parts of the Earth,
like

like those of other firm Bodies, being heavy, and perhaps gross, and either having no constant motion at all, or at least a far more remiss agitation, then that of our Sensories; it seems to follow, that the Earth must seem cold to us, unless it be by the communicated heat, or motion of some extrinseck Agent, put into a degree of agitation, that belongs not to its nature; and for the like reason I think it not improbable, that pure Earth should in its own Nature be colder, then either pure Water or pure Air, since the Earth being a consistent Body, its component particles are at rest among themselves, or at least mov'd with an almost infinite slowness, whereas Water and Air being fluids, their component particles must be in a restless and various motion, and consequently be less remote from heat, which is a state wherein the various agitation of the minute particles is more vehement.

12. And if those, that plead for the Earth, had declar'd, that they meant not the pure or Elementary Earth,

Earth, but that part of the Terrestrial Globe, that is distinct from the Sea, and other Waters, that make it up, and would have Earth in that sense not to be the *primum frigidum*, but only the *summum frigidum*, perhaps they might have a better plea for their Opinion, then they can urge for theirs, who contend for the Water or the Air, especially, if to countenance their Opinion, this memorable observation be added, which I

It was not the Sea, nor the nearness unto the Pole, but the Ice about the land, that let and hindred us (as I said before) for that as soon as we made from the land, and put more into the Sea, although it was much further northward, presently we felt more warmth, and in that opinion our Pilot William Barents dyed, who notwithstanding the fearful and intollerable Cold that he indur'd, yet he was not discourag'd, but offer'd to lay wagers with divers of us, that by Gods help he would bring that pretended Voyage to an end, if he held his course North-east from the North Cape. Gerard de Veer in Purchas, pag. 474.

have met with among those Navigators, that have had the greatest Experience of the Frigid Zone; for the Dutch, that sail'd thrice to *Nova Zembla*, and once wintered there, affirm in their first voyage, that the

highest degrees of Cold are not to be met with in the main Sea, where yet men are most expos'd to the Operations

tions of the Air, and of the Water, but either upon the Land or near it. That accurate Geometrician and Hydrographer *Fournier* tells us, that in 1595. the *Hollanders* being intercepted by Icy Scholes in the strait of *Weigats*, and meeting with certain *Muscovites*, demanded of them, whether those Seas were always frozen, and were answered, that neither the Northern Sea, nor that of *Tartary* did ever freez, and that 'twas only that strait with the Sea contiguous to the shores of some Bays and Gulphs, that were frozen; and our judicious Author, not only adds, that in effect all those that sail into those parts relate, That all those Lumps of Ice are such as have been loosened, and severed from the Islands, and the Rivers of the *Samojeds* and *Tartars*, but adventures to affirm in general terms, that 'tis certain, the *main Seas* never freez, and that 'tis but the confines, and shores of some of them, that are frozen.

13. That the water is the *primum frigidum*, the Opinion of *Aristotle* has made it to be, that of the schools, and

of the generality of Philosophers. But I can as little acquiesce in this opinion, as in the former, not finding it agreeable to what experience teaches us.

14. For not to mention, that it would be very difficult to prove, that divers very cold Bodies, as Gold and Silver, and Crystal, and several other fusible stones have in them any water at all, to which their coldness may with any degree of probability be ascribed; nor to urge the Arguments, that some Modern contenders for the supreme coldness of the Air are wont to imploy; not (I say) to insist on such things, I shall content my self to make use of this obvious *παῖνόμενον* of Cold, that in Rivers, Ponds, and other receptacles of water, the congelation begins at the Top where the liquor is expos'd to the immediate contact of the Air, which sufficiently argues, that the Air is colder then the Water, since it is able not only sensibly to refrigerate it, but to deprive it of its fluidity, and congeal it into Ice, whereas if the water it self were the *primum frigidum*,

dum, either it ought to be, at least as to the *major* part of it, always congeal'd, or we may justly demand a reason, why, when it does freez, the glaciation should not begin in the middle, or at the bottom, as soon as at the Top, if not sooner. And our Arguments against the precedency of the water in point of coldness, may be strengthen'd by this, That frosts are wont to be hardest, when the Air is very clear, and freeest from Aqueous vapors, whereas in rainy weather, wherein such vapors most abound, the cold is wont to be far more remiss : To which we may add, what we lately deliver'd from the observation of Navigators, that even in the frigid Zone the main Sea, where yet the water is in the greatest mass, and so most likely, as well as advantag'd to disclose its nature, never freezes, though the Straits, and Bays, and Gulphs be frozen over, which argues, that the greatest degrees of Cold are rather to be assign'd to the Air, or to the Earth, then to the Water, which by the practise formerly mention'd of the Masters of

the French Salt Marshes appears to be (when it is of a considerable depth) fitter to preserve Bodies from congelation, then to congeal them, which instance I the rather repeat, because it seems to argue, that the water is not so much as dispos'd to receive any very intense degree of cold at a remote distance from the Air: for though Navigators tell us of exceeding thick pieces of Ice, yet, as we have already elsewhere noted, we are not bound to believe, that the congealing cold has pierced any thing near so much as that thickness amounts to from the superficies of the Sea directly downwards; for though it were no great matter if it did, in comparison of that depth of the Sea, which, though the water be naturally cold, the sharpest Air is unable to congeal, yet we have elsewhere proved, that those thick masses of Ice, are not solid and intire pieces, but rather heaps of many flakes, and other fragments of Ice, which running upon one another, or sliding under one another, are by the congelation of the intercepted water (and per-

perchance half thaw'd snow) as it were, cemented together into misshapen and unweildy masses; which conjecture agrees very well with that observation of the Ingenious Captain James, which he delivers in these words.

It seldom rains after the middle of September, but snows, and that snow will not melt on the lands, nor sands: At low water, when it snows (which it doth very often) the sands are all covered over with it, which the half tide carries officiously (twice in twenty four hours) into the great Bay, which is the common Rendezvous of it. Every low water, are the sands left clear to gather more to the increase of it. Thus doth it dayly gather in this manner, till the latter end of Octob. and by that time hath it brought the Sea to that coldness, that as it snows, the snow will lye upon the water in flakes, without changing its colour, but with the wind is wrought together, and as the Winter goes forward, it begins to freeze on the surface of it, two or three inches, or more in one night, which being carried with the half tide, meets with some obstacle (as it soon doth) and then it crumples, and so runs

upon it self, that in few hours it will be five or six foot thick; the half tide still flowing, carries it so fast away, that by December it is grown to an infinite multiplication of Ice. Thus far this Navigator, to which I shall add another passage out of one of his Countrey-men (Mr. Hudson) (famous for the Northern Discoveries, that bare his name) by which, added to what has been elsewhere deliver'd to the same purpose, we may be invited to believe, that the vast Hills and Islands of Ice, that are to be met with about the Straits of *Weigats* and elsewhere, are not generated of the Sea it self. *Its no marvel* (says he) *that there is so much Ice in the Sea towards the Pole, so many Sounds and Rivers being in the Lands of Nova Zembla, and Newland to ingender it, besides the coasts of Pechora, Russia, and Greenland, with Lappia, as by proof I find by my Travel in these parts.*

Furchas.
lib. 3. cap.
15. pag.
679.

15. But for all this, I think not fit, as does the Ingenious *Gassendus*, and some others, to make the water indifferent, as to heat and cold. For, as I formerly noted concerning the
Earth;

Earth; so I must now represent touching the water, that, setting aside the heat of the Sun, which is but adventitious, where it does operate, and which leaves many vast portions of that Element, which it does not constantly reach, the insensible parts of water are much less agitated, then those of our Sensories temperately dispos'd, and consequently may in regard of us be judg'd cold. For though water being a Liquor, I readily allow it a various Motion of its component Corpuscles, (that being requisite to make a Body fluid,) yet such an agitation, which is sufficient for fluidity, may be, and often is, far more remiss, then that of the spirits, Blood, and other liquors of so hot a Sanguineous animal as Man, as we see, that Urine, though after it has been long omitted, it continues a fluid Body, yet its parts are far less agitated, then they were, when it came hot, and reeking out of the Bladder.

16. And upon this occasion, I shall add, what by inquiry I have learned, that (except the parts somewhat near the superficies of the water, which

the heat of the Sun, or the warmth of the neighbouring lower Region of the Air may give some warmth to) the whole Body of the Sea is very cold ; for being very well acquainted with one, that for some time got a livelihood, by going down into the Bottom of the Sea, to fetch up what could be recovered out of shipwrack vessels, I purposely inquired of him, what cold he felt under water, and he more then once told me, that though near the Top of the water the cold were very moderate, yet when he was necessitated to descend a great depth, he found it so great, that he could not very long support it ; and particularly he told me, that having occasion to descend about twelve or fourteen fathom deep (which is nothing in comparison of the depth of many Seas) to fasten ropes to the Ordinance of a great ship, that was some years since cast away, near the coast of one of the Northern Countries, though the Engine that was let down with him supplied him so well with Air, that he was not incommodated in point of Respiration, and though

though he felt no other inconveniencies, that might dissuade his tarrying longer, yet the cold was so great, and troublesome, that he was not able to endure it above two or three hours, but was constrain'd to remount to a milder, as well as a higher Region. I wish'd several times he had had with him a seal'd Weather-glass (for ordinary Thermometers would on that occasion have been unserviceable) to prevent some little doubt, that might be made, whether the intense Cold he felt might not be only and chiefly in reference to his Body, which might be so alter'd, and dispos'd by this new Briny Ambient, as to make such a disturbance in the course or texture of his Blood, as that which makes Aguish persons so cold at the beginning of the fit, though the temperature of the Ambient Body continue the same. But this is not the only person, that found the Sea Exceeding cold, for I remember *Beguinus* relates from the mouth of a *Marseillian* Knight, that was overseer of the Coral-fishing in the Kingdom of *Tunis*, that having

upon

Beguinus
in *Tyrocinio* Chy-
mice, lib.
2. cap. 1.

*Josephus
Acosta lib.
2. cap. 11.*

upon that coast let down a young man, to feel, whether Coral were hard or soft, as it grew in the water, when this man was come about eight fathom, near the Bottom of the Sea, he felt it exceeding cold. To which we shall add the testimony of a sober Traveller, *Josephus Acosta*, who tells us, *That it is a thing remarkable, that in the depth of the Ocean, the water cannot be made hot by the violence of the Sun, as in Rivers: Finally (he subjoyns) even as Salt-Petre (though it be of the nature of Salt) hath the property to cool water, even so we see by experience, that in some parts and havens, the salt water doth refresh, the which we have observed in that of Callao, where they put the water or wine which they drink, into the Sea in Flaggons to be refreshed, whereby we may undoubtedly find, that the Ocean hath this property to temper and moderate the excessive heat. For this cause we feel greater heat at Land then at Sea, cæteris paribus, and commonly Countries lying near the Sea, are cooler then those that are farther off. By all these testimonies, it seems to appear, that both in very cold Regions, and very*

very hot, the deep parts of the Sea seem to be very Cold, the Sun beams being not able to penetrate the Sea to any great depth; for I remember, that having enquired of the Diver I lately mentioned, whether he could discern the light of the Sun at any great distance from the surface of the water, he answered me, that he could not, but as he went down deeper and deeper, so he found it darker and darker, and that to a degree, that would scarce have been expected in so Diaphanous a Body as water is.

17. But this submarine cold (if I may so call it) though it be great and considerable, is not so intense, as to intitle water to be the *primum frigidum*, since as cold as our *Divers* found it at the bottom of the Sea, they did not find it cold enough to freez the water there, as the Air often does at the Top.

18. The next Opinion we are to consider, is that of the *Stoicks* of old, and adopted by the generality of Modern Philosophers, that are not Peripateticks, who assert the Air to be the *primum frigidum*: But being ere
long

long more particularly to treat of the Temperature of the Air, we will reserve till then to examine, whether it be cold of its own nature or not; but in the mean time, we shall *here* take leave to question, whether it ought to be esteem'd the *primum frigidum*. For not to mention, that *Aristotle*, and the Schools, with many other learned men, think the Air so far from being the coldest of the Elements, that they reckon it among the hot ones, because I confess their opinion is not mine, not to represent the heat of the Air in the Torrid Zone, nor that by the generality of Philosophers, the upper Region of the Air, which is believed to make incomparably the greatest part of it, is always hot, and the lower Region is so too, in comparison of the middle, though the coldness even of this is not perhaps unquestionable, not to urge any of these things, I say, I shall in this place mention only two observations.

19. The one is that, which I lately recited, touching the great coldness of the water in the deeper parts of
of

of the Sea, for 'tis not easie to show, how this great cold proceeds from that of the Air, whose operation seems not (as may be judg'd by that little way that frosts pierce into the moist Earth) to reach very far beneath the surface of the water, (inso-much that Captain *James*, who had very good opportunity to try, allows not, in case the Ice be not made by accumulation, that the Frost pierces above two yards perpendicularly downwards from the surface of the water, even in the coldest habitable Regions.) And this will seem the more rational, if we consider, that in case the coldness of the Sea proceeded constantly from the Air, as such, the cold would be greater near the surface, where 'tis contiguous to the Air, then in the parts remoter from it, and yet the contrary may appear by the passages lately recited.

20. But if it be objected, that this at best can prove no more, then that the Air is not the *primum frigidum*, notwithstanding which, it may be the *summum frigidum*. For answer,

I must proceed to my second Argument, which will perhaps evince, that it is not that neither, for by the same way of arguing, by which those I am now dealing with endeavour to prove the Air to be the coldest Body in the World, I shall endeavour to prove, that it is not so: For their grand, and (as far as I remember) their only considerable Argument is drawn from Experience, which shows, that water begins to freeze at the Top, where 'tis exposed to the Air; but to this vulgar Experiment I oppose that of mine, which I have often mentioned already to other purposes, that by an application of salt and snow, I can make water, that would else freeze at the Top, begin to freeze at the Bottom, or at any side I please, and that much sooner than the common Air, even in a sharp frosty night, would be able to congeal it; and when in exceeding cold weather the Ambient Nocturnal Air had reduc'd a parcel of Air purposely included in a convenient glass, to as great a degree of condensation as it could: I have more than once by the

External

External application of other things, been able to condense it much farther, which argues, that 'tis not the Air as such, but some adventitious frigori-
fick Corpuscles (taking that term as I do in this Treatise in a large sense) that may sometimes be mingled with it, which produce the notablest degrees of cold, or upon whose Account the Air produces them. And if these be duly applied, water will be congealed, whether Air comes to touch the surface of it or no; nay, though Bodies, which the Air can never penetrate nor congeal any of their parts, be interpos'd, as may appear by the Experiments formerly mention'd of freezing water included in glass bubbles, and suspended in oyl of Turpentine, and other uncongealed Liquors; and it is worth taking notice of, by them that conclude the Airs being the *primum frigidum*, from the waters beginning to freeze at the Top, where 'tis contiguous to the Air, that it is there also where the Ice begins to thaw.

21. Besides the three Opinions we have hitherto examin'd, there is a fourth,

fourth, that justly deserves to be seriously consider'd; for the learned and ingenious *Gassendus* is suppos'd, though I doubt how truly, to be the Author of it, and though according to his custom, he speaks warily, and not so confidently of it, yet in his last writings he much countenances it; yet some eminently learned men, as well of our own, as of other Nations, have resolutely enough embraced it. According then to these, the congelation of Liquors, and the cold we meet with in the Air, Water, and other Bodies, proceeds from the admixture of Nitrous exhalations, or Corpuscles introduc'd into them: And as I have a great respect for divers of these mens persons, so I like very well in their opinion, that they do not ascribe the supreme degree of frigefactive Virtue to the Air itself, but to some adventitious thing, that is mingled with it; but whereas they pitch upon *Nitre*, as the grand Universal efficient of cold, I confess I cannot yet fully acquiesce in that Tenent. For though I am not averse from allowing Salt-Petre to be one
of

of those Bodies, that are endued with a refrigerating power, and to be copiously enough dispers'd through several portions of the Earth, yet for ought I know, there may be not only divers other causes of cold, but divers other Bodies qualified to be Efficients of cold, as well as Salt-Petre.

22. And first, if cold be not a positive quality, but the absence of heat, the removing of calorifick Agents will in many cases suffice to produce cold without the introduction of any Nitrous particles into the Body to be refrigerated. But because 'tis disputable, whether cold be a positive quality or no, we will urge this Argument no further, till the Controversie be decided, and till then, as it will remain not improbable, we propose it as no other, but proceed to the next.

23. In the second place, I see not as yet any proof, that the great cold, we have formerly mention'd to be met with in the depths of that vast Body the Sea, especially when it is greater elsewhere, then nearer the

Top, where the Air may better communicate its coldness to it, must be the effect of Nitrous Atoms, which must certainly swarm in prodigious multitudes to be able to refrigerate every drop and sensible particle of so stupendiously vast a Body as the Ocean. Besides that I remember not to have found or known it observ'd, that Nitre, especially in vast quantities reaches near so deep in the Earth, as those parts of the Sea, that are found exceeding cold. And as the halitusous part of Nitre is more dispos'd to fly up into the Air, then dive down into the Sea, so we find no great documents of its having its grosser and sensible parts abounding in the Seawater, since the evaporations of that leaves not behind it Salt-petre, but common Salt. But these, though no light considerations, are not those, that most weigh with me.

24. For (in the next place) I am not satisfied with the Experiences I find alledged to prove, that 'tis by Nitre, that the Air and the neighboring parts of the Earth, and Water (not to repeat the objections I lately borrow-

borrowed from the Sea) receive their highest degrees of Cold. For when *Gassendus* and others tell us, that 'tis Nitre resolv'd into exhalations, that make the gelid Wind, which refrigerates all things it touches, and penetrating into the water, congeals it, this, I say, to me will seem precarious, untill *Gassendus* (or some other for him) tell us, what Experiments they are (which he seems in one place to intimate) that this new Doctrine depends on; for, I, confess, that for my part, I who have perhaps had more opportunity to resolve Nitre, have seen no great feats, that the steams of it have done, more then those of other saline Bodies in the production of cold; and the spirit of Nitre, which is a liquor consisting of the volatile parts of that resolved salt, not only does not (that I have observed) appear to the touch to have considerably, if at all, a greater *actual* cold, then that of divers other Liquors, but seems to have a *potential* heat. For whether or no the Exhalations of Nitre be able to congeal water into Ice, I have formerly ob-

serv'd, that the spirit of Nitre or *Aqua fortis* will dissolve Ice into water, very near, if not altogether as soon as the spirit of Wine it self, which inflamable Liquor is generally acknowledg'd to be in a high degree *potentially* hot. If *Gassendus* did not mean such steams of Salt-petre as these which I have been speaking of, it had not been amiss to have signified what other kind of Corpuscles of resolved Nitre he meant, without leaving his Reader to divine it; and if we may judge of other Experiments,

Gassendi Phy. Lib. 6. Sect. 1. pag. 399. De qualitatibus rerum— ac addi quidem fortassis potest, precipua frigoris semina, si quæ constant, potissimum ex frigorificis Atomis abire in halinitrum corporaque ipsis affinia, quando experimur non exsolvi halinitrum, quin & penetrando in aquam, ipsam congelet & universa à se contacta refrigeret, & abeundo in halitum creet gelidum seu frigidum ventum, sed res pendet ex variis, quæ non possunt hoc loco commemorari, Experimentis.

which we lately took notice, that *Gassendus* seems to intimate, by that which he sets down a little after, compar'd with that he had mention'd a little before: I am not likely much to be convinc'd by them, but shall rather be tempted to suspect, that learned man might be impos'd upon by others to write that, as matter of fact, which

which he never had tried, and yet own not the having it only by report. For whereas he seems to say, that dissolved Nitre mingling it self with water, freezes it, and that in Summer, yet I must freely profess, that although some other Learned Moderns teach the same thing (but without any mans avouching it, that I know, upon his own experience) I, who am no stranger to Nitrous Experiments, have never been able to produce, or so fortunate, as to see any such effect, and 'tis somewhat strange to me, that *Chymists*, who make such frequent solutions of Nitre, and oftentimes with less water, then is sufficient to dissolve it all, so that by consequence the proportion of the Nitre to the Water, must have run through almost all the possible measures of proportion, should never so much, as by chance (as I can hear) have observ'd any such matter: and that which makes me thus interpret *Gassendus* his meaning, (though in one of the two passages, wherein he sets down this Experiment, he mentions also snow, or ice to be added to

the Nitre) is, that in the first of those two passages, he ascribes the congelation to Nitre alone, without speaking of either ice or snow ; and in the other place, not only his words seem

Ib. pag. 400. Quomodo possunt corpuscula Nitri in aquam infusi illam præter modum adeo frigidam reddere imò, & per æstatem etiam congelare, dum nitrum nivi glaciæque detritæ commistum lagenæ circumponitur, ipsaque præter corpus Lagenæ penetrant in aquam contentam.

to import, that notwithstanding the addition of the other ingredients, the Corpuscles of the Nitre expiring

out of the mixture, and penetrating into the water, are they that make it freeze, but the Exigence of his discourse seems to require such an interpretation : for to say it is the Corpuscles of the Nitre, that were harbour'd in the ice or snow, that freeze the water they invade, is no better then to beg the Question. For besides that, he ought to prove, that there are multitudes of the Corpuscles of Nitre, lodg'd in snow and ice : Besides this, I say, since these two Bodies are said to be water before they were congealed, to grant what his Explication supposes about ice and snow, is to grant in effect, that

that Nitre alone (without ice or snow) can turn water into ice, which is the thing that Experience warranted us lately to deny ; and if this be all, that is meant by the Experiment, the mixing of Nitre with the ice, or the snow, will signifie very little, to evince what should be proved. For, if instead of Nitre you take Sea-salt, or the Spirit of Salt, nay, the inflammable part of Wine, the Experiment will succeed ; and yet I think *Gassendus* would not have the Corpuscles of these Bodies to be frigoric, like those of Nitre, which yet they may be prov'd to be by the same Argument, which is imployed to show, that the Corpuscles of the Nitre, which is added as a distinct ingredient to the ice, or to the snow, are the Efficients of the Congelation.

25. Having thus examin'd *Gassendus* his Experiments, we will now, as our next and last Argument touching this subject, subjoyn our own, as far as we can find any of them among our notes, some of which follow in these words.

26. [As cold as they think Saltpetre to be, who teach its spirituous parts to be the Grand and Catholick efficient of cold, yet we found, that it would dissolve ice readily enough, as well as Sea-salt, &c. are wont to do, as we collected from this, That roch'd Petre mingled with ice, would freez the vapors wandring in the Air, to the outside of the single Vial, wherein we made the Experiment, which the ice alone would not have done; and having placed some grosse beaten Nitre (of the same parcel) in little heaps here and there upon plates of ice, we manifestly found them to sink into the ice, which argued their dissolving it; and having put some of it upon a thick and smooth piece of ice, we found, that it had pierc'd a hole quite through it, whilest the surrounding part of the ice remain'd of a good thickness.]

27. [We took a large single Vial, almost full of water, and put it into as much roch'd Petre, as by keeping it a good while by the fires side, we could dissolve in it, of which one mark was, that there remain'd

main'd a pretty deal of Salt intire at the Bottom of the liquor, this being expos'd to the Air, during an extremely sharp night, and a good part of the day, the solution was frozen so hard to the very Top of the liquor, that having broken the glass, we could hardly break the included mass. But at the Bottom there appear'd some liquor, with Crystals of Nitre well figur'd, that seem'd to have shot in it, and argued the Water to be sufficiently impregnated with the Salt.]

28. [As for the spirituous parts of Nitre, so far forth as their temper, as to heat or cold, can be judg'd by distillation, and by Weather-glasses, they are not *actually* more cold then some other Liquors, and appear rather to be *potentially* hot, then cold, at least they seem indispos'd to turn water into ice, since we have tri'd, that the spirit of Nitre will readily enough turn ice into water.]

29. These three foregoing notes show, that Salt-petre is no such wonderfully cold Body, but that there are others colder, as being able to
freez

freez water, which Nitre could not congeal. Nay, they manifest, that Nitre, which is said to be the efficient of ice, does thaw and dissolve it, and so seems at least in reference to It, to be rather hot then cold.

30. I shall now add one note more, to show it does not always make water so much as equally cold with the common Air; the Experiment I find thus recorded.

Aug. 1.

31. [We took a seal'd Weather-glass, and by a little pulley fastned to a frame, suspended it in a solution of roch'd-Petre, as strong as we could make it, without heat, as appear'd by a pretty Quantity of Nitre, that had continued some days undissolved in the vessel, which was a Beer-glass, with a flat Bottom. After the Ball of the Weather-glass had been suspended in this liquor, to try, whether the Ambient Air were not at this time colder then the Liquor, (it being a cloudy and windy day, and betwixt the hours of 11. and 12.) though both the Weather-glass and it, had stood some days in the same place, I lifted up the glass out of
the

the water by the string it hung by; that I might not touch it with my warm hands, and found the Liquor in the glass to descend by degrees, about two divisions (which were eights of an inch) and then by the string lifting up the Weather-glass, and putting again the solution of Nitre under it, the included Liquor was impell'd up again two divisions, and sometimes two divisions and a half, for to satisfy my self the more fully, I repeated the Experiment several times, and observ'd, that the included liquor usually ascended the first division, so fast, that the eye could perceive its progress, and that the ascent upon the immersion in the dissolv'd Nitre was discernably quicker, then the descent upon the removal of the Weather-glass into the open Air, though the space both of the one and of the other were about, either two divisions, or two divisions and a half.]

32. If it be here demanded, what then I think of the frigifactive Virtue of Nitre, I must answer, that I have not yet fully satisfi'd my self concerning

ing it, but thus much I am not willing to deny, That among divers other Bodies, that upon several occasions exhale from the Terrestrial Globe, those Corpuscles that are of a Nitrous Nature, may be for the most part well qualified to refrigerate the Air, and I am not indispos'd to think, that there may be store of little saline Bodies of kin to Nitre, that (especially at certain times) rove in great multitudes to and fro, in some parts of the Atmosphere; but that this aerial salt, which some moderns call volatile Nitre, should be true and perfect Salt-petre is more than I am sure of, and that this Salt alone should be the *summum frigidum*, is more than as yet I am convinc'd of; especially, since, for ought I know, there may be in the bowels of the Earth, (whence I have seen many concretes digg'd out, whose very names and outsides are for the most part unknown, even to Chymists themselves) divers other Bodies besides Salt-petre, whose steams may have a power of refrigerating the Air, as great in proportion to their

Quan-

Quantity, as those of Salt-petre; and since common salt in artificial glaciations, is found to cooperate as powerfully, as Salt-petre it self, and since it is undeniably a Body, of which there is a vast quantity in the Terrestrial Globe, and which by reason of the Sea, where it abounds, is exceedingly diffus'd, I see no great reason, why we may not aswel esteem that kind of Salt among the Catholick efficientes of Cold, and the rather, because that the smallest Corpuscles, our eye discerns of Sea-salt, are wont to be, (though not exactly) of a Cubical figure, which is that figure, *Philoponus* informs us, the great *Democritus* of old (justly admir'd by *Gassendus*) assign'd to the Atoms of cold, whereas, according to *Gassendus* himself, the Corpuscles of Nitre, at least as far as sense has inform'd us, are not the most conveniently shap'd to produce cold, since he labours to show, that the figure of frigorifick Atoms is to be Tetrahedral or Pyramidal, whereas the Crystals, or Grains, great or small, into which good Salt-petre shoots,

are

are wont to be Prismatical having their base Sexangular; but to return to what I was saying, concerning the congealing of water, with ice, I shall subjoyn, that the same Experiment countenances my conjecturing, that oftentimes it may not be emanations of one Salt, or other Body, but a peculiar and lucky conjunction of those of two or more sorts of them, that produces the intense degree of cold, as we see, that ice and snow themselves have their coldness advanced (as to its effects) by the mixture either of Sea-salt or Nitre, or spirit of Wine, or any other appropriated additaments. Nay, I may elsewhere have occasion to shew, that actual Cold, may be manifestly promoted, if not generated, by the addition of a Body that is not actually Cold. But to all this I must add, that I doubt whether any of those saline or Terrestrial exspirations, either single or conjoyned, be the adequate causes of cold, since, for ought I know, there may be other ways of producing it, besides the introduction of frigorifick, whether Atoms or

Corr

Corpuscles, of which we may have occasion to take some notice hereafter. In the mean time, having discours'd thus long against the admitting a *primum frigidum*, I think it not amiss to take notice once more, that my design in playing the Sceptick on this subject, is not so much to reject other mens probable opinions, of a *primum frigidum*, as absolutely false, as 'tis to give an account, why I look upon them, as doubtful.

Title



Title XVIII.

*Experiments and Observations
touching the Coldness and
Temperature of the Air.*

I. **I** Have shewn in the former Section, that the Air is not the *Primum Frigidum*, but yet I cannot readily yield my assent to the Opinion of the learned *Gassendus*, and some others, (who have written before, and since him) that the Air is of it self indifferent, that is, neither cold, nor hot, but as it happens to be made, either the one or the other by external Agents. For if we take Cold in the obvious and received Acception of the word, that is, for a Quality relative to the senses of a Man, whose Organs are in a good or middle Temper, in reference to Cold and Heat, I am hitherto inclinable to think, that

that we may rather attribute Coldness to the Air, then either Heat, or a perfect Neutrality as to Heat and Cold. For to make a Body cold as to sense, it seems to be sufficient, that its minute Corpuscles do less agitate the small parts of our Organs of Feeling, then they are wont to be agitated by the Blood, and other fluid parts of the Body; and consequently, if supposing the Air devoid of those calorifick and frigorisick Atoms, to which the learned Men, I was naming, ascribe its heat and cold, it would constitute a fluid, which either by reason of the minuteness of its parts, or their want of a sufficiently vehement motion, would less affect the sensory of Feeling, then the internal liquors, and spirits of the body are wont to do, and so it would appear actually cold. Nor is it necessary, that all liquors, much less all fluids, should be as much agitated as the blood and vital humors of a humane body, as we see (to omit what in the last Section is mention'd about newly emitted Urine, and to skip other obvious instances)

in those Fishes and other Animals, whose Blood and analogous Juices are always, and that in the state, which passes for their natural state, actually Cold to our Touch. And I see no sufficient reason, why we should not conceive the Air even in its natural state, (at least as far forth as it can be said to have a natural state) to be one of the number of cold Fluids. For as to the main, if not only, Argument of *Gassendus*, and others, namely, That, as we see the Air to be easily heated by the Action of the Sun, or the fire, so we see it as easily refrigerated by ice, and snow, and Northerly winds, and other Efficients of Cold, and that heat and cold reign in it by turns in Summer and in Winter: This only proves, what I readily grant, that the Air is easily susceptible at several times of both these contrary Qualities, but it does not shew, that one is not more connatural to it, then the other, as we see, that the water may be easily depriv'd of its fluidity by the circumposition of snow and salt, and reduc'd to be fluid again by the Sun,
or

or the Fire; and yet according to them, as well as others, fluidity, not Firmness, is the natural quality of water. But this is not that, which I lay most weight upon, for I considered, that it is manifest and acknowledged by these learned Men themselves, that the heat of the Air is adventitious to it, and communicated by the beams of the Sun, or of the Fire, or by some other Agents naturally productive of heat, as well in other Bodies as the Air: And 'tis also evident, that upon the bare absence, (for ought else that appears) of the Sun, or Extinction of the Fire, or removal of the other causes of heat, the Air will, as it were of its own accord, be reduc'd to Coldness. Whereas, that there are swarms of frigorifick Atoms diffus'd through the Air, from which all its coldness proceeds, is but an *Hypothesis* of their own, far from being manifest in it self, and not hitherto, that I know of, prov'd by any fit Experiment or cogent reason. And though in some cases I am not adverse to the admitting such Corpuscles, as may in a

sense, be styl'd frigidifick, yet I see
 not why we should have recourse to
 them in cases where such a bare cessa-
 tion, or lessening of former motion,
 as may easily be ascrib'd to manifest
 causes, may serve the turn, as to
 a *Sensible* (for I now consider not the
 causes of the *Intenser*) Coldness in the
 Air, without taking them in. And
 the opinion, I incline to, has at least
 this advantage, that the Air seems to
 be as rightfully term'd cold, as Iron,
 Marble, Mercury, Crystal, Salt-
 petre, and such other Bodies, which
 men unanimously look upon as such,
 there being none of these to which
 the Argument imploy'd against the
 coldness of the Air, is not applicable,
 save that the Air being a fluid of a
 looser and finer Texture does sooner
 receive, and lose the impressions of
 heat and cold. And yet if a Block of
 Marble, for instance, or an Iron Bul-
 let were remov'd into one of those
 empty spaces, that *Gassendus* and some
 others suppos'd to be beyond the
 bounds of this world, I see not why
 it should not be rather cold, then
 either warm, or in a state of perfect
 Neut

Neutrality: Since when the Corpuscles of Heat, and those of Cold had extricated themselves, and were flown away into the neighbouring *Vacuum*, the component Particles of the stone or metal, whose implicated Texture would hinder their Dissolution, remaining much less agitated than our Organs of feeling are by the warm blood and spirits, that vivifie them, must, if applied to those sensories, appear Cold.

2. But I shall not upon this subject spend any farther discourse, since perhaps the dispute, either may *be*, or at least may easily *be made* Verbal: For in case those I argue with, should so explain their opinion, as not to deny, that in its own nature the Air, left to its self, may be reputed *Cold* in reference to the sensories of men, who are warm animals: But say, that nevertheless, comparing it indefinitely to other then humane bodies here below, it is so easily susceptible of both the contrary qualities, that neither of them seems predominant in it; and that when it is considerably either cold or hot, it is made

so by adventitious agents : I shall not much contend with them, especially if it can clearly be made out, that there are great quantities of such cold spirits, as *Cabeus* and *Gassendus* suppos'd to be universally productive of cold (more or less) in all bodies, where they get admission ; but of these cold spirits more perhaps elsewhere. Our principal business in this Section being to deliver *Experiments* and *Observations*, and because we shall mention but few of the former sort, we will dispatch them first.

3. [November the 20. 1662. we took a Weather-glass fill'd to a convenient height with well rectifi'd spirit of Wine, and Hermetically seal'd, this we inclos'd in a glass Receiver of a Cylindrical form, of about two inches Diameter, and about a foot and a half high, and having cemented on the Receiver, we let it alone for some hours, that it might perfectly cool. Then drawing out the Air, and watching it narrowly, we observ'd, that the liquor in the Weather-glass descended a little, though but a very little upon the first
Ex-

Exuction of the Air, and a little, though it seem'd somewhat less, upon the second, but afterwards we did not find it sensibly to descend. This subsidence of the liquor in all amounting to about the length of a Barley corn, we attributed to the stretching of the glass by the spring of the included Air, when the ambient was withdrawn, and accordingly upon our allowing a Regress to the excluded Air, we saw the spirit in the Thermometer, rise about half a Barley-corns length to the place whence it began to subside. Afterwards we suck'd out, and let in the Air of the Receiver, as before, with like success, as to the descent and remounting of the liquor.

4. *N.B.* We tri'd with a very hot Handkerchief appli'd in a convenient place to the outside of the Receiver, whether the included Weather-glass would receive impressions from it, the Air, that was wont to be intermediate, being remov'd; but we did not find the liquor in the Weather-glass sensibly to swell, either by this way, or by casting upon it the concentrated

beams of a candle trajected through a double convex glass. But when the Air was readmitted into the Cavity of the Receiver, then the same Handkerchief, heated a fresh, and applied, made the spirit of Wine sensibly, though but little more, to ascend: Of which yet it seem'd something difficult by reason of the Nicety of the Experiment to estimate with any thing of certainty the Cause.] So that upon the whole matter, till the Experiment be repeated in Airs of differing tempers, to verifie, whether 'twas the withdrawing of the wonted pressure, or the recess of the substance of the Air, that made the liquor included in the Thermoscope subside, and till the Experiment be repeated with the further observation of other circumstances (which reiteration of the Trial we intended, but were by intervening accidents hindered) the recited Experiment will not afford much more then good hints towards the Discovery of the Temperature of the Air.

* In the
third Preliminary
Discourse.

5. I have * elsewhere taken notice, that air included in Vessels sufficiently

ly strong and well clos'd, was not *sensibly*, or at least not *considerably* condens'd by Cold, but when the Air was not so included, as not to be in some part or other expos'd to the pressure of the outward Air or Atmosphere, it would then by a degree of Cold, capable to freeze water, be manifestly reduc'd into a less room. But how much this Contraction or Condensation of the air may amount to, I did not there subjoin, nor has the measuring of it been, that I know of, attempted by any man. Wherefore we thought fit to endeavour something in this kind, of which we shall annex a brief account, whereby it will appear upon the whole matter, that in the Climate, we live in, the Cold does not so considerably condense the Air, as most men seem to have hitherto imagin'd.

6. And first, it will not be amiss to intimate, that among other ways we tried to measure the shrinking of the Air by sealing it up in glasses furnish'd with long and very slender stems, that by breaking off the tips of those glasses immers'd under water, when by
the

the Cold Air of a frosty night, or the Circumposition of snow and salt, the included air was highly refrigerated, the water might (by the pressure of the Atmosphere upon it) be impell'd into the Cylindrical cavity of the broken glass, and by its greater or lesser Ascent therein shew, how much the internal Air had been made to shrink upon the account of the Cold. But this way, for reasons too long to be here deduc'd, we found it troublesome and difficult to practise with any thing of certainty. Nor did we ever, that I remember, by this way bring the refrigerated air to lose above a 30. part of its former dimensions.

7. We would have tried also to measure the Condensation of the air by the ascent of water into the stem of a Bolthead, so inverted, that the orifice of the stem might be under the surface of the water, and the Bolt-head kept erected. But this way we disapproved, because it was likely (and indeed we found it so by experience) that the external air would first freeze the uppermost part of the
water

water contain'd in the ſtem, and thereby hinder its aſcent, and perhaps occaſion the burſting of the lower part of the ſaid ſtem.

8. Wherefore though for want of a ſufficient Quantity of ſome liquor, that would neither freez like water, and aqueous Bodies, nor congeal like common oyl, and the like unctuous Juices, we found it for a while ſomewhat difficult to practiſe the Experiment, yet bethinking our ſelves of the indiſpoſition that Brine has to Congelation, we made ſo ſtrong a Brine with common ſalt, that with it (and as I remember, with oyl of Turpentine alſo, of which we chanc'd to have ſome quantity by us) we made divers Trials, of which I had two among our Collections, which we ſhall here ſubjoyn, whereof the one informs us, that an Egg being inverted into ſalt water, the Cold of a froſty night made the air ſhrink in the Pipe near five inches ; and the other (which is the accurateſt I meet with among my Collections) gives me this account, That *January* the 29. the Air extended into 2057, ſpaces,
was

was by the cold of the sharp and frosty night contracted into 1965. spaces, so that in extraordinarily cold weather, the most we could make the Air lose of its former dimensions by the additional Cold of the Atmosphere, was a 22. part, and a little more then a third; And this was the greatest condensation of the Air, that we remember our selves to have observ'd, though we were so careful, as after we had placed marks, where the incongealable liquor reach'd in the pipe, that when the internal air was expos'd abroad to the cold, we caused servants to watch, and from time to time to take notice (by placing marks) of the various ascents of the liquor, especially early in the morning, least we should omit taking notice of the greatest contraction of the air, which omission (by reason that the Coldness of the ambient air does oftentimes begin to be remitted before we can feel it to be so) is not easily avoided without watchfulness.

9. But having thus observ'd the Condensation of included air by the natu-

ral

ral and unassisted Cold of the external air, we thought fit to prosecute the trial somewhat further, and in regard we conceiv'd the Cold of a mixture of snow and salt to be far more intense, then that of the mere ambient air alone, we endeavour'd to measure, as near as we could, how much the one exceeded the other: And though we found, that by prosecuting the lately mention'd Trial in the glass-Egg by the application of ice and salt to the Elliptical part of the vessel, the liquor rose by our Estimate near four inches more (then those five w^{ch} it had risen already, upon the account of the Refrigeration of the included air by the bare cold of the external:) Yet by prosecuting the other Experiment (made the 29. of *January*) at the same time, when we were making it, we did somewhat more accurately determine the matter. For by applying ice and salt to the outside of the vessel, we found, that the included air was contracted from 1965. spaces, to which the Cold of the ambient air had reduc'd it, into 1860. spaces, so that the

the Circumposition of ice and salt did as much, nay somewhat more condense it, after the mere Cold of the external air had contracted it as far as it could, then the bare, though intense, Cold of the ambient air could condense it at first, and the greatest degree of adventitious Cold we were able to give by the help of nature or of art, did not make the air expos'd to it, lose a full tenth part of its former Dimensions : on which occasion it may not be unworthy observation, That there is no greater Disparity betwixt the proportion in which the Cold was able to condense the Air, and that wherein the Cold was able to expand water.

10. This is all that at present I think fit to say concerning the interest that Winds may have in the Temperature of the Air. And therefore I will now proceed to those other particulars, wherewith I not long since said, that I intended to close up this Section; and I might on this occasion subjoyn many things, but partly haste, and partly other considerations will confine me to those, that relate to the

the effects of Cold upon the Air in a more general way.

11. And first, we will observe, that Cold may hinder in an almost incredible measure, the warming operation of the Sun upon the Air, not only in the hottest part of the Day (for that may sometimes happen, even in our Climate) but at several times of the Day, even in the heat of Summer.

12. I remember I once accidentally met with an intelligent and sober Gentleman, who had several times sail'd upon the frigid Zone, and though an intervening accident separated us so suddenly, that I had not opportunity to obtain from him the resolution of above two or three questions; yet this I learned of him belonging to our present purpose, That by the help of a Journal he kept, he call'd to mind, that upon the coast of *Greenland* he had observ'd it to snow all Midsummer night, which affirmation of so credible a person, imboldens me to add some other relations, which I should else have scrupled at.

Mr.

13. Mr. *Logan* an English Merchant, that Winter'd at *Pecora*, one of the Northern Towns of *Muscovy*, relates, that being there at a great Salmon-fishing, there hapned about the close of *August* (which in many Countries is wont to be the hottest time of all the year) so strong a Frost, which lasted till the fourth day, That the *Ozera* was frozen over, and the Ice driving in the River to and again, broke all the Nets, so that they got no Salmon, no not so much as for their own *Viſuals*.

Purchase
lib. 4. pag.
542.

14. Captain *G. Weymouth* mentions, that in *July*, though he was not near the Latitude of *Nova Zembla*, much less of *Greenland*, yet sailing in a thick fog, when by reason of the darkness, it occasioned, he thought good to take in some of his sails, when his men came to hand them, they found their Sails, Ropes, and Tacklings so hard frozen, that it did (says he) seem very strange unto us, being in the chiefest time of Summer.

Purchase
pag. 811.

These voy-
ages are
extant in
Purchase
lib. 1. cap.
13. and
this pas-
sage is in
pag. 560.

15. In the fifth Voyage of the English to *Cherry Island*, which lies betwixt 74. and 75. degrees of Latitude, they observ'd, that the wind being at north-

North-east upon the 24. of July, *It* freez'd so hard, that the Ice did hang on their clothes. And in the seventh Voyage (which was made three years after) to the same Island, they mention, that on the 14. of July the wind being Northerly, they had both snow and frost. *Purchase pag. 564.*

16. The next thing that we shall take notice of, is the degree of Cold, which the Efficient causes of that Quality, whatever they be, are able to produce in the air; but of this we must not here treat indefinitely, the strange effects of cold upon other bodies being most of them produc'd by the intervention of the cold first diffus'd in the Air, and those are treated of in a distinct Section, wherefore we shall now give two or three instances of the sudden operations of the Cold harbour'd in the Air.

The formerly mention'd English Ambassador into *Russia*, Dr. Fletcher, gives us two instances very memorable to our present purpose. *When you pass (says he) out of a warm Room into a Cold, you will sensibly feel your breath to wax stark, and even stifling* *Purchase pag. 415.*

with the cold, as you draw it in and out. So powerfully and nimbly does the intensely refrigerated Air work upon the Organs of respiration.

[And whereas a very credible person, now chief Physician to the *Russian* Emperor, being ask'd by me concerning the truth of what is reported, sometimes to happen at *Musco*, and is reputed the eminentest proof that is readily observable of the extreme coldness of the air, assur'd me, that he himself saw the water thrown up into the air, fall down actually congeal'd into ice: Dr. *Fletcher* confirms this Report. For] our Ambassador also says, That the sharpness of the Air you may judge of by this, for that water dropped down, or cast up into the Air, congeal'd into Ice before it come to ground. And I remember, that inquiring about the probability of such Relations, he answered me, That being at the famous Seige of *Smolensko* in *Russia*, he observ'd it to be so extremely cold in the fields, that his Spittle would freez in falling betwixt his mouth and the ground, and that if he spit against a Tree, or a piece

Purchase
pag. 414.

piece of wood, it would not stick, but fall to the foot of it.

17. Among the *Phænomena* of Cold, relating to the air, I endeavour'd to observe, whether upon the change of the Weather, from warm or mild, to cold and frosty, there would appear any difference of the weight of the Atmosphere by its being plentifully furnish'd with a new stock of such frigorifick Corpuscles as several of the modern Philosophers ascribe its coldness to, but though I several times observ'd by comparing a good Barometer (and sometimes also unseal'd Weather-glasses furnish'd one with a tincted Liquor, and the other with Quicksilver) with a good seal'd Weather-glass, furnished with pure spirit of Wine, that upon the coming in of clear and frosty weather, the Atmosphere would very early appear sensibly heavier then before, and continue so, as long as the cold and clear weather lasted; yet by reason of some considerations and Trials, that breed some scruple in me, I refer the matter to more frequent and lasting observations, then I yet have been

able to make, in which it will concern those that have a mind to prosecute such Trials, not only to consider, whether or no the increased gravity of the Atmosphere may not proceed from some other Cause, then the coming of frigorifick Atoms into the Air; but to have a special care, that their Barascopes be more carefully freed from the Air, that is wont to lurk in Quicksilver it self, as well as other Liquors, then those in the making of the Torricellian Experiment Tubes usually are, lest that Air getting up into the deserted part of the Tube, do by its expansion and contraction, obtain an unsuspected interest in the rising and falling of the subjacent Mercurial Cylinder, and so impose upon them.

18. Another Effect that the Cold especially in Northern Countries has oftentimes upon the Atmosphere, is, the making the Air more or less clear then usually it is. For in the Northern Voyages, the Seamen frequently complain of thick and lasting Fogs, whose causes I shall not now consider, but some help to guess at them
may

may be given by what we are about to add, namely, that it very frequently happens on the contrary, That when the cold is very intense, the air grows much clearer then at other times, probably because the Cold by condensing precipitates the vapours, that thicken the air, and by freezing the surface of the earth, keeps in the steams, that would else arise to thicken the air. Not to dispute, whether it may not also somewhat repress the vapours, that would be afforded by the water it self, since some of our Navigators observe, that even when it was not cold enough to freeze the surface of the Sea, it would so far chill and infrigidate it, that the snow would lye on it without melting.

19. I remember a *Swedish* extraordinary Ambassador, and a very knowing person, whom I had the honour to be particularly acquainted with, would say, when he saw a frosty day accompanied with great clearness, that it then look'd like a *Swedish* winter, where when once the frosty weather is settled, the sky is wont for a

very long time to be very serene and pleasant, and here in *England* we usually observe the sharpest frosty nights to be the clearest. But to confirm our Observation by a very remarkable instance, I shall borrow it from a Navigator very curious of Celestial Observations, which circumstance I mention to bring the greater credit to the following observation of Captain *James*, which in his Journal is thus delivered: *The thirtieth and one and thirtieth of January, there appeared in the beginning of the night more Stars in the Firmament, then ever I had before seen by two thirds. I could see the Cloud in Cancer full of small Stars.*

Fig. 62.

20. To determine what effect the coldness of the air may have upon the Refractions of the Luminaries and other Stars, I look upon as a work of no small difficulty, and that would require much consideration as well as time, wherefore I shall only add two or three narratives, supplied me by Navigators, without adding at present any thing to the matters of fact.

21. The first is that famous Observation of the *Dutch* in *Nova Zembla*, who
take

take great pains to evince by several circumstances, some of them highly probable, that they were not mistaken in their account of time, according to which they concluded, that they saw the Sun, whom they had lost sight of eleven weeks before, about fourteen days sooner then he ought to have appear'd to them, which difference has been, for ought I know to the contrary, by all that have taken notice of it, ascrib'd to the strangely great Refraction in that Gelid and Northern air.

22. And as for that other extremely cold Country, where Captain *James* wintered, it appears by his Journal, that he there made divers Celestial, and other observations, which gave him opportunity to take notice of the Refraction, and he seems to complain, that he found it very great, though among the particulars he takes notice of, there are some that seem not very strange, nor are there any that are near so wonderful, as that newly mention'd of the *Hollanders* in *Nova Zembla*, however in regard of the extreme coldness of the Winter

air in Charleton Island, it may be worth while to take notice of the following passages out of his Journal, since they may at least help us to conjecture what is not to be expected in reference to Refractions from the coldness of the air as such. The 21.

Pag. 61.

of January (says he) *I observed the Latitude with what exactness I could (it being very clear Sunshine weather) which I found to be 51. 52. This difference is by reason that here is a great Refraction.*

Pag. 46.

Which last clause is very obscure, unless it refers, as one may guess it does, to what he had elsewhere said, That his first coming to the Island, he took the Latitude with two Quadrants, and found it to be just 52. degrees, without any minutes. Elsewhere; my ob-

Pag. 64.

servations (says he) *by these glasses I compar'd to the Stars coming to the Meridian. By this means we found the Sun to rise twenty minutes before it should, and in the evening to remain above the horizon twenty minutes (or thereabouts) longer then it should. And all this by reason of the Refraction.*

And in another place, *March the*

15. *This evening (says he) the moon* Pag. 66.
rose in a very long oval amongst the Hori-
zon.

I shall add one passage more out of our Author, concerning Refractions, not only because it may bear Testimony to some relations of the like kind, that I have mention'd in another Treatise; but because it is concluded with an observation, that (if there be nothing of mistake in it) is odd enough. *I had often (says he) observed the difference betwixt clear weather, and misty Refractive weather in this manner. From a little Hill, which was near adjoyning to our house, in the clearest weather, when the Sun shone, with all the purity of Air that I could conceive, we could not see a little Island, which bare of us south south-east some four leagues of; but if the weather were misty (as afore-said) then we could often see it from the lowest place.* Pag. 69.

23. Hitherto I have treated of the Temperature of the Air in general, and though the past Discourse have been prolix enough, yet possibly I may have no fewer things to say, if I would at present fall upon the particular

* A Scep-
tical Dis-
quisition of
Antiperi-
stasis.

cular consideration of the three Regions into which the Air is wont to be distinguished. For I confess I am not altogether without scruples, both as to the Number, and as to the Limits, and as to the Qualities assign'd to these Aerial Regions. But (as I have partly declar'd in another * Tract) though I had time to enter upon so intricate a Disquisition, yet till I have an opportunity to consult some other papers, I know not whether what I have noted touching these difficulties, may not more properly belong to another Treatise, than this of Cold.

