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*, 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.

LONDON.

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

National Oceanic and Atmospheric Administration

Rare Books from 1600-1800

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THE PUBLISHER TO THE INGENIOUS READER.

I Am ſully 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 furniſht 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, touch-*

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 appear'd)

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 slowness of the Press, and therefore allow'd himself to subjoyn to the *History*, the discourse of *Antiperistasis*, and the *Examen* of Mr. *Hobs*'s Doctrine *, as belonging to the same subject: 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,

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

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, composed by Mr. *Boyle*, I have no more to say, but that the Author being so Generous as to oblige Forrain Nations as well as his Own, has already taken care of having it put into Latine, Farewel.

London, March 10. 1664.

H. O.

THE

THE AUTHORS

PREFACE

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 satisfie the first of these Queries, I have several things to say.*

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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 called 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 oftententimes 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.*

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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.) 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,*

Sceptical Chymist.

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 uncapable to be* improved.

Another inducement to me was, that having

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 remembring, 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.*

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

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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

 treated

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

what circumstantially, not only the Events, but the Manner of my Trials, that I might at once, both the better satisfie the scrupulous, and be assistant to those that would examine or repeat such Experiments, and also gratifie 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

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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 then once, by Perſons that are meer*

ſtrangers

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-

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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 expresly taken notice of them, either because I judg'd them easie enough to be

answered

answered without my help, or because the things themselves were not considerable enough to deserve a long or sollicitous 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 my self 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

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 indeavour, that those Experiments, which for want of opportunity I my self could not try, might be tri'd by others, who may be befriended by more savourable 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

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 sometimes

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 satisfie my self 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

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 ostentimes 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*

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 injoy 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*

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*

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,*

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 Mechanitians 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 then 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 Unsuccesfulness of Experiments.

 what

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, then 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 suspition 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 justifie me for having said, That Weather-glasses & 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.

* 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

ONE

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

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 Præliminary 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 Sphærical 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

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*

times Westward, *and varies that Declination uncertainly as to us, does nevertheless so far respect the North, as in spight 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 Jejunely; 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,*

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 needlesly 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 my self make in this Temperate Climate; I must either make*

use

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 Acounts 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 ingag'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

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*

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 satisfie 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 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 insuing 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

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 indeavouring 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,*

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

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*

is of very suspected Credit, and delivers some things upon hear-say, which, they are kinder to him then I, that are pleas's'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. 1. Titulo de frig. Asperitate, pag. 9.

And

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*

competent Judges appear to have made of him. For having been, not only imployed 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

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 puzling 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*

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 then 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

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*

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*

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 sollicitously 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-*

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 sollicitations 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 with-hold from the Curious what assistance my Collections could afford them, to make use of this Winter to prosecute Experiments of Cold. And remembring how favorable an entertainment my former Endeavours to gratifie Ingenious Men, had found among them, I took a Course,*

wherein

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, whilest I was writing the following History, from adventuring upon some Trials, that might (probably) have inrich'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*

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 Imployment, to Dig in Mines of Copper, then in those of Gold: and Men being oftentimes obliged 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.

The Contents of the Experimental History of Cold.

Title I.

Experiments touching Bodies capable of Freezing others. pag. 108.

Title II.

Experiments and Observations touching Bodies disposed to be Frozen. p. 133.

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Experiments touching Bodies, Indisposed to be Frozen. p. 140.

Title IV.

Experiments and Observations touching the degrees of Cold in several Bodies. p. 149.

Title V.

Experiments touching the Tendency of Cold, upwards or downwards. p. 173.

Title VI.

Experiments and Observations touching the Preservation and Destruction of

(*Eggs, Aples, 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.

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

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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 Congelation, and of highly compressing Air without Engines. p. 382.

Title XIII.

Experiments and Observations touching 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.

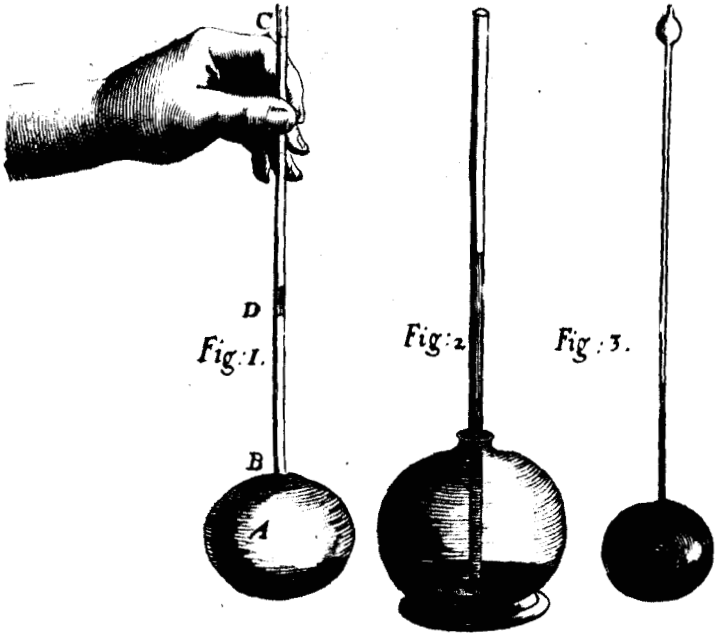

Figure 1. *Page* 9, 10, 11, & 98.
A the Ball or Egg.
B C the Stem.
D the little Aqueous Cylinder.

Figure 2. the open Weather-glass mentioned *pag.* 24, & 43.

Figure 3. the seal'd Weather-glass or Thermoscope mentioned *pag.* 24, 55, 56.

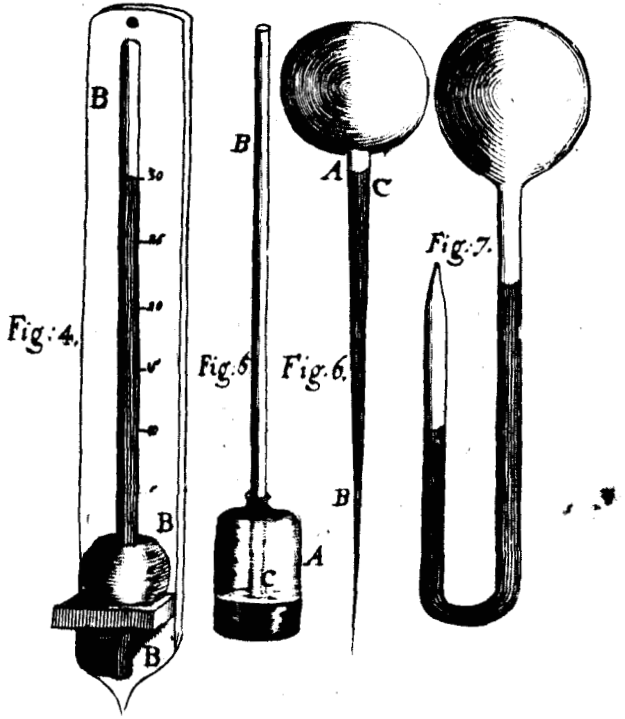

Figure 4. the Barometer or Mercurial Standard placed in a Frame B B mentioned *pag.* 25.

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.

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 appear a work of needless Curiosity, or superfluous 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

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 imploy 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 uselefs 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 satisfie 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 not 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 Chirurgion 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 Feavers, and some other distempers; or produce that strange and universal Coldness of the external parts, which is frequently enough observ'd among other Symptomes 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 (whose 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 Bason 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 Baſon 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 those

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 Exercile 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 *Phænomenon*. 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 Ἰδιοσυγκρασία.

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 pretermit that memorable Account, that is given us by *Martinius* in that Noble piece of Geography which he calls *Atlas Chinenſis*, where ſpeaking of the Air of that populous Countrey he has this ſingular paſſage: *Ad Cali* (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 concrescunt gelu, ut currus equoſque*

Pag. 27, 28.

as

ac gravissima etiam onera glaciei ferat, innoxiè ac securissimè transeant: ex iis ingentia etiam glaciei frusta exscindũtur, quæ in futuram æstatem ad delicias servant. His mensibus omnes naves ita in ipsâ glacie defixæ sunt, ut progredi nequeant ubicunque illas frigus occupat (quod certo certius circa medium Novembris ingruere solet) per quatuor illos menses immotæ ibi perstare coguntur, neque enim resolvitur glacies ante Martii initium; hæc plerumque glaciei concretio uno fit die, cum non nisi pluribus fiat liquefactio; to which he adds what makes most to our present purpose, *omnino illud mirum, tantum non videri aut sentiri illud frigus ut Europeos ad hypocausta subeunda videatur posse cogere, aut in Europâ ad glaciem producendam sufficere unde ad subterraneas illic exhalationes pro harum rerum causis indaganda omnino recurrendum est*, &c.

But all that I have been implying of the Necessity and Usefulness of the Weather-glass, is no way inconsistent with the truth of the latter part of our formerly propos'd paradox, namely that we are not rashly to rely upon the Informations even of common

Weather-glasses themselves. For though they be an excellent Invention, and their Informations in many Cases preferable to those of our senses, because those Dead Engins are not in such cases obnoxious to the same Causes of uncertainty with our Living Bodies, yet I fear they have too much ascribed to them, when they are look'd upon as such exact Instruments to measure heat and cold by, that we neither can have nor need desire any better. For, not yet to mention some inconveniences in the contrivance of them, which makes them unapplicable to some purposes, and less proper in others, then Thermoscopes *might* be made, even in divers cases, wherein they are presum'd to be unexceptionable, their Reports are not to me, I confess, quite exempt from suspicion. For in ordinary Weather-glasses some part of the Liquor being contiguous to the External Air, it is subject to be impell'd more or less upwards, not only according as heat or cold affects the included Air, but according as the incumbent Air happens to be heavier or lighter.

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*, aud 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 *Atmosphærical* pillar, that presses upon the Water in the Weather-glass, is considerably longer or shorter, lighter or heavier then is usual.

See the 18. *of our* New Physico-Mechanical Experiments.

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 *Atmosphære* 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 then 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 then That would upon the *A*mbient *A*irs growing more heavy. But the particular *A*ccount of this *A*ttempt belonging to another place, the trial ought not to be more then 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 *A*ir, the weight of the *A*tmosphære 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 *A*ir as to Cold and Heat, but the differing gravity of the *A*tmosphære, 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 *A*ir, as is more fully made out in the Treatise above related to. *A*nd having by the intervention of a Learned *A*cquaintance desir'd to have some Experiments made of the Effect of the *A*ir upon Weather-glasses in deep Pits or Mines, where the *A*tmosphærical 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. *A*nd 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 *A*tmosphærical 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. P.*

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 Mercury in the Torricellian Tube (which I keep purpoſely by me in a frame) were riſen or fallen, and conſequently 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 *A*tmosphære 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 *I*ncumbent *A*ir, was no less then 17. Divisions higher then the others, and comparing the height the two *I*nstruments 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

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-glasses, 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 ¼ 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 ¾ 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, whilest 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 Quickfilver 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, whilest 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 Glass was no lower then the 41: nor probably would there be now so great a difference, if the Atmosphære 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 *I*nches, and a pretty deal leſs then a half.

*S*ince 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 ſet 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 regiſtring 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 occurr'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 ¼ 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 ſhow what Effect ſo great and ſudden a change of the Atmoſphæres gravity would have upon it; I ſaw the tinctted Liquor in the ſhank depreſs'd a full Inch or more beneath the Surface of the Ambient Liquor in the Viol,

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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 whereto 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 setled 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 satisfie 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 resent 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

An ingenious man has some hopes that another way of setling a Standard for Weather-glasses; namely, by observing the coldness, which is requisite to make distill'd water begin to freez: But though the accuracy of this way may be as well as the other justly Question'd, and cannot often be put in practise, even in Winter it self, nor without trouble; yet it may also be advantagiously made use of, when the cold happens to be great enough to freez water.

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 it self, 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 Sphærical, 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 recongeal'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 setling 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 Cylindri-

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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 Sphærical 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 discrib'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 therewith 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 External, 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 Cylynder, slender enough, the proportion between the part of the Viol possest by included Air, and the Cavity of the Cylinder, in which the Liquor is to play up and down, may be easily made so great, as to make the Liquor 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 rariſi'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.

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III. I consider too, that though men are wont confidently enough to conclude, that in case (for instance) the Coldness of the weather make the Liquor in a Thermoscope yesterday an Inch higher then 'twas the day before, and this day an Inch higher then 'twas yesterday, the Air must be this day as cold again as it was yesterday, or at least that the increase of Cold must be double to what it was yesterday, and so in other proportions, yet the Validity of this Collection may very justly be Question'd; For, though we should grant, that Cold is that which of it self, or by its own power contracts the Air, yet how does it appear, that a double degree of Cold must produce a double degree of condensation in the Air, and not either more or less. Since besides that 'tis taken for granted, but not prov'd, that the differing Quantities of included Air in several Instruments, and the differing bignesses of the Pipes, and the differing degrees of Expansion, wherein the included Air may happen to be, when the Ascension of the Water begins to

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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 superadded to the former, (which makes that a double force, or equivalent to 20 pound weight,) will drive up that already compress't *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 possest, and to what degree of density it was reduc'd, before the application of the then force; and we must remember, that the resistence 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

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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 ſubtile 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 *Aquafortis* it ſelf, and yet theſe may be congeal'd by far leſs degrees of Cold, then ſuch, as would yet prove ineffectual to freez 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,

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why it ſhould be abſurd to ſuſpect, that though there be ſome kind of Bodies, which ſeem fitted to produce 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 remiſneſs proceeds) it ſeems not impoſſible, but that beſides thoſe Bodies or Corpuſcles, that may be look'd upon as the Catholick Efficients of Cold, there may be particular Agents, which in reference to this or that particular Body may be call'd frigorifick, though they would not ſo much refrigerate another Body, which perhaps would be more eaſily affected, then the former, by other efficients 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 formerly

merly related upon the credit of that great Traveller, the Jesuit *Martinius*, 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 then 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 my self 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

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the various Agitation of the minute parts of a Liquor, whereon its Fluidity depends, may be hindred or suppressed by the intervention of adventitious Corpuscles: But do not *clearly* show, That the Liquor by being deprivʼd 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 offerʼd 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 those 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 cauſed 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 tried 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 Atmoſphære (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 rendred 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 coarser Liquors used in common Weather-glasses, which are some of them not unapt to freez, 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) 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, then 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 *Effluviums* of the Loadstone do readily per-meat 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, though ten 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 whilest 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 Item 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 *I*nstrument 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 *I*nch; 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 *I*nches 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

as an *Argument* by a famous Mathematician (the Jesuite *Casatus*) whose expressions are such, as if he himself had observ'd, that even in stopt Glasses, the forementioned 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 stopt 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 quiescere 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 Præliminary 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 Præliminary Discourses, who thinks it requisite to give the Reader this Advertisement, because *Casatus* himself did not, as he should have done, intimate that he de'iver'd this but upon anothers credit.

ſenſibly ſo work, as *I* had not obſerved it to do upon other Liquors, wherein the ſpirit of Wine abounded. *A*nd 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

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 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 congealablenels of Oyl of Aniseeds, 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 unskilfully 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 loaden 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' Hydrographie du P. Fournier, 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 preferrable 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 Atmo-

ſphære, as well as by Heat and Cold, may (upon that, and perhaps ſome other accounts) eaſily miſ-inform us in ſeveral caſes, unleſs in ſuch Caſes we obſerve by other *Inſtruments* the preſent weight of the Atmoſphære.

3. That the *ſeal'd Weather-glaſſes*, we have been mentioning, are ſo far preferrable to the *Common* ones, as (eſpecially they not being obnoxious to the various preſſure of the external *Air*) that there ſeems no need in moſt caſes to decline their reports, or poſtpoſe Them to thoſe of any other *Inſtruments*: But yet in ſome nice Caſes it may be prudent (where it may conveniently be done) to make uſe alſo of other ways of examining the Coldneſs of Bodies, that the concurrence or variance to be met with in ſuch ways of Examination, may either confirm the Teſtimony of the Weather-glaſs, or excite or aſſiſt us to a further and ſeverer inquiry.

4. That *I* would not have Men too eaſily deterr'd from deviſing and trying various Experiments (if otherwiſe not unlikely or irrational) about the

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 ill grounded. 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 inrich 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 suted to the present Occasion, yet the main Body of the Discourse may be (I think not improperly) annex'd to what has been already said about Weather-glasses, since it examines the causes of the principal *Phænomenon* of them, and will perhaps help to discover the incompleteness of mens Notions about Cold, by showing that the true cause, ev'n of the most obvious *Phænomenon* of Common Weather-glasses (though almost every man thinks he understands It) has not yet been sufficiently inquir'd into.

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

—To prosecute our Disquisition satisfactorily, it will concern us to consider, upon what Account the water rises in Cold Weather and falls in Hot, in common Weather-glasses, whose Construction being so well known, that we need not spend time to set it down, we may forthwith proceed to take notice, That concerning the reason, why in these

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 possest, 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 ¼ 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 *A*ir expanded into more space then they respectively possest before; *I* see not, how a *Vacuum* or a worse *I*nconvenience will be avoided; for *I* demand, since Glass is granted to be impervious to *A*ir 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,

*A*ir, 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 *Brouncher*, to indeavour 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 *A*rticles 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 *A*ir, that is to say, by a constant wind that presseth them.

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 proofless 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 *raked* by the wind, as Mr. *Hob*'s conceipt 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 otherwhere. 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. 11. of the same 30. Chap.

make out the *Phænomenon* according to his doctrine. Besides that his Explication gives us no account of the Condensation of the Air by cold in such Weather-glasses, as those, wherein the water descends with Cold and rises with Heat.

Fourthly and lastly, whereas Mr. *Hobs* takes notice of no other cause of the depression of water in Weather-glasses by Heat, but its own gravity, he seems to have but slightly consider'd the matter. For though in some cases the gravity of the water may suffice to depress it, yet in other cases that gravity alone, will by no means serve the turn, but we must have recourse to the expansive Motion or spring of the Air included in the Cavity of the Glass. For if you place a Thermometer with a large Ball, wherein the water ascends but a little way into the shank, in a window expos'd to the warm Sun, you will often perceive the surface of the water in the Pipe to be a good deal lower, then that of the water on the outside of the Pipe, which shews, that this depression proceeds

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 it self, 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 possest before, the water in the subjacent 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 whirles 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 increased by the Heat, as may appear, if the seal'd Tip be broken under water, by the eruption of Bubbles by the indeavour 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 distroy'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 possest 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 3½ 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 poſſeſt 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 latitant 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

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

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

See more concerning these Weather-glasses in the first of these three Discourses.

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 ſeald, 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 Atmosphære, 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 depreſt, according as the weakned 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 so 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 among 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;

That there is ſomewhat or other in the Buſineſs of Weather-glaſſes, which (I fear) we do not *yet* ſufficiently *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 being to be brought in, and preſented to the *Royal Society*, it was put into the hands of (its moſt worthy Preſident) the Lord Viſcount *Brounker*, was as followeth.

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, concerning 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 Perusal, if you were not aswel qualified to supply Deficiencies and Imperfections as to discern them. For of all the Old Observations, 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 season, besides that I was several times diverted 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, hindred 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 London, *and was thereby reduc'd to leave several trials, I should have made, either unattempted, or unprosecuted. But lest You should think, that, what I intend only to excuse my unaccurateness, 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 *ſubjects* 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 whileſt 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

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 powred 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* then *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 *Vitriol*, or *Sal Armoniack*, 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 Corpuscles, 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

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 freez ſo much as a little *Eſſence*-bottle full of common water.

5. Wherefore we bethought ourſelves 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 stopt the vessel, did carefully shake together, and thereby agitate the mixture in it, yet the Glass 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 signifie 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 occasi-

ons

ons we seal'd up the vial, that contained the snow and the other frigorifick 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 glass, 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 glass, and in a pretty thick glass stopp'd only with a Cork.

9. Afterwards we successfully 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 Empyreumatical 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 sublimate 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 intenſly 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 ſlower 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 freez liquors of more difficult conglaciation then fair water.

We took then some snow, and mingled with it some of the newly mentioned spirit of Nitre in so luckly 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 diſcernably 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 Icekles croſſing each other, and quickly vaniſhing, but (which was far prettier) having often obſerved, that *Sal Armoniack* being diſſolved in water, and the ſolution being put very ſlowly to evaporate 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 reſembling combs with teeth on both ſides, and others reſembling feathers; having obſerv'd this, I ſay, and being deſirous to try, whether the ſpirit of *Sal Armoniack*, diſtilled by the help of quick Lime, being put to congeal on the outſide of a glaſs, would not afford a Reſemblingly figured 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

cording 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 ſwiming 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 ſetling 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-

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, afters 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 proceded 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

Title II.

Experiments and Observations touching Bodies Disposed to be Frozen.

1. 'TWere 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 Alume, 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 Alume, 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 glaſs, 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 lixiviate 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 rocked 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 expressly 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) *fit in Laneis Calcibus, non pellibus, aut Coriis unctis: vis enim frigoris, quodcunque sit unctuosum convertit in Lubricitatem glacialem.*

Pag. 58.

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

ſed 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. WE 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 mentioned trial was made with water and Pot-ashes, we likewise, in another glass, exposed a solution, wherein the proportion of salt of Pot-ashes, in reference to the water was four times greater, there being in this ℥ij of the salt to ℥i only of water, and this solution, though the glass 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 considerably strong solution of salt of Pot-ashes, that was exposed with it, did. So that these Experiments about the glaciation of Lixiviate Liquors must be repeated to be reduc'd to a certainty.

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 matter 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 convenient vial, continued fluid; notwithstanding a more then ordinary sharpness 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 unfluid; which differing Events seem a little to Countenance, but more to disfavour the Report of *Olaus Magnus*, who writes, That whereas in Northern Regions 'tis usual for strong places to lose in winter the protection afforded them in Summer, by their Ditches, though never so wide and deep, because the frost makes them easily passable to the Enemy: This inconvenicy is wont to be prevented by pouring into the Ditches, the Ice, if there be need, being first broken, great store of this Train oyl, which swimming upon the surface of the water, and being incongealable

Olaus Magnus in Historia Gentium Septentrionalium, lib. 11. cap. 20. & 21.

gealable by the cold, protects the subjacent 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

 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.

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 meritus, in glaciei frustis è portu nostro allatis.*

Cap. 6. pag. 42.

Title

Title IIII.

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

1. AFter 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, then 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 imploy'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 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

See the Prælimi-nary Dis-courses.

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 maing 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 ſollicitous 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 imploy'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 guess 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 5¾ 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 1½ 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 5½: 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 ℥i, ʒi, 56½ 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 careſully 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 together, which weigh'd ℥ij, ʒvii, and 34½ gr. then we put in by degrees a quarter of a Drachm, and with a Diamond careſully mark'd, how high it reach'd in the pipe, and ſo we continued putting in ſeveral Quantities of oyl, ſtill careſully 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 Liquor and the Glaſs together weighed ℥iij, ℥j, 4½ grains; then we put fair water

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, containing 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 Diamond this ſtation alſo of the Liquor, we afterwards remov'd the Glaſs into a warmer Air, till the Oyl by expanding 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

* *See the latter part of the next Title.*

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 9½ 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 Glass, 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 Glass from the weight of the whole Oyl and Glass, to obtain the weight of the oyl alone; and having divi-

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 Bolthead, 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 circumposition 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 Glass; 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 glass) 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 freez before the end of the same hour, at whose beginning the there warmer Air had given it its greatest Expansion, and so the Diffe-

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 reason 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.