24. Having thus dispatch'd the few Experiments I can meet with among my papers, concerning the Coldness of the Air, I now proceed to subjoyn some observations, that have occur'd to me in the writings or verbal Relations of Navigators and Travellers about that subject. But in regard, that the greatest part of the *Phænomena* of Cold, which nature of her own accord presents us with, seem to be produc'd, either mediately or immediately by the Air, we intend not here

here to treat of the coldness of the air in the largest sense, but only to take notice of some of the choicer instances, that seem to belong to our present Argument. And these we shall annex, either as Promiscuous Observations at the Close of this Section, or as Illustrations or proofs of the three following Observations.

I. The first I shall propose in these terms, *that the greater or lesser coldness of the Air in several Climates and Countries, is nothing near so regularly proportionate to their respective distances from the Pole, or their vicinity to the Equator, as men are wont to presume.*

This puts me in mind of what I have formerly, either heard from a skilful man, or observ'd my self about the difference betwixt places of the same latitude in the Northern and Southern Hemisphere; namely, That of places equally distant the one from the Northern, the other from the Southern Pole, the latter are generally much colder then the former. And as I remember, I long since noted some things to this purpose; but being not at present able to recover them,

them, I shall propose this only, as that which may deserve an inquiry, being not yet satisfi'd, but that in the Examples I had taken notice of, some accidental and concurrent causes may have occasion'd the greater coldness observ'd in the places seated on the other side of the Line; as on this side of it, the like causes may much vary the coldness of differing places of equal latitudes, as we are now going to shew by the following testimonies.

1. How excessive a Cold reigns at *Musco* and thereabouts in the Winter time, when many men lose their noses or their toes, and some their lives by the extremity of the cold, we have several times occasion to take notice of in this Treatise. And yet at *Edenburgh*, which I find some of our modern Navigators to place more Northerly by above a degree, there, I say, and in the neighbouring places, the air is known to be temperate enough, and the cold very tolerable: And 'tis affirm'd, that the snow very rarely lyes any long time on the ground after it is fallen.

2. In

2. In the Voyage made for discovery northward, by Mr. Poole, in the year 1610. I find this passage, *I was* Pag. 702.
certifi'd, that all the Ponds and Lakes were unfrozen, they being fresh water, which putteth me in hope of a mild Summer here, after so sharp a beginning, as I have had, and my opinion is such (and I assure my self it is so) that a passage may be as soon attain'd this way by the Pole, as any unknown way whatsoever, by reason the Sun doth give a great heat in this Climate; and the Ice (near the 79. degree) I mean that that freezeth here, is nothing so huge as I have seen in 73. degrees.

To this agrees the testimony of the *Hollanders* in their first Voyage to *Nova Zembla*, in which the writer of it, *Gerat de Veer*, speaks thus, *We have* Pag. 473.
assuredly found, that the only and most 474.
hinderance to our Voyage, was the Ice, that we found about Nova Zembla, under 73, 74, 75, and 76. degrees, and not so much upon the Sea, between both the lands, whereby it appeareth, that not the nearness of the North Pole, but the Ice that cometh in and out from the Tatarian Sea about Nova Zembla, caused

us to feel the greatest cold. Therefore in regard, that the nearness of the Pole was not the cause of the great cold that we felt, &c. And a little after, — It is true (says he) that in the Country lying under 80. degrees (which we esteem to be Greenland) there is both leaves and grass to be seen, wherein such beasts, as feed of leaves and grass, as Harts, Hinds, and such like beasts, live, whereas to the contrary in Nova Zembla, there groweth neither leaves nor grass, and there are no beasts there, but such as eat flesh, as Bears and Foxes, &c. although Nova Zembla lyeth 4, 5, and 6. degrees more Southerly from the Pole, then the other land aforesaid.

*Acosta lib.
2. cap. 9.
pag. 101.*

And to this purpose I remember what is related by the learned Josephus Acosta, concerning the Heats and Colds in the Torrid Zone, and elsewhere: When I pass'd (says he) to the Indies, I will tell what chanc'd unto me, having read what Poets and Philosophers write of the burning Zone, I perswaded myself, that coming to the *Æquinoctial*, I should not indure the violent heat, but it fell out otherwise, for when I pass'd, which was when the Sun was there for Zenith,

nith, being entered into Aries, in the moneth of March I felt so great a cold, as I was forc'd to go into the Sun to warm me : what could I else do then but laugh at Aristotles *Meteors*, and his Philosophy, seeing that in that place, and at that season, when as all should be scorch'd with heat, according to his rules, I and all my companions were a cold ? in truth there is no Region in the world more pleasant and temperate, then under the Equinoctial, although it be not in all parts of an equal temperature, but have great diversities. The burning Zone in some parts is very temperate, as in Quitto, and on the plains of Peru, in some parts very cold, as at Potosi, and in some very hot, as in Ethiopia, Brasile, and the Molucques. And within two Chapters after, he discourses more largely of some of these Particulars. And again Chapter the 12. You may continually (says he) see upon the tops of these mountains snow, hail, and frozen waters, and the cold so bitter, as the grass is all wither'd, so as the men and beasts, which pass that way, are benumm'd with cold. This, as I have said, is in the burning Zone, and it happens most commonly when

when they have the Sun for Zenith.

Purchase
pag. 578.

These Testimonies of a learned man, that writes upon his own knowledge, I thought it worth producing, to make it probable, that as in several Countries the heat does not always answer to the nearness of places to the Line, so in Northern Regions the cold may not always be proportionate to their vicinity to the Pole. In Mr. Hudsons second voyage written by himself, he mentions that above 71. degrees, though they were much pester'd with ice, about the end of June, that day (when this hapned) was calm, clear, and hot weather, adding of the next day also, that it was calm, hot, and fair weather. And Acosta tells us, that we see these differences, not only on the land, but also on the Sea: there are some Seas where they feel great heat, as the report of that of Mazambigus, and Ormus in the east, and of the Sea of Panama in the west. There are other Seas in the same degree of height very cold, as that of Peru, in the which we were a cold, when we first sail'd it, which was in March, when the Sun was directly over us. In truth

truth on this continent, where the Land and Sea are of one sort, we cannot imagine any other cause of this so great a difference, but the quality of the wind that doth refresh them.

But to multiply no more instances, we shall conclude with this one, That *Charleton Island*, where Captain *James* winter'd (and of which we so often have occasion to make mention in our History) though it seems by the effects to be a colder Region, then even the Countrey about *Musco*, and perhaps as cold as *Nova Zembla* it self; yet Captain *James*, who had several times occasion to take the latitude of it, and assigns it the same Elevation, and consequently, the same Distance from the Pole with *Cambridge*, whose latitude he reckons to be 51. degrees besides minutes, and whose air is very well known to be very temperate. And it is remarkable, that though this place, whose latitude is short of 52. degrees, was found uninhabitable by reason of the cold, yet not only in Mr. *Hudsons* Voyage, the writers admonish the Readers to take notice, That although

See James
voyage,
pag. 61. &
81. and
elsewhere;

Purchase
pag. 569.

Josephus
Acosta lib.
2. pag.
111, 112.

they ran along near the shore, they found no great cold, which made them think, that if they had been on shore the place is temperate: And yet in this place they reckon themselves to have reach'd the 78. degree of latitude: And our recent Navigations inform us, that several parts of *Greenland*, to which this newly mentioned coast belong'd, are well enough inhabited: And one of our English Navigators assures us, that the true height of *Pustozera* in *Russia* is no less then 68. degrees and a half, if not more, and yet that is a town not only well inhabited, but of great trade; but in *Hudsons* voyage I find what is more strange, That under the 81. degree of latitude, beyond which they discovered land very far off, but (beyond which none is thought to have actually sail'd toward the Pole) they found it during the whole day clear weather, with little wind, and reasonable warm. And beyond 80. degrees, they not only found a stream or two of fresh water, but found it hot on the shore, and drank water to cool their thirst, which they also commended.

Purchase
pag. 571.

II. The

II. The next observable I am to propose about the coldness of the Air, is this, That the degrees both of Heat and Cold in the air may be much greater in the same climate, and the same place, at several seasons of the year, or even at several times of the same day, then most men would believe.

For the proof of this Proposition, we shall subjoyn two sorts of Testimonies, of Travellers, and Navigators, the former shewing, that in Countries, where it is very cold in Winter, it may nevertheless be hot in Summer; and the latter manifesting, that even on the same day, as well as in the same place, the heat and cold, that succeed one another, may be one of them sensible, though the other were extreme, or may perhaps be both of them considerable.

To make this good, we shall produce the following Testimonies.

1. Dr. *Giles Fletcher*, English Ambassador to the *Muscovian* Emperor, in his Treatise of *Russia*, and the adjoining Regions, has this memorable

Kk 2

passage

passage to our present purpose. The
 whole Countrey (says he) differeth very
 much from itself, by reason of the year,
 so that a man would marvel to see the great
 alteration, and difference betwixt the
 Winters and Summers in Russia. The
 whole Countrey in the Winter lyeth under
 snow, which falleth continually, and is
 sometime of a yard or two thick, but great-
 er towards the North; the Rivers, and
 other waters are all frozen up, a yard or
 more thick, how swift or broad soever
 they be, and this continueth commonly for
 five moneths, to wit, from the beginning
 of November, till towards the end of
 March; what time the snow beginneth to
 melt, so that it would breed a frost in a
 man to look abroad at that time, and see
 the Winters face of that Countrey. And
 a little after he adds: And yet in the
 Summer time you shall see such a new hew
 and face of a Countrey, the woods (which
 for the most part are all of Fir and Birch)
 so fresh, and so sweet; the Pastures and
 Meadows so green, and well grown
 (and that upon the sudden) such variety
 of flowers, such noise of Birds (especially
 of Nightingals, that seem to be more loud,
 and of a more variable note, then in other
 Coun-

Pag. 414.

Purchase
pag. 415.

Countries) that a man shall not lightly travel in a more pleasant Countrey. And some lines after, *As the Winter exceedeth in cold, so the Summer inclineth to over much heat, especially in the moneths of June, July, and August, being much warmer then the Summer Air in England.*

Almost like things have been much more recently affirm'd by the learned *Olearius*, Secretary to the Duke of *Holstein's* Embassy into *Russia*, and now *Bibliothecarius* to the present Prince of *Holstein*. And an acquaintance of mine, who, after having liv'd in *Italy*, pass'd a Summer in *Russia*, assur'd me, that he scarce in *Italy* did ever eat better Melons, then some which he had eaten at *Musco*, of a strange bigness, which bears witness to that almost incredible Relation of *Olearius*, who (after having much prais'd their goodnels at *Musco*) affirms, that he there met with Melons of 40. pound weight, of which he there teaches the Culture.

*Voyage de
Moscovie
& de Per-
se, livre 3.
p.m. 117,
118, 119.*

Pag. 119.

At the royal City of *China*, *Pequin*. which scarce exceeding the 42. de-

degrees of latitude, one would expect, that as the Summer is very warm, so the Winter should be very mild, as it is observ'd to be in divers places of *Spain, Italy, and Greece*, that have the same, or a more Northern latitude: and yet the learned Jesuite *Martinus*, who liv'd many years in *China*, assures us, that usually for four whole moneths together, all the Rivers are so hard frozen, that not only all Ships are clos'd, and kept immovable by the Ice, but that also horses, wagons, and even the heaviest carriages do securely pass over the Ice. Concerning which, he adds this strange circumstance, that 'tis usually made in one day, though to its dissolution it require many.

Lib. 1. cap.
6.

Prosper Alpinus in his learned *Treatise de medicina Aegyptiorum*, tells us, that at *Grand Cayro*, where he practis'd Physick, though that famous *Metropolis* of *Aegypt* be distant but six degrees from the Tropick of *Cancer*, yet the Air, which in Summer is almost insupportably hot, in Winter is sometimes very considerably cold; adding, that there is not
any

any sort of Diseases that proceed (as he is pleas'd to speak) from distillations from the head, to which the people are not there subject : To these instances we shall annex but two more, but those remarkable ones.

The first is mention'd by *Purchase*, as communicated to him by an eye witness, in these words. This I thought good at our parting to advertise thee, That Mr. *Hebey* hath affirm'd to me, touching the diversity of weather in *Greenland*, that one day it hath been so cold (the wind blowing out of some quarter) that they could scarce handle the frozen Sails ; another day so hot, that the pitch melted of the Ship, so that hardly they could keep their Clothes from pollution : yea, he hath seen at midnight Tobacco lighted or fired by the Sun beams with a glass. The other example I am to produce, is no less remarkable ; namely, that in the often mention'd *Charleton Island*, where that winter was as sharp, perhaps as any known place of the habitable world, Captain *James* his Journal gives us this account of the

Pag. 81.

weather : In June the sixteenth (says he) was wondrous hot, with some thunder and lightning, so that our men did go into the Ponds ashore to swim, and cool themselves, yet was the water very cold still. Here had lately appeared divers sorts of Flies, as Butterflies, Butcher-flies, Horse-flies, and such an infinite abundance of blood-thirsty Muskitoes, that we were more tormented with them, then ever we were with the cold weather. These (I think) lye dead in the old rotten wood all the Winter, and in Summer they revive again. Here be likewise infinite companies of Ants, and Frogs in the Ponds upon the land.

Thus we see, what difference there may be in the same place, betwixt the temperature of the Air in Winter, and Summer. We shall now add what may appear more strange, that there may be very great disparities in the heat and coldness of the air, not only in the same place, but within the compass of the same day.

The lately mention'd *Alpinus*, affords me an example to this purpose, in *Egypt* its self, where one
would

would expect a much more uniform heat. Hyeme (says he) *nocturnus aer admodum frigidus observatur, qui ob orto* Pag. 9.
sole paulo post, parum incalcescit, in meri-
dieque plurimum: adveniente vere no-
cte rursum infrigidum permutatur, ita,
ut aer ille valde inæqualis sit dicendus,
ab ipsiusque illa inæqualitate plurimi
morbi originem ducunt atque generantur,
qui eo tempore per urbem vagantur.

The learned Olearius relating how he travelled with the Ambassadors, whose Secretary he was, over a branch of mount *Taurus*, takes notice, that it being after the middle of *June*, the air of that hot region of *Persia* oblig'd them only to travel by night, and yet the nocturnal cold was so great, that they were all benumbed with it, insomuch, that they were hardly able to alight from their Horses; adding, that the sudden change from an extreme cold, to the excessive heat, they were again expos'd to the next day, cast no less than 15. of their company into strong burning feavers at once. (Which brought into my mind the complaint of good *Jacob*, who, though he liv'd
in

in an Eastern Countrey, when he had said, *that in the day the drought consumed him, adds, and the frost by night.*)

And the same curious traveller mentions, that in another Countrey in *Persia*, call'd *Faclu*, notwithstanding the heat of the region (at the end of *March*, at which time they pass'd that way) they saw and felt in one night, which they were forc't to pass without their tents, both lightning, and thunders, and winds, and rain, and snow, and ice.

We will conclude with a remarkable instance, afforded us by the Journal of the English that wintered at *Charleton Island*. The season here in this Climate (says the often quoted Author of the voyage) is most unnatural ; for in the day time it will be extreme hot, yea, not indurable in the Sun, which is, by reason that it is a sandy Countrey. In the night again, it will freeze an inch thick in the Ponds, and in the Tubs about, and in our house, and all this towards the latter end of June.

III. The third observable I intended to take notice of, about the Coldness of the Air, may be compriz'd in this

this Proposition, That in many places the Temperature of the Air, as to Cold and Heat, seems not to depend so much upon the Elevation of the Pole, as upon the Nature and Circumstances of the winds that blow there.

It would require a very long Discourse, to treat in this place of Winds in general, and much more to examine the several causes of winds, that are assign'd by several Authors, and therefore when I have once given this intimation, that divers of these opinions may be more easily reconcil'd, than the maintainers of them seem to have thought, to the Truth, if not to one another: The causes that may produce wind, being so various, that many of those propos'd, may each of them in some cases be true, though none of them in all cases be sufficient: having hinted this, I say, it may suffice on this occasion, to subjoyn three or four observations, to prove and illustrate the matter of fact delivered in the Proposition.

And first, 'tis a known Observation

vation in these parts of the world, that Northerly and Northeasterly winds, do at all times of the year bring cold along with them, and commonly if it be Winter, Frost. And here in *England* I have sometimes wondred at the power of the winds, to bring not only sudden Frosts, but sudden Thaws, when the frost was expected to be settled, and durable, which yet seems to hold commonly, but not without exception. For during one of the considerablest Fits of Frost and Snow, that I have taken notice of in *England*, I remember, that I observed (not

The weather was snowy and foggy, freezing our rigging, and making every thing so slippery, that a man can scarce stand. And all this with the wind Southerly, says Captain James (page 104.) in his Journal 26. of August.

without some wonder) that the Wind was many days Southerly, unless it may be said, That this

Southerly Wind was but the Return of a stream of Northerly Wind, which had blown for many days before, and might by some obstacles, and agents, not here to be inquir'd after, be made to wheel about, or recoil hither, before it had lost the greatest

greatest portion of the refrigerating Corpuscles it consisted of before.

The formerly mention'd *Prosper Alpinus*, attributes strange things to the Northerly wind, that blows in *Ægypt*, as to the cooling and refreshing the Air, in spite of the violent heats, that would otherwise be intollerable. (And many in *Ægypt*

* ascribe to the *Ætesian* Winds, that almost miraculous ceasing of the Plague at *Grand Cairo*, of which we else-

* *Ab his ventis aerem alteratum, esse causam cur pestis illa dissolvatur, multi illorum Affirmant. Quod etiam non videtur penitus à veritate alienū, quando id multis etiam rationibus nobis persuaderi possit, in primisque, &c. Prosp. Alpin. lib. 1. De medicina Ægypt. cap. 18.*

where speak.) *Dominatur autem aer* Ibid. lib. 1. cap. 6. (says he) *summè calidus, ipsius cæli,* cap. 6. *ut dictum est, ratione, quod hæc civitas à Tropico Cancræ tantum 6. gradibus distet. Quâ brevi inter-capedine dum sol ad illum accedit Tropicum, & illorum Zenith sit propinquior, aer ille valdè incalcescit, & nisi Ætesia venti tunc à septentrione spirarent, vehementissimus, & qui vix à nostris perferri possit, caloris æstus sentiretur.*

Advenæ nostri iis provenientibus ad Ibid. lib. 1. cap. 7. pag. 11. *subterranea loca confugiunt, in quibus morantur*

morantur quousque ille ventorum ardor
residerit atque cessaverit. Conjunxit
hæc incommoda Deus Optimus, cum aliis
quibusdam bonis, nam ubi calidissimi illi
venti conticuere, statim à Septentrione
flare alii incipiunt, qui subitaneum in-
flammatis atque laxatis corporibus solati-
um præstant. Si enim illi diu perseve-
raverint, nemo in eâ regione vivere possit.

Whence winds should have
this power to change the Constituti-
on of the Air, and especially to bring
cold along with them, is not so easie
to be determin'd. Indeed the other
Qualities, and even the heat, that is
observable in winds, may for the
most part be probably enough deriv'd
from the Qualities of the places, by
which they pass. Of this we have
already given an example or two in
the passages lately mention'd. And
it may be further confirm'd by what
Acosta lays, that he himself saw in
some parts of the *Indies*: namely,
That the Iron Grates were so rusted and
consumed by a peculiar wind, that pres-
sing the mettall between your fingers, it
would be dissolv'd, and crumbled, as if
it had been Hay or parched Straw. And
this

Josephus
Acosta, lib.
3. cap. 9.

this Learned Traveller, who seems to have taken peculiar notice of the winds, affords us in divers places of his Book several Examples to confirm what we were saying (though he take not the nature of the regions, along which the wind blows, to be alone in all cases a sufficient Cause of their Qualities) of which yet we shall now mention but these two memorable passages. *In a small distance* Lib. 3. cap. 2. p. 120.
 (says he) you shall see in one wind many diversities. For example, the Solanus or Eastern wind is commonly hot and troublesome in Spain ; and in Murria, it is the coldest and healthfullest that is, for that it passeth by the Orchards, and that large Champiane which we see very fresh. In Carthagene, which is not far from thence, the same wind is troublesome, and unwholsome. The Meridional (which they of the Ocean call South, and those of the Mediterranean Sea, Mezo Giorno) commonly is rainy, and boisterous, and in the same City, whereof I speak, it is wholsome and pleasant. And in his Description of Peru, speaking of the South and South-west, Lib. 3. cap. 3.
 he affirms, that this wind yet in this region

region is marvellous pleasing.

But though, as we were saying, many other Qualities of winds may be deduc'd from the Nature and Condition of the places, by which they pass: And though the heat also, which *Prosper Alpinus* (as we lately took notice) attributes to the Southerly winds, that blow in *Egypt*, may be probably ascrib'd to the heated Exhalations and vapours they bring from the Southern and parched Regions they blow over; yet whence the great coldness of Northern and Easterly winds should come, may be scrupled at by many of the modern Philosophers, who with divers *Cartesians* will not admit, that there are any Corpuscles of Cold.

And possibly I could, about these matters, propose some other difficulties, not so easie to be resolved. But not being now to discuss the *Hypothesis* about Cold, I think it will be more proper in this place, instead of entring upon disputes and Speculations, to subjoyn an Experiment that I made, to give some light about this matter.

Con-

Considering then that I had not met with any Trial of the Nature of that I am about to mention, and that such a Trial might possibly prove Luciferous, I caused a pretty large pair of ordinary Bellows to be kept a good while in the Room, where the Experiment was to be made, that it might receive the Temperature of the Air in that Chamber, then placing upon a board, one of those flat Bottom'd Weather-glasses, that I elsewhere describe to contain a movable drop of pendulous water, by blowing at several times with intermissions upon the bubble or lower end of the Weather-glass, though the wind blown against my hand, were, as to sense, very manifestly cold, yet it did not cool the air included in the Bubble, but rather a little warm'd it, as appear'd by a small, but sensible, ascension of the pendulous drop each time, that, after some interpos'd rest, the lower part of the glass was blown upon, which seem'd to proceed from some small alteration towards warmth, that the air received by its stay (though short) in the Bellows,

as seem'd deducible from hence, that if by closely covering the Clack, the matter were so ordered, that the Air, that should come into the Bellows, must come in all at the nose; if this nose being held very near the bubble of the Weather-glass, the Air were, by opening the Bellows, suddenly drawn in, that stream of air or wind coming from a part of the window, where the air was a little cooler, then that which was wont to come out of the Bellows, would not, as the other, make the pendulous drop rise, but rather the contrary.

This done, we proceeded to shew by Experiment, That though a wind were nothing, but a stream of Air, yet in its passage it might acquire a considerable coldness distinct from that which it has by vertue of its motion, though upon the score of that, we see that air mov'd by a fan, (or as in our newly mentioned Trial) by a pair of Bellows, might to our touch, feel Cold, nor did we forbear to expect a good event of our Trial, upon the doubt that may be rais'd, whether there be frigorifick
Cor-

Corpuscles or no : For whatever be-
 come of that question, I thought I
 might expect, that whether or no Ice
 emit Corpuscles, that are universal-
 ly frigorifick, yet the air being, ei-
 ther by them, or upon what account
 soever, highly refrigerated, the
 Corpuscles that compose this cold
 Air, being most of them driven on
 before it, by the wind that meets
 them in its way, will, in a sense,
 prove frigorifick, in regard of a les
 cold body, which they shall happen
 to be blown upon, and accordingly,
 having provided a ridge Tyle invert-
 ed, and half fill'd the Cavity, which
 look'd upwards, with a mixture of
 ice and salt, and having likewise put
 the Iron pipe of the Bellows upon
 that mixture, and then covered it
 with more of the same, that so the
 Pipe being surrounded, as far as con-
 veniently it could be, with ice and
 salt, the air contain'd in it, might
 thereby be highly refrigerated, I
 found, that blowing wind out of the
 Bellows upon my hand, that wind
 felt much more cold, then that which
 had been before blown upon my hand,

out of the same Bellows, before the frigeſactive mixture was appli'd to it. But for fear my ſenſe of feeling ſhould deceive me, I cauſ'd a Weather-glaſs, made after the common manner, but with a more ſlender pipe, to be ſo plac'd, that the noſe of the Bellows (which together with the Tyle and Ice, was upheld with a frame) lay in a level with the bubble of the Thermometer, and then blowing the refrigerated air of the Bellows upon the globular part of the glaſs, I ſaw the water in the Cylindrical part and ſhank, manifeſtly aſcend, as it was wont to do upon the refrigeration of the included air: And as this Aſcenſion of the liquor continued, during three or four blaſts of the Bellows, ſo upon the ceſſation of the artificial wind, the water ſubſided by degrees again, till by freſh blaſts it was made to aſcend. Laſtly, having repeated this Experiment, we thought fit to trye, how much the air, refrigerated immediately by the frigorifick mixture, would produce a colder wind then the former, and accordingly, drawing back the noſe of
of

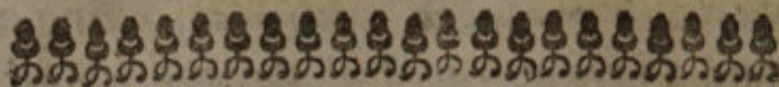
of the Bellows, that the air, that should be blown out, might pass along the Cavity left in the frigorifick mixture by the Iron pipe (of the Bellows) which we had withdrawn, the wind was manifestly more cold, then before, and had a greater operation on the Weather-glass, it was blown upon.

This Experiment, if carried on, and prosecuted, may possibly prove more Luciferous; but I will not take upon me here to determine, whether all cold winds must be necessarily made so, by frigorifick Corpuscles properly so call'd, since I have sometimes suspected, that some winds may be cold, only by consisting of, or driving before them, those higher parts of the Air, that, by reason of the languid Reflection of the Sunbeams, in that upper (or perhaps Arctick) region of the Air, are for the most part very cold. For it may be observ'd, that Rains oftentimes very much and suddenly refrigerate the lower Air, when no wind, but what the clouds and rain make, accompanies them, as if they

brought down store of cold air with them from that uper Region; which *Acosta*, and one I conversed with, that visited far higher mountains, then the *Alps*, affirm to be in some places (for I am not satisfi'd, that 'tis so every where) exceedingly cold, both in hot Climates, and in hot seasons of the year. And I observe, that the *Hollanders* do, in more places then one or two, mention the Northerly and North-easterly winds, to be those, that brought them the prodigious colds they met with, though *Nova Zembla*, where they were expos'd to them, be so Northwards, that it lies within 16. or 17. degrees of the Pole it self. This being a bare suspicion, it may suffice to have touch'd it. But I shall subjoyn two or three instances on the occasion of our proposition, concerning the influence of the winds upon the air, and to show more particularly, That even cold winds receive not always their Qualities, so much from the Quarter whence they blow, as from the Regions over which they blow: I shall therefore begin with what is deli-

delivered by Mr. Wood, in his New
 Englands prospect. *whereas in Eng.* Part I.
 land (says he) most of the cold winds cap. 2.
 and weathers come from the Sea; and
 those situations, are counted most unwhol-
 some, that are near the Sea-coast, in that
 Countrey it is not so, but otherwise. And
 having added, as his reason, that the
 North-east wind, coming from the Sea,
 produces warm weather, melting the snow,
 and thawing the ground; he subjoyns,
 only the North-west wind coming over the
 Land, is the cause of extreme cold wea-
 ther, being always accompanied with deep
 snows, and bitter frosts, &c. To
 which passages we shall add only
 one out of Captain James, as being
 considerable to our present purpose.
 The winds (says he) since we came hi-
 ther, have been very variable and uncon-
 stant; and till within this fortnight, the
 Southerly wind was coldest. The reason I
 conceive to be, for that it did blow from
 the main Land, which was all covered
 with snow, and for that the North winds
 came out of the great Bay, which hitherto
 was open.

*Captain
 James's
 voyage,
 pag. 52, 53.*



Title XIX.

Of the strange Effects of Cold.

1. **T**O enumerate and prosecute all the several Effects of Cold, being the chief work of the whole Book, it is not to be expected, that they should be particularly treated of in this one Section of it, wherein I shall therefore confine my self to mention only those Effects of Cold, that are not familiar, but seem to have in them something of wonderful; nor must I take notice of All them neither, least I should be guilty of useless Repetitions, but only of them, which either are not at all, or are but incidentally or transiently delivered in the foregoing Sections. Nor is it to be expected, that I should pawn my credit for the truth of every one of the Relations I am about to subjoyn. For if they had not something

thing of extraordinary, and consequently, that may beget some Diffidence in wary men, they would not be proper for the title of this Section, and most of them, that they may be fit to be plac'd here, must be the Effects of such extreme degrees of Cold, that I cannot in this temperate Climate of ours, examine the truth of them by my own Trials, so that all I can do, is, to make choice of such Relations, as are almost all of them delivered by the Relators, as upon their own Knowledge. And even this may perchance, not only gratifie and excite the Curiosity of some, who are pleas'd with no things so much, as with those, that have somewhat in them of Prodigy, and (which is more considerable) their Narratives may afford the Ingenious such strange *Phænomena*, that the Explication of them may serve, both to exercise their wits, and try their *Hypothesis*.

2. It seems not necessary, in the marshalling these observations, to be scrupulous about method, but yet to avoid confusion, we shall first mention

tion the Effects of Cold, as to those four great Bodies, of that part of the Sublunary World we live in, that are commonly reputed Elements, and thence we will proceed to take notice of the Effects of Cold upon some other inanimate Bodies, and, for an instance of its operation on living Creatures, upon men.

3. Of the power of Cold, either to straiten the sphere of activity of fire, or to hinder its wonted effects, the chief examples I have met with are recorded, partly by the *Dutch* in *Nova Zembla*, and partly by *Captain James*, when he winter'd in *Charleton Island*. These *Hollanders* in one place speak thus; *The twentieth it was fair and still weather, the wind Easterly, then we wash'd our Sheets, but it was so cold, that when we had wash'd and wrung them, they presently froze so stiff, that although we laid them by a great fire, the side that laid next the fire thaw'd, but the other side was hard frozen, &c.* Elsewhere thus, *We were in great fear, that if the extremity of the Cold grew to be more and more, we should all dye there, with cold, for what fire soever we made, it would not*

warm

Purchase
lib. 3. cap.
5. Sect. 2.
pag. 495.

warm us. And because it were tedious to transcribe all that their Journals afford us to our present purpose, we will conclude with this passage, *Hereby we were so fast shut up into the House, as if we had been prisoners, and it was so extreme Cold, that the fire almost cast no heat, for as we put our feet to the fire, we burnt our hose before we could feel the heat, so that we had work enough to do to patch our hose, and which is more, if we had not sooner smelt then felt them, we should have burnt them ere we had known it.* Though Captain James wintred in a Countrey many degrees remoter from the Pole, then *Nova Zembla*, yet in one place he gives us this account of the colds power to restrain or oppose the action of fire. *The Cooks Tubs, wherein he did water his meat, standing about a yard from the fire, and which he did all day ply with melted snow water, yet in the night season, while he slept but one watch, would they be firm frozen to the very Bottom.* And therefore was he fain to water his meat in a Brass Kettle, close adjoyning to the fire; and I have many times both seen and felt, by putting my hand into it;

Captain
James 65.
that

that side which was next the fire was very warm, and the other an inch frozen. I leave the rest to our Cook, who will almost speak miracles of the Cold.

Purchase
lib 1. cap.
4. pag. 74.

3. Thus far our English Navigator, whose relation compar'd with those of the *Hollanders*, make me not so much wonder, as I once did, that men should relate to *Marcus Polus*, that there is a certain Plain in *Tartary*, situated between some of the highest mountains in the World, where if fire be kindled, it is not so bright, nor so effectual to boil any thing, as in other places. For so *Purchase* renders that passage; whence occasion has been taken to impute to *Marcus Polus*, a writer not always half so fabulous, as many think him, that he affirm'd, that there was a Countrey in *Tartary*, where fire could not be kindled.

4. And as for the other newly mention'd relations of Seamen and Travellers, though to us, that live in *England*, they cannot but seem very strange; yet I am kept from rejecting them as utterly incredible, by considering, that ice and snow having before their Congelation been water, must

must in probability owe their Coldness, to that which reign'd in the Air: So that if in any place Nature has, either so plentifully stock'd the Air it self with frigorisick exspirations, or other Corpuscles (if we will admit any such) or have upon any other account rendred it as cold as it can make ice and snow to be even here amongst us, I know not why the Northerness of the climate, and perhaps some saline exspirations from the Earth and Sea, may not there diffuse through the air a cold superior to that, which by small Quantities of ice (or snow) and salt, can at a small distance be produc'd here. And this cold is so intense, that by pouring some water on a Joynt-stool, and placing on it a silver Tankard, or other convenient vessel, we may, as experience has assur'd me, with beaten ice (or snow) and salt, and a little water (which is added to hasten the solution of the other) nimbly stirr'd together in the pot, make the mixture freez the external water quite through the Tankard; and they may be by this way so hard frozen together,

ther, as that by lifting up the pot, you may lift up the Joynt-stool too, and that (which is the circumstance, for which I mention this) just by the fire, which in this case is unable to hinder so difficult an operation of the Cold.

5. Thus much of the effects of cold, in reference to fire. What the same quality may perform upon Air, we shall say but little of in this place, because we treat of those *Phænomena*, partly in the foregoing Section of the coldness of the Air, and partly in other places. Only we shall not here pretermitt a testimony of the learned *Olearius*, who, as an eye-witness, confirms what we elsewhere deliver of the high degree of cold, to which the Air may be brought. For he tells us, *That in Muscovy he experimentally found, that which others left recorded in their writings, That ones spittle would be congeal'd before it reach'd the ground, and that water would freeze as it was dropping down.*

Olear.lib.
3.p.m.117

6. Of the effects of cold upon water, we shall not need to say much in this place, since the two notablest of

of them being, the power cold has to congeal water suddenly, and the force it has to turn vast quantities of it into solid ice. Of the former I have newly given, out of *Olearius*, an example as eminent as almost any that is to be met with, and of the latter also, I have given several instances in the Section, that treats of ice : Yet two or three notable instances, which we do not elsewhere mention, 'twill not be improper to deliver in this place.

7. The first declares, that notwithstanding the warmth of the inside of a mans mouth, his spittle may be frozen even there. *The 27. of September* (they are the words of *Gerat de Veer*) *it blew hard Northeast, and it froze so hard, that as we put a nail into our mouthes (as when men work Carpenters work they use to do) there would Ice hang thereon, when we took it out again, and make the blood follow.* The like relation (if I misremember not) I have met with in a modern English Navigator, and it is very little, if at all more strange, then what is affirm'd by *Queen Elizabeths Ambassador* to the

Purchase
pag. 491.

the *Russian* Emperor: In the extremity of winter (says Doctor Fletcher, speaking of *Muscovia*) if you hold a pewter Dish, or Pot in your hand, or any other mettall, except in some chamber, where their warm Stoves be, your fingers will stick fast to it, and draw off the skin at the parting.

8. The other instance I intended to mention, is this, that though *Macrobius*, and other learned men, both ancient and modern, will not allow salt water to be congealable; yet the *Dutch* at *Nova Zembla* relate, that even in the midst of *September* (and as the Marginal note says, in a night)

Purchase
pag. 491.

It froze two inches thick in the salt water.
9. As to the effects of violent colds upon the Earth, what they would prove upon pure and Elementary Earth (if any such there be) I can but conjecture; but as for that impure or mingled Earth, which we commonly tread on, the effects of extreme cold upon that, may be very notable. For *Olearius* relates, that in the year 1634. the cold was so bitter at *Musco*, that in the great market-place, he saw the ground open'd by it so, that
there

there was made a cleft of many yards long, and a foot broad. [And the present great Duke of *Muscovies* Physician being asked by me concerning the truth of such relations, answered me, that he himself had in those parts seen the ground reduc'd by the cold, to gape so wide, that a child's head might well have been put into the cleft.]

10. 'Tis somewhat strange, that the violent heat of Summer, and the extreme cold of Winter should both of them be able to produce in the ground the like effects; but whether to make these gaping chinks, that we have been speaking of, the surface of the ground expos'd to the air, being first frozen, is afterwards broken by the expansive force of the moist earth underneath, to which the cold at length pierces, and congealing it, makes it swell, and heave, and so burst or cleave the hard and frozen crust of the ground, which cannot sufficiently yield to it, whether this (I say) may produce the clefts we were speaking of, or whether they must be deriv'd from some other cause, not having yet made the expe-

Purchas.
pag. 491.

riments, I thought upon, to clear the matter one way or other, I do not as yet pretend to determine, but will rather subjoyn the second observation I purpos'd to mention of a strange operation of Cold upon the ground, and it is afforded us by the *Dutch* in their often quoted third voyage to *Nova Zembla*: In one place of which they tell us, *That when they had built them a wooden house, and were going to shut themselves up in it, for the winter, they made a great fire, without the house, therewith to thaw the ground, that they might so lay it, viz. the wood about the house, that it might be the closer, but it was all lost labour, for the earth was so hard, and frozen so deep into the ground, that they could not thaw it, and it would have cost them too much wood, and therefore they were forced to leave off that labour.*

II. After what we have said about the strange effects of cold, in reference to fire, air, water, and earth, we will now proceed to take notice of its effects upon confessedly compounded Bodies, whether inanimate or living; but of the former
sort

sort of mix'd Bodies (I mean those that have not Life) it will not be necessary to say much in this Section, in regard that we have in many other places, upon several occasions had opportunities to mention already most of the particulars that belong to that head. For we elsewhere take notice, that violent Colds will freeze Beer, Ale, Vinegre, Oyl, common Wine, and even Sack and Alegant themselves. We have likewise noted, that the Cold may have a notable operation, upon Wood, Bricks, Stone, vessels of Glafs, Earth, and even Pewter, and Iron themselves, to which *Bartholinus* out of *Fanus Muncks Voyage to Greenland*, allows us to add vessels of Brals (though these are not immediately broken by the Cold, but by the included Liquors which it dilates) and divers strange effects of Cold upon inanimate Bodies, which 'twere here troublesome to recapitulate, may be met with dispers'd in several places of the present History. Wherefore having only intimated in general, that, though many plants are pre-

serv'd by a moderate cold, yet it has been observ'd, that most Garden-plants are destroy'd by excessive degrees of it, we will pass on to consider the effects of Cold upon animals, and of the many observations, that we have met with among Travellers, concerning this subject, we shall, to avoid prolixity, deliver only the considerablest, and those that we find attested by very credible Writers.

12. Captain James speaking of the last of the three differences he makes of Cold (namely, that which he and his company felt in the woods) gives this account of it; *As for the last, it would be so extreme, that it was not endurable; no clothes were proof against it, no motion could resist it. It would moreover so freeze the hair of our Eye-lids, that we could not see; and I verily believe, that it would have stifled a man in a very few hours.*

Captain
James's
voyage, 64.

13. Olearius giving an account of the Air of Muscovy, and especially the Capital City of it, *The Cold (says he) is there so violent, that no Furs can hinder it, but sometimes mens Noses, and Ears, Feet and Hands will be*

Livre 3.
p.m. 117.

be frozen, and all fall off. He adds, that in the year 1634. when he was there, they could not go 50. paces without being benumm'd with cold, and in danger of losing some of their Limbs. And yet to add, that remarkable observation upon the by, the same Author, near the same place, speaking of Musco, and the neighbouring Provinces distinguished from the rest of that vast Empire, says, *That the Air* Livre 3.
116. is good and healthy, so that there one scarce ever hears of the Plague, or any other Epidemical diseases. And he adds, that for that reason, when in the year 1654. the Plague made havock in that great City, the thing was very surprizing, nothing like it having been seen there in the memory of men.

14. Our already divers times mention'd English Ambassador Dr. Fletcher, speaking of the cold that sometimes happens in Russia, witnesseth thus much of it. *Divers* (says he) not only that travel abroad, but in the very markets and streets of their Towns are mortally pinch'd, and kill'd with all; so that you shall see many drop down in the streets, many Travellers brought into the

Towns sitting dead, and stiff in their sleds. Divers lose their Noses, the Tips of their Ears, and the Balls of their Cheeks, their Toes, Feet, &c. Many times when the winter is very hard and extreme, the Bears and Wolves issue by troops out of the woods, driven by hunger, and enter the Villages, tearing and ravening all they can find, so that the inhabitants are fain to flee for the safeguard of their Lives.

15. To descend now to observations, that do somewhat more punctually set forth the more particular Phenomena of Cold, in reference to mens Bodies, take the following Observation. The 15. of March some of their men, that had been abroad to kill Deer, returned so disabled with cold, which did rise up in blisters under the soles of their feet, and upon their legs, to the bigness of Walnuts, that they could not recover their former estate (which was not very well) in a fortnight after. This may be confirmed by that passage of the *Hollanders*, where speaking of their preparing springes to take Foxes, they add, that they did it with no small trouble; for that if they stay'd long without doors, there

there arose Blisters upon their Faces and Ears. We did dayly find by experience (says Captain James) that the cold in *Pag. 64.* the woods would freez our faces, or any part of our flesh, that was bare; but it was not so mortifying, &c.

16. The Dutch speaking of the pains they were fain to take to dig away the snow, that cover'd the house, and choaked up their door, adds, that in that laborious work, *Pag. 497.* they were forc'd to use great speed, for they could not long endure without the house, because of the extreme cold, although they wore Foxes skins about their heads, and double apparel upon their backs.

17. The lately mention'd Captain James relates, that in Charleton Island he was fain to cut the hair of his head short, and shave away all the hair of his face, because the Isicles, that would be fastned to it, made it, as he *Pag. 56.* speaks, become intolerable.

18. And he elsewhere relates, that once he and his Companions, having been for a little while parted into two companies, had their faces, hair and clothes so frozen over, that they could not

Pag. 52:

know each other by their habits, nor (which is a considerable circumstance, for whose sake chiefly I mention this passage) by their voices.

19. And the same Author gives this account of the death of the Gunner of his Ship, whom he calls a strong hearted Man, and who died before the end of November. He had (says our Author) a close boarded Cabin in the Gun-room, which was very close indeed, and as many clothes on him as was convenient, (for we wanted no clothes) and a pan with coals of fire continually in his Cabin, for all which warmth his plaister would freeze at his wound, and his bottle of Sack at his head.

Purchas.

lib 3. cap.

3. pag. 496

20. The 11. of December (says Gerard de Veer) it was fair weather, and a clear Air, but very cold, which he that felt not would not believe, for our shooes froze as hard as horns upon our feet, and within they were white, so that we could not wear our shooes, but were forc'd to make great pattents, the upper part being sheep skins, which we put on over three or four pair of socks, and so went in them to keep our feet warm, yea, and the clothes upon our backs were white over with frost.

21. Which

21. Which may be somewhat confirmed by this passage of Captain James. *The clothes on our Beds would be covered with hoar frost, which in this little habitacle was not far from the fire.* Pag. 65. We might adde to all these, this other passage of the often mentioned Gerard de Veer. *The 26. of December, it was foul weather, the wind North-west, and it was so cold, that we could not warm us, although we used all the means we could with great fires, good store of clothes, and with hot stones and Billets laid upon our feet, and upon our Bodies, as we lay in our Cabbins, but notwithstanding all this, in the morning our Cabbins were frozen, &c.* Purchas. pag. 497. But we shall not insist on such passages, as this last recited, because that of the force of cold to repress and withstand the fire, we have already deliver'd as remarkable things, as will be easily met with, in approved Writers, in the former part of this present Section.

22. I have my self met with a knowing and very credible person, that related to me of the cold of Russia, where he travelled, little less strange things, then those I have mentioned

tioned of it out of Books; and if I did not want the Historians name, I should make small difficulty to add, That since I made a good progress in this present Section, a very learned Traveller (though not into cold Countries) related to me, upon the occasion of what I was treating, what he affirm'd to have met with in an approv'd History of the strange operation of the inclemency of the Air upon multitudes of men at once, namely, that about the year (if he rightly remember it) 1498. an Army of the Turks making an incursion into *Poland*, upon their return was surprized with such an extremity of Cold and of Snow, that though it were but (if he mistake not) in *November*, forty thousand of them (the whole Army consisting of seventy thousand) perish'd through the extremity upon the place.

23. Amongst the many Relations I have met with of the fatal Effects of Cold in the Northern Countries, I took notice not without a little wonder, as well as trouble, that I could not find, that any of the Relators

tors had the curiosity to see what change was made in the internal parts of the Bodies so destroy'd, which yet were an inquiry very proper to have been made, but at length the other day an ingenious Person having shew'd me a Book newly publish'd in French, containing the Description of a *Polonian* Province he calls *Ukraine*, as I was skimming it over, with hope to find some observations about Cold, I lighted on a relation, which though not such as I desir'd, is more then I have any where else found, and I take the more notice of it, because, that though the very name of this Province is scarce hitherto known to us in *England*, yet having a while after by good chance met with an intelligent *Polonian* Lord, and having inquired of him, whether he had ever been in that Country, he both told me, that he had been quarter'd there, and by his Answers and Relations did countenance divers particularities of it, mention'd by this French officer (named *Monsieur de Beauplan*) who liv'd long there. This Author then
after

after having taken notice, that this fertile Province, though but situated in the same height of the Pole with *Normandy*, is oftentimes subject to excessive colds (which circumstance I mention as a further confirmation of something of the same nature delivered in the former Section) gives an Account of two differing Effects of this Cold upon the Bodies of men : The one being a peculiar kind of sickness, the other Death.

24. The first which I remember not to have elsewhere met with, is, that sometimes when the natural heat proves strong enough to protect the Toes, and Cheeks, and Ears, and other parts, that are either more remote from the heart, or more tender from a sudden mortification ; yet unless nature be assisted, either by good Precautions, or Remedies, she cannot hinder the cold from producing in these parts Cancers, as painful as those which are caus'd by a scalding and malignant humour, and which let me see (says my Author) when I was in those Countreys, that cold was not less cutting nor powerful

ful to destroy things, then the fire to consume them: He adds, that the beginning of these Cancerous sores is so small, that what produces the pain scarce equals the bigness of a Pea, and yet in few days, nay sometimes in few hours it spreads so, as to destroy the whole part it invades, which he confirms by the Example of two persons of his acquaintance, who in a trice lost by Congelation the badges of their Sex.

25. As to those that are kill'd with Cold, our Author informs us, that they perish'd by two differing kinds of death. For some being not sufficiently fortifi'd against the cold by their own internal heat, nor competently arm'd against it by Furs, Inunctions, and other external means, after having had their hands and feet first seized by the cold, till they grow past feeling it, there the rest of their Bodies are so invaded, that they are taken with a (kind of Lethargick) Drowziness, that gives them extreme Propensity to sleep, which if indulg'd to, they can no more awake out of, but dye insensibly. And
from

from this kind of Death our Author adds, that he was several times snatch'd by his servants, who were more accustom'd to the cold, and seasonably forc'd him to awake out of those drowzinesses, which they knew to be most dangerous. And that sometimes the death by cold is indolent enough, the Relations of some intelligent acquaintances of mine, who have been in exceeding cold Countries, do confirm.

26. But the other way whereby cold destroys men, is that, which is the most remarkable in our Author, and though less sudden is more cruel. For he tells us, that sometimes the cold seizes mens Bodies in the reins, and all about the Waist (and especially horse-men underneath the Armor of the Back and Breast) and straitens, as he speaks, those parts so forcibly, that it freezes all the parts of the Belly, especially the Guts, so that though they have keen appetites, they cannot digest, or so much as retain the lightest and easiest Aliments, without excepting Broths themselves, but presently reject them by vomit, with

with unspeakable gripings and pains, and so continually complaining of their condition, and sometimes crying out, as if some body were tearing out their bowels, they end their miserable lives, being often brought by the violence of their torments to the brink of madness and despair, before they come to that of the grave. And our Author having seen some of these departed wretches open'd, says, that they found the greatest part of their guts black, burn'd up, and as it were glew'd together, whence he thinks it probable, that, as their bowels came to be spoil'd and gangrenated, they were forc'd to those complaints and exclamations; and we may add, That probably upon the same cause depended those continual vomits of what they eat or drunk; the Gangrene of the guts hindering the descent of Excrements downwards, as it often falls out, in the true *Iliaca Passio*, and the Peristaltick, or the usual motion of the parts being inverted, as it also frequently happens in the same disease. There is no doubt but Anatomists and Physicians will think

think this account very imperfect, but yet I think my self beholden to the Author for it, because 'tis not the best, but the only, that I have hitherto yet met with of this matter, though I could wish it had been much more full and particular, and that he had also open'd those Animals, and especially their brains, that he mentions to have been kill'd suddenly, and without pain, by cold. For such informations (whose want, as far as our Climate will permit, I have had thoughts of supplying by Experiments upon other animals) would perhaps satisfy me one way or other about a conjecture I have had, and been able to countenance by several trials upon Vegetables and dead Animals, about the cause of mortifications produc'd by excessive cold.

27. What effects a violent Cold may have upon the bodies of other animals then men, I scarce find at all taken notice of by the Writers I have met with, and what I remember upon that subject amounts to but few particulars: The French Author lately quoted, takes notice in general, that

that the cold in *Ukrain*, as the *Polanders* call it, is sometimes so great, as to be scarce supportable by horses, and some other tame beasts.

28. This same Author also mentions a certain fourfooted Animal called *Bohack*, which is said to be peculiar to those parts, and hides himself under ground in the Winter; and having inquir'd of the lately mention'd *Polish* Nobleman concerning this beast, he told me, that being in that Province he had one presented him as a rarity, upon an occasion proper enough to be mention'd here: For some of the *Poles* chancing to dig (for some purpose that I remember not) in a certain retir'd place, were surpriz'd to find under ground, an Animal not familiar to them, and though this creature was so frozen and stiff, that they thought it to be stark dead, yet when they came to flea it for its skin, being awaken'd by pain, it recovered life again, as was brought as a rarity to the Commander, from whom I have the relation.

29. That some other animals may

N n

be

That upon the by, I took notice, that provident Nature to arm them against the cold, had afforded him a Coat, that might have pass'd for a Fur.

30. Yet these two things seem remarkable in favour of the efficacy of cold, the one, that in several cold Countries, as particularly *Greenland*, and *Livonia*, even Modern describers of them affirm, that Hares will grow

Lepores coloris & pellis mutatione anni tempestates sequuntur, ac hiberno tempore albis pilis vestiti, æstivis mensibus eosdem cinereos habent. Livonia nova discriptio, Pag. 303.

white in Winter, and return to their native colour in Summer.

And the other,

that though *Charleton Island* differ not one degree in Latitude from *London*; yet (as the cold is there prodigious, so) I remember, that Captain *James* some where takes notice of his having seen there, both divers Foxes, that were pied black and white, and white Partridges, though he could not catch them. But of the whiteness of Animals I elsewhere treat among other subjects, that belong to the History of Colours. And having already been more prolix than I intended

Pag. 46. &
Pag. 89.

ed

ed in setting down the observations of others, I think it now time for me to resume the mention of my own Experiments, divers of which, though made before others, that have been already mention'd, X or XII. Sections of, I thought fit for to reserve for this place, both for other reasons, and because, this place seems proper for Experiments, that have a nearer tendency to the hinting or the examining the more general *Hypothesis* about Cold.

N n 3

Title



Title XX.