11. 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 retrigerated, 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 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,

if we may judge of the ſhrinking and condenſation of the water by the Increment 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 delivered concerning the very inconſiderable 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 Turpentines 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. Whether 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 conclude, 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 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 *Norway*, 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 reſiſtence 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 imploy 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 observed. 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 differingly agitated, as common Water, high rectifi'd spirit of Wine, and even rectifi'd oyl of Turpentine, (I add not *Dephlegm'd oyl of Vitriol*, because of some odd *Phænomena* 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 Imployments for his Time, was willing to decline ſo tedious and nice a Task.

Title

Title V.

Experiments touching the Tendency of Cold Upwards or Downwards.

1. THough, 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, aud 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, wherewith 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 freez 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 Glass was cas'd with Ice, or covered with hoar frost, directly over against that part of the inside of the Glass, 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

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

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 Paticular 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 glaſs-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 then before, and forcibly endeavouring to Expand it ſelf every way, finds it leſs difficult to burſt the

glaſs, then liſt 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 freez 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 liſt up the incumbent liquor toward the higher and empty parts of the glaſs: And this liquor alſo I could afterwards freez 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 freez 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 freez 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 freez, 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 *explicitely* 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.

Title VI.

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

1. IT is a Tradition common enough, though not here in *England*, yet among thoſe that have given us Accounts of very cold countreys, 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 harmleſly, 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 relye 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 cruſted 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 woodden-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 (*cæteris paribus*) faſter when immerſed in water, then when ſurrounded only with Air.

5. [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 suspition, 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 then 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 freez 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 mention'd, 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 Glaſs, 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, whilest 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-

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 freez 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 ſatisfie 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 contexed 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, whose 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 plaustro suo impositum, cum deduxisset*

Gulielmus Fabritius Hildanus de Gangr. & Sphacelo Cap. 10.

isset in Diversorium, hospes illico demersit in frigidam, quo facto undiquaque ita erupit Gelu, ut ipsius Corpus glacie, seu ferreo Thorace contectum conspiceretur. Tum quoque propinatum illi aiebat Cyathum ampliorem Hydromelitis, quo illi seu potu ordinario utuntur, addito pulvere Cinamomi, Caryophyllorum & Macis, unde sudor in lecto provocatus est; atque ita ægrum ad se rediisse amissis dum ax at 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

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 Lucifer-ous.

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 discomposing or vitiating their Texture) when they come to be thaw'd, though whilest the Frost is in them, it keep 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 then 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 sine arctè compactam servant in Cellis Nivariis, in quibus fervente æstate vidi carnes mactatorum Animalium à putredine diu se conservasse.* The next thing I shall mention to our present purpose, 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 Coldest Regions of the World, was not great enough to spoil the meat and drink, that had layen all that time under 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 water all the Winter; yet we could not perceive, that it was any thing the worse;* which is the more remarkable, because 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, if 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 jote 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 Rinwaldios, 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 quærantur. Vidimus ipſi triſte hoc ſpectaculum*, &c. Barthol. de figurâ nivis pag. 79

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

not moſt inanimate Bodies, (I ſay *inanimate*, becauſe of the *Gangrænes* and *Sphacelations* that often rob living men of frozen Toes, Noſes, and ſometimes other parts) if they be actually frozen, will not diſcloſe any putrefaction, whileſt 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 Avolations, 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*. *Notandum, Corpora occisorum hyeme eodem positu, eademque figurâ permanere rigidâ, quâ ante eadem deprehensa sunt. Visum id extra urbem nostram, quum* 11. *Feb.* 1659. *oppugnantes hostes repellerentur, magnaque 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 resoluto,* Barthol. de usu Nivis pag. 83.

eques

eques equo suo insidens integer emersit, 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-

Capt. James's Trav. pag. 76.

Merchants of *Copenhagen*, that return thither from *Spitzberg*, a place in *Greenland*, *The extreme Cold will there suffer nothing to putrifie and corrupt, insomuch that Buried Bodies are preserved 30. years intire and inviolated by any rottenness.* Barthol. de usu Nivis Cap. 12.

21. Thirdly, though whilest Bodies continue frozen, the cold (as may be supposed) by arresting the insensible particles, from whose tumultuary motions, and disorderly Avolations Corruption is wont to proceed, may keep the ill operations of Cold upon the violated Textures of Bodies from appearing; yet when once that impediment is removed, divers bodies make haste to discover, that their Texture was discompos'd, if not quite vitiated by the excessive cold. I might alledge on this occasion, that I have shown divers ingenious Men by an Experiment I have taught in another * *Treatise*, that the change produc'd in the Textures of some Bodies by glaciation, may be made manifest even to the sight. For by freezing an Oxes Eye, the Crystalline humour, which

* *Of the usefulness of Experimental 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, 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*

Purch. Lib. 3. cap. 5. Sect. 2. pag. 493.

Pag. 73.

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 throughly 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 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.* Pag. 67.

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 suffi-

ciently

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 privileg'd from the Destructive Operations of some Degrees of Cold. For, *Ex ære facta opera* (says he in his Curious *Musæum*) *vi frigoris quandoque rumpuntur, quod tamen pauci credunt, id tamen expertus est Eratostenes, & Nostras Johannis 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*

Lib. 1. *Sect.* 3. *Cap.* 5. *pag.* 122.

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 Josam Barrels, were also frozen in pieces. But though this Testimony seems to prove, that extreme Cold may break even Iron it self, 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 mentiond 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 benummedneſs, 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 throughly 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 unputrified 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 rosted, but if before it was thaw'd, it were laid down to the Fire, it would not ever be well rosted, 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 satisfie 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. THat 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 then 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 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

Nicholaus Zucchius, & Melchior Cornaeus

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 it 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 medling 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

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 stopt 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 Glass 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 Nature's 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 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 freez 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 cruft of Ice increafes 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 satisfie 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 glass-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 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 Peripatetieks 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 waterifh 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, then 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 conſi-

der

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 invisible 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 Isicles 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 so fasten themselves to the walls, to the Roof, and even to their Cabins,

It froze so sore within the house, that 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

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 absence of the Sun for many weeks together, should not all the Winter long be aware of this. Besides this, I say, I find that in Captain *James*'s wintering 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 congruity between the Teeth, *&c.* as I have tri'd, that some parts of an Iron Instrument, I caus'd to be made, would by no means fit one within another, when

Pag. 64.

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

Title VIII.

Experiments touching the Contraction of Liquors by Cold.

1. BUt 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 exposed 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 Bolthead 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 infrigidated, 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 begining 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 sight 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.

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 satisfie 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

could be brought to freez these oyls, and whether it would expand or condense them; wherefore exposing, in conveniently shap'd vessels, some good oyl of *Guajacum*, that was diaphanous enough, though very highly colour'd, to the greatest Cold we could produce, we attempted, but in vain, to deprive it of its fluidity. All that we were able to effect, being to make it very manifestly shrink.

Title

Title IX.

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

1. SInce 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 Argument, to take some notice of the levity 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 oftentimes 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 Cases, 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, inſomuch 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 glass to be pervious to common Air, we shall at present press them with this Experiment, which we have divers times made.

We took a glass 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

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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 ſatisfie ſo jealous an obſerver. But yet I ſhall annex our trials, *not only* becauſe the thing has not been, that we know of, ſomuch 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 latitant Air can adequately *fill*, unless the same parcel of Matter could truly *fill* much more space at one time then 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

that pre-existent in the water, or stollen 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 then 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 throughly frozen, and then we found

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it to have rifen a great way above its firft height, and removing it into an Air temper'd like that wherein the firft part of the Experiment was made, & having left it there in a quiet place for ten or twelve hours to thaw leifurely (left too warm an Air, or too much ftirring the glafs might be an occafion of generating new bubbles,) in the exterior part of the ice near the glafs, we faw pretty ftore of bubbles, but when that was thaw'd, the reft of the ice appeared of a peculiar and unufual texture, having no determinate bubbles, that I could eafily diftinguifh, but feeming almoft like a piece of frofted glafs, where the Parts, that made the Afperity, were exceeding thick fet, but this ice fwam in the water, wherein-to the reft had been diffolved before it was all thawed: when there yet remained a lump about the bignefs of a fmall Walnut, we reconveyed it into the Receiver, to try whether upon the exuction of the Air, the ice would be prefently melted, but the alteration produced, was fo fmall, if any, that we durft not ground any

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 wrs 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

wch 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 some thing that is already delivered, or is hereafter to be delivered in the present *History of Cold*.

11. [We took this day a glass of the form of an Egg, but of double 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 *Feb.* 4. 1661.

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 groſly 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 ¾ 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 nimbly 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.]

13. [An

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 *Laminæ*. After an hour and a quarter, those *Laminæ*, 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 bignes 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 Bason into the Cavity of the pipe, insomuch, that when we took it out, which we did, as soon as we thought nomore 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 ſubtile 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 Baſon, with ſome *Impetus* and noiſe driven up into the Cavity of the glaſs; and, the glaſs being ſeaſonably and warily remov'd from the Baſon, we found there had been impell'd up of the water in the Baſon, a little more then eleven inches, ſo that there ſeem'd to be near *Decemb.*

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

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 guess 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 Contraction of Liquors by Cold.

1. TO the Experiments (mention'd in the Seventh and Ninth Titles) which shew, that water has an Expansion, it will be proper to subjoyn some of those, whereby we endeavoured to measure that Expansion. And here we shall not content our selves to say, that whereas the Authors, we had formerly occasion to point at, take notice of their having raised 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-

panſion 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 glacia-

tion;

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

weighing as much as the whole Body. For Captain *James* in his often cited Voyage, makes mention of great pieces of Ice, that were twice as high as the Top-maſt-head of his Ship.

6. And the *Hollanders* in their famous Voyage to *Nova Zembla*, mention one ſtupendious Hill of Ice, which I therefore take notice of here, not only becauſe it has been thought the greateſt that men have met with, but becauſe they deliver its Dimenſions, not as Captain *James* and Navigators are wont to do, by compariſon with the unknown heights of ſome of the Maſts of their Ships, but by certain and determinate Meaſures, which in the Icy Iſland, we are ſpeaking of, were ſo divided by the ſurface of the water, that there was 16. fathome extant above it, though there were but 36. beneath it, which though a vaſt depth in it ſelf, yet does but little exceed double the height.

And the *Daniſh* Navigator *Janus Munckius*, imploy'd by his King to bring him an Account of *Greenland*, mentions ſome floating pieces of Ice, that

Barthol. de Nivis uſu Chap. 6.

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 saltness 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 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 then 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 intensly refrigerated, and thereby more condens'd then 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 suggested 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 that these floating Hills and Islands of Ice, are not intire and solid pieces of it, we shall otherwhere 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, were *compiled of store of Snow frozen together*.

* *In our Hydrostatical Paradoxes.*

Ex nive copiosa glaciata compacta.

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

 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-

Pag. 14.

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 immerſed 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. fathome, 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 obſerve, that of this Icy mountain it is ſaid, that *it lay faſt on the ground.* So that as on the one ſide it ſeems probable, 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 related, that his company was

*Mr. Hudsons Voyage for the discovery of the North-west passage, written partly by Mr. Abacuck 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 hindred 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.

11. 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

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

there or here) the water expos'd to freez be put into a vessel very advantageously shap'd, or brought out of some warm Chamber or other place, where the heat of the Air, that surrounded it, had rarifi'd it. But to examine the measures of Contraction 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 *Degrees of Cold*, touching the contraction of spirit of Wine, and of oyl of Turpentine, by the differing degrees of that Quality. And as for the condensation 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 Experiment will be more opportunely deliver'd in * one of the following Discourses.