*Experiments touching the weight
of Bodies frozen and unfrozen.*

1. **S**INCE divers of those ingenious men, that have of late revived, and embraced the Doctrine of the old *Atomists*, teach us, that water is turned into ice by the introduction of frigorisick Corpuscles, which *Democritus* of old is said to have believed to be cubical (and to which other Philosophers of late have assigned other shapes indeed, but yet determinate ones) we thought fit not so much for our own satisfaction, as for that of others, to try, whether or no a Liquor by its increale of weight, when frozen, would betray any substantial accession of the Corpuscles of Cold, which according to the *Epicureans*

rean Principles, may, by reason of their smallness, pass in freely, and in vast multitudes, at the pores of other Bodies, and even of glals, and which by reason of the same smallness, must be supposed exceedingly numerous to be able to arrest the motions of such multitudes of minute Corpuscles, as must go to the making up of any considerable quantity of water.

2. And first we made a trial with Eggs, of which our Notes give us the following account,

3. [We took a good pair of Scales and placing them upon a frame (purposely made for such Experiments, as required, that the things to be weighed should remain long in the balance) we put into one of these a couple of Eggs, and having counterpoised them with brass weights, we suffered them to continue all night in a Turret (built as it had been made for an observatory) that the breaking of the Eggs, or any such other accidents might not hinder the success of our endeavours (which were to try, whether the Corpuscles of Cold,

which divers Philosophers suppose to be the Efficients of Congelation, would make them any whit heavier,) but we were somewhat surprized, when the next morning, after a very sharp night, going up to the Turret, we found (the scales and frame being in good plight) the Eggs to be grown lighter by very near four grains.]

Thus far the Note.

4. But though we afterwards repeated the Experiment once or twice (if not oftner) yet having been by intervening avocations diverted from registering the circumstances of the events; I dare not now trust my memory for any more, then that some of the circumstances seemed odd enough, but uncertain, and that I desisted from prosecuting the Experiment, chiefly for this reason, that an increase of weight in exposed Eggs was scarcely to be hoped for, because it seemed probable, that part of the more subtile and spirituous Corpuscles contained in the Egg do continually, by little and little, get away through the pores of the skin and shell; that, seeming to be the reason

son why Eggs long kept have usually within the shell, a manifest, and sometimes very considerable cavity unfilled with either yelk or white, which Cavity seems to have been left by the recess of the subtile parts we have been mentioning, so that although the frigorifick Atoms should by their ingress add some, not altogether insensible weight to the Egg, yet that would not, unless perhaps in the very nick of time, when the Congelation is first actually made, be taken notice of, by reason of the greater decrement of weight, that proceeds from the Avolation of the more subtile parts of the Egg it self.

5. And to satisfie our selves about this matter, we took four hen Eggs, and counterpoised them carefully in a good pair of Scales, which were suspended at a frame, that the balance might be kept unstirr'd in a quiet room, wherein we had placed it, and suffering it to continue there for a pretty while, we observ'd, that though it were Winter, and though the room wherein it stood were destitute of a Chimney, yet that Scale
wherein

wherein the Eggs lay, did almost dayly grow manifestly lighter, so that it was requisite, from time to time, to take a grain out of the opposite scale, to reduce the ballance to an *Equilibrium*. And by this means we found the Eggs after some time to have lost eight grains of their former weight, but how much more they would have lost, if we had continued the Experiment, the need we had of the Scales kept us from discovering.

6. Upon this occasion I will add, that I us'd some endeavours to satisfy my self about this inquiry, *viz.* whether Eggs being once actually frozen (for those mention'd in the former Note, might lose their weight before they were so) and kept in a pair of good Scales fasten'd to a frame in some quiet place, well fenc'd from the Sun, would by the cold of the Air in freezing weather, be kept for any considerable time, without a sensible diminution of weight, but an unexpected thaw hindered us from seeing the success of what we design'd of this nature, both as to Eggs, and also

also some other Bodies: For if the Experiment were very carefully tri'd upon a competent variety of them, it might possibly assist us to guess, especially in Camphire, and some other easily exhalible bodies, what interest Cold may have in suppressing or diminishing the expiration of their *Effluvia*.

7. But to return to the weight of Bodies frozen and unfrozen, we attempted to discover somewhat about it by several ways, according as the differing accommodations, we were furnish'd with, permitted. And of these trials I will mention four or five, as well of the less, as of the more accurate, as my memory or Notes supply me with them.

8. One of the less Accurate ways we imployed to try, whether ice, in which according to the Atomists, great store of these frigorifick Corpuscles must be wedged, would not upon their expulsion or recess, leave the water lighter then was the ice, was that which follows, wherein to hasten the Experiment, we mingled a little salt. And though we fore-
saw

law, there would be a difficulty from the Adhæſion of the vapors of the external Air, to the outside of the glaſs we were to employ, we thought, that inconvenience might be remedied by well wiping off the froſt, or dew from the outside of the glaſs, till it were clean and dry: The event of the trial we find ſuccinctly ſet down among our Notes as follows. [A ſingle vial ſealed up with ice and ſalt, being wiped dry, and weighed, was found to weigh four ounces four drachms and a half, when it was quite thawed, it was found to weigh ſomewhat more then a grain leſs then its former counterpoize.]

But more accurate and ſatisfactory Trials about this matter, I find thus ſet down in one of my papers.

9. [We took a vial more thin then thoſe that are commonly uſ'd, that, of the Aggregate of that and the Li-
quor, the glaſs might make ſo much the leſſer part: This vial was furniſhed with a ſomewhat long neck, which at the flame of a Lamp was drawn by degrees ſlenderer and ſlenderer, that being very narrow at the
Top,

Top, it might the more readily and conveniently be seal'd, notwithstanding the waters being in it; then we *almost* fill'd it with that Liquor, I say almost, because a competent space ought to be left unfill'd, to allow the water, swell'd by glaciation, room to expand itself: This vial with the liquor in it, was plac'd in a mixture of snow and salt after our usual manner, and when the glass appear'd almost full of ice, it was taken out, and nimbly clos'd with *Hermes's* seal, presently after this was weigh'd in a pair of very good Scales, and the vial together with the contain'd liquor, amounted to $\text{zv. } 38. \text{ gr. } \beta$, which yet was not all ice, because these things could not be done so nimbly, but that some of the ice began to thaw, before we were able to dispatch them quite, the vial thus seal'd being remov'd, and suffered for two or three hours to thaw, when the ice was vanish'd, we weigh'd again the seal'd glass in the same Scales, and found, that it weigh'd, as before, at least, if there were any difference, it seem'd to weigh a little more.] But
 this

this Increment that amounted not quite to $\frac{1}{2}$ a grain, might easily be attributed to some difference in the weights and grains themselves, wherein 'tis not easie to find a perfect exactness, or to some little unheeded moisture, that might adhere to some part of the vial.

10. And because it may be wished, that as this Experiment shews the weight of Ice resolv'd into water, to be the same with that of the solid ice, so we had tri'd, whether the weight of water congeal'd into ice, would be the same with that of the former fluid water, we will subjoyn what immediately follows in the same paper in these words.

11. [We took a seal'd vial, very thin, that it might be lighter, but not so large as the other, by about a third, as amounting in the lately mention'd Scales but to $\text{ziiiij. } \text{3ij. gr. } 41.$ when we had seal'd it up with the water in it. This vial we plac'd as we had done the other, in a mixture of snow and salt, freezing it warily, lest being seal'd, it should break, then we remov'd it into the same Scales,

Scales, to try, whether it had got any weight by the suppos'd subingression of the Atoms of Cold, which many learned men take to be the efficient of Congelation; but it either weighed just as before, or if there were any difference, it seem'd to have lost $\frac{1}{4}$ of a grain. Being suffer'd to thaw, and put into the same Scales again, it weigh'd just as much as it did, when frozen, though the weights were numerically the same, and about $\frac{1}{4}$ would sway the Scales, or at least be sensible upon them. But note, that I was careful this last time to wipe the outside of the glass with a linen cloth, because I have observ'd, according to what I elsewhere deliver, that, in case ice be any thing hastily thaw'd, it may produce a dew on the outside of the glass, as I suspected, that even the warm Air might in some measure do in this, and if it had not been for this suspicion, some adhering dew, that I was thereby enabled to detect and wipe off, before I put the vial into the Scales, might easily have impos'd upon us.

12. These Trials I presume may give

give some satisfaction about the inquiry, for the resolving whereof, I thought fit to make them.

13. But I was also desirous to see, whether any difference, as to weight, would be produc'd by freezing and thawing (if I may use those expressions in this case) Iron, Stone, Wood, or the like solid and permanent Bodies, which I intended to have exactly weigh'd, before and after their being expos'd to the Air, and also after the frost was gone, (and all this against Counterpoizes not expos'd to so great a Cold) would discover any sensible alteration, as to weight, that might safely be ascrib'd to the Cold. And though Avocations, and the negligence of one that we imploy'd, kept us from bringing the matter to such an issue as was desired, yet the Trials seem'd not altogether irrational, since we have formerly made it probable (and have since met with fresh instances to confirm it) that even Stones, and Metals, may resent some change of Texture by the operation of some degrees of Cold. And indeed induc'd by such considerations
of

of that kind, as seem'd the least doubtful, I remember I sometime made several experiments of the weight of some metals, and stones, both before and after they had been much expos'd to a more vehement Cold, then would have suffic'd to turn water into ice, and also after they had been, if I may so speak, thaw'd in a warm Air. But the paper in which we registred the events of these trials having been mislay'd, I dare not charge my memory with the particulars. Only, if I mistake not, one or two of the stones seem'd to have increased in weight, after having been buried in our frigorifick mixture, which I was apt to impute to some particles of the ice resolv'd into water by the salt, that was mingled with it, and (being perhaps made more piercing by the saline particles associated with them) imbib'd into the pores of the stone. For I remember, that having procur'd an Experiment, that I then wanted conveniency to try my self, to be made by an ingenious person, upon a stone hard enough to bear a good polish,

I was by him inform'd, that the stone by having been kept a while in water, did, though it were afterwards wip't dry, discover a manifest increas of weight: and in confirmation of my conjecture, I shall add, that from a sort of stones, that are of a texture close enough to be usually polish't; I did, as I expected, obtain by distillation (and that without a naked fire) a considerable quantity of an almost insipid liquor, which I suspected to be in good part but water soaked in to the stone, for reasons, that 'tis not worth while here to discourse of; the cause of my mentioning these particulars being, that (I hope) they may make thole, that shall hereafter try such Experiments, cautious how they draw inferences from them, and may invite them to expose the bodies, they would make trial of, rather to the cold of the free Air in very sharp weather, (for want of which, we our selves could not do what we advise) then to artificial glaciations at least, unless they be so ordered, that nothing that's moist come to touch the bodies to be wrought upon.

14. But such Trials as these newly mention'd, and others of the like kind, we must leave to be prosecuted by those, that are furnish'd with accurate Scales, and leisure; for want of the latter of which, and sometimes too of the former, we were fain to give over the pursuit of them, which troubled us the less, because those made with the seal'd Vials were diligently made; and as for divers others, we made them, as we were saying, more to be able to gratifie others, then to satisfy our selves, because though in case there should unquestionably appear some sensible increase or decrement of weight, upon that which the Atomists would call *the Accession or Expiration* of frigorifick Corpuscles; it would afford a plausible Argument in favour of the *Epicurean Doctrine*, about the generation of ice; yet if no such change of weight should be found upon the freezing or the thawing of water, or any other Body, I doubt whether it may, on the contrary, be safely concluded, that the Atomists *Theory of Cold* is false. For

possibly they may pretend, that the Atoms of Cold may not have either gravity or levity, any more then the streams of Electrical Bodies, or the *Effluvia* of the Loadstone. Nay, though we should admit the frigorifick Corpuscles not to be altogether devoid of gravity, it may yet be said, that when they invade the Body, they freez, they expel thence some other preexistent Atomes, that may also have some little weight, and that the frigorifick Corpuscles, that flie, or are driven away, may be succeeded by some such, when bodies come to be thaw'd. But of this no more at present.

Appendix to the XX. Title.

THe Experiments recorded in the foregoing Section, may perchance in this regard prove more useful then I was aware of, that they may keep men from being misled by the contrary accounts, that I find to have

have been given of the weight of ice, and water, by no obscure writers. For (to spare one of the famousst of the Ancients) *Helmont* in the Treatise he calls *Gas Aquæ*, where he gives an account of the congelation of water, which I confess to be unintelligible enough to me, and where he is pleased to ascribe to I know not what extenuation of part of the sulphur he supposes to be in water, that levity of ice, which the bubbles, it contains, afford us an intelligible and ready account of, delivers very positively this Experiment. *Imple* (says he) *lagenam Vitream & magnam frustis Glaciei, collum verò claudatur sigillo Hermetis, id est, per vitri ibidem liquationem: ponatur hæc tum lagena in bilance adjecto pondere in oppositum, & videbis quod prope modum octava sui parte aqua post resolutam glaciem erit ponderosior seipsa glacie. Quod cum millesies ex eadem aqua fieri possit, &c.* Thus far *Helmont*, who in case he take *lagena vitrea* in the ordinary acception of the word, would have made us some amends for this erroneous account, if he had taught us the way how he could seal such a

Num. 35.

broad vessel, as a glass flagon, *Hermetically*. But what has been deliver'd in the foregoing Section, will sufficiently shew, what is to be thought of this Experiment of *Helmonts*. And for further confirmation, we have several times weigh'd ice frozen, and reduc'd to water, without finding any cause to doubt, but that *Helmont* was mistaken. And particularly upon the last Trial I made of this kind, having fill'd a wide mouth'd glass with solid fragments of ice, together with it amounting to a pound (of which the glass alone weigh'd somewhat above five ounces) I whelm'd over the mouth of it another flat bottom glass, that if any vapours should ascend, they might be condens'd into drops, as in the like case I had formerly observ'd them to do. And this ice being thaw'd in a warm room, as no drops were seen to stick to the inside of the inverted glass, so the other glass being again put into the same Scales, appear'd almost exactly of the same weight as formerly, whereas the ice alone, that had been resolv'd, amounting

mounting to much above eight ounces, according to *Helmonts* proportion, the weights should have been augmented by a whole ounce at least: And I make little doubt, but that if the Experiment had been tri'd in greater quantities of ice, the event would have been very little, if at all, different. But I purposely chose in the *Statical* Experiments about cold, to make my Trials in no greater quantities of matter than I have done, because 'tis very difficult to get scales strong enough to weigh, without being injur'd, much greater weights, and yet be accurate enough to discover truly such small differences, as are fit to be taken notice of in such Experiments. But to return to *Helmont*, notwithstanding all that we have said against what he delivers about the weight of ice, yet because I take this inquisitive *Chymist* to have been, in spite of all his extravagancies, a Benefactor to experimental learning, I am willing to suggest on his behalf, that possibly much of the additional weight he ascribes to the resolv'd ice, may have proceed-

ed from that which would not have been taken notice of by an ordinary Experimenter, For (as I not long since intimated) I have (sometimes purposely, and sometimes by chance) by thawing ice in clos'd vessels somewhat hastily, produc'd a copious dew on the outside of the vessels, which dew, as being made by the condens'd vapours of the ambient Air, ought to be wip'd off, before the vessel be put into the scales to weigh the melted ice: And 'tis possible also, that *Helmont* may have err'd in the manner of weighing his *Lagena*, whatever he mean by it, it being usual even for learned men, that are not vers'd in *Statick's*, to mistake in Experiments, which require, that things be skilfully and nicely weigh'd: How far this

— *Hinc gelidam congelatamque aquam graviores esse non congelata expertus est Jo. Manelphus, Com. in 4. Meteor. Aristot. Inquit Tho. Bartholinus de Nivis usu cap. 12.*

excuse may be appli'd to a late Commentator upon *Aristotles* *Meteors*, who says, he tri'd, that water frozen is heavier then unfrozen, being a stranger to that Authors writings, I shall not consider: only whereas *Helmont* and

○ ○

and He seem to agree very little in their Affirmations, it will be perhaps more difficult to accord them, then to determine, by the help of our formerly register'd Experiments, what may be thought of both their Relations.

Yet I shall add on this occasion, That if I had not devis'd the above mention'd way of freezing water by Art in *Hermetically* seal'd glasses, I should have found it difficult to reduce, what is affirm'd by *Manelphus*, which I then dreamt not of, to an accurate Experiment; for though I had imploy'd a seal'd glass, (which I have not heard, that he or any other has yet made use of to that purpose) yet if I had in that vessel expos'd the water to be frozen the common way, 'tis odds (though it be not absolutely certain) that the water beginning, as 'tis wont to congeal at the Top, the Expansion of the subsequently freezing water would break the glass, and so spoil the Experiment: And for the same reason I have sometimes in vain attempted, to examine the weight of water frozen, by nature, accord-

ing

ing to her wonted method in open vials. And if instead of glasses, you make use of strong earthen vessels, there is danger, that something may be imbib'd, or adhere to the porous vessel, and increase the weight, and by some such way, or by some mistake in weighing, 'tis very probable *Manelphus* may have been deceiv'd, which I am the more inclin'd to think, if we suppose him a sincere writer, not only because of some things I have taken notice of about congelations made in earthen vessels, but because, when I have instead of an earthen, made use of a metalline pottinger (both which sorts of vessels have in common this inconvenience, that their ponderousness makes them less fit for accurate Scales) there appear'd cause to suspect, either that our Author did not use metalline vessels, or, which I rather suspect, that he wanted skill or diligence in weighing. For as I find no intimation of his having imploy'd any peculiar or artificial sort of vessels, so, if he us'd such as we have newly been speaking of, and had weigh'd them carefully, I cannot

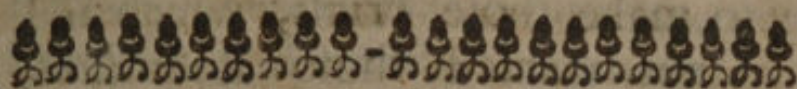
cannot but think, that instead of finding the ice heavier then the water 'twas made of, he would have rather found it lighter. For I remember, that having once expos'd all night a pottinger almost full of common water, to an exceeding sharp Air, and having caus'd it the next morning to be brought me, when the liquor was throughly frozen, I found it to have lost about 50. grains (if I misremember not) of its former weight, and though this event were consonant enough to my conjectures, yet for greater certainty I repeated the Experiments another frosty night with this new caution; that the pottinger and water, together with the counterpoise, were kept suspended in the Scales, to be sure that no effusion of any part of the water in carrying it abroad to the open Air, should be made without being taken notice of; but the next morning (somewhat late) the vessel with the contain'd water now congeal'd, appear'd to have lost about 60. grains: and with the like success the Trial was reiterated once more, and that in weather

so sharp, that I am not apt to think, the water expos'd by *Manelphus*, began to freez sooner then ours. But the event was not unexpected, for besides that I consider'd, that in these kind of Experiments, part of the water, notwithstanding the exceeding coldness of the Air, must in all likelihood fly away before the surface of it began to be congeal'd, I judge it not improbable, that not only the fluid part, but even that, which was already congeal'd, might continually lose some of its Corpuscles, and by their recess lose also somewhat of its weight. And least these conjectures should seem too too unlikely, 'twill not be amiss to add in favour of the first of them, that having purposely provided a large Pewter Box, with a cover to screw on it, and having fill'd it almost full of water, (I say almost, because if the vessel had been quite full, the congealing cold might have burst it) and carefully weigh'd the Aggregate of both (which amounted to 3v. 3ij. gr. 11. whereof the vessel weigh'd $\text{3ij. 3vß. and gr. 8.}$) we expos'd the water after the Top of

of the pot was screw'd on, to hinder the Avolation of it, to the freezing Air all night, and the next morning found it frozen from the top to the bottom, though not uniformly and perfectly, but found not one grain difference betwixt its present and its former weight: And as for the second conjecture newly propos'd, though it may seem somewhat strange, yet it is confirmable by this Experiment; that having plac'd divers lumps of solid ice in a Pottinger, which together with them weigh'd a pound, consisting of 16 $\frac{3}{4}$, and having expos'd these things in the same scales, wherein they were weigh'd, to the free Air on a very frosty night, we found the ice to have lost the next morning 24. grains of its weight, and the weather continuing so cold, that it froze hard all day long in the shade, I gave order to have it kept out of the Sun in the same scales, during all that time, and a good part of the following night, and then weighing it the second time, found, that the whole decrement of weight, did now amount to five grains above two drachms,

drachms, though the weight of the ice without the pottinger were but about seven ounces; and when we had kept about 13. ounces of ice in a very frosty night expos'd to the cold Air, it had lost as early as the next morning 'a good deal' above two drachms of its former weight: But these *Statical* observations have perhaps already but too much swell'd this *Appendix*.

Title



Title XXI.

Promiscuous Experiments and Observations concerning Cold.

i. **I** Hope it will not be imagined, that I have such narrow thoughts of the subject I treat of, Cold, as to believe, that I have compriz'd under those few Titles, prefix'd to the Sections of this Historical Treatise, all the Particulars that I knew to belong to so comprehensive a Theme, as would readily appear, if I thought it convenient to insert here the Scheme of Articles of inquiry, that I drew up to direct my self, what inquiries and Experiments to make. But though there were divers of those Heads, to which I could say so little, that I judg'd it improper to assign them distinct Titles, *because* as to some of them,

them, I had not time and opportunity to make those Trials, which if I had not wanted those Requisites, might have been made even here in *England*: and *because* also, as to more of them, I conceiv'd my self unable to produce in this temperate Climate, so strong and durable a Cold, as seem'd necessary to make the trials, that might be referr'd to them, succeed so far, as to satisfie my doubts, either affirmatively, or negatively: Though, I say, these, and some other Considerations kept me from increasing the Number of the Titles, among which I have distributed the Experiments and Observations, that make up the foregoing part of this Treatise, yet since divers particulars have occur'd to me, which though they seem not properly reducible to the foregoing Titles, do yet belong to the subject and design of this Treatise, I think it fit to annex them in this place, and without any other order than that, wherein they shall happen to occur to me, throw them into this one Section, together with some loose Experiments, and divers
 Rela-

Relations, that I have met with among Navigators and Authors, that have travell'd into the Northern Climates, touching Cold, not forbearing to insert promiscuously among them, some few *Paralipomena*, which if they had seasonably come to my hands, or into my mind, might have had a more proper place among the foregoing Sections, or have composed a Title by themselves. Wherefore though the Observations will not be altogether unaccompanied with Experiments, yet for the reasons above intimated, much the greater part of what is to be deliver'd under this Title, will consist of Collections out of Voyages, in which the strange things mention'd, being such as we cannot examine by our own Trials, I can equitably be thought answerable for the Truth of nothing, but the Citations.

2. I remember I tri'd at several times divers Experiments, to discover, whether or no congelation would by constriction of the pores of Bodies, or vitiating their Texture, or arresting the motion of their parts,

hinder them from emitting those *Effluvia*, that we call odors, but the Register of these Observations, being unhappily lost in one of my late removes, I dare add but these few, wherein I have no cause to distrust my memory.

3. I did in the Moneths of *December* and *January*, at several times gather differing sorts of flowers in frosty weather, but in most when they were freshly gather'd, and hastily smelt to, I could scarce perceive any sensible smell, whether it were, that the causes above hinted, hinder'd the expiration of the odoriferous steams, or that the cold had some undiscerned influence upon the Organ of smelling, which made the sense more dull, or that the same cold kept the Alimental juice of the flowers from rising in such plenty, and abounding so much with spirituous parts, as was usual at the more friendly seasons of the year: and this seem'd the more likely to be one reason of the *Phænomēon*, because most of the flowers were flaggy, and as it were ready to wither, and because also a *Primrose*,
that

that was vigorous and fresh in its kind, had an odor, that was manifestly (and 'twill easily be believ'd, that it was not strongly) sweet, and genuine.

4. I took also about an ounce by guess of Rose-water, and putting it into a small vial, after I had smelt to it, it was expos'd to freez in the open Air, and when it began to have ice in it, I then smelt to it again, but found not the perfume considerably, if so much as manifestly abated, and lastly, having suffer'd it to continue in the Air, that was then very sharp, till 'twas quite frozen, and discover'd no liquor, when the vial was turn'd upside down, the ice notwithstanding was not destitute of a graceful and genuine sent, though it seem'd somewhat faint ; but after the ice was reduc'd to water again, the fragrancy appear'd considerable. But on this occasion 'twill not be improper to subjoyn this Caution, That care must be had in Trials of this Nature, to make ones estimate betimes, for if a man should stay too long about it, there is danger, that the warmth of

ones breath and face may relax the pores, or thaw the surface of the ice, that is held near his Nose, and both free and excite the Corpuscles of smell, that are imprison'd there, that so instead of ice he may smell a liquor. The reasonableness of which advertisement may be justifi'd by an Experiment that I am about to annex. For being pretty well confirm'd by the casual and unwilling Observations of one of my friends, curious in making sweet water, That even Liquors, more easie to be spoilt then Rose-water, would not have their fragrancy destroy'd, though perhaps impair'd, nor so much as their odors for the time quite imprison'd and suppress'd by congelation, and this appearing congruous to what I formerly noted of the *Effluvia*, that may by the Decrement of weight be gathered to issue from ice it self, I thought it worth while to try, whether stinking Liquors would not be more alter'd by congelation, then odoriferous ones: and accordingly having procur'd some rain water, that had been kept in a Tub, till it

stunck

stunck so strongly, that I could hardly endure it near my nose, I caus'd a pottinger full of it to be expos'd all night to a very sharp Air, and examining it the next morning, when it was all turn'd into ice, neither I nor some others, to whom it was offer'd, could perceive any stinck at all in it: and having in another place, but with as stinking water, repeated the Experiment, when the pottinger was the next morning brought to my beds side, I found it to smell abominably, whereupon guessing, that this difference proceeded from some thaw made by the warmth of the room in the superficial parts of the ice, I found it to be so indeed, partly by the help of the light, which discovered a little liquor upon the ice, and partly by exposing the vessel with that liquor in it to the cold Air again, by whose operations an ice was produc'd, that was perfectly inodorous; and I remember, that one of these parcels of ice being thaw'd, seem'd to be less stinking then before

* If it had not been for the negligence or mistake of one, that I ordered in my absence to freeze and thaw the same water, divers times one after another, I might have added the success of that Experiment, which I was sorry to miss of, because it might possibly have afforded an useful hint about a way to correct stinking water in some Climates or seasons.

* it had been frozen, and if I had not been diverted, I should have tried, whether this ice, that did not emit odors, would emit like other

ice, *Effluvia*, discoverable by the Scales: for whether the ice would lose of its weight, which seem'd the more probable, or would not, the event may afford a not inconsiderable hint.

5. It is a thing not only remarkable, but scarce credible, that though the Cold has such strange and Tragical effects at *Musco*, and elsewhere in Cold Countries, as we have formerly mention'd, especially a little after the beginning of this 18. and somewhere in the 19. Section, yet this happens to the *Russians* and *Livonians* themselves, who not only by living in such a Countrey, must be accustomed to bitter Colds, but, who to harden themselves to the Cold, have us'd themselves, and thereby brought themselves to be able to

to pass to a great degree of Cold, from no less a degree of heat, without any visible prejudice to their healths. For I remember, that having inquired of a *Virtuoso* of unquestionable credit, whether the report of our Merchants, concerning this strange custom of the *Muscovites* and *Livonians* were certainly true, he assur'd me, that it was so, at least as to the *Livonians*, among whom being in their Countrey, he had known it practis'd. And the same was affirm'd to me by an ingenious person, a Doctor of Divinity, that had occasion some years since to make a journey to *Musco*. And the Tradition is abundantly confirm'd by *Olearius*, whose Testimony we shall subjoyn, because this seems one of the eminentest, and least credible instances, that we have yet met with of the strange power that custom may have, even upon the Bodies of men. 'Tis a wonderful thing, says he, to see how far those Bodies (speaking of the *Russians*, that are accustomed and hardned to the Cold) can endure heat, and how when it makes them ready to faint, they

Olearius,
livre 3.
pag. 168.

go out of their Stoves stark naked, both men and women, and cast themselves into cold water, or cause it to be pour'd upon their Bodies, and even in Winter wallow in the Snow. To which passage our Author adds from his own observation particular Examples of the Truth of what he delivers.

6. I had several years since, the curiosity to try, whether there were any truth in that tradition, which is confidently affirm'd, (and experience by some is pretended for it) that the Beams of the Moon are cold, but though I were not able to find any such matter, either by the ununited beams of the Moon, or by the same beams concentred by such Burning-glasses as I then had; yet having some years after furnish'd my self with a large and extraordinary good metalline Concave, I resolv'd to try, whether those beams were not only devoid of cold, but also somewhat warmish, since they are the Sun's beams, though reflected from the Moon. And we see, that his beams, though reflected from glasses not shap'd for Burning, may yet produce some

some not insensible degree of warmth. But notwithstanding my care to make my Trials in clear weather, when the Moon was about the full, and, if I misremember not, with a Weather glass, I could not perceive by any concentration of the Lunar beams, no not upon a black object, that her light did produce any sensible degree, either of cold or heat; but perhaps others with very large glasses may be more succesful in their Trials.

7. On this occasion I shall add, that meeting the other day in a Booksellers shop, with the works of the Learned Physician *Sanctorius* (whom I look upon as an inquisitive man, considering when and where he liv'd) a Picture drew my eyes to take off an Experiment, whereby he thinks to evince the light of the Moon to be considerably hot, which he says, he tri'd by a Burning-glass, through which the Moons light being cast upon the Ball of a common Weather-glass, the water was thereby depressed a good way, as appear'd to many of his disciples, amidst whom the obser-

observation was made. But though this may invite me, when opportunity shall serve, to repeat my Trials, yet I must till then suspend my assent to his Conclusion. For my Burning-glass was much better, then by the Narrative his seems to have been, and my Trials were perhaps at least as carefully and impartially made, as his Experiment in which this may probably have impos'd upon him; That performing the Experiment, a company of his Scholars, whilst they stood round about his Thermoscope, and stoop'd (as in likelihood their curiosity made them to do) to see by so dim a light the event of the Experiment, the unheeded warmth of their breath and bodies might, unawares to *Sanctorius*, somewhat affect the Air included in the Weather-glass, and by rarifying it, cause that depression of the water, which he ascrib'd to the Moon beams. But because this is a conjecture, I intend, if God permit, to repeat the Experiment, when I shall have opportunity to do with a more tender Weather-glass, then I had by me, when I made my former Observations,

To the XI. Title.

BY the unsuccessfulness of the former attempts made with an Iron instrument, I was invited, especially being at another place, where I was unfurnish'd with such hollow Iron balls, as are mention'd *Num. the 10.* to substitute the following Experiment. I caus'd a skilful Smith to take a Pistol barrel, guess'd to be of about two foot in length, and of a proportionable bore, and when he had by riveting in a piece of Iron, exactly stopp'd the touch-hole, I caus'd him to fit to the nose of the barrel a screw, to go as close as well he could make it, and then having fill'd it to the very top with water, I caus'd the screw to be thrust in (which could not be done without the Effusion of some of the water) as forcibly as the Party I employ'd was able to do it, that the water, dilated by Congelation, might not either drive out the screw, or get between it and the top of the Barrel,

rel, and having then suspended this barrel in a perpendicular posture in the free Air, in a very cold night, which then unexpectedly happen'd, and gave me the opportunity of making the trial, I found the next morning, that the intumescent water had thrust out a great part of the screw, notwithstanding, that to fill up intervals, I had oyl'd it before, and was got out betwixt the remaining part of it, and the barrel, as appear'd by some ice, that was got out, and stuck round about the screw; wherefore the bitter cold continuing one day longer, I did the next night cause the intervals, that might be left betwixt the male and female screws, to be fill'd up with melted Bees wax, which I presum'd would keep the screw from being turn'd by the water: and having in other points proceeded as formerly, I found the next morning, that the screw held, as I desir'd, and the preceding night having been exceeding bitter, the cold had so forcibly congeal'd and expanded the water, that it burst the Iron barrel somewhat near the top, and made a considerable

derable and oblique crack in it, about which a pretty quantity of ice appear'd to stick, besides that there were three or four other flaws, at some of which smaller quantities of water appear'd to have got out. At the same time, that I bespoke this Iron Barrel of the Smith, I order'd him to get me a brass one fill'd up after the same manner, to make the Experiment the more satisfactory. But though he could not procure it, yet the success was not unwelcome, because it was manifest, that there were cracks in the Iron in one place conspicuous, and in others easily discoverable, by blowing into the barrel, and putting on the outside of the suspected parts, either spittle, or some fit liquor, whose agitation plainly disclos'd the egress of the wind, and there appear'd small cause to doubt, but that these cracks were produc'd by the operation of the cold, since not only the Smith was a skilful man in his trade, and one that I us'd to imploy about Instruments, and also the barrel had been sometimes kept many hours fill'd with water, without

out appearing other then very stanch : but which is the considerablest circumstance the night before, the frost as I lately noted, was not able to make the water break out at any of these clefts, though it were able to force it self a way out at the screw, in spite of all the care we had taken to make it go close. I have only this circumstance to add about this matter, that when by thawing one part of the ice, some pieces of the rest were got out of the barrel, all I took notice of appear'd to be full enough of Bubbles, but yet such as seem'd lesser then ordinary, whether they were so by chance, or were determined to be so, by the resistance or compression, which the freezing water found upon its endeavouring to expand it self in the barrel.

An

Appendix to the XVII. Title.

Long since the writing of the foregoing Section, meeting with a passage in *Bartholinus*, where he vouches *Cabæus* for the Experiment of congealing water (without limiting it to any season of the year) by putting Salt petre into it, and shaking it strongly, I was thereby confirmed, that I was not mistaken, in supposing, that *Gassendus* (mention'd in the former Section) did not exclude that corporal and visible Nitre out of the number of the grand efficients of congelation. For *Cabæus* having publish'd his comment upon *Aristotles* *Meteors* (whence this experiment is taken by *Bartholinus*) before *Gassendus* publisht his Book, 'tis probable, that he as well as others borrowed the Experiment from him, and *Cabæus*, as *Bartholinus* quotes him, prescribes the putting the Salt-petre its self into water, which being a while put into a brisk motion, will
after

after some agitation, not only refrigerate that water, but bring it to a true and proper congelation.

Wherefore suspecting, that this relation, wherein *Bartholinus* says, he will believe him without an oath, may have given rise to the opinions and affirmations of those ingenious writers, that have since ascrib'd such wonderful coldness to Nitre, and finding in *Bartholinus*, that *Cabæus's* proportion betwixt the Nitre and the water, was that of 35. to a 100. that is almost as *one* to *three*, I thought it very well worth while to make Trial of an Experiment, which seem'd to me little less unlikely then considerable.

I took then a pound of good Saltpetre, and near 3. pound of common water (to observe the more narrowly *Cabæus's* proportion) these being put into a large new Pipkin, were kept constantly and nimbly stirr'd about, sometimes by me, sometimes by one or other of my Domesticks relieving one another, when they were weary, but though the mixture was with a kind of broad glass spattle
kept

kept in a brisk motion, that for the most part was after the manner of a whirle-pool, and sometimes a more confus'd agitation, and though we kept it thus stirring for almost an hour and a half, till we saw no likelihood of effecting any thing by trying our selves any further, yet not only we could not perceive, that any Atom of true ice was produc'd, whereas according to our Authors we might have expected a true and perfect congelation of all or the greatest part of the water, but we did not find, that there was so much as any freezing of the vapours on the outside of the vessel; and for this reason we thought fit, about the same time, to try the Experiments by another kind of Agitation, and mixing two ounces of Salt-petre with about six of water, in a conveniently siz'd vial, we did several of us successively vehemently shake the vial too and fro, till we were almost tyr'd; but neither this way was there produced the least ice within the glass, or the least congelation of the vapours of the Air on the outside of it. 'Tis

true, that when so great a proportion of Salt-petre began to be dissolv'd in the Pipkin, the water had a sensible increase of coldness, which afterwards seem'd to diminish, when once the Nitre was dissolv'd; but not to mention, that (if I much mistake not) we have observ'd the water to be refrigerated, when upon the dissolution of common salt, multitudes of actually cold and solid Corpuscles came to be every way dispers'd through it; this coldness produc'd by the Nitre, was very far short of the degree requisite to congelation: for to satisfy myself, that my sense did not misinform me, I took a good seal'd Weather-glass of about ten or twelve inches long, and immersing it into the cold mixture of Nitre and Water, I observ'd the tinged spirit of Wine in the stem to descend not inconsiderably, and when I perceived that degree of cold to have wrought its effect, I remov'd the Thermoscope into a vial fill'd with common water, about which I had caus'd to be plac'd a mixture of beaten ice and salt, to refrigerate the

con

contained water, in which the ball of the Instrument being plac'd, the spirit of Wine hastily descended two or three inches below that place at which it stood, when 'twas remov'd out of the Nitrous solution : And for further satisfaction removing the Thermoscope once again into that solution, the spirit of Wine in the stem was hastily impell'd up, as if the bubble had been put into warm water. And once more the Weather-glass being remov'd into the formerly mention'd refrigerated water, the tincted liquor began to fall down hastily again, and within a while subsided almost into the bubble, whereupon to avoid injuring the instrument, we thought fit to take it out ; so that upon the whole matter, if the learned *Cabeus* were not deluded by mistaking some Crystals of Nitre (which I have observ'd easily to shoot again in water, that has been glutted with it) for true and proper ice, I cannot but wonder at his assertion, and must take the liberty to think my self warranted by so many Harmonious Trials, as I have found

unfavourable to the suppos'd supremacy of Cold in Salt-petre, to retain my former opinion about it, till more succesful Experiments withdraw me from it.

'Tis a receiv'd Tradition among the Water-men and many others, that the Rivers, if not Ponds also, are frozen first at the bottom, and begin to thaw there. But though I find this opinion to be in request, not only among English Water-men, but among the French too, yet I think it may be very warrantably question'd : For 'tis evident in waters we expose to freez in large vessels, that the congelations begin at the surface, where the liquor is contiguous to the Air, and thence as the cold continues to prevail, the ice increases and thickens downwards, and therefore we see, that Frogs retire themselves in frosty weather to the bottom of ditches, whence I have had many of them taken out very brisk and vigorous, from under the thick ice that cover'd the water. And I have been inform'd by an observing person, that at least in some places, 'tis usual in Winter

Winter for shoals of Fishes to retire to those depths of the Sea, if not of Rivers also, where they are not to be found in Summer. Besides if Rivers were frozen at the bottom, we must very frequently meet in the emergent pieces of ice, the shapes of those irregular Cavities and Protuberances, that are often to be found in the uneven soils, over which Rivers take their course, whereas generally those emergent pieces of ice are flat, as those flakes, that are generated on the surface of the water. Moreover if even deep rivers freez first at the bottom, why should not very many Springs and Wells freez first at the bottom too, the contrary of which nevertheless is obvious to be observ'd. In confirmation of all which we may make use of what we formerly noted (in the Section of the *Primum Frigidum*) about the practise of the Masters of the French Salt-works, who by overflowing the Banks and Causeways all the winter, keep them from being spoil'd by the frost, which could not be done, if the waters they stand under froze as well at the bottom, as at the Top, But

But I find, that that, which deceives our Water-men, is, that they often observe flakes of ice to ascend from the bottom of Rivers, to the Top, and indeed it often happens, that after the hard frost has continued a while, these emergent pieces of ice, do very much contribute to the freezing over of Rivers. For, coming, in some of the narrower parts of them, to be stopp'd by the superficial ice, that reaches on each side of the River a good way from the Banks towards the middle, those flat icy bodies are easily cemented by the violence of the cold, and by the help of the contiguous water, to one another, and by degrees straitning, and at length choaking up the passage, they give a stop to the other flakes of ice, that either emerging from the bottom, or loosened from the banks of the River, or carried down the stream towards them, and these being also by the same Cold cemented to the rest, the River is at length quite frozen over. And the reason why so many flakes of ice come from the bottom of the River, seems to be, that

that after the water has been frozen all along near the banks, either the warmth of the Sun by day, or some of those many casualties, that may perform such a thing, does by thawing the ground, or otherwise loosen many pieces of that ice together with the earth, stones, &c. that they adher'd to, from the more stable parts of the banks, and these heavy bodies do by their weight carry down with them the ice they are fastned to; but then the water at the bottom of the river being warm in comparison of the Air in frosty weather (since that even common water is so, we have manifested by experience, where we show how much sooner ice will be dissolv'd in water, then thaw'd in Air) the dispers'd ice is by degrees so wrought upon, that those parts by which it held to the stones, earth, or other heavy bodies being resolv'd, the remaining ice being much lighter bulk for bulk, then water, gets loose, and straightway emerges, and may perhaps carry up with it divers stones and clods of earth, that may yet happen to stick to it, or be inclos'd in it,

In the Section touching the duration of Ice.

the sight of which perswades the Water-man, that the flakes of ice were generated at the bottom of the river, whereas a large piece of ice may carry up and support bodies of that kind of a great bigness, in case the ice itself be proportionably great, so that the Aggregate of the ice, and heavy bodies, exceed not the weight of an equal bulk of water. On which occasion I remember, that Captain *James Hall* in a voyage, extant in *Purchas*, relates, that upon a large piece of ice in the Sea they found a great stone, which they judg'd to be three hundred pound weight. But of the Tradition of the Water-men we shall say no more, then that this hath been discours'd, but upon no great information, though the best we could procure; so that for further satisfaction, it were to be desir'd, that either by sending down a Diver, or by letting down some instrument fit to feel (if I may so speak) the bottom of Rivers with, and to try, whether ice, if it met with any, be loose from, or uniformly coherent to the ground, and also bring up parcels of whate-

whatever stuff it meets with there, the matter were by Competent Experiments put out of doubt.

We took a seal'd Weather-glass furnish'd with spirit of Wine, and though not above 10. inches long in all, yet sensible enough, and having caus'd a hole to be made in the Cover of a Box, just wide enough for the smaller end of the Glass to be thrust in at, we inverted the Thermometer, so that the ball of it rested upon the cover of a Box, and the pipe pointed directly downwards, then we placed about the ball a little beaten ice and salt, and observ'd, whether, according to our expectation, the tinctured spirit, that reach'd to the middle of the pipe, or thereabouts, would be retracted upon the refrigeration of the liquor in the ball, and accordingly the spirit did in very few minutes ascend in that short pipe above an inch higher, then a mark whereby we took notice of its former station, and would perhaps have ascended much more, if the application of the frigorifick mixture had been continued, by which, and another succeeding

ing Experiment to the same purpose, it seems, that the condensation of liquors by cold, is not always effected by their proper gravity only, which ordinarily may be sufficient to make the parts fall closer together : but whether in our case the contraction be assisted by some little tenacity in the liquor, or by the spring of some little aerial, or other spirituous and Elastick particles, from which the instrument was not perfectly freed, when it was seal'd up, or which happened to be generated within it afterwards, will be among other things more properly inquir'd into in another place, where we may have occasion to make use of this Experiment.

There is a famous Tradition, that in *Muscovy*, and some other cold Countries, 'tis usual out of Ponds and Rivers to take up good numbers of Swallows inclos'd in pieces of ice, and that the benumm'd birds upon the thawing of the ice in a warm room, will come to themselves again, and fly about amazedly for a while, but not long survive so great
and

and sudden a change. I have in another Treatise already said somewhat about this Tradition, and therefore shall now say no more of it, then these two things. First, that I since was assur'd by a person of honour, that is very curious, and was commanded by (a many ways) great Prince to inquire out the truth of it, when he was in some of those Countries, where the thing is said to be familiar enough, and that the eminentest and soberest persons he could ask affirm'd the thing to be true: But (secondly) having lately inquired about this matter of a knowing person of quality, that was born and bred in *Poland*, he answered me, That in the parts where he liv'd, it was a very general and unquestion'd opinion, that Swallows often hid themselves all the Winter under water in Ponds and Lakes, and Seggy places, and that the Fishermen, when having broken the ice, they cast their Nets for Fish, do draw them up benumbed, but not dead, so that they quickly in Stoves recover their wings, but seldom after that prolong their
lives :

lives : But as for their being taken up in ice, he told me, he had not heard of it, though I see not why in case they commit themselves to shallow waters, as those of Ponds and Seggy places, often are a sharp lasting frost may not sometimes reach them. And therefore that which left me the greatest scruple about this Tradition, is, That this Gentleman, notwithstanding his curiosity, could not affirm, that ever he himself had seen any example of the thing he related.

But I will take this occasion to add, that having a mind in frosty weather to try some Anatomical Experiments about Frogs, one that I imploy'd breaking in a Ditch some ice that was very thick, and of which he was to bring me a quantity, found in the water, that was under the ice, good store of Frogs (besides some Toads) which I found to be very lively, and divers of which I kept for certain uses a good while after.

To confirm, and to add some *Paralipomena* unto what I have deliver'd in the Second, and in the Twentieth Titles, about the frosts getting into
hard

hard and solid bodies, I shall here subjoyn some particulars there omitted, which I have learned partly from Experiments, and partly from persons worthy of credit, whom I purposely consulted about this matter.

And first as to the freezing of Wood, we have sometimes tri'd it by purposely exposing partly other Wood, and partly branches cut off from growing Trees, to an intense degree of Cold, by which the wood seem'd in one night to be for some little depth manifestly enough invaded by the frost. But a domestick of mine having a little while since had occasion to fell an old Apple-tree, on a day that had been preceded by a fortnights bitter frost, came and informed me, That he found, that the frost had evidently pierc'd into the very middle of it, though it were about a foot in Diameter. And an Experienc'd Artificer, whose head and hand were much imploy'd about the building of great mens houses, told me, that he had often seen here in *England* pieces of Timber it self manifestly

nifestly frozen, and rendred exceeding difficult to be saw'd, the frost also appearing by evident signs to continue in the saw-dust. And therefore it will be the less strange, if in *Poland* the effects of Cold upon wood be more conspicuous. For a learned native assur'd me, that in his Countrey 'twas usual to have wood frozen so hard, that the Hatchets would not cut it, but rebound from it, and that 'twas very usual to hear in the night a great many loud cracks, almost like the reports of Pistols, of the shingles or wooden ryles, wherewith in many places they cover their houses instead of Slate, and this (as I purposely ask'd) when the weather was dry, and excessively cold. When I likewise inquir'd about the thawing of wood, he told me, he had several times seen pieces of Timber, which having been thoroughly frozen in the Air, did, when brought into rooms made warm by Stoves, become cover'd with a kind of hoar frost, and made them look white, and that though his Bow (which he shew'd me) were very strong and tough,

tough, as being made not of wood, but horn, and other close materials, it would be so chang'd by the frost, that unless special care were had in the thawing of it, it would break.

That Marle and Chalk, and other less solid terrestrial Concretions will be shatter'd by strong and durable frosts, is observ'd by Husbandmen, who thereby find it the better fitted to manure their land, the Texture of those bodies, during whose intireness, the parts most proper to feed grass and corn, are more lock'd up, being by congelation in great part dissolv'd, but that true and solid stones wont to be imploy'd in noble and durable Buildings, should be spoil'd by the frost, will perhaps to most readers seem very improbable. And therefore I shall here add what I have learn'd by inquiry of the ingeniousest and most experienc'd Mason I have met with, because it may not only surprize most readers, but prove an useful observation to him. Having then inquir'd of this Tradesman, whether he did not find, that some free stone, a name vulgarly known, would

would not be spoil'd by the frost, he told me, that he had often observ'd both free stone and harder stones then that, to be exceedingly spoil'd by the frost, and reduc'd to crack or scale off, to the blemishing and prejudice of the houles, that are built of them. But because it may be objected against this, that experience shews us, that divers of the stateliest Fabricks in *England* have these stones for their chief materials, and yet indure very well the inclemencies of the Air, the reply may be, that the difference may not consist in the peculiar natures of the stones imploy'd, but in the several seasons in which the same kind of stones are digg'd out of the Quarry. For if they be digg'd up, when the cold weather is already come in, and imploy'd in building the same Winter, they will, upon very hard frosts, be apt to be shatter'd or scale, but if they be digg'd early in the Summer, and suffer'd to lye expos'd to the Sun and Air, during all the heat of the Summer, these season'd stones, if I may so call them, may outlast many sharp

sharp Winters unimpair'd. It seems to me worth trying, whether during their insolation, if that term may be allow'd me, there do not by the operation of the heat and air upon them, exhale a certain unripe mineral, sap, or moisture (whose recess may perhaps be discover'd by weight) which if it remain in the stone, may by very piercing frosts be congeal'd almost like the sap in Timber-trees, and shatter the Texture of the stone, which agrees well with what was told me by an understanding person, that is Master of a great Glass-house, of whom having purposely inquir'd, whether he did not find, that his great earthen pots, which are made up with as little water as is possible, & are deservedly famous for their durable Texture, had not that Texture alter'd and impair'd by very piercing frosts ; he assur'd me, that if he did not take care to keep the frost (as they speak) from getting into them, those great and solid vessels, where in he us'd to keep his glass in fusion, would in the fire scale or crack (and perhaps fly) and become unservice-

R r

able

able no less then some weeks sooner, then if they had never been impair'd by the frost. And when I inquired, whether also glass it self would not be much prejudiced thereby, he affirmed to me, that oftentimes in very hard frosts many glasses, that had continued intire for many weeks (for that circumstance I was sollicitous to ask about) would as it were of their own own accord crack with loud noises. But whatever prove to be the issue of such Trials, it will not be amiss to confirm the *Phænomenon* it self, by the testimony of an illiterate, but very experienc'd French Aurhor, who on a certain occasion tells us, (as I also take notice in another * Treatise) That he knows the stones of the mountains of Ardenne (famous enough in France) are harder then Marble, and yet the inhabitants of that Countrey do not draw them out of the Quarry in winter, because they are subject to the frost. And it has been divers times seen, that upon thaws, the rocks without being cut, have fallen down, and kill'd many.

But it may yet seem far more unlikely,

* Of the
imperfe-
ction of
Physicks.
Maitre
Bernard
Palissy.

likely, that frosts should get into mettals themselves, and yet having ask'd the newly mention'd *Polonian*, whether he had observ'd any thing of that kind, he answer'd, that he had often by drawing out his sword and pulling out his pistols, when he had been long in the field, and came into a hot room, found them quickly almost whitened over, by a kind of small hoar frost. But whether this were, as he conceiv'd any thing, that was drawn out of the Steel, and setled on the surface of it, I want circumstances enough to make me willing to determine. But if we will credit *Olaus Magnus*, it must be confess'd, that considerably thick pieces of Iron and Steel it self, will in the Northern Regions be render'd so brittle by the extreme frost, that they are fain to temper their instruments after a peculiar manner: his words, which being remarkable, I forbear to alter, are these, *Videntur præterea*

ferrei ligones certa ratione fabricati, quia his spissa atque indurata glacies cæteris instrumentis ferreis non cedens facilius infringitur dum aliæ secures chalybe

*Lib. 1.
pag. mihi
23.*

*permixta, in vehementi frigore ad so-
lum glaciei vel virentis arboris idum in-
star vitri rumpuntur, ubi ligones prædi-
cti siue ferreae hastæ fortissimi manent.*

Which testimony, notwithstanding what some have written to this Authors disparagement, does not seem to me at all incredible. For I remember, that even here in *England* I have had the curiosity to cause trials to be made in very frosty weather, whereby, if an expert Smith I then us'd to imploy, did not *gratis* deceive me in the Irons I imploy'd, that mettall may by such degrees of cold, as even our Climate is capable of, be render'd exceeding brittle, as he several times affirm'd to me, that there are some kinds of iron which he could hammer, and turn, as they phrase it, cold in open weather, which yet in very hard frosts would become so brittle, as by the same way of working easily to break, if not to flye asunder. And this he affirm'd both of Iron and Steel, of which latter mettall another very skilful workman, whom I also consulted, certifi'd the like: but though this disagreed not with

with trials purposely made on Iron rods had inform'd me, yet presuming, that in such a nice piece of work as a spring, some further satisfaction about this matter might be obtain'd, I inquired of a very dexterous Artificer, that was skill'd in making springs for others, whether or no he found a necessity of giving springs another temper in very frosty weather, then at other seasons, and he answered me, that in such weather, if he gave his springs the same temper, that he did in mild and open weather, they would be very apt to break. And therefore in very sharp seasons he us'd to take them down lower, as they speak, that is, give them a softer temper then at other times, which as it makes it probable, that the cold may have a considerable operation upon bodies, upon which most men would not suspect it to have one, so that discovery may afford a hint, that may possibly reach further then we are yet aware of, touching the interest that cold may have in many of the *Phænomena* of nature.

I should here subjoyn, that in prosecution of what is deliver'd in the XX. Section about the weight of solid bodies, that I there wish'd might be expos'd to a congealing Air, I did cause some Trials of that kind to be made in a very frosty night, especially with Bricks, but something that happened to the only Scales I then had fit for such an Experiment, made me doubt, whether some little increase of weight, that seem'd to be gain'd by congelation, were to be reli'd upon, though there did not appear any hoar frost, or other thing outwardly adhering, to which the effect could be ascrib'd.

It is a Tradition, which the Schools and others have receiv'd with great veneration from their Master *Aristotle*, that hot water will sooner freeze than cold; but I do not much wonder, that the learned *Cabeus*, as I find him quoted by *Bartholinus*, should contradict this Tradition, though he be himself a commentator upon that Book of *Aristotle*, wherein 'tis deliver'd. For I could never satisfy my self, that there is (at least with

with our water, and in our Climate) any truth in the Assertion, though I have made trial of it more ways then one, but it may very well suffice to mention a few of the plainest and easiest Trials, with whose success I am well satisfi'd as to the main, as the Reader also will, I doubt not, be ; though not having, for want of health, been able to have so immediate an inspection of these, as of the rest of my Experiments, I was sometimes fain to trust the watchfulness of my servants (whom I was careful to send out often) to bring me word how long after the first freezing of the cold water, it was before the other began to be congeal'd.

We took then three pottingers, as near of a size as we could, and the one we fill'd almost to the top with cold water, the other with water, that had been boil'd before, and was moderately cool'd again, and the third with hot water ; these three vessels were expos'd together in the same place to the freezing Air.

In the Entry of one of the Trials, I find, that being all three put out at

half an hour after eight of the clock. That the pottinger that contain'd the cold liquor began to freez at $\frac{1}{4}$ after ten.

That which contain'd the water heated and cool'd again, began to freez $\frac{3}{4}$ past ten.

And that which contain'd the hot water, at half an hour after eleven, and somewhat better. So that though all froze within the compass of two hours, yet the cold water began this time to freez an hour and a $\frac{1}{4}$ sooner than the hot.

These pottingers were earthen, but I elsewhere made the Trial in others of mettall, and there also the cold water began to freez, both before that which had been heated and cooled again, and long before the hot.

Another time I measured out the water by spoonfuls into pottingers (not having then by me any fit Scales to weigh it) to be the more sure, that the quantities of water should not be considerably unequal, and then also the cold water froze a considerable while before the hot.

But my usual jealousy in the making

king nice Experiments, tempting me to inquire, whether the water in some of the former Trials had not been heated in a stone Bottle, not a Skillet, it was confels'd, that it was so, but that the bottle us'd to contain nothing but Beer, and had been wash'd before-hand: And though I did not think, that the bottle could have any considerable influence on the Experiment; yet least it should be suspected, that the scalding water, might have imbib'd some spirituous parts remaining yet among the minute dregs of Beer in the pores of the bottle, for the greater security I caus'd the water to be heated in a Skillet, and because in one of the Trials made in a Village, where we had not choice of pottingers, the cold water chanc'd to be put into one, that afterwards seem'd less, then that wherein the hot was expos'd, I did this very day repeat the Experiment, by putting cold water into a somewhat larger pottinger, heating the other water in a Skillet, and the event of the Trials is this,

That the cold water being put out
with

Unable to display this page

are not so proper for this. For having formerly had the curiosity to take two pipes of glass made of the same Cylinder, that they might be of equal bore, and having seal'd each of them at one end, and having fill'd both to the same height, and then stirr'd them too and fro together in a mixture of beaten ice, water and salt, (which mixture I make use of for the effecting sudden Congelations) I found both waters to freez too quickly to make a notable disparity in the length of times, that they remain'd uncongeal'd: And we will not on this occasion omit one *Phænomenon* afforded us by these Trials, because it may admonish men, how cautious they ought to be in making nice Experiments. For having once made the formerly mention'd Trial, with glass pipes, that were but slender (as not exceeding the bigness of a mans fore-finger) and having for greater caution put the hot water first into one glass, and then into another, we found one time, that the hot water froze first, and wondering at it, we examin'd the glasses, and percei-

perceiving one of them to be more Conical or acuminate, where it had been seal'd up then the other, it seem'd probable, and afterwards appear'd true, that the water in this acuminate part, being suddenly frozen by reason of the slenderness of the glass there, promoted and accelerated the Congelation of the rest, so that whether it were the cold or the hot water, that was put into that pipe, it would thereby gain a manifest advantage.

In the foregoing Experiments (made in pottingers) I made use not only of cold and hot water, but of water that had been heated and cool'd again, though not reduc'd to its full pristine coldness, to prevent the Objections of some, that might pretend, that such water would have frozen sooner then Cold, which yet would not salve the common opinion which specifies not such water.

Postscript.

Postscript.

And it seems, that such Cautions as I have been mentioning, are not altogether useless. For accidentally casting my eye upon the *Circulus Pisanus* of *Berigardus* upon *Aristotles* *Meteors*, I somewhat wonder'd to find, that an Author, who is look'd upon to be a great adversary of *Aristotle*, except in his dangerous and ill-grounded conceit of the eternity of the world, and some other erroneous opinions, does yet endeavour to justify *Aristotle* by affirming, that his Experiment will succeed, if by heated water we understand, that which having been heated, is suffered to cool again, till it be reduc'd to the temper of other water which was not heated. For this refrigerated water he says, he has found to congeal much sooner then the other water, but this I confess I am very unapt to believe. For having divers times caus'd cold water to be expos'd to the
Air

Air in frosty weather, with that which had been heated and cool'd again, and having set sometimes one of my Domesticks, sometimes another, to watch them, the events did very much disavour the assertion of our Author, though care was had of the circumstances most considerable in such an Experiment, as the matter, size and shape of the vessels; the equal degree of cold in the two several parcels of water (into both which I sometimes dipp'd my finger to judge of them before they were expos'd) and the place, in which they were put both together to be frozen. But for further satisfaction, we *elsewhere* took two pottingers, bought purposely for the making of Experiments, of the same size and shape, and in the same shop; one of these we almost fill'd with cold water out of a glass, wherein we mark'd how high that water reach'd, that by filling the same glass to the same height with the refrigerated water, we might be able to measure out the same quantity into the other pottinger. This done, I appointed one, whose

whose care I had no reason to distrust,
 to examine the tempers of the several
 waters, with a more then ordina-
 rily sensible Weather-glass, as a far
 safer Criterion then the bare touch,
 to judge of the coldness of liquors;
 these being reduc'd to the same tem-
 per, were expos'd to a very sharp
 Air, and there watch'd by the per-
 son, whom (being not well, and un-
 able to support such weather my self)
 I appointed to attend the Experi-
 ment, and he according to direction
 finding them begin to freez, as
 'twere at the very same time, brought
 me in the two pottingers, in each of
 which I saw the beginnings, and but
 the beginnings of congelation, where
 the upper surfaces of the waters were
 contiguous to the containing vessels:
 so that having made this Experiment
 with much greater exactness then
 probably *Berigardus* did, or, for
 want of such instruments as I us'd,
 could make it, I cannot but suspect,
 supposing the common waters, he and
 I us'd, to be of the same nature, that
 he was either negligent or overseen
 in affirming, that heated and refri-
 gerated

gerated water, will cool *so much sooner*, as he would persuade us, then other. * And

* *Quare ferventem aquam adhibuisse oportet qui asserit eam esse minus gelabilem, præcipue salsam. Pag. 571.*

as I am not convinc'd by experience, that it will

freez sooner *at all*, so till he have better made out the reason he seems to give of the *Phænomenon*, I must question whether he rightly ascribe after *Cabæus* (if I much misremember not) the congelation of water to a certain *Coagulum*, distinct from the cold spirits, that plentifully mingle with the water, which *Coagulum* it seems (for his style is not wont to be very perspicuous) that he would have to consist of certain dry *Cora*puscles, no less necessary to congelate water, then Runnet to curdle Milk : And for what this Author

* *Tam cito illa congelabat, ut eximerem ex eo crustam unam aut alteram antequam non calefacta vel levissime concrevisset. Pag. 572.*

says, * that he must have imploy'd boiling or scalding water,

who affirms it to be less congealable then other, that mistake may be sufficiently disprov'd by the several above recited Trials, wherein we found

found water, moderately refrigerated, to freez much later then cold, and whereas *Berigardus* intimates, that the person whoever he be, that he dissents from, does unskillfully suppose warm salt-water to be the less dispos'd to congelation for being salt, our Author is therein also mistaken; for though it be true what he alledges, that salt *outwardly appli'd* promotes the congelation of water, yet, that *dissolv'd* in water, it has a contrary effect, may appear by the familiar observation, that Sea-water is much more difficult to be congeal'd then fresh water: and to show, that 'tis not a property of Sea-water, but a water impregnated with common Salt, I have several times tri'd, that a strong solution of such salt in ordinary water, will not at all be congeal'd by the being expos'd to the Air, even in very sharp frosts, as may be easily collected from some of the Experiments mention'd in the former part of this Book. Another particular there is (about the use of Allume in reference to freezing) in this often cited passage of *Berigardus*,
Sf which

Here the
Postscript
ends.

which I might here examine, if my
hast and my indisposedness to ingage
in a controversie of small moment,
did not injoyne me to defer it till a fir-
ter occasion.

To confirm the power ascrib'd in
the VI. Section to cold, as to the
long preservation of bodies from cor-
ruption, 'twill not be amiss to add
these two remarkable passages, the
latter of which affords a good in-
stance of the improvement, that may
be made of some degrees of cold to
the uses of humane life.

Purchas
lib. 4. cap.
19. pag.
844.

The first observation is afforded
us by some of our Countrey-men, in
a Voyage extant in *Purchas*, where
the writer of it speaks thus: *Of the*
Samojeds, whose Countrey he visi-
ted, *Their Dead they bury on the side of*
the hills, where they live (which is com-
monly on some small Islands) making a
pile of stones over them, yet not so close,
but that we might see the dead Body, the
Air being so piercing, that it keepeth
them from much stincking savour: so
likewise I have seen their Dogs buried in
the same manner. The

The other observation is given us in the description of *Iceland* (made by one that visited it) to be met with in the same *Purchas's Collections*, where among other things he gives us this Account, which if I mistake not, I have had confirm'd by others, of their strange way of ordering and preserving their Fish. *Having taken* Lib. 3.
cap. 22. *them, they pluck out the lones, and lay up their bowels, and make Fat or Oyl of them: They heap up their Fish in the open Air, and the purity of the Air is such there, that they are hardned only with the Wind and Sun, without Salt, better surely then if they were corned with Salt. And if they kill any Beast, they preserve the flesh without stinck or putrefaction, without Salt, hardned only with the Wind.*

I know not whether 'twill be worth while to add to the fifth and sixth Numbers of the VII. Title, that, for further confirmation of our opinion, that 'tis not Nature's abhorrence of a *Vacuum*, but the distension of the water, that breaks glasses, when the contain'd liquors come to be congeal'd, I did on set purpose fill several vials (some at one time, and

some at another) to the lower parts of their necks (most of which were purposely made long) with common water, and though they were all left unstopp'd, that the external Air might come in freely to them; yet not only one of them, that I stirr'd up and down in a mixture of beaten ice, salt, and water, was hastily broken upon the congelation of the contain'd water, but several others, that were expos'd to be frozen more leisurely by the cold Air only, were likewise broken to pieces, by the expansion of the freezing water, as appear'd both by the gaping cracks, and also by this, that the ice was considerably risen in the necks above the waters former stations, which had been noted by marks before; and if it had been more easie for the included water to make it self room, either by stretching the glass, or (rather) leaving the superficial ice congeal'd at first in the neck, or by both those ways together, then to break the vessel, the vial would *probably* have remained intire.

I say *probably*, because I am not sure,

sure, that there may not sometimes intervene in these Experiments some, what that may need further observation and inquiring. For *as* it seems, that what I have been lately saying may be confirmed by an unstopp'd vial, which was expos'd at the same time to congelation, with this success, that without breaking the vial the water was frozen, and the ice in the neck impell'd up a good way above the height, at which the liquor rested before it began to congeal; *so* on the other side I remember, that I have sometimes had a good store of liquor frozen in a vial, without breaking the glass, though a vial were stopp'd: as if the difference, that I have on other occasions observed betwixt glasses, whereof some are very brittle, and others more apt to yield, might have an influence on such Experiments, or that some peculiar softness, or other property of the ice, that afforded me my observation, or else some other thing not yet taken notice of, were able to vary their success.

In confirmation of what is deliver-

ed in the VII. Section, about the expansion of water by freezing, I shall add, that having caus'd some strong glais-Bottles of a not inconsiderable bigness to be fill'd with a congealable liquor, excepting the necks, which were fill'd with Sallet oyl, I observ'd, that in a somewhat long, and very sharp frost the contained water was so far expanded by congelation, that it not only thrust up the corks, but the cold having taken away the defluency of the oyl, that liquor together with the water, that could no longer be contain'd in the Cavities of the glasses, being as it seem'd, frozen as fast as it was thrust out of the neck, there appear'd quite above the upper part of the Bottles, Cylinders of divers inches in height, consisting partly of concreted oyl, and partly of congeal'd water, having on their tops the corks that had been rais'd by them.

It is a Tradition very currant among us, that when Ponds or Rivers are frozen over, unless the ice be seasonably broken in several places, the Fishes will dye for want of Air. And

And I find this Tradition to be more general, then, before I made particular inquiry into it, I knew of. For *Olaus Magnus* mentions it more

Volentes igitur piscari sub glacie duo magna foramina latitudine 8. vel 10. pedum, centum & quinquaginta vel 200. passibus à se invicem directa distantia, aperiunt, interque 30. vel 40. minora foramina, latitudine unius pedis & semis, ab utroque latere distantia 30. pedum intermedia constituent, tum per ea, &c. Olai Mag. lib. 20.

then once, without at all questioning the truth of it, but rather, as if the general practise of the Northern Nations to break in divers places their frozen Ponds and Rivers, were grounded upon the certainty of it. In the twentieth Book (which treats of Fishes) after having spoke of the reasons, why the Northern Fishermen imploy so much pains and industry to fish under the ice, and having said among other things, that the nature of the Fish exacts it, he adds this reason, that, *Nisi glacie perforata re-*

spiracula susciperent, quotquot in flumine vel stagno versantur, subito morerentur. Another passage of the same Author, and taken likewise out of the same *Quæ (Anguillæ) si totaliter glacie constricta fuerint simul omnes respiraculum ab aere non habentes pariter suffocata moriuntur.*

Olaus Mag. Titulo, De cursu glaciali, pro piscibus.

(20.) Book you may meet with in the Margent, though in another place he seems to intimate another, and not an absurd, reason of the death of Fishes in Winter, where advising the Reader, that Ponds and Lakes did generally begin to freez in

* *Præmittendum est quod generaliter omnes lacus, & stagnales Aquæ in mense Octobri incipiunt congelari, glaciesque aucto frigore in plerisque locis tantum condensari, ut ubi venæ lacus & stagna viventis aquæ non intrant, pisces suffocati tempore resolutionis glaciei inspiciantur, verum ne hæc suffocatio tam dispendiosa fiat, diligentia piscatorum continue glacies ipsa perfringitur ne congeletur. Olai Magni lib. 1. Titulo de transitu glaciali, &c.*

* *October*, he adds, that Fishes are usually found suffocated, when the Thaw comes, where veins (or springs) of living water do not enter: by which passage he seems to make

the want of shifted water cooperate to the suffocation of the Fishes. And to the same purpose I shall now add, that having inquir'd of a learned Native, that had had about *Cracovia*, (whose Territory is said to abound much in Ponds) whether the *Polanders* also us'd the same custome, he answered me, that they did, and that sometimes in larger Ponds they were careful to break the ice in eight or

or ten several places, to make so many, either vents or Air-holes, for the preservation (as they suppos'd) of the Fish. And when I inquir'd of the often mention'd *Russian* Emperors Physician, whether in *Muscovy* the frost kill'd the Fishes in the Ponds, in case the ice were not broken to give them Air, he answered, that in ordinary Ponds it were not to be doubted, but that in great Lakes he could not tell, because the Fishermen use to break many great holes in the ice for the taking of the Fish. For at each of these holes they thrust in a Net, and all these Nets are drawn up together in one great breach made in some convenient place near the middle of the rest.

It appears then, that the Tradition is general enough, but whether it be well grounded, I dare not determine, either affirmatively or negatively, till trial have been made in Ponds with more of design or of curiosity, and watchfulness, then I have known hitherto done, men seeming to have acquiesc'd in the Tradition without examining it, and
to

to have been more careful, not to omit what is generally believ'd necessary to the preservation of their Fish, then to try, whether they would escape without it: Wherefore, though for ought I know the Tradition may prove true, yet to induce men not to think it certain, till experience has duly convinc'd them of it, I shall represent, That as much as I have in other Treatises manifested, how necessary Air is to Animals; yet whether Fishes may not live, either without Air, or without any more of it, then they may find interspers'd in the water they swim in, has not yet, that I know of, been sufficiently prov'd. For what we have attempted of that nature in our Pneumatical Engine, whether it be satisfactory or not, is not yet divulged. And I remember not to have hitherto met with any writer, (except *Olaus* be construed to intimate so much) that affirms *upon his own observation*, that the want of breaking ice in Ponds has destroy'd all the Fish. Besides, that possibly in frozen Ponds, there may be other reasons

sons of the death of the Fishes, that are kill'd (if any store of them be so) by very sharp frosts. For who knows what the locking up of some kinds of subterranean steams, that are wont freely to ascend through water unfrozen, may do to vitiate and infect the unventulated water, and make it noxious to the Fishes, that live in it: perhaps also the excrementitious steams, that insensibly issue out of the bodies of the Fishes themselves, may by being penn'd up by the ice, contribute in some cases to the vitiating of the water, at least in reference to some sort of Fishes. For being desirous to learn from a person curious of the ways of preserving and transporting Fish, whether some Fishes would not quickly languish, grow sick, and sometimes dy out-right, if the water they swam in were not often shifted, he assur'd me, that some kinds of them would: and it has not yet, that I hear of, been tri'd, whether or no, though Ponds seldom freez to the bottom, yet the water that remains under the ice (in which it self some Fishes may be now
and

and then intercepted) may not, even whilest it continues uncongeal'd, admit a degree of cold, that though not great enough to turn water into ice, may yet be great enough, when it continues very long, to destroy Fishes, though not immediately, yet within a less space of time, then that, during which the surface of the Pond continues frozen. But 'tis not worth while to be solicitous about conjectures of causes, till we are sure of the Truth of the *Phænomenon*; and these things are propos'd not so much to confute the Tradition, we have been speaking of, as to bring it to a Trial, which, having no opportunity to make in Ponds, I endeavour'd as well this Winter as formerly, to obtain what information I could from Trials made in small vessels, with the few Fishes I was able to procure. And I shall subjoyn most of these Trials, not because I think them very considerable, but because they are, for ought I know, the only attempts of the kind, that have yet been made.

To satisfy my self, whether the
ices

ices denying access to the Air, was that which destroy'd Fishes in frozen Ponds, I thought upon this Expedient, I procur'd a glass vessel with a large belly, and a long neck, but so slender, that it was only wide enough for the body of the Fishes to pass through, and then having fill'd the vessel with some live Gudgeons, and a good Quantity of water, the neck of it was made to pass through a hole that was left, or made for it in the midst of a metalline plate, or wooden Trencher, which could descend no lower then the neck, because of the inferior part of the glass that would not suffer it, and which serv'd to support a mixture of Ice (or Snow) and Salt, which was appli'd round about the extant neck of the glass. By this contrivance I propos'd to my self a double advantage: the *first*, that, whereas in broad vessels 'tis not always so easie, as one would think to be sure, that the surface of the water is quite frozen over in every part, by this way I could easily satisfie my self, by inverting the glass, and observing, that the ice had so exactly choak'd

choak'd up and stopt the neck, that no drop of water could get out, nor any bubble of Air get in, and yet the Fishes had liberty enough to play in the subjacent water. The *other* conveniency was, that, the frigorifick mixture being appli'd to the neck, no water was congeal'd, or extremely refrigerated, but that which was contain'd in the neck, so that there seem'd no cause to suspect, that in case the Fishes, thus debarr'd of Air, should not be able to live in the water, it was rather Cold, then want of Air that kill'd them. But though not having then been able, by reason of a remove, to prosecute these Trials to the utmost, nor to register all the circumstances, I shall not lay much weight upon it, yet I remember, that the included Fishes continued long enough alive, to make me shrowdly suspect the Truth of the vulgar Tradition.

Another time being destitute of the conveniency of such glasses, I caus'd some of the same kind of Fishes to be put into a broad and flat earthen vessel, with not much more water, then
suffic'd

suffic'd perfectly to cover them, and having expos'd them all night to a very intense degree of cold, I found the next morning, that some hours after day, they were alive, and seem'd not to have been much prejudiced by the cold, or exclusion of Air. 'Tis true, that there was a very large moveable bubble under the ice, but that seem'd to have been generated by the Air, or some Analogous substance, emitted out of the Gills or bodies of the Fishes themselves: for, that the surface of the water was exactly frozen over (which does not in such Trials happen so often, as one would think) I found, by being able to hold the vessel quite inverted, without losing one drop of water. And that this large bubble might possibly proceed from the Fishes themselves, I was induc'd to suspect, because having at different seasons of the year, for divers purposes kept several sorts of Fishes, and particularly Gudgeons, for many days in glass vessels, to satisfy myself about some *Phænomena* I had a mind to observe, I have often by watch-

watching them, seen them lift up their mouths above the surface of the water, and seem to gape and take in Air, and afterwards let go under water out of their mouths and gills divers bubbles, which seem'd to be portions of the Air they had taken in, perhaps a little alter'd in their bodies. And particularly in Lampries (of which odd sort of Fishes I elsewhere make mention) I have with pleasure, both observ'd and shew'd to ingenious men, that being taken out of the water into the Air, and then held under water again, they very manifestly appear'd to squeez out, and that not without some force, at those several little holes, which are commonly mistaken for their eyes, numerous and conspicuous bubbles of Air, which they seem'd to have taken in at their mouths, if not also at those holes. But of these matters a fitter occasion may perhaps invite me to say more. To return now to our Gudgeons, I shall add, that to satisfy my self further, what cold and want of Air they may be brought to support, I expos'd a couple of them

them in a bason, to an exceeding bitter night, and though the next day I found the ice frozen in the vessel to a great thickness, and one of the Fishes frozen up in it, there remaining a little water unfrozen, the other Fish appear'd through the ice to move to and fro, and the ice being afterwards partly thaw'd, and partly broken, not only that Fish was found lively enough, but the other, which I alone judg'd not to be quite dead, though, when the ice was broke, it lay moveless, did in a few minutes so far recover, as to tow after it (if I may so speak) a good piece, into which his tail remain'd yet inserted; and though one of these, and some other Gudgeons, that had been already weakned by long keeping, were once more expos'd in the Bason to the frost, and suffer'd to lye there, till they were frozen up, yet the ice being broken, in which they were inclos'd, though their bodies were stiff and crooked, and seem'd to be stark dead, lying in the water with their bellies upwards, yet one of them quickly recovered, and the other

Tt

not

not very long after began to show manifest signs of life, though he could not in many hours after so far recover, as to swim with his back upwards. 'Tis true, that these Fishes did not long survive, but of that, two or three, not improbable reasons, might be given, if it were worth while to name here any other than this, that the ice, they had been frozen up in, or the violence that was offered them by the fragments of it, when it was broken, had wounded them, as was manifest enough by some hurts, that appear'd upon their bodies; yet some other Gudgeons were irrecoverably frozen to death, by being kept inclos'd in ice, during (if I misremember not the time) three days. And as for other Animals, I caus'd a couple of Frogs to be artificially frozen in a wide mouth'd glass, furnish'd with a convenient quantity of water, but though they seem'd at first inclos'd in ice, yet looking nearer, I found, that about each of them there remain'd a little turbid liquor unfrozen, as if it had been kept so by some expirations from their bodies.

dies. Wherefore causing either the same, or two others, (for I do not punctually remember that circumstance) to be carefully frozen, and for a considerable while, I found, that notwithstanding the ice, into which most part of the water was reduc'd, not only one of them before the ice was broken appear'd to be perfectly alive, but the other that was moveless and stiff, and lying with the belly upwards in a Bason of cold water, whereinto it was cast, did in a very few minutes begin to swim about in it. I should have made more Trials at least, if not also more satisfactory ones, if I could have had Fishes and vessels, and cold weather at command: But upon the whole matter, though the Tradition, we have been examining, may perhaps have some thing of truth in it, yet it seems to deserve to be further inquired into, both in reference to the truth of the matter of fact, *the death of Fishes in frozen Ponds and Rivers*, and in reference to the cause, whereto that effect is imputed.

I met with an odd passage in Cap-

Pag. 82.

tain James's voyage, which if it had been circumstantially enough set down, might prove of moment in reference to the weight of bodies frozen and unfrozen, and therefore though I would not build any thing on it, yet I shall not omit it. The ninth (says he) we hoisted out our Beer and Cydar, and made a Raft of it, fastning it to our Shore-Anchor. The Beer and Cydar sunck presently to the ground, which was nothing strange to us, for that any wood or pipe-staves, that had layen under the ice all Winter, would also sinck down so soon, as ever it was hear'd over board.

About the duration of ice I forgot, through hast, to add a relation of Capt. James, whereby it may appear, That though Wine abounds with very spirituous and nimble parts, whence it resists congelation far more then water, yet if even this liquor came once to be congeal'd, the ice made of it may be very durable. For he sets down in his Journal, that when he came to his Ship again, he found a But of Wine, that had been all the Winter in the upper deck,

to continue as yet all firm frozen, though *Pag. 47.*
it were then the moneth of May.

When I treated of the great proportion in some pieces of ice, that were aground, instead of taking notice of the great piece of ice mention'd by *Gerard de Veer*, to be 52. fathom deep, the passage that was to be transcrib'd, was this other, hard by, which contains two examples of towers of ice, where the extant part reach'd upwards more then half as much as the immersed part reach'd downwards. *We saw* (says he) *another great piece of ice not far from us, lying fast in the Sea, that was as sharp above, as if it had been a Tower, whereunto we rowed, and casting out our lead, we found that it lay 20. fathom fast on the ground under the water, and 12. fathom above the water.* — *We rowed to another piece of ice, and cast out our Lead, and found that it lay 18. fathom deep, fast on the ground under the water, and 10. fathom above the water.*

*Purchas
lib. 3. cap.
5. pag. 487*

That snow lying long, and too long on the ground, does much conduce to the fertilizing of it, is a common obleruation of our Husbandmen.

And *Bartholinus* in his Treatise of the use of snow, brings several passages out of Authors to make it good : to which I shall add the testimony of our learned English Ambassador, Dr. *Fletcher*, who speaking of the fruitfulness of the soil, and hasty growth of many things in the great Empire of *Russia*, gives this account of it.

Purchas
lib. 3. cap.
1. pag.
415.

This fresh and speedy growth of the Spring there, seemeth to proceed from the benefit of the snow, which all the Winter time being spread over the whole Country, as a white robe, and keeping it warm from the rigour of the frost, in the Spring time (when the Sun waxeth warm, and dissolveth it into water) doth so thoroughly drench and soak the ground, that it is somewhat of a slight and sandy mold, and then shineth so hotly upon it again, that it draweth the herbs and plants forth in great plenty, and variety, in a very short time.

As we made some Trials to discover, whether congelation would destroy or considerably alter the odors of bodies, so we had the like curiosity in reference to divers other qualities,

ries, not only those that are reputed manifest, as colours and tastes, the latter of which we sometimes found to be notably chang'd for the worse in flesh congeal'd, but also those that are wont to be call'd *occult*, and among the qualities of this sort, I had particularly a mind to try, whether the purging faculty of Catharticks would be advanc'd or impair'd, or destroy'd by congelation, and for this purpose I caus'd to be expos'd thereunto divers purging liquors, some of a more benigne, and some of a brisker nature, and that in differing forms, as of syrup, decoction, infusion, &c. But for want of opportunity, to try upon the bodies of animals, what change the cold had made in the purging liquors, it had congeal'd, I was unable to give my self an account of the success of such Experiments; only since, in some of these Trials I had a care to make use of Cathartick liquors prepar'd by fermentation, (which way of preparing them, is it self a thing, I elsewhere take notice of, as not unworthy to be prosecuted.) I shall add on

this occasion, that fermentation is so noble and important a subject, that the influence of cold upon it may deserve a particular inquiry. And I am invited to think, that that influence may be very considerable, *partly* by my having observ'd (upon a Trial purposely made) both that Raisins and water, (with which I was us'd to make Artificial Wines) did not in many days, whilst the weather was very frosty, so much as manifestly begin to ferment, though the water were kept fluid; and *partly* by my having observ'd, that Beer will continue as it were new, and be kept from being, as they call it, ready to drink much longer then one would readily suspect, if very frosty weather supervene, before it have quite finished its fermentation, insomuch, that an experienc'd person, of whom I afterwards inquir'd about this matter, assur'd me, that Beer not duly ripe, would not sometimes in five or six weeks of very frosty weather, be brought to be as ripe as in one week of warm and friendly weather. But we have a nobler instance to our present

sent purpose, if that be true which I learn'd from an intelligent Frenchman, whom I consulted about this matter. For according to this experienc'd person, the way to keep Wine in the Must (in which state its sweetness makes it desir'd by many) is to take newly express'd juice of Grapes, and having turn'd it up before it begins to work, to let down the vessels (which ought to be very carefully clos'd) to the bottom of some deep Well or River, for six or eight weeks, during which time the liquor will be so well settled (if I may so speak) in the constitution, it has so long obtain'd, that afterwards it may be kept in almost the same state, and for divers moneths continue a sweet, and not yet fermented liquor, which some, in imitation of the French and Latins, call in one word, *Must*. And how by the help of Cold well appli'd, some other juices, that are wont to work early, and to be thereby soon spoil'd, may be long kept from working, the Reader may perchance learn in another Treatise, to which such matters more properly belong,

'Tis

'Tis known, that the Schools define cold by the property, they ascribe to it, of congregating both Heterogeneous and Homogeneous things. I thought it not amiss to attempt the making some separations in bodies by the force of Cold. For if that hold true in this climate, which has been observ'd by Travellers and Navigators in Northern Regions; that men may obtain from Beer and Wine a very strong spirit, and a phlegme by congelation, it seems probable, that in *divers* other liquors the waterish part will begin to freez before the more spirituous and saline, and if so, we may be assisted to make divers separations, as well by cold, as by heat, and dephlegme, if I may so speak, some liquors, as well by congelation as by distillation: but I doubt, whether the ordinary frosts of this Countrey can produce a degree of cold great enough to make such divisions and separations in bodies, as have been observ'd in the more Northern Climates. For though having purposely hung out a glass-bottle with a quart of Beer in it,

in

in an extraordinarily sharp night, I found the next morning, that much the greatest part of the Beer being turn'd into ice, there remain'd somewhat nearer the middle, but nearer the bottom, an uncongeal'd liquor, which to me and others seem'd stronger then the Beer, and was at least manifestly stronger then the thaw'd ice, which made but a spiritless, and, as it were, but a dead drink; yet in some other Trials my success was not so considerable as some would have expected. For having put one part of high rectifi'd spirit of Wine, to about five or six parts, if I misremember not, of common water, and having put them into a round glass, and plac'd that in beaten ice and salt, though the mixture were in great part turn'd into ice; yet I could not perceive, that even two liquors so slightly mingled, were any thing accurately severed from one another, although once, to enable my self the better to judge of it, the spirit of Wine I imploy'd was beforehand deeply tinted with Cochinele, and therefore I the less wonder,

der, that in Claret Wine I could not make any exact separation of the red and the colourless parts: However I thought it not amiss to try, how far in some other liquors this way of separating the waterish, and more easily congealable part from the rest, would or would not succeed. And I remember, that a large glass vessel, wherein spirit of Vinegre was exposed to the cold, a considerable part was turned into ice, whose swimming argued it to be lighter then the rest of the liquor: but though I put some of this ice in a glass by it self, to examine by its weight and taste, when thaw'd, how much it differ'd from the uncongeal'd part of the spirit, my hopes were disappointed by a misfortune, which was not repaired by my exposing afterwards a smaller quantity of spirit of Vinegre to the Nocturnal Air, for that proved so cold, that the whole was turned into ice, wherefore I must reserve for another opportunity the prosecuting that Experiment, as also the trying, whether a separation of the Serous or the Oleaginous parts of Milk may

may be effected. For though once the frost seem'd to have promoted a separation of Creme, notwithstanding that heat also may do it, and though another time there seem'd to be another kind of divulsion of parts made by congelation; yet for want of leisure to prosecute such Trials, they prov'd not satisfactory, no more then did some attempts of the like nature, that I made upon blood by freezing it. But notwithstanding these discouragements, I resolv'd to try, what I could do upon Brine. For calling to mind the Relations mentioned in the XV. Title, and elsewhere, which seem to argue, that in some cases the ice of the Sea-water may, being thaw'd, yield fresh water, and being the more inclin'd to think it worth Trial, by a Physician, I since happened to discourse with about this matter, who affirm'd to me, that sailing along the coast of *Germany*, he had taken out of the Sea ice, that being thaw'd, he found to afford good fresh water, I began to consider, whether we might not by cold, free salt water at some seasons of the year,
from

from a great deal of the phlegme, which 'tis wont to cost much to free them from by fire, and other means. For a little help towards the diminution of the fresh water, is look'd upon as so useful an Experiment, by many that boil salt out of the salt springs, that in some Countries, that are thought the skilfullest in that trade, they make their salt-water fall upon great bundles of small brush-wood, that being thereby divided, and reduc'd to a far greater superficies, there may, in falling through, some of the purely Aqueous parts exhale away; wherefore dissolving one part of common salt in 44. times its weight of common water, that it might be reduc'd, either exactly, or near, to the degree of saltness, that has been by several writers observed in the water of our neighbouring Seas, and having likewise caus'd another and much stronger Brine to be made, by putting in to the water a far greater proportion of salt, (for so there is in many of our salt springs) we expos'd these several solutions to the congealing cold of the Air in frosty

sty weather, where the last menti-
 on'd solution being too strongly im-
 pregnated with the salt, continued
 some days and nights altogether un-
 congeal'd ; but that weaker solution,
 which emulated Sea water, being ex-
 pos'd in a shallow and wide mouth'd
 vessel (that shape being judg'd the
 most proper we could procure for
 our design) the large superficies,
 that was expos'd to the Air, did, as
 we expected, afford us a cake of ice,
 which being taken off, and the rest of
 the liquor expos'd again to the Air in
 the same vessel, we obtain'd a second
 cake of ice, and taking the remain-
 ing, which seem'd to be indispos'd
 enough to congelation, we found,
 that by comparing it with that,
 which was afforded us by the first
 cake of ice permitted to thaw, there
 appear'd a very manifest difference
 betwixt the water, whereinto the ice
 was resolv'd, scarce tasting so much as
 brackish, whereas the liquor, that had
 continued uncongeal'd, was considera-
 bly salt in taste. And if I had had the
 conveniency of examining my self
 these two liquors *Hydrostatically*, as I
 was

was fain to have them examin'd by another, I doubt not but by their weight, I should have discovered precisely enough the difference between them (which the person I employ'd found to be *considerable*) and consequently should have been assisted to make an estimate of the advantage, that might be afforded by the operation of the cold towards the freezing of the Brine from its superfluous water. But though I had not a quantity of ice great enough to satisfy me, whether that little brackishness of taste, I have mention'd, proceeded from some saline Corpuscles, that concurr'd to the constituting of the ice it self, or did only adhere to the lower part of it, among other particles of the liquor, that remain'd uncongeal'd, yet perhaps 'twere not amiss to try, whether in very large, though not deep vessels, this Experiment, especially promoted by some expedients, that practise may suggest, may not in some seasons and places, be brought to be of some advantage.

Whilest I was endeavouring by
some

some of the above recited Experiments, to make some separations in liquors by congelation, I thought fit to try by the same means, what separations I could make in some bodies, betwixt liquors, and those more stable parts among which they were engag'd, hoping, upon considerations, which 'twere too long to enumerate, that, if such attempts should succeed, they might afford hints of a *Luciferous* nature. I took then divers vegetable substances of differing kinds, as Turnips, Carrets, Beets, Apples, and tender wood, freshly cut off from growing trees, as also divers Animal substances, as Musculous flesh, Livers, Brains, Eyes, Tongues, and other parts, and expos'd them to a very sharp cold, that they might be thoroughly frozen. Now one of the chief things, that I propos'd to myself in this attempt, was, to try how far I could by congelation make discovery of any thing about the Texture of Animals and Plants, that had not been taken notice of by *Anatomists* themselves, and would scarce otherwise be render'd visible. And

I easily found, that I had not ground-
 lessly imagin'd, that in divers Succu-
 lent bodies, both vegetable and ani-
 mal, the sap or the juice, that was
 so dispers'd among the other parts,
 and divided into such minute porti-
 ons, as not to be manifestly enough
 discriminated, might by congelation
 be both discern'd and separated from
 the rest. For in divers Plants, I
 found the Alimental juice to be con-
 geal'd into vast multitudes of distinct
 Corpuscles of ice; some of which,
 when the bodies were tranversly cut
 with a sharp knife, and left a while
 in the Air, might be wip'd or scrap'd
 off from the superficies of the body,
 upon which 'twould after a while ap-
 pear in the form of an Efflorescence,
 almost like meal: but in others I
 took a better and quicker course, for
 by warily compressing the frozen bo-
 dies, I could presently make the icy
 Corpuscles start in vast numbers out
 of their little holes, and though some
 of these were so minute as to invite
 me to use a *Microscope*, that magni-
 fi'd a little, (not having then any of
 my best at hand) yet in some bodies,
 and

and especially in Carrets and Beets, the icy Corpuscles were big enough to be distinctly or apart conspicuous, insomuch, that I was not mistaken in hoping, that the *figures*, as well as *sizes* (for as to the *Colour* it was scarce discernible in the ice, produc'd in so deeply crimson a Root, as the Beet it self) of these little pieces of ice, might be guess'd at by the bigness and shape of the Pores, that were left in the more stable part, or (if I may so call it) the *Parenchyma* of the root, though in making an estimate of these Cavities, as well as in discovering the order, wherein they are rang'd, I found it useful to cut the frozen roots, sometimes according to their length, and sometimes quite cross. For by that means there would appear in Carrets, for example, of the larger sort, a great disparity in the order of the Pores, which, when the root was divided by a plain parallel to the *Basis*, appear'd plac'd in lines almost streight, tending almost like the spoaks of a wheel, from the middle to the circumference. But if the Carret were slit from one end to

*Of the use-
fulness of
Experi-
mental
Philosophy.*

wards the other, the icy Corpuscles and pores would seem rang'd in an order, that would appear very differing, but which I have not now the leisure to describe, no more then what I observed with a Microscope, about the ice and pores of Apples, the Tongues of Animals, Chips of green and sappy wood, &c. expos'd to congelation: only this I shall not pretermitt, That as I many years since made (and, as I now find, too freely communicated) an Experiment, (mention'd long after in other papers) of freezing the eyes of Oxen, and other Animals, whereby the soft and the fluid humors of that admirable organ may be so hardned, as to become tractable, even to unskilful Dissectors: So I did on this occasion apply that Experiment to the brains of Animals, which though too soft to be easily dissected, especially by those that are not dexterous, may by congelation be made very manageable by them: And besides, that in dissecting the hardned brain, it sometimes seem'd, that the knife did cut through multitudes of icy Corpuscles,

pufcles (as when one cuts a frozen Apple) the fubftance of the brain feem'd alfo to the eye to be ftuffed with them, and the Ventricles of it did at leaft conspicuously *harbour* pieces of ice, if it were not *fill'd* up with them ; and the *manifeft* difference of Texture, that there is between the white and yelk of a througly frozen Egg, and alfo betwixt the Cryftalline and the Aqueous, and the Vitreous humors of the eye, wherein by congelation the Cryftalline alone lofes its transparency, but acquires no conspicuous ice, whileft the others are full of ice, and that diaphanous, thefe and fuch like difparities, I fay, may invite one to hope, that fome things may by congealing of bodies, be difcovered about their Texture, that may afford fagacious *Anatomifts* improvable hints.

I know not, whether it will be thought worth while to take notice, That neither an Eye, nor a Liver, nor a lean piece of flefh, nor a live Fifh, nor a living Frog, being frozen and put into cold water, was obferv'd to be upon its thawing cas'd with ice,

as frozen Eggs and Apples are wont to be : because having forgot to make the Experiment above once, I dare not much rely on it ; but whereas we have formerly observ'd, that congelation does most commonly spoil, or at least impair Eggs, and Apples, and Flesh, and many other bodies, I think it may not be unworthy to be consider'd, how far, and in what cases we may give a Mechanical account of this *Phenomenon*. For though the immersion of frozen bodies in cold water be allowed to thaw them, with less prejudice, than if they were thaw'd hastily by the fire, or suffer'd to thaw themselves in the Air : yet there have been complaints made ; That notwithstanding this expedient, several bodies have been much the worse for having been thoroughly frozen, now since I have lately shown, that in many stable bodies, the Alimental juice is by congelation turn'd into ice, and have formerly evinc'd, that water and aqueous liquors are expanded by congelation, I see not why we may not suspect, that the innumerable icy

Cor-

Corpuscles, into which the Aliment-
 tal juice is turn'd by the frost, being
 each of them expanded proportiona-
 bly to their respective bignesses, may
 not only prejudice the whole, by ha-
 ving their own constitution impair'd,
 as has been formerly observ'd in Ali-
 gant, and other Vinous liquors, but
 may upon their expansion crush in
 some places, and distend in others,
 the more stable parts, in whose Cavi-
 ties they were harbour'd, and there-
 by so vitiate their Texture, as to im-
 pair some of their qualities, and dis-
 pose the *Compositum* to corruption.
 How much Contusion may prejudice
 tender bodies, and accelerate putre-
 faction, is evident in many fruits,
 especially the more tender ones,
 which having been bruised, quickly
 begin to rot in those parts, that have
 been injur'd. And 'tis agreeable to
 what has been formerly shown, to
 conceive, that in congelation there
 seems to happen an almost innume-
 rable multitude of little contusions,
 made by the fluid parts harden'd and
 expanded by frost, of the formerly
 more stable parts every where inter-

cepted between them : And though these icy Corpuscles be but small, yet the sides of that stable matter, that separates them, and which they endeavour to stretch or crush, are oftentimes proportionably thin.

And we have formerly noted, That, besides that Eggs will be burst by having their Alimental juice frozen, both shingles and stones themselves may have their Texture spoil'd by the congelation of the Mineral sap, that is in exceeding minute and insensible particles dispers'd through those bodies : and the violation of the Texture of Plants, Herbs, and Animals, by the expansion of the aqueous and juicy particles, which, though they be not congregated, do abound in them, will be the less wonder'd at, if it be remember'd, that our former Trials manifest, that a few ounces of water congeal'd, did not only burst Glass and Pewter vessels, but even the Iron barrel of a Gun.

Whilest I was upon these Trials, I had also a curiosity to know, whether by freezing Animals to death, I could discover any such change in the
qualis

qualities or structure of their parts,
 as might help us to discover, by what
 means it is, that excessive colds kill
 men in Northern Countries, since
 such a discovery might probably be
 of good use to the People that live in
 those gelid Regions: But having ta-
 ken a young Rabbet, as the tenderest
 and fittest beast, I could then procure
 for such a Trial, and having expos'd
 him all night to an extraordinarily
 bitter frost, without finding him
 otherwise mischiefed by it, then that
 one of his legs was swell'd and grown
 stiff, I was more inclin'd to resign
 over to others, then to repeat my
 self what seem'd to be an ill natur'd
 Experiment, though perhaps it may
 have much less of cruelty, then one
 would think, since some of our for-
 mer observations have made it pro-
 bable, that oftentimes the extinction
 of life by cold is a more indolent kind
 of death, then almost any other.
 But in a Rabbet purposely strangled,
 and presently expos'd intire to a bit-
 ter cold, we found ice produc'd in
 such parts, as would have made us
 prosecute the Trial, had the want of
 such

such Animals and of leisure not hinder'd us.

It is affirm'd by divers eminent writers, and those modern ones too, that water impregnated with the saline parts of Plants, and afterwards frozen, will exhibit in the ice, the shape of the same Plant: And the learned, but I fear, too credulous, *Gaffarel* tells us, that this is no Rarity, being daily shewn by one *Monsieur de la Clave*. But to what we have already publish'd in another Treatise, * to shew, that this Experiment as it is wont to be deliver'd, is either *untrue*, or very *contingent*; we shall need but to add, that, since the Experiments there mention'd, we did again lately try, what could be done with Decoctions, that were richly imbu'd, and highly ting'd with the spirituous parts of the Vegetables; but this ice was by no means so figur'd as the Patrons of the Tradition promise: And I remember, that having also made, for curiosity sake, a *Lixivium* with 16. parts of water, and but one of salt of Potashes, that the mixture might be sure to freez, and

* Of the
Unsuccess-
fulness of
Experi-
ments.

and having expos'd the liquor in a thin glass vial to an exceeding cold Air, we found the copious ice produc'd, to lye on the top in little sticks, not unlike those Prismatical bodies, wherein Salt-petre is wont to roch, and those parts of this ice, that were beneath the water, were shot in thin parallel plates, exceeding numerous, but (as one of our notes expressly informs us) no way in the shape of Trees, by whose Incineration ne vertheless *Polonian* Potashes, (as eye witnesses, that deal in them, inform me) are made.

Long after the making of the newly recited Experiment, I chanc'd to find, that the learned *Bartholinus* in the Treatise, we have often had occasion to take notice of, says, That the water, wherein

Cabbage has been decocted, will, when frozen, represent a

Cabbage, the

vegetable spirits being, as he supposes, concentrated by the cold. How well this Experiment may succeed,

when

*Rem vero adeo obscuram exemplis
similibus illustrabo Brassica: aqua con-
gelata Brassicam representat, spiriti-
bus vegetalibus à frigore concentratis.
Tho. Bartholinus de usu Nivis, pag.
17.*

when made in a cold Countrey like his, I do not know : but not having my self, when I first took notice of it, the opportunity to try it satisfactorily by help of a frosty night, all I could do, was, to take a good decoction of Cabbage, and filtrate it through Cap paper, that it might be, though yellow, yet clear, and then by the circumposition of our frigorifick mixture, we froze this liquor in a thin glass vial, but the ice did not, either to me or others, appear to have any thing in it like a Cabbage, or remarkably differing from other ice. And being afterwards befriended with two or three frosty nights, we expos'd a decoction of Cabbage, to be congeal'd by the Nocturnal Air alone, without the help of Art ; but neither this way did the Experiment succeed well. And though once a few ounces of the decoction being lightly frozen in a vial, there appeared in the thin ice, that adher'd to the inside of the glass, a figure not so very unlike that of a Cabbage leaf, but that some such accident may have invited our learned Author to think,
that

that the representations of Cabbages would constantly appear in their frozen decoctions, yet I was inclin'd to think this figuration rather casual, by the curiosity I have had to freez the decoctions of several Herbs, some of them spirituous enough, as Rosemary, and Penny-royal, without being able to find in the ice, I obtain'd from them, any conviction of the truth of the Tradition we are examining. And I have lately had more then once, by freezing fair water alone, after a certain manner, ice, that seem'd much more to exhibite the shapes of vegetables, then any decoctions of them, that I have made. And particularly I found more then once, that by putting hot water into a somewhat slender Cylinder of glass, and agitating it in a frigorifick mixture, consisting of beaten ice, salt, and water, so that it was very speedily frozen thereby, it was congeal'd into an ice much more regularly and prettily figur'd, then I have seen it in divers of the waters impregnated with the fix'd salts of Plants, though of these we are told such wonders.