* In the Sect. about the Temperature of the Air.

Title XI.

Experiments touching the Expansive Force of Freezing Water.

1. HAving 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 endeavoured 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, capable to contain, as we gueſs'd, about 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 metalline, and though purpoſely for this trial we had choſen it quite new, the crack appeared to be in the very ſubſtance of the Pewter. This Experiment 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 Triffle, ſo great a Monarch, and ſo great a *Virtuoſo*) bring to his Majeſty, to ſatisfie him, by the wideneſs of the crack, and the Protuberance 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 scru'd 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 Packthread, 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 $\frac{1}{16}$ of an inch, and the thickest $\frac{1}{8}$ 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 whileſt 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 mettal 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 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 metal 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 thee 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 then 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 reſiſtence, 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 possest before it began to freez; whereas Air may by Heat (which * yet we have 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 † *Mersennus*'s observation, seventy times, the Dimensions it had before Rarefacti-

* *New Exp. Physico-mech. Exper.6.*

† *See the forecited place.*

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 successful, 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

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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 ſatisfie 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 reſiſtence 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,

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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 permeat 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 Expansive force of Congelation, and of highly compressing Air without Engines.

1. THere 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

Dilitation

Dilatation of the one, and the compression of the other; our Experiments being made in a place where we were not provided of such glasses, 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

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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

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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 then 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

bignels of my middle finger, and pouring in water, till it reach'd about a fingers breadth higher then the bottom of the ſtem, we ſet 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 another 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 adding freſh for about 24. hours to obſerve, 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 (accounting by Eſtimation for the ſlender pipe newly taken notice of) almoſt $9\frac{1}{8}$ inches. This ſpace we obſerved the aſcending water as the ice increas'd below, to invade by degrees: (for we watch'd it, and meaſur'd

ſur'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 careſully 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 botled 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 concourſe made them run down into it, or for ſome other reaſon, we determine not.]

Another.

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

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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 mettal 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 $\frac{1}{2}$; 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 reach-

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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 whose 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 *Basis* 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,

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 reſiſtence 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 ascended 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 surveying 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, whose 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 unsuccesfulness 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 Accuratenes, we omitted, partly through forgetfulness and Avocations, and sometimes for want of conveniency to try it. But by the first of

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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 poſſeſt 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 Activity of Cold.

1. THe 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, then 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 vertue, 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, whileſt he ſhall feel

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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 Predispositi-on of the Organ impose upon the unskilful or unwary. Fifthly, The very bulk of a cold Body may very much inlarge 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 fœtida*, *Castoreum*, *Camphire*, and the like; nay, and even in comparison of the sphere of Activity of the more vigo-

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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

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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

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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 thin glass, 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 contained 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 Moscovie & de Perse, 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 ſhel-

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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 my self 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 unſuccesfulneſs 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, aud 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 freez 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 imbu'd, 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 piere'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 grassels 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 guess'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 habi-

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 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. 63. 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 glass 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

Title XIV.

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

1. IN 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, least 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 Basons glaz'd both within and without, we observ'd, that the outside of both those sorts of vessels was crusted over with ice: though, however the bak'd Earth had not been compact, nor the vitrifi'd surfaces of a very close Texture; the very thickness of the vessels was so great, that it seem'd it would scarce have been able to freez at a greater distance.

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

3. And this may be somewhat confirm'd by one of the prettiest 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 triffles, 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 whilest 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 Natures 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 thoſe to whom 'tis new, and becauſe having purpoſely tri'd it in large and thick funnels of glaſs, it may be pertinently enough deliver'd in this place, (where we are treating of the Tranſmiſſion or Propagation of Cold, through cloſe and thick *Mediums*,) *then* becauſe we expect to make of it that uſe, eſpecially that Oeconomical uſe, that has been lately intimated. 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 Experiment, 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 ti'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 imploy'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 Funnels, 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

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 then 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

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 wholsome 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 then 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 then 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 then mine, by so sensible and piercing a Cold.

8. It may be doubted, whether in case

cafe water be not fluid upon the account of a congenite motion in the Corpufcles it confifts of, its fluidnefs may not proceed from the agitation of the ambient Air, either immediately contiguous to the furface, or communicating its agitation to the water, by propagation of its Impulfe through the veffel that interpofes betwixt them. To contribute to the clearing of this, and fome other things, we devis'd the following Experiment. We provided a glafs-bubble of about the bignefs of a Walnut, and the form almoft of a Pear, whofe ftem was purpofely made crooked for the conveniency of fufpenfion. This being fill'd with water (which is troublefome enough to be done, unlefs one have the knack) we hung it at one end of a thread, whofe other end we paft through a Cork, by a perforation purpofely made: into which, we afterwards faftned the thread, by thrufting in a fmall peg to rivet it in. Then filling a glafs not very broad, but yet furnifhed with a mouth wide enough to receive the bubble, with oyl of Tur-

pentine, ſuch as we bought it at the ſhops, we ſtopp'd the orifice with the newly mention'd Cork, ſo that the ſeal'd Bubble hanging at it, was covered, and every way ſurrounded by the oyl of Turpentine, which being a liquor, that (at leaſt in ſuch Colds as we here have) will not freez, we plac'd the glaſs in beaten Ice and Salt, and as it were buri'd it therein, and at the end of about three hours (having been diverted by ſome occaſions from taking it ſooner out) we found, as we had conjectured, that notwithſtanding that, the oyl of Turpentine continued perfectly fluid as before, yet the Bubble totally immerſed in this heating Chymical oyl, was frozen throughout, not excepting that which was harboured in the little Neck or Stalk, and when I came to lift it out of the liquor, the glaſs being crack'd (as we ſuppoſed by the Cold) the ſtring brought up a little part of that which was neareſt to it; the reſt in the form above mentioned, ſtaying behind and ſubſiding. And that which was remarkable in this piece of Ice, was, that when

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 freez 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 glaſs was crack'd, with lines running from a point almoſt like the Pole and Meridian in a Globe, whence we concluded the glaſs 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 glaſs 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 glaſs, well ſtopt, ſo that the bubble was every way at a good diſtance from the ſides, bottom, and top of the glaſs. 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.]

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 occurr'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. Aud 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 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, aud 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 strangness 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 then 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 ſatisfie my ſelf, 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 freez water in a hollow metalline Cylinder, and taking out the ice, and keeping it in a Perpendicular po-

ſture

ſture caſt into a ſcale weigh'd beforehand, 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 uniform, 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 woodden *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 unſuccesſully) 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 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 breadth was, I know not how, omitted in the note, but as I remember, it was about an 8. part of an Inch.*

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 ⅔ 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 freez 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

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 ſowre 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 paſt 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 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 inferiùs de bellis Hyemalibus memorandum erit.* *Olaus Ma. Gent. Septentr. Hist. Lib. 1. Cap. 14.*

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 satisfie 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

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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.

11. 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 Seamens Journals, and elsewhere. I say to *some*, because to enumerate them all, would spend more time and labour then 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

to ſcrape with glaſs, 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.

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 expreſs, 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

Purchaſ. lib. 4. cap. 13. pag. 813.

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 freez 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 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-

* *Neither hereafter will I marvel, though the strait of* Weigats *be stopped up to the Northeast, 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* Weigates, *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.

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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 ninty 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.*

Pag. 14.

Pag. 106.

But the ſtupendiouſeſt 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* (whose 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 occurr'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 Islands of Ice, some of which were above 200. foot high above water, as I prov'd by one shortly after, which I found to be* Purchas. lib. 4. cap. 18. pag. 837.

240.

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 bigness

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 indeavouring 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 æstivo 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, amplitèr vel strictè collocatis.* Olai Mag. lib. 3. cap. 2. pag. 334

I pretermit 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 may ſeem much more ſtrange, which I will therefore add, that as in the North Countries frequently, ſo ſometimes even in the warmer Regions of the Eaſt, the Sea it ſelf, has by

*— *Sæpe aliàs & his annis fatalibus tam profundè congelavit (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, Theophilo imperante, ut hyems sæva mare cogeret in glaciem ad profunditatem sanè immensam, humidúmque 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.

Bartholin. de nivis usu, cap. 6.

Glycas apud Fournier, 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, *Cærulea glacie concretæ, atque imbribus 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 portaverat, vix hominem nunc sufferre possit inermem.* Olaus lib. 1. 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 Canons.* 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

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.

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 *Blefkenius*, 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

Olaus Magnus lib. 11. & Blefkenius in Purch. lib. 3. cap. 22.

'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 it self 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 deſtroying of them by the Air and ſeveral Liquors.

1. IT may be an Experiment, as well inſtructive as new, to determine, what liquor diſſolves ice ſooner then 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, then 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 reduce

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 Crevice, that is wont to remain betwixt the two halfs 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-

without warming them, for by that way we could indeed loosen the ice, but then we could not avoid thawing it too, and that most times not uniformly: wherefore we tried by greasing the inside of the moulds to keep the ice from sticking so close to them, (notwithstanding the distention the water suffered by its being frozen) but that we might pick out the bullet entire, and this succeeding well enough, we hoped by this way to obtain our end, which was to have a competent number of pieces of ice of equal bulk, and of the same figure to be put at once to thaw in several liquors; but we could by no means procure moulds, which had any number of distinct cells of the same bigness, those long pairs of moulds that were to be met with in shops, having their distinct cells generally made on purpose of very different bignesses, which rendred them altogether useless for our design. Wherefore we were fain, for want of an exacter way, to take a glass pipe of the most even and Cylindrical that we had, and of a bore capable to admit

mit a big mans little finger, this glaſs being ſtopt 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 hindred 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 ambient Air might have time to reduce them, as far as it could, to its temper, 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 ruler divided into inches and eights, being laid alongſt it, with a knife a little warmed, the ice was ſoon, and yet not careleſly, 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 exposed 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 satisfie 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 ſpight of my diſſwaſion, 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 ſlight, 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 efficients, as the light of the Air depends upon 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 calorifick expirations from the earth, is furnisht 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 icickles, 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 enimies, 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

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, then 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 Tresle, upon which lies a kind of woodden 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, hipped 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, instead 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, whilest 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 circumſpect 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 spight 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 unexspectedly 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 durableness 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.*

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Pag. 65.

Title

Title XVII.

Considerations and Experiments touching the Primum Frigidum.

1. THe 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 expres'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 ſollicitouſly, 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 whoſe 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 remiſly: So that, till it be determin'd,

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 specifi'd.)

4. Secondly, Though it be taken for granted, not only by the *Schools*, but by their Adversaries the *Chymists*, that heat and moisture, dryness and gravity, and I know not how many other qualities, must have each of them a πρῶτον δεκτικὸν, 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 δεκτικὸν 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 δεκτικὸν 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 Palissey au Traitté du Sel commum.

7. Besides, the Earth being (according to those we reason with) the coldest, heaviest, and solidest of Elements, it is not so probable, as to ex-

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 itſelf, 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 Subterraneal fires, which breaking forth in many places of the Earth, as in *Ætna*, *Vesuvius*, *Hecla*, the *Pico of Tenariffe*, &c. seem to argue a Subterraneal 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 Subterraneal 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 Subterraneal heat or warmth, at leaſt generally to be met with: For even where there appear no manifeſt ſigns of Subterraneal 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 Subterraneal 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, because a *Virtuoso*, that had a Lead-Mine of his own, in which he wrought himself for curiosity, answered 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 respiration incommodated in such places, unless 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 *Antiperistasis*. 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

De Claves *au second Livre des pierres & pierreris, Cap. 2.*

Ibid.

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 Subterraneal 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 Mineralist affirms, that the lower they go, the more vapours, exhalations, and heat they find.

Monsieur de Claves livre 11. cap. 2.