Such

Such particulars as these joyn'd with what I have elsewhere observ'd to the same purpose, make me, I confels, somewhat surpriz'd to meet in *Berigardus's* forecited discourse upon *Aristotles* *Meteors*, such a passage as *Pag. 573.* this; *Paucis notum est, cur intra glaciem cernuntur interdum multiformes stirpium imagines in Ampullis vitreis, aquae superficie tenus congelatae plenae. Hoc autem fit injecto in Phialas sale diversarum stirpium, nam ubi erit sal alicujus plantae & Artemisiae, in suo Lixivio glacies adhærens vitro, refert ejus folia laciniosa: similiter in alia Phiala videbuntur folia Plantae, cujus sal in suam aquam fuerit injectus. Et nè quis hoc fortuito cadere putet, in aquâ sæpius solutâ, & congelatâ eadem imagines semper occurrent, ut vel ex eo dixeris multiplicem spiritum salis in principiis naturalibus esse ponendum.* Thus far this Author, who would have done well, if he had been so much more lucky, then other men, as to have performed these things, to assure expressly of his having done so, those many ingenious men, that much distrust the relations of those Chymists, that are not of the

the best sort : and 'tis of such suspicious Authors, that I here declare once for all, that I would have the Reader understand all the passages of this Book, wherein I may seem to say any thing (for avoiding of tediousness) *indefinitely* to the disparagement of Chymists. And in case he had not tri'd them, he should, in gratitude to the Authors of them, have told us, he had, what he delivers of them, but from others, and not have authoriz'd the untri'd reports of writers, not always too veracious by his building Theories upon them. And as for what he immediately subjoyns, and seems to rely on, out of *Quercitan*, (and other *Spagyricall* writers, who possibly had it themselves from *him*) about the seminal virtues surviving in the Ashes of burnt plants ; though I will not here examine, or absolutely reject the *opinion*, because the discussion of it belongs to another place, yet as to the Experiment whereon *Berigardus* and others relye, namely, that the *Lixiviums* made of the Ashes of Plants, will exhibite, being congealed, the
figures

figures of the pristine vegetables ; besides that a general conclusion, as to other Plants, seems to be inferred from what happened in *Netles* only, I much doubt, whether that famous Experiment it self of the frozen *Lixivium* of *Netles*, were more then casual, if it were not also assisted by an indulgent phansie. For having, after divers Experiments made with other fixed salts, purposely repair'd, for greater security, to the notedst Chymist in *England*, to obtain from him some fixed salts, very faithfully prepared, and intimating withal, that 'twas to try such an Experiment (which he was a favourer of) I did by mingling these salts each in a distinct vial, sometimes with one, and sometimes with another proportion of water, and afterwards exposing them to the cold Air, obtain indeed divers portions of ice, but without any such figurations, as the learned *Berigardus* would have expected; though some of these Trials were made more then once, as well with the *Lixivium* of *Netles*, as with the Lees of other Plants: so that I doubt

doubt this Author is more scrupulous in admitting some important truths, in which the best Philosophers, as well Heathen as Christian, agree, then in examining the uncertain Traditions of the Chymists, whose unsatisfactory way of setting down matters of fact, I am induc'd to take notice of his imitating, by finding, that in the very same page (that I have newly cited) he relates another Chymical Experiment in these terms, *Velim porro ostendere mirabili Experienciâ, quàm sint penetrabiles aliqui spiritus Corporei: Exarentur in charta literæ, aceto albo, quarum nullum vestigium deprehendatur, claudaturque primis foliis Crassissimi alicujus Libri. Paretur alia Charta, quæ inficiatur aqua illa fætida, ubi dilutum fuerit Auripigmentum, & exsiccata claudatur postremis foliis ejusdem Libri leviter compressi, statim videbis in priori charta literas conspicuas, perinde ac si atramento ductæ fuissent.* Now, though some thing like what is here propos'd to be done, may be perform'd, and other Phenomena of the Experiment, such as he seems not to have been acquainted

* In the
unpublish-
ed Section
of the use-
fulness
of Experi-
mental
Philosophy.

with, may be also exhibited, after the manner I have * elsewhere particularly set down, yet he must have good luck, that performs it only by the directions here given by our Author, who by omitting one of the chief ingredients, and some requisite circumstances, appears indeed manifestly enough to have *heard* of such an Experiment, but without seeming to have sufficiently *known*, what he pretends to teach (at least as far as his bringing this Experiment as a proof, and the obscure style, he is wont to imploy in the little I have yet read of his Book, permits me to judge.)

Of the un-
successful-
ness of Ex-
periments.

But to return to the figurations of ice, notwithstanding such unsuccessful Trials about them, as I have been mentioning, I will not deny it to be possible, that a prepossessed and favourable spectator may think himself to have discern'd in the ice, the figures he look'd for there. For since the writing of the Essay not long since quoted, we have found, that several Bodies, and even Sea-salt, and Allume, to whom Nature has
given

given their own determinate figures, have, when dissolv'd in water, concurr'd with it to exhibite an ice very oddly, as well as prettily figur'd (nor will I presume to determine the utmost, that a lucky observer may *sometimes* meet with in this kind) but to name at present no other Arguments, the figures this way produc'd, were too various and extravagant not to be referred to chance, and not to afford instances how much That can perform in the exhibiting of such Aparitions.

On which occasion I shall add, that I remember, I once show'd at the *Royal Society*, a glass head, whose inside was lin'd with a certain substance, that pass'd for saline, fashioned into the figures of Trees, as curious, as if they had been drawn by a Limner; and yet as I produc'd these figures only by rectifying common oyl of Turpentine, from Sea-salt (which salt I elsewhere shew not to be *necessary*) in a certain degree of heat: so by varying that degree of heat, I could make the ascending steams settle in other figures; and I

can easily produce very pretty shapes of Trees, by distillation of that, which belongs not to the *vegetable*, but the *animal* Kingdom. And to these I could add divers other instances of the like tendency, to make it still the more probable, that though oftentimes one may happen to find pretty *Ideas's*, or Apparitions in ice; yet the like, or as fine, may be produc'd by chance. And I have sometimes obtained by freezing Infusions, Decoctions, Spirits, Solutions, and other Liquors, as Vinegre (and particularly) Milk, and even common water, figures, that were so pretty, but withal so unconstantly produc'd, and so easily variable by circumstances, that as it would fill a Book particularly to describe them (which for that reason I hope to be excus'd for declining) so they would much delude him, that should expect to find them *every time* the same, that he had found them *once*.

And to intimate That by the by, to make several Trials in a short time, and thereby produce variety of figures, 'tis not an ill expedient to expose

pose the liquor, one would have congeal'd, in very shallow vessels, or if it be put into other vessels, to leave it, but of very little depth. And if the vessel it self be highly refrigerated, either by the cold Air, or by having salt and ice applied to the outside of it, the congelation may succeed much the more nimbly; so that within a short while the same liquor, being divers times thaw'd and frozen again, may possibly exhibit variety of figures. And the production of ice may be also much accelerated, by dipping into the liquor, one would have congealed, the convex surface of some glass or other smooth body, that will not imbibe water; for thereby the depth of the liquor will be exceedingly extenuated, and how much such a thinness or want of depth, may dispose a liquor to be speedily penetrated and congealed by the cold, may be guessed, by what is above delivered in the Section out of *Olearius*, of the way of multiplying ice in *Persia*, by making water thinly diffuse it self over a plate of ice, or some other aptly figured,

and very cold body : In confirmati-
on whereof I will add on this occasi-
on, that I have seen a pair of Stairs,
on which, though they were situated
near to three Chimneys, commonly
furnished with fire, almost all the
day long, the water that was im-
ployed to wash them, being thinly
spread with a Mop, would presently
congeal (though they assur'd me it
was hot, when 'twas begun to be
laid on) and cover the Stairs with
glossy filmes of ice. And I have
likewise observed in a very sharp
night, that the water which dropp'd
down from the nose of a Pump, was
so well congealed, as 'twas sliding
away, that the ice thus arrested in
its passage (in which 'twill easily
be granted that it spreads it self very
thinly) had rais'd a kind of icy pyra-
mid of a considerable bigness and
height.

I forgot to mention in due places
(and therefore think fit to take notice
of it here) that when I was conside-
ring of the ways, whereby it might
be manifested, to those that want nice
scales, or distrust their skill to use
them,

them, whence that ice comes, that appears on the outside of frozen Eggs put to thaw in cold water, I found it somewhat difficult to pitch upon such a liquor as I desir'd. For if common water be the liquor imploy'd, it may be said, that it affords the matter, whereof the ice in question is made: and if I imploy'd liquors, that were spirituous or saline, it might be pretended, that the frost (as they speak) did indeed come out of the frozen Egg, though the shell did not appear cas'd with ice, because as fast as the frost came to the outside of the Egg, it was resolv'd by the spirituous or saline Corpuscles of the liquor: wherefore as an expedient, I resolv'd to make use of oyl of Turpentine, as a liquor, which I had found incongealable by the greatest cold, I had observed in our Climate, and which yet (as may appear by the third Paragraph of the XVI. Title) was more indispos'd, then common water it self, to thaw any icy Efflorescence, that might be emitted by the Egg. But the Experiment was tri'd, without uniformity in the successes.

For the first time I put a frozen Egg into oyl of Turpentine, I did not observe, that any ice was produced on the outside : neither was the event differing, when another time I put two frozen Eggs together into a small vessel full of that oyl, though to refrigerate the liquor, the vessel was for a while placed upon a mixture of salt and ice, and though also the Eggshells at their gaping cracks (produced by congelation) discovered, that the contained liquor was well frozen. I intended to prosecute the Experiment another time (wanting ice to do it then) because that once, when during the Trial I was hindered from watching it, one of my Domesticks, whom I ordered to look after it, assured me, that the Egg, that was put to thaw in the oyl of Turpentine, had there obtained ice on the outside of it, which I should readily have believed, upon the score of a like observation, I had made myself, in two Eggs that were frozen to the bottom of the vessel, wherein they had been put to thaw, were it not, that one or both of them had been, by a mistake, dipt
in

in water, before they were put into the above mentioned oyl.

Some Readers may have expected to find among the examples recited of the influence of cold upon the Air, that strange story, which is related by the learned *Josephus Acosta*, of the mountains of *Pariacaca* (which he several times tra-

versed) * but besides that I have delivered a great part of it already

**Where a wonderfully piercing, though not sensibly violent cold, does sometimes suddenly kill men, and yet preserve their Bodies untainted whole years together.*

in another Treatise, I was loath to say more, till I had leisure (which I have not now) to discuss the scruples, that I have, not so much about the matter of fact, as about the cause, which perhaps may be something besides cold. But since I have mention'd this XVIII. Section, I will here take notice of what I then intended, but forgot to set down, namely, That to the instances alledged to show, the coldness of regions not to be always proportionate to their greater and less vicinity to the Pole, we may add a memorable one afforded us by a Countrey so well known

known to many of us, as *New England*, where, though the Winters are so long and bitter, as we have formerly related out of Mr. *Woods's* Prospect of that Countrey (which has been confirm'd to me by an *American* Physician, that liv'd there) yet that Region, which is so very much colder then ours, is in many places no less then a 10. or 11. degrees remoter from the Pole.

I shall add to the same XVIII. Section, that as to the Experiment I there mention'd concerning Winds, and which I associate with the testimony of the newly named Mr. *Wood*; I find that the season of the year, and some other circumstances may vary it more, then one would easily have suspected. For though I faithfully recited the *Phænomena*, as I then (and that sometimes with witnesses) took notice of them, yet some moneths after, and in other weather, having occasion to repeat the former part of that Experiment, I was somewhat surpriz'd at the success. For coming to blow upon the Ball of a seal'd Weather-glass, which though in its kind

kind very tender, might be probably presumed to be less so, then a Thermoscope made with a pendulous drop of water (such as that, mention'd in the forecited Paragraph) I found, that if I continued to blow any thing long and briskly, the highly rectified spirit of Wine (which circumstance I therefore name, because possibly the nature of That may somewhat alter the case) would *sometimes* manifestly enough subside. And in that Paragraph of the 18. Title, where I recited the Experiment of the infrigidating Winds, I should more expressly have taken notice of this circumstance, that, to satisfy myself, that 'twas not the bare Wind, as such, whose operation upon the Air included in the Ball of a Weather-glass, made the liquor to ascend, we put a mark upon the height it stood at, when we had a pretty while blown upon it, and then without removing the Bellows, put ice and salt about the Iron pipe of it. By which mixture the Air, that was afterwards blown through that pipe, was so cool'd in its passage, as to
make

make the liquor very manifestly to ascend, even in a Weather-glass, where I did imploy (as I have else where declared, that I often do) Quicksilver instead of water, or spirit of Wine. And least the vicinity of the frigorifick mixture should be suspected to have caus'd this contraction of the included Air, we did sometimes purposely intermit the moving of the Bellows, without removing the Weather-glass; and though notwithstanding that vicinity, the liquor would begin a little to subside; yet when ever the cold spirits or the Corpuscles of the highly refrigerated Air, were by the playing of the Bellows anew, approach'd to, or rather brought to touch in swarms the globular part of the instrument, the *Mercury* would manifestly ascend. And since we are speaking of Weather-glasses, I shall on this occasion subjoyn, That certain circumstances may also vary the success of another Experiment (somewhat of kin to that lately repeated, about the pendulous Drop) which is briefly mentioned not far from the beginning of the

the first Præliminary Discourse. For though the common Thermometers, that are here wont to be sold in shops, have usually the *Pipe* of the Bolthead very *large* in proportion to the *Ball*, and therefore are in that place said to be *Weather-glasses not nice*, and though on such instruments in certain Temperatures of the Air (intimated by the word *sometimes*, imploy'd in that passage) the Air blown out of a pair of Bellows against some part of the included Air, would not, especially at the beginning, make the Air sensibly contract it self, and the liquor ascend; though at the very first and second blast, the coldness of this artificial Wind, might be very sensible to the touch (*which was the thing intended to be taught in that passage*) yet having the curiosity with other Bellows, at another season of the year, to blow long upon the Ball of a not *common*, but *nice* Weather-glass of my own making, furnished with a pipe, that was very slender, I divers times (but not always) found the tinged liquor manifestly enough to ascend, as if
the

the Wind, consisting of a more compressed Air, did by containing a greater number of cold particles in the same room, more affect the internal Air, then the contact of the calm and lax outward Air did before; which disparity of events has given me the design of making further Trials with differing Thermoscopes, at other seasons of the year, to see if I can bring the matter to some certainty, by discovering the cause of this contingency, in which I afterwards suspected, that some light degree of warmth or coolness in the Bellows themselves, which, as being unmanifest to the sense, escap'd unheeded, might have an interest. When I was about some of the former Experiments, I would willingly have had an opportunity of trying, with a good seal'd Weather-glass, what difference there would be, betwixt the cold of the nocturnal Air in a frosty night, in places where the Air was kept calm, by being shelter'd from the wind, not by inhabited buildings, but by some Wall, or other body, whence any warm *Efflu-*

viuum were least to be expected, and betwixt the cold of the same Air, in places where cold winds, especially Northerly or Easterly did freely and strongly blow. But my occasions then confining me to a Town, I had not conveniency to make any secure observations of that nature; and even in a more commodious place, unless it were determined, whether there be Corpuscles properly and constantly frigorisick, upon whose account some winds are so much colder than others, there may arise more scruples about this matter, then I must now stay to discuss.

There is one thing more, that, it may be, is not impertinent to mention, before I take leave of the XVIII. Title; for in confirmation of what is there delivered, concerning the Vicissitudes of these troublesome degrees of cold and heat, within the the compass of the same Natural day, complain'd of by the Patriarch *Jacob*, and by *Olearius*, I shall add, that having since had opportunity to inquire about such matters, of a learned Physician, lately come from the *Indies*,

Unable to display this page

compose. To produce such a mixture more ways then one, it was not difficult for me, by the help of some Experiments, I had provided to add to my *History of fluidity and firmness*. But though a strong solution of *Minium* (or calcined Lead) in spirit of Vineger, or a very strong infusion of good quick-Lime in water, will either of them (and one of them I did make use of, though I have forgotten which) coagulate a just proportion of good Sallet Oyl (to name no other made by expression) into such a consistence as I have been speaking of; yet for want of a seal'd *Thermoscope*, tender enough, I cannot now repeat the Experiment, and till I do, I dare not draw any conclusion from it, though, if I much misremember not, when I show'd it an ingenious person, neither he nor I could perceive, that the liquors, by being depriv'd of their fluidity, had acquir'd any thing of coldness discoverable by the seal'd Weather-glass.

It is much controverted among the Curious, whether water be capable of Compression, and divers have of

late inclin'd to the negative, upon observing a want of cogency in the Experiments, that have been brought to evince the affirmative. What Trials and Observations we long since made about this matter, may be met with in some of our other Treatises, wherefore I shall now subjoyn, that having imagin'd, that Cold might afford a hopefuller way, then (for ought I know) any man has us'd, of bringing this controversie to the decision of an Experiment, I made that attempt, that is mention'd in the XII. Title; in prosecution of which, as soon as I could procure some, though but some of the accommodations, which I long wanted; I made an Experiment, which I shall subjoyn, because, though it be not so considerable, as with better implements I could have made it, yet the way, I chose, has (as I partly intimated elsewhere) these two advantages; that the force imploy'd to compress the Air, is both very great, and very gradually, and slowly appli'd; and that the vessel will not, like those that have been hitherto made use of, give
any

any passage through its pores to water, though violently compress'd.

We took then a Round Ball of glass, furnish'd with a moderately long Pipe, and having fill'd it with water, till the liquor reach'd within some inches of the top, it was *Hermetically* seal'd up, and then the water by a mixture of beaten ice and salt, was made to freez from the bottom upwards, that without breaking the glass, the unfrozen water, by the expansive endeavour of that which was freezing, might be impell'd upwards, and so at once, both compress the Air, and be press'd upon by it, having by this means condens'd the Air, as far as we thought safe to do in a glass, that was not strong, we cropt off the small Apex of the glass, and immediately the compress'd Air flew out with a great noise, and that part of the Pipe which was unfill'd with water, was fill'd with smoak, that made it look white, and great store of little bubbles hastily ascend- ed from the lower parts of the water, to the upper (where most of them quickly broke) in such a way, as put

me in mind of what usually happens upon the opening of vessels that contain'd bottled Beer. But that which was principally to be noted, was this, that besides the bubbles or froth, the *water it self* (at least supposing, that no little unheeded bubbles that did not quite emerge, could sensibly contribute to its height) immediately ascended in the Pipe about $\frac{3}{4}$ of an inch, which (having carefully mark'd the first and second stations, with a Diamond on the outside of the glass) 'twas easie for us to measure.

I have elsewhere propos'd a suspicion, that in the attempts, that had been till then made, to compress water, the condensation (in case there were really any) might perchance proceed from the compression of the Aerial particles, that I have shown to be wont to ly dispers'd in the pores of common water. But though the considerable expansion of water, notwithstanding the breaking of the bubbles in our present Experiment, seems manifestly to argue, that this could be but a concurrent cause (if it had any sensible effect at all) of our *Phænomena*,
yet

yet I dare not absolutely *rely*, even upon an Experiment, that seems so cogent, till I have satisfi'd my self, that no springiness, which I have sometimes suspected, *might* be in the ice, had any interest in the produc'd effect; and that the great pressure of the forcibly condens'd Air, did not make the glass it self stretch or yield. For if it were able to do so, then the parts of the violently distended glass, upon the removal of the forcible pressure of the Air (which must cease upon the breaking of the *Hermetical* seal) returning to their former straitness below, will make the water ascend somewhat higher in the pipe. But though I could not procure glasses, as well very thick, as conveniently shaped, wherewith to examine this suspicion, which I would likewise have tri'd by the bulk of the glass in water, before and after the letting out of the compress'd Air; yet because most Readers will probably think so much caution more then necessary, I shall add, that if I had not wanted conveniencies, and had not had mischances, the Experiment would in likelihood

have been advanc'd, especially care being taken, that the Air left in the pipe should be well refrigerated before its being seal'd up (as we sometimes did by ice and salt, applied in a perforated Box to the outside) left part of its spring should depend upon an evanid degree of heat, upon which account the pipe ought beforehand to be drawn so slender, that the glass may be melted together in a trice. For though for want of strong glasses, & the best sort of instruments to seal up such with, the success was not still so considerable as I hop'd for; yet as 4. or 5. other Trials, made, as well with another liquor, as with water, did exhibit a manifest intumescence of the liquors (without computing the froth produc'd at the top ;) so in the Experiment lately mention'd, if we had judg'd them strong enough to indure such a compression of the included Air, as we have often made on other occasions, the effect would probably have been much more considerable: For though the difference betwixt the length of the same water compress'd and uncompress'd, amounted to an

Aqueous

Aqueous Cylinder of $\frac{2}{3}$ of an inch in height, yet the Air, that made this compression of the water, was it self reduc'd but from 8. inches to 5. so that it took up almost half its former room, whereas we have sometimes reduc'd it to an 18. or 20. part thereof. If I had been accommodated with one of my Pneumatical Engines, I should have tri'd, whether water being first carefully freed from the latent Air in the exhausted Receiver, and then compress'd after the manner hitherto recited, the event of the Trial would have been considerably varied.

I might add as other *Phænomena* of our Experiment, that when we broke off the seal'd Apex of the glals, before the included Air was much compress'd, there neither would be any great noise made, nor any considerable froth produc'd, at the top of the water, and that having had the curiosity to repeat the Experiment in one of the same glasses, that had been already us'd, and with the same water, that had been already compress'd in it, we found, that upon the break-

ing off the *Hermetical* seal the second time, the water did nevertheless ascend in the Pipe betwixt $\frac{1}{8}$ and $\frac{1}{4}$ part of an inch. And to these particulars I could both *add* other *circumstances*, that I took notice of in the same Experiment, and *subjoyn* many other *Experiments* and *Observations*, but that I am already tyr'd. And though I have not found Cold to be a subject over-fruitful in Experiments, Pleasing and Curious, yet now I am grown somewhat acquainted with it, I find it may suggest so many other new ones, that since the Barrenness of my Theme, will not easily put a period to this Treatise, 'tis fit that now at length I should let my Weariness and want of Leisure do it.

FINIS.



AN
ADVERTISEMENT.

T*Hat the Reader may not wonder to find the following Dialogue cited in the History of Cold, whereunto nevertheless it is subjoyn'd; he is to be inform'd, that a Section About Antiperistasis was really both written and transcrib'd before any part of that History was sent to the Press. But finding, that the Accession of new Particulars had so much swell'd it, that 'twas unfit to pass (as I first design'd it should) for one of the Titles of the History of Cold, I judg'd it convenient to sever it from the rest, upon the score of its bulk, and yet annex it to them upon the Account of those many Historical Passages in it, that belong to the same subject, that is handled in those Sections.*

The Reader will quickly find, that the Tract consists of two parts, whereof the first (which to allow the more Freedom of Inquiry and Discourse, written in the way of Dialogue,) contains an Examen of Antiperistasis, without pretending to question it absolutely and indefinitely, but rather, As it is wont to be Taught and Prov'd. And this Dialogue, for reasons, that it too little concerns the Reader to know, and would take up too much time to tell him, both begins as a Continuation of some former Discourse, and somewhere mentions the Author, as a Third or Absent Person. And to make it the likelier to other Dialogues, the Quotations are not made with the Author's punctualness in the rest of this Book, but yet with his usual Faithfulness; nor hath his Introducing men Discoursing (as it were by chance) kept him from putting into the Margent the very words of some Passages, which he thought the most important and likely to be distrusted. But though this first Part be entire and finish'd in its kind, and so might very well (if not best) have been
put

put forth Single, to invalidate the common Doctrine of Antiperistasis, (in the sense wherein 'tis there oppos'd ;) yet because in Philosophical Matters, 'tis not so much Victory or Applause, that is to be sought, as Truth ; I forbore not to subjoyn to a Discourse, that may perchance satisfie most of my Readers, some scruples about which I wish'd for further satisfaction and Certainty my self ; of the chiefest of which, the Sceptical Consideration will give the Reader an Account.

AN

the first thing, to establish the com-
mon doctrine of Antiphrastus (in the
first chapter of the 1st book) yet be-
cause in Philosophical matters, it is not
to be taken for granted, that it is to be
thought, as it is; I have not to be
taken for granted, that any person
can be made of any kind, for the
above which I wish to further state
on the Christian in the 1st of the 1st
of which, the Spectator's Consideration will
give the Reader an account.

AN



AN
Examen of Antiperistasis,
 AS
It is wont to be Taught and
Prov'd.

Themistius, Carneades, Eleutherius.

Themistius.

I. **A**S for *Antiperistasis*, the Truth of it is a thing so conspicuous, and so generally acknowledg'd, that I cannot imagine what should make some men deny it, except it be, that they find all others to confess it. For though in other cases they are wont to pretend Experience for their quitting the receiv'd Opinions, yet here they quit Experience it self for singularity, and chuse rather to depart from the Testimony of their senses,
 then

then not to depart from the Generality of Men.

2. And to evince, that this is not said *gratis*, I might observe to you, That there are no less then three grand inducements, that have lead both the Vulgar and Philosophers (two sorts of men, that seldom agree in other things) to consent in the acknowledgment of *Antiperistasis*; Authority, Reason, and Experience. But though I think fit to name them all three, yet since the first of them, by having, as I just now noted, invited our Adversaries to dissent from the Truth, is a somewhat unlikely *Medium* to prevail on them to acknowledge it, I shall insist only on the two latter, having once declar'd, that I lay aside the first not as worthless in it self, but needless to my cause.

3. To begin then with the Arguments afforded us by Reason.

What can there be more agreeable to the wisdom and goodness of Nature, who designing the Preservation of things, is wont to be careful of fitting them with requisites for that preservation; then to furnish
cold

cold and heat, with that self invigorating power, which each of them may put forth, when 'tis environ'd with its contrary. For the order of the universe requiring, that cold and heat should reside in those Bodies, that often happen to be mingled with one another, those two noble and necessary Qualities, would be too often destroy'd in the particular subjects that harboured them, if provident Nature had not so ordered the matter, that when a Body, wherein either of them resides, happens to be surrounded by other Bodies, wherein the contrary Quality is predominant, the besieg'd Quality by retreating to the innermost parts of that which it possesses, and there by recollecting its forces, and as it were, animating it self to a vigorous defence, is intended or increas'd in its degree, and so becomes able to resist an Adversary, that would otherwise easily destroy it.

4. To illustrate as well as supply this Argument drawn from Reason, we shall need but to subjoyn the other afforded us by Experience, which
does

does almost every day give us not only opportunity to observe, but cause to admire the effects of this self invigorating power, which, when occasionally exerted, we call *Antiperistasis*: And these *Phænomena* ought the more to be acquiesced in, because they may safely be looked upon as genuine *Declarations*, which Nature makes of Her own accord, and not as confessions extorted from Her by Artificial and compulsory Experiments, when being tortured by Instruments and Engines, as upon so many Racks, she is forced to seem to confess whatever the Tormentors please.

5. To proceed then to the spontaneous *Phænomena* of Nature I was recommending, we see, that whereas in Summer the lowest and highest Regions of the Air are made almost unsufferable to us by their heat, the cold expelled from the earth and water by the Suns scorching beams, retires to the middle Region of the Air, and there defends it self against the heat of the other two, though in the one that Quality be assisted by the almost perpendicular reflection of the Sun-

Sun-beams, and in the other it be rendered very considerable by the vastness of the upper Region of the Air, and its Vicinity to the Element of fire.

And as the cold maintains it self in the middle Region, by vertue of the intensness, which it acquires upon the account of *Antiperistasis*; so the Lightning that flashes out of the Clouds, is but a fire produc'd in that middle Region by the hot Exhalations penn'd up, and intended in point of heat by the ambient Cold, to a degree that amounts to ascension.

6. But though these be unquestionably the effects of that excessive coldness; yet we need not go so far as the tops of mountains to fetch proofs of our doctrine, since we may find them at the bottom of our Wells. For though *Carneades* perhaps *will not*, yet the earth as well as the Air *doth* readily acknowledge the power of *Antiperistasis*. And if the reason above alledged did not evince it, our very senses would. For as in Summer, when the Air about us is sultry hot, we find, to our great refreshment,

ment, that the Air in Cellars and Vaults, to which the cold then retreats, is eminent for the opposite Quality; so in Winter when the outward Air freezes the very Lakes and Rivers, where their surfaces are expos'd to it, the internal Air in Vaults and Cellars in Winter, which becomes the sanctuary of heat, as in Summer it was of Cold, is able not only to keep our Bodies from freezing, but to put them into sweats. And not only Wells and Springs upon the account of their resting in, or coming out of the deepest parts of the earth, continue fluid, whilst all the waters, that are contiguous to the Air, are by the excessive cold hardened into ice; but the water freshly drawn from such Wells, feels warm, or at least tepid to a mans hand put into it. And as if Nature design'd men should not be able to contradict the doctrine of *Antiperistasis*, without contradicting more then one of their own senses, she has taken care, that oftentimes the water, that is freshly drawn out of the deeper sorts of Wells and Springs, should manifestly,

ly, as I have seen it, smoak, as if it had been but lately taken off the fire. And this may be said, without a Metaphor, to demonstrate *ad oculum*, the reality of *Antiperistasis*, there being no other cause to which this warmth can be attributed, then the retiring of the heat from the cold external Air, to the lower parts of the earth and water: since both these Elements themselves being naturally cold, and one of them in the supreme degree, the heat we are mentioning is so far from being likely to be generated in so unfit a place, that if it were not very great, it must be extinguished there, by the coldness of the superior Air, and that of the inferior parts of the Earth.

Eleutherius.

7. That *Carneades* may have but one trouble to answer the Allegations to be made in favour of *Antiperistasis*, I hope he will give me leave (according to my custom of siding with either party, as occasion invites me) to add to the familiar Observations mentioned by *Themistius*, some others that

that are less obvious. For I frankly confess to you, that when I consider what interest, the unheeded dispositions of our own Bodies may have in the estimates we make of the degrees of cold and heat, in other Bodies; I should not lay much weight upon the *Phænomena*, that are wont to be urg'd as proofs of *Antiperistasis*, if some instances somewhat less lyable to suspicion, did not countenance the doctrine they are urg'd for. I know that *Carneades* being wont so to propose his opinion about *Antiperistasis*, as only to deny, that it is clearly made out by the reasons or Experiments, that are commonly produc'd to evince it, it were somewhat improper to urge him with observations, that are not familiar, and wont to be employ'd; but I know too, that he is not so rigid an Adversary, as not to allow me to mention some uncommon relations, that I learned from men of good credit. I shall tell you then, that having purposely inquired of ingenious men, that had been very deep underground, some in Coal-pits, and some

some in Mines. One of them affirmed, that at the bottom of the Grove (as they call it) or Pit, he found it very hot in *September*. And another, that he often found it hot enough to be troublesome in Winter. And a third (who is himself a great seeker for Mines, and a Master of considerable ones) that he found it to be hot all the year long. And to manifest, that such Observations will hold even in gelid Regions, I shall repeat to you, what I remember I read in the voyage of that ingenious Navigator, Captain *James*, who giving an account of *Charleton Island*, which by his relation seems to be as cold as *Iceland* itself, says, *That his men found it more mortifying cold to wade through the water in the beginning of June, when the Sea was all full of ice, then in December, when it was increasing.* And he adds, that which makes more to our present purpose, and proves the other part of the doctrine of *Antiperistasis*; *That from their Well, out of which they had water in December, they had none in July.* And to strengthen the observation yet further, I will acquaint

Pag. 36.

quaint you with a relation to this purpose not unworthy your notice. For hearing of an ingenious Physician, that liv'd some years in, and about *Musco*, I applied my self to him (as possibly you may have done, for if I mistake not, I have seen you together) to know, whether in that frozen Region he observed the Cellars to be hot in Winter. And his answer to That, and some other Questions of the like nature I put to him, amounted in short to this, That when I enquired, whether their Springs and Wells were not all frozen in the Winter, he told me, that he saw some Springs, whose warers froze not at all near the Spring-head, but, at a good distance from thence, it began to be thinly cas'd over with ice. He added, That his own Well was about six fathoms deep, between the surface of the earth, and that of the water, and that the water in it, was, as I remember, about three or four fathoms deep, and that not only this Well froze not all the Winter, but that the Well of his neighbour, which was but one fathom deep to the

the superficies of the water, did not freez neither. And to satisfie my curiosity about the steams of this water, he told me, that when a Bucket of water was newly drawn, if it were agitated, it would smoak. But that from the Well it self, when the water in it was left quiet and unstirred, he did not perceive any smoak to arise.

8. To all this I shall add this further circumstance, that having purposely inquired, whether in the Winter he found it as hot in Cellars at *Musco*, as it is wont to be in that season in ours? He answered me, that when the doors and windows were carefully shut, to hinder the immediate commerce betwixt the included and external Air, he often found, if he stay'd long in his Cellar, it would not only defend him from the sharpness of the *Russian* cold, as bitter as that is wont to be in Winter, but keep him warm enough, to be ready to sweat, though he laid by his Furs. So that if we may rely, either upon the Testimony of our senses, we must necessarily admit Cellars to be

warmer in Winter, then in Summer, and consequently allow an *Antiperistasis*.

9. *Carneades*. Though I were not in haste, I should not think it necessary to reply any thing else to the first part of what was said by *Themistius*, then that, what he alledges of the Universality of the Opinion he maintains, may serve to recommend that which he opposes. For the vulgar Doctrine about *Antiperistasis*, being, as he urges, receiv'd, and taught in all the Schools, the Innovators he declaims against, must have learned it there among the other *Peripatetick* tenents, that youth is wont to be imbued with in those places; so that it may rather seem the love of truth, then of singularity, that engages them against an opinion, which before was their own, as well as that of the generality of Scholars, and consequently against which they cannot maintain a Paradox, that does not imply a Retraction. But I shall not prosecute my Answer to *Themistius's* preamble, since

since *Eleutherius*, whom I am chiefly to speak to, is too much a Philosopher to think Truth less her self, for being slenderly attended; or to think any men the less like to be Her followers, because they are but few. To come then directly to the controverſie it ſelf, I think I need not tell one of you, that the other miſtakes my opinion about it. For I perceive, *Eleutherius* hath not quite forgotten, that I have not been wont to deny an *Antiperiſtaſis*, as it may be, but only as it is wont to be explicated. But ſince *Themiftius* ſeems to be willing to have me his Antagoniſt in this controverſie, and ſince *Eleutherius* himſelf ſeems to conſpire with him, I am content to act for a while the part, you Gentlemen would have me take upon me, and will propoſe to you part of what I would ſay, for the opinion you impute to me, in caſe I were really of it.

10. To come then to the controverſie it ſelf, though *Themiftius* has drawn his proofs for the *Antiperiſtaſis* of the Schools, partly from Reason, and partly from Experience; yet the ve-

ry same two Topicks seem to me to afford considerations, that may justly warrant our calling it in question.

11. And first, if we look upon the reason of the thing considered abstractedly from the Experiments, that are pretended to evince an *Antiperistasis*, we cannot but think it may be very rational, I say not, to doubt of it, but to reject it. For in the first place, according to the course of Nature, one contrary, ought to destroy, not to corroborate, the other. And next, 'tis a maxime among the *Peripateticks* themselves, That natural causes always act as much as they can. And certainly as to our case, wherein we treat not of living creatures, I cannot but think the Axiom physically demonstrative. For inanimate Agents act not by choice, but by a necessary impulse, and not being endow'd with Understanding and Will, cannot of themselves be able to moderate or to suspend their actions. And as for what *Themistius* alledges, that it was necessary for the Preservation of Cold and Heat, that they should be endowed with such a power of intend-

tending themselves, I must freely
 confess, that though in living crea-
 tures, and especially in the bodies of
 the perfecter sorts of Animals, I do
 in divers cases allow arguments
 drawn from final causes; yet where
 only inanimate bodies are concern'd,
 I do not easily suffer my self to be pre-
 vail'd upon by such Arguments. Nor
 is there any danger, that Cold and
 Heat, whose causes are so radicated
 in Nature, should be lost out of the
 World, in case each parcel of mat-
 ter, that happens to be surrounded
 with bodies, wherein a contrary
 quality is predominant, were not en-
 dowed with an incomprehensible fa-
 culty of self invigoration. And Na-
 ture either does not need the help of
 this imaginary power, or oftentimes
 has recourse unto it to very little pur-
 pose; since we see, that these Qua-
 lities subsist in the world, and yet *de*
facto the bottles of Water, Wine,
 and other Liquors, that are carried
 up and down in the Summer, are re-
 gularly warmed by the Ambient Air.
 And in *Muscovy* and other cold Nor-
 thern Countries, Men, and other

Animals have oftentimes their Vital Heat destroyed by the cold that surrounds them, being thereby actually frozen to death. And I somewhat wonder, that the followers of *Aristotle* should not take notice of that famous Experiment, which he himself delivers, where he teaches, that hot water will sooner congeal then cold. For if the matter of fact were true, it would sufficiently manifest, that the heat harboured in the water, is destroyed, not invigorated by the coldness of the Air that surrounds it; so that *Themistius* must, I fear, on this occasion, take sanctuary in my observation, and to keep *Aristotle* from destroying his own opinion, with his own Experiment, had best say, as I do, that it is not true. And though it is not to be denied, that white surrounded with black, or black with white, becomes thereby the more conspicuous; yet 'tis acknowledged, that there is no real increase, or intension of either quality, but only a comparative one in reference to our senses obtain'd by this Collation. Nor does a Pumice-stone grow more
dry

dry, then it was in the fire or earth, by being transferred into the Air or Water, and consequently environed with either of those two fluids, which *Themistius* and his Schools teach us to be moist Elements: neither will you expect to find a piece of dim glass become really more transparent, though one should set it in a frame of Ebony, though that wood be so opacous as to be black. And whereas 'tis commonly alledged, as a proof of the power, Nature has given Bodies of flying their contraries, that drops of water falling upon a Table, will gather themselves into little globes, to avoid the contrary quality in the Table, and keep themselves from being swallowed up by the dry wood; the cause pretended has no interest in the effect, but little drops of water, where the gravity is not great enough to surmount the action of the ambient fluid, if they meet with small dust upon a Table, they do as they roul along, gather it up, and their surfaces being covered with it, do not immediately touch the board, which else they would stick to.

to. And to show you, that the Globular figure, which the drops of water, and other Liquors, sometimes acquire, proceeds not from their flying of driness, but either from their being every way press'd, at least almost equally (for in some cases also they are not exactly round) by some ambient fluid, of a disagreeing Nature, or from some other cause differing from that the Schools would give, I shall desire you to take notice, that the drops of water, that swim in Oyl, so as to be surrounded with it, will likewise be Globular; and yet Oyl is a true and moistening liquor, as well as water. And the drops of Quicksilver, though upon a Table they are more disposed, then water, to gather themselves into a round figure; yet that they do it not as humid Bodies, is evident, because Quicksilver broken into drops, will have most of them Globular, not only in Oyl, but in Water. And to show you, that 'tis from the incongruity, it has to certain bodies, that its drops will not stick upon a Table, nor upon some other bodies, but gather

ther themselves into little sphæres, as if they designed to touch the wooden Plain but in a Point: To manifest this, I say, we need but take notice, that though the same drops will retain the same figure on Stone or Iron, yet they will readily adhere to Gold, and lose their Globulousness upon it, though Gold be a far drier body then Wood, which, as far as distillation can manifest, must have in it store of humid parts of several kinds, (I mean both watery and unctuous.) But this may relish of a digression; my task being only to examine the *Antiperistasis* of cold and heat, concerning which I think I had very just cause to pronounce the vulgar conceit very unconsonant to the nature of inanimate beings. For the *Peripateticks* talk of Cold and Heat surrounded by the opposite quality, as if both of them had an understanding, and foresight, that in case it did not gather up its spirits, and stoutly play its part against the opposite that distresses it, it must infallibly perish: and as if being conscious to its self, of having a power of self invigo-

See the
History of
fluidity,
Sect. 19.

invigoration, at the presence of its Adversary, it were able to encourage it self like the Heroe in the Poet, that said, *Nunc animis opus est, Aenea nunc pectore firmo*, which indeed is to transform Physical agents into Moral ones.

12. *Eleuth.* The validity of the *Peripatetick* Argument, drawn from Reason, considered abstractedly from Experience, I shall leave *Themistius* to dispute out with you, at more leisure. And since you well know, that the only Arguments I alledge to countenance *Antiperistasis*, were built upon Experience, as judging them either the best or the only good ones, I long to hear what you will say to the Examples that have been produced of that which you deny.

13. *Carneades.* That, *Eleutherius*, which I have to answer to the examples that are urged, either by the Schools, or by you in favour of *Antiperistasis*, consists of two parts. For first I might

might show, that as *reason* declares openly against the common Opinion, so there are *Experiments* which favour mine, and which may be opposed to those you have alledged for the contrary doctrine. And *secondly*, I might represent, that of those examples, some are *false*, others *doubtful*; and those that are neither of these two, are *insufficient*, or *capable of being otherwise explicated*, without the help of your *Hypothesis*. But for brevities sake, I shall not manage these two replies apart, but mention, as occasion shall serve, the Experiments, that favour my opinion, among my other answers, to what you have been pleased to urge on the behalf of *Aristotle*.

14. To begin then with that grand Experiment, which I remember a late Champion for *Antiperistasis*, makes his leading Argument to establish it, and which is so generally urged on that occasion, I mean the heating of quick-Lime in cold water. I confess I cannot but admire the Laziness and Credulity of Mankind, which have so long, and generally acquiesc'd

acquiesc'd in what they might so easily have found to be false. This I say, because I was possibly the first, that has had both the curiosity and boldness to examine so general and constant a Tradition; yet I doubt not, that you will soon be brought to take it as well as I, for as great as popular an error. For to let you manifestly see, how little the Incalcescence of the quick-Lime needs be allowed to proceed from the coldness of the ambient water, if instead of cold water, you quench it with hot water, the Ebullition of the liquor, will not only be as great, as if the water were cold, but oftentimes far greater. As I have sometimes for curiosity removed boiling water from the fire, and when the liquor had left of boiling, but was yet scalding hot, I put into it a convenient quantity of quick-Lime, and after a while, the water, which, as I said, had ceas'd from boiling, began to boil afresh, with so much vehemence, and such large and copious bubbles, that it threatened to run over the Pot, of which, before the effervescence, a considerable
part

part was left unfill'd. And this was no more then what I might well look for, hot water being much fitter then cold to pervade nimbly the body of the Lime, and hastily dissolve, and set at liberty the igneous and saline parts, wherewith it abounds. And how much a greater interest salts may have in such incallescencies, then Cold, I have also taken pleasure to try, by pouring Acid spirits, and particularly spirit of salt upon good quick-Lime. For by this means there would be a far greater degree of heat excited, then if I had instead of spirit of Salt used common water: And this, whether I imploy'd the spirit cold or hot. For in either case, so small a portion as about the bigness of a Walnut of Lime put into a small glass, would by the addition of a little spirit of Salt put to it by degrees, both hiss, and smoak, and boil very surprizingly, and notwithstanding the small quantity of the matter, would conceive so great a heat, that I was not able to hold the glass in my hand. And to show some friends, how little, heat excited
in

in quick-Lime by cold water, proceeds barely from the coldness of that liquor; I caus'd a parcel of good Lime to be beaten small, and putting one part of it into a glass vessel, I drench'd it plentifully with oyl of Turpentine, more then it would imbibe, and the other portion of the Lime I likewise drench'd with common water: both these liquors having stood in the same room, that they might be reduc'd by the same Ambient Air, to a like degree of coldness, the event of this Trial was (what I look'd for) that the oyl of Turpentine, notwithstanding its actual coldness, and the great subtilty and piercingness of parts, which it has in common with other Chymical oyls, being of an incongruous Texture seem'd not to make any dissolution of the powdered Lime, and did not, for several hours, that I kept it, produce, that I perceived, any sensible heat in the Lime. Whereas to show, that 'twas not the fault of the Lime, that part of it, on which common water had been poured, did after a little while conceive so strong a heat,

heat, that it broke a large open-mouth'd-glass, into whose bottom it was put, and not only grew so hot, that I could not endure to hold it in my hand, but sent out at the mouth of the glass, though that were considerably distant from the Lime, a copious white fume, so hot, that I could not well suffer the holding of my hand over it. And to prevent a possible, though invalid, objection, which I forelaw might be drawn against the Experiment made with oyl of Turpentine, from the Oleaginous Nature of that liquor; I covered a piece of the same sort of quick-Lime, I have been speaking of, with highly rectified spirit of Wine; but though I left them together all night, yet I perceived not, that the liquor had at all slack'd the Lime, which continued in an intire lump, till upon the substituting of common water, it did, as I remember, quickly appear to be slack'd, since it fell assunder into a kind of minute white powder, which was (bating the colour) almost like mud, and would easily by a little shaking be disperst, like it, through the water.

A a a

Eleu-

15. *Eleutherius*. I ingeniously confess to you, *Carneades*, that what you say surprizes me, for I thought it superfluous to try my self, so acknowledged an Experiment, being not able to imagine, that so many learned men for so many Ages, should so unanimously and confidently deliver a matter of fact, of which, if it were not true, the falsity could be so easily discovered.

16. *Carneades*. For my part, *Eleutherius*, I confess I am wont to doubt of what they teach, that seldom or never doubt. And I hope you will forgive me, if having found an assertion so general and uncontroul'd of a falsity so easie to be disprov'd, I be inclinable to suspect the Truth of their other inferior Traditions about *Antiperistasis*; and of these I will mention the two chiefest I have met with among the moderns (for being contriv'd Experiments, I presume you will easily believe they came not from *Aristotle*, nor the Ancient School-

Schoolmen that commented upon Him.)

17. The first of these is the freezing a Pot to a Joynt-stool, by a mixture of snow and salt, by the fires side: in which case 'tis pretended, that the fire does so intend the cold, as to enable it to congeal the water, that stagnated upon the surface of the stool, betwixt That and the bottom of the Pot. But how little need there is of *Antiperistasis* in this Experiment, you may guess by this, that I have purposely made it with good success, in a place in which there neither was, nor ever probably had been a fire, the room being destitute of a Chimney. And this Trial of mine I could confirm by divers other Experiments of the like nature, but that this one is sufficient.

18. I proceed therefore to the other Experiment, which is delivered by very learned men, and for whom I have a great respect: according to these, if you take a somewhat large Pot, and having fill'd it almost with snow, place in the middle of the snow a Vial full of water; this

A a a 2

Pot

Pot being put over the fire, the coldness of the snow will be so intended by the heat, from which it flies into the water, that it will turn that liquor into ice. But though I several times tri'd this Experiment, yet neither in earthen, nor in silver vessels, could I ever produce the promised ice. And I remember, that an eminently learned man, that wondered to find me so diffident of what he said, he knew to be true, readily undertook to convince me by an Ocular proof, but with no better success then I had had before. So that the Argument may be plausibly enough retorted upon them that urge it.

19. And in case the Trial should succeed sometime or other (for that it doth not ordinarily, I have shown already) yet will there be no necessity of deriving the effects from *Antiperistasis*. For though in such cases the fire would contribute to the production of the effect, by hastening the dissolution of the snow, yet the heat of the fire does but remotely, and by accident cause the production of ice, since other Agents will do the same thing,

thing, that are qualified to make a quick dissolution of the snow, whether they be hot or no; as I have tri'd that spirit and crude salt of Nitre, will either of them by a due application, bring snow, by dissolving it, to congeal water, though the Spirit and the Nitre be generally agreed upon to *actually* cold, and one, if not both of them, to be *potentially* cold too.

20. Having thus dispatch'd the *Experiments* pretended to evince an *Antiperistasis*, I must now examine the *Observations*, that are alledg'd to that purpose, of which the principal, if not the only, are these. The coldness of the middle Region of the Air. The increase of mens stomachs in Winter. The generation of Hail; and the heat and cold in Cellars, and other Subterranean places, when the contrary Quality reigns in the Air.

21. To begin with the first of these: I will not now dispute, whether the second Region of the Air, have really that coldness that is wont to be ascrib'd to it: Though our Friend Mr. Boyle seems to doubt, whether that Regions being always,

and every where cold, have been as strongly proved, as asserted. But passing over that Question, I see no need of imploring the help of *Antiperistasis*, to keep the second Region of the Air for the most part cool. For without at all taking in the cause imagin'd by the Schools, an obvious and sufficient one may be easily assign'd. For the Air being, as to sense, cold of its own nature, so that when we feel it hot, it is made so by some adventitious agent, and that agent being for the most part the Sun, who heats the Air chiefly, though not *only*, by its reflected beams; their heat is so languid, by that time they arrive, dispers'd, at the second Region of the Air, that they are not able to overpower its Natural coldness, increased perchance by some frigorifick spirits, that may find a more commodious harbour there, then in other parts of the Atmosphere. And whatever be the true cause of the coldness in the middle region of the Air, I cannot but admire to find, that Coldness so confidently ascrib'd to *Antiperistasis*,
by

by *Themistius* and his Friends the *Aristotelians*: For according to them, 'tis the Nature of the Element of Air to be as well hot as moist, and according to the same *Peripateticks*, both the upper Region of the Air always, and the lower in Summer is hot, the former by the neighbourhood of the imaginary Element of fire, and the latter by the reflection of the Sunbeams from the Earth: which two Positions being laid together, I would fain learn of any *Aristotelian*, how *Antiperistasis* comes to take place here? For, according to them, those Bodies have their cold and heat increas'd by *Antiperistasis*, that are on both hands assail'd by Bodies of a contrary Quality, to that which is natural to the surrounded Body, whereas the whole Element of Air, and consequently the middle Region, being, as they would persuade us, hot, of its own Nature; what shadow of probability is there, that the highest and lowest Regions, by being hot, should make the middle Region, which is also naturally hot, intensely and durably cold.

But though the objection is so clear, that it needs not to be insisted on; yet because 'tis but an Argument *ad hominem*, I shall add this for their sakes, that are not in this point *Peripateticks*, That it does not appear to me, that if the Air be naturally rather cold, then hot, the second Region must owe the *Intensness* of that Quality to *Antiperistasis*. For the ground of the opinion, I oppose, being this, That both the first and the third Regions are considerably hot, I would gladly find it prov'd as to the upper Region. I confess I have not found the assertion contradicted, but that, as little convinces me, as the uncontrouledness of the Tradition about quick-Lime, that I lately confuted. 'Tis true, there are two reasons alledged, to evince the heat of the supreme Region of the Air, but neither of them to me seems cogent. For the first is, that the Vicinity of this Region to the Element of fire makes it partake a high degree of Heat. But if we consider the distance of that Element, which they place contiguous to the Orb of the Moon,

Moon, and how little nearer to it the concave part of the upper Region is, then the Convex of the middle, we may easily conceive, that in two distances, that are both of them so *immense*, so *small* a disparity cannot be much (if at all) more considerable, then the greater nearness of one side of a sheet of paper held at three yards distance from an ordinary fire, in comparison of the distance of the other side of the same paper; or then the distances of a small Wart, and of the neighbouring parts of the face, when a man comes within 2. or 3. yards of the fire. But 'tis not worth while to prosecute this Consideration, because the Argument against which 'tis alledg'd, is built upon the groundless supposition of the Element of fire, a figment which many of themselves do dayly grow asham'd of, as indeed its existence is as little to be discovered by reason, as perceiv'd by sense.

22. The other Argument for the heat of the third Region of the Air, is, that fiery Meteors are kindled by it. But not now to question, whether

ther all Meteors that shine, and therefore pass for fiery, are really *kindled* exhalations; we see, that in the lower Region of the Air, and in Winter, those fires that are called either *Helena*, or *Castor* and *Pollux*, are generated in great storms, and hang about the sails and shrouds of Ships. Nay, do not we much more frequently see, that Lightning is produced at all seasons of the year (for in warmer Countries thousands have observed it to thunder (and so have I) in Winter) in the middle region of the Air. And *since* 'tis not the heat of the inferior part of the Air, that kindles those Exhalations; and *if* notwithstanding the Coldness of the second Region, fiery Meteors may be frequently generated *there*; I see no reason why the Production of such Meteors should argue the heat of the third Region of the Air. And if that Region be not hot, then it will, I presume, be easily granted, that the coldness of the second must very improperly be attributed to such an *Antiperistasis*, as it is generally ascribed to.

23. I come next to consider that Aphoristical saying of *Hypocrates*, *Ventres hyeme esse calidiores*, together with the Observation whereon it seems to have been grounded. I will not now examine, whether any arguments for the contrary may be drawn from the heat and thirst men feel in Summer, and the refreshment they then find by Drinks and Fruits, and other Aliments that are actually cold. For that which I principally intended to say, is this, That I much more doubt the matter of fact delivered in the Aphorism, then that, in case it be true, it may be made out without the help of *Antiperistasis* in the vulgar and Scholastick notion of that Term.