9. But because this learned man delivers this circumstance in a dogmatical, rather then an historical way, I will add somewhat out of a relation (whence I have

* elsewhere taken other particulars) made by a French Physician likewise, that had the curiosity to descend himself into the deep Mines of *Hungary*, 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 Laborors 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 præfecto. Respondit, ex partibus inferioribus, inferius enim perpetuo calet. Quod responsum magis adhuc miratus, quæsivi anres ita sese haberet in fodinis omnibus. Respondit ita se habere in omnibus, saltem profundis, ut post profundum Terræ frigidæ 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,

* — *Percunctatus sum an quo magis accedatur ad terræ centrum, calor ille major percipereturr. Respondit, id nunquam fuisse animadversum, nisi interdum dum fodiendo occurebant venæ calidorum Mineralium.*
— *Hæc Responsa non in unicâ fodinâ, & ab unico præfecto accepi; sed, &c.*

unless

unless by accident they happen to dig through vains 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 Subterraneal Spring in the Mine its self, 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 woodden 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 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

* *Cum deſcendendo calorem illum magis ac magis augeri ſentirem: hujus rationem petii à præfecto, quod in nullâ adhuc fodina ſimilem caloris intenſionem percipiſſem. Reſpondit, Mineram Vitrioli paulo inferius exiſtere, quæ calorem multiplicaret.*

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 concurrance 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 *Ætna* and *Vesuvius*. And if it should be objected, that this Subterraneal 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 Subterraneal 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 extrinsick 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 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

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.* Gerat de Veer in Purchas, pag. 474.

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 φαινόμενον 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 freest 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 freez 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 Countreymen (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.*

Purchas. lib. 3. cap. 15. pag. 579.

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 shipwrackt 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

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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 Chymico, lib. 2. cap. 1.

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

Josephus Acosta lib. 2. cap. 11.

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*

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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 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, (insomuch 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 freez 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 freez at the Top, begin to freez at the Bottom, or at any ſide I pleaſe, and that much ſooner then 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 then once by the

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External application of other things, been able to condense it much farther, which argues, that 'tis not the Air as such, but some adventitious frigorifick 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 freez 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 halitu-ous 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 Sea-water, 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, 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

Gassendi Phy. Lib. 6. Sect. 1. pag. 399. *De qualitatibus rerum—— ac addi quidem fortassis. potest, præcipua 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 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 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 freez, 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 freez 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

Ib. pag. 400. *Quomodo possunt corpuscula Nitri in aquam infusi illam præter modum adeo frigidam reddere imò, & per æstatem etiam congelare, dum nitrum nivi glaceive detritæ commistum lagenæ circumponitur, ipsaque præter corpus Lagenæ penetrant in aquam contentam.*

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 inflamable part of Wine, the Experiment will ſucceed; and yet I think *Gaſſendus* would not have the Corpuſcles of theſe Bodies to be frigorifick, 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 efficients 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 grossie 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 ſatisfie 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 concern-

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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 then I am sure of, and that this Salt alone should be the *summum frigidum*, is more then 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

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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 efficients 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 Tetrahedrical or Pyramidal, whereas the Crystals, or Grains, great or small, into which good Salt-petre shoots,

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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 advanc'd (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 expirations, 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

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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.

1. 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 frigorifick 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,

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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 acknowledg'd 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 frigorifick, yet I ſee not why we ſhould have recourſe to them in caſes where ſuch a bare ceſſation, 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, Saltpetre, 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 Bullet 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

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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 Diſſilition, remaining much leſs agitated then 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 ſuſceptable 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 *Cabæus* 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 hindred) the recited Experiment will not afford much more then good hints towards the Discovery of the Temperature of the Air.

5. I have * ellewhere taken notice, that air included in Vessels sufficiently

* *In the third Preliminary Discourse.*

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 freez 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 ſubjoyn, nor has the meaſuring of it been, that I know of, attempted by any man. Wherefore we thought fit to indeavour 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 Bolthead 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 freez 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 natural

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 endeavoured 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 rise by our Estimate near four inches more (then those five wch 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 happned 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 Victuals.*

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 voyages are extant in Purchase *lib.1.cap. 13. and this passage 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, least 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 freez 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 setled, 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 Firmanent, then ever I had before seen by two thirds. I could see the Cloud in* Cancer *full of small Stars.*

Pag. 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.* 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*
Pag.46. *took the Latitude with two Quadrants, and found it to be just* 52. *degrees*, with-
Pag.64. out any minutes. Elsewhere; *my observations* (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.

15. *This evening* (says he) *the moon rose in a very long oval alongst the Horizon.* Pag. 66.

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 Refractious 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 aforesaid) 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

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, then this of Cold.

* *A Sceptical Disquisition of Antiperistasis.*

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 occurr'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 discoveries northward, by Mr. *Poole*, in the year 1610. I find this passage, *I was 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.* Pag. 702.

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 assuredly found, that the only and most 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* Tartarian *Sea about* Nova Zembla, *caused* Pag. 473, 474.

us

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 Zemola *lyeth* 4, 5, *and* 6. *degrees more Southerly from the Pole, then the other land aforesaid.*

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 my self, 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,*

Acosta lib. 2. *cap.* 9. *pag.* 101.

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* Pag. 109.

when

when they have the Sun for Zenith.

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*

Purchase pag. 578.

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 assignes 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 recenter 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 adjoyning Regions, has this memorable

passage to our present purpose. *The whole Countrey* (says he) *differeth very much from it self, by reason of the year,*
Pag. 414. *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 greater 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*
Purchase pag. 415. *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*

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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 bignes, which bears witness to that almost incredible Relation of *Olearius*, who (after having much prais'd their goodness at *Musco*) affirms, that he there met with Melons of 40. pound weight, of which he there teaches the Culture.

Voyage de Moscovie & de Perse, livre 3. p. m. 117, 118, 119.

Pag. 119.

At the royal City of *China*, which scarce exceeding the 42. de-

Pequin.

grees 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 *Martinius*, 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 Ægyptiorum*, tells us, that at *Grand Cayro*, where he practis'd Physick, though that famous *Metropolis* of *Ægypt* 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

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, Butchers-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.*

Pag. 81.

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 *Ægypt* its self, where one would

would expect a much more uniform heat. *Hyeme* (says he) *nocturnus aer admodum frigidus observatur, qui ob orto sole paulo post, parum incalescit, in meridieque plurimum: adveniente vere nocte 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.* Pag. 9.

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 benummed 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 then 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 wintred 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 freez 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, then 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 setled, 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 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 recoyl hither, before it had lost the greatest

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*.

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 spight of the violent heats, that would otherwise be intollerable. (And many in *Egypt* * ascribe to the *Ætesian* Winds, that almost miraculous ceasing of the Plague at *Grand Cairo*, of which we elsewhere speak.) *Dominatur autem aer* (says he) *summè calidus, ipsius cæli, ut dictum est, ratione, quod hæc civitas à Tropico Cancri tantum 6. gradibus distet. Quâ brevi inter-capedine dum sol ad illum accedit Tropicum, & illorum Zenith fit propinquior, aer ille valdè incalescit, & nisi Ætesiæ venti tunc à septentrione spirarent, vehementissimus, & qui vix à nostris perferri possit, caloris æstus sentiretur.* Ibid. lib. 1. cap. 6.

Advenæ nostri iis provenientibus ad subterranea loca confugiunt, in quibus morantur Ibid. lib. 1. cap. 7. pag. 11.

* *Ab his ventis aerem alteratum, esse causam cur pestis illa dissolvatur, multi illorum Affirmant. Quod etiam non videtur penitus à veritate alienũ, quando id multis etiam rationibus nobis persuaderi possit, in primisque, &c.* Prosp. Alpin. lib. 1. *De medicina Ægypt.* cap. 18.

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 inflammatis atque laxatis corporibus solatium præstant. Si enim illi diu perseveraverint, nemo in eâ regione vivere possit.

Whence winds should have this power to change the Constitution 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* says, 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 pressing the mettal 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* (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, he affirms, that this wind yet in this region

Lib. 3. cap. 2. p. 120.

Lib. 3. cap. 3.

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 become of that queſtion, I thought I might expect, that whether or no Ice emit Corpuſcles, that are univerſally frigorifick, yet the air being, either 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 leſs cold body, which they ſhall happen to be blown upon, and accordingly, having provided a ridge Tyle inverted, 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 conveniently 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 frigefactive 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 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 Sun beams, 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 ſuſpition, 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* England (says he) *most of the cold winds and weathers come from the Sea; and those situations, are counted most unwholsome, 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 weather, 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 hither, have been very variable and unconstant; 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.*

Part 1. cap. 2.

Captain James's voyage, pag. 52, 53.

Title XIX.

Of the strange Effects of Cold.

1. TO 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 uselefs *R*epetitions, 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 *R*elations 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.

3. Thus far our Enlish 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.

Purchase lib. 1. cap. 4. pag. 74.

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 frigorifick 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 expirations 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 pretermit 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 freez 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 sollid 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 mettal, 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) *It froze two inches thick in the salt water.*

Purchase pag.491.

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 childs 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-

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.*

Purchas. pag. 491.

11. 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 freez 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 Glaſs, Earth, and even Pewter, and Iron themſelves, to which *Bartholinus* out of *Janus Muncks* Voyage to *Greenland*, allows us to add veſſels of Braſs (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 freez 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 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.* Livre 3. 116.

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* Purchas. lib. 3. Pag. 415.

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 *Phænomena* 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 soals 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*

Purchas. pag. 497.

there arose Blisters upon their Faces and Ears. We did dayly find by experience (says Captain *James*) *that the cold in the woods would freez our faces, or any part of our flesh, that was bare; but it was not so mortifying*, &c. Pag. 64.

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, *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.* Pag. 497.

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 speaks, *become intolerable.* Pag. 56.

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 Cabbin 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 Cabbin, for all which warmth his plaister would freez at his wound, and his bottle of Sack at his head.*

Purchas. lib. 3. cap. 5. 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 *Ukranie*, 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 power-

full

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 Waſt (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 lifes, 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 ſatisfie 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 *Boback*, 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

be frozen till they are ſtiff, and yet recover, I ſhall (ere long) have occaſion to obſerve at the cloſe of the 21. Section. And therefore I ſhall now add but this, That whereas 'tis a Tradition among Travellers into Northern Climates, that both Birds and wild Beaſts are in icy and ſnowy Countries ordinarily turn'd white, if not at all times, yet at leaſt in the Winter by the coldneſs of thoſe gelid Climates, I dare neither admit the poſition as a thing that is true univerſally, nor reject it as a thing that is never ſo. For not now to enquire, whether whiteneſs proceeds from the coldneſs of the Countrey, or from ſome ſetled ſeminary impreſſion, or from the imagination of the females affected by the vivid whiteneſs of the ſnow, that almoſt all the year long is the conſtant object of their ſight: I find by the Voyages I have perus'd, that Navigators often mention their meeting with ſtore of white Bears and Foxes in *Nova Zembla*, and other very Northern Regions, as alſo their meeting ſometimes with herds of white Deer: And in the *Alps*, al-

ways

ways covered with snow, good Authors mention their having met with white Partridges; to which purpose I remember, that when I was in *Savoy*, and the neighbouring Countries, which have mountains almost perpetually cap'd with snow, I heard them often talk of a certain white kind of Pheasants to be met with in the upper parts of the mountains, which for the excellency of their taste were accounted very great delicacies. But on the other side, the same Navigators treating even of the coldest Climates, seem to distinguish the white Bears from others of those parts. * And as for a herd of white Deer, their colour may proceed from seminal impressions, since here in *England* I have seen several Deer of that colour, and though *Greenland* be by some degrees nearer to the Pole then *Nova Zembla*, yet I have seen a live Deer brought thence somewhat differingly shap'd from ours, whose skin was not white, but rather a kind of dun: And to add

* *And 'tis from very Northern Countries, that we usually receive very dark colour'd Furs, and the skins as well of black Foxes as of white ones.*

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 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 then I intended

Lepores coloris & pellis mutatione anni tempestates sequuntur, ac hiberno tempore albis pilis vestiti, æstivis mensibus eosdem cinereos habent. Livoniæ nova discriptio, Pag. 303.

Pag. 46. *&* *Pag.* 89.

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.

Title XX.

Experiments touching the weight of Bodies frozen and unfrozen.

1. SInce 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 frigorifick 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 increase of weight, when frozen, would betray any substantial accession of the Corpuscles of Cold, which according to the *Epicurean*

rean Principles, may, by reason of their smallness, pass in freely, and in vast multitudes, at the pores of other Bodies, and even of glass, 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 ballance) 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 registring 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 ballance 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 ſatisfie 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 foreſaw

ſaw, 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 counterpoiſe.]

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 Liquor, 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 it ſelf: 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 ℥v. ʒ8. gr. ß, 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 ½ 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 ℥iiij. ʒij. 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 efficients 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 $\frac{1}{8}$ 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 suspition, 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 ingenions 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 increaſe 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 poliſht; 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 into 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 thoſe, 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

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 satisfie ourselves, 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 steams 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 famousest 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 propemodum 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 then 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 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

— *Hinc gelidam congelatamque aquam graviorem eſſe non congelata expertus eſt Jo. Manelphus, Com. in 4. Meteor. Ariſtot. Inquit* Tho. Bartholinus *de Nivis uſu cap.* 12.

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, according

ing to her wonted method in open vials. And if insteed 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

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 ℥v. ʒij. gr. 11. whereof the veſſel weigh'd ℥ij. ʒvß. 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 ℥, and having exposed 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.

1. 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 occurr'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 then that, wherein they shall happen to occur to me, throw them into this one Section, together with some loose Experiments, and divers

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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 *Phænomenon*, 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 manifesty 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 distitute 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 advertiſment may be juſtifi'd by an Experiment that I am about to annex. For being pretty well confirmed 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 *Effluviums*, 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 freez 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 affirmed 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 mettalline Concave, I reſolv'd to try, whether thoſe beams were not only devoid of cold, but alſo ſomewhat warmiſh, ſince they are the Sun-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 ſucceſsful in their Trials.

7. On this occaſion I ſhall add, that meeting the other day in a Book-ſellers 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, whileſt 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

To the XI. *Tale.*

BY the unſucceſulneſs 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 imploy'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 conſi-
derable

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 cleſts, though it were able to force it ſelf a way out at the ſcrew, in ſpight 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 reſiſtence or compreſſion, which the freezing water found upon its endeavouring to expand it ſelf in the barrel.

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,

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 satisfie my self, 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 tincted 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 *Cabæus* 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 supremeness of Cold in Salt-petre, to retain my former opinion about it, till more successful 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 informed 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 it ſelf 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 tinctedſpirit, 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 orher 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 benummed, 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 tyles, 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 throughly 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 houſes, 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, wherein he us'd to keep his glaſs in fuſion, would in the fire ſcale or crack (and perhaps fly) and become unſervice-

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 Author, 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.*

* *Of the imperfection of Physicks. Maistre Bernard Palissy.*

But it may yet seem far more unlikely,

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 settled 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 faciliùs infringitur dum aliæ secures chalybe* Lib. 1. pag. mihi 23.

permixtæ, in vehementi frigore ad solum glaciei vel virentis arboris ictum instar vitri rumpuntur, ubi ligones prædicti sive ferreæ 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 mettal may by such degrees of cold, as even our Climate is capable of, be rendered 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 mettal another very skilful workman, whom I also consulted, certifi'd the like: but though this disagreed not

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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 freez then cold; but I do not much wonder, that the learned *Calæus*, 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 ſatisfie my ſelf, that there is (at leaſt

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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 then the hot.

These pottingers were earthen, but I elsewhere made the Trial in others of mettal, 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 jealousie 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 confeſs'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, mighr 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

with the rest at $\frac{3}{4}$ after 6. began to freez somewhat before $\frac{1}{2}$ after 7.

The water heated and cool'd again, began to freez $\frac{3}{4}$ after 7. And having these frozen waters a pretty while by me, I sent in for my own further satisfaction, for the hot water, and found it not to be, in the least, frozen at half a quarter after 8. So that supposing it to continue half a quarter of an hour longer before the beginning of its congelation, * it was twice as long ere it began to freez, as the cold water had been.

* *As it afterwards did at the least.*

By which we may see how well bestow'd their labour has been, that have puzled themselves and others, to give the reason of a *Phænomenon*, which perhaps with half the pains they might have found to be but *Chymærical*.

I have been the more circumstantial in setting down these Trials, that I may express a civility to so famous a Philosopher as *Aristotle*, and also because Artificial Congelations, which we can commonly best command, and which we have the oftenest us'd about our other Experiments, are

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 *Phenomenon* 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 acuminated, where it had been ſeal'd up then the other, it ſeem'd probable, and afterwards appear'd true, that the water in this acuminated 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 indeavour to justifie *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 disfavour 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 ordinarily 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 temper, were expos'd to a very sharp Air, and there watch'd by the person, whom (being not well, and unable to support such weather my self) I appointed to attend the Experiment, 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 over-seen in affirming, that heated and refri-

gerated

gerated water, will cool *so much sooner*, as he would perswade us, then other. * And 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* Corpuscles, no less necessary to conglaciate water, then Runnet to curdle Milk: And for what this Author 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

* *Quare ferventem aquam adhibuisse oportet qui asserit eam esse minus gelabilem, præcipuè salsam.* Pag. 571.

* *Tam citò illa congelabat, ut eximerem ex eo crustam unam aut alteram antequam non calefacta vel levissime concrevisset.* Pag. 572.

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 unskilfully 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*,

 which

which I might here examine, my haſt and my indiſpoſedneſs to ingage in a controverſie of ſmall moment, did not injoyn me to defer it till a fitter occaſion.

Here the Poſtſcript ends.

To confirm the power aſcrib'd in the VI. Section to cold, as to the long preſervation of bodies from corruption, 'twill not be amiſs to add theſe two remarkable paſſages, the latter of which affords a good inſtance of the improvement, that may be made of ſome degrees of cold to the uſes of humane life.

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 viſited, *Their Dead they bury on the ſide of the hills, where they live (which is commonly 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

Purchas lib.4.cap. 19.pag. 844.

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 them, they pluck out the bones, 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.* Lib. 3. cap. 22.

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 Natures abhorrencie 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 ſomewhat 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 glass-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 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 respiracula susciperent, quotquot in flumine vel stagno versantur, subito morerentur.* Another passage of the same Author, and taken likewise out of the same

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 constituunt, tum per ea, &c. Olai Mag. lib. 20.

Olaus Mag. Titulo, De cursu glaciali, pro piscibus.

Quæ (Anguillæ) si totaliter glaciæ constrictæ fuerint simul omnes respiraculum ab aere non habentes pariter suffocatæ moriuntur.

(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 advertising the Reader, that Ponds and Lakes did generally begin to freez in * *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

* *Præmittendum est quod generaliter omnes lacus, & stagnales Aquæ in mense Octobri incipiunt congelari, glacieſque auctio 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, diligentiâ piscatorum continue glacies ipsa perfringitur ne congeletur.* Olai Magni lib. 1. Titulo de transitu glaciali, &c.

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 ſubterraneal 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 ſollicitous 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 ſatisfie my ſelf, whether the ices

ices denying access to the Air, was that which destroy'd Fishes in frozen Ponds, I thought upon this Epedient, 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 ſatisfie my ſelf about ſome *Phænomena* I had a mind to obſerve, I have often by watch-

watching them, ſeen them lift up their mouthes above the ſurface of the water, and ſeem to gape and take in Air, and afterwards let go under water out of their mouthes 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 ſhow'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 mouthes, 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 ſatisfie 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

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 then 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-

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*
Pag. 82. *ninth* (ſays he) *we hoiſted out our Beer and Cydar, and made a Raft of it, faſtening 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 heav'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 it were then the moneth of May. Pag. 47.

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 observation 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 throughly 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,

ſies, 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, whilest 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 ſetled (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 monerhs 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,

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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 tincted 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 itself, 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 fro-

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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 ſatisfie 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 ingag'd, hoping, upon conſiderations, which 'twere too long to enumerate, that, if ſuch attempts ſhould ſucceed, they might afford hints of a *Lucifer-ous* 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 throughly frozen. Now one of the chief things, that I propos'd to my ſelf 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 groundlesly imagin'd, that in divers Succulent bodies, both vegetable and animal, the sap or the juice, that was so dispers'd among the other parts, and divided into such minute portions, 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 congeal'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 appear 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 bodies, 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 magnifi'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-

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 pretermit, 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,

Of the uſefulneſs of Experimental Philoſophy.

puſcles (as when one cuts a frozen Apple) the ſubſtance of the brain ſeem'd alſo to the eye to be ſtuffed with them, and the Ventricles of it did at leaſt conſpicuouſly *harbour* pieces of ice, if it were not *fill'd* up with them; and the manifeſt difference of Texture, that there is between the white and yelk of a througly frozen Egg; and alſo betwixt the Cryſtalline and the Aqueous, and the Vitreous humors of the eye, wherein by congelation the Cryſtalline alone loſes its tranſparency, but acquires no conſpicuous ice, whileſt the others are full of ice, and that diaphanous, theſe and ſuch like diſparities, I ſay, may invite one to hope, that ſome things may by congealing of bodies, be diſcovered about their Texture, that may afford ſagacious *Anatomiſts* 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 fleſh, nor a live Fiſh, nor a living Frog, being frozen and put into cold water, was obſerv'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 *Phænomenon*. For though the immersion of frozen bodies in cold water be allowed to thaw them, with less prejudice, then 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 throughly 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 Alimental juice is turn'd by the froſt, being each of them expanded proportionably to their reſpective bigneſſes, may not only prejudice the whole, by having their own conſtitution impair'd, as has been formerly obſerv'd in Aligant, and other Vinous liquors, but may upon their expanſion cruſh in ſome places, and diſtend in others, the more ſtable parts, in whoſe Cavities they were harbour'd, and thereby ſo vitiate their Texture, as to impair ſome of their qualities, and diſpoſe the *Compoſitum* to corruption. How much Contuſion may prejudice tender bodies, and accelerate putrefaction, 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 innumerable 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 indeavour 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 quali-

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 taken 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 miſchieſed 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 former obſervations have made it probable, 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 bitter 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 exhibite in the ice, the ſhape of the ſame Plant: And the learned, but I fear, too credulous, *Gaſſarel* tells us, that this is no Rarity, being dayly 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 Unſucceſſulneſs of Experiments.*

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 expresly informs us) no way in the shape of Trees, by whose Incineration nevertheless *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 Brassicæ aqua congelata Brassicam repræsentat, spiritibus 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 confess, somewhat surpriz'd to meet in *Berigardus*'s forecited discourse upon *Aristotles* Meteors, such a passage as this; *Paucis notum est, cur intra glaciem cernuntur interdum multiformes stirpium imagines in Ampullis vitreis, aquæ superficie tenus congelatæ plenis. Hoc autem fit injecto in Phialas sale diversarum stirpium, nam ubi erit sal alicujus plantæ & Artemisiæ, in suo Lixivio glacies adhærens vitro, refert ejus folia laciniosa: similiter in alia Phiala videbuntur folia Plantæ, cujus sal in suam aquam fuerit injectus. Et nè quis hoc fortuito cadere putet, in aquâ sæpius solutâ, & congelatâ eædem 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 *expresly* of his having done so, those many ingenious men, that much distrust the relations of those Chymists, that are not of the

Pag. 573.

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 *Spagyrical* 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 noted st 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 Experientiâ, 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 *Phænomena* of the Experiment, ſuch as he ſeems not to have been acquainted

 with,

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.)