24. I consider then first, that the proof, that is wont to be brought of the greater heat of mens stomachs in Winter, is, that men are wont to have *then* a greater appetite to their meat. But though I pay so much respect to the great *Hypocrates*, as to allow the Aphorism in a sense; yet I admit it to be true but upon an *Hypothesis*, that I do not admit to be so. For the Aphorism supposes, that the
dige-

digestion of meat in the stomach is made by heat, and consequently, that the stronger digestion, that is wont to be made in Winter, is an argument of the stomachs being *then* hotter, then at other seasons of the year. But the Erroniousness of this supposition, I think, I need not solemnly prove to *Eleutherius*, who I doubt not has taken notice of several things in Nature, that agree not with it, and particularly of the strong concoction, that is made in the stomachs of divers ravenous fishes, whose stomachs and blood are yet, as I have purposely observed, sensibly cold: but if it should in some cases prove true, that there is really in mens bodies a far greater heat in Winter then in Summer; yet this would not infer an *Antiperistasis* in the sense, wherein I oppose it. For the vital heat lodg'd in the heart, always generating out of the blood and juices, that continually circulate through that part, great store of spirits and warm exhalations, which are wont to transpire through the pores of the skin in much greater quantities, then, notwithstanding

standing the affirmations of *Sanctorius*, any thing but my own Trials could have perswaded me, these warm steams finding the pores of the skin straitned and shut up, grow more and more copious in the body, and thereby heat the stomack, as well as the other internal parts of it: And perhaps also the same frigorifick Corpuscles or Temperature of the Air, that produce cold in Winter, may by shutting in certain kinds of *Effluvia*, or perhaps altering the motion or Texture of the blood, reduce it to such a disposition, as that the appetite shall be increas'd, as well as the concoction in the stomack promoted by the *Stomachical menstruum*, or ferment, which either is newly generated in Winter, or more copiously supplied (by the circulating of the blood to the stomack) in that season then in others. And to show, that a good appetite may be procur'd by agents endow'd with very distinct and contrary qualities: do not we see, that spicy Sauces, Wine and Vineger do all of them, in most men, beget an appetite, though
the

the two former be confessedly hot, and the latter cold. And so Wormwood, and juice of Lemmons have both of them frequently reliv'd dull and weak stomacks, though the one be confessedly a hot simple, and the other a cold. And in some cases, either the frigorifick Corpuscles themselves, and perhaps some other unknown to us, that they may bring along with them, may so sollicit the stomach, as to breed an eager appetite, not precisely by their being cold or hot, but by their peculiar nature; as we have instances of some, that in these parts by walking on the snow, procure to themselves a *Bulimius*. And the learned *Fromundus* relating, how he himself by walking long on the snow, was surpriz'd with such a *Βελιμία*, takes notice, that the chief cause of the fainting was in the stomach: And that he found by his own experience, that that part was discompos'd, convell'd, and provok'd to cast. To which he adds, (what makes much for my present purpose) that he now thinks the chief cause of the *Bulimia* to consist in certain

tain steams, that do peculiarly affect the stomach, which they gnaw and distend. And just before he observes, that straining to fetch deep coughs is a present remedy in this distemper, by discharging the stomach and Lungs of those snowy spirits, which were either attracted in respiration, or had some other way insinuated themselves into those parts: So that besides the cold abstractedly consider'd, the stomach may be peculiarly affected by other, either attributes or concomitants, of the frigorifick Corpuscles, that grow powerful in frosty weather; with which it well agrees, that divers have been observed to be subject to *Bulimias's* in these parts of the world, though in our warmer Climates such men endure nothing near so great a cold, nor are so much inconvenienced by it, as multitudes of others, that in *Nova Zembla*, and other gelid Regions never complain'd of having contracted even in the midst of Winter, any such disease.

25. Another argument that is specious enough, urg'd in favour of *Antiperistasis*,

tiperistasis, is borrowed from the production of Hail, which is presum'd to be generated in Summer only, not in Winter, and, according to *Aristotle* and the Schools, is made in the lowest Region of the Air, by the cold of the falling drops of rain so highly intended by the warmth it meets with in the Air near the Earth, as to congeal the water wherein 'tis harbour'd. But though I freely confess to you, that I think the generation of Hail difficult enough to be solidly explicated; yet I scruple not to reject the receiv'd doctrine about it, for several reasons, of which I will now name four.

26. For in the first place, 'tis not universally true, as is suppos'd, and the *Aristotelian* doctrine requires, that Hail falls not but in Summer, or very hot weather. For I have myself observ'd it within this twelve moneth, to Hail at the latter end of *November*, and that, when some frosty days have preceded, and when the coldness of the weather was complain'd of. Nay, the longest shower of Hail, that either I, or some others remember

remember our selves to have ever known, I observ'd to fall about a week before the end of *January*, on a night preceded by a very frosty day, which it self was preceded by a sharp fit of frosty weather. And here I must not pretermitt this circumstance, that when the tedious shower was over, there came to the house, where I then was, a maid, that is servant to one of my Domesticks, and related to her Master, and others, how she was for a good while misled out of the beaten way, where the storm found her by an *Ignis fatuus*, which she followed, till by its passing over a place, where she found an unpassable hedge, it both show'd her, that she was out of her way, and that it was no candle, though she had so confidently thought it one, that she call'd out to the party, she presum'd it to be carried by. I will leave *Themistius* to unriddle, how the Nocturnal Air could kindle a fiery Meteor by its coldness, and at the same time congeal the falling drops of water into ice by its warmth, and shall only add, that I doubt not but

other observations of the like kind have been often made, though perhaps seldom recorded. For within the compass of a very few weeks of the storm, some servants of mine affirm'd themselves to have observed it to Hail two or three times besides that already mention'd.

27. Next, if *Aristotle* have rightly assign'd the cause of Hail, 'tis somewhat strange it should not fall far more frequently in Summer, and especially in hot Climates, then it does, considering how often in all probability the drops of rain fall cold out of the second Region into the warm Air of the first. And more strange it is, That even in those parts of *Ægypt*, where it rains frequently enough and plentifully (for so *Prosper Alpinus*, that liv'd long there, assures us it does) though not about *Grand Cairo*, yet about *Alexandria* and *Pelusi-um*, it should never Hail no more then Snow, as the same learned Physician (a witness above exception) affirms. Besides, whereas it is pretended, that Snow is generated in the upper Region of the Air, and
Hail

Hail always in the lower, my own observation has afforded me many instances, that seem to contradict the Tradition. For I have observed in I know not how many great grains of Hail, that besides a hard transparent icy shell, there was as 'twere a snowy Pith of a soft and white substance, and this snowy part was *most commonly* in the middle of the icy, which made me call it Pith, but *sometimes* otherwise. And lastly, whereas the favourers of *Antiperistasis* would have the Drops of rain in their descent, to be congeal'd apart in the ambient Air; not to urge, how little the irregular and Angular figures we often meet with in Hail does countenance this doctrine; Hail often falls in grains, too great by odds to be fit to comply with *Aristotles* conceit. For not to mention the grains of Hail I have observed my self to be of a bigness unsuitable to this opinion, divers learned eye-witnesses have inform'd me of their having observ'd much greater then those I have done; and particularly an eminent *Virtuoso* of unquestionable credit, affirm'd both

to me and to an Assembly of *Virtuosi*, that he had some years ago at *Lyons* in *France* observ'd a shower of Hail, many of whose grains were as big as ordinary Tennis-balls, and which did the Windows and Tyles a mischief answerable to that unusual bulk. And *Bartholinus* affirms, that he himself observ'd, in another shower of Hail, grains of a more unwonted size; a single grain weighing no less then a whole pound. But though this it self is little in comparison of what I remember I have somewhere met with in learned Authors, yet it may abundantly suffice to disprove the vulgar conceit about the generation of Hail, till we meet in these Countries with showers of rain, whose single drops prove to be of such a bigness; which I presume those that ascribe Hail to *Antiperistasis* will not easily show us.

28. I come now to consider the last and indeed the chiefest example, that is given of *Antiperistasis*, namely the coldness of Cellars, and other subterranean Vaults in Summer, and their heat in Winter. And as the Argument, wont to be drawn from
hence

hence, consists of two parts, I will examine each of them by its self.

29. And first, as to the refreshing coldness, that subterranean places are wont to afford us in Summer, I both deny, that they are *then* colder than in Winter; and I say, that though they were, that coldness would not necessarily infer an *Antiperistasis*.

30. We must consider then, that in Summer our Bodies having for many days, if not some weeks, or perhaps months, been constantly environ'd with an Air, which, at that season of the year, is much hotter, then 'tis wont to be in Winter, or in other seasons, our senses may easily impose upon us, and we may be much mistaken, by concluding upon their Testimony, that the subterranean Air we then find so cool, is really colder, then it was in Winter, or at the Spring; as they that come out of hot Baths think the Air of the adjoining rooms very fresh and cool, which they found to be very warm, when coming out of the open Air, they went through those warm rooms to the Bath, and the deepness

and retiredness of these subterranean Caves keep the Air, they harbour'd, from being any thing near so much affected with the changes of the season, as the outward Air that is freely expos'd to the Suns warming beams, which pierces with any sensible force so little a way into the ground, that Diggers are not wont to observe the Earth to be dried and discolour'd by them beyond the depth of a very few feet. And I have found, that in very shallow Mines not exceeding six or seven yards in depth, though the mouth were wide, and the descent perpendicular enough, the Air was cool in the heat of Summer; so that the free Air and our Bodies that are always immers'd in it, being much warmer in Summer than at other times, and the subterranean Air by reason of its remoteness from those causes of alteration, continuing still the same, or but very little chang'd, it's no wonder, there should appear a difference as to sense, when our bodies pass from one of them to another.

31. And supposing, but not yielding,
ing,

ing, that the Air of Cellars and Vaults were really colder in Summer than in Winter, that is, were discovered to have a greater coldness, not only as to our sense of feeling, but as to Weather-glasses; yet why should we for all that have recourse for the solution of the difficulty to an *Antiperistasis*, which 'tis much harder to understand, than to find out the cause of the *Phænomenon*, which seems in short to be this, That whereas (which I shall soon have occasion to manifest) there are warm Exhalations, that in all seasons are plentifully sent up by the subterranean heat, from the lower to the superficial parts of the Earth, these steams, that in Winter are in great part repress'd, or check'd in their ascent, by the cold frost or snow, that constipates the surface of the Earth, and choaks up its pores, these Exhalations, I say, that being detain'd in the ground would temper the Native coldness of the Earth and Water, and consequently that of Springs, and of the subterranean Air, are by the heat that reigns in the outward Air, call'd out at the

many pores and chinks, which that heat opens on the surface of the ground, by which means the water of deep Springs and Wells, and the subterranean Air, being depriv'd of that, which is wont to allay their Native or wonted coldness, are left to disclose a higher degree of it, and seem to have that quality increas'd, when indeed it is but freed from the mixture of its contrary that weakened it.

32. As for the heat, we find in Cellars and Vaults in Winter, the solutions already given will be applicable to that *Phænomenon* also, which by this way is yet more easie to be accounted for then the other. For having first question'd the matter of fact, 'twill not be difficult to show, that though it were true, it need not be ascrib'd to *Antiperistasis*.

33. I think then, that it may be justly question'd, whether Cellars in general are hotter in Winter then they are in Summer. For as for the Testimony of our senses, upon which alone men are wont to conclude the affirmative, it may in this case easily
and

and much delude us. For those places being [shelter'd from the winds, and kept from a free communication with the outward Air, are much less expos'd then others to the action of those agents, whatever they be, that produce cold in the Air. So that our bodies being constantly immers'd in the Air refrigerated by the Winter, and consequently brought nearer to the temper of that Air, when we bring those bodies into Cellars the subterranean air must seem warm to us, though in it self it were really invaried as to its temper.

34. Now that many Cellars are indeed colder in the midst of Winter, then in the heat of Summer, though not in respect of our senses, yet in respect of other bodies that have not the same predispositions, I am induc'd to believe by some Experiments of mine own, purposely made. And first in a frosty evening having hung out in a Garden two seal'd Weather-glasses, that they might be reduc'd as near as could be to the temper of the ambient Air, I brought one of them into a Cellar, and it soon began manifestly

manifestly to rise, and in two or three hours ascended five or six divisions, whilst the water in another seal'd Weather-glass, that continued suspended in the same part of the Garden, did rather a little subside, then at all rise, which is agreeable to the first part of what I was saying; namely, that the Air, harbour'd in Cellars, is not so powerfully affected by the ordinary efficient of cold, as the free and external air. And now as to the second part of what I was saying, that the subterranean Air, though it be less affected by the outward cold, may be somewhat affected by it, instead of growing hotter by *Antiperistasis*; I shall add, that early in the morning in frosty Weather the liquor in the same Weather-glass appear'd more subsided, then over-night, which shows, that the external air did lessen, not increase the warmth of the air in the Cellar. And having there plac'd a wide mouth'd glass of oyl, which in thawing weather remain'd all night fluid as before, the same liquor, the very next night, which was a bitter frost,

was

was so far frozen and congeal'd, as to sink in other oyl, and keep its surface exactly, though the glass were inclined and turned upside down. And prosecuting my Trial, I found, that in a sharp frost, and great snow, the liquor, that on the Thursday night was beneath the fourth knob or mark of division, a sudden thaw coming with a South wind, the next morning in the same Cellar the liquor was ascended to the eighth mark. And continuing the Weather-glass in the same Cellar for a good while, to watch its alterations every night and morning, I remember I met with, and registred more observations, that confirm'd me in my opinion, though 'tis so long ago, that I have forgot the particular circumstances. And after these Trials meeting with a learned *Polander*, I did without declaring my opinion, inquire of him, whether in his Country he had at any time observ'd Beer to freez in Cellars in frosty weather, to which he answer'd, that in the coldest Winters, if the Beer were small, the Barrels would oftentimes be frozen, but not if

if it were strong. But I need not have recourse to forrain Testimony, having my self observed here in *England* more then one Barrel of Beer to be frozen in the Cellar in exceeding cold weather. Infomuch that one of the Barrels being full, and the liquor expanded by freezing, was forc'd out at certain chinks, which seem to have been made by that expansive force, and the liquor so ejected, adhered in a considerable lump to the outside of the vessel; and yet this Cellar had its Windows carefully shut, and not only was near a Kitchen, where fire was constantly kept, but, which was more considerable, it had this principal mark of being a good Cellar, that in the heat of Summer it us'd to afford me drink sufficiently cool. And now to requite *Eleutherius* with the Testimony of that very person, Physician to the *Russian* Emperor, whose authority he lately alledg'd against me, I shall confess, that as he suspects, I had conference with this Doctor, and when I diligently enquired of him, whether their Cellars at *Musco* were really

really very cold in Summer, he answered me, that they were not, and that they had distinct Cellars for Summer and for Winter, that their small Beer would quickly grow sour in their Cellars in Summer, if their vessels were not kept in Snow, that therefore their way was to make at the bottom of their Summer Cellars (to which belong'd a Well to receive the water dropping from the melted Snow) a deep layer of snow, on which they afterwards cast a convenient quantity of water, that the whole mass might be turn'd into a kind of ice. In this snow they keep their Casks, making sometimes a layer of Snow, and a layer of Cask, and digging out their vessels, as they had occasion to use them.] By all which it may appear how groundlessly it's *universally* affirm'd of Cellars, that as they seem *to the sense*, so they *really* are hotter in Winter then in Summer.

35. But if it should happen, (as in some places 'tis not impossible, but that it may) that some Vaults and Cellars are really warmer in Summer then in Winter ;

Winter ; yet I see not why this should reduce us to the acknowledgment of an *Antiperistasis* ; for neither could the effect be made out by that, nor would there be any necessity to have recourse to it.

36. And first I might content myself to repeat, what I have formerly said, to shew the incongruity of *Antiperistasis* in general to Nature's ways of acting. And I might add, that to imagine with some late *Peripateticks*, (whom all their reverence to *Aristotle* has not so far blinded, as not to let them see the unreasonableness of his conceit) that in Winter the warmth of the ambient air retreats into Cellars and Vaults to shun its contrary, is to make meer accidents, or at best inanimate agents, act with knowledge and design. But I will rather represent, that, though *Antiperistasis* were intelligible, it were improper to alledge it in our case. For to invigorate the warmth of the air by the Cold, the air must according to them be environ'd with other cold bodies, and the heat must retire it self as far as it can from them. And accordingly

ingly 'tis observ'd, that in Winter the deepest Cellars are warmest; but in the case before us the subterranean air, though above, it have the cold that reigns in Winter; yet beneath, the subterranean heat makes the Earth very warm. This I shall not wonder, if you look upon, as new and Paradoxical. And therefore I shall apply my self to the proof of it, and to convince you, I shall not imploy the observations of Chymists and Mineralists, for fear you should suspect them of ignorance or design, but I will use only the authority of a learned Physician, *who* I think was also a professor of Mathematicks, *who* in but too many points is a stout *Peripatetick*, and *who* above all this professes himself to be an eye-witness of what he relates. This Author then informs us, that about the year 1615. he had a curiosity to visit the Mines of *Hungary*, and particularly to go down into the deep *Goden Mine* at *Cremnitz*, and that after he had descended fourscore or a hundred fathom, he found it excessively hot, though he had but a slight linnen garment

ment on, and though he be a maintainer of *Antiperistasis*, yet he affirms that not only the Overseer and workmen of that Mine, but also thole of divers other Mines unanimously assured him, That that lower Region of the Earth was all the year long very hot, and as well in Winter as he found it in Summer; so that it seems in Winter the heat of the subterraneal parts less remote from the superficies, cannot be intended by the coldness of the more internal parts of the Earth, those parts being themselves not always cold, but always hot.

— *Cœpimus in hanc fodinam per gradus valde strictos profunde admodum descendere, per regionem certè frigidissimam, quam solis vestibus metallicis opertus, multo frigidiorē sensi, &c. p. m. 130.*

37. *Eleutherius*. But you may, *Carneades*, remember, that this very Author tells you, that

he found the supreme region of the Earth, as he calls it, which is that next the air, exceedingly cold, both as he went down into the Mine, and as he came up again, and that he ascribes that coldness to *Antiperistasis*.

Carnea-

38. *Carneades.*

Right, but you may remember too, that he relates, that 'twas in *July*, and in very hot weather, that he went down into the Mine, and that to avoid fouling his clothes, he put them off, and exchanged them for a light loose Linnen garment, such as the Diggers wore, and this himself mentions, as that which much increased the coldness he felt: So that if besides this, we consider, that he descended into a cooler place, with a Body already affected with the great heat, which he elsewhere takes notice, that that season had given the outward air, and perhaps much heated by riding or walking to the Mine, we shall not wonder, that he found the change very sensible as he went down; and we shall less wonder, that he found the upper Region of the Earth, as he calls it, more cold when he came up again: since besides the toil of going to and fro, and ascending through narrow, low, and difficult passages, he came out of a place excessively

— *Inquam descendi mense Julio, quo anni tempestas vigeat calidissima, siccissimaque. p. m. 130.*

C c c

hot;

Pag. 142,
& 143.

hot; insomuch that he tells us, that the Overseer of the Mine would not go back with him the same way he came, but took a far shorter, though it were a more dangerous way, causing himself to be drawn up in a perpendicular Groove, and rendring this reason, that 'twas very unhealthy, when one comes out of a place where the Diggers work naked, and where one is even melting into sweat, to make any long stay in the superior Region of the Earth. So that besides that this Author, although he maintains *Antiperistasis*, yet he allows this upper Region to be hot in Winter, as well as cold in Summer, and consequently, that in Winter it has not a cold region beneath, as well as above it, which is enough to vindicate the thing for which I first alledged his Testimony: Besides this, I say, to me, who, though I willingly thank him for his Narrative, am much more sway'd by what he *relates*, then by what he *thinks*; the matter of fact seems very favourable to my opinion; for you see, that I can justly refer the cold he felt
near

near the surface of the Earth, to the deception of his sense, but the heat he felt within the bowels of the Earth cannot be referred to the same cause, since he tells us, that at the top of that great and perpendicular Groove, by which the Mine-master was drawn up, there ascended a plentiful smoak, that was, even above the mouth of it, felt actually hot; and besides his

Pag. 125.

own confession, that the deep parts of the Mine, were more then seemingly hot, I can draw further proofs from these two circumstances, that I have elsewhere met with in his Narrative: The one, that on the surface of the Earth, it was then excessively hot; another, that the smoak, which, notwithstanding this heat

appeared hot, had in its ascent passed through four or five hun-

Exhalatio aere levior per ipsum puteum ascendit magno impetu, in ejus summitate adhuc sensibilibiter calida ipsa æstate, licet supremam terræ regionem tunc frigidissimam permeat. Pag. 128.
see also pag. 125.

dred foot of a cold region of the Earth, whereby it may well be supposed, to have been much in frigidated. To these relations of the learned *Morinus*, I will add, that the

Archbishop of *Upsal* affirms, that in the year 1528. being in *Poland*, he went to visit those deep mountains

In Polonia vero montes profundissimi salis sunt, præsertim in Vieliscæ & Bochna ubi videndi causâ transcendis scellis, vidi in profundioribus locis laboratores nudes ob calorem, ferreis instrumentis eruere opulentissimum Thesaurum salis, veluti Aurum & Argentum ex Mineris inexhaustis. Olaus Mag. lib. 13. p. 382.

(as he terms them) whence they dig solid salt, and having descended fifty Ladders, found in the deeper places, that the

workmen were naked, because of the heat : so that supposing the time of the year not to be considerable in this case, it seems by this relation, that, provided a man descends low enough into the bowels of the Earth, he will find it very hot, even in places that want those Metals, or Marchisites, or other like Mineral substances, by the action of saline liquors, or exhalations, upon which, you, *Eleutherius*, have, I remember, sometimes suspected, that the heat observed in Mines may be produced.

39. I have hitherto shown, that the heat of Cellars and Vaults in Winter, has been very *improperly*, and

and now I come to show, that it has been as *unnecessarily* ascrib'd to *Antiperistasis*. For as the air of those places is protected from the greatest part of the adventitious Coldness that reigns in the outward Air: so the subterranean air has a positive cause of heat in Winter, that it has not in Summer. For as I formerly took notice, in Summer the pores of the Earth, being dilated and opened by heat, the warm exhalations, that were wont to be mingled with moist vapours in the bowels of the Earth, are call'd out, and exhal'd away. For as in the Winter the surface of the Earth being hardned by frost, or the pores of it choak'd up, or at least much obstructed, the hot steams, that, as I lately prov'd by our *French* Authors Testimony (to which I could add, that of eminent Chymists and Mineralists) do continually, and copiously enough ascend from the warm Region, or lower parts of the Earth, are in great part detained and imprisoned in Cellars, and other subterranean cavities, where consequently they produce such a heat, as

Unable to display this page

ly ascended out of the great Groove in the *Hungarian* Mine, may keep us from thinking incredible.

40. And now by what I have hitherto discours'd, I have made way for the solution of a *Phænomenon*, that is wont to be much urg'd in favour of *Antiperistasis*, namely, the smoaking of water, that is drawn in frosty weather, out of deep Wells and Springs.

41. But first I must advertise you, that 'tis improperly enough, that some urge for *Antiperistasis*, such examples as the strange Spring near the Temple of *Jupiter Ammon*, which *Lucretius* and others have observed to have been exceeding cold in the day time, and as hot at night; for, not now to examine, whether this story be not fabulous, or might not be ascrib'd to some crafty trick of the Idolatrous Priests, that had a mind to impose upon *Alexander*, as well as others, and procure an admiration to the place; I consider, that this, and other the like cases, such as are the Springs mentioned in the Islands of *Maldivia*, by *Pyrard* (a French

Pag. 63.

Author, that was shipwrack'd, and liv'd long in those parts) must be referred to the peculiar Nature of the Springs, or some other hidden cause, since, if the water of them were but ordinary, and the *Phænomena* were the effects of *Antiperistasis*, it might justly be expected, that the like should happen in all Springs, or at least in very many, which, that it does not, common experience shows us. And I would say, that this might be the case of the Spring, you mention out of Captain James's Voyage, but that *besides*, that he does not say expressly, that it was frozen in *July*, but only that then it afforded him no water, which might happen upon divers other accounts: And *besides*, that 'tis manifest, that in far hotter Countries, where the excessive heat of the Air might more intend the subterranean cold, if *Antiperistasis* could do it, there is no talk of any such degree of cold in Summer, as to freeze the Springs; *besides* this, I say, there seems to be, through some mistake or other, a contradiction in the relation it self, since in the same Voyage, speaking

speaking of the same month of *December*, he expressly says, that their Well was then frozen up, so, *that dig* Pag. 58.
as deep as they could, they could come by
no water. And he complains on that

occasion, of the unwholsomness of melted snow-water. 'Tis true, that he soon after mentions a Spring, that he found under a hills side, which Pag. 59.
 did not so freez, but that he could break the ice and come to it, but by his very sending far from his house to that Spring, it appears to have been a Consequence, and therefore a Proof, of the uselessness of his Well in *December*; as his affirmation, that it continued all the year so, as to be serviceable, when the ice was broken, shows, that the *Antiperistasis* did not freez it up in Summer. And having cleared my self of such a Testimony of this ingenious Navigator, as would appear very illustrious, if there had been no mistake about it, I shall not scruple to add, that the late publisher of the Latin Description of *Denmark* and *Norway* informs us, that in or near that little Danish Island *Hueena*, wherein the famous *Tycho* built

built his *Urani-Burgum*, there is one Spring among many ordinary ones, that even in the coldest Winter is never frozen, which, subjoyns my Author, does in these regions exceeding rarely happen to be found. *Olaus*

Hancque naturam, lacum similem, prope Metropolin Nidrosiensem Regni Norvegiae, habere compertum est, eo praecipue Argumento, quod in mediis frigoribus nunquam congelatur. Lib. 2.

Magnus also relates, that in another part of the King of *Denmarks* Domini-

ons, namely, near *Nidrosia*, one of the chief Cities of *Norway*, there is a Lake, that even in that Northern Region never freezes. And the learned *Josephus Acosta* mentions, that among a very great number of hot Springs to be met with in *Peru*, At the Baths, which they call the Baths of *Ingua*, there is a course of water, which comes forth all hot and boiling, and joyning unto it, there is another, whose water is as cold as ice. He adds, That the *Ingua* (or the *Peruvian* Emperor) was accustomed to temper the one with the other, and that it is a wonderful thing to see Springs of so contrary qualities, so near one to another. These relations as I was saying, I scruple not to mention, though

*Joseph.
Acost.
Hist. Ind.
pag. 174.*

though at first sight they may seem to disavour my cause. For by these and some others it may appear, that Springs may obtain very peculiar and strange qualities from the nature of the places whence they come, or through which they pass, or from some other causes, that are as hidden from us, as the originals of these rare waters. And this being once prov'd, who knows what interest, such causes, as we are strangers to, may have in some *Phænomena*, that are wont to be wholly ascrib'd to the heat and cold of the superficial part of the ground, and what influence they have upon many other Springs (besides those above mentioned) some of which that are very deep, may rise from the warm region of the Earth, where they may be affected by the place, as both these and others may be by Mineral juices and steams (*such, perhaps, as we know nothing of*) though we well know, that some of them that are saline, without being at all sensibly hot, will powerfully resist congelation.

42. But having hinted thus much
on

on this occasion, I shall now proceed to consider, The smoaking of waters drawn from deep places in frosty weather, and show, that it does not necessarily conclude, such water to be warmer in Winter, since that effect may proceed not from the greater warmth of the water in such weather, but from the greater coldness of the Air. For we may take notice, that a mans breath in Summer, or in mild Winter weather, becomes very visible, the cold ambient Air nimbly condensing the fuliginous steams, which are discharg'd by the Lungs, and which in warmer weather are readily diffus'd in imperceptible particles through the air. And I have observed upon the opening of issues in some mens arms, that though no smoak be visible in Summer, it will be very conspicuous in exceeding sharp weather, though mens arms, at least the external parts of them, seem to have less heat in frosty weather, then in Summer; since in the former of those seasons, they are wont to be manifestly more slender, the fleshy parts and juices being condensed

densed by the coldness of the Air. And though the insensible Transpirations, that continually exhale from all the parts of our bodies, are not wont to be visible here, even in Winter; yet in extremely cold Countries, as *Nova Zembla*, or *Charleton Island*, those *Effluvia* have been observ'd, not only to be thickened, but to be turned into ice it self, sometimes within the Sea-mens shooes. And here in *England*, having not long since imployed a labouring man to dig a deep hole in very frosty weather, two Servants of mine, that stood by to see him work, did both of them assure me, when they return'd, that the steams of his heated body, were frozen upon the outside of his Waistcoat, which, one of them, whilst the other was about to give me notice of it, inconsiderately wip'd off.

43. And since we see how fast the water in Ponds and Ditches, wastes and decreases in Summer, there is no cause to doubt, but that it does then continually emit Exhalations as well, if not much more plentifully, then in Winter,

Winter, which may be manifestly confirmed by this, that in the Summer, one shall often see in the mornings or evenings, the face of the water cover'd with a mist or smoak, that rises out of it. And I have sometimes taken pleasure to see this aggregate of Exhalations, hover over the water, and make, as it were, another River of a lighter liquor, that conform'd it self, for a considerable way, to the breadth and windings of the stream, whence it proceeded. And I think it will be easily granted, that the water in Summer time is at least as warm at noon, when such Exhalations are not visible, as in the morning when they are, though the Air be colder at this part of the day, then at that; which observation gives us the true reason of the *Phenomenon*.

44. And though notwithstanding all this, it were made to appear, that in some cases, the smoaking water of Springs may be really warmer in Winter than in Summer; yet a sufficient reason of the *Phenomenon* may be fetch'd from what I have already
deli-

delivered about the detention of the warm subterranean vapours by the frost, and snow, and rain, that make the earth less perspirable in Winter.

45. And because I know *Themistius* will look upon a thing so disagreeable to the vulgar opinion, Of the Coldness of the whole Element of Earth, as a Paradox; I will take this opportunity to add a further confirmation, to what I have been saying.

46. And first, that there arise copious and warm steams from the lower parts of the Earth, may be prov'd, not only by what I have already mentioned, touching the *Hungarian* Mines, but by the common complaint of Diggers in most, though not in all deep Mines, That they are oftentimes troubled, and sometimes endangered by sudden damps, which do frequently so stuff up and thicken the subterranean Air, that they make it not only unfit for respiration, but able to extinguish the Lamps and Candles, that the Miners use, to give them light to work by. And I remember,

member, that I have visited Mines, where having inquired of the diggers, whether those hot exhalations, that compose their damps, did not sometimes actually take fire within the bowels of the Earth, I was answered, that in some of their Pits (and particularly in one, that they show'd me) though not in all, they did, insomuch that the exhalation suddenly kindling, would make a report at the mouth of the Pit like a Musquet, or a small piece of Ordinance, and the flame would actually burn off the hair, and scorch the skins of those workmen, that did not seasonably get out of the Pit, when the exhalation appear'd to be near an ascension, or did not nimbly fall down flat with their faces to the ground, till the flame was gone out. And one of these workmen that I ask'd, affirm'd himself to have been several times, to his no small trouble, so burned, and that (if I much misremember not) twice in one day. And it seems to me as well as to *Morinus* very probable, that those great quantities of rain and snow, and storms, and (perhaps)

haps) some other Meteors, that are taken notice of in Winter, may rather consist of these subterraneal steams, then the vapours and exhalations attracted by the Sun (or at least may as much consist of the former, as the latter.) For his heat is then very languid, and acts upon the ground but during the day time, which is very short (whereas those Meteors are generated indifferently at all hours of the day and night) and the sky is oftentimes, for many days together, quite overcast with clouds, and the surface of the ground so constipated with frost, that it will sometimes freez even in the Sun-shine: So that 'tis not near so likely, that the heat of the Sun, in the midst of all these disadvantages, should be able to elevate so great a plenty of exhalations and vapours, as are requisite to compose the rain, and snow, and storms, that sometimes last almost all the Winter, as that they should be suppli'd by subterraneal steams copiously sent up from the heat that continually reigns in the lower parts of the Earth, and by traversing the

D d d Sea,

Sea, and at other vents, get up into the Air.

47. To make out this, my formerly quoted French Author relates a very memorable thing, that was told him by the Masters of thole Mines in *Hungary* (which are at least as deep as any that I remember I have seen or read of;) namely, that the Miners were able certainly to foretel sooner than any other mortals, the Tempests and sudden mutations, that were to happen in the Air. For when they perceived by the burning blew of their Lights, and by other manifest signs, that they could easily take notice of in their Grooves, that store of the Tempestuous Damp (if I may so call it) was ascending from the lower parts of the Earth, though the sky above were clear, and the Air calm; yet they could assuredly foretel the approach of a storm, or some other great alteration in the Air, which would accordingly ensue within no very long time after. And to confirm this Narrative, I shall add, not only that 'tis agreeable to what I lately told you was affirm'd to me by other

other Mine-men, but that having enquir'd of a very ingenious Physician, who liv'd many years in *Cornwall*, (a Country you know famous for Tin-Mines, some of which are infamous for the damps that infest them) he told me, that divers of the experienced Fishermen assur'd him, that oftentimes they did perceive fires shining in the night, sometimes in one place, sometimes in another, which were suppos'd to be kindled by the sulphurous and other subterraneous exhalations, and that, when they perceiv'd those fires, (especially if any number appear'd in several places) those that were well acquainted with the coast, would not continue long out at Sea, but rather quit an opportunity of catching Fish, then not make seasonably to the shore, having often observed, and particularly this last year, that bold and unexperienced Mariners, by slighting these forerunners of storms, were in few hours shipwrack'd by them.

48. To this I shall add, what happened some years since, upon the *Irish* coast, near a strong Fortress,

D d d 2

called

called *Duncannon*, where divers of the ships Royal of *England* lying at anchor, in a place where they apprehended no danger from the wind, there seem'd suddenly to ascend out of the water, not far from them, a black cloud, in shape and bigness not much unlike a Barrel, which mounting upwards, was not long after follow'd, as the most experienced Pilot foretold, so hideous a storm, as forc'd those ships to go to Sea again, and had like to have cast them away in it. And this account was both written by the principal officers of the Squadron, to their superiors in *England*, and given soon after it happened, by the chief of those eye-witnesses (and particularly by the Pilot) to a very near kinsman of mine (well vers'd in Maritime affairs) that commanded the land forces in those parts, as a truth no less known then memorable.

49. And on occasion of what I was saying, about the eruption of hot steams, in several parts of the Earth, I now call to mind something that I have met with in a very small, but

curi-

curious Dissertation, *De admirandis Hungariæ aquis*, whole Anonymous Author I gather from some passages in the Tract it self, to have been a Nobleman, Governor of *Saros*, and some other places in *Hungary*, and to have written this

discourse, both for, and to that inquisitive German, Baron *Sigismundus Liber*, famous for the ac-

Quia verò in Comitatum Zoliensem, dum aquas persequimur, ventum est, non possum praterire hiatum terræ iisdem in locis famosum ob pestilentes expirationes, quibus Aves supervolantes, & quævis alia animantia extingui constat, manifesto eorum experimento, qui, &c. Pag. 74.

count he gave the world of the Ambassy, whereon he was sent by the German to the Russian Emperor. This Anonymous, but noble writer, tells us then, that in that part of *Hungary*, which he calls *Comitatus Zoliensis*, there is a gaping piece of ground, which does emit such mortal expirations, that they suffocate, not only Cats and Dogs, purposely held at the end of long poles over the cleft, but kill even Birds, that attempt to fly over it. And in other places of the same Tract, I have met with many other relations, which if I had time to make a particular mention of,

would much countenance what I have been lately saying : but though I pretermitt several other instances, I cannot but take especial notice of one, which (together with what I lately mention'd to have happened near *Duncannon*) may make it probable, that not only under the surface of the dry ground, but in that part of the Terrestrial Globe, that is covered with water, there may arise streams (and consequently Exhalations) *actually*, and that *considerably*, hot. For

Ibidem est sub dio fons calidarum cæteris amplior, quem Purgatorium vocavere, ea nimirum ratione, quod, quamadmodum proditum est in purgatorio pœnas nocentium pro nozarum modo, alias acerbiores alias mitiores, ita quedam insunt Aquæ hoc in fonte discrimina, namquâ in eum à Danubii ripâ aditus est, subfrigida primum, mox tepida, & quo in eum penetratis altius hoc magis calet. In recessu vero interiore tam est calida, ut ferri non possit. Est etiam is calor haud dubie aquæ hujus proprius; nam alia, quæ dixi, temperamenta verisimile est à Danubio accedere, qui crepidinem hujus fontis lambit, & cum vel modicè excrescit, totum inundat, neque tamen ita restinguit, quin caleat. Quin intra ipsam ripam, qua Danubio perennis cursus est, calidæ ebulliunt, ubi qui altius mergi volunt lavare consueverunt. Pag. 57.

in one place he takes notice, that, not far from the well known City of *Buda*, there is a hot Spring (which they call *Purgatory*) which the waters of *Danubius* it self are not able to keep from being hot; nay, within the very Banks, betwixt which that great River runs, there

there boil up hot Springs, where those that will go deep enough into the water, may commodiously bath themselves. And elsewhere speaking of the River *Istrogranum*, in the same County, he adds, That not only the Banks of

it, but within the very River it self, one may discover hot Springs, by re-

Neque in ripâ tantum eruantur Calida, sed etiam intra amnem, si fundum ejus pedibus suffodias. Calet autem immodicè, nec sunt Idoneæ balneis, nisi temperentur, quod Admissione frigida de proximo haustæ in proclivi est.
Pag. 65.

moving the Sand at the bottom with ones feet. To this I shall add, That having heard of a Ditch in the North of *England* (in some regards more strange, though less famous than the sulphureous *Grotta* near *Naples*) whence not only subterranean steams, but those so sulphureous, as to be easily *Inflamable*, did constantly and plentifully ascend into the Air, I had the curiosity to make inquiry about it, of the Minister of the place, (a very learned Man, and conversant in Mines) who then happened to be my neighbour, and he attested the truth of the relation upon his own knowledge. And it was confirm'd to me

by a very ingenious Gentleman, who went purposely to visit this place, and found it true, That a lighted Candle, or some such actually burning body being held where this Exhalation issued out of the Earth, would kindle it, and make it actually flame for a good while, and (if I misremember not) as long as one pleas'd. And as this place was but few years since taken notice of, so there may be probably very many others, yet undiscovered, that may supply the Air with store of Mineral exhalations, proper to generate fiery Meteors and Winds; I remember, that having lately ask'd an inquisitive Gentleman, that is a great searcher after Mines, whether he did not observe some meteors near those places, where he is most conversant, he told me, that 'tis very usual in some of them, to see certain great fires moving in the Air, which in those places, diggers, because of some resemblance (real or imaginary) are wont to call Dragons. [And the Russian Emperors Phyfician, you were speaking of, inform'd me a while since,

since, that he had, not long ago, observ'd in Winter a River in *Muscovy*, where though the rest of the surface was frozen, there was a part of it near a mile long, that remain'd uncovered with ice, which probably was kept from being generated there by those subterraneous Exhalations, since he says he saw them ascend up all the way like the smoak of an Oven.] And in case the matter of

fact delivered by *Olavus Magnus* be true, concerning the strange thaws that sometimes happen, with terrible noises, in the great Lake *Veter*, those wonderful *Phænomena*, may not im-

Nec prætereundum hic puto Lacum esse LX. miliarum in longitudine, & XX. in latitudine Italicorum, Veter appellatum in Regno Ostrogothorum, quæ talis est natura, quod cum tempestuoso vento congelatus fuerit, & tempus resolutionis immineat, vehementissimo strepitu incipit fundo ebullire & commoveri, magna violentiâ perumpere in parvas rimas, vel scissuras, quæ fiunt in glacie, & has in modico temporis spatio faciens valdè latas, licet pro tunc glacies in spissitudine habuerit, plusquam unum, vel duo brachia. Lib. primo, pag. 23.

probably be ascrib'd to the ascent of great store of hot subterranean steams, which suddenly cracking the thick and solid ice in many places at once, produce the hideous Noises, and the hasty Thaw that he speaks of. And this suspici-

on

on may be countenanced partly by this circumstance, that before these sudden thaws, the Lake begins with great noise to boil at the bottom, and partly by what is related by a more Authentick writer, I mean, that learned Traveller the Jesuite *Martinius*, who witnesses, that at *Peking*, the royal City of *China*, 'tis very usual, that after the Rivers and Ponds have continued hard frozen over, during the Winter, the Thaw is made in one day; which, since the freezing of the waters (as he tells us) required many, makes it very probable, That the sudden thaw is effected (as he also inclines to think) by subterranean steams, which I may well suppose to be exceeding copious, and to diffuse themselves every way to a very great extent, since they are able so soon to thaw the Rivers and Ponds of a large Territory, and that (which makes mainly for my present purpose) beginning contrary to vulgar thaws, from the bottom upwards.

50. And having thus manifested, that the lower parts of the Earth do send up great store of Exhalations
and

and Vapours to the upper parts, it will be obvious to conceive, that as in divers places of the Terrestrial Globe, these steams get into the Air, either by the advantage of finding vents, such as those I have already mentioned, or by growing copious enough to force themselves a passage: So in most other places, where the ascending steams find no commodious vents, or are too faintly driven up to gain themselves a passage, they must be repress'd or detain'd beneath the surface of the Earth, which has its pores in Winter usually choak'd up with snow or rain, or its surface constipated and hardened with ice or frost, so that these exhalations being pent up, and receiving fresh supplies, from time to time, from beneath, 'twere no wonder, if they should somewhat warm deep Cellars and Wells, where they are thus detain'd; and therefore our Husbandmen do not speak altogether so improperly, when they say, that the snow keeps the ground warm. And I remember, that Dr. *Smith*, the learned English Ambassador into *Musco*,

Musco, makes it to be one of the principal reasons of the great fertility, he justly ascribes to the Country there about, that during almost all the Winter, the ground is to a great height covered with snow, which does not only enrich it by the fertilizing salt, which the Earth gains from the snow, when that comes to be melted, but does also contribute to its improvement, by choaking up, or obstructing the pores, at which the Nitro-sulphureous, and other useful Corpuscles, that are sent up by the subterranean heat, would easily get away. And least (Gentlemen) you should think, that 'tis only by the Ratiocination, that I conclude, that there is really great store of warm steams detain'd under ground in the Winter: I shall add this sensible observation, receiv'd from the *Russian* Emperors Physician already often mention'd, by whom I have been assured, that about *Musco*, where the surface of the ground is far more constipated in Winter, this 'tis in these parts, and where they are wont to keep their Cellars much closer, the

the subterraneous Exhalations being hinder'd to fly abroad, will in time multiply so fast, that he assures me, that upon the unwary opening of the doors of Cellars, that have been long kept shut, there would fall out a warm smoak, and very thick, almost like that of a furnace, and sometimes the steam that issues out will be so gross and plentiful, that it has brought men into danger of being suffocated by it.

§ 1. And now, Gentlemen, having shown, that though Experience be so confidently appeal'd to, by the maintainers of *Antiperistasis*, yet she has not hitherto afforded them any thing, that much favours their Cause, it remains, that I show, that she bears witness against it. For besides that some passages of my late Discourses do really contain *Phænomena*, that not only do not favour *Antiperistasis*, but may justly be imploy'd as Experiments against it, I shall *ex abundanti* (as they speak) present you with something, which I necessitated Experience to supply me with, that seems expressly to overthrow it.

52. I might urge against those, who, though they begin to be ashamed of the Doctrine of the Schools, would establish an *Antiperistasis* upon the account of what they call a *fuga Contrarii*, that the very instance they are wont to bring for their opinion, may be retorted upon them. For when they tell us, that in Winter, the heat, to fly the cold of the external Air, retires it self into the lower parts of the Earth, and there harbours in Cellars and Wells, as may be prov'd by the smoaking of water drawn from deep Wells, which argues its heat, the vapours which fly away, being, as vapours, hot in comparison of the outward Air; we may easily answer, by demanding, why, if the heat, that was harbour'd in a smoaking Bucket of water, have the wit or instinct to fly from its Contrary, it does not in the Bucket, as 'tis said to do in the Well, retire it self as far as it can from the surrounding cold of the ambient Air, but instead of retiring to the innermost parts of the water (those being remotest from that) it needlessly flies abroad, with
the

the vapours it excites, and does, as it were, of its own accord cast it self into the arms of the enemies it should shun. And indeed what I just now mention'd to you, as related to me by the great Duke of *Muscovies* Physician, does sufficiently manifest, that the cause, why the Corpuscles, that keep Cellars warm, abide beneath the surface of the Earth in Winter, is not that they fly the cold as their enemy, but that they are pent up beneath the ground, since, when vent is given them, they immediately rush into the open Air, without fearing the cold even of *Russia* in the very midst of Winter.

Dr. Sam.
Collins.

53. But I shall press this no further, but rather add, that the doctrine of *Antiperistasis* is as little beholding to the following Experiment, which I sometimes tri'd, in order to the disabusing some Abettors of *Themistius*. I took then an Iron-rod, of about the bigness of a mans finger, having at one end of it a very broad and thick piece of Iron (shap'd almost like a spattule) that the quantity of the matter, might up-
on

on the ignition of the Iron, make the heat very considerable: then having caus'd this thick end to be made red hot in the fire, and having suddenly quench'd it in cold water, I could not perceive, that the other end of the rod, by which it was wont to be held, did at all grow sensibly hot, as a favourer of *Antiperistasis* would have expected it should do to a very high degree, as presuming, that the innumerable particles of heat, that swarmed in the compact body of the red hot part of the Iron, must, to fly the cold of the water, retire in throngs towards the other extreme of the Iron, and make it exceedingly hot. And least any preexistent warmth should hinder me from perceiving an increase of heat, in case any were produc'd in the handle of the Iron, I caus'd it the next time the Trial was made, to be kept in cold water, and yet even then the immersion of the broad and candent end into the cold water, brought as little of sensible heat to the other end, that I held in my hand, as it had done the time before, and having caus'd the

Expe-

Experiment to be tri'd by another, the account I receiv'd was, that it succeeded with him, as it had done with me.

54. But this is not the main thing (Gentlemen) that I intended to acquaint you with, there being an Expedient, that I purposely devised to make *one Experiment*, more considerable against *Antiperistasis*, then are the *several mistaken observations* of the *Peripetaticks* to establish it.

55. I took then a good seal'd Weather-glass, 12. or 14. inches long, furnished with good spirit of Wine, and having provided an open mouth'd glass of a convenient shape and size, and fill'd it but to a due height (that it might not afterwards run over) with common water, I so ordered the matter, that the stem of the Thermoscope being supported by the cork, into which by a perforation or slit it was inserted, when the glass was stopp'd by the cork, the whole ball of the Thermometer was immers'd in the water, that fill'd the wide mouth'd glass, and did nowhere touch either the bottom or the

E e e

sides

sides of the glass, so that the ball or bubble was every way surrounded with water. The instrument being thus prepar'd, we observ'd at what station the ambient cold water had made the tinted spirit rest in the stem of the Thermoscope, and then having provided a fit proportion of warm water in a commodiously shaped vessel, I remov'd the instrument into it, and plac'd it so, as that the external warm water reach'd to a convenient height on the outside of the open mouth'd glass: But though I carefully watch'd, whether the heat of the external water, would increase or strike inwards the cold of that water, which did immediately incompass the ball of the Weather-glass; yet I perceived no such matter, the tinted spirit in the stem keeping its station (without sinking beneath it) till the heat, after a while, having by degrees been diffus'd through the formerly cold water, by the intervention of that now warmed, the tinted spirit in the Thermometer began to ascend.

56. And to reduce the other part too,

too, of the doctrine of *Antiperistasis*, to the determination of an Experiment, the same Thermoscope was plac'd in the same wide mouth'd glass just after the former manner, only instead of the cold water, that, which immediately surrounded the glass, was warm, and when the warmth had impell'd up the tinted spirit, till its ascent began to be very slow, I immers'd the instrument to a convenient depth in a vessel, that contain'd highly refrigerated water, mingled with divers pieces of ice, But notwithstanding my watchfulness, it did not appear to me, that the warmth of the water, that did immediately encompass the ball of the Weather-glass, was at all increas'd or intended, by that Liquors being besieged by water exceeding cold; for the languid motion of the tinted spirit upwards, was not hereby so much as *sensibly* accelerated (as it must have been *considerably*, if the heat of the internal water had been so augmented, or struck inwards by the cold of the external, as the Schools Doctrine would have made one expect) but

rather the ascent was by the chilling-
ness of the contiguous water quickly
check'd, and the formerly ascending
spirit was soon brought to subside
again. And to give my self the ful-
ler satisfaction about some of the
chief *Phænomena* of this, and the for-
mer Experiment, I had the curiosity
to observe them more then once.

POST-



POSTSCRIPT.

A Sceptical Consideration of the Heat of Cellars in Winter, and their Coldness in Summer.

THe foregoing Discourses of *Carneades* seem to have sufficiently shaken the Foundations of the Vulgar Doctrine of *Antiperistasis*, so far forth as 'tis superstructed upon the Vulgar Observations and *Phænomena*, whereon men are wont to build it; and it seems to have also made it highly Probable, that in case some of the Examples wont to be produc'd in favour of *Antiperistasis*, should prove Historically true, yet those *Phænomena* may more congruously, to the wonted proceedings of Nature, be explicated by the detention of calo-

rifick or frigorifick Corpuscles, by
 the operation of the external cold or
 heat, then to a certain inexplicable
 self invigoration, which is commonly
 propos'd in such a way as invests in-
 animate bodies with the prerogatives
 of free Agents. But though *Carnea-*
des his Adversaries seem not to have
 well made out the Historical part of
 the receiv'd Doctrine concerning
 cold, yet upon an impartial survey
 of what has been alledg'd on both
 sides, I freely confess, that to me
 some of the matters of fact them-
 selves seem not yet so clearly deter-
 mined as I could wish: for as to the
 obvious *Phænomena*, that nature does,
 as it were, of Her own accord pre-
 sent us, they seem to have been but
 perfunctorily considered, and our
 senses only being the judges of them,
 we may easily, as *Carneades* argues,
 be impos'd upon by the unheeded
 predispositions of our Organs. And
 as for contriv'd and Artificial Expe-
 riments, there scarce seem to have
 been any made fit to clear the diffi-
 culties, that invite me to suspend my
 judgement as to the grand Question
 (of

(of fact) whether Cellars, and other subterraneous places be really hotter in Winter then in Summer.

'Tis true, that I have scarce met with any point, wherein the modern Schoolmen seem to have so much consulted Nature, as in this of *Antiperistasis*. For inquiring what has been written of that subject, that may either confirm or oppose what has in the precedent Dialogue been deliver'd about *Antiperistasis*; I found that the curiousness and importance of the subject have made two or three of thole writers less negligent then I suspected. But though I have lately met with in them an Experiment or two, that seem cogently to evince, I do not say an *Antiperistasis* in the sense of the Schools, but, that subterranean places are really hotter in Winter then in Summer, yet I must for a while longer continue my suspension of judgement, which, that even such persons as are circumspect themselves, may not think unreasonable, I will briefly subjoyn the grounds of my Scepticism about this matter.

First then the learned Jesuite *Zucchi*, who is wont to be far more industrious than other *Aristotelians* (and on some subjects is careful to propose Experiments, though he be not so clear and happy in expressing his thoughts) assures us somewhere, that having kept a good seal'd Weather-glass, for three years together in a good Cellar, he found the water to rise by the Coldness of the ambient Air in the Summer, and to be depressed by the rarefaction of it in the winter; which seems undeniably to infer, that whatever be the reason of it, the heat in subterranean places is indeed greater in Winter than in Summer. And another recent Schoolman, who, as I am told, is of the same order, though the learned Man publish'd his little Book under one of his Disciples Names, affirms, that he found by a Weather-glass, that a Well at the place where he lived, was colder in Summer and hotter in Winter. And these assertions of *Zucchi*, and the other Jesuite, do I confess restrain me for a while from yielding a full assent to what

Carneas

Carneades hath delivered, as to the matter of subterranean Cold and Heat. But on the other side, I am not hitherto reduc'd by these Experiments, to declare with his Adversaries against him, because of the following scruples.