* In the unpublished Section of the usefulness of Experimental Philosophy.

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

Of the unsuccesfulness of Experiments.

given their own determinate figures, have, when diſſolv'd in water, concurred 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 paſſed 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 *Idæas*'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 confirmation whereof I will add on this occasion, 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 imployed 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 pyramid 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 considering 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 reſolved 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 Egg-shells 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 hindred 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 my self, 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 traversed) * but besides that I have delivered a great part of it already 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

**Where a wonderfully piercing, though not sensibly violent cold, does sometimes suddenly kill men, and yet preserve their Bodies untainted whole years together.*

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 witness) 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 expresly have taken notice of this circumstance, that, to satisfie my self, 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 elsewhere 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 tincted liquor manifestly enough to ascend, as if the

the Wind, consisting of a more compress'd 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, scap'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 *Effluviums*

viums 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 frigorifick, upon whoſe account ſome winds are ſo much colder then 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*,

dies, he aſſur'd me, that notwithſtanding the violent heats of the day, he uſually obſerved the nights to be ſo very cold, that he was perſwaded ſome poſitively frigoriſick ſteams, did in the night aſcend out of the Earth, and make it very expedient, if not neceſſary, for thoſe Engliſh, that live in the warmer parts of *America*, to imitate the Natives, in keeping fires under their Hammacks, or hanging Beds.

I thought it might be a *Luciferous* Experiment, in relation to an *Hypotheſis*, that might be propos'd about cold, to try, whether, if two ſuch liquors were provided, as by being mix'd together, would ſo far forth loſe their fluidity, as to obtain at leaſt the conſiſtence of an Unguent, this impediment put to the former confuſed and greater agitation of their parts, would produce any ſenſible degree of cold; this I thought fit to try, by immerſing for a competent time, the Ball of a tender ſeal'd Weather-glaſs, into each of the liquors apart, and then into the ſoft mixture, their coalition would com-

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

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 dicision 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 of 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 aſcended 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 ¾ 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 somotimes did by ice and salt, applied in a perforated Box to the outside) lest 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 $\frac{1}{4}$ 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 latitant 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 glass, 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 $\frac{1}{3}$ 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.

THat 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 Authors'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

AN
Examen of *Antiperistasis*,
AS
It is wont to be *Taught* and *Prov'd*.

Themistius, Carneades, Eleutherius.

Themistius.

1. AS 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 retiring 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 increased 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 midle 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 refresh-

ment,

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, whileſt 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 manifeſt-

ly,

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 franckly confess to you, that when I consider what interest, the unheeded dipositions 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 imploy'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 under ground, 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* it ſelf, 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 ac-

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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 careſully 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, aud consequently against which they cannot maintain a Paradox, that does not imply a Retractation. 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 *Themiſtius* 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 *Themiſtius* 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 creatures, 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 prevail'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 matter, that happens to be ſurrounded with bodies, wherein a contrary quality is predominant, were not endowed with an incomprehenſible faculty of ſelf invigoration. And Nature either does not need the help of this imaginary power, or oftentimes has recourſe unto it to very little purpoſe; ſince we ſee, that theſe Qualities 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 regularly warmed by the Ambient Air. And in *Muſcovy* and other cold Northern 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, Ænea 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 Incalescence 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 threatned 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 incaleſcencies, 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

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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 foreſaw 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.

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 Ancienter 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 midle of the snow a Vial full of water; this

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 ſtomacks in Winter. The generation of Hail; and the heat and cold in Cellars, and other Subterraneal 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 Sun-beams 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 assaul'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, intensly 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 stomacks 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 ſtomack is made by heat, and conſequently, that the ſtronger digeſtion, that is wont to be made in Winter, is an argument of the ſtomacks 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 ſtomacks of divers ravenous fiſhes, whoſe ſtomacks 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

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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 sollicite the stomack, 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 *Bulimus*. And the learned *Fromundus* relating, how he himself by walking long on the snow, was surpriz'd with such a Βολιμία, takes notice, that the chief cause of the fainting was in the stomack: 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 stomack, 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 stomack 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 stomack 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 my self 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 preter mit 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 *Peluſium*, 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 subterraneal 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 subterraneal 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 subterraneal 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 adjoyning 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 subterraneal 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 then at other times, and the subterraneal 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, that the Air of Cellars and Vaults were really colder in Summer then 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, then 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 subterraneal 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 subterraneal 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 ſubterraneal 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

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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 subterraneal 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 seald 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, whilest 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 efficients of cold, as the free and external air. And now as to the second part of what I was saying, that the subterraneal 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 knub 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. Inſomuch 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 Kitchin, 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 sowr 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 groundlesly 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 my ſelf to repeat, what I have formerly ſaid, to ſhew the incongruity of *Antiperiſtaſis* in general to Natures 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 ſubterraneal air, though above, it have the cold that reigns in Winter; yet beneath, the ſubterraneal 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 those 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 frigidiorem 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 exchang'd 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 vigebat calidissima, siccissimaque.* p. m. 130.

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 swayd 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

Pag. 142, & 143.

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 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 hundred foot of a cold region of the Earth, whereby it may well be supposed, to have been much infrigidated. To these relations of the learned *Morinus*, I will add, that the

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Exhalatio aere levior per ipsum puteum ascendit magno impetu, in ejus sumitate adhuc sensibiliter calida ipsa æstate, licet supremam terræ regionem tunc frigidissimam permeat. Pag. 128. see also pag. 125.

Archbishop of *Upsal* affirms, that in the year 1528. being in *Poland*, he went to visit those deep mountains (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.

In Polonia vero montes profundissimi salis sunt, præsertim in Vieliscad & Bochna ubi videndi causâ transcensis scalis, 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.

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 ſubterraneal 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 ſubterraneal cavities, where conſequently they produce ſuch a heat, as

to those that come out of the cold air, may be very sensible. And the rather, because whilest men, by the coldness of the season, are more then ordinarily careful, to stop up the passages, at which the external air may get in, they do, though designlesly, stop up the vents, at which the subterraneous exhalations might get out. And to shew you, that this last circumstance is not impertinently taken notice of, I shall tell you, that a very grave Author having occasion to mention Cellars, relates it, as a practise in divers houses of a Town, where he had been, to keep vents in their deep Cellars, which in the Summer. were from time to time opened, partly to keep the places sweet and wholsom, and partly to let out the warm Exhalations, that would else hinder their liquors from keeping so fresh, and well. And these steams were affirm'd to have been several times taken notice of to ascend visibly into the free air like a smoak, which several *Phænomena*, and particularly what I formerly related of the hot fumes, that manifest-

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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

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 expreſly, 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 ſubterraneal cold, if *Antiperiſtaſis* could do it, there is no talk of any ſuch degree of cold in Summer, as to freez 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,

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ſpeaking

ſpeaking of the ſame month of *December*, he expreſly ſays, that their Well was then frozen up, ſo, *that dig 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 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 uſeleſneſs 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

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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 Magnus* alſo relates, that in another part of the King of *Denmarks* Dominions, 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

Hanque naturam, lacum ſimilem, prope Metropolin Nidroſienſem Regni Norvegiæ, habere compertum eſt, eo præcipue Argumento, quod in mediis frigoribus nunquam congelatur. Lib. 2.

Joſeph. Acoſt. Hiſt. Ind. pag. 174.

though at first sight they may seem to disfavour 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 thickned, 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 Waſtcoat, which, one of them, whileſt 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 *Phænomenon*.

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 then in Summer; yet a sufficient reason of the *Phænomenon* may be fetch'd from what I have already deli-

delivered about the detention of the warm ſubterraneal 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 ſubterraneal 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

Sea, and at other vents, get up into the Air.

47. To make out this, my form-
P.m.136. erly quoted French Author relates a very memorable thing, that was told him by the Masters of those 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 then 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 conld 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,

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 Maritine 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*, whose 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 account 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 exspirations, 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,

Quia verò in Comitatum Zoliensem, dum aquas persequimur, ventum est, non possum præterire hiatum terræ iisdem in locis famosum ob pestilentes expirationes, quibus Aves supervolantes, & quævis alia animantia extingui constat, manifesto eorum experimento, qui, &c. Pag. 74.

would much countenance what I have been lately saying: but though I pretermit 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 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

Ibidem est sub dio fons calidarum cæteris amplior, quem Purgatorium vocavere, ea nimirum ratione, quod, quamadmodum proditum est in purgatorio pœnas nocentium pro noxarum modo, alias acerbiores alias mitiores, ita quædam insunt Aquæ hoc in fonte discrimina, namquâ in eum à Danubii ripâ aditus est, subfrigida primum, mox tepida, & quo in eum penetrari 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.

there boil up hot Srings, 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 removing 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* then the sulphureous *Grotta* near *Naples*) whence not only subterraneal 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

Neque in ripâ tantum eruuntur Calidæ, sed etiam intra amnem, si fundum ejus pedibus suffodias. Calet autem immodicè, nec sunt Idoneæ balneis, nisi temperentur, quod Admistione frigidæ de proximo haustæ in proclivi est. Pag. 65.

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 Draggons. [And the *Russian* Emperors Physician, 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 *Olaus Magnus* be true, concerning the ſtrange thaws that ſometimes happen, with terrible noiſes, in the great Lake *Veter*, thoſe wonderful *Phænomena*, may not improbably be aſcrib'd to the aſcent of great ſtore of hot ſubterraneal 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 ſuſpicion

Nec præterendum hic puto Lacum eſſe LX. *milliarum in longitudine, & XX. in latitudine Italicorum*, Veter *appellatum in Regno Oſtrogothorum, quæ talis eſt naturæ, 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.

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 ſubterraneal 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 inrich 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 subterraneal 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 ſally 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.

51. 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 expreſly to overthrow it.

52. I might urge against those, who, though they begin to be asham'd 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 needlesly 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 Abetters 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 upon

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 no where touch either the bottom or the

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 tincted 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 careſully 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 tincted 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 tincted 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 tincted 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 notwitstanding 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 besieg'd by water exceeding cold; for the languid motion of the tincted 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 chillingness of the contiguous water quickly check'd, and the formerly ascending spirit was soon brought to subside again. And to give my self the fuller satisfaction about some of the chief *Phænomena* of this, and the former 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 cerain inexplicable ſelf invigoration, which is commonly propos'd in ſuch a way as inveſts inanimate bodies with the prerogatives of free Agents. But though *Carneades* 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 themſelves ſeem not yet ſo clearly determined as I could wiſh: for as to the obvious *Phænomena*, that nature does, as it were, of Her own accord preſent us, they ſeem to have been but perſunctorily 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 Experiments, there ſcarce ſeem to have been any made fit to clear the difficulties, that invite me to ſuſpend my judgement as to the grand Queſtion

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(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 those 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 subterraneal 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 *Zucchius*, who is wont to be far more industrious then 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 subterraneal places is indeed greater in Winter then 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 *Zucchius*, and the other Jesuite, do I confess restrain me for a while from yielding a full assent to what

Carneades hath delivered, as to the matter of ſubterraneal 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 ſubterraneal places are hotter in Winter then 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 ſubterraneal 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 ſubterraneal Places, the deepeſt of all do not, as far as the Authority of Mineraliſts above alledg'd

alledg'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 ſubterraneal places, though by reaſon of their being fenc'd from the outward Air, they are not ſo ſubject to the alterationsof 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

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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 subterraneal Vaults are shallower, the Air is observ'd to be colder in Winter then in Summer, though at *Rome* in their deep Cellars the contrary has been found. So that the lower-most sort of subterraneal cavities being, for ought appears, perpetually hot, and the upper or shallower sort of them, being colder, not hotter in cold weather then '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 easie 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 subterraneal Air, and the greater pressure, which the Air may there have, by reason of its pressing, according to an Atmospherical 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 Atmospherical 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 *Zucchius* 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 subterraneal

Air

Air was colder in the former ſeaſon, then in the latter, but becauſe the Atmoſphere chanc'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 *Zucchius* 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 ſubterraneal Exhalations have a peculiar, and not languid influence; as *Carneades* has towards the cloſe of his Diſcourſe made probable, out of the Relations of

See the ſecond Preliminary diſcourſe, that accompanies the Hiſtory of Cold.

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 *Zucchius* 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 ascribe, as he does, a *fuga Contrarii* to

My backwardness to admit a fuga Contrarii, *may 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 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 præcedente 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.

 æstate

æstate 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 ſpecioſly 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 *Zucchius*, 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.

AN

ADVERTISEMENT.

THe Author of the following Discourse intending it should make a part of certain Considerations upon the four famousest 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, then 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 inform-*

ed, upon what Acccount Mr. Hobs's *Opinions come to be examined in a Historical Treatise; and may not wonder, either to find, that divers passages of It are omitted, that are unfavourable enough to Mr.* Hobs's *Doctrine, or to meet with in a Discourse postpon'd to the* History of Cold, *some Experiments, that seem to argue it to have been written before they were cast into the Order, wherein they now appear. To this I have nothing to add, but that whereas through haste the Scheme referred 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 is 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 explicate their causes rightly or not) in the Production of lesser degrees of Cold, but, (how improbably soever) of congelation*

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it ſelf, ſhall think the ſight of that Scheme of any Importance: this Learned Mans Book De Corpore, *is in ſo many hands, that any Reader that ſhall deſire it, may very eaſily have an opportunity to conſult the Scheme in the particularly cited place.*

AN

AN EXAMEN OF Mr. *Hobs*'s Doctrine, touching *Cold*.

1. 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 sollicitous Examination, but that in proposing it, he scruples not to talk to his Readers of his Demonstrations, and the preferrence, 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 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 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 *Æquator* 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 *Artic. 6.*

ſay, as the Circles which are parallel to the *Æquator* 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, may be 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 diſcendding 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 remiſs 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, at 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. *Hobs*'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. *Hobs* 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 introdnc'd, not only into water, but into Wine, and divers other partly Aqueous liquors, there is a plain intumescence, and consequently indeavour 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 thoſe 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

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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 otherwise Hot. The noble * *Avicen* also 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 very cold. But that will perhaps seem more remarkable, which I shall further add, namely, that I know a Nobleman, who follow'd the Wars in several Countries, and has signaliz'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 assures me, that the stones, that torment him, and which he usually voids, are but very small; yet whilest the fit continues, which oftentimes

* *Quoted by* Paul. Neworantz. De Purpurâ, *Cap.* 12.

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 indeavour to retire *inwards*, that seems but an inconsiderate direction. For in *compressions*, that are made by *surrounding bodies*, there is produc'd an indeavour inward of the parts of the compreſt 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 taken notice of, being chiefly design'd to shew, that the *notion of cold in general* 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 accurately and warily propos'd by Mr. *Hobs*: I shall not any further prosecute that discourse, but proceed to what remains. Next then, the Cause he assigns, why a man can blow hot or cold with the same breath, is very questionable; partly because he supposes in part of the breath such a *simple motion*, as he calls it, of the small particles of the same breath, as he will not easily Prove, and as *eminent Astronomers and Mathematicians have Rejected; and partly because that without the suspected supposition, I could (by putting together the Conjectures of two learned Writers, and what I have elsewhere added of my own) give a more probable account of the *Phænomenon*, if I had not some scruples about the matter of Fact it self: which last clause I add,

* Doctor S. Ward (now the worthy Bishop of Exeter) and Dr. J. Wallis (the learned Savilian Professor of Geometry.)

because, though I am not sure, that further Trials may not satisfie 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 jealousie 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 encourage-

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 subjoyns, 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

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) empti-

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 freez, 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 ſtopt, 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 perfectedly 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 ſurround-

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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 then 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 flammas, nec non pulveribus obscurissimas nubes eo asportasse videatur. And elsewhere, — *Prima æstatis parte calidissimâ inæqualissimaque ob vehementissimum Meridionalium Ventorum calorem,* &c. Prosper Alpinus *de Medicina Ægyptiorum.*

From 9. till noon, there blows a wind with such extreme heat from the sands, that it swallows up a mans breath, and stifleth him. — The King of Chermain *sent an Army of sixteen hundred horse, and five thousand foot, against the Lord of* Ormus, *for not paying his Tribute, which were all surprized and stifled with that wind.* Marcus Polus in Purchas's Pilgrims, lib. III. p. m. 71.

learned *Alpinus*, have complain'd, that the winds coming to them in the Summer, from more torrid Regions, have appear'd to them almost like the steam that comes out at the open mouth of a heated Oven. And if *Marcus Polus Venetus* be to be credited, (for I mention his Testimony but *ex abundanti*) the Southern winds 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 oftentimes do so but by accident; for, as we elsewhere likewise teach, the steams that issue out of our bodies being usually warmer then the ambient Air, (whence in great Assemblies, even

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 restagnant 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 generation and motion of the wind upon the ſurface of the Earth, and ſhall only take notice in the remaining part of that Section of thus much; That the moſt of what Mr. *Hobs* here ſhews us, is but, that there is an expanſion of the air, or a wind generated by the motion and action of the Sun; but why this wind thus generated muſt produce cold, I do not ſee that he ſhews; nor does his affirming, 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 degrees 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 then 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 affear'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

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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 beat 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 ſuperficiate 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

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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, wherein any degree of compreſſion, that has been imploy'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 Experiments 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 Hiſtory 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

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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

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 vertue 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 touched 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

See the IV. *Section of the History of Cold.*

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

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 freez 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 entring 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 entring into the Earth at its pores, and other lesser cavities (for that seems to be his meaning by the laxity of the Earths 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 Antiperistasis.

from approaching the included water by divers yards; and very many Wells, that are subject to freez, 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 History 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 haſt, 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, 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, Pag. 673.

to which it belongs, then not gratifie some of the Curious, to whom the *Phænomena* I shew'd them of it, seemed no less 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 parts (or thereabouts *) of (yellow) Orpiment one part, of fair water 15. or 16. parts; beat the Lime grosly, and powder the Orpiment (with care to avoid the noxious Dust that may fly up *) and having put these two ingredients into the water, let them remain there for two or three hours, or longer, it needs be, remembring to shake or stir the mixture from time to time. By this means you will obtain a somewhat fœtid Liquor, whereof by warily Decanting, or by Filtrating it, the Clear part must be severed from the rest.

* *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.*

* *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.*

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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 common * *Minium*, and two or three times its weight of spirit of Vineger (which needs not be for this 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-

* *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.*

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, 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 decanted 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.

* *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 suspition, 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 pleased.*

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 then 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 causlesly 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 Con-

fess their secrets. But I must reserve the Reflections, and other particulars 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 communicate 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.

Philosophical Writings * already publish'd by this Author.

* *For those that concern Divinity belong not to this Catalogue.*

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 divers 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 Creatures.

Some Additional notes design'd by way of Appendix to the Physico-Mechanical *Treatise.*

Two Historical Dialogues, one concerning 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 (manifest and occult) by Art.

The Sceptical Naturalist, being a Letter about the Imperfections of Natural Philosophy, as we yet have it.

A Discourse of Improbable Truths.

AN

AN ADVERTISEMENT TO THE READERS OF

The following Experiments, by the Author of the foregoing History.

AT 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 Honorable Person, *&c.*

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 afterwards scrupulously abstained from borrowing the Trials mentioned in his Papers, to inrich mine: Which forbearance 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 Occasions, where the Learned Doctor and I happen to treat of the same subjects; our Trials are but Very few of them coincident; upon which score, the Reader will meet with more Variety betwixt us, then probably he would have expected to find on such an Occasion.

Having drawn up this Advertisement about the Doctors Papers, as supposing them the very same, he presented to the Royal Society; *upon a sight*

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 December 1664. *I presented to the* Royal 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 Experiments, 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 Curiosity,* 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-*

See the Publisher's Advertisement to the Reader.

*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 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*

* Among which I am since informed, that he had tried divers, before he saw my Papers.

* 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 Jejuness 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 another subject, which divers of the Virtuosi *do not more Expect, then Desire, to have communicated to them.*

An

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, boltsheads and ſome Conical ones. Moſt whereof by the diverſity of their figure, their openneſs or cloſeneſs pro-

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 φαινόμενα (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

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 *Arſenick* 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 ½ 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 freez 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 freez 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 ¾ full with this oyl, and about ¼ 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.

A beer-glass of *Hull* Ale in 24. hours contracted a crust of ice as thick as an Crown, and proceeding as in Sea-salt water, the φαινόμενα were the very same, all the *Laminæ* 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 freez 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 soft, 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. *Quære*, 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 whitings boil'd. The Tunicles, Fat and Muscles were also frozen, as appeared by their stifness, 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 freez 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 regiſtred 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 horse-raddish 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 Horse-raddish 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 Mariæ, Cælestis*, &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, *Aquafortis*, 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 then the spirit its self, and inflamable also.

But here 'tis to be observed that the said spirits that would not freez 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 smoother ice. The middle rib (if I may so say) as in plants was much bigger then the lateral ones, all which seemed but different *striæ*, 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, Scurvigrass, 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 ſhewed 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 pretermit, that the ſented waters ſeemed 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 *laminæ* of ice, ſome more ſome leſs wide, from which proceeded *ſtiriæ*, or teeth pointing inwards, and ſet at pretty equal diſtances, ſo that the *laminæ* and *ſtiriæ* reſembled very much ſo many combs placed in no order, ſome lying directly, others obliquely, none tranſverſly, having intervals betwixt each of them; betwixt ſome of them I could put my finger without breaking the points of the *ſtiriæ*: 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 resistence. 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 freez, 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 ¼ 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 water-mark. And here I shall briefly add two things, first, that if glasses be fill'd about ⅔ 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 stopt 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 befals 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 then 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 brass Cocks, and also the barrels in pumps made of brass 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 Inkhorn, 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 expoſed 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.

Alablaſter 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 adhæſion, 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 cohæſion 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 adhæſion to the table, but not ſo firm as it, *viz. Kelp*, *Sandever*, *Sal Indus*, *Gem. Prunellæ* and *Armoniac*, and Pot-aſhes, but not Alume or *Vitriol*.

The next experiment of adhæſion 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 ſoot, 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 putrifaction, healthiness, and confirming the tone of all animals, and thickning 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 ¼ of an inch in diameter, which so glase 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

and ¼, 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 apparant 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, whileſt 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 φαινόμενα 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 'twill 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 elegancy 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 dazled. *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, putrifie 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 Housewifes to prevent the rotting of Onions, commonly hang them up in their Kitchins, 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 Squils 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 in groſs, 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 ſetled in their ſtations the 15. of *October* 1662. the ſpirit in both having the altitude of three inches juſt. When the glaſs 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 $\frac{1}{4}$ 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 $\frac{1}{4}$. 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 $\frac{1}{4}$. 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 $\frac{1}{4}$ 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 Crystallized 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 soft greese. 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 ſence, 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 incompaſſed in a veſſel with ſnow and ſalt, they did freez to the thickneſs of an inch or more, and the air then beginning to relax, I broke the flasks, and the deſolved 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

 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 cruſted over with ice, as Eggs and Apples are, as if the freezing Atoms did paſs 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 immerſing them into water, they ſoon became more limber and flexible, and more eaſily yielding to the knife, and compaſſed with a cruſt of ice of the thickneſs 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 outſides, 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 freez 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 *Southwark* 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 Chanels where no constant current is, that water coming from the houses, soon fill the Chanels 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 imployed 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 Horſe-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 freez firſt, but the other with very great uncertainty. The Horſe-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.

FINIS.