First then I consider, that 'tis not universally true, which is wont to be indefinitely affirm'd, and believ'd, that Cellars and other subterranean places are hotter in Winter than in Summer. For the instances produced by *Carneades*, seem plainly enough to manifest the contrary, and my own observations made in a Cellar with a seal'd Weather-glass, do keep me from dissenting from *Carneades* as to that point. I would therefore make a distinction of subterranean places; for some are deep, as the best sort of Cellars, other deeper yet, as the *Hungarian* Mines, mention'd by *Carneades* out of *Morinus*; and some again are but shallow, as many ordinary Cellars and Vaults: of these three sorts of subterranean Places, the deepest of all do not, as far as the Authority of Mineralists above alledg'd

alleg'd may be reli'd on (for I am yet inquiring further) grow hot and cold, according to the several seasons of the year, as the vulgar doctrine of *Antiperistasis* requires, but are continually hot: The shallower sort of subterranean places, though by reason of their being fenc'd from the outward Air, they are not so subject to the alterations of it, whether to heat or cold, as open places are, yet by reason of their vicinity to the surface of the Earth, they are so far affected with the mutations, which the outward Air is liable to in several seasons of the year, that in Winter, though they be warm in respect of the colder Air abroad, yet they are really (at least some of them) as far as I have tri'd, colder in very cold weather, and less cold in warm weather. And in this opinion, I am confirm'd by two things; the one, that having purposely inquir'd of the *Polonian* Nobleman mentioned by *Carneades*, whether he had observ'd in his Country, that in sharp Winters small Beer would freez in Cellars, that were not very deep, but would continue fluid
in

in those that were, he assured me he had taken notice of it: The other thing is the Confession of the Anonymous Jesuite lately mention'd, who acknowledges, that he found but little difference between the Temperature of the water in the Well he examin'd in Summer and in Winter, though it were a considerably deep one, and adds a while after, that at *Florence*, where the subterranean Vaults are shallower, the Air is observ'd to be colder in Winter than in Summer, though at *Rome* in their deep Cellars the contrary has been found. So that the lower-most sort of subterranean cavities being, for ought appears, perpetually hot, and the upper or shallower sort of them, being colder, not hotter in cold weather than 'tis in warm, 'tis about the Temperature of the middle sorts of them, such as are the deeper and better Cellars, that the question remains to be determined. And thus much of my first consideration.

The next thing I shall offer to be consider'd is this, That 'tis not so easy a matter, as even Philosophers
and

and Mathematicians may think it, to make with the weather-glasses hitherto in use, an Experiment to our present purpose, that shall not be liable to some exception, especially if the Cellars or Wells, where the observations are to be made, be very deep. For the gravity of that thick and vapid subterranean Air, and the greater pressure, which the Air may there have, by reason of its pressing, according to an Atmospheric Pillar lengthened by the depth of the Cellar or Well, may in very deep Cavities, as well alter the height of the water in common Weather-glasses, as heat and cold do, and so make it uncertain, when the mutation is to be ascrib'd to the one, and when to the other, or at least very difficult to determine distinctly, what share is due to the pressure, and what to the temperature of the Air. And this uncertainty may be much increas'd by this more important Consideration, that not only in places where the heights of the Atmospheric Cylinders are differing, the pressures of the Air upon the stagnant water in the Weather-

Weather-glasses may be so too, but even in the self same place the instrument remaining unmov'd, the pressure of the Atmosphere may, as I have often observ'd, hastily and considerably alter, and that without any constant and manifest cause (at least that I could hitherto discover,) so that the erroneous estimate, that may be hereby suggested of the temperature of the Air can scarce possibly be avoided, without the help of a seal'd Weather-glass, where the included liquor is subject to be wrought upon by the heat and cold, not pressure of the Air. So that to apply this to *Zucchi*'s his Experiment, unless he had been aware of this, and unless I knew, that he had divers times made his observations, with the assistance of a seal'd Weather-glass, it may be suspected, that he might accidentally find the water in his common Weather-glass (for such a one it appears he us'd, as probably knowing no other) to be higher, when he look'd on it in Summer, then when he look'd on it in Winter, not because really the subterranean
Air

See the
second
Prelimi-
nary dis-
course, that
accompa-
nies the
History of
Cold.

Air was colder in the former season, then in the latter, but because the Atmosphere chang'd then to be heavier: and when I remember in how few hours I have sometimes, and that not long since, observ'd the Quicksilver, both in a good Barometer, and even in an unseal'd Weather-glass furnished with Quicksilver, to rise almost an inch perpendicularly, without any manifest Cause proceeding from cold, I cannot think it impossible, that in long Weather glasses furnish'd only with water, or some such liquor, the undiscerned alterations of the Atmospheres pressure, may produce very notable ones in the height of the water in such instruments. But this is not all, that a jealous man might suspect. For *Zucchi* having, for ought appears, made his Observations but in one place, we are not sure, but that may be one of those, whereof there may be many, on which the subterranean Exhalations have a peculiar, and not languid influence; as *Carneades* has towards the close of his Discourse made probable, out of the Relations
of

of *Olaus Magnus*, and *Martinius*, touching the great and sudden thaws, that sometimes begin from the bottom; and thereby argue their being produc'd by copious steams, that ascend from the lower parts of the Terrestrial Globe, which may be further confirm'd, by what he formerly noted of the sudden Damps, that happen in many Mines. But that which is of the most importance about our present inquiry, remains yet to be mentioned, which is, that having had the curiosity to inquire, whether no body else had made Experiments of the same kind; I find, that the learned *Maignan* had the same curiosity that *Zucchi* had, but with very differing success; and therefore, though this inquisitive person do admit in his Disputation about

Antiperistasis, a Notion, that I confess I cannot approve, (since to as-
cribe, as he does, a *fuga Contrarii* to

be somewhat confirmed by what I lately learned from the English Extraordinary Ambassador (the Earl of Carlisle) into Russia, newly returned thence. For meeting the other day with an opportunity of asking his Lordship a few Questions (which he was pleased to answer with his wonted civility) about the Cold in Muscovy: I was informed by one of his answers, That his Excellency had there the curiosity to observe some Bottles of choice

My backwardness to admit a fuga Contrarii, may

and

and strong Wine, that were vehemently frozen, and the opportunity to take notice, that the liquor was quite congeal'd throughout, and turned into solid ice, whence he rationally inferred, that the spirituous parts of the Wine did not in these Bottles (for ought he acknowledg'd, that in greater vessels, that may sometimes hold true, which is said of the production of spirit of Wine by congelation) retire to the Center, and remain there unfrozen; and his Lordship ingeniously persued the Experiment, and confirmed the conjecture, by causing the ice taken out of the broken Bottles to be thawed by degrees into several vessels, by which means he found, that the liquor afforded by the exterior parts of the resolved ice, was very little, if at all less strong, then that which was obtained from the internal parts of the same ice; from which Observation Carneades would argue, That at least 'tis not universall, but in particular cases, and therefore probably by accident, or upon particular accounts, that the Concentration of the spirits of Liquors is consequent upon being exposed to Cold.

Cold and Hot spirits, is in my apprehension to turn inanimate Bodies into intelligent and designing Beings;) yet he does justly and rationally reject with Carneades, the vulgar doctrine of *Antiperistasis*, and confirms his rejection of it by two Experiments. For first, he says, that he found with a Thermometer, that when in Winter a cold Northerly wind froze the water without doors, it was not less cold in Wine-Cellars, then 'twas at the same season, and at the same hour of the day in his Study only the Paper-shuts of his window, that regarded likewise the

the North, being put to. And though, if he had said nothing else. I should have suspected, that this might have proceeded from the shallowness of the Cellars he made his Trial in, yet he prevents that suspicion, by taking notice in one clause of his Relation, that the Cellars were of the very best of their kind, in which in Summer the greatest Cold was wont to be felt. But his next Experiment is yet more considerable, which I shall therefore deliver in his own words that follow.

Expertus ego sum (says he) Thermometro fidelissimo, & à precedente hyeme in sequentem æstatem prorsus invariato, instructo etiam tali aquâ, nempe in hoc ipsum ex præscripto Trebellii, ita comparata ut non exhaletur, neque minuatur, expertus (inquam) sum in supradictis optimis Cellis Vinariis maximum, quod ardentissima æstate fuit, frigus, non adæquasse illud quod ibidem erat brumali tempore, ut dixi in superiori Experimento, siquidem in Tubo Vitrei Thermometri quatuor circiter palmos longo, & in octo gradus Graduumque minuta diviso, aqua hyeme ascendit ad gradus 7. cum semisse,

I presume
he means
Cornelius
Drebell.

F f f æstate

estate autem vix gradum Sextum superavit, cum tamen ad sensum multo magis vigeret frigus istud æstivum.

Thus far this learned, as well as resolute Author, who seeming by the Mathematical part of his *Perspectiva Horaria*, to be an accurate and industrious maker of observations, we may oppose his newly recited Experiment to that of *Zucchius*, which it flatly contradicts; and therefore *since* the depth of the Cellars is of great moment in Experiments of this Nature; *since* also the particular Nature of the place or soil, where the Cellar or other Cavities happen to be, may in some cases not be inconsiderable; and *since* lastly, neither *Zucchius* nor *Maignan* seem to have been aware of the differing weights of the Atmosphere, in the self same place, (as not having seen the XVIII. of our *Physico-mechanical Experiments*, before which I never saw nor heard of any thing publish'd, or otherwise written to that purpose) I hope I shall be excus'd, if I retain some scruples about the Historical Question I have been considering, till the
Experi-

Experiment have been carefully made, for a competent space of time in several places, and that not with common Weather-glasses (like those us'd by my two learned Authors) wherein the liquor may be made to rise and fall by the differing gravities of the Air, but with seal'd Thermoscopes, wherein the alterations may more safely be suppos'd to proceed only from its heat and cold.

And to conclude, since *Carneades* has speciously enough answered the other Observations, that are wont to be produc'd in favour of the *Aristotelian Antiperistasis*, if *Maignans* relation be better warranted by future Experiments, then that of *Zucchi*, it will very much disfavour the whole Doctrine it self, which seeming to have been devis'd, but to give an account of the *Phænomena*, to which 'tis wont to be appli'd, considering men will be but little invited to imbrace it, if the *matter of fact* be as little Certain as what is propos'd in the *Hypothesis* is Intelligible.

FINIS.

Experiment have been carefully
made, for a comparison of the
in several places, and that not with
common Weather-glasses, but those
used by my two learned Authors
wherein the liquor was, by means of
ells and tall pyric burning glasses
of the Air, but with little success
scopes wherein the observations
more likely be supposed to proceed
only from heat and cold.
And in conclusion, since Cavendish
has specifically shown, that the
other Observations, that are wont to
be produced in favour of the Air,
than Necessity, if it be true, that
on the better warranted by future Ex-
periments, than that of Exclusion, it
will very much disprove the whole
Doctrine it self, which seeming to
have been desired, but to give an ac-
count of the Phenomenon, to which it
wont to be applied, considering men
will be but little inclined to imbrace
it, if the manner of proof be as little
Certain as what is proposed in the
Hypothesis is intelligible.

F I N I S

AN
ADVERTISEMENT.

THe Author of the following Discourse intending it should make a part of certain Considerations upon the four famousst Hypotheses, or Opinions, of the Nature and Cause of Cold; which (Considerations) he thought fit to reserve for the latter end of the History of that Quality, was invited to suppress it ever since the former part of the year, that preceded the last. And though this Discourse, (both for other Reasons, and because he found it more ready and finished, than some other Papers, that belonged to the same part of the newly mentioned History) comes abroad unaccompanied; yet he judged it not amiss, to intimate thus much, That the Reader may be informed,
fff ed,

ed, upon what Account Mr. Hobs
Opinions come to be examined in
Historical Treatise; and may not
wonder, either to find, that diverse
passages of It are omitted, that are un-
favourable enough to Mr. Hobs's Do-
ctrine, or to meet with in a Discourse
postpon'd to the History of Cold, some
Experiments, that seem to argue it
have been written before they were ca-
into the Order, wherein they now appear.
To this I have nothing to add, but that
whereas through haste the Scheme refer-
red to in the long citation out of
Mr. Hobs's, has not been added to the
others, that belong to this Book, I am
not much troubled at the Omission, (as
also that in other Quotations the place
not always as well mentioned as the
words,) because, that if any shall be
found, that after having considered
what I urge against the (Great, but
Imaginary) Interest, Mr. Hobs would
ascribe to winds (whether he explicates
their causes rightly or not) in the Pro-
duction of lesser degrees of Cold, but
(how improbably soever) of congelation.

Hobbes
ed in
ay a
dis
are
os's
discom
d, for
ue it
ere
appea
but th
ne refer
out
ed to
I
on, (
place
as
shall
sider
at,
os wo
expli
the P
ld,
geat

Self, shall think the sight of that
Scheme of any Importance: this Learned
Mans Book De Corpore, is in so many
ands, that any Reader that shall desire
t, may very easily have an opportunity to
onsult the Scheme in the particularly ci-
ed place.

AN

to sell. I shall think the right of that
scheme of any importance: this I intend
to say. Book De Corporat. is a very
handsome, that any reader that shall come
to may very easily have an opportunity to
consult the Scheme in the particularly ci-
ted place.

AN



AN
E X A M E N
OF

Mr. *Hobs's* Doctrine,
touching *Cold*.

Mr. *Hobs's* Theory concerning Cold, does to me, I confess, appear so inconsiderately pitch'd upon, and so slightly made out, that I should not think, it merited, especially in an Historical Treatise, a particular or solicitous Examination, but that in proposing it, he scruples not to talk to his Readers of his Demonstrations; and the preference, he is wont to give himself above the Eminentest, as well of Modern as of Ancient Writers, has had no small effect upon many, who not knowing how indulgent some writers are wont

to be, to the issues of their own brain, as such are apt to mistake Confidence for Evidence, and may be modest enough to think, that their not discerning a clearness in his Explications and Reasonings, is rather the fault of their Understandings, then of his Doctrine. Mr. *Hobs* delivers his Theory in the seven first Articles of the 28. Chapter of the fourth part of his Elements. But because the whole discourse is too long to be here transcrib'd, and because in the 2, 3, and 4. Sections, that which he treats of, is the generation of winds, and that which he handles in the fifth, is the notion of a hard body; we may safely leave out those four Sections, especially since, though there be in them divers things about the motion of the Sun, and other matters, that are more strongly asserted then prov'd, yet his doctrine tending but to shew how the winds are generated, though it were granted, would make but very little, if any thing at all, towards the evincing of his Theory about cold.

2. And that we may not be suspected

to

to injure his opinion or his arguments; we will, though the Citation will be somewhat prolix, first recite them as himself delivers them in those three Sections, that treat immediately of Cold, and then we will subjoyn our Animadversions on them.

3. [These things (says he) being *Artic. 6.*
 premis'd, I shall shew a possible cause, why there is greater cold near the Poles of the Earth, then further from them. The motion of the Sun between the Tropicks, driving the Air towards that part of the Earths superficies, which is perpendicular under it, makes it spread it self every way; and the velocity of this expansion of the Air grows greater and greater, as the superficies of the Earth comes more and more to be straitned; that is to say, as the Circles which are parallel to the *Aequator* come to be less and less. Wherefore this expansive motion of the air, drives before it the parts of the air, which are in its way continually towards the Poles more and more strongly, as its force comes to be more and more united, that is to

say, as the Circles which are parallel to the *Aequator* are less and less; that is so much the more, by how much they are nearer to the Poles of the Earth. In those places therefore which are nearer to the Poles, there is greater cold, then in those which are more remote from them. Now this expansion of the air upon the superficies of the Earth from East to West, doth by reason of the Suns perpetual accession to the places which are successively under it, make it cold at the time of the Suns rising and setting, but as the Sun comes to be more and more perpendicular to those cooled places, so by the heat, which is generated by the supervening simple motion of the Sun, that cold is again remitted, and can never be great, because the action by which it was generated was not permanent. Wherefore I have rendred a possible cause of cold in those places, that are near the Pole, or where the obliquity of the Sun is great.

Artic. 7. 4. How water may be congealed by Cold, maybe explained in this manner. Let *A*, (in the first figure) represent

present the Sun, and *B.* the Earth; *A.* will therefore be much greater then *B.* Let *E. F.* be in the plain of the *Æquinoctial*, to which let *G. H. I. K.* and *L. C.* be parallel. Lastly, let *C.* and *D.* be the Poles of the Earth. The air therefore by its action in those parallels will rake the superficies of the Earth; and that with a motion so much the stronger, by how much the parallel Circles towards the Poles grew less and less. From whence must arise a wind which will force together the uppermost parts of the water, and withal raise them a little, weakening their endeavour towards the Center of the Earth. And from their endeavour towards the Center of the Earth, joyned with the endeavour of the said wind, the uppermost parts of the water will be press'd together and coagulated, that is to say, the top of the water will be skinned over and hardened, and so again the water next the Top will be hardened in the same manner, till at length the ice be thick. And this ice being now compacted of little hard Bodies, must also contain many particles

ticles of air receiv'd into it. As Rivers and Seas, so also in the like manner may the Clouds be frozen: For when by the ascending and descending of several clouds at the same time, the air intercepted between them is by compression forced out, it rakes, and by little and little hardens them. And though those small drops (which usually make clouds) be not yet united into greater bodies, yet the same wind will be made, and by it, as water is congealed into ice, so will vapours in the same manner be congealed into snow. From the same cause it is, that ice may be made by art, and that not far from the fire: for it is done by the mingling snow and salt together, and by burying in it a small vessel full of water. Now when the snow and salt (which have in them a great deal of air) are melting, the air which is pressed out every way in wind, rakes the sides of the vessel; and as the wind by its motion rakes the vessel, so the vessel by the same motion and action congeals the water within it.

5. We find by Experience, that cold
is

is always more remits in places where it rains, and where the weather is cloudy (things being alike in all other respects) then where the air is clear. And this agreeth very well with what I said before ; for in clear weather the course of the wind, which (as I said even now) rak'd the superficies of the Earth, as it is free from all interruption, so also it is very strong. But when small drops of water are either rising or falling, that wind is repelled, broken and dissipated by them ; and the less the wind is, the less is the cold.

6. We find also by experience, that in deep Wells the water freezeth not so much, as it doth upon the superficies of the Earth. For the wind by which ice is made, entring into the Earth (by reason of the laxity of its parts) more or less loseth some of its force, though not much. So that if the Well be not deep, it will freez, whereas if it be so deep, as that the wind, which causeth cold, cannot reach it, it will not freez.

7. We find moreover by experience, that ice is lighter then water, the
cause

cause whereof is manifest from that which I have already shown, namely, that the air is receiv'd in, and mingled with the particles of the water, whilest it is congealing.]

8. To examine now Mr. *Hobbs's* Theory concerning Cold, we may in the first place take notice, that his very Notion of Cold is not so accurately, nor warily deliver'd. I will not here urge, that it may well be Question'd, whether the tending outwards of the spirits and fluid parts of the Bodies of animals, do necessarily proceed from, and argue heat. Since in our Pneumatical Engine, when the air is withdrawn from about an included viper (to mention no other Animals) there is a great intumescence, and consequently a greater indeavour outwards of the fluid parts of the body, then we see made by any degree of heat of the ambient Air, wont to be produc'd by the Sun. This, I say, I will not insist on, but rather take notice, that though Mr. *Hobbs* tells us, that to cool, is to make the exterior parts of the body indeavour inwards : yet our Experiments tell us, that
when

when a very high degree of Cold is introduced, not only into water, but into Wine, and divers other partly Aqueous liquors, there is a plain intumescence, and consequently endeavour outwards of the parts of the refrigerated Body. And certainly Cold having an operation upon a great multitude and variety of bodies, as well as upon *our* Sensories, he that would give a satisfactory definition of it, must take into his consideration divers other effects, besides those it produces on humane bodies. And even in *these*, he will not easily prove, that in every case any such *indeavour inwards* from the Ambient Ætherial substance, as his Doctrine seems to suppose, is necessary to the perception of Cold, since as the mind perceives divers other qualities, by various motions in the Nervous or Membranous parts of the sentient; so Cold may be perceiv'd, *either* by the Decrement of the agitation of the parts of the Object, in reference to those of the Sensory; *or* else by some differing impulse of the sensitive parts occasion'd by some change

change made in the motion of the blood or spirits, upon the deadning of that motion; *or* by the turbulent motion of thole excrementitious steams, that are wont, when the blood circulates as nimbly, and the pores are kept as open as before, to be dissipated by insensible transpiration.

9. It may afford some illustration to this matter to add, That having inquir'd of some Hysterical Women, who complain'd to me of their distempers, whether they did not sometimes find a very great coldness in some parts of their heads, especially at the Top, I was answered, that they did so, and one of them complain'd, that she felt in the upper part of her head such a Coldness, as if some body were pouring cold water upon it. And having inquired of a couple of eminent Physicians, of great practise, about this matter, they both assur'd me, that many of their Hysterical patients had made complaints to them, of such great Coldness in the upper part of the head, and some also along the

Verte-

Vertebra's of the Neck and Back.
 And one of these Experienc'd Doctors
 added, that this happen'd to some
 of his Patients, when they seem'd to
 him and to themselves to be other-
 wise Hot. The noble * *Avicen* also * Quoted
by Paul.
Newor-
antz. De
Purpurâ,
Cap. 12.
 some where takes notice, that the
 invenom'd Bitings of some kinds of
 Serpents; (creatures too well known
 in the Hot Countries where he liv'd)
 made those that were bitten by them,
 either become or think themselves ve-
 ry cold. But that will perhaps seem
 more remarkable, which I shall fur-
 ther add, namely, that I know a
 Nobleman, who follow'd the Wars
 in several Countries, and has signa-
 liz'd his Valour in them; and yet
 though his stature be proportionate
 to his courage; yet when this person
 falls (as frequently he has
 done) in a fit of the stone, he
 feels an universal cold over his whole
 body, just like that which begins the
 fit of an Ague. And though he as-
 sures me, that the stones, that tor-
 ment him, and which he usually
 voids, are but very small; yet
 whilst the fit continues, which of-
 tentimes

tentimes lasts many hours, he does not only feel an extraordinary Coldness, but which is more strange, and which I particularly inquir'd after, cannot by clothes, or almost any other means, keep himself warm.

10. I elsewhere take notice of some other Observations, agreeable to these, by some of which we may be perswaded, that there may be other ways, besides those already mention'd, of perceiving cold, though the outward parts of our bodies were not prest inwards. And whereas Mr. *Hobs* infers, that *He*, who would know the cause of cold, must find by what motion or motions the exterior parts of any body endeavour to retire *inwards*, that seems but an inconsiderate direction. For in *compressions*, that are made by *surrounding bodies*, there is produc'd an endeavour inward of the parts of the compressed body, though no Cold, but sometimes rather Heat be thereby generated. And I hope Mr. *Hobs* will not object, that in this case the parts do not *retire*, but *are thrust* inwards, since according to him no body at all can be moved,

ved, but by a body contiguous and
 mov'd. But what I have hitherto ta-
 ken notice of, being chiefly design'd
 to shew, that the *notion of cold in ge-*
neral is not so obvious a thing to be
 rightly pitch'd upon, as many think,
 and that therefore it needs be no
 wonder, that it hath not been accurate-
 ly and warily propos'd by Mr. *Hobs*:
 I shall not any further prosecute that
 discourse, but proceed to what re-
 mains. Next then, the Cause he as-
 signs, why a man can blow hot or
 cold with the same breath, is very
 questionable; partly because he sup-
 poses in part of the breath such a *sim-*
ple motion, as he calls it, of the small
 particles of the same breath, as he
 will not easily Prove, and as **eminent* * *Doctor S.*
Astronomers and Mathematicians *Ward*
 have Rejected; and partly because *(now the*
 that without the suspected suppositi- *worthy*
 on, I could (by putting together the *Bishop of*
Conjectures of two learned Writers, *Exeter)*
 and what I have elsewhere added of *and Dr. J.*
my own *Wallis* *(the learn-*
 count of the *Phenomenon*, if I had not *ed Savili-*
 some scruples about the matter of *an Profes-*
 Fact it self: which last clause I add, *or of Geo-*
metry.)

G g g

because,

because, though I am not sure, that further Trials may not satisfy me, That the Wind or Breath, that is blown out at the middle of the compress'd Lips, has in it such a real coldness, as men have generally ascrib'd to it; yet hitherto some Trials, that my jealousy led me to make, incline me to suspect, there may be a mistake about this matter, and that, in estimating the Temper of the produc'd Wind, our senses may impose upon us. For having taken a very good and tender seal'd Weather-glass, and blown upon it through a glass-Pipe (of about half a yard long) that was chosen slender, to be sure that my breath should issue out in a small stream; by this wind beating upon the ball of the Weather-glass, I could not make the included spirit of Wine subside, but manifestly, though not much, ascend, though the Wind, that I presently blew through the same Pipe, seem'd sensibly cold, both to the hand of bystanders, and to my own, and yet mine was then more then ordinarily cold. So that having no great en-
courage-

encouragement to enter into a dispute about the cause of a *Phænomenon*, whose Historical circumstances are not yet sufficiently known and cleared, I will now proceed to add, that whatever be the cause of the effect, there are divers things that make Mr. *Hobs's Hypothesis* of the Cause of Cold unfit to be acquiesc'd in. For we see that the grand cause, he assigns of cold and its effects, is *wind*, which according to him is *Air moved in a considerable quantity, and that either forwards only, or in an undulating motion*: and he tells us too, that when the breath is more strongly blown out of the mouth, then is the direct motion prevalent (over the simple motion) which, says he, makes us feel cold; for, says he, the direct motion of the breath or air is wind, and all wind cools or diminishes former heat. To which words in the very next line he subjoins, that not only great, but almost any ventilation, and stirring of the Air doth refrigerate. But against this doctrine I have several things to object.

*Chap. 28.
Sect. 2. at
the beginning.*

11. For first, we see there are very

G g g 2

hard

hard frosts, not only continued, but oftentimes begun, when the Air is calm and free from winds, and high and boisterous Southerly winds are not here wont to be near so cold as far weaker winds, that blow from the North-east.

12. Next, if Mr. *Hobs* teach us, that 'tis the direct motion of the stream of breath, that is more strongly blown out, that makes us feel Cold, he is obliged to render a reason, why in an *Æolipile* with a long neck, the stream that issues out, though oftentimes far stronger then that, which is wont to be made by compressing the Lips, at a pretty distance from the hole, it issues out of, is not cold, but hot.

13. Thirdly, Mr. *Hobs* elsewhere teaches, that when in our Engine the pump has been long imploy'd to exhaust (as we say) the Receiver, there must be a vehement wind produc'd in that Receiver, and yet by one of our other Experiments, it appear'd, that for all this in a good seal'd Weather-glass plac'd there, before the included Air begins to be (as we say) empty.

emptied, there appear'd no sign of any intense degree of cold produc'd by this suppos'd wind, so that either the wind is but imaginary, or else Mr. *Hobs* ascribes to winds as such, an infrigidating efficacy, that does not belong to them.

14. Fourthly, we find by experience, that in hard frosts water will freeze, not only though there be no wind stirring in the ambient Air, but though the liquor be kept in a close room, where, though the wind were high abroad, it could not get admittance; and some of our Experiments carefully made have assured us, that water seal'd up in one glass, and that glass kept suspended in another glass carefully stop'd, to keep out not only all wind, but all Adventitious Air, may nevertheless be not only much cool'd, but turn'd into ice.

15. Fifthly, we found by other Experiments, that a frozen Egg, though suspended in, and perfectly surrounded with water, where no wind can come at it, will be every way crusted over with ice, in which case there is no probability, that the ice

*See the
VI. Section
of the
History of
Cold.*

should be generated according to the way propos'd by Mr. *Hobs*. For he will scarce prove, nor is there any likelihood, that a wind pierc'd the shell and closer coats of the Egg to get into the contain'd liquors, and freez them; and a *more* unlikely assertion it would be, to pretend, (as he that maintains Mr. *Hobs*'s doctrine, must) that so very little Air, if there be any, as is mingled with the juices of the Egg, is, by the Cold, which is not wont to expand Air (nor water, till it be ready to make it freez) turn'd into a wind subtile enough, freely to penetrate the shell and coats of the Egg, and great enough to diffuse it self every way, and turn on every side the neighbouring water into ice; and all this notwithstanding, that not only it appear'd not by bubbles breaking through the water, that there is any Adventitious Air, that comes out of the Egg at all; but that also, supposing there were some such contain'd in the Egg, yet what shadow of reason is there to conceive, that the Air which was engag'd in, and surrounded

ed with the substances of the white, and the yelk of the Egg, must needs be a wind, since, according to Mr. *Hobs*, that requires a considerable motion of most of the parts of the mov'd Air the same way, and according to him also a body cannot be put into motion, but by another body contiguous and mov'd.

16. Sixtly, Mr. *Hobs* does indeed affirm, that all wind cools, but is so far from proving, that the *highest degrees* of Cold must needs proceed from wind, that he does not well evince, that all winds refrigerate. Nor are we bound to believe it without proof, since wind being, according to him, but Air mov'd in a considerable quantity, either in a direct or undulating motion, it does not appear how Motion should, rather than Rest, make Air grow cold. For though it be true, that *usually* winds seem Cold to us; yet (in the first place) it is not *universally* true, since some, that have travelled into hot Countries, and particularly the

Euri, Austrique venti à Meridie loca
 Arenosa summoque calore inflammata
 transeuntes atque Ægyptum spirantes
 tantum caloris æstus, pulverumque &
 inflammatarum Arenarum evehunt ut
 ignitas fornacis flammæ, nec non pul-
 veribus obscurissimas nubes eo aspor-
 tasse videatur. And elsewhere,
 — Prima æstatis parte calidissimâ
 inæqualissimaque ob vehementissimum
 Meridionalium Ventorum calorem, &c.
 Prosper Alpinus de Medicina Æ-
 gyptiorum.

From 9. till noon, there blows a wind
 with such extreme heat from the
 sands, that it swallows up a mans
 breath, and stifles him. — The
 King of Chermain sent an Army of
 sixteen hundred horse, and five thou-
 sand foot, against the Lord of Ormus,
 for not paying his Tribute, which
 were all surprized and stifled with
 that wind. Marcus Polus in Pur-
 chas's Pilgrims, lib. 111. p. m. 71.

near Ormus, have been sometimes so
 hot, as to destroy an Army it self at
 once. And secondly, even when the
 wind does feel cold to us, it may of-
 tentimes do so but by accident; for,
 as we elsewhere likewise teach, the
 steams that issue out of our bodies be-
 ing usually warmer then the ambient
 Air, (whence in great Assemblies,
 even

learned *Alpinus*,
 have complain'd,
 that the winds
 coming to them
 in the Summer,
 from more tor-
 rid Regions, have
 appear'd to them
 almost like the
 steam that comes
 out at the open
 mouth of a heat-
 ed Oven. And
 if *Marcus Polus*
Venetus be to be
 credited, (for I
 mention his Te-
 stimony but ex
abundanti) the
 Southern winds

even those that are not throng'd, find it exceeding hot, and I have several times observ'd a hot wind to come from those throngs, and beat upon my face:) and the more inward parts of our bodies themselves, being very much hotter then the ambient Air, especially that which is not yet full of warm steams; the same causes that turn the Air into a wind, put it into a motion, that both displaces the more neighbouring and more heated Air, and also makes it pierce far deeper into the pores of the skin, whereby coming to be sensible to those parts, that are somewhat more *inward* then the *Cuticula*, and far more *hot*, the Air turn'd into wind seems to us more cold, then the *re-*stagnant Air (if I may so speak,) upon such another account, as that, upon which, if a man has one of his hands hot, and another not, the same body that will appear lukewarm to this, will appear cold to the other; because, though the felt body be the same, yet the Organs of feeling are differingly dispos'd. And to confirm this doctrine by an Experiment

See this difficulty more largely handled in the first Preliminary discourse.

riment (which has succeeded Often enough, and need not succeed Always to serve our present purpose,) we will add, that though Air blown through a pair of Bellows upon ones hand, when 'tis in a moderate temper, will seem very cold ; yet, that the ambient Air by being thus turn'd into wind, does indeed acquire a relative coldness, so as to seem cold to our senses, but yet without acquiring such a cold as is presum'd, may appear by this, that by blowing the same air with the same Bellows upon Weather-glasses, though made more then ordinarily long, and by an Artist eminent at making them, we could not observe, that this winds beating upon them did sensibly refrigerate either the Air or the liquor. Though 'tis not impossible, but that in *some* cases the wind may cool even *inanimate* bodies, by driving away a parcel of ambient air, impregnated with exhalations less cold, then the air that composes the wind. But this is not much, if at all, more then would be effected, if, without a wind, some other body should precipitate

pitate out of the air near the Weather-glass, the warmer *Effluvia* we have been mentioning, especially if the Precipitating Body introduce in the room of the displaced Particles, such as may in a safe sense be term'd *Frigorifick*.

17. Seventhly, Nor can we admit without a favourable construction, Mr. *Hobs* his way of expressing himself, where he says, as we have lately seen, that *All wind cools or deminishes former heat*. For if we take heat in the most common sense, wherein the word is used, not only by other writers, but also by Philosophers, to make wind the adequate cause of cold, it must in many cases do more then diminish former heat. For water, for instance, that is ready to freez, is already actually cold in a high degree, and yet the wind (if Mr. *Hobs* will needs have that to be the efficient of freezing) must make this not hot, but already very cold liquor, more cold yet, before it can quite turn it into ice.

18. These things thus establisht, it will not be difficult to dispatch the remaining

maining part of Mr. *Hobs* his Theory
 of Cold ; for to proceed to his sixth
 Section, we shall pass by what a
 Cosmographer would perhaps except
 against in his doctrine, about the ge-
 neration and motion of the wind up-
 on the surface of the Earth, and
 shall only take notice in the remain-
 ing part of that Section of thus much ;
 That the most of what Mr. *Hobs* here
 shews us, is but, that there is an ex-
 pansion of the air, or a wind genera-
 ted by the motion and action of the
 Sun ; but why this wind thus genera-
 ted must produce cold, I do not see
 that he shews ; nor does his affirm-
 ing, that it moves towards the Poles,
 help the matter, for besides that we
 have shewn, that wind as such, is
 not sufficient to produce far less de-
 grees of cold, then those that are felt
 in many Northern Regions, there
 must be some other cause, then the
 motion of the air or steams driven
 away by the Sun, to make bodies
 not in themselves cold, (for so they
 were suppos'd not to be, when the
 Sun began to put them in motion)
become vehemently cold in their pas-
sage.

sage. For Mr. *Hobs* cannot, as other Naturalists, derive the coldness of freezing winds from the cold steams they meet with, and carry along with them in their passage through cold Regions, since then those steams rather than the wind would be the cause of that vehement coldness; and so it might justly be demanded, whence the coldness of those cold exhalations proceeds. Besides that, 'tis very precarious and unconsonant to observation, to imagine such a wind, as he talks of, to blow, whenever great frosts happen, since, as we noted before, very vehement glaciations may be observ'd, especially in Northern Regions, when the air is calm and free from winds.

19. The account he gives in his seventh Section of turning water into ice, is the most unsatisfactory I have ever yet met with: for a good part of that Section is so written, as if he were appear'd to be understood. But whereas he supposes, that by the indeavour of the wind to raise the parts of the water, joyn'd with the indeavour of the parts of the water
towards

towards the Center of the Earth, the uppermost parts of the water will be prest together and coagulated, he says that, which is very far from satisfactory. For first, ice is often produced, where no wind can come to bear upon the uppermost parts of the water, and to raise them; and in vessels Hermetically seal'd, which exactly keep out air and wind, ice may be generated, as many of our Experiments evince. And this alone were a sufficient answer, since the whole explication is built upon the action of the wind. But this is not all we have to object; for not to urge, that he should have prov'd, that the uppermost parts of the water must be raised in congelation, especially since oyl and divers other liquors are contracted by it, not to urge this, I say, what shew of probability is there, that by the bare indeavour of the wind, and the gravity of the superficial parts of the water, there should be any such forcible compression made, as he is pleas'd to take for granted. And yet this it self is less improbable, then that supposing the
upper-

upermost parts of the water to be pressed together, that pressure is sufficient to coagulate, as he speaks, or rather congeal them into ice. So bold and unlikely an assertion should at least have been countenanced by some plausible reason, or an example in some measure parallel. For I remember not any one instance, where in any degree of compression, that has been employ'd, much less so slight a one as this must be, considering the causes whence 'tis said to proceed, can harden any liquor into ice, or any other hard body. And in the Experiment, we have elsewhere mentioned of filling a Pewter vessel with water, and when 'tis exactly clos'd, compressing it by the knocks of a Hammer, till the water be reduc'd to penetrate the very Pewter, we found not that so violent a compression did give the water the least disposition to turn a hard body. And as for the way Mr. *Hobs* assigns of Increasing the thickness of ice, 'tis very difficult to conceive, how a cake of ice on the top of the water being hard frozen to the sides of the containing vessel,

*In the new
Experi-
ments
touching
the Spring
of the Air.*

vessel, and thereby severing betwixt the included water and the external air; the wind that cannot come to touch the water, because of the interposition of the hard and rigid ice, should yet be able, sometimes at the depth of nine or ten foot, or much further, to beat upon the subjacent water, and turn it into ice. And it is yet more difficult to conceive, how the wind must do all this, when, as was lately noted, the water does very often freez more and more downwards, to a great depth, in places where the wind cannot come to beat upon it at all. And as to what Mr. *Hobs* further teaches, that the ice must contain many particles of air receiv'd into it, we have elsewhere occasion to show, how erroneously he discourses about those Icy Bubbles.

See the
IX. Title
of the Hi-
story of
Cold.

20. The reason he assigns of the freezing of water with Snow and Salt, does as little satisfie as the rest of his Theory of Cold. For not to mention, that he affirms without proving it, that Snow and Salt have in them a great deal of air; it is very precarious to assert, that this air must be prest

prest out every way in wind, which must rake the sides of the vessel, for 'tis strange, that far more diligent observers then Mr. *Hobs* should take no notice of any such wind, if any such wind there were; but this is yet less strange, then that which follows; namely, that this wind must so rake the sides of the vessel, as to make the vessel by the same motion and action congeal the water within it. For what affinity is there between a wind, passing along the outside of a glass, altogether impervious to it, and the turning a fluid body, included in that glass, into a hard and brittle body. The wind indeed may perhaps, if it be strong, a little shake or agitate the particles that compose the glass, and those may communicate some of their motion to the contiguous parts of the water; but why all this must amount to the turning of that water into ice, is more, I confess, by far then I can apprehend. Especially seeing, that though you long blow upon a glass of water with a pair of Bellows, where there is not an Imaginary wind, as Mr. *Hobs's*, but a

H h h

Real

See the
IV. Section
of the
History of
Cold.

Real and manifest one; yet the water will be so far from being frozen, that our formerly mentioned Experiments (of blowing upon Thermometers) make it probable, that it will scarce be cool'd. And if Sea-salt do contain so much air, by virtue of which, it, as well as the Snow, produces so intense a degree of Cold, how chance that being resolv'd in a little water without Snow, it does not produce at least a far greater degree of cold then we find it to do? Besides, in the Experiment we made (and elsewhere mention) of freezing water seal'd up in Bubbles, though the Bubbles were suspended in other glasses, whose sides no where touch'd them, and the remaining part of whose cavities were fill'd some with air, and some with unfreezing liquors; what likelihood is there, that Mr. Hobs's insensible Wind should be able to occasion so many successive Rakings through differing Bodies, as there must be, to propagate the congelative motion (if I may so call it) of the wind, through the first glass, to the included Air or Liquor, and through

through that new *Medium* to the glass containing immediately the water, and through that to the innermost parts of the seal'd up water. And it might be further objected, if it were worth while, that Mr. *Hobs* does not so much as offer at a reason, why spirit of Wine, *Aqua fortis*, or even Brine, if it be of the strongest sort, are not either by this mixture, or (here in *England*) by the Wind in the open Air turn'd into Ice, as well as many other Liquors are.

21. The reason why Cold is wont to be more remiss in rainy or cloudy weather, then in that which is more clear, is not better given by Mr. *Hobs*; then by some others that have written before him: for not to mention, that I have seen great frosts, and lasting enough in cloudy, and sometimes very dark weather; that which he talks of the winds being more strong in clear weather, then in cloudy, is of no great importance, since common Experience shews, that in clear weather the Air may be very cold, and the frost very great, where no wind is felt to rake, as he would

H h h 2

have

have it, the superficies of the Earth. Nor does experience bear witness to what he not warily enough pronounces, that *the less the wind is, the less is the Cold.* There are but two *Phænomena* more, which in this Section Mr. *Hobs* pretends to explicate; The one is, that in deep Wells the water does not freeze so much, as it does upon the superficies of the Earth. But the reason of this we elsewhere take occasion to consider, & therefore in this place we need only note, that Mr. *Hobs* has not rightly assigned it by ascribing it to the winds entering more or less into the Earth, by reason of the laxity of its parts; since besides that it is very improbable, that the wind should not, as he says it does not, lose much of its force by entering into the Earth at its pores, and other lesser cavities (for that seems to be his meaning by the laxity of the Earth's parts) to so great a depth as water lies in several Wells subject to freezing: besides this, I say, Experience teaches us, that Wells may be frozen, though their Orifices be well covered, and the wind be thereby kept from

See the
Examen of
Antiperi-
stasis.

from approaching the included water by divers yards; and very many Wells, that are subject to freeze, when Northerly and Eastwardly winds reign, will likewise be frozen in very cold Winters, whether any wind blows, or not.

22. The other and last *Phænomenon*, Mr. *Hobs* attempts to explicate, is, That ice is lighter then water; the cause whereof, says he, is manifest from what I have already shewn; namely, That air is receiv'd in, and mingled with the particles of the water whilest it is in congealing. But that this is not the true reason, may be argued from hence, that if a conveniently shap'd glass-vessel be fill'd top full with water, and expos'd either unseal'd or seal'd to congelation, the ice will have store of bubbles, which, at least in the seal'd vessel, cannot by Mr. *Hobs*, who will not affirm glass to be pervious to the Air, be pretended to proceed from bubbles, that got from without into the water, whilest it was in congealing. And we have sometimes had occasion to manifest by particular

See the
IX. Title
of the Hi-
story of
Cold.

Experiments purposely made, how little of Air there is even in those bubbles that are generated in ice, made in vessels, where the Air was not kept from being contiguous to the water.

23. And thus have we gone through Mr. *Hobs's* Theory of Cold, In his Proposing of which, we wish'd he had in Divers places been more Clear; and in our cursory *Examination* of which, we have seen that most of the particulars are either *precarious* or *erroneous*, and were they neither, yet the whole Theory would, I fear, prove very *insufficient*. Since an attentive Reader cannot but have marked, that this learned Author has past by far the greatest part even of the more obvious *Phænomena* of Cold, without attempting to Explicate them, or so much as shewing in a general way, that he had Consider'd them, & thought them explicable by his *Hypothesis*: By which he that will fairly explain all the *Phænomena* recited in the Notes we have been drawing together, and which yet contain but a Beginning of the *History of Cold*, shall give me a very good opinion of his Sagacity. A

A Postscript.

THough the haste, I am obliged to comply with, keep me from annexing the Historical Papers, wherewith I had thoughts to Conclude this Book, concerning Cold; yet since the Nature of the past *Examen* gave me but little Opportunity to teach the Reader any thing more considerable, then that Mr. *Hobs's* Doctrine is Erroneous; I am very inclinable to make him here some such little amends, as the Time will permit, for that Paucity of Experiments. And therefore since in the last Section of the foregoing History, *Pag. 673.* upon occasion of an Experiment very Imperfectly, and not intelligibly deliver'd by *Berigardus*, I intimate my having elsewhere Plainly set down, either the same he meant, or one of that Nature; and that with considerable *Phænomena* unmention'd by him: I chuse rather to borrow some Account of it from another Treatise,

to which it belongs, then not gratifie some of the Curious, to whom the *Phænomena* I shew'd them of it, seem'd no les pretty then surprizing.

The way then that I us'd in making this Experiment, may be gathered from the following directions.

Take of good unslak'd Lime three

* According to the goodness of the Lime, of which, if it be very strong, two parts may serve the turn, and which, if it be too weak, may make the Experiment miscarry.

parts (or thereabouts *) of

(yellow) Orpiment one part, of fair water 15.

or 16. parts; beat the Lime grossly, and powder the Orpiment (with care to avoid the noxious Dust that

* To prevent which, I usually cause the Orpiment to be beaten, wrapp'd up in divers papers, or some other way secur'd from Avolation, and from harming the vessel wherein 'tis pounded.

may fly up *) and having put these two ingredients into the water, let them

remain there for two or three hours, or longer, if needs be, remembering to shake or stir the mixture from time to time. By this means you will obtain a somewhat fætid Liquor, whereof by warily Decanting, or by Filtrating it, the Clear part must be severed from the rest.

In

In the mean time take a piece of Cork, and having lighted it so, that it is kindled throughout, remove it from the fire, whilest 'tis yet burning, and by a quick immersion, quench it in fair water. And having by this means reduc'd it to a coal, you may (in case you have not err'd in the Operation) by grinding it with a convenient Quantity of Gum-water *, bring it to the colour and consistence of a good black Ink, that you may use with an ordinary Pen.

** Which for this use 'twill suffice to make by dissolving Gum-Arabick in a little fair water.*

Whilest these things are doing, you may take what quantity you think fit of com-

mon * *Minium*, and two or three times its weight of spirit of Vineger (which needs not be for this

** This is known in the shops by the name of Red-Lead, and is here specified, as being cheap and easie to be procur'd, though I suppose that other Calxes or powders of the same metal, if they be not sophisticated, may serve the turn,*

purpose much stronger then phlegm, and to which even undistill'd Vineger may be a *succedaneum*) and putting the powder and liquor into a glass-

glass-Vial, or any other convenient vessel, let them infuse over hot Embers, or in some considerably warm place for two or three hours more or less, till the liquor have acquir'd a very sweet taste.

All things being thus prepar'd, take a new, or at least a clean Pen, and write with it some such thing, as you either desire or need not fear to have read, *between* (if you please) or,

** If you write first with the black Ink, and then with the solution, the Lines must be made somewhat distant, that those which express your secret may have room between the others, and therefore the better to avoid suspicion, I chose rather to write first with the invisible Ink, and then over that with the black, as if I had writ on an ordinary piece of white paper, by which means I could write the black Lines as close as I pleas'd.*

which is safer, ** Over the Lines, which contain your secret, and which are to be trac'd with the solution of Miuium; for this Liquor, if it be either well decan-*

ted or filtred, will be so clear, that what is written with it by a new Pen, will not be seen upon the Paper when it is dry.

Lastly, when you would show the Experiment, dip a small piece of Sponge, or a Linnen-rag (or for a need, a little paper wreath'd) in the water,

water, that was made with Lime and *Auripigmentum*, and with this liquor, which, though it smell ill, will look limpid and clear, wiping over the Paper, it will presently at once, both wipe out or obliterate what was written with the black Ink, and make all that was written with the invisible Ink, though perhaps in the self-same Lines, appear black, so as to be very easily and plainly legible.

This is the way, to which many years ago my Trials led me, of making this odd Experiment. For the performing whereof, if any can propose a more Easie and Better way (for I find by an Inquisitive * Traveller, that there

are more ways than one) I shall willingly learn it.

* That learned Gentleman Mr. H. Oldenburg, Secretary to the Royal Society.

In the mean time the Reader may perceive, that I did not causlessly intimate, That the learned *Berigardus*, though he would manifest a great thing in Philosophy by this Experiment, did yet either not Understand himself that part of it, he pretends to Teach,

Teach, or has omitted one of the main Ingredients of the water of Orpiment he speaks of. For I did not find, that even by a long Infusion, nor by some Decoction of the Orpiment alone (without the Quick-lime) there would be produc'd a Liquor, either obviously fætid, or that would perform so much as a Less matter, then what that, which he mentions, should. And whereas he seems to commend this way (though but between Lines written with common Ink) for the writing of things one would not have to be discovered, and though I have yet met with no body, that having seen the Experiment, is not of his mind ; yet I remember, that, when many years ago, I was making Trials concerning the several ways of making invisible Inks, my Conjectures led me to discover, that I could very readily bring, what was written with a solution of *Minium*, to be Legible, by the help of the fire ; as well as I could also detect by the same way several invisible Inks, which are believ'd to require appropriated Liquors to make them Confess

fess their secrets. But I must reserve
 the Reflections, and other particu-
 lars that relate to this Experiment,
 for the Treatise to which it belong'd.
 Only I will now add, That besides
 the above-specified motives to com-
 municate what I have at present
 witten of it, I was the rather induc'd
 to do so, because I had mention'd, but
 not taught this Experiment, in the
History of Whiteness and Blackness; and
 because also *Berigardus* is not the only
 Author of Note I have met with, that
 having made particular mention of
 the Experiment, has given the Curious
 but a Lame and unsatisfactory
 Account of it.

FINIS.



* For those
that con-
cern Divi-
nity belong
not to this
Catalogue.

Philosophical Writings *
already publish'd by
this Author.

New Experiments Physico-mechanical, touching the Air, 1660. publish'd about Midsummer.

Certain Physiological Essays, written on several occasions, 1661. in March.

The Sceptical Chymist, 1661. in August.

A defence of the Doctrine touching the Spring and Weight of the Air, against the Objections of Franciscus Linus, 1662. in the Spring.

The Usefulness of Experimental Philosophy, 1663. in June.

Experiments and Considerations touching Colours, 1664. in May.

Such

Such Philosophical Writings
of the same Author, as being occasionally
mention'd (here and there) in the
above-nam'd Books, are not yet
publish'd, but (though not
absolutely promis'd) by di-
vers of the Curious expected.

THe second Section of the second part
of the usefulness of Experimental
Philosophy.

Two Essays concerning the Concealments
and Disguises of the Seeds of Living Crea-
tures.

Some Additional notes design'd by way
of Appendix to the Physico-Mechani-
cal Treatise.

Two Historical Dialogues, one concern-
ing Flame, the other concerning Heat.

Hydrostatical Paradoxes made out by
Physico-Mechanical Experiments.

An Essay of the Origine of Forms and
Qualities.

Of the Production of Qualities (mani-
fest and occult) by Art.

The Sceptical Naturalist, being a Let-
ter about the Imperfections of Natural Phi-
losophy, as we yet have it.

A Discourse of Improbable Truths.

AN

The Spectral Nature of the



AN
ADVERTISEMENT
TO THE
READERS
OF

The following Experi-
ments, by the Author
of the foregoing
History.

A *the same*
time, that
the Royal
Society required

This is pointed at in the third
Page of the following Account,
where mention is made of an Ho-
norable Person, &c.

aa

of

of me an Account of what I had observed, or tried, concerning Cold, they recommended the making of Trials, about that Subject, to the Learned Dr. C. Merret, who having dispatched what he intended, much earlier then I could bring in my far more Voluminous Papers, he long ago presented His to that Illustrious Company: and since That, has thought fit to let them indear my Treatise, by their being Annexed to it, and composing a part of It; and that such a part, as much might be said of it, if after I have inform'd the Reader of its having obtained the Thanks of a Society, that is too much accustomed to receive and produce Excellent things, to be suspected of valuing Trifles, I could think it needful and proper to give those Papers any other Elogium. And it falling out fortunately enough, That the Doctor and I (being at some miles distance) did not communicate our Designs to one another; as I knew Nothing of what he had been doing, till I heard it publickly read at Gresham Colledge, when far the greatest part of my Experiments

ments were (as is known to more Persons
then one) already recorded ; So I af-
terwards scrupulously abstained from bor-
rowing the Trials mentioned in his Pa-
pers, to enrich mine : which forbear-
ance was the more easie to me, because
after the first time I heard those Papers
read, I never Desired a Copy, nor
Had a Sight of them. By this means it
happened ; That besides those many
Titles, which being handled at large
in the History, are left untouched in
the following Tract, even on those Oc-
casions, where the Learned Doctor and
I happen to treat of the same subjects ;
our Trials are but Very few of them coin-
cident ; upon which score, the Rea-
der will meet with more Variety betwixt
us, then probably he would have ex-
pected to find on such an Occasi-
on.

HAVING drawn up this Advertise-
ment about the Doctors Papers, as sup-
posing them the very same, he presented
to the Royal Society ; upon a sight
a a 2 of

See the
Publisher's
Advertise-
ment to
the Rea-
der.

of the following Sheets, (as they were
some hours since brought me from the
Press ;) the Additions I there find,
make it appear necessary to say something
further to the Reader. I must inform
him then, that about the middle of
this Winter, and about the end of De-
cember 1664. I presented to the Roy-
al Society several Books, containing
each of them Eighteen or Nineteen of
the Twenty One Titles, whereof my
History consists; that the Virtuosi
might have the Opportunity of the Cold
(which then began to be so strong, as
to keep the Press from dispatching the
rest of the Book) to examine my Experi-
ments, and add to them; and one of
these being delivered to the Doctor, as
the likeliest Person to make use of it,
together with an Order to the Stationer,
to let him have the remaining Sheets of
the Book, as fast as they should from time
to time be Printed; he had the Curio-
sity, as to Enlarge some of the things
he had already tried and brought in
himself; (as is intimated in the Forty
Sixth Page) so to make Trial of some
parti-

particulars, that I had proposed and performed, which either their Importance (as the way of freezing from the Bottom upwards, by me suggested, and the weight of Bodies frozen and unfrozen) or his Opportunity invited him to make choice of; and has been pleased to afford them place among his own Experiments *; by

which means, though the coincidence of what we deliver

* Among which I am since informed, that he had tried divers, before he saw my Papers.

will appear to happen more frequently, then the Advertisement will make one expect: yet to such Readers as do not prefer Variety before Certainty, these coincident Passages will not in likelihood be unacceptable. For in those Cases, where the Events of our Trials are the same, 'tis like the Truth will be the more confirmed *; and in Cases where

* So one of the

chief Passages of the Examen of Antiperistasis is much confirmed by the Forty Fourth and Forty Fifth Pages (of the following Papers) which contain an Account of a Trial made by the command of the Royal Society, to whom it was proposed by the Author of the Examen, with a request, that they would be pleased to order it to be made.

the successes are very differing, the Reader will be excited to make further Trials himself, and will be thereby enabled to judge, which Trials have been the most carefully made, and the most warily delivered. And, though I think it but a Necessary Profession for me, to say on this occasion, That I am pretty Confident of my having performed my Duty, as to the Historical part; yet this need not hinder, but that most of the differing successes, we are speaking of, may prove but Instances of the Truth of what I long since admonished the Reader (in my Preface,) That there are among the Experiments of Cold, divers that are liable to Contingencies: So that, as I would not have the Papers of this Learned Man comprehended in what I said, of the Fejuness of the writers I had met with, who treat of Cold, in a Preface written, when I was not sure the following Papers would be made publick; so I hope the Reception of these Papers of this Ingenious Person will be such, as may invite him

him to hasten the Publication of those
fruits of his Learning and Industry on ^{The Art}
another subject, which divers of the ^{of Porte-}
Virtuosi do not more Expect, then ^{ry.}
Desire, to have communicated to
them.

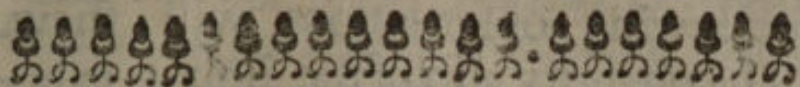
Place the before of Breeding is ob-
served, I will say daily promise
these following observations, that
these experiments were made in very
hard weather, yet with some
success, the frost continuing
above six weeks. And the place I
chose was, in Room window
facing to the North and West. **An**
windy and foggy day the evening.
The following observations of the
insects, earthworms, and
other animals, were made in the
evening.

him to hasten the Publication of those
fruits of his Learning and Industry on
another subject, which directs of the Office
Virtually do not want Express, thereby
Desires to have communicated to
them.

As
the

As

As



*An Account of Freezing made in
December and January,
1662.*

Since the business of Freezing is obnoxious to many various contingencies, I must necessarily premise these following circumstances, that these experiments were made in very hard weather, yet with some alternate relaxations, the frost continuing above six weeks. And the place I chose was in stone-windows, exposed to the North, and North-east winds, and some upon the ground. The vessels were Glass-canes of several bores, earthen and pewter, small pans and porringers, spoons of pewter, and silver, glasses of various figures, as Vials, Cylindrical, round, and square, flasques, recipients, bolts-heads and some Conical ones. Most whereof by the diversity of their figure, their opennes or closeness produce

duce various effects in freezing, as the following observations will shew. The quantity also of the liquor exposed is to be considered, for what will shew a small thin plate of ice in a small parcel of some liquors, will shew none in a greater.

The method I shall follow in delivering my observations shall be, first to run over the various liquors or bodies, whether fluid or consistent, simple or compound, &c. used in this work. Secondly, what figures observable in those ices. Thirdly, some effects arising thence. Fourthly, some properties and qualities. Fifthly, some lets or helps both to freezing and thawing. Sixthly, some uses of ice.

In pursuance of which particulars, I had recourse to those ingenious *Queries* of Mr. *Henshaw*, registred in your *Cimelia*, and then to *Bartholinus* his late Book *De Nive*, and to my own collected notes from various Authors, adding whatsoever trials I thought meet. And in all these I have barely set down matter of fact, neither mentioning the Authors nor
their

their errors, which would have been both nauseous and tedious, nor shall I endeavour to render a reason of the various *παράμενα* (which cannot be done without a volume) but shall leave that province to an Honourable person of this Society, who hath had much experience and reflections on this subject. And now to my task. As to my first head of things used, I shall begin with common water, which I exposed in a triple state, in like quantities, and in open pans, *viz.* first cold, secondly, boiling hot, thirdly, an equal mixture of both the former. The effect was this, the cold was frozen in one hour, the boiling hot in two hours, and the mixt in hour 1 and $\frac{1}{2}$; but with this difference, that the cold did freez first at the top, and sides, and had a large thick crust before there was any shew of ice in the boiling hot; but the mixt and boiling hot began to freez first at the bottom of the vessels, and when the top was cold then it freezed there also, leaving betwixt the bottom and top of the vessel a cavity for the water, which in time was wholly
 blueo A a 2 converted

converted to ice. The same succeeded most manifestly in these waters powred on a smooth table, where the cold water was presently frozen before the boiling hot water could become cold at the bottom.

Water exhausted of air in Mr. *Boyles* engine was frozen almost as soon as a like quantity expos'd in an open pan. The ice whereof appeared white, and to consist purely of bubbles. The glass used was a four ounce round vial, and a small Tube one foot long half filled with water.

Fair water wherein *Arsnick* had been infused eight moneths, congealed much sooner then a like quantity of water, into very white ice.

Solutions of all the sorts of *Vitriols* freezed sooner in pans and Tubes, then water or any other solution of the other salts by much, though that of *Alume* came very little short of it. The ice kept both colour and taste upon the least touch of the tongue, in all of them.

A solution of *Alume* did freez into an ice whiter then milk, and stuck so close to the sides of the pan, that it could

could hardly be separated from it: this was the firmest ice offered to me in all my trials, next to which in both these qualities were the *Vitriols*, especially the *Roman*.

Sandever quickly freezeth, *Frit* sooner then it, and *Kelp* then them both, all of them into lumps very white, and consequently not *Diaphanous*.

Sal Armoniac shewed some variety in point of time, for in the same pan, quantity, and place with the other salted waters 'twould for the most part freez long after the former, though once it did freez before them.

Common salt two drachms dissolved in four ounces of common water (for that proportion I observed in all my solutions) did in 30. hours space in the hardest season turn to pretty hard and white ice, whereas the former solutions became so in two or three hours at the most.

A beer-glass was filled with stinking Sea-water full of salt, which within 26. hours acquired at the top a plate of ice of the thickness of an $\frac{1}{2}$ a Crown piece, with few bubbles in

it. This tasted salt and stinking as before, but being dissolved at the fire, or thaw'd of its self, the stinking taste was gone, but the saltish continued. The residue in the glass within four days (the season continuing) and plates taken off (once in 24. hours) was frozen throughout, but that at the bottom of the glass seem'd to have a much brisker taste then that at the top, neither was it so firm and friable as that. I tried another beer glass with the same water, which froze most part of it, but the season continued not so constantly sharp so long together, as in the former experiment, and therefore I could conclude nothing therefrom. But in small broad earthen-pans set in ice in 36. hours the same water became ice throughout, and with the addition of a parcel of ice or snow much sooner.

Some water was impregnated with as much bay-salt, some with as much Salt Petre, some with as much *Sal Armoniac* as the water was capable to receive, and neither of these did congeal with the highest degree of

of cold, continued six days together.

A solution of salt of Tartar soon converted into ice, but in much longer time then common water. I observed that it began to freeze in a Tube at the top, bottom, and sides first, leaving the liquor in the middle unfrozen, whereas other solutions and liquors congealed uniformly, by descending, or ascending, or both at the same time, from side to side through the middle: of this I made but one Trial.

Salt Peter required 28. hours in a very cold season, and in that time became in the open pan a most pure white ice perfectly like *Sal Prunellæ*, which an Apothecary mistook it for. This ice thrown into the fire (after the aqueous humidity was evaporated) did sparkle as that salt useth to do. A strong *Lixivium* made hereof with an addition of Copperas or Alume singly, or mixt, set in snow and salt, or snow alone, was frozen in one night.

Sal Gem alone of all the salts, though snow and ice were mixed with it in great proportion, and

though the pan was set in salt and snow, could not all that time be brought to congelation: an odd experiment. Phlegm of *Vitriol* did freeze sooner then the solutions before mentioned.

Oyl of *Vitriol* begins congelation (or coagulation rather) near as soon as fair water. A pretty large Tube was fill'd $\frac{3}{4}$ full with this oyl, and about $\frac{1}{4}$ thereof was frozen, the rest remaining at the bottom uncongealed. This Tube was broken in the presence, and by the command of this Honourable society, the coagulated part whereof was tasted by many then present, and concluded by all those, that it was a strong *Vitriolate* taste. This coagulated part was of a paler colour then the other, and both these mixed and powred into a vial-glass heated it so hot, that none there could hold it. This coagulated part kept so in the air a week after all my other liquors had been thaw'd, and would in probability have continued so much longer had not the glass been broken. I exposed another lesser Tube with the same oyl, which became

became frozen throughout, and required very much relaxation in the air to return to its former fluidity.

I had set a mark on these Tubes (as on all the rest, to observe their several risings) and the oyl of *Vitriol*, when coagulated, sunk more then half an inch below it, and being dissolved at the fire returned to its first station, as you also saw. And this *φαινόμενον* is peculiar to this oyl alone, all other liquors rising higher then the mark.

I now come to my stronger liquors of Beer, Ale and Wines.

I exposed at the same time a flask of small Beer, and another of strong Ale, the former whereof was frozen throughout in 38. hours, but three pints of the Ale continued unfrozen after six days continuance of very hard weather. And the air then disposed to thawing, I broke the flask, and with the unfrozen liquor made an excellent mornings draught at four in the morning. This Ale in colour, strength, and quickness seemed to me and the other three tasters that sate up with me, much better then when 'twas first put into the flask, and by
compa-

comparing it with some other in the house of the same barrel, we plainly found the said difference. After this I took the icy part of the Ale and thawed it at a fire, which was in all a pint of liquor (though the flagon containing three pints of liquor, was fill'd with that ice) very pale, and of a quick and alish taste, very much resembling that drink which the brewers call blew John. This ice was not so firm as that of water, but fuller of bubbles.

I assayed the same a second time, but could not by reason of the changableness of the Weather attain so great a thickness of ice as in the former. And in this also I found the same changes as before.

VI A beer-glass of *Hull Ale* in 24. hours contracted a crust of ice as thick as an $\frac{1}{2}$ Crown, and proceeding as in Sea-salt water, the *σαινόμενα* were the very same, all the *Lamine* taken off, appeared of the same colour and taste, and the lowest ice was the most tender. Another glass of the same Ale exposed did not freeze throughout (no crust being taken off) in

in five days, when my own Ale did in a like glaſs, both being ſet out together. Now the taſte and colour appeared the ſame, or at leaſt had no ſenſible difference, when they had been thawed of themſelves, and when firſt expoſed. *Hull Ale* hath a brackiſh taſte.

Claret very ſtrong expoſed in a ſpoon in 35. hours hard freezing became an ice all of it, it was loſt, kept its former colour and taſte, ſoon diſcovering to the tongue of one who knew not whence it was, its nature, quality and kind.

Canary at the ſame time in a ſpoon expoſed in 38. hours acquired on its ſurface an exceeding thin plate of ice as thin as the fineſt paper, and proceeded no farther in four days following.

Neither Claret nor Canary would ſhew the leaſt ſign of congelation in Tubes, much leſs in Bottles.

Two ounces of the beſt ſpirit of Wine expoſed in an earthen pan did all evaporate in leſs then 12. hours, but the ſame quantity of Brandee left near a ſpoonful of inſipid ice without
any

any taste of the spirit, which cast into the fire flamed not at all. I could discern no bubbles in this phlegmatick ice, but having interposed it betwixt mine eye and a candle, it manifested many bubbles by its shadows. *Quere*, whether this may not turn to profit in colder Countries in rectifying spirits of Wine.

We now come to consistent bodies, and shall begin with animals and their parts.

Two eyes, the one of an Ox, the other of a Sheep in one night were both totally frozen, the three humors very hard, not separable one from another, neither of them Diaphanous, as naturally they are, and the Chrystalline was as white as that of a whittings boil'd. The Tunicles, Fat and Muscles were also frozen, as appeared by their stiffness, and by putting them into cold water. The ice of the waterish and glassy humors seemed to be made of flakes.

A pint of Sheeps blood did freeze at the top, and all the sides of the dish wherein 'twas put, and was nothing else but the *serum* of the blood. This

This ice being separated from the blood, and thaw'd at the fire, and then again exposed congealed into a seeming membranous substance, and was taken for such by some that saw it, and so continued in a warm season, and appeared in all respects a membrane. This also was seen and registered in the Journal. The blood remaining gave me no signs that frost had taken it.

I dissected a Dog and a Cat, having lain dead in the open air, and found their entrails, nay the very heart stiff, and some little ice in the Ventricles of their hearts, and their *Vena Cava*.

Milk soon freezeth into most white flakes of ice, retaining the proper taste of Milk: these flakes are soft and manifest not many bubbles.

Several Eggs were exposed, and both yolk and white in one night were hard frozen. They require a longer time to freez then Apples do. The best way to thaw them both is to lay them on *Newcastle*-coal, or in a deep Cellar. Whether Eggs once frozen will produce Chicken or no,

I cannot say, but have been told by good house-wives they will. Some affirm that Eggs and Apples put into water, the ice will be thawed within them, and the ice appear on the shell and skin. 'Tis true, if you hold either of them near the surface of the water, they will soon gather a very thick crust upon their outsides, but if you then break the one or cut the other, you shall see them full of ice, and the Eggs then poched will taste very tough. So that this ice seems to be gathered from without, and not to come from within. And besides if it did so, they must needs lose their weight, the contrary whereof will anon appear. But for the more surety I proceeded to this farther experiment. I immersed in my Cistern an Egg and an Apple two foot deep into water, and there suspended them with strings tied about them to keep them from sinking for the space of 24. hours, and then took them out and opened them. I could never observe in that time, though I often looked at them, any ice on their outsides, and the one being broken,
and

and the other cut, were found both of them full within of ice.

The next order shall be Vegetables, and of them a few instances, especially of those which are of a biting or sowre taste. Now for the first I employed the roots of horseraddish and Onions (for other edible roots and plants every one knows will freez) which shewed the frost had taken them by their taste, and ice was found betwixt each of the skins of the Onions retaining the taste of the root; yet I have observed Beer wherein Horseraddish and garden Scurvigrass have been infused, will not freez so soon as other stronger Beer without them.

Oranges and Limons frozen have a tough and hard rind, their icy juices lose much of their genuine taste; they were both frozen hard in 26. hours or a little more, having a thick rinde. They as other fruits, when thawed, soon become rotten, and therefore the Fruiterers keeps them under ground in low Cellars, and cover them with straw, as they do their Apples.

Which

Which did exposed in one night freez throughout. If you cut one of them through the middle, 'twill have on both the plains a most pure thin ice hardly discernable by the eye, but easily by the touch, or by scraping it off with a knife. The cores of these Apples soon turn brown, and begin their corruption there.

Oyl exposed did acquire the consistency of butter melted and cool'd again; but in Caves and Cellars I could never see it more then candy.

Strong White-wine Vinegre did all soon freez in a Tube, and without any apparent bubbles.

And to conclude without mentioning Nuts, Bread, Butter, Cheese, Soap, and many other things which came under my trial, 'tis most certain, that whatsoever hath any waterish humidity in it, is capable of congelation: what are not you have in the next Paragraph.

Having now done with what will freez, I shall briefly recount some things whereon the cold hath no such effect.

We mentioned before spirit of Wine,

Wine, add to it such strong waters as are made of it, viz. *Aqua Mariae*, *Celestis*, &c. and Canary Wines in larger vessels. Secondly, the strong Lees of Soap-boylers, and others made of other salts, to which refer the spirits extracted from salt; *Vitriol*, Salt Petre, *Aqua fortis*, and spirit of *Sulphur*, which last precipitated to the bottom of the Tube, a small quantity of powder very like in colour to *Sulphur Vivum*, which being separated from the spirit (for nothing of that evaporated) cracked between my teeth, and tasted like Brimstone, and being put into water, made it as white as *Lac Sulphuris* doth, but 'twould not flame, perhaps because too much of its strong acid spirit was mixed with it. Spirit of Soot afforded also a precipitation or sediment (the spirit not congealing) at the bottom of the Tube of a yellowish colour, but much bitterer than the spirit its self, and inflamable also.

But here 'tis to be observed that the said spirits that would not freeze alone, yet with the mixture of about 12.

parts of water, or less of ice, or snow, did freez throughout; except the spirits of Salt, of Nitre, and *Aqua fortis*, which would not freez with those quantities of water, ice, and snow. I intended to have tried them with a greater quantity of the said ingredients, but the weather failed me.

Whether the salt water freez in the Sea, I cannot experimentally determine, but shall add what was told me by one that said he had dissolved ice in the Northern Seas, and found it very salt.

The next proposed was the figure of liquors frozen; wherein I shall observe in general, that most of the liquors differed one from another in their figures, and being permitted to freez, and thaw often, they still returned to the same figure, most whereof were branched. Alume appeared in lumps, Salt Petre, Tartar, milk, Ale, Wine, and *Sal Armoniac* in plates, and other liquors mentioned to freez into a very soft ice, seeming to be made up of small *globuli* adhering each to other. Fair
water

water kelp and the frits resembled an oaken leaf, the leafie parts being taken away, and the fibres only remaining, the *interstitia* being fill'd up with smother ice. The middle rib (if I may so say) as in plants was much bigger then the lateral ones, all which seemed but different *stiria*, whose points extended towards the outside of the vessel containing the water, and made acute angles with the middle rib towards the lesser end of the said leaf.

Concerning the figures of frozen Urine I shall say nothing, the accurate description of curious Mr. *Hook* having so fully and truly performed that part of my task.

Now as to the famous experiment of *Quercetan*, and affirmed by many other Chymists, I made experiments in these following Vegetables, Rosemary, Rue, Scurvigrafs, Mints, and Plantane, wherewith I thus proceeded, I mixed with $\frac{1}{2}$ a pint of their distilled waters $\frac{1}{2}$ or $\frac{3}{4}$ of an ounce of their own salts, the Rosemary and Rue were calcined, and their salts extracted with their own waters, and

then were added to their salts their own distill'd waters in the above mentioned proportions. The glasses wherein the Rue and Plantane were put, being seal'd with *Hermes* seal, and the other glasses left open. The effect was, that neither of them shew'd the least resemblance of the plants, from which they were extracted, neither figure nor shew of roots, stalks, branches nor leaves, (but only a lump or heap of small *globuli*) much less of flour or seed. Besides the kelp frozen hath many fibres, which is made the most of it of *Alga Marina*, whose leaf is long and smooth without fibres in it. This one thing I cannot pretermitt, that the sented waters seem'd upon their thawing to have acquired, and advanced much in their sents, and especially the Rosemary, whose salt hath no smell, and its water but little; yet thawed, they smelt as strong almost as fresh leaves rubb'd and smelt too.

A large recipient was fill'd with water, which being frozen throughout, and the upper crust of the ice broken, there appeared in the middle
of

of it a multitude of thin *lamine* of ice, some more some less wide, from which proceeded *stiriae*, or teeth pointing inwards, and set at pretty equal distances, so that the *lamine* and *stiriae* resembled very much so many combs placed in no order, some lying directly, others obliquely, none transversely, having intervals betwixt each of them; betwixt some of them I could put my finger without breaking the points of the *stiriae*: these combs were placed round about a cavity in the middle of the receiver, sufficient to receive two of my fingers.

In a flask filled competently with water, when 'twas frozen there appeared throughout the ice infinite silver-coloured bubbles, very like unto tailed hail-shot of several sizes, the largest about $\frac{1}{4}$ of an inch long, where thickest, of the bigness of a great pins-head, others much less in all dimensions. The points of them all looked outwards, and the bigger part inwards towards the Centre, where also were the largest. For there they would easily admit a little

pin into all their cavity, without the least resistance. The figures of them were pretty regular, first a small thread, and then a head as big as a shot, and thence gradually ended in a point. Some of these were straight, most a little crooked. There was a cavity in the centre of this ice filled with unfrozen water, from which I could find multitudes of cavities of bubbles, not fully formed. And in the more solid parts of the ice cut, you may discern them by a black spot where the hole enters into the cavity. All the same *Phænomena* appeared in a second trial, but that the bubbles were shorter and larger, and not so sharp pointed. The like I also observed in a Conical glass seal'd up.

The next thing to be treated of, is the effects of freezing, viz. the expansion of liquors frozen, and consequently thereunto the breaking of bodies wherein they are inclosed. All the liquors tried did sensibly in glass Tubes rise beyond my mark, before the liquors could sensibly be discerned to freeze, and after rose somewhat higher with freezing. The height

height of the rising I shall here set of a few experiments, instead of many made (having troubled your patience too long in the former Paragraphs) in several processes. Vinegre and Urine rose about half an inch, and Lees made with salts of Rosemary kelp the frits about $\frac{1}{4}$ of an inch. Solutions of Alume and Copperas somewhat less, and in general the saline liquors less then water, which rose a full inch, and small Beer in a very narrow Tube four inches, but water in the small capillary Tubes could not be perceived, either to expand its self, and certainly not to freez at all. Oyl of *Vitriol* alone (as hath been said) sinks below the mark. Hot water put into a Tube first sinketh till 'tis cold, and then riseth before it freez.

Open-mouth'd glasses, such as Beer-glasses, &c. fill'd with water up to the brim, when frozen, the ice will manifestly rise above the superficies, and make a solid triangle there. But narrow necked glasses more plainly shew this rising. In a flask filled with water four inches be-

low the mouth, the ice rose above the mouth, and hung two inches without it. And once in a Bolthead the ice rose five inches above the watermark. And here I shall briefly add two things, first, that if glasses be fill'd about $\frac{2}{3}$ full they seldom break, but if more they will for the most part break. Secondly, that round figured or spherical glasses for the most part break in an uniform manner. I fill'd a Bolthead full to the neck, and stop't it at the top, which was 12. inches distant from the body, with a piece of melted candle. The ice rose above three inches in the neck, and the glass brake in the thinnest part of the body, from the point of breaking, as from a pole, the cracks run as so many meridians, but unequally distant each from other, and consequently concurred not in an opposite pole on the other side, besides there was great difference in the length of those cracks, none whereof went round the glass. In a flask thus crackt, in many places the cracks were very irregular in all the places, for some of them ran from their centres upwards,

upwards, others downwards, some somewhat parallel, but most obliquely, and few of them were considerably straight. Glass-bottles, and especially stone Jugs keep very little, and the last no method in their breaking; the same also betals square glasses: woods follow their grain, and metals no order at all.

And now I come to some remarks proceeding (as I said) from this expansion, *viz.* the breaking of the vessels, or force of freezing, wherein also you may take notice of that quality of cold, mentioned by the Poet, *penetrabile frigus*, piercing where light comes not.

Two Oval Boxes, the one of Box, the other of Maple (both firm woods) containing each above two ounces of water, were fill'd full, and with screws closed very fast, both these Boxes were rended from the bottom to the top in one night, with gaps big enough to receive a barley corn into them; these woods stretch but little, and therefore break more surely, and with larger rents than softer wood will do.

Secondly,

Secondly, a Pepper Box of Laton made of Iron, covered with Tin, had the neck broken off, and holes made in the top near the neck; and the bottom, where 'twas souldred, was so dissevered that water would easily run out there.

Leaden pipes laid above ground were broken in many places. One I saw 20. yards long broken in seven places, and another in my Cellar six yards long broken in two places. I saw likewise in many places of this City Leaden pipes, above a foot deep under ground, broken in several parts.

Cocks of Cisterns, and other brasse Cocks, and also the barrels in pumps made of brasse or lead, usually break with the frost.

I exposed a Copper Box of a pear fashion, which did bear three several freezings, by reason of the great extensibility of that metal, but at the fourth assay it crackt all along one side of it, almost to the screw.

Next I tried a Cylindrical silver Ink-horn, but that did bear five trials, and therein I could perceive neither crack;

crack, nor dilatation of its superficies. I intended to have tried it in a small bottle, but the weather fail'd me. I expos'd also a round silver ball of the bigness of a large Nut, the silver became very sensibly extended to a larger superficies, but did not suffer any solution of its continuity.

Tobacco-pipes, and all earthen ware taking any frost in their drying (before they are burnt) become very brittle, and being put into a strong fire will certainly break into many pieces. Tyles of houses, and hard stones in buildings, scale and break off upon thawing, and thence 'tis that the Northern sides of stone-buildings first decay, and moulder away, as 'tis most manifest in ancient magnificent structures.

Alabaster and Marble having any chinks in them, frequently break with frost, and the Statuaries tell me, they never saw any solid Marble break: as for Flints, Paving-stones, precious stones, and such as will receive a polish, the bitumens, as Amber, Kennel-coal, &c. I could never see any effect on them.

The

The next effect shall be that of adhesion, concerning which take the following experiments.

A smooth piece of ice was laid on a smooth Table, and common salt throwed upon it, the effect was, that the ice stuck so firmly to it, that it could not be severed from the table, without breaking the ice into many small pieces; & 'twill continue in this close cohesion till the salt hath corroded through the ice to the very table (making many holes in the ice) and hath melted it to the very bottom. But if you lay salt first upon the table, and ice upon it, then the ice sticketh not, but thaweth. These following salts applied (as before common salt was) cause adhesion to the table, but not so firm as it, viz. *Kelp*, *Sandever*, *Sal Indus*, *Gem*, *Prunella* and *Armoniac*, and Potashes, but not *Alume* or *Vitriol*.

The next experiment of adhesion was this; I held a nail betwixt my lips in the open air a very little space, which stuck so firmly to them, that I could not pull it thence without difficulty and pain.

Another

Another effect is concentration of spirits and colours. Concerning the former you have already as much as I know, especially in the Paragraph of freezing Beer and Ale. Concerning the latter take these following trials. Cochanele was boiled in water to a very high tincture, and frozen, and to twice four ounces of this decoction was added in one glass a little spoonful of spirit of Wine, and in another as much Sea salt-water. All these were frozen throughout, and every part of this ice seemed to me of an equal colour, though the edges, as thinner and nearer the light appeared of a brighter colour (as they do unfrozen) but the glasses being broken, shewed no discernable difference in any of them, neither as to colour nor taste. The like trials were made with Maddes weed and Indico, and the success was the same.

Secondly, I exposed a pint Porringer full of the decoction of foot, which (the air relaxing) did only freez an inch thick, this continued above a week consistent (in a thawing season) and very solid. Some
that

that saw it judged it to be brown Sugar Candy, the taste whereof was near, if not altogether as strong as the uncongealed liquor remaining at the bottom. And in another trial, when the whole was frozen, no concentration was seen. But though it was not my hap to find this effect, my trials having been made in Vials, square, Cylindrical or round, yet Mr. *Hauk* a worthy fellow of this Society happily lighted on it, as you may perceive by his relation, and Schemes of his Glasses hereunto annexed.

Some affirm as an effect of freezing, an addition of weight made in the bodies frozen, but this affirmation answers not my trials. For in four Eggs and four Apples fully frozen, I found the weight of them the same when frozen, and thawed, as they had before they were exposed, each of the Eggs and Apples being weighed in this triple state both severally and joyntly, with the particular weights I shall not trouble you. Besides that freezing adds no weight, 'tis apparent in sealed Glasses, from whence

whence nothing can expire, and by exact ponderation of them, I could not perceive any the least difference in weight in the said triple state. This I tried several times with as much exactness as possibly I could, and still found the same event.

Another property of freezing is to render many bodies more friable and brittle, as most woods, as also Iron and Steel, as every one knoweth that hath used Crosbows in frosty seasons, and so likewise the bones of animals, and 'tis commonly observed by Chirurgions, that more men break their legs and arms in such seasons, then at any other time of the year, especially such who have been tainted with the *Lues Venerea*, as *Hildanus* somewhere notes.

I shall now conclude the effects of freezing by ranging them into good and bad. The good are the long preserving bodies most subject to putrefaction, healthiness, and confirming the tone of all animals, and thickening the hairs and furs of such as have them, fatten some. Besides it exceedingly clears the air, and other bodies,

as

as 'tis manifest by the stinking Sea-salt-water before mentioned, as also by this that follows, namely, I took six of the most musty stone-Bottles I could procure, and competently fill'd them with water, which after freezing and thawing again, became as sweet as ever they were before.

Bad effects are the killing and destroying animals, and vegetables by congealing and stopping their vital and nourishing juices, rendring them totally immovable. 'Tis observable that in *Greenland* and *Nova Zembla* nothing but grass grows, as also what was told me by *Dr. Collins* the present Physician of the Emperor of *Russia*, that no thorny plant nor thistles grow in that Countrey. And this present year most of the *Rosemary* and *Sage* about *London* was wholly destroyed, besides most of the more tender Plants.

My fourth proposal was the properties and qualities of ice, some whereof my task engageth me to enumerate only, such are its slipperiness, smoothness, hardness, whereby and by its bulk and motion it breaks down
bridges,

bridges, &c. its firmness and strength to bear carriages, and burdens; its diaphaneity, which is much less then the liquor of which 'tis made. For I could never discern any object, though but confusedly, a foot beyond the clearest piece of ice, by reason of the many bubbles and luminous parts within it. Which bubbles shew only shadows, but the ice its self interposed betwixt your eye and a candle, appears in many round circles, from which proceeds many rays of light, four or five or more, in the form of a Star of about a $\frac{1}{4}$ of an inch in diameter, which so glaze your eyes, you can scarcely see any thing, but bright light and shadow.

As for its penetration and thickness something hath been said above, to which I shall add, that I have seen the *Thames* ice of the thickness of eight inches, or more near the middle of the River, and on the sides much more. And in Garden walks the earth frozen near two foot deep, whereas on the sides of the same walks, on a richer mould, the frost did not reach much above one foot

and $\frac{1}{4}$, and Pipes of Lead have been broken above a foot under the surface of the ground. I shall not mention the huge mountains of ice found in the most Northerly Seas, but proceed to its weight.

'Tis generally known, that ice swims upon the water. But I have seen snow-balls moistened only with water, and then compressed with a strong force, and afterwards frozen, to sink: besides the congealed oyl of Vitriol descends in water, and common ice is frequently observed under water; whether the solutions of salts frozen will sink, was by me forgotten to observe, and whether coagulated oyl will sink in unfrozen, as *Bartholine* affirms. Some affirm that snow-balls hard pressed, without addition of water, will sink, but experience teacheth me the contrary.

As for its tactile qualities, every one knows 'tis colder then water, which you may increase by adding salt unto it, or rather snow.

Smell it hath none, but it binds up that quality in all, but most spirituous bodies, which it also in some degrees refracts in them. Last-

Lastly, ice yields both reflection and refraction, whereof I shall speak when I come to its uses.

My fifth head was lets and helps in freezing, which I shall briefly dispatch. Those besides the North and North-east winds, the absence of the Sun, and the highest parts of houses or mountains, are the mixture of snow and salt (then which there's nothing more painfully and unsufferably cold to my feeling) as is apparent by the trick of freezing with snow and salt by the fire side, as also by the ingenious way of making cups of ice, invented by an incomparable person.

Add hereunto, that water falling or thrown upon ice or snow, soon becomes congealed. A mixture also of ice beaten into powder, and mixed with common Sea-salt (which is best) or with Kelp, Alume, Vitriol or Nitre. And here note, that vessels fill'd with water, and set in these mixtures, begin their freezing at the bottom of the liquor, and consequently are not so subject to be broken, as those are which are not

set in these mixtures, and that the water riseth higher with, then without them. I find also, that oyl of Vitriol alone, mixed with snow or ice have the same effect, though not so powerful.

One affirms, that Salt-peter dissolved in water, and put into a Bolt-head, and long agitated, not only cools the hand exceedingly (which is very true) but also converts it to ice, yea, in the very Summer month, which answereth not my trial, though kept a whole hour in that agitation in the hardest season.

This following Experiment also I add, proposed to me.

I fill'd a Bolt-head containing a quart of water, and set it in an Iron pan, surrounding it on every side with snow, which covered also part of the neck, and then set the Kettle over the fire, and took now and then the Bolt-head from the fire, whilst the snow was thawing, but not the least sign of freezing appeared in the water put into the Bolt-head.

As for the helps of thawing, take this Experiment. I set in the same
Cellar

Cellar three pans full of ice, one on *Newcastle* coal, a second on sand, a third on the earthen floor, they thawed in the same order they are mentioned, which was thrice repeated, and once that placed on the coal did thaw, when the other continued their ice. Seal'd glasses seem neither to promote or hinder this act of freezing. The same success I had with Eggs and Apples in my Cellar.

The last thing I shall speak to is the use of ice, you may therewith make a siphon, being fashioned and applied as usually siphons are, and this will happen, whether you make it one continued piece of ice, or two contiguous ones, for in both the water will run exceeding fast, and this siphon soon empties all the water out.

A second use is for refraction, whereof Mr. *Hook* hath given you already a learned demonstration. And I having formed some smooth ice into various figures, like most of those mentioned by the Dioptrick writers, the *φανόμενα* were the very same as in the like figured glasses; but how

Des-Cartes made Dioptrick glasses of it I know not, especially to make use of them : and lastly you may make a *speculum* of it, especially if a piece of blacked paper be placed behind it, and if you hold a candle at a convenient distance, there will appear very many *speculums* to your eye, according to the number of the bubbles contained in the ice. But I could not observe any heat proceed from ice, though cut in the true figure for burning-glasses, and exposed in naked ice, but frozen in spherical glasses it will heat a little.

I shall here subjoyn some propositions of learned *Bartholinus*, taken from his book *De Nive*, being near to the former Argument, who affirms.

1. That the more subtile distilled spirits gain a clear splendor and elegance from snow placed about them.

2. That the rays from snow newly fallen glitter, and excel in a kind of splendor wherewith the eyes are dazzled. Both these are true, and have

but

but one common cause, viz. the multitude of reflections caused by the infinite globuli, whereof every flake of snow consists.

3. That he saw Cabbage growing in his garden, putrified on that part, which was above the snow. 'Tis certain, that frost alone, with or without snow, hath this effect on Cabbage, being of the tribe of succulent plants, and I observed, that this year 1644. our great Houseleek or American Aloes (usually hung up in houses) kept in an upper room, was totally destroy'd by the cold. And that Apples will rot I have said before, and Housewives to prevent the rotting of Onions, commonly hang them up in their Kitchens, or keep them in Ovens, or some close place. And this present year 1662. I saw at Mr. Boxes, the eminent Druggists house, abundance of Squills or Sea-Onions quite rotten, they were laid not in an open, but close Garret.

4. When snow melts by the Suns heat, copious vapours from the Earth cloud the Sun. He should rather have said vapours from the melted snow, and 'tis no wonder, that vapours cloud the Sun.

5. Snow melts and falls off from

Ivy. I have observed all the sorts of *Ivies*, and ever-greens with us, and some biting plants too, but find in them all the contrary to what is here asserted. Nay, no difference hath been observed even in hoar frosts, which fall equally, and continues on all sorts of Plants.

6. He excludes not a small portion of earth from snow, though pure, which, saith he, is manifest from distillation. This Experiment I have found true by evaporation, which is tantamount to distillation, and indeed all melted snow leaves an earthy and foul settling behind it.

7. Viscosity with softness is greater in new, then in old snow, and therefore 'tis brought into a mass. Viscosity in it I understand not, its softness indeed is manifest too, by the tracks of beasts, which appear more fair, the snow not rising on the sides of the impression made by their feet (as it doth in old) but retains their perfect character.

8. Watercresses and Scurvigrass grow under the snow in Gardens. I apprehend not that any Plant whatsoever grows at all in hard seasons, my meaning is, that no Plant acquires any greater

er bulk of quantity, but keeps at a stand only, and this Country-men affirm of grass and corn, and Gardiners of other Plants. 'Tis true many Plants will upon thawing shew a finer Verdure, and if warm weather presently follow, all Vegetables will thrive exceedingly. For how they should thus grow when their nourishing liquor is congeal'd, and consequently become immoveable, I understand not.

9. Air is included in Snow, which this way of mine to make snow, fully convinceth. I took the whites of Eggs and beat them in the open air with a spoon, into a frothy consistence, as women do to make their snow possets, and then taking a little of this substance, and laying it on a trencher, it soon became plain flakes of snow, so that none that saw them could judge otherwise. Another accidental Experiment proves the same, for having put water into a Tube, and having long and strongly agitated it, there arose many bubbles at the top, which soon freezing (my agitation ceasing) became perfect snow. And now having here set down the way of counterfeiting, at least, if not of making snow, I will add how a pruina or hoar frost also may be imitated. I took

a Pail filled with warm water, and hung over it Hair, Moss, and a piece of Rosemary, now the atomical vapours rising from the water, fixing themselves on the Moss, Hair and Rosemary, became on them a perfect hoar frost. The like is dayly seen on the Beards and Hair of men and horses, travelling in cold Winter nights or mornings, proceeding from their breaths, steams of their bodies, or moist atoms of the Air. I tried also to make hail with drops of water, but could not hit on't, for they would never become white: whence 'tis manifest, that hail is not drops of rain suffering glaciation in the falling, as the received opinion of Philosophers asserts.

10. Snow abounds with fat. *This I understand not.*

11. Snow with ice swims on water. This is a clear consequence from the seventh assertion.

12. Snow-water boils meat sooner, and makes the flesh whiter. *I tried this in flesh and fish, but could find no manifest difference, either to their sooner boiling or whiteness.*

13. Snow newly fallen hath no taste,

taste, but lying long on the ground, or frozen, somewhat bites the tongue. *My taste was not so acute, as to distinguish the biting of one from the other. 'Tis true indeed, that snow frozen doth more affect the tongue with its coldness, then snow alone.*

14. Worms are sometimes found in snow. *This neither my own observation, nor relation from others can make out.*

15. From snow by a peculiar art, a salt of wonderful strength is drawn. *He saith not this of his own observation, nor teacheth the way to extract it.*

16. After much snow plenty of Nuts. This frequently suits with the Country-mans observation, but many times fails, such years also commonly produce plenty of Wheat, other seasons concurring.

I shall here also insert two remarks out of the same Authors concerning freezing. The one is, that the great Duke of *Tuscany* distilled spirit from Wine, only by putting snow upon the Alembick, without help of fire. The second, that the Duke of *Mantua* had a powder which
soon

soon congealed water into ice, even in the Summer.

And to conclude, take these general observations made by the command of the Royal Society, with Weather-glasses fram'd after the *Italian* mode, and fill'd in part with tinged spirit of Wine. Which I shall deliver briefly and ingross, and not each days alteration apart; I took then two of the said glasses of equal dimensions, as near as might be, and fill'd them with the same spirit of Wine, one of them I placed in my Study-window, standing North-west, the other in Mr. *Pulleyns* Warehouse under St. *Pauls*-Church, and chose there a small recess or room, which was most remote from the entrance, and the warmest in the whole Warehouse; both the glasses were settled in their stations the 15. of *October* 1662. the spirit in both having the altitude of three inches just. When the glals in my Study was depress'd, by the cold, an inch, I went and observed that in the Warehouse to have received no manifest change in its station. And
at

at a second visit the spirit was depressed $\frac{1}{4}$ of an inch below, when that above-ground was depressed near two inches. And during the long continuance of all that hard Winter, it never descended above $\frac{3}{4}$ of an inch, and never was higher there then three inches and $\frac{1}{4}$ in a mild season in *April* following, by which time the papers fixed to the glass, and whereon were fixed the degrees, was quite rotten, and the characters scarcely legible. And at the same time, that in my Study was raised to four inches $\frac{3}{4}$. By which it appears, that the said Warehouse was in the coldest season as warm as in a mild *March*, for at that station the glass in my Study stood, commonly betwixt two inches and 2. and $\frac{1}{2}$. And so indeed this place appeared to one that went into it at the coldest season. And to this purpose I several times sent in at night my hardest frozen liquors, which were constantly thawed in the morning, though it freezed exceeding hard above ground.

The glass in my Study, after two days hard freezing, was sunk below
my

my marks, into the very ball, so that I could make no farther observations concerning the cold above ground.

From the former observations, that popular error is manifestly refuted, *viz.* that Cellars and Subterraneous places are hotter in the Winter then in the Summer, which though they appear so to us, because they warm us in the Winter, and cool us in the Summer, yet they are not so in themselves, for it appears by the former Experiment, that in the coldest season the spirit was depressed to two inches and $\frac{1}{4}$, and rose in *April* to $3\frac{1}{2}$, and no doubt would have risen about $\frac{3}{4}$ of an inch higher, had it continued there till the hottest season of the year.

One thing more I observed, *viz.* that the tinged spirit of Wine had in this subterraneous Vault totally lost its colour, whereas that in my Study (two years after) still retains its former tincture.

Since the printing of the foregoing Papers, *viz.* 1664. (there being no frosts in *England* 1663.) I made these

these following Experiments.

Finding the third of *January* the season disposed to freezing, I exposed a Pint bottle of *Claret*, and a glass-Cane filled with *Canary*, a solution of *Sal Gem*, *Train-oyl*, and the Oyl of *Fructus musæ*, and on the fourth of the same month, the night being the coldest and sharpest that I ever felt, (which all I spake with the next day confirmed) the wind then blowing hard at South-west, I found in the morning all the liquors frozen, except the *Sal Gem* exposed in an earthen pan, which shewed at the bottom of the dish some seemingly *Crystalized salt*, the oyl of the said fruit became very friable, and of a milky white colour, but the *Train-oyl* only lost its fluidity, and became of the consistence of lost greece. And the same night a bottle of the *Rhenish Wine*, called *Backrag*, and another of lusty *White-wine*, standing in a room a story high, exposed to the said wind, had most of the *Wine* frozen in them, the ices whereof being taken out, tasted somewhat weaker then the *Wine* it self, All the

the same things happened the sixth night of the same month. It is to be observed, that the pint of Claret, and the Sack in the tube, were both frozen throughout these two nights, and after their double freezing and thawing, they lost nothing of their spirit, colour, and taste; nay, the Claret being a strong *Burgundy* Wine, though it often suffered glaciation and thawing for three weeks together, yet in all that time suffered no manifest alteration, but appeared the same to sense, as when it was exposed, in colour, taste, and strength.

As to the concentration of coloured liquors, Mr. *Haak* shewed me an Oval glass, having at one end a narrow Cane above an inch long, almost filled with water, tinged with *Cochineel*, frozen throughout, the ice round about, towards the sides of the glass, shewed wholly colourless, but that in the midst was of an exceeding high dye, but the ice that was raised to the neck of the glass, was lightly tinged with a scarlet hue. Hereupon having some flasks by me, I put
into

into one a strong decoction of Cochineel, and into another a like decoction of Soot, which being exposed to the air, and encompassed in a vessel with snow and salt, they did freeze to the thickness of an inch or more, and the air then beginning to relax, I broke the flasks, and the dissolved ice yielded a water totally colourless. I made also an Experiment with a very strong decoction of Gentian roots, which being exposed in a four ounce vial, the ice thereof had a far deeper colour, and bitterer taste in the middle, and towards the bottom, then towards the outsides of it.

And whereas *Barclay* relates, that King *James* being in *Denmark* to fetch his Queen thence, in the Winter season had his nose and ears in danger of Gangreening, which being timely perceived by some of the King of *Denmarks* Nobility, they caused the parts to be rubbed with snow, and so the danger was avoided; the same travellers affirm, that in the Northern parts, where men become stiff with cold, and almost frozen to
D d death,

death, that they rub the frozen parts with snow, or else cast the whole body into water, by which means the whole body is crufted over with ice, as Eggs and Apples are, as if the freezing Atoms did pals from the body frozen into the water or snow; and this way of curing Gangreens from cold, *Sennertus* doth prescribe. To make some Experiment hereof, I exposed flesh and fish, and found, that by immerfing them into water, they soon became more limber and flexible, and more easily yielding to the knife, and compassed with a cruft of ice of the thicknefs of about half a crown, manifest tokens of their thawing, and being cut, they discovered nothing of ice in them. This for more certainty, I often reiterated, as also in Eggs and Apples, above a dozen times, and never failed of unthawing them by this way. 'Tis to be noted, if you immerse the flesh, fish, eggs, or apples deep into the water, no ice will appear on their outfides, but only when you hold them neer the surface of the water.

As

As to the *Persian* Experiment mentioned by *Olearius*, of making huge heaps of ice to be preserved for cooling of their drinks, I observed, that by pouring water into an open Pan, or into a Flask gradually, some at one time, some at another, I could quickly freeze by this way a whole Flaskfull, when near half of a Flask filled at one, though helped by art, was unfrozen. I observed also, that the ditches betwixt *Southmark* and *Redderiff* had acquired an exceeding thickness of ice, caused by the flowing of the water in them at full Tide, for new water being brought in by the Tide, was there congeal'd to the thickness of some inches every ebbing and flowing. I observed also the ice on the banks of *Thames* above two yards thick; the inhabitants told me they had seen it three or four yards thick, which thus came to pass; the Tide flowing in, and meeting with great flakes of ice, drove them to the banks, and lodged them on the ice there frozen, which flakes uniting there with the former ice, raised it to that excessive height or thickness.

Besides every one may observe in *London* Streets, and elsewhere, in Channels where no constant current is, that water coming from the houses, soon fill the Channels with thick ice, for running but a little at a time, it freezeth almost as fast as it cometh thither. Nay, I have seen ice of some yards thickness in such places, where a small rill or stream of water gently falls on the side of a hill.

Amongst those things that will freez, Mortar and Plaister of *Paris* were omitted, and thence 'tis that Plaisterers and Bricklayers play all the Winter.

My Lord *Verulam* in his natural History (and some from him have affirmed to me) that Apples and Eggs covered with a wet cloath, will not freez, but I find no difference in those that are thus covered, and them that are not.

Add to those that sink upon congelation, all oyls from Animals, and from Vegetables, that are extracted by expression or boiling.

Add to those that freez not water and Sugar boiled to the consistence of

a Syrup, and also all other Syrups, none whereof I could ever take notice, or learn by others, that they would freez. 'Tis true, that water having an equal quantity of Sugar dissolved in it will freez, but with a little more mixed therewith, freezeth not.

To try the effect of cold upon Loadstones, I exposed several of them in the open Air, and also within rooms in the most severe weather, the needle being kept in a warm place. At other times I exposed the needle to the cold air, keeping the stones warm, at other times both were exposed, but in none of my Experiments could I conclude any thing certain to their attractive faculty, for the sphere of their activity was found to be sometimes greater, and sometimes less, to a considerable difference, in ten several good stones employed for this purpose.

I essayed also to find out a standard of cold, whereby to fit the tinged spirit of Wine for the Weather-glasses, and to that end made use of Conduit water, and the distilled wa-

ters of Plantane, Poppies, Black-Cherry, Nightshade, Scurvigrass, and Horfe-raddish; all which were first placed in the same room where a fire was kept, and then removed, and measured out into spoons in equal quantities, and also a drop of them dropt on the same bench, but though this was often tried, I could not make any sure inference from them, only I observed that the black-Cherry water did for the most part freeze first, but the other with very great uncertainty. The Horfe-raddish and Scurvigrass waters were for the most part froze last. The best way to discover the very beginning of freezing of liquors, is to move a Pin or Needle through the liquors, whereby the ice will be raised, and become discernable, when the naked eye can discover none at all.

F I N I S.



Fig: 1.



Fig: 2.



Fig: 3.

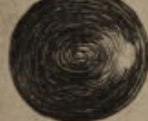


Figure 1. Page 9, 10, 11, & 98.

A the Ball or Egg.

B C the Stem.

D the little Aqueous Cylinder.

Figure 2. the open Weather-glass mentioned pag. 24, & 43.

Figure 3. the seal'd Weather-glass or Thermoscope mentioned pag. 24, 55, 56.

Figure 4. the Barometer or Mercurial Standard placed in a Frame B B mentioned pag. 25.

Fig: 4.



B

Fig: 6.

Fig: 6.

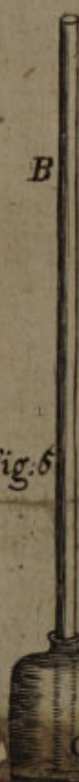


Fig: 7.



Figure 5. an Instrument mentioned pag. 93.

A the Vial.

B C the Pipe cemented into the neck of the Vial, open at C and seal'd at B.

Figure 6. pag. 97.

A the Bolt-head.

B the small Stem.

B C the Cylinder of water inclos'd.

Figure 7. pag. 101.